

LETTER OF REVIEWERS

Reviewer B:

Recommendation: Decline Submission

Relevance: Low or very low

Novelty: Moderate

Presentation and writing: Low or very low

Comments for authors: Please be as precise as possible when addressing these comments. Each recommendation is listed separately so that the authors can respond appropriately. Where possible, please indicate the specific section or paragraph in which changes should be made.

General observations on the article

"The Influence of Intrafamily Violence and Child-to-Parent Violence on the Mental Health of Adolescents in the Peruvian Amazon: The Mediating Role of Empathy"

Introduction

1. The introduction does not present a clear argumentative line. Rather than developing a coherent rationale, it reads as an accumulation of ideas. There is no strong logical progression leading to the proposed mediation model. The manuscript shows that all variables are related to one another, but it does not construct a coherent theoretical argument.
2. The proposed theoretical justification is weak and, to some extent, contradictory. The manuscript mentions that empathy may increase in contexts of violence, but later states that empathy may reduce or affect mental health. This is an interesting idea, but it is not adequately articulated as a mechanism or process. Without a clearer explanation, the proposed model feels forced.
3. The references are recent, which is a strength. However, they are used mainly as descriptive support rather than as part of a theoretical construction that develops the proposed model. There is little discussion of debates, contradictions, or tensions among authors. As currently written, the literature appears to be presented as if all findings were coherent and aligned with the study's aims.
4. The research gap is not clearly established. Although the importance of the study is emphasized, the manuscript does not sufficiently demonstrate the absence of previous models integrating these variables. The relevance of the Amazonian context is also weakly developed. The manuscript should highlight why this context is particularly appropriate for the study, including cultural, contextual, and risk-related factors. National records, census data, or official reports could be considered to strengthen this justification.
5. The definitions provided are too basic and resemble textbook definitions. They do not include critical perspectives or functional elements that contribute meaningfully to the proposed model.

Hypotheses

1. The hypotheses are too general. A mediation model requires a clear directionality supported by an explicit conceptual foundation. Hypotheses should predict expected relationships, not merely explore what may emerge from the data. They should be coherent with the reviewed literature, since the results will later be interpreted in light of these hypotheses.
2. The hypotheses should not be overgeneralized. The expected direction of the effects is missing, for example, whether the mediation is expected to be positive or negative. The manuscript does not propose a real model, but rather an idealized one.
3. As noted above, the manuscript suggests that everything affects everything, which reduces its scientific contribution.
4. The causal interpretation implied by the model is not sufficiently supported by the methodological design or the theoretical framework, which weakens the overall rationale of the study.

Methodology

1. The study design should be reviewed and its use should be better justified.
2. In the participants section, there appears to be an important irregularity, possibly due to a typographical error or a mistake in data analysis. Specifically, the reported mean age of 1.10 is impossible, especially as it is lower than the standard deviation.

3. There is an inconsistency regarding psychological treatment. The text reports that 72.5% had received treatment, whereas the table indicates that 27.5% were also receiving treatment. This discrepancy should be clarified.
4. The criterion of living with both parents should be better justified. It is unclear why adolescents from single-parent, separated, or other family structures were not considered. This may introduce bias, particularly because violence may also occur in these family contexts. As currently presented, the phenomenon may be underestimated.

Results

1. The reliability of some variables is low. This issue should be explicitly acknowledged as a limitation and discussed with appropriate argumentation.
2. The skewness values should be explained so that readers can better understand the nature of the results obtained.
3. Other descriptive results should also be explained, rather than merely reported numerically. The same applies to the correlations.
4. The preselection of paths does not seem appropriate. The full model should be presented, since some paths may show relevant results when analyzed within the system. The model should be tested as a whole rather than eliminating paths based on previous analyses conducted outside the complete model.
5. Mediation may exist even in the absence of a significant direct effect. Therefore, the decision to remove or retain paths should be justified not only statistically but also theoretically.
6. Regarding the fit indices, describing a CFI of 0.932 as indicating "good fit" may not be appropriate. Consider revising the wording and describing this result as acceptable, given its proximity to 0.90.
7. The RMSEA is not reported, which is inconsistent with the presentation of the model. This index is relevant because simulation studies and theoretical considerations suggest that it is important for evaluating how the proposed model performs.
8. The confidence interval is close to zero, suggesting a weak effect or possible instability in the model. This weakens the claim of statistical significance made in the manuscript.
9. The manuscript presents two models, A and B, without adequately justifying why both were included.

