Reviewer’s report

Title: Having mentors prevents depressive symptoms via decreasing internet gaming: A moderated mediation analysis

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Reviewer: Sander Matthijs Eggers

Reviewer’s report:

This manuscript discusses the mediating role of three variables, namely the hours spent searching the internet, hours spent gaming, and sleep duration, on the association between the amount of worries experienced and the level of depression, and whether the number of mentors or social networks moderates this indirect effect. The results showed that sleep duration and the hours spent gaming, but not the hours spent searching the internet, mediated the effect of worries on depression. Furthermore, this indirect effect was moderated by the number of mentors and the number of social networks, which implies that having more social networks or mentors is a potential protective factor for the development of depressive feelings.

This idea is interesting and original, although some serious amendments are needed to make the manuscript suitable for publication.

Major revisions:

1. First, with regard to the title, I would suggest the authors use a different formulation. More specifically, the words ‘prevents’ and ‘decreasing’ are not quite accurate, since causality cannot be inferred from cross-sectional research. Please correct this throughout the manuscript.

2. The manuscript contains many English grammar and formulation issues that could be resolved by sending it to a proofreading service.

3. When describing the student population (in Table 1, method or results section), I would recommend to describe the variance in depression scores for the student population more carefully. Especially since its mean score (4.51) and the potential range (0-63) imply that the scores were quite skewed and because it is the main variable of interest.

4. The results section needs restructuring. Start with explaining the mediation results carefully (do the hours spent gaming / the hours spent searching the internet / sleep duration mediate the association between worries and level of depression). Follow-up with the results of the moderated mediation analysis and its interactions (Table 2 & 3). Next, report the conditional indirect effects. And finally, report the ‘additional analyses’. Please also add corresponding headers to these paragraphs.

5. The second paragraph of the results section starts with “As a result, there was no significant result when the sum of daily amount of internet video game playing
and internet searching was treated as one mediator”. First of all, it is not clear from the introduction that this is what was originally intended (treating both these variables as one), and second, the lack of effect when combining the two variables is not ‘a result’ of the inability of the PROCESS macro to allow two moderators (mentioned in the first sentence of that paragraph). I would recommend dropping the sentences that explain why you treated hours spent gaming and hours spent searching the internet separately, since most scholars would agree that they are conceptually distinct and should be treated as such. Alternatively, the introduction could be rewritten to make it clearer why these variables were treated as one.

6. The results section contains some conclusions that should be reserved for the discussion.

7. In relation to the previous comment, the authors mention at the end of the fifth paragraph of the results section that the ‘direct effect of number of worries on depression appeared to be moderated by the number of mentors (interaction 4)’. Next they mention that they ‘unexpectedly found that the conditional indirect effect was non-significant’. However, the previous sentence was related to a direct effect, so it is unclear which effect the authors are referring to.

8. In the final sentence of the results section, the authors refer to figure 4 as the final moderated mediation model. From my understanding of the results, one moderation arrow is missing, namely the moderating effect on the direct relationship between worries and depression (Interaction 4) (the answer to this could be related to the previous comment).

9. Throughout the discussion, the authors overestimate the implications of their study by assuming causality and making statements like ‘The higher the number of networks or activities the student had, the lower the amount of time students spent playing video games, which in turn decreased depressive symptoms’. Please avoid words such as ‘decrease, increase, affect, influence’, unless they are described as potential causal effects.

10. One remark should be added to either the discussion or the introduction: The measures used in the current study only assessed the quantity of worries/networks/mentors/sleep. A useful recommendation for future research would be to investigate whether including quality/severity of the worries/networks/mentors/sleep would yield similar results.

Minor revisions:

1. In the third paragraph of the introduction, the term ‘IMing’ is used without clarification. I assume this refers to instant messaging?

