Author's response to reviews

Title: Having mentors moderates the impact of worries and video gaming on the development of depressive symptoms: A moderated mediation analysis

Authors:

JongSun Lee (sunny597@gmail.com)
Bumseok Jeong (bumseok.jeong@gmail.com)

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Response to Peer Review Comments

Thank you for your editorial letter regarding the manuscript entitled “Having mentors buffers the impact of worries and internet gaming on the development of depressive symptoms: A moderated mediation analysis”. As requested, we have attached a revised manuscript in which we have addressed the points raised by the reviewers. We have also highlighted the changes to the manuscript. Professional editors have checked the English (Certification: https://lang.kaist.ac.kr/popups/certification/299).

Reviewer1: Christopher Ferguson

Major, compulsory Revisions:

1) As one issue, the report has such a large sample size that many very minor effect sizes (for example $r = .04$ for video game use and depression) are reported as "statistically significant" without noting that these effects are so small (almost zero) that they have little practical relevance. This is a big risk of studies with large sample sizes...you can get a lot of spurious effects that pop up as statistically significant, despite being very small. I'd advocate selecting a minimal effect size, typically either $r = .1$ or $r = .2$ as minimal for practical significance and I would treat all values below that as non-significant. This would help with overreporting of spurious effects. Also the authors should, in their discussion, be careful to highlight...and I mean repetitively...that the effects they are seeing are very, very small, and should not be overinterpreted.

R. We agree with the reviewer’s point. Therefore, in the results section under preliminary correlation analyses, we have described that “although p values between the variables (e.g., $r = .04$ for video gaming and depression) were significant, their effect sizes appeared to be very weak”.

2) The Gentile et al., study reported by the authors is not a very good one and has come under criticism for the non-valid measures used in the study and for their eagerness to overinterpret very weak effect sizes. I would suggest dropping that study due to it’s flawed nature, or spending some time being very honest about it’s significant flaws (and I mean several sentences, not just a brief note).

R. We have dropped Gentile et al.’s study from the document.

3) I wanted to see more detail on the measures, particularly worry and mentors, including coefficient alphas for the current sample.

R. We have tried to give more detail on the measures. For example, we have added the actual question and the unit used to record the answer for daily sleep duration and daily amount of
video gaming. However, we could not report coefficients alphas for some measures (e.g.,
number of mentors or worries) due to their nature, being only a single question.

4) In the multivariate analyses, I think you are reporting unstandardized coefficients
but calling them "beta"...beta, however, is usually reserved for standardized
regression coefficients. I'd actually rather you report the standardized regression
coefficients, not unstandardized (standardized are more useful). If these ARE
standardized coefficients, I wonder if there may be a problem...they appear to be
much larger than the bivariate r which is the opposite of what you'd expect and is
typically a red flag. Have you run multicollinearity diagnostics?

R. As we intended to present unstandardized coefficient, Greek beta symbols have been
changed into capital Bs in Tables 2 and 3.

6) Have the authors examined for any curvilinear effects? See Allahverdipour et al.,
2010 as an example of a study showing curvilinear rather than linear correlations
between gaming and mental health.

R. We have reviewed the paper conducted by Allahverdipour et al., (2010), and found that
Table 3 was related to “curvilinear relation”. This was very useful information. We have
considered the curvilinear relations between the variables used in the present study (e.g.,
daily hours spent video gaming and depression). However, we have found this difficult since
there is no standardized criteria to classify daily hours spent video gaming. To our
knowledge, we need to categorize daily hours spent video gaming into at least three classes
(dummy variable). In addition, we are a bit concerned about whether adding a curvilinear
analysis would be really relevant to a moderated mediation which is the main aim of the
present study. Please advise us what as to your view on this.

Minor Essential Revisions:

1) When talking about links between pathological gaming and mental health
outcomes I might suggest a brief discussion of some of the conclusions of the
Ferguson, Coulson and Barnett (2011) meta-analysis on this issue in Journal of
Psychiatry Research.

R. We have mentioned the summary of a meta-analysis conducted by Ferguson, Coulson and
Reviewer 2: Sander Mattijs Eggers

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.

R. We agree with the reviewer’s point. The title has been changed into “Having mentors moderates the impact of worries and internet gaming on the development of depressive symptoms: A moderated mediation analysis”. The manuscript has been also revised accordingly.