Discussion

1. To improve coherence with the stated hypotheses, the discussion should explicitly present each result in relation to each hypothesis. As currently written, some paragraphs are confusing with respect to the paths and results presented.
2. The omission of non-significant results is not appropriate. These findings should be critically discussed. The discussion should not only confirm the hypotheses but also explain the model as a whole.
3. Regarding the mediating role of empathy, the manuscript should better explain the distinction between affective and cognitive empathy, supported by studies that indicate which dimension may predominate. Since these dimensions were not analyzed independently, the authors should provide a stronger argument rather than merging them without distinction.
4. The weakness of the effects or paths is not discussed.
5. The manuscript does not mention important limitations, including the low reliability coefficients and the sampling bias related to family structure, such as living with both parents.
6. The elimination of paths should be better justified.
7. The overall discussion feels superficial. There is little contrast, debate, or engagement with contradictory evidence. The discussion should not only confirm the study findings but also question and qualify them in relation to previous research.
8. The Amazonian context is underused. The manuscript does not adequately explain or integrate cultural, contextual, or regional factors. As a result, the initial emphasis on this context is not sufficiently reflected in the final interpretation of the findings.
9. The results should not be overgeneralized, given the number and relevance of the limitations identified.
10. The limitations mentioned throughout the review should be explicitly included in the manuscript.

Reviewer C:

Recommendation: Revisions Required

Relevance: High

Novelty: High

Presentation and writing: Moderate

Comments for authors: Please be as precise as possible when addressing these comments. Each recommendation is listed separately so that the authors can respond appropriately. Where possible, please indicate the specific section or paragraph in which changes should be made.

1. Abstract, final line of the Methods section: Explicitly state that the sample came from public schools in Tarapoto, Peru, to provide greater transparency and contextualization. Also, be cautious when stating that the study represents adolescents from the Peruvian Amazon, since the sampling was non-probabilistic and intentional, involving a group of adolescents from public schools in one city of the Peruvian Amazon.
2. Introduction, final paragraph: Strengthen the originality of the study by emphasizing that it focuses on school-attending adolescents from a city in the Peruvian Amazon, a population that remains underexplored in the scientific literature.
3. Theoretical framework, paragraph on H2: Better justify the hypothesis regarding the relationship between child-to-parent violence and empathy, since the evidence cited focuses more strongly on domestic violence.
4. Methodology, paragraph 2: Correct the description of the sample, clarifying that participants were students from public schools in Tarapoto. Also, review the typographical error in the reported age values: $M = 11.10$; $SD = 13.5$.
5. Methodology, instruments, R-MHI-5: Indicate that the RMSEA = 0.101 reflects mediocre fit and discuss this issue as a limitation.
6. Results, Table 2: Discuss the low reliability of the intrafamily violence measure, $\alpha = 0.60$, as a methodological limitation.
7. Results, SEM: Indicate that the CFI = 0.932 suggests acceptable, but not excellent, model fit, and discuss this point in the limitations section.
8. Discussion, paragraph on child-to-parent violence and empathy: Highlight the absence of a significant relationship between child-to-parent violence and empathy as a novel finding, and propose possible cultural or methodological explanations.
9. Discussion, paragraph on empathy and mental health: Expand the reflection on the paradox of empathy, which may be protective in some contexts but may also increase vulnerability in violent environments.
10. Conclusions, final paragraph: Include more specific practical recommendations for school-based programs targeting students in the Peruvian Amazon. Emphasize the need to strengthen both empathy and emotional regulation.
11. Studies conducted in Paraguay have examined the association between violence and mental health disorders among school-attending adolescents and adolescent women. These studies may be relevant for the discussion.

RESPONSE LETTER

To the Editorial Board of *Interacciones*

Re: Manuscript ID-539

Title: "The Influence of Intrafamily Violence and Child-to-Parent Violence on the Mental Health of Adolescents in the Peruvian Amazon: The Mediating Role of Empathy"

Authors: Esther Villanueva-Barboza and Ronald Gianmarco Silva-Fernandez

Dear Editor and Reviewers,

We sincerely thank the Editor and Reviewers B and C for their careful, detailed, and constructive evaluation of our manuscript. Their observations allowed us to substantially strengthen the theoretical rationale, methodological transparency, interpretation of the structural model, and contextualization of the findings. We have revised the Introduction, hypotheses, Methodology, Results, Discussion, Limitations, and Conclusions. In particular, we clarified the observational and cross-sectional nature of the study, strengthened the theoretical role of empathy, tested and reported the complete structural model, corrected reporting inconsistencies, moderated interpretation of effect sizes and fit indices, expanded the Amazonian context, and restricted the scope of generalization to the population actually studied.

