To the Editor,

I am submitting a manuscript titled “Emotional information-processing correlates of mental health in adolescence: A network analysis approach”. I confirm that this manuscript is not currently under review in any other journal. A preprint for the manuscript has been uploaded and can be found at <https://psyarxiv.com/8ygnh>.

This manuscript was previously under review at Clinical Psychological Science. In the last stage of review, the original reviewer two was complementary of our paper, the analyses, and our careful discussion of the results – with no additional comments to address other than a typographical error. Reviewer 3 was added during the second round of review (as reviewer 1 declined to review for a second round), and had a different impression of the paper than reviewer 2. Below I have copied in the reviewers comments, and I describe the revisions we have made to the manuscript.

As additional context, it might be worth mentioning that our original submission was made in 2017, with our first round of revisions submitted late 2019. The time delay was due to two reasons: first, the lead author was submitting his thesis; second, we were made aware of moderated network approaches, necessitating a larger rewrite of the analyses and manuscript. During this time we finished data collection for waves 2 and 3 of the CogBIAS project. Therefore, we appreciate reviewer 3’s comments on using the full longitudinal sample – which we intend to do in future analyses. Yet, for continuity with this manuscript we retained the original question, i.e. does mental health moderate the network structure of emotional cognitive biases?

The action editor for our Clinical Psychological Science confirmed that they are happy for us to share the reviewers’ comments in order to assist you with your decision. We have commented on each reviewer comment in the hope that it will assist with your reviews on this manuscript – we are also happy for this to be forwarded to reviewers.

Reviewer: 2  
  
Comments to the Author  
The paper reads well. The authors have done a thorough job of utilizing existing methods to assess the stability. Of these network estimates. I like that this paper utilizes two types of networks to assess the same question, and since the resulting conclusions from each seem very similar (e.g. sparser network for high mental health vs low) it gives me more confidence in the results despite the sample size issues. The authors are also careful in the discussion to not over interpret relations as causal.  
  
Only question is why is the same age's mean/sd NA? Its reported twice that way, is that an error?

This was a small coding error, we have fixed this.

Reviewer: 3

1. Perhaps most importantly, authors have the opportunity to learn something quite interesting about how cognitive biases interact with each other and individual inter-acting symptoms, over time, over the course of adolescence. The Booth et al longitudinal study data permit such a secondary analysis and set of tests. I believe that such a study would be a quite innovative and potentially important contribution to this literature. [Assuming authors can demonstrate that the measures of cognitive biases that they analyze have reasonable reliability to justify their use as nodes in a network.]

Authors refer a number of times to a longitudinal study (Booth et al). Authors write, “This study is the only one known to the authors to incorporate a longitudinal design, with a range of cognitive biases measured at three time-points in an adolescent sample.” Later in the paper, authors note, “Moving forward, we will be able to utilise the full three waves of data in the CogBIAS longitudinal study (Booth et al., 2017).” Yet, as best I understood, authors analyze one (cross-sectional) wave of a 3-wave study – essentially, a data analytically sophisticated partial correlation matrix of cross-sectional data of two questionnaires and one task. It was not clear why authors did not already analyze the data from waves 2 and 3 - even if only to replicate the network over multiple cross-sectional analyses? Furthermore, in light of the potential to generate not only more interesting questions about how these processes actually interact over time and contribute to mental health, and vice-versa, authors could also readily examine inter-node associations between waves. There are now multiple published studies and available tools to examine networks in precisely such repeated measures data. This would then be a robust developmental psychopathology study of interest.

This manuscript was initially developed before the full longitudinal dataset was collected for the CogBIAS project. We agree with the reviewer that a longitudinal network analysis would address additional questions as a robust developmental psychopathology study of interest. Yet, we opted to complete this manuscript first – including implementing the moderated network analysis (also a very recent development). We plan to use a longitudinal network analysis in future studies, however that would address a different question from the moderation question posed in this manuscript.