2. In the fourth paragraph of the introduction a transition is made from ‘video gaming in general’ to ‘internet gaming’. From the introduction, it is unclear whether there is an actual distinction between the two that should be taken into account. In other words, is there a reason for specifically referring to ‘internet gaming’ (throughout the manuscript) instead of ‘video gaming’, which is a more commonly used term. If so, please mention this in the introduction.
3. In the sixth paragraph of the introduction, the authors write: “In particular, this approach identifies ranges of the moderators”. Here, the authors cite a publication of Preacher et al. (2007), which is relevant, but the technique used to ‘identify ranges of moderators’ is from Johnson and Neyman (J-N). Please refer to it as such.

4. In addition, the particular sentence in the previous comment ends with ‘SEM uses ± 1 standard deviation from the mean of the moderator’. This is not entirely correct since SEM is a modelling technique that does not make specific assumptions about how to assess moderated effects / simple slopes. It is usually the choice of software (Mplus, EQS, Amos, LISREL, etc.) that defines the operationalization of moderating effects.

5. Please italicize all statistical abbreviations such as r, M, N, SD, and leave a space between the symbols (for example, N = 1450).

6. In the measurements section, the authors refer to the BDI and that it has a ‘good alpha coefficient’. Please mention if it is the Cronbach’s alpha that is referred to. In addition, it is unclear whether the psychometric properties mentioned (alpha, test-retest, consistency), are based on the current study or on a previous study (Most likely Beck and Steer, 1988), since there is no citation in the final sentence.

7. From the measurements section it is unclear how sleep duration and hours spent gaming/internet searching was measured. Please mention the actual question and the unit used to record the answers (hours/minutes?).

8. In the statistical analyses section, please change ‘two mediators’ into ‘three mediators’ since gaming/internet searching were analyzed as separate mediators.

9. I am unfamiliar with the ‘Eigenvalue’ technique mentioned in the statistical analysis section. From my limited understanding of eigenvalues, I understand that they are an indication of the amount of variance that can be explained by latent factors. They can therefore be used to assess how many latent factors should be drawn from a pool of items (factor analysis). Please provide a reference that indicates how they can be used for the purpose of assessing moderation or explain in (slightly) more detail.

10. In the analysis section, it is mentioned that variables were mean-centered prior to the analysis. Usually this is done to aid interpretation (e.g. going up/down one unit in comparison to the mean). However, when using variables such as the ‘number of mentors’ and ‘number of social networks’, mean centering does not aid interpretation. In Table 2 and 3 for example, the interpretation of the different levels of the moderator (number of mentors/social networks) is quite troublesome (i.e. the tables show negative numbers of mentors/social networks). I would therefore recommend to not mean-center these variables.

11. In the analysis section, it is mentioned that normality of the variables was examined by checking their respective skewness and kurtosis values. Please mention in one or two sentences what the findings were. I assume some variables (such as hours spent gaming or level of depression) were quite
skewed? Given the large sample size, this may not be an issue, but it is still informative to mention.

12. Please provide a reference, if applicable, when citing the ‘conditions to demonstrate moderated mediation’ at the end of the statistical analysis section.

13. With regard to Table 1, the measurement section, and the description of the sample, I would recommend mentioning the range of the scores. I was surprised, for example, to see a mean score of depression of 4.51 (SD = 5.20), when the range mentioned in the method section is 0-63. Similarly, it is unclear from the table and method section what the maximum (and minimum) number of worries/networks/mentors was that students could indicate.

14. In Table 1, please change the footnote to the more commonly used description: *** p < .001, ** p < .01, * p < .05

15. The third and fourth paragraph of the results section start with an overview of main effects in the mediator and dependent variable model. However, these models include interaction terms, which make the interpretation of the supposed main effects more complex. I would suggest discussing these main results in an earlier paragraph that discusses the mediation models (without interaction terms).

16. In the last sentence of the sixth paragraph of the discussion, results are mentioned of the reversed mediation model. Please mention them in the results section under ‘additional analyses’.

17. It is unclear whether beta coefficients presented in the manuscript are raw coefficients or standardized coefficients. I suspect the PROCESS output presents raw coefficients, and I would therefore recommend to change all Greek beta symbols (#) into capital B’s.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Not suitable for publication unless extensively edited

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

**Declaration of competing interests:**

I declare I have no competing interests