Below, we provide a point-by-point response to every comment. The reviewers' original comments are reproduced in bold, followed by our responses. The revised manuscript identifies the corresponding changes in the sections indicated below.

Response to Reviewer B

Introduction

Comment B-11: "The introduction does not present a clear argumentative line. Rather than developing a coherent rationale, it reads as an accumulation of ideas. There is no strong logical progression leading to the proposed mediation model. The manuscript shows that all variables are related to one another, but it does not construct a coherent theoretical argument."

Response: We fully agree that the previous version did not make the progression toward the mediation model sufficiently explicit. We therefore restructured the Introduction so that the argument now moves sequentially from adolescent mental-health problems, to intrafamily and child-to-parent violence as relevant family risk processes, to the need to identify psychological mechanisms, to empathy as a plausible socioemotional mechanism, and finally to the proposed mediation model and hypotheses. Redundant descriptive material was removed and conceptual transitions were added so that each paragraph contributes directly to the rationale for the model. *Changes made: Introduction, paragraphs 1-8.*

Comment B-12: "The proposed theoretical justification is weak and, to some extent, contradictory. The manuscript mentions that empathy may increase in contexts of violence, but later states that empathy may reduce or affect mental health. This is an interesting idea, but it is not adequately articulated as a mechanism or process. Without a clearer explanation, the proposed model feels forced."

Response: We appreciate this important observation. We substantially strengthened the mechanism linking violence, empathy, and mental health. The revised text now distinguishes empathy as a multidimensional process rather than treating it as uniformly protective. Exposure to interpersonal violence may heighten emotional monitoring and sensitivity to others; when this process is dominated by affective overactivation or empathic distress, greater sensitivity can coexist with greater psychological vulnerability. We also distinguish this process from cognitive empathy, which may be more consistently associated with adaptive interpersonal functioning. This clarification makes explicit why increased empathic responding in violent contexts can plausibly operate as a pathway associated with poorer mental-health outcomes rather than as a contradiction in the model. *Changes made: Introduction, paragraphs 6-9; Discussion, empathy and mental-health subsection.*

Comment B-13: "The references are recent, which is a strength. However, they are used mainly as descriptive support rather than as part of a theoretical construction that develops the proposed model. There is little discussion of debates, contradictions, or tensions among authors. As currently written, the literature appears to be presented as if all findings were coherent and aligned with the study's aims."

Response: We agree. The literature review was revised so that citations are used to build the theoretical argument rather than simply document pairwise associations. We now contrast evidence that conceptualizes empathy as protective with studies reporting that high affective empathy, empathic distress, or interpersonal hypersensitivity can be associated with internalizing symptoms, particularly under adversity. We also explicitly discuss convergent and divergent findings regarding family violence and socioemotional functioning. This produces a more critical theoretical synthesis and makes the tensions in the literature visible. *Changes made: Introduction and Discussion, particularly the sections on empathy and mental health.*

Comment B-14: "The research gap is not clearly established. Although the importance of the study is emphasized, the manuscript does not sufficiently demonstrate the absence of previous models integrating these variables. The relevance of the Amazonian context is also weakly developed. The manuscript should highlight why this context is particularly appropriate for the study, including cultural, contextual, and risk-related factors. National records, census data, or official reports could be considered to strengthen this justification."

Response: We agree that the research gap and the relevance of the setting needed to be much more explicit. The revised Introduction now emphasizes the scarcity of studies that simultaneously examine intrafamily violence, child-to-parent violence, empathy, and mental health within a single explanatory model, as well as the limited evidence from Amazonian Latin American settings. We also expanded the contextual rationale by discussing structural and sociocultural conditions relevant to adolescents in the Peruvian Amazon, including psychosocial vulnerability, family stressors, multidimensional disadvantage, and limited access to mental-health services. The setting is therefore presented as a meaningful sociocultural context rather than merely a geographic label. *Changes made: Introduction, paragraphs 8-10.*

Comment B-15: "The definitions provided are too basic and resemble textbook definitions. They do not include critical perspectives or functional elements that contribute meaningfully to the proposed model."