We have endeavoured to make the timecourse of this manuscript (i.e. that the initial analyses were conducted before ) clearer in the manuscript itself, including in the introduction, data analysis plan, and discussing future directions.

2. I wonder whether there is a simpler (methodological/artifactual) explanation for authors’ primary observation (i.e., the finding that in the network including healthy participants – most inter-node pathways (edges) were 0/null). Was there sufficient variability to model inter-node associations after authors systematically limit variability in each measure? And was there significantly more variability in these measures among the sub-sample with elevated symptoms? If there was limited variability in node (questionnaire and task) scores in the high mental health sub-sample, couldn’t that simply explain why no inter-node connectivity was observed in that network? I didn’t find these descriptive data in the manuscript, but I may have missed them.

We have tested for differences in observed variance between groups and included this in the manuscript. We have also added a second table to the supplemental materials that includes the SDs and variances of each variable, separately for the high and low mental health groups. There was less variance in the high-MH group, but the variance was not so low that we should conclude that low connectivity is a result of low variance.

3. Similarly, I wonder if there is a simpler (methodological/artifactual) explanation for reported associations between memory and interpretation bias nodes in the high mental health problems network (i.e., when inter-node connectivity was observed). For example, authors note, “…our network does not include edges connecting non-social interpretation biases and memory biases.” Couldn’t this and similar inter-node associations be more parsimoniously explained by the shared (or unique) item content on the different measures tested? Interpretation of the findings imply that associations between the different nodes (measures) represent substantive associations (or lack thereof) between the measured processes - memory and interpretation cognitive processes. Yet, it could be that they more simply (or additionally) relate to different stimulus content.

* It is always possible that content overlap might explain some of the associations between variables.

a. It would be useful to have a list of the stimulus content for the different measures – to examine whether item-overlap (and lack thereof) may largely account for inter-node relations between measures.

* I guess this could be included as an appendix?

4. Authors report that they divided the distribution of positive mental health symptoms, and compared network connectivity among those with low and high positive mental health scores. I did not understand why authors should split the data in this way to begin with? It would seem to be more useful to conduct the moderation analyses that authors later report in the manuscript (see below) first; and, then, if robust moderation was observed, to visually illustrate that moderation using visualization of the networks by dividing the data into low, mid and high mental health scores; or using a median split (i.e., much like how a significant interaction in a regression is probed and heuristically illustrated through plotting the interaction).

We opted to report our analyses in this way for two main reasons. First, this represents the development of the research question from a more limited analysis to a more appropriate one in parallel with the methodological developments in network analyses. Our final figure plots the networks conditioned on mental health, achieving the point we think reviewer 3 was intending here. We have also included more discussion of this in the manuscript to clarify.

5. Authors may want to consider a more formal way to quantify and thereby interpret intra-network relations between nodes. Currently, authors interpret how nodes relate to one another informally/visually. Authors could instead consider applying communality/cluster extraction procedure (e.g., via exploratory graph analysis (EGA) or a spinglass algorithm (e.g., Yang, Algesheier, Tessone, 2016).

Originally this was not a focus of the paper so we mentioned the groupings of positive and negative biases more descriptively. We have now included a communality analysis using a spinglass algorithm to formalise this more clearly.

6. I did not understand whether found or report evidence of moderation. Late in the paper, authors note, “Larger samples would be needed to increase the stability of the moderation effects. Although some edges showed a relatively consistent pattern of moderation by mental health, no edge uniformly showed this moderation across resamples. Moderation effects are typically small (e.g. Haslbeck et al., 2018), which may explain the lack of stability of some moderation effects. We have endeavoured to interpret the moderation effects with some caution.” Does that mean that there was evidence of moderation but that moderation was not reliable/stable or robust; or that there was no evidence of moderation? This left me confused. What was actually found?

