Reviewer’s report

Title: Similar polysomnographic pattern in primary insomnia and major depression with objective insomnia: A sign of common pathophysiology?

Version: 1 Date: 09 May 2017

Reviewer: Jennifer Goldschmied

Reviewer's report:

The authors provide a well-conducted and interesting study investigating the relationship between polysomnographically derived variables and insomnia in major depression and primary insomnia. The study is novel in its methodology and its conclusions are fairly interesting, however the writing should be revised substantially to improve readability (several areas of repetitive statements, preposition confusion, overall wordiness). I am including several concerns that limit my overall enthusiasm for this paper, but feel that addressing these concerns may allow this to be an interesting, albeit incremental addition to the literature:

1. The rationale of the present study seems to rest on a reference to Hubain et al that contradicts most commonly found PSG measures in MDD. However, the summary of these findings and their clinical relevance, as it is currently written, is not particularly compelling, which results in the overall aim of the present study being left on shaky ground.

2. Sleep efficiency was used as an inclusion criterion for both the insomnia and MDOI groups, however the authors did not discuss how SE was obtained. My assumption is that PSG defined SE from night 2 was used, however this should be clarified. Additionally, it is difficult to interpret conclusions based on differences in SE (as compared to HC) when SE was used as an inclusion criterion.

3. Relatedly, the authors suggest that due to the limited differences in PSG measures between insomnia and MDOI, the two disorders must share a common pathophysiology. However, since the MDOI group was selected based on their similarity in symptomatology to primary insomnia, this renders this kind of interpretation questionable (I believe the authors may hint at this concern, although the language in the paragraph at the top of page 17 is somewhat hard to follow). Instead, I would suggest changing the aims of this study to ask if an additional diagnosis of MDD adds incremental PSG disturbance to a diagnosis of insomnia, as this seems to be a more parsimonious question.

4. The results section (and corresponding tables) reads like a laundry list of findings with no cohesive narrative - I had a very hard time identifying the most important results. I would suggest narrowing the results down to key areas and discussing how certain findings complement (or do not) one another. Additionally, representing some data graphically,
instead of in a table, would also help to highlight the most important findings. Furthermore, the addition of tables 3 and 5 seem to be redundant - this information can also be represented in tables 2 and 4. As a note, I actually suggest highlighting the analysis of PI vs MDOI, than HC vs PI and MDOI, as it stands currently.

5. The discussion of increased hyperarousal in insomnia and MDD in the discussion section is an important one, however, as it is currently written, it is neither clear nor compelling. Similar to my suggestion for the results section, I would encourage the authors to identify the main, key points and highlight these with regard to the present data and past research findings to allow for a more cohesive narrative.

6. The rationale for the inclusion of analysis of the 'thirds' of the night in the introduction was not particularly compelling since it has limited support (Sventnik et al). Additionally, since there didn't seem to be many significant results regarding the split by thirds, I would suggest to discuss these findings sparingly, and not to include it as a primary aim. As a note, the discussion regarding the distribution of the night as the reason behind the SWS deficit being limited to the first third while the REM deficit is present in the first and last third being does not seem to fit, however the discussion of the WASO changes in the 'thirds' of the night does seem to have some potential - I would suggest exploring and expanding this piece.

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

Yes

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Yes

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

Yes

**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.

I am able to assess the statistics
Quality of written English
Please indicate the quality of language in the manuscript:

Not suitable for publication unless extensively edited

Declaration of competing interests
Please complete a declaration of competing interests, considering the following questions:

1. Have you in the past five years received reimbursements, fees, funding, or salary from an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

2. Do you hold any stocks or shares in an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

3. Do you hold or are you currently applying for any patents relating to the content of the manuscript?

4. Have you received reimbursements, fees, funding, or salary from an organization that holds or has applied for patents relating to the content of the manuscript?

5. Do you have any other financial competing interests?

6. Do you have any non-financial competing interests in relation to this paper?

If you can answer no to all of the above, write 'I declare that I have no competing interests' below. If your reply is yes to any, please give details below.

I declare that I have no competing interests

I agree to the open peer review policy of the journal. I understand that my name will be included on my report to the authors and, if the manuscript is accepted for publication, my named report including any attachments I upload will be posted on the website along with the authors' responses. I agree for my report to be made available under an Open Access Creative Commons CC-BY license (http://creativecommons.org/licenses/by/4.0/). I understand that any comments which I do not wish to be included in my named report can be included as confidential comments to the editors, which will not be published.

I agree to the open peer review policy of the journal