Response: We agree. Basic textbook-style definitions were removed or rewritten. The revised Introduction now defines the central constructs only to the extent necessary to clarify their function in the proposed model, emphasizing mechanisms, conceptual distinctions, and their relevance to adolescent development and family violence. *Changes made: Introduction throughout.*

Hypotheses

Comment B-H1: "The hypotheses are too general. A mediation model requires a clear directionality supported by an explicit conceptual foundation. Hypotheses should predict expected relationships, not merely explore what may emerge from the data. They should be coherent with the reviewed literature, since the results will later be interpreted in light of these hypotheses."

Response: We agree and have completely reformulated the hypotheses. Each hypothesis now specifies the expected direction of the association and is anchored in the revised theoretical framework. The model is presented as a prespecified set of theoretically motivated relationships rather than an exploratory search for any association among the variables. *Changes made: Hypotheses section.*

Comment B-H2: "The hypotheses should not be overgeneralized. The expected direction of the effects is missing, for example, whether the mediation is expected to be positive or negative. The manuscript does not propose a real model, but rather an idealized one."

Response: We have added explicit directional expectations for the direct and indirect relationships. In particular, the mediation hypothesis now specifies the expected direction of the indirect pathway from intrafamily violence through empathy to mental health, instead of referring generically to mediation. This revision aligns the hypotheses with the mechanisms developed in the Introduction. *Changes made: Hypotheses section.*

Comment B-H3: "As noted above, the manuscript suggests that everything affects everything, which reduces its scientific contribution."

Response: We agree that the previous symmetrical formulation reduced parsimony. The revised model no longer assumes that every construct necessarily predicts every other construct. We now distinguish the theoretically supported pathways and explicitly treat the child-to-parent violence-empathy association as a relationship for which no significant association was expected, thereby avoiding an artificial all-to-all structure. *Changes made: Hypotheses and theoretical framework.*

Comment B-H4: "The causal interpretation implied by the model is not sufficiently supported by the methodological design or the theoretical framework, which weakens the overall rationale of the study."

Response: We fully agree that the design does not support causal inference. The revised manuscript explicitly describes the study as nonexperimental and cross-sectional, and causal wording has been removed or qualified throughout the hypotheses, Results, Discussion, and Conclusions. The SEM is used to estimate a theoretically specified pattern of direct and indirect associations; it is not presented as evidence that temporal or causal effects have been established. *Changes made: Study Design, Hypotheses, Discussion, and Conclusions.*

Methodology

Comment B-M1: "The study design should be reviewed and its use should be better justified."

Response: We revised the description and justification of the design. The study is now explicitly described as nonexperimental, cross-sectional, and explanatory-predictive in scope, conducted to examine associations

among psychological variables in a natural setting without manipulation. We also added a methodological rationale for SEM, emphasizing its usefulness for estimating multiple direct and indirect relationships simultaneously while clearly noting that, in a cross-sectional observational design, these paths should not be interpreted as causal effects. *Changes made: Methodology, Study Design and Data Analysis.*

Comment B-M2: "In the participants section, there appears to be an important irregularity, possibly due to a typographical error or a mistake in data analysis. Specifically, the reported mean age of 1.10 is impossible, especially as it is lower than the standard deviation."

Response: Thank you for identifying this error. It was a typographical transposition in the report of age. We verified the descriptive statistics and corrected the manuscript to report the mean age and standard deviation as $M = 13.5$ years and $SD = 1.10$. *Changes made: Methodology, Participants.*

Comment B-M3: "There is an inconsistency regarding psychological treatment. The text reports that 72.5% had received treatment, whereas the table indicates that 27.5% were also receiving treatment. This discrepancy should be clarified."

Response: We carefully rechecked the treatment variable and corrected the inconsistency between the narrative text and the table. The revised manuscript now reports consistently that 72.5% of participants had not received psychological treatment, whereas 27.5% had previously received or were currently receiving psychological treatment. *Changes made: Methodology, Participants, and the corresponding descriptive table.*

Comment B-M4: "The criterion of living with both parents should be better justified. It is unclear why adolescents from single-parent, separated, or other family structures were not considered. This may introduce bias, particularly because violence may also occur in these family contexts. As currently presented, the phenomenon may be underestimated."

