Author's response to reviews

Title: Chronic Depression: Development and Evaluation of the Luebeck Questionnaire for Recording Preoperational Thinking (LQPT)

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Author's response to reviews: see over
Dear Editors,

we thank you and the reviewers for the very constructive comments and for the positive response to our manuscript.

We have taken all the comments into consideration. Here we give a point to point response to the concerns/suggestions.

Reviewer: Iris Liwowsky.

First of all thanks for the positive review with regard to the abstract, the well defined question, the Construction of the LQPT and the discussion.

We took the concerns regarding the evaluation part into consideration:

1. We have described the power analysis more detailed in the "results" section and made a necessary correction: In the new version of the manuscript we write in the section “results”: “Power analysis was made with PASS 2002 (Power Analysis and Sample Size Software for Windows). Power is the probability of rejecting a false null hypothesis. The post-hoc Power analysis revealed that the total sample of 90 achieves 100% Power to detect differences among the means vs. the alternative of equal means using an ANOVA with a 0.01 significance level.”

2. Variance analysis/post-hoc test: We have described the used tests more carefully. In our revised manuscript we write:

“A one-way analysis of variance was carried out with SPSS 17.0. Because of unequal variances and the small sample size we used Welch-Test. The analysis showed that the values differed significantly in the three groups at a level of 0.01 (Welch test: statistic. 41.667, df1 2, df2 44.517, p <0.001). Eta² was 0.466.

For the Post-Hoc comparisons Tamhane’s T2 was used. The group of chronically depressed patients differed significantly from the group of episodic depressive patients (p <0.001) and the group of healthy subjects (p <0.001). The group of episodic depressive patients differed significantly from the group of healthy subjects (p = 0.001). “
3. The reviewer suggested presenting the results of the factor analysis in the “results” section: We decided that we do not present the results of the factor analysis. The reason is that it was not the aim of the current study to do a factor analysis. This should be an aim of further studies with the final form of the LQPT and further samples. So in the new version of the manuscript we still mention it as a limitation in the discussion.

We write in the section “Discussion”: “Further studies to assess the reliability of the LQPT shall also be important: The aim was to record a uniform construct. The internal consistency (0.901) indicates that one characteristic could be reliably illustrated. A factor analysis resulted in no clear and meaningful multifactorial solution, but did show that the test appears to be more heterogeneous than is to be expected from a one-dimensional construct. Further studies must be done to investigate this.”

**Reviewer: Erik Roj Larsen**

We are grateful that this reviewer states that it is an article of importance in its field.

We took the concerns (Discretionary Revisions) into consideration:

1. and 2. we wrote in our first version manuscript: “This consideration led to the idea of developing a standardized instrument which contributes on the one hand to diagnosing chronic depressive disorders, and which on the other hand facilitates therapeutic decisions (in the form of an indication-oriented diagnosis). In addition, the instrument should also serve to evaluate therapeutic success more effectively (e.g. in the form of an evaluative diagnosis).”

It was suggested to take less strong messages. In the new manuscript we write:

“This consideration led to the idea of developing a standardized instrument which contributes on the one hand to diagnosing chronic depressive disorders by identifying the process and quality of preoperational thinking and which on the other hand facilitates therapeutic decisions (in the form of an indication-oriented diagnosis). In addition, the instrument should also serve to evaluate therapeutic success in changing preoperational thinking (e.g. in the form of an evaluative diagnosis).”
3. We likewise don’t think that it is a fact. But it is an assumption which is mentioned in the following article: Klein DN: Chronic Depression: Diagnosis and Classification. Curr Dir in Psychol Science 2010, 19: 96-100. We write in our revised manuscript: “Today it is assumed that chronic depressive disorders are a separate category within mood disorders.”

Additionally in DSM-5 it is planned to combine dysthymia and chronic major depression in the category: Chronic Depressive Disorder (D03). This is due to the fact that chronic depressive disorders differ from non-chronic major depression (D01/D02). (see http://www.dsm5.org/proposedrevision/Pages/DepressiveDisorders.aspx).

4. It was asked how the scores of the items are done (a) and if there is a cut-off point and if we made an ROC curve (b).

We describe this aspect in our revised manuscript more carefully.

a.) “It was assumed that these characteristics all constitute and record a unified concept. The scores of the items are all summed to produce a total value. That means that when the reaction is chosen which is the preoperational one a “0” is given. When the reaction is chosen which is the non-preoperational one a “1” is given. A low total value means a high level of preoperational thinking, while a high total value means that the level of preoperational thinking is low.”

b.) Cut-off point and ROC curves are very important. With the data included in the manuscript (pilot study) we didn’t do that so far.

One reason is that the sample size of 90 is small to test the psychometric properties of a new questionnaire. Consequently additional studies with larger samples must be done. Another important reason is that to make useful declarations concerning e.g. selectivity/specificity additional results like e.g. comparison with achieved value in the LQPT and external observations of preoperational thinking are needed.

