






Co-Parenting Quality and Parenting Stress as Predictors of Child Behavioral Adjustment

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E d i t o r	R e v i e w e r s
Eman Tadros  Family Department, Governors State University, University Park, Illinois, USA emantadros@gmail.com	Reviewer 1: Hooman Namvar  Assistant Professor, Department of Psychology, Saveh Branch, Islamic Azad University, Saveh, Iran. Email: hnamvar@iau-saveh.ac.ir Reviewer 2: Fahime Bahonar  Department of counseling, Universty of Isfahan, Isfahan, Iran. Email: Fahime.bahonar@edu.ui.ac.ir

1. Round 1

1.1. Reviewer 1

Reviewer:

The Introduction's second paragraph discusses father involvement but lacks sufficient detail about how this variable intersects with co-parenting. Including a brief sentence that clarifies whether father involvement is measured separately or implied within the CRS would help.

In the Introduction, the final paragraph mentions "empirical evidence from North African contexts such as Morocco remains scarce." Consider expanding this with one to two references showing the scarcity or listing policy-relevant reasons why Moroccan data are essential.

The description of the Co-Parenting Relationship Scale (CRS) is strong, but the sentence "Responses are provided on a 7-point Likert scale..." could benefit from a statement on how total scores are computed (e.g., sum vs. average).

In the Data Analysis section, the sentence "To examine the bivariate associations..." should clarify whether variables were tested for outliers or extreme values, especially since regression was later conducted.

The Findings section reports demographic distributions, but it would be helpful to note if there were any significant demographic differences in the independent or dependent variables (e.g., urban vs. rural).

In the Discussion, the sentence "parenting stress emerged as the stronger predictor" is accurate but could be analytically expanded by discussing the beta difference (.48 vs. -.31) in psychological and clinical terms.

In the paragraph beginning “Interestingly, the findings also reinforce...,” you suggest interaction effects without testing them statistically. A suggestion should be added that future studies employ moderation analyses to explore this possibility empirically.

The paragraph discussing Morocco’s collectivist culture notes extended family involvement but doesn’t specify if this was measured or controlled for. Consider noting this as a limitation or suggesting it for future study.

In the paragraph citing affective computing and real-time behavioral data (Chen et al., 2023; MacNeill et al., 2021), there is a mismatch between the high-tech methodologies referenced and the self-report tools used in this study. A brief reflective note on methodological limitations may strengthen the discussion’s balance.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

In the Methods section, the sentence “Participants were eligible... co-parenting arrangement...” should clarify whether co-parenting was self-reported or operationalized using a specific cutoff or definition from the CRS scale.

The Measures section describes the CBCL but uses the term “child behavioral adjustment,” which does not align with the CBCL’s original terminology of “problem behaviors.” Clarify this discrepancy or justify the re-labeling.

In the Measures section on the PSI-SF, the phrase “level of stress parents experience in the parenting role” is vague. Consider specifying whether this refers to emotional, cognitive, or behavioral aspects of stress as captured by the instrument.

In Table 1’s interpretive paragraph, the sentence “reflecting generally positive co-parenting experiences among participants” is interpretive. You should justify what range constitutes “positive” based on normative CRS data or previous studies.

In the Findings section discussing assumptions for regression, the sentence “no pattern was detected” regarding residuals could be supported by a mention of which type of residual plot was used (e.g., standardized vs. studentized).

In Table 2’s description, the correlation between parenting stress and co-parenting quality is significant ($r = -.47$), but this relationship is not fully unpacked in the Discussion. Consider elaborating on this bidirectional relationship more explicitly.

The regression model in Table 3 shows $R^2 = .40$. The Discussion should address whether this level of explained variance is typical in family psychology research and provide a comparative benchmark.

Response: Revised and uploaded the manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.