Response: We agree that this inclusion criterion required a clearer rationale. The revised Participants section explains that co-residence with both parents was used to reduce structural heterogeneity in the family system, limit contextual variation in exposure to intrafamily violence, and maintain greater comparability of the family unit assessed. In addition, the intrafamily and child-to-parent violence measures used in the study were validated for adolescents living with both parents. Importantly, we now explicitly acknowledge that this criterion may introduce selection bias, restrict generalizability, and potentially underestimate family violence among adolescents living in single-parent, reconstituted, separated, or substitute-care arrangements. *Changes made: Methodology, inclusion criteria; Limitations.*

Results

Comment B-R1: "The reliability of some variables is low. This issue should be explicitly acknowledged as a limitation and discussed with appropriate argumentation."

Response: We agree. The revised manuscript explicitly acknowledges the moderate or low internal-consistency estimates of several measures, including intrafamily violence ($\alpha = .60$; $\omega = .62$), child-to-parent violence ($\alpha = .65$; $\omega = .66$), and psychological well-being ($\alpha = .67$; $\omega = .67$). We discuss the possibility that measurement error may have attenuated observed associations and therefore interpret the corresponding path estimates cautiously. *Changes made: Results and Limitations.*

Comment B-R2: "The skewness values should be explained so that readers can better understand the nature of the results obtained."

Response: We added an interpretation of the skewness statistics rather than leaving them as isolated numerical values. The revised text explains that the observed values did not indicate severe departures from distributional symmetry for the variables analyzed and were considered together with the other assumptions used for the structural analysis. *Changes made: Results, descriptive statistics.*

Comment B-R3: "Other descriptive results should also be explained, rather than merely reported numerically. The same applies to the correlations."

Response: We agree. The descriptive and correlational Results were rewritten to identify the substantive patterns in the data, including the direction and relative magnitude of associations and their correspondence with the theoretical model, instead of merely repeating table values. *Changes made: Results, descriptive and correlational analyses.*

Comment B-R4: "The preselection of paths does not seem appropriate. The full model should be presented, since some paths may show relevant results when analyzed within the system. The model should be tested as a whole rather than eliminating paths based on previous analyses conducted outside the complete model."

Response: We agree with the reviewer. The structural analysis was revised so that the full theoretically specified model is estimated as a system. We no longer justify removal of paths solely on the basis of preliminary bivariate or external significance tests. All theoretically justified paths are evaluated

simultaneously in the SEM, which is more consistent with a confirmatory mediation framework. *Changes made: Results, SEM specification and Figure/model description.*

Comment B-R5: "Mediation may exist even in the absence of a significant direct effect. Therefore, the decision to remove or retain paths should be justified not only statistically but also theoretically."

Response: We agree. The manuscript now explicitly states that a statistically significant indirect effect can be present even when the corresponding direct effect is not statistically significant. Decisions regarding model specification are therefore grounded in the theoretical model and the joint pattern of direct and indirect effects rather than in a requirement for a significant direct path. *Changes made: Results and Discussion, mediation interpretation.*

Comment B-R6: "Regarding the fit indices, describing a CFI of 0.932 as indicating "good fit" may not be appropriate. Consider revising the wording and describing this result as acceptable, given its proximity to 0.90."

Response: We corrected the wording. CFI = 0.932 is now described as indicating acceptable model fit rather than good or excellent fit. The limitations section also notes that the model fit is adequate but not uniformly strong across all indices. *Changes made: Results, SEM; Limitations.*

Comment B-R7: "The RMSEA is not reported, which is inconsistent with the presentation of the model. This index is relevant because simulation studies and theoretical considerations suggest that it is important for evaluating how the proposed model performs."

Response: Thank you for noting the omission. RMSEA is now reported alongside the other global fit indices. The revised manuscript reports RMSEA = 0.025 and interprets this value as evidence of good approximate fit, considered together with the remaining fit indices rather than in isolation. *Changes made: Results, SEM fit indices.*

Comment B-R8: "The confidence interval is close to zero, suggesting a weak effect or possible instability in the model. This weakens the claim of statistical significance made in the manuscript."

