




Social Adjustment as Predicted by Cultural Identity and Acculturative Stress

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E d i t o r	R e v i e w e r s
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1. Round 1

1.1. Reviewer 1

Reviewer:

In the Introduction, paragraph 4: The phrase “has been identified as a robust predictor...” when referring to acculturative stress should include a brief summary of the mechanisms (e.g., role strain, emotional dissonance) through which stress leads to impaired adjustment, to enhance conceptual clarity.

In the Methods, subsection “Study Design and Participants”: The sentence “The purpose and procedures of the study were clearly explained...” would benefit from specifying whether any institutional ethics committee approved the study, including an IRB number if applicable.

In the Measures, under “Social Adjustment”: The authors mention that “higher scores indicate greater social maladjustment”. This is confusing since most readers would interpret a higher score as better adjustment. Clarify whether reverse scoring was applied in analysis or if higher raw scores indicate worse outcomes.

In the Measures, under “Cultural Identity”: The phrase “strong psychometric properties” is too vague. Please report the Cronbach’s alpha for MEIM from the current sample to support reliability claims.

In the Measures, under “Acculturative Stress”: The authors mention that SAFE has “construct and criterion validity in numerous empirical studies,” yet there is no mention of how validity was assessed in the current sample. Include at least a brief description of internal consistency or factor loadings if available.

In the regression analysis paragraph (before Table 3): The authors report that “the model explained 31% of the variance...” but should contextualize this effect size using Cohen’s (1988) guidelines to help readers interpret the practical significance.

In the Discussion, paragraph 1: The phrase “Cultural identity emerged as a positive predictor...” is accurate, but the discussion would benefit from deeper exploration of which MEIM subscale (exploration vs. commitment) was more predictive, if that analysis was possible.

In the Discussion, paragraph 7: The authors mention “coexistence of both risk and resilience factors” but do not explore interaction effects. Consider whether a moderation analysis (e.g., cultural identity moderating the effect of stress) could be conducted or at least discussed as a future direction.

Authors revised the manuscript and uploaded the document.

1.2. Reviewer 2

Reviewer:

In the Introduction, paragraph 5: The citation of Behara et al. (2018) lacks a deeper engagement with the findings. It would improve the argument to briefly describe the specific moderating role of assimilation versus separation.

In the Introduction, paragraph 9: The sentence “Despite growing research in this domain...” rightly points to a gap in Indian research but should acknowledge existing studies on intra-Indian acculturation among regional ethnic groups for more nuanced context.

In the last paragraph of the Introduction: The authors write “This study hypothesizes that cultural identity will positively predict social adjustment...”. Please state this as a formal hypothesis (e.g., H1 and H2), which would improve clarity and structure.

In the Data Analysis paragraph: The sentence “Assumptions of linearity, normality...” lists the diagnostics but lacks detail. Please report test statistics (e.g., Shapiro-Wilk $W = .XX$) or effect sizes where appropriate to support claims of assumption validity.

In the Findings and Results section, demographics paragraph: The representation of ethnic groups is helpful but lacks information on religious or linguistic subgroups, which could be relevant in a multicultural Indian context. Consider addressing this limitation.

In the paragraph before Table 2: The sentence “These values are consistent with prior studies...” should cite specific prior studies with similar mean scores to support this comparative claim.

In the Discussion, paragraph 3: The authors refer to “integration acculturation strategy” but do not tie this back to any data in the current study. Since the SAFE scale does not measure strategy directly, this interpretation may be speculative unless justified more carefully.

In the Discussion, paragraph 5: The mention of “alterations in cortisol patterns” from García et al. (2017) is compelling, but the biological mechanisms are beyond the scope of this study. Consider streamlining this or clarifying that it's a related line of evidence.

Authors revised the manuscript and uploaded the document.

2. Revised

Editor’s decision: Accepted.

Editor in Chief’s decision: Accepted.