5. Item analysis: it was asked if a Rasch Modell was used and was mentioned that the test used are missing.
First of all in the study we focused on “classical test theory” and therefore didn’t use a Rasch model. In the revised manuscript we explain that the selectivity and difficulty indices were computed with SPSS Version 17.0.

Reviewer: Eva-Lotta Brakemeier

We are grateful that this reviewer states that it is an article of outstanding merit and important interest in its field. We thank a lot for the many helpful and constructive comments and suggestions.

We took the comments (Minor essential revisions) in our revised manuscript into consideration:

1. Language:

Actually the first version of the manuscript was reviewed by an English native speaker. But we have reconsidered the language issues again in our revised manuscript.

2. Title:

a) We decided against changing the title in: “The Specific Psychopathology of Chronic Depression: Development and Evaluation of the Luebeck Questionnaire for Recording Preoperational Thinking (LQPT)”. The reason is that preoperational thinking is only one aspect of chronic depressive patients. According to McCullough it is the specific psychopathology of chronic depressive patients. But we think that future research must show if it is the specific one because there are other features like negative feedback seeking and reinsurance which might also be important.

b) LÜQ_PreT sounds smoother, but we think that LQPT can be better remembered and is more internationally, so we decided for not changing the abbreviation.

3. Background:

Generally we have considered all comments and we have made several changes in the revised manuscript.
a) and b): we discussed the comments concerning focus and order. We decided against changing the order because the focus of the study is the development of the LQPT and we wanted to describe first of all our deliberations/reasons for developing the instrument. Additionally there are many excellent articles describing CBASP.

c) The suggestion was to mention all efficacy studies including the Schramm studies and to discuss the Kocsis study in detail. We decided against this suggestion because it was not the aim of the current manuscript to summarize all the efficacy studies. But we have included one additional study. In our revised manuscript we write: “German studies as well show that CBASP seems promising. In a randomized pilot study Schramm et al. 18 included 30 patients with early-onset chronic depression. The patients were randomized to 22 sessions of CBASP or Interpersonal Psychotherapy (IPT) provided in 16 weeks. While the primary outcome (score on the 24-item Hamilton Rating Scale for Depression (HRSD) assessed post treatment by an independent blinded evaluator) was not significant, secondary measures (remission (HRSD≤8) rates and the Beck Depression Inventory (BDI)) showed relevant benefits of CBASP over IPT.”

d) It was suggested describing the few studies having tried to test this hypothesis. In the revised manuscript we write: “Only few studies have tried to test the hypothesis that chronically depressed patients show particular characteristics in cognitive psychopathology. Wilbertz et al. (19) investigated 16 chronically depressed patients with early-onset depression and compared them with 16 healthy controls using a "ToM"-test (the MASC - Movie for the Assessment of Social Cognition), a self-assessment questionnaire for the detection of empathy (the IRI - Interpersonal Reactivity Index) and a structured assessment of preoperational behavior by the therapist. The findings suggested that the chronic depressed patients did not significantly differ from healthy subjects in their ToM-performance. In the estimation of empathy the chronically depressed patients were classified as being inferior to healthy control subjects. In addition, the therapists were able to observe a range of preoperational behaviors amongst the patients (19). Zobel et al. (20) studied chronically depressed patients (n = 30) using the "cartoon picture story"-test. They were able to show that patients differed significantly in their ToM-performance from healthy subjects. However, after control for logical memory and working memory, ToM-performance was not able to predict patients as such.
One criticism of findings from the ToM is that the materials used may not be suitable for investigating ToM-deficits in adulthood and that more appropriate methods still need to be developed (19). The assessment of preoperational thinking in adulthood also suffers from a lack of adequate tools. There is no instrument directly measuring preoperational thinking in adults.”

e) In the revised manuscript we have claimed that our study has three aims.

4. Methods:

a) It was suggested to add the word “interpersonal” short stories. We have not changed it and still write in the revised manuscript short-stories. The reason for this decision is that not all items are interpersonal (e.g.: snapshot perspective-intrapersonal: Item 2).

b) Description of preoperational thinking is given in the Background section. Here only a list of the components is given.

c) Yes, it is “Lack of empathy” and not “empathy”. We have changed it in our revised manuscript.

d) We know that dysfunctional emotional control is a fundamental aspect of preoperational thinking according to McCullough. But we decided against integrating items concerning this feature in the LQPT because there are many good instruments which record this specific aspect (e.g. Stress Coping Questionaire).