Response: We agree that the magnitude of the indirect effect requires a cautious interpretation. The revised Discussion now emphasizes that the indirect effect is small and that the confidence interval being close to zero is consistent with a modest effect. We therefore avoid equating statistical significance with substantive strength and present empathy as one potentially relevant mechanism among multiple psychological and contextual processes linking family violence to mental health. *Changes made: Results and Discussion, mediation subsection.*

Comment B-R9: "The manuscript presents two models, A and B, without adequately justifying why both were included."

Response: We removed the ambiguity created by presenting two insufficiently justified models. The revised Results focus on the complete theoretically specified structural model and clearly distinguish any preliminary or alternative analysis from the primary inferential model. The interpretation and conclusions are based on the primary model only. *Changes made: Results, SEM model presentation.*

Discussion

Comment B-D1: "To improve coherence with the stated hypotheses, the discussion should explicitly present each result in relation to each hypothesis. As currently written, some paragraphs are confusing with respect to the paths and results presented."

Response: We reorganized the Discussion to follow the hypotheses in order. Each subsection or paragraph now identifies the relevant hypothesis, states whether the finding supported it, and then interprets and contrasts that result with previous evidence. This restructuring makes the correspondence between hypotheses, paths, and interpretation explicit. *Changes made: Discussion, paragraphs 2-7.*

Comment B-D2: "The omission of non-significant results is not appropriate. These findings should be critically discussed. The discussion should not only confirm the hypotheses but also explain the model as a whole."

Response: We agree. The non-significant relationship between child-to-parent violence and empathy is now discussed directly rather than omitted. We consider the possibility that child-to-parent violence may be more closely linked to coercive family processes, impulsivity, and behavioral dysregulation than to empathic sensitivity, and we note that different forms of family violence may activate distinct psychological mechanisms. *Changes made: Discussion, H2 subsection.*

Comment B-D3: "Regarding the mediating role of empathy, the manuscript should better explain the distinction between affective and cognitive empathy, supported by studies that indicate which dimension may predominate. Since these dimensions were not analyzed independently, the authors should provide a stronger argument rather than merging them without distinction."

Response: We substantially expanded this section. The revised Discussion differentiates cognitive empathy from affective empathy and explains that cognitive empathy is often associated with perspective-taking and adaptive social functioning, whereas high affective resonance in adverse environments may be accompanied by empathic distress and internalizing symptoms. Because the instrument used in this study did not estimate these dimensions independently, we explicitly identify this as a measurement limitation and avoid attributing the observed mediation to one empathy component with certainty. *Changes made: Discussion, empathy subsection; Limitations.*

Comment B-D4: "The weakness of the effects or paths is not discussed."

Response: We agree. The revised Discussion now addresses the magnitude, not only the statistical significance, of the estimated effects. We explicitly note that the child-to-parent violence to mental-health path ($\beta = -0.11$) and the indirect effect ($\beta = -0.071$) are small, suggesting that important explanatory processes remain outside the present model. *Changes made: Discussion, direct effects and mediation subsections.*

Comment B-D5: "The manuscript does not mention important limitations, including the low reliability coefficients and the sampling bias related to family structure, such as living with both parents."

Response: The Limitations section has been substantially expanded. It now includes the moderate/low reliability of several measures, possible attenuation due to measurement error, the non-probability sampling strategy, the restriction to adolescents living with both parents, exclusive reliance on self-report, and the lack of separate measurement of affective and cognitive empathy. *Changes made: Limitations.*

Comment B-D6: "The elimination of paths should be better justified."

Response: We agree. Because the revised analysis estimates the complete theoretically specified model, the manuscript no longer relies on data-driven elimination of theoretically relevant paths. We also clarify that path retention is based on conceptual justification and the logic of mediation rather than on isolated significance tests. *Changes made: Results, SEM specification; Discussion.*

Comment B-D7: "The overall discussion feels superficial. There is little contrast, debate, or engagement with contradictory evidence. The discussion should not only confirm the study findings but also question and qualify them in relation to previous research."

Response: We substantially reconstructed the Discussion to include comparison across studies, contradictory evidence, alternative explanations, conceptual qualification, and more cautious interpretation of the observed associations. The revised version therefore moves beyond confirmation of the hypotheses and addresses where our findings converge with or diverge from previous research on family violence, trauma, empathy, and adolescent mental health. *Changes made: Discussion throughout.*

Comment B-D8: "The Amazonian context is underused. The manuscript does not adequately explain or integrate cultural, contextual, or regional factors. As a result, the initial emphasis on this context is not sufficiently reflected in the final interpretation of the findings."

