Reviewer’s report

Title: Sleep complaints in adolescent depression: One year naturalistic follow-up study

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Reviewer: Brandy Roane

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This manuscript has several strengths including posing a relevant question and using a clinical sample to examine the question. As such, the manuscript is relevant, but does have several areas that need to be addressed to strengthen its merit.

Major Compulsory Revisions

1. More information is needed on the sample used for these analyses. The authors reported a sample size of 218 in the larger study with a sub-sample of 166 included in these analyses; however, exclusion criteria were not clear. Were these exclusions applied to the larger study or the subsample? If it was applied to the larger sample and not the sub-sample, why were 52 participants excluded from the sub-sample?

   a. If it was the result of attrition for their exclusion, data on those that did not continue should be included. What did their measures look like in comparison to those who completed? This is particularly relevant given the conclusion the authors have drawn from their data indicating that “sleep disturbances at baseline do not necessary lead to poorer clinical outcome during follow-up.”

   b. If attrition was an exclusion for participants to be included in the final larger sample, this should still be listed as a limitation in the results. In addition, it really tempers the conclusion as the data cannot speak to this without knowing if those that did not complete showed more severe baseline measures than those who did complete. It’s possible that for the participants who did complete treatment, sleep disturbance at baseline did not predict worse outcomes; however, it is also just as possible that sleep disturbance at baseline predicts completion with those who exhibit more severe sleep disturbances also are those more likely to not complete treatment.

2. Overall, more specific information is needed on attrition. This is really not touched on until the discussion, but no data is included with regards to rates of attrition. The authors starting on line 307 discussing attrition. They noted that there “was a slight but no major difference in attrition rates in favour of adolescents with the least sleep complaints,” but what does this mean? Was it a significant difference? Did they examine statistical significance?

3. Clearer presentation of the variables being examined is needed (i.e., clearly identify the variables and define them). At present, all but 2 of the variables are contained within one paragraph. Brevity is great, but not at the cost of clarity.
4. Similarly, rationales need to be provided for the variables. For instance, why was GAF scores used for global functioning rather than a more objective (and reliable) assessment? Why were the sleep variables excluded from the BDI? While this is a reasonable call, why was it done? What support do you have for doing it? Excluding items from an assessment can impact the integrity of the assessment, whether the assessment is a validated one or not. Thus, it would also be very beneficial to report on the reliability of the measures in your sample (e.g., listing Cronbach’s alphas for your specific sample).

5. The results section is confusing at times, particularly the interaction section. A table would greatly help with presenting and discussing the data.

6. In the discussion (lines 272-275), the authors indicated that “these findings highlight the tight link between sleep complaints and depressive state and support the view of sleep complaints as state-dependent features of adolescent depression, as opposed to sleep disturbances representing persistent vulnerability traits” is an overstatement of the findings. This type of statement requires a more rigorous hypothesis testing and likely more data points. Like depression, sleep disturbances can remit and reappear over time. At the very least an additional assessment point (say another year later) would need to be included or data on how sleep changed throughout the course of treatment.

7. Similar to the above point, the authors note the limitation of the use of sleep items on a non-sleep specific measure to determine the presence of a sleep complaint; however, their conclusions seem to forget this limitation (lines 301-305). Specifically, their conclusions exclude the fact that their measure of sleep disturbance is based on a single time-point measure at baseline and at follow-up as opposed to more frequent assessment. The questions are from a self-report questionnaire that can be influenced by mood state and is plagued with retrospective recall issues. The decrease in sleep complaints may not be due to an actual decrease in symptoms, but due to an improvement in mood. The actual change cannot be sufficiently determined with the limited assessment of symptoms.

8. Later in the discussion (lines 314-321) the authors again seem to ignore previously reported fluctuations in sleep complaints and the very complex relationship between sleep and mood. One possible alternative conclusion given the data is that the sleep disturbance may have been driving the depression for some of the teens. The successful resolution of the sleep disturbance subsequently resulted in a reduction in depression symptoms. The treatments used, particularly the SSRIs, are prescribed for patients with insomnia. Thus, the SSRI, while prescribed to target the depression, may have actually successfully managed the sleep disturbance, and, thereby, the depression improved.

Minor Essential Revisions

9. Consistent naming of variables throughout the manuscript. If you are reporting on symptoms and not actual diagnosis, establish the term you will use to refer to it and stick with that term. For instance, the authors referred to anxiety symptoms as “anxiety levels,” “anxiety,” and “anxiety symptom severity.” These do not all mean the same thing.
10. A rationale is needed for why sleep disturbances was categorized the way the authors chose to. The division appeared very arbitrary. Why was a complaint of non-restorative sleep not considered a “complaint of sleep disturbance,” but instead was a “minor sleep complaint” that was grouped with “no complaint”?

11. Lines 220-221 state findings that are not supported with statistics. According to what analysis was age associated with suicidality/self-harm symptoms and sex associated with BDI scores?

12. The first two figures appear somewhat redundant and could be combined.

Discretionary Revisions

13. The manuscript overall reads well, but a few grammar/punctuation edits would be useful.

14. The terms “subgroup A,” “subgroup B,” and “subgroup C” is confusing once you get past the designation. As indicated above, a table with data would help with presenting the findings. It would likely also help with cementing these terms throughout the manuscript.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests.