

Psychometric assessment of the Persian translation of the higher education version of the Social Emotional Health Survey (SEHS-HE) with students

Niloofer. Movahedi¹, Negar. Zare¹, Simin. Hosseini¹, Azade. Taheri^{1*}

¹ Department of Counseling, Faculty of Education and Psychology, Alzahra University

* Corresponding author email address: a.taheri@alzahra.ac.ir

E d i t o r	R e v i e w e r s
Salahadin Lotfi ^{id} PhD in Cognitive Psychology & Neuroscience, UWM & Rogers Behavioral Health Verified, Lecturer at University of Wisconsin slotfi@uwm.edu	Reviewer 1: Zahra Yousefi ^{id} Assistant Professor, Department of Psychology, Khorasgan Branch, Islamic Azad University, Isfahan, Iran. Email: yousefi1393@khuif.ac.ir Reviewer 2: Kamdin. Parsakia ^{id} Department of Psychology and Counseling, KMAN Research Institute, Richmond Hill, Ontario, Canada. Email: kamdinarsakia@kmanresce.ca

1. Round 1

1.1. Reviewer 1

Reviewer:

In the Introduction, the paragraph beginning "*Human development is inherently characterized by personal, social, and economic transitions...*" provides a broad overview of mental health among university students but does not sufficiently explain why the SEHS-HE offers advantages over existing multidimensional well-being measures. The authors should explicitly identify the conceptual gap that cannot be addressed by instruments such as the Mental Health Continuum-Short Form, Warwick-Edinburgh Mental Well-being Scale, or Ryff's Psychological Well-being Scale, thereby strengthening the theoretical rationale for validating the SEHS-HE.

The paragraph beginning "*Mental health extends beyond the alleviation of anxiety and the reduction of problems*" contains an oversimplified discussion of hedonic and eudaimonic well-being. The manuscript would benefit from a clearer conceptual integration between Keyes' complete mental health model and the covitality framework underlying the SEHS-HE. At present, these theoretical models are presented sequentially without adequately explaining their conceptual relationship or how the SEHS-HE operationalizes positive mental health beyond symptom reduction.

The paragraph beginning *"In this regard, Furlong et al. (2014) have presented one of the proposed composite constructs..."* should include a more detailed justification for the hypothesized four-factor higher-order structure. The manuscript introduces the four domains descriptively but does not explain why a second-order model is theoretically preferable to correlated first-order factors. Since confirmatory factor analysis is central to the paper, the theoretical basis for the hierarchical factor structure should be developed before presenting the empirical analyses.

The sentence *"Accordingly, the first target of the current research is to assess the psychometric properties of the SEHS with a sample of Iranian youths"* is inconsistent with the sampled population. Throughout the manuscript the participants are female university students aged 20–25 years, whereas the term "Iranian youths" implies a substantially broader population. The authors should consistently distinguish between university students and the general youth population to avoid overstating the generalizability of the findings.

In the Materials and Method section, the sentence *"The statistical population of the research comprises of the students of Al-Zahra University"* raises concerns regarding representativeness. Because Al-Zahra University is an all-female institution, the sample is inherently gender-specific. The authors should explicitly acknowledge how the institutional characteristics may influence the observed factor structure and psychometric estimates, particularly for dimensions such as peer support and family support.

The manuscript reports convergent validity using AVE and correlations with SWLS but does not evaluate discriminant validity among the four latent constructs. Given the substantial inter-factor correlations reported in Table 6, the authors should calculate and report discriminant validity using the Fornell-Larcker criterion and/or the Heterotrait-Monotrait Ratio (HTMT). Without these analyses, evidence supporting construct distinctiveness remains incomplete.

The correlation matrix presented in Table 6 reports relatively strong associations among the four dimensions and Covitality, but confidence intervals and significance levels beyond simple correlation coefficients are not reported. The authors should additionally report confidence intervals and discuss whether multicollinearity among higher-order factors could influence the CFA estimates or latent construct interpretation.

The Discussion begins with the sentence *"The current study was purposed to investigate the psychometric properties..."* but focuses predominantly on interpreting associations with life satisfaction and psychological distress. The discussion should instead devote greater attention to interpreting the psychometric evidence itself, including translation quality, factorial validity, internal consistency, and comparison of fit indices with previous international validation studies. This would better align the discussion with the primary objective of a validation study.

Authors uploaded the revised manuscript.

1.2. Reviewer 2

Reviewer:

The Procedure section states, *"First two English experts translated the items into Persian language independently and then a third translator... back-translated..."* While this follows a conventional translation procedure, internationally accepted cross-cultural adaptation guidelines also recommend expert committee review, cognitive debriefing, documentation of semantic equivalence, and reconciliation of conceptual discrepancies. Please clarify whether these additional stages were performed and cite established translation guidelines (e.g., Beaton et al. or ISPOR recommendations) if applicable.

The Results section reports, *"There was no missing data."* Such a finding is uncommon in survey-based psychological research involving over 300 respondents. The authors should explain whether the online survey required mandatory responses before submission or whether missing data screening and imputation procedures were conducted. This clarification would improve methodological transparency.

The paragraph beginning *"The Mahalanobis index is used to detect outlier data"* contains an inaccurate interpretation. Dividing the maximum Mahalanobis distance (121) by the number of questionnaire items (36) is not a recognized statistical criterion for identifying multivariate outliers. Instead, Mahalanobis distances should be compared against the chi-square

distribution with degrees of freedom equal to the number of observed variables. This section requires correction and appropriate statistical justification.

The Content Validity section reports that *"8 experts (4 consultants and 4 psychologists) were asked..."* However, the manuscript does not describe the selection criteria, academic qualifications, years of professional experience, or areas of expertise of these experts. Since CVR and CVI depend heavily on panel expertise, additional information regarding the expert panel should be provided to enhance methodological credibility.

The paragraph introducing confirmatory factor analysis states, *"The sample size required for confirmatory factor analysis is considered to be 5 to 20 times more than the total number of the scale items."* This rule-of-thumb is outdated and insufficient as the primary justification for sample adequacy. The authors should instead discuss sample adequacy in relation to model complexity, parameter estimation, communalities, or Monte Carlo recommendations, which provide a more contemporary justification for structural equation modeling sample size.

The sentence *"All factor loadings of the items were positive, less than 1 and greater than 0.4"* would benefit from a complete table reporting standardized factor loadings, standard errors, critical ratios, and significance values for each item. Although Figure 1 presents standardized coefficients visually, reviewers and readers require a tabulated presentation to evaluate item performance more precisely.

Table 4 reports the fit indices as RMSEA = 0.06, CFI = 0.86, TLI = 0.85, GFI = 0.86, and NFI = 0.78. Nevertheless, the manuscript concludes that *"all dimensions have a good fit."* This interpretation is not supported by commonly accepted SEM criteria because CFI, TLI, GFI, and especially NFI remain below the conventional threshold of .90. The discussion should acknowledge that model fit is acceptable or moderate rather than unequivocally good, and possible reasons for the lower fit indices should be discussed.

Table 5 shows that the AVE for Emotional Competence equals 0.47, while the manuscript states that *"the four dimensions... have a favorable convergent validity."* Since AVE values below .50 generally indicate inadequate convergent validity unless justified by high composite reliability, this discrepancy requires explicit discussion. The authors should explain why this construct was retained despite failing to meet the conventional AVE criterion.

Authors uploaded the revised manuscript.

2. Revised

Editor's decision after revisions: Accepted.

Editor in Chief's decision: Accepted.