Response: We agree. The Peruvian Amazonian context is now integrated into both the Introduction and the Discussion. We discuss how structural and sociocultural conditions, psychosocial vulnerability, family stressors, and barriers to mental-health care may shape the experience and consequences of family violence among adolescents in this setting. These contextual considerations are presented as plausible interpretive factors rather than as causal explanations demonstrated by our data. *Changes made: Introduction and Discussion, contextual interpretation.*

Comment B-D9: "The results should not be overgeneralized, given the number and relevance of the limitations identified."

Response: We have moderated the scope of the conclusions and removed language implying representativeness of Peruvian adolescents generally. The revised manuscript specifies that the findings apply to the sampled students from public schools in Tarapoto and should not be generalized to all adolescents in the Peruvian Amazon or Peru without replication in more diverse and probabilistic samples. *Changes made: Discussion and Conclusions.*

Comment B-D10: "The limitations mentioned throughout the review should be explicitly included in the manuscript."

Response: All major limitations identified across the review have now been consolidated explicitly in the Limitations section, including reliability, sampling and family-structure restrictions, cross-sectional design, self-report measurement, limited differentiation of empathy dimensions, and limits to generalizability. *Changes made: Limitations.*

Response to Reviewer C

Comment C1: "Abstract, final line of the Methods section: Explicitly state that the sample came from public schools in Tarapoto, Peru, to provide greater transparency and contextualization. Also, be cautious when stating that the study represents adolescents from the Peruvian Amazon, since the sampling was non-probabilistic and intentional, involving a group of adolescents from public schools in one city of the Peruvian Amazon."

Response: We agree and revised the Abstract to specify the source of the sample. The Methods sentence now states that participants were adolescents attending public schools in Tarapoto, Peru. We also revised wording throughout the manuscript to avoid implying that this non-probability, purposively selected sample represents adolescents across the Peruvian Amazon. The population is now described more precisely as school-attending adolescents from one city in the Peruvian Amazon. *Changes made: Abstract; Methodology, Participants; Discussion and Conclusions.*

Comment C2: "Introduction, final paragraph: Strengthen the originality of the study by emphasizing that it focuses on school-attending adolescents from a city in the Peruvian Amazon, a population that remains underexplored in the scientific literature."

Response: We strengthened the final paragraph of the Introduction to make the originality of the population explicit. The revised text emphasizes that the study focuses on school-attending adolescents from Tarapoto, a city in the Peruvian Amazon, and that this population remains comparatively underrepresented in research integrating family violence, empathy, and mental-health outcomes. *Changes made: Introduction, final paragraphs.*

Comment C3: "Theoretical framework, paragraph on H2: Better justify the hypothesis regarding the relationship between child-to-parent violence and empathy, since the evidence cited focuses more strongly on domestic violence."

Response: We agree that the original evidence supporting H2 relied too heavily on intrafamily or domestic violence. We revised the theoretical framework to distinguish child-to-parent violence from other forms of family violence and to clarify why its association with empathy may differ. The revised rationale discusses child-to-parent violence in relation to coercive family processes, behavioral dysregulation, and impulsivity, and it avoids assuming that evidence from exposure to domestic violence can be transferred directly to violence directed by adolescents toward parents. This distinction provides a more defensible rationale for the hypothesized absence of a significant child-to-parent violence-empathy association. *Changes made: Theoretical framework and H2.*

Comment C4: "Methodology, paragraph 2: Correct the description of the sample, clarifying that participants were students from public schools in Tarapoto. Also, review the typographical error in the reported age values: M = 11.10; SD = 13.5."

Response: Thank you. The Participants section now explicitly states that the sample consisted of students recruited from public schools in Tarapoto. We also verified the age data and corrected the typographical transposition. The correct descriptive values are M = 13.5 years and SD = 1.10. *Changes made: Methodology, Participants.*

Comment C5: "Methodology, instruments, R-MHI-5: Indicate that the RMSEA = 0.101 reflects mediocre fit and discuss this issue as a limitation."

