Reviewer’s report

Title: Association between fatigue, motivational measures (BIS/BAS) and semi-structured psychosocial interview in hemodialytic treatment

Version: 1 Date: 27 Mar 2019

Reviewer: Haikel Lim

Reviewer’s report:

Thank you for the opportunity to review this revised manuscript. I appreciate the amount of work the authors have put into revising the manuscript. I have a few suggestions that I think might improve upon the manuscript quality for publication in BMC Psychology.

1. The authors might need to clarify if only central or both physical and central fatigue are related to (mediated by?) inflammation (line 55). It would also be good if the authors could give examples of such clinical/biomarkers associated with fatigue; and on related note I would generally caution the authors against making such a statement given that, at best, these associations are non-causational--ill patients generally have elevated inflammatory markers independent of fatigue.

2. Given the broad reach of BMC Psychology, it would benefit readers if the authors explicated briefly how "inflammatory processes have...influence[d] the functioning of basal ganglia" (line 59). Further, I think the line of argument may need to be refined; at present the authors are arguing that: fatigue involves some amount of inflammation, inflammation influences basal ganglia ?functioning, basal ganglia and prefrontal cortex structures influence motivation/reward, BIS/BAS has been associated with prefrontal cortex ?therefore may influence fatigue. The issue here is that, as the authors argue, the BIS/BAS framework seems to have been only associated with the PFC--if there any evidence to suggest that it might be related to the basal ganglia it would greatly help the line of argument.

3. While I think the authors are operationally and functionally using the "scale" for BIS/BAS, might I suggest that the authors themselves actually mean to use the theoretical framework of the BIS/BAS for understanding behavioural motivation in patients on HD? Specifically highlighting the tool may bring about significant criticism regarding the psychometric properties of the tool and its interpretation, which I assume is not really the crux of this manuscript--rather, the authors seem to want to lens the BIS/BAS to better understand fatigue in patients on HD, which seems to be a novel contribution to the literature.

4. I am still having some trouble seeing the value the qualitative component brings to this manuscript (lines 106-111) given that the authors have rightly pointed out other studies that have since been done on fatigue amongst patients on HD (lines 103-105). Perhaps if the qualitative component were to better reinforce or elucidate the findings from an as-yet theoretical conceptualisation of BIS/BAS as mediators/moderators of fatigue, this might be clear to readers and would capitalise on the value of their contribution of this manuscript to the extant literature.
It is important for the aims to come across strongly to the readers because the analyses later may come across as confusing.

5. The hypotheses might need to be clarified; might I suggest the authors present their hypotheses in accordance to their aims? Based on my interpretation of the manuscript, I am assuming that the authors' aims were to (a) identify the influence of BIS/BAS as a theoretical framework to understand fatigue in patients on HD; (b) examine the influence of gender in the relationship between BIS/BAS and fatigue; and (c) explore how patients' lived experiences further reflect and reinforced the relationship between BIS/BAS and fatigue. Therefore, in line with these aims, it seems like the hypotheses are (a1) as the authors rightly described (lines 117-118); (a2) a negative correlation between BAS and fatigue scores (again I am postulating but am not sure if this is what the authors are gunning for); (b1) gender would moderate the relationship between BIS/BAS and fatigue; (c1) the BIS component would be correlated with more negative(?) themes... ; and (c2) the BAS component would be correlated with more positive(?) themes. This is the assumed hypotheses that I am going on when reviewing the rest of the paper.

6. It would be helpful for the authors to also use the full phrase for the FSS in page 6 (line 118) as I believe this is where we first encounter the term.

7. It would be helpful for readers if the authors included their sampling frame numbers or the response rate just as a gauge for sample representativeness. It might also be helpful if the authors highlighted that all N=94 participants were interviewed (I am assuming).

8. I am assuming the clinical markers are those that may influence disease control/progression/severity and may be related to fatigue (anemia, etc.); but the authors may need to justify their use of the BDI and STAI since these are not mentioned in the hypotheses but are used all throughout the results. It might also be helpful to label Y1 as State and Y2 as Trait.

9. Were the additional medication taken (BB, CCB, ACEi, EPO, etc.) taken into consideration in the analyses given their influence of "fatigue" scores? If so this should be highlighted, and if not, this should be included as a limitation.

10. It would be helpful for readers who are not familiar with these scales (FSS BIS/BAS BDI STAI) if the authors provided the Cronbach's alpha in their sample.

11. Might the interview questions, especially the closed questions, that were used be available as supplementary material for readers? It would be helpful to also understand which items were used as dichotomous responses, and which were scored on a Likert-type scale. If one item could be graded both dichotomously and as a Likert-type format this needs to be explained and justified.

12. It is also unclear what the authors are describing from lines 220-225. Based on my reading it seems like the closed-ended questions were graded on a dichotomous or Likert-type scale, and the qualitative components were also then given scores. It is not very clear why the qualitative components were given scores, and how these were scored. Was each transcript graded on how frequent the topics (1-8) appeared as compared to itself, or to the rest of the 93 transcripts? Given
the subjective nature of agreement between judges, the scientific and quantitative rigour applied
to the qualitative interviews need to be better explicated.

13. Given the large spread of "dialytic vintage", might the authors suspect that this would
influence results? This should be discussed. Further, was this, and other variables, "normal"
based on the kurtosis/asymmetry? Given the parametric tests used I am assuming this is the case.

14. If the authors are going to compare gender (as alluded to in their hypotheses; Table 3), please
also state which test was being used in the statistical analyses portion--no statistical values other
than the P value was used so readers are left wondering which tests were employed. If the goal is
to investigate the moderating effect of gender, there are many other suitable tests for these aside
from just splitting the sample in two.

15. I think the authors using mean BDI and STAI scores are somewhat problematic given the
spread of their data. Again, the use of these scores need to be justified, and if it truly is to rule out
patients who may have comorbid probable depression/anxiety then perhaps the authors might
want to use another approach (exclusion, controlling for these in regression/correlation models,
etc.).

16. I am of course basing this comment on my assumption of what the hypothesis is, but I
believe that a regression model or at least a partial correlation model would be more suitable to
investigate the BIS/BAS relationship on fatigue (controlling for the effect of depression/anxiety/clinical lab parameters). The hypotheses again needs to be clear so that we as
readers can follow the line of thought and better appreciate the results.

**Are the methods appropriate and well described?**

If not, please specify what is required in your comments to the authors.

No

**Does the work include the necessary controls?**

If not, please specify which controls are required in your comments to the authors.

Unable to assess

**Are the conclusions drawn adequately supported by the data shown?**

If not, please explain in your comments to the authors.

Unable to assess

**Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?**

If an additional statistical review is recommended, please specify what aspects require further
assessment in your comments to the editors.

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Please indicate the quality of language in the manuscript:

Needs some language corrections before being published

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