Reviewer’s report

Title: Unravelling the Complex Nature of Resilience Factors and their Changes between Early and Later Adolescence

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Reviewer: Jennifer S. Cheavens

Reviewer's report:

In this study, the authors examined mean- and network-level differences in potential resilience factors between adolescents who had experienced childhood adversity (CA+) and those who had not (CA-) at ages 14 and 17. Using a relatively large sample, the authors found that CA- adolescents reported higher mean levels of resilience factors at both ages. They found evidence that three resilience factors increased and two decreased at similar rates in both groups from ages 14-17. The authors also found that the networks of resilience factors, with and without general distress, differed between CA+ and CA- groups at age 14 but not at age 17 and did not differ within groups from 14-17 years old. In general, this is a concise paper that utilizes supplemental material well to increase the transparency of their methods. The authors use appropriate and advanced network analytic methods to test their hypotheses and clearly define what these methods show (especially with the network comparisons). Some particular points could be clarified in the manuscript in order to enhance interpretability and be more consistent with the data at hand:

Although the focus of the paper is on resilience factors, many of the variables used in the analyses are actually reverse scored risk factors. There has been quite a bit written about whether resilience and risk factors are just opposite ends of the same continua or whether they are separate classes of variables. It might be at least worth discussing what qualifies something as a resilience or risk factor.

The authors provided excellent details about their variable preparation. At the same time, it would be easier for the reader to understand (and compare to the authors' previous work and future samples) if the mean level comparisons between groups and over time were based on mean values rather than factor scores. It would be most helpful if the authors would either include a rationale for using factor scores in these analyses or replace the mean-level factor score comparisons with raw mean level comparisons.

Given that the authors re-scored their RF measures to be in the same direction, what do they make of the negative edges between a not insubstantial proportion of nodes in the RF networks? The authors explicitly (and appropriately) note the potential for these results to be a result of conditioning on a collider while some of the items with more consistently negative edges (distress tolerance and expressive suppression) are parent-, rather than participant-reported. Do the authors believe these or other explanations may be relevant?

Given that the authors invoke "the proximity of CA" as a potential explanation for some of the
differences they found at age 14 vs. 17 and that participants were classified as CA+ if they had CA at any of 3 time periods, it would be helpful if the authors included the number of participants who reported CA at each of these time periods in Table 1 or in the main text.

On author lines 238-239, the authors write that "the shortest pathways in the CA+ group were significantly stronger at age 14 than at age 17" but list the DP14 = 58.33 and DP17 = 82.36. Are these DP values applied to the correct age or should they be reversed? It's my understanding that higher DP values indicate stronger direct pathways.

On author lines 250-251, the authors write that "RFs in the CA+ group seemed to reduce distress more strongly". Similarly, on author line 298, they write "CA impacts the levels of RFs at both age 14 and 17". However, these data cannot speak to such strong causal claims so this language should be revised.

On author lines 315-317, the authors do a good job being explicitly cautious about their interpretation of the potential instability of CA+ networks. It may be even more helpful to contextualize this by reminding the reader in line 315 that there were 4 interrelation changes out of X total interrelations in the CA+ group and 1 interrelation change out of Y total interrelations in the CA- group. It may also be more accurate to characterize the interpretation as "potential greater instability of RF interrelations in the CA+ network".

On author lines 333-335, the authors reflect that the direct associations between RFs and distress were stronger in the CA+ group than the CA- group. The authors write that these associations are "potentially protective". However, given that these are associations and the authors acknowledge on line 334 that the true causal direction may be the reverse (distress to RFs), this may mean that greater experiences of distress are more reliably related to lower RFs in the CA+ group than the CA- group (and, since the CA+ group reports higher distress than the CA- group, this result is more likely). The authors should consider revising these claims to be more descriptive to more accurately reflect their results.

Throughout the manuscript, the authors refer to the idea that more positively connected networks indicate that nodes can enhance each other. Given that these are between-person cross-sectional networks, I wonder how accurate this claim is. It may be helpful to stick to more descriptive language in most of the paper (e.g., "more strongly connected" than "enhancing") and/or limit the use of the idea of nodes "enhancing" each other to the discussion, where it can be linked to the use of more longitudinal data.

**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?**
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Not applicable
Are the conclusions drawn adequately supported by the data shown?
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