

## LETTER OF REVIEWERS

Reviewer A:

Recommendation: Revisions Required

**Relevance:** Moderated

**Novelty:** Moderated

**Presentation and writing:** Moderated

### Comments for authors:

#### ABSTRACT

1. The reported p-value in the  $\chi^2$  test appears as “p = 004”, which seems to be a typographical error. It should be corrected to “p = .004”.
2. It is recommended to remove "Peru" from the title and instead specify the setting in the abstract.

#### INTRODUCTION

3. There is substantial repetition in the discussion of the historical role of mothers as primary caregivers (“Historically, the responsibility of caring...”), which affects the overall clarity and flow of the text.
4. While the manuscript notes that “there is a lack of studies assessing the psychometric properties of the instrument in samples of Peruvian mothers,” this statement should be strengthened by critically explaining why previous validations (e.g., unpublished theses) are insufficient from a scientific perspective.

#### METHOD

5. The manuscript would benefit from a "setting" section. For readers unfamiliar with the Peruvian context, it is unclear what distinguishes Arequipa and Puno, such as maternal literacy rates, income levels, or linguistic diversity. This information would provide a relevant cultural and contextual background.
6. No sample adequacy analysis is reported (e.g., number of participants, power analysis for CFA). It is recommended to use a statistical power calculator for CFA, such as [wnarfin.github.io/ssc\\_web.html](http://wnarfin.github.io/ssc_web.html). Informal rules like “10 participants per item” should be avoided.
7. The authors report that “42.7% had intellectual disabilities, 29.7% had Autism Spectrum Disorders, and 15.3% had multiple disabilities.” However, it is unclear how these data were obtained—whether they are self-reported, based on clinical diagnoses by pediatric psychiatrists, or derived using another criterion.
8. For the instruments, it should be specified whether any linguistic or cultural adaptation was performed, even if the language remains the same.
9. The conceptual justification for including both positive and negative factors within a single construct of resilience should be provided, as this may raise interpretive challenges.
10. It is unclear whether the model is one of correlated dimensions, a second-order factor model, or another structural model.
11. The handling of missing data is not reported. Even with incidental sampling, missingness mechanisms may be present. Were data tested for MCAR (Missing Completely at Random), MAR (Missing At Random), or MNAR (Missing Not At Random)?
12. It is unclear why DWLS was used instead of WLSMV, which is generally considered a more robust estimator.
13. Only two types of internal validity evidence are presented (content and internal structure). It would strengthen the paper to include group invariance testing or a multimethod design, such as IRT analysis and evaluation of potential DIF. This would enhance the robustness of the study by incorporating external sources of validity.

#### RESULTS

14. The results are clearly presented but overly descriptive. Brief interpretative comments should be added to help the reader understand the relevance of the reported indices.
15. Consider switching from DWLS to WLSMV. This is important because DWLS is generally more permissive than

WLSMV.

16. Items eliminated based on Aiken's V are mentioned, but the conceptual impact of their exclusion on the instrument's original structure is not discussed. This is important, as the construct may now be only partially assessed.

17. It is strongly recommended to perform a three-parameter IRT analysis and group invariance testing.

#### DISCUSSION

18. The discussion section tends to reiterate findings without critically evaluating them. It is recommended to compare results with similar international studies and explicitly discuss the theoretical implications of retaining factors with marginal internal consistency.

19. The potential of this instrument to inform clinical or public health interventions should be discussed.

20. The limitations section is underdeveloped. It should at least include limitations related to the cross-sectional design, self-selection bias, and cultural and regional constraints. A major limitation is the lack of evidence for external validity.

21. The conclusion should adopt a more cautious tone and acknowledge that the results provide only preliminary support for the instrument's validity, especially in light of the methodological limitations identified.

**Interacciones seeks greater transparency in the review process and to provide credit to reviewers. If the editors decide to accept the manuscript, would you like your name to appear as a reviewer of the article?**

No

## RESPONSE LETTER

Through this letter, the authors of this manuscript report that the reviewer's comments have been corrected or refuted as detailed below:

Comment 1: The typographical error in the p-value was corrected in both sections of the abstract (English and Spanish) and in the result section (Abstract section, page 1 and 2 and result section, paragraph 1, page 10).

Comment 2: It was removed “Perú” from the title and the specification within the summary was highlighted in yellow (“Peruvian mother”) (Title and abstract section, page 1).

Comment 3: The paragraphs related to the historical context of primary caregivers were removed (Introduction section, paragraph 5, page 4) because they were redundant with information that had already been mentioned in previous paragraphs (Introduction section, paragraph 1, page 3).

Comment 4: The explanation of why previous validations are insufficient from a scientific perspective was expanded (Introduction section, paragraph 8, page 4).

Comment 5: It is considered unnecessary to include a “setting” section because the study does not seek to compare the two regions; consequently, no invariance analysis was performed.

Comment 6: A post hoc statistical power analysis using Arifin’s calculator was added (Method: Participants section, paragraph 2, page 5). Also, the reference was added.

Comment 7: It was added that the characteristics of the sample (mothers and children with disabilities) were based on the mothers' self-reports (Method: Participants section, paragraph 1, page 5).

Comment 8: It was added information about the cultural adaptation of items that was performed (Method: Instruments section, paragraph 2, page 6).

Comment 9: The conceptual justification for including both positive and negative factors within a single construct of resilience was provided in the introduction section (paragraph 3, page 3).

Comment 10: It was specified that the model is one of six correlated dimensions (Method: Instruments section, paragraph 2, page 5 and Results section, paragraph 3, page 10).

Comment 11: The report of the handling of missing data has been added (Method: Procedure section, paragraph 2, page 6).

Comment 12 and 15: It was switched from DWLS to WLSMV and there were no changes in factor loads and the fit indexes (Method: Data analysis section, paragraph 2, page 7 and Results section, paragraph 2, page 8).

Comment 13 and 17: Additional analyses could not be performed due to the small sample size. For an Item Response Theory (IRT) analysis, a minimum of 500 participants is recommended. Meanwhile, for a DIF analysis, it is suggested that each compared group has at least 200 subjects. This greatly exceeds the sample size of the present study.

Comment 14: A brief interpretative comment in the results was added (Results section, paragraph 4, page 11). However, the interpretation of all results can be found in the discussion section.

Comment 16: It was added the reasons for the removal of two items and the conceptual impact of their exclusion on the instrument’s original structure (Results section, paragraph 1, page 7).

Comment 18: The comparison of the study results with international studies was expanded. It should be noted that due to the scarcity of psychometric studies on this variable, the discussion was limited to the comparison with the

original study by Roque et al. (2009) and the study by Lozano and Romero (2022) (Discussion section, paragraph 1-4, pages 12-13).

Comment 19: Information was added on the importance of the study instrument, especially in the field of educational intervention with mothers to improve the well-being of their children with disabilities and their own (Discussion section, paragraph 5, pages 12-13).

Comment 20: The limitations section was expanded considering the comments provided (Discussion: limitations section, paragraph 6, page 13).

Comment 21: The wording of the conclusion was corrected to adopt a more cautious tone in light of the methodological limitations identified and the results of the study (Discussion: conclusion section, paragraph 7, page 13-14).

Additionally, a typographical error in the TLI value was corrected in all sections where its value is reported (abstract, summary, and in the third paragraph of results).

Without further ado, we send you our warmest regards.

Sincerely,

The authors of the attached manuscript.