Reviewer's report

Title: Agreement between parents and adolescents on emotional and behavioral problems and its associated factors among Chinese school adolescents: a cross-sectional study

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Reviewer: Leslie Rescorla

Reviewer’s report:

The authors have made numerous changes to their original submission in this revision based on reviewers’ comments, all of which have improved the manuscript. The authors have also prepared a lengthy response letter, which is very helpful. However, there are still a few clarifications, corrections, and explanations that I think should be made in the manuscript before it is accepted for publication, as detailed below.

1. The authors now summarize on p. 4 some of the findings from the Rescorla et al. (2013) report of CBCL-YSR cross-informant agreement in 25 societies, which is an important addition to their literature review. As those 25 societies included China, I think the authors should report in the literature review that the mean r averaged across 17 scales was .46 for China, in addition to citing the range of correlations (.35-.60) reported in the Wang et al. (2005) for the same Chinese sample (plus 400 dyads excluded from the Rescorla et al. study due to selection criteria).

2. Although the authors mention on p. 4 that previous studies (including the 2013 Rescorla study) have reported higher problem scores for adolescent self-reports than parent reports in general population samples, they do not note that Rescorla et al. reported that YSR scores were higher than CBCL scores in all 25 societies. The informant effect sizes of 22%, 16%, and 18% for Total Problems, Internalizing, and Externalizing are much larger than those the authors report in Table 3. Therefore, on p. 14 in the Discussion when the authors comment on their YSR>CBCL finding, I think that they should cite the 2013 study and make it clear that their study replicated a finding already reported previously for 25 societies, including China. The Abstract is also misleading in this respect, because the authors state that China had a “different pattern” of agreement, followed by a sentence about adolescents reporting more problems than their parent. I would argue that the pattern of results obtained in this study is not different from that reported for the 25 societies in the Rescorla study, albeit the level of agreement is at the high end of those reported by Rescorla.

3. It is noteworthy that rs for internalizing and Externalizing were virtually identical in this study (both .61), also replicating the findings reported by Rescorla et al. (2013) (.45 and .46) but contrary to the common assumption that agreement is better for problems that are more overt.

4. In their response letter, the authors address my previous comment that they
need to state why they used the 1991 version of the CBCL and YSR, even though their data were collected in 2010 and the 2001 versions of these instruments were available in Chinese at that time. However, some of the information they provide in the response letter is incorrect. The 1991 and 2011 versions of the CBCL and YSR have the same number of items (and only 6 items were replaced). Additionally, the Wang et al. study used the 1991 version early in their data collection process but the 2001 version later in their study. As noted in my previous review, the authors need to say something about this issue in their manuscript. I suggest on p. 8 they include something like the following statement: “The current study used the 1991 versions of the CBCL and YSR because the Chinese 2001 versions of the instruments (on which six 1991 items were replaced) were not yet widely available at the time the study was conducted. Therefore, the 1991 versions of the CBCL and YSR syndromes were used in the current study, which differ slightly from the 2001 syndromes in item composition and name.”

5. As the authors have now conducted the 2 x 2 ANOVAs with informant as a within-subjects factor, reporting t-test results as well seems superfluous.

6. On p. 7, the authors imply that the purpose of the study is to help clinicians decide which instrument to use to screen for problems. Given that they state on p. 4 in the first paragraph that “both parents and adolescents are needed to obtain a comprehensive picture,” it seems odd three pages later to imply that only one instrument is needed. I suggest they revise the p. 7 sentence to state something like “providing evidence of the importance of obtaining reports from multiple informants.”

7. On p. 8, I think the authors should state that both the CBCL and YSR were sent home to be completed (i.e., the YSR was not completed at school).

8. On p. 8 and elsewhere the names of the instruments (e.g., Child Behavior Checklist) should be capitalized, as should the names of the syndrome scales (e.g., Attention Problems). This makes them easier to spot in the text, which makes the results easier to follow.

9. On p. 9, the wording of the questions posed about parental expectations needs revision. Did the question state “How high are your parents’ expectations for your learning performance?”

10. On p. 11, I suggest the authors present the actual Cohen’s d values rather than the mean difference and SD. They should also indicate that the d values are quite small, based on Cohen (1988).

11. On p. 12, line 9, I suggest the authors remind readers that they are reporting adolescent reports of their parents’ expectations. I also suggest they inform readers whether the FES means are low, high, or average, based on samples in the US or other countries.

12. On p. 14, the authors suggest that parents underestimate adolescents’ problems. However, as the Rescorla et al. (2013) study clearly indicates, the mean YSR>CBCL discrepancy, although a highly consistent finding across societies, fails to capture the fact that in many dyad CBCL scores are higher than YSR scores. The categorical agreement results reported for 25 societies clearly
indicate that when “deviance” is analyzed (i.e., scores above a cutpoint), it is about equally common for parental reports of deviance to not be corroborated by adolescents as for adolescent reports of deviance to not be corroborated by parents. This is an important point that the authors should address on p. 14, if not earlier.

13. In their response letter, the authors make some interesting points about why YSR-CBCL agreement might be higher in China than in Hong Kong. However, the mean rs were very similar in the Wang Beijing and Leung Hong Kong samples, as reported by Rescorla et al. (2013). Furthermore, one might expect the rapid economic changes in China in the past decade (plus more Western cultural influence) would presumably have served to make China more similar to Hong Kong rather than less so, suggesting that data for the current study, collected about a decade later than the Wang data, would have lower correlations, but in fact they are higher. I think the authors should address these cultural issues in a more nuanced way in their Discussion. For example, perhaps agreement in the Liao Ning region of China is different from that in Beijing? Furthermore, among the largest rs in the Rescorla study were those for Denmark, clearly a Western country. The authors note in their response letter that Bilenberg’s Danish monograph included both clinical and general population samples, but the Danish sample in the Rescorla study only used the general population data. Therefore, the large rs for Denmark are not due to inclusion of clinical samples. Additionally, Algeria had quite large YSR-CBCL rs, as the authors note. The East vs. West dichotomy does not seem to fit well to characterize Algeria, which is clearly very different from China and had many centuries of French colonization. In short, I think the authors are making too simplistic an argument about Eastern vs. Western cultures and their manuscript would be improved by a more considered treatment of this important issue. The interesting data they report on family factors suggest one way they might approach this question. That is, whether within or between societies, in families with more closeness, cohesion, and communication, parent-adolescent agreement is likely to be higher.

Overall, this study has the potential to be a good addition to the literature on parent-adolescent agreement, but I would like to see the issues summarized above addressed before it is accepted for publication.

**Level of interest:** An article of importance in its field

**Quality of written English:** Needs some language corrections before being published

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

**Declaration of competing interests:**

I receive royalties for CBCL and YSR English-language products.