We have reworded some sections of the paper to make this clearer. We found evidence of moderation, figure 3 visualised the strength of the interaction effect (with CIs) as well as the proportion of resamples that showed the moderation effect – two edges showed moderation in more than 80% of the resamples. We have strived to explain that we found some evidence of moderation, and yet larger samples would be needed to add precision to the estimates.

7. I had a few questions about methods/measures selected and omitted.

a. First, authors write: “internal consistency of each of the attention bias indices (n = 448 following removal of three participants for < 70% accuracy) was below any acceptable threshold, in this sample; angry = 0.02 , 95% CI [-0.14, 0.17]; happy = 0.17 , 95% CI [0.02, 0.31]; pain = -0.07 , 95% CI [-0.22, 0.09]. These outcome measures are unsuitable for any analyses based on correlational measures and were omitted from any further analyses. For full details about the task, see Booth et al. (2019, 2017).” This is very clear and justified.

Yet, I was surprised to learn about this decision in the Results. These are secondary data from Booth et al – data already analyzed and seemingly reported. So authors knew that these data were not reliable for this secondary analysis. So I did not understand why write the intro and aims setting up a study of attention, memory and interpretation biases? In practice, the paper relates to how memory recall of self-referential emotion words are related to interpretation of social and non-social emotion words.

This comment relates to our above note on the history of this manuscript. We have amended the introduction to avoid focus on attention bias, and have dedicated minimal space to the dot-probe measure in the methods section. We opted to retain minimal information to still demonstrate why we did not analyse this data.

b. Can authors clarify how SRET-based memory recall scores reflect biases of memory per se. Did authors quantify memory bias of recalled words – include those endorsed and not endorsed? If it only the latter, then it seems that the memory bias could simply reflect self-referential relevance of endorsed stimuli, right? Authors could test (and if relevant) rule-out this alternative explanation of bias scores.

* Not sure how to test this

c. I did not understand the rationale for the authors’ focus on “positive” mental health nor use of the reported measure. “An implication of the dual continua model is that positive mental health may be characterized by distinct patterns of selective processing styles or biases, just as the “symptoms” of mental health and mental illness differ from each other. We therefore used Keyes’ Mental Health Continum (MHC; Keyes, 2009) scale which intends to index psychological, social, and cognitive wellbeing as positive mental health.” Could the authors clarify why a more common measure of symptoms was not included in the network?

* As we explain in the manuscript, our focus was on the positive side of mental health, rather than on symptoms.

Furthermore, authors may want to explain/justify why the positive mental health total scale score was used rather than the individual items/symptoms. It seems that the premise of modeling such networks is precisely to examine how individual symptoms and processes of interest (e.g., low-level cognitive processes) inter- and trans-act.

The premise may have been modelling individual items from the mental health continua measure. However, this would have been a different research question. Additionally, testing for moderation across all items from the measure would have been unwieldy in the analyses. In order to reduce the number of parameters to estimate we opted to use the summary score of the MHC.

d. I did not understand why authors could not compute an index of reliability for their memory bias score? i.e., in light of serious problems with reliability with measures of cognitive bias and near 0 reliability of attention bias in these data and other cognitive-experimental task studies of children and adolescents, this was concerning.

At the time of writing we were unsure whether the permutation approach would be applicable for a measure obtained from a subset of endorsed items that are later recalled. Since then, Parsons has added functionality to the splithalf package to achieve reliability estimates suitable for the memory bias task. We report these in the revised manuscript.

Minor Comments

1. Variables of Figure 3 would benefit from different labels – they are hard to understand (page 20)

We assume that the reviewer is referring to the y-axis labels. We have opted to retain the labels to keep consistency across the rest of the manuscript.

2. The paragraph in the discussion on network models and resilience (p. 25/26) may benefit from being related more closely to the study methods/findings or simply be omitted.

We have removed this paragraph.

3. Authors report that they omitted all participant data that was incomplete. Can authors clarify why this was done and whether these data were in fact missingness completely at random?

The analyses currently require complete data, necessitating removal of participant data that was incomplete on these measures.