5. Evaluation:

a) We have described the power analysis more detailed and made a necessary correction: In the revised manuscript we write: “Power analysis was made with PASS 2002 (Power Analysis and Sample Size Software for Windows). Power is the probability of rejecting a false null hypothesis. The post-hoc Power analysis revealed that the total sample of 90 achieves 100% Power to detect differences among the means vs. the alternative of equal means using an ANOVA with a 0.01 significance level.”

b) Actually it is a small sample size and we pinpointed it more clearly in the discussion. In the revised manuscript we write in the “discussion” section: “Furthermore the total sample of 90 is small to test the psychometric properties of a new questionnaire. Consequently additional studies with larger sample sizes must be done.”
c) It was suggested to pinpoint the exclusion and inclusion criteria. From our point of view they are described in the text. But we have added in the revised manuscript the following sentence: “An exclusion criterion for the “healthy volunteers” was the existence of any mental illness in the present or the past.”

d) Yes, standard deviations are important. The standard deviations concerning age are added in the revised manuscript.

e) Indeed we have informations concerning early or late onset of depression because it is an important fact/distinction. In the current study we do not differentiate between early and late onset chronic depression because of the small sample size (n=30). This might be an aim of future studies.

f) In our revised manuscript we give the information concerning our sample in the section “results”.

g) Thanks for the suggestion. Analyzing correlations between the CTQ and the LQPT is very interesting and might be an aim of future studies.

h) We have described the statistical tests. In our revised manuscript we write: “To analyze this it was planned to carry out an ANCOVA. For testing the differences between the three groups it was planned to apply an ANOVA.”

6. Results:

a) We describe our three study groups at the of the section “results”. The aim is that the reader can see that there are no differences regarding the demographic characteristics. But we do not show the data in an additional Table. The reason for this is that we didn’t want to overtax the reader with too much redundant information.

b) We have mentioned the terms “construct validity” and “discriminative validity/known-groups validity” more clearly.

c) In our revised manuscript we have checked the English terms for analysis of covariance.

d) In our revised manuscript we have integrated a table (Tab. 3) which shows the correlations between the BDI and the LQPT (total sample and differentiated according to group).

7. Discussion:
a) It was suggested to write in the discussion: “The first aim…” We still write in our revised manuscript: “The aim of the study…”. The reason is that from our point of view it fits better. It is not the first aim but the main aim.

b) We have considered the suggestions and have added the suggested sentences. We write in our revised manuscript: “The LQPT is able to distinguish between chronically and episodically depressed patients at the level of preoperational thinking. As a valuable consequence with the help of the LQPT the special subgroup of chronically depressed patients could be described more thoroughly in future research. As such, the LQPT may in future be a valuable contribution to the study of the effectiveness of therapies. In addition, the LQPT could be used to systematically investigate the specific pathology of chronically depressed patients. Up until now the preoperational behaviors or thoughts of chronically depressed patients have usually only been described in case studies or in the form of external observations. Finally, it would be interesting to find out whether the LQPT even provides the opportunity to predict who will respond or who will not respond to CBAP or other therapies.

c) It was a suggestion to state our limitations more clearly. Generally we have considered the suggestion and we have made some changes in the revised manuscript.

d) We have worked out what we want to say concerning the comparison between ToM and LQPT.

8. Conclusion:

a) We have considered the suggestions and write in our revised manuscript: “Overall, the results of this study showed that the LQPT is a useful, reliable and valid instrument to measure McCullough’s assumed preoperational thinking in chronically depressed patients.”

9. Tables:

a) Tab. 1: We have added lines to make the table less hard to read.

b) Tab. 2: We have added the statistical test values in the table.

10. References:

We have corrected the spelling. In our revised manuscript we write: Zobel.
Reviewer: Ulrich Palm

We are grateful that this reviewer states that it is an article of importance in its field and that he states that we make a timely and necessary contribution for the non-pharmacologic diagnostic/treatment of chronic depressive disorders.

We have considered the suggestions/concerns:

1. It was suggested that we could add a synopsis of the relevant literature regarding diagnostic tools of chronic depression for emphasizing the lack of adequate tools and better emphasizing the necessity to develop the LQPT. In our revised manuscript we write: “Only few studies have tried to test the hypothesis that chronically depressed patients show particular characteristics in cognitive psychopathology. Wilbertz et al. (19) investigated 16 chronically depressed patients with early-onset depression and compared them with 16 healthy controls using a "ToM"-test (the MASC - Movie for the Assessment of Social Cognition), a self-assessment questionnaire for the detection of empathy (the IRI - Interpersonal Reactivity Index) and a structured assessment of preoperational behavior by the therapist. The findings suggested that the chronic depressed patients did not significantly differ from healthy subjects in their ToM-performance. In the estimation of empathy the chronically depressed patients were classified as being inferior to healthy control subjects. In addition, the therapists were able to observe a range of preoperational behaviors amongst the patients (19). Zobel et al. (20) studied chronically depressed patients (n = 30) using the "cartoon picture story"-test. They were able to show that patients differed significantly in their ToM-performance from healthy subjects. However, after control for logical memory and working memory, ToM-performance was not able to predict patients as such.

One criticism of findings from the ToM is that the materials used may not be suitable for investigating ToM-deficits in adulthood and that more appropriate methods still need to be developed (19). The assessment of preoperational thinking in adulthood also suffers from a lack of adequate tools. There is no instrument directly measuring preoperational thinking in adults.”

We hope that this supplement emphasizes the necessity to develop the LQPT more clearly. A synopsis of all the literature regarding used tools in chronic depressive disorders would go beyond the scope of the current manuscript.
2. Language corrections were carefully made in the revised manuscript.

**Journal style:**

We also ensure that our revised manuscript conforms to the journal style.

Sincerely Tanja Kühnen