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

R. Professional editors have checked the English.

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.

R. We have presented range including maximum and minimum, skewness and kurtosis for each variable in Table 1. The values of skewness and kurtosis appeared to be acceptable ranges (Klein, 2005). For a moderated mediation, we also used a bootstrapping method which respects non-normality.

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.

R. As the reviewer indicated, we have now reported mediation, moderated mediation, conditional indirect effects and additional analyses separately, using subheadings.

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.

R. We agree with the reviewer’s point. The sentence “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” has been removed and the introduction has been revised accordingly; we have described video gaming and internet searching as a separate variable in the introduction. Figure 1 also has thus been revised accordingly.

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

R. Please see the response below.

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.

R. We intended to describe ‘the conditional direct effect’ instead of the ‘conditional indirect effect’. This has been revised accordingly (please see the last two sentences on page 8).

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).

R. On a first step, the direct effect between worries and depression appeared to be moderated by the number of mentors or campus social network (interaction 4). However, when we examined this using a bootstrapping method, the conditional 95% CI at all ranges of moderators included zero, thus the hypothesis that the relationship between number of worries and depression was moderated by the number of mentors or the number of social networks was finally rejected. To our knowledge, to satisfy the hypothesis that the direct effect of worries and depression would be moderated by the number of mentors or social
networks, both interaction and conditional direct effects should be significant. However, please let us know if you have a different opinion.

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.

R. We have removed the sentence “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”. Also we have avoided using “decrease, increase, affect, influence” throughout the results and discussion.

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.

R. This is a very good point. We added your comments in the discussion and highlighted them in yellow.

Minor revisions:

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

R. ‘IMing’ has been changed 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.

R. As per the reviewer’s advice, we have consistently used “video gaming” instead of “internet gaming”.
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.

R. The Johnson and Neyman Technique has been added in the text.

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.

R. The sentences the reviewer indicated have been removed from the document.

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

R. This has been updated.

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.

R. alpha coefficient has been changed into ‘the Cronbach’s alpha coefficient”. The reference [44] for the final sentence has been added.

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?).

R. We have added the actual question and the unit used to record the answers. (page 6).

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

R. ‘two mediators’ has been changed to “three mediators” (page 6).
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.

R. To examine a significant effect of one moderator (e.g., mentors), the other moderator (e.g., campus social networks) was controlled in the proposed model, and vice versa in the other model. Here the authors thought that both mentors and campus social networks might play a role as moderators and that the two variables needed to be inserted simultaneously in one model, rather than being controlled. Although including the sum or average of these two variables are an option, we believed an eigenvector may represent our data better than the sum or average. As you know well, an eigenvector is a direction and an eigenvalue is a number, telling us how much variance there is in the data in that direction. In other words, the eigenvalue is a number telling us how spread out the data is on a certain line. That’s why we would like to perform a principal component analysis for dimension reduction and accept the principal component as a moderator which is the eigenvector with the highest eigenvalue. We can not find the reference which explain how an eigenvalue can be used for the purpose of assessing moderation, but we can find a clinical trial which used 3 eigenvalues, instead of 10 psychopathological variables, for assessment of a treatment effect in major depressive disorder. (Vrieze et al. Dimensions in major depressive disorder and their relevance for treatment outcome. Journal of Affective Disorders 155(2014) 35-41)

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.

R. As the reviewer indicated, we have now reported the values of number of mentors or social networks in Tables 2 and 3.

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.

R, we have now reported range, skewness and kurtosis values for each variable in Table 1. Please see also answer to 3) for Major changes
12. Please provide a reference, if applicable, when citing the ‘conditions to demonstrate moderated mediation’ at the end of the statistical analysis section.

R. The reference has been updated on page 6.

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.

R. We have updated range including maximum and minimum for each variable in Table 1.

14. In Table 1, please change the footnote to the more commonly used description:

*** p < .001, ** p < .01, * p < .05

R. The footnote describing p values have been changed in Table 1.

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).

R. We have now reported mediation and moderated mediation separately. As the reviewer indicated, interaction terms have been removed from the results of the mediation analysis.

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’.

R. The last paragraph staring with “Finally, given that the nature of cross section data~” has been moved into 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.

R. Greek beta symbols have been changed to capital B.