Response: We agree. The instrument description now states explicitly that RMSEA = 0.101 for the R-MHI-5 reflects mediocre/less-than-optimal approximate fit rather than presenting the evidence uncritically. We also added this psychometric issue to the Limitations section and note that measurement limitations in the mental-health indicator may affect the precision of associations estimated in the structural model. *Changes made: Methodology, Instruments (R-MHI-5); Limitations.*

Comment C6: "Results, Table 2: Discuss the low reliability of the intrafamily violence measure, alpha = 0.60, as a methodological limitation."

Response: We agree and now discuss the low internal consistency of the intrafamily violence measure directly. The manuscript reports alpha = .60 (and omega = .62) and acknowledges that this level of reliability may increase measurement error and attenuate the magnitude of observed associations. Corresponding inferences are therefore interpreted cautiously. *Changes made: Results, Table 2 interpretation; Limitations.*

Comment C7: "Results, SEM: Indicate that the CFI = 0.932 suggests acceptable, but not excellent, model fit, and discuss this point in the limitations section."

Response: We corrected the interpretation of the structural model fit. CFI = 0.932 is now described as acceptable rather than excellent or good. The Limitations section further notes that the overall fit should be

considered adequate but not uniformly strong across indices, which supports a cautious interpretation of the model. *Changes made: Results, SEM; Limitations.*

Comment C8: "Discussion, paragraph on child-to-parent violence and empathy: Highlight the absence of a significant relationship between child-to-parent violence and empathy as a novel finding, and propose possible cultural or methodological explanations."

Response: We appreciate this suggestion and now highlight the non-significant child-to-parent violence-empathy association as an informative and potentially novel finding rather than treating it as a failed hypothesis. We discuss several cautious explanations, including the possibility that child-to-parent violence is more strongly linked to coercive interaction patterns, impulsivity, and behavioral dysregulation than to empathic sensitivity, as well as the possibility that contextual or measurement characteristics of the Tarapoto school sample contributed to the observed null association. These explanations are presented as hypotheses for future research rather than as established mechanisms. *Changes made: Discussion, H2 subsection.*

Comment C9: "Discussion, paragraph on empathy and mental health: Expand the reflection on the paradox of empathy, which may be protective in some contexts but may also increase vulnerability in violent environments."

Response: We expanded the Discussion of the apparent empathy paradox. The revised text explains that empathy may be protective when it supports perspective-taking, prosocial behavior, and regulated interpersonal responding, while high affective resonance in violent or threatening environments may also be accompanied by hypervigilance, emotional overactivation, or empathic distress. This distinction helps explain why empathy should not be interpreted as uniformly beneficial or harmful and reinforces the need to separate cognitive and affective components in future research. *Changes made: Discussion, empathy and mental-health subsection.*

Comment C10: "Conclusions, final paragraph: Include more specific practical recommendations for school-based programs targeting students in the Peruvian Amazon. Emphasize the need to strengthen both empathy and emotional regulation."

Response: We revised the final paragraph of the Conclusions to provide more concrete implications for schools serving adolescents in the Peruvian Amazon. We now recommend that school-based prevention and support programs combine the development of perspective-taking and prosocial empathic skills with emotional-regulation strategies, management of empathic distress, identification of family-violence risk, and clear referral pathways for students requiring specialized support. We also emphasize that these recommendations should be adapted to local school and community resources and evaluated prospectively before broader implementation. *Changes made: Conclusions, final paragraph.*

Comment C11: "Studies conducted in Paraguay have examined the association between violence and mental health disorders among school-attending adolescents and adolescent women. These studies may be relevant for the discussion."

Response: Thank you for this useful regional suggestion. We incorporated relevant Paraguayan evidence on violence and mental-health outcomes among school-attending adolescents and adolescent women into the Discussion and reference list. These studies are used to situate our findings within a broader South American context and to compare the consistency of associations between violence exposure and mental-health problems across settings, while also noting differences in population, design, and measures. *Changes made: Discussion, regional evidence; References.*

We are grateful for the time and expertise invested by the Editor and Reviewers B and C. We believe that the revised manuscript is substantially stronger, more theoretically coherent, methodologically transparent, and appropriately cautious in its interpretation. We hope that the revisions satisfactorily address the concerns raised and that the manuscript may now be reconsidered for publication in *Interacciones*.

Sincerely,

Esther Villanueva-Barboza

Ronald Gianmarco Silva-Fernandez

On behalf of the authors