

Designing and Validating a Model for Emotional Agility

Sara. Hemmatiyan¹, Mohamadreza. Dalvi Esfahan^{2*}, Alireza. Shirvani Jozdani³

¹ PhD Student, Human Department of Management, Deh.C., Islamic Azad University, Isfahan, Iran

² Professor, Department of Management, Deh.C., Islamic Azad University, Isfahan, Iran

³ Department of Management, Deh.C., Islamic Azad University, Isfahan, Iran

* Corresponding author email address: mr.dalvi1349@iau.ac.ir

E d i t o r	R e v i e w e r s
Izet Pehlić [✉] Full professor for Educational sciences, Islamic pedagogical faculty of the University of Zenica, Bosnia and Herzegovina izet.pehlic@unze.ba	Reviewer 1: Masoud Asadi [✉] Assistant Professor, Department of Psychology and Counseling, Arak University, Arak, Iran. Email: m-asadi@araku.ac.ir Reviewer 2: Mahdi Khanjani [✉] Associate Professor, Department of Psychology, Allameh Tabataba'i University, Tehran, Iran. Email: khanjani_m@atu.ac.ir

1. Round 1

1.1. Reviewer 1

Reviewer:

The sampling description in the Method requires substantially greater detail. The manuscript states that “stratified random sampling was employed based on demographic variables such as gender and age group,” but the Findings show a markedly unequal gender distribution of 172 men and 36 women. The authors should explain the sampling frame, recruitment setting, strata definitions, allocation procedure, and method used to randomly select participants within each stratum. It should also be clarified why a procedure described as stratified random sampling resulted in such substantial gender imbalance. If recruitment was actually convenience-based with subsequent attempts to diversify the sample, the sampling method should be reported accurately rather than characterized as probability sampling.

There is an important inconsistency between the Abstract and the second-order CFA results. The Abstract states, “The dimension of Value-Driven Action and Emotional Meaning-Making, with the highest path coefficient, constituted the central core of emotional agility,” whereas Table 4 and the accompanying Results indicate that Value-Driven Action has the lowest reported coefficient (.458), while Emotional Awareness and Clarity is reported as 1.000 and Emotional Acceptance and Mindfulness as .941. This contradiction must be corrected. More importantly, the authors should determine whether the value

of 1.000 for Emotional Awareness and Clarity is a standardized loading or a fixed unstandardized reference loading used for model identification. If it is a fixed reference parameter, it must not be interpreted as evidence that this factor has the strongest association with emotional agility.

Table 3 requires substantial revision because its title, “Descriptive Statistics of the Research Variables,” does not correspond to its actual contents. The table presents quantities labeled SE, SC, CR, p, and “Result,” apparently related to confirmatory factor analysis rather than descriptive statistics. The authors should rename the table to reflect its actual function, define every abbreviation in a table note, and distinguish clearly between standardized factor loadings, unstandardized estimates, standard errors, critical ratios, and significance levels. As presently displayed, several combinations of values appear mathematically inconsistent—for example, an estimate, standard error, and CR should satisfy $CR = \text{estimate}/SE$. The numerical columns therefore need to be checked against the original AMOS output to ensure that values have not been transposed.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

The manuscript needs a dedicated Ethical Considerations subsection. The current Method does not report approval by an institutional ethics committee, informed consent procedures, voluntary participation, confidentiality protections, anonymity, data-security procedures, or participants’ right to withdraw. These elements are particularly important because the study collected psychological self-report data from human participants and obtained judgments from expert panelists across multiple Delphi rounds. The authors should provide the ethics approval code and approving institution, state that informed consent was obtained, and explain how participants’ and experts’ information was protected.

In the description of the researcher-developed Emotional Agility Questionnaire, the sentence, “The questionnaire items were scored on a 5-point Likert scale ranging from completely relevant (1) to completely irrelevant (5),” raises a serious conceptual concern. “Completely relevant” to “completely irrelevant” is an appropriate response format for expert content-validity judgments, but it is generally inappropriate for measuring respondents’ level of emotional agility. Participants should ordinarily indicate agreement, frequency, typicality, or degree of self-description—for example, “strongly disagree” to “strongly agree” or “never” to “always.” The authors must clarify whether the quantitative sample rated the relevance of items or endorsed statements describing their own emotional functioning. If general participants actually responded on a relevance scale, the instrument may not currently constitute a valid self-report measure of emotional agility and its scoring and interpretation would need major reconsideration.

The questionnaire scoring direction also requires clarification. The manuscript states that responses ranged from “completely relevant (1) to completely irrelevant (5),” while the Results interpret weighted means above the theoretical midpoint of 3 as indicating “a positive and relatively favorable perception of their emotional capabilities.” If lower scores correspond to greater relevance or stronger endorsement, higher means would logically represent lower rather than higher emotional agility unless the items were reverse-scored before analysis. The authors should explain precisely how items were scored, whether any reverse coding was conducted, how dimension scores were calculated, and why values greater than 3 indicate higher emotional agility. This issue must be resolved because it directly affects the interpretation of Table 2 and potentially all subsequent substantive conclusions.

The evidence provided for content validity is currently insufficient. The Method states that “the content validity of the developed questionnaire was confirmed based on the opinions of relevant faculty members and specialists,” but no quantitative content-validity indices are reported. For a newly developed psychological instrument, expert judgment should ideally be quantified using measures such as Lawshe’s Content Validity Ratio (CVR), item-level and scale-level Content Validity Index (I-CVI and S-CVI), modified kappa, or another established method. The authors should report the criteria used to judge relevance, clarity, simplicity, and necessity, together with item-level results and the thresholds used to retain or revise items.

In the paragraph reporting reliability, the authors state that “Cronbach’s alpha was 0.913 for the total instrument and ranged from 0.768 to 0.913 for the seven dimensions.” Although these values are generally acceptable, Cronbach’s alpha alone is insufficient for comprehensive reliability assessment of a multidimensional scale. The authors should consider reporting McDonald’s omega and, where appropriate, composite reliability for each latent factor. Moreover, the manuscript should report corrected item-total correlations, alpha-if-item-deleted statistics, and preferably temporal stability through test–retest reliability in an independent subsample. This is particularly important because one dimension contains only three items and another contains four, making reliability interpretation more sensitive to item number.

Response: Revised and uploaded the manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.