

Article history:
Received 08 June 2026
Revised 03 September 2026
Accepted 10 September 2026
Initial Publication 13 September 2026
Final Publication 01 January 2027

Structural Model of Workplace Spirituality Based on Spiritual Health, Meaning in Life, and Self-Differentiation with the Mediating Roles of Job Crafting and Moral Identity among Faculty Members of Islamic Azad University, Zanjan Branch

Parvin. Shiri Ghidari¹, Afsaneh. Sobhi^{1*}, Qamar. Kiani¹

¹ Department of Psychology, Za.C, Islamic Azad University, Zanjan, Iran

* Corresponding author email address: A.sobhi@iau.ac.ir

E d i t o r	R e v i e w e r s
Seyed Hamid Atashpour ¹ Associate Professor, Department of Psychology, Isfahan (Khorasgan) Branch, Islamic Azad University, Isfahan, Iran hamidatashpour@gmail.com	Reviewer 1: Hajar Torkan ¹ Assistant Professor, Department of Psychology, Islamic Azad University, Isfahan Branch (Khorasgan), Isfahan, Iran. h.torkan@khuisf.ac.ir Reviewer 2: Farhad Namjoo ² Department of Psychology and Counseling, KMAN Research Institute, Richmond Hill, Ontario, Canada. Email: farhadnamjoo@kmanresce.ca

1. Round 1

1.1. Reviewer 1

Reviewer:

In the paragraph stating that “In predictive models, a sample size ranging from 300 to 500 participants can be considered adequate (Seabrook, 2025),” the justification for sample size is not sufficiently specific to covariance-based structural equation modeling. A generic numerical range does not establish statistical adequacy for the tested model. The authors should provide a model-specific justification based on the number of latent variables, observed indicators, free parameters, anticipated effect sizes, desired power, estimator, and model complexity. Since the manuscript reports 300 analyzed cases, it would also be useful to provide a priori or sensitivity power information and to explain whether the final sample satisfies recommended cases-to-parameter considerations. The citation itself should be carefully evaluated because a paper on multiple-regression sample size is not necessarily sufficient justification for an SEM sample.

The paragraph describing participant flow contains numerical and methodological ambiguity: “350 participants were initially selected... Of these, 50 questionnaires were excluded... consequently, data analysis was conducted using 300 questionnaires,” whereas the data-collection paragraph again states that “300 participants were selected using convenience sampling.” The manuscript should distinguish clearly among the numbers invited, consented, returned questionnaires, excluded

cases, and final analyzable sample. A participant-flow statement or diagram would improve transparency. The response rate should also be reported. Without this information, it is impossible to assess the potential for nonresponse bias or determine whether the 50 exclusions occurred after 350 completed instruments were received or after 350 individuals were merely approached.

The exclusion criterion stating that participants with “acute psychological disorders” were excluded “based on self-report” needs stronger methodological and ethical specification. The authors should explain whether participants were directly asked about psychiatric diagnoses or current acute symptoms, how this information was operationalized, and why such exclusion was necessary for the research question. A vague self-reported mental-health exclusion can introduce selection bias and raises concerns about reproducibility. If no validated screening instrument or documented clinical assessment was used, the wording should be narrowed to the actual procedure rather than implying diagnostic ascertainment. The authors should also indicate whether disclosure of such information was voluntary and how confidentiality was protected.

The data-collection paragraph states that questionnaires were administered through a “mixed-mode approach involving both in-person and online administration,” but no evidence is provided that administration mode did not systematically influence responses. This issue is particularly relevant for sensitive constructs such as spirituality, moral identity, and self-differentiation. The authors should report the number of participants completing paper versus online forms, describe whether item wording and response presentation were identical across formats, and test whether the main variable means or covariance structure differed by administration mode. At minimum, administration mode should be acknowledged as a potential source of method variance, especially if mode was related to workload, availability, age, or academic rank.

In the Workplace Spirituality Questionnaire description, the sentence “The Workplace Spirituality Questionnaire was developed by Milliman et al. (2003)” should be followed by a clearer account of the measurement model used in the current SEM. The manuscript reports three dimensions—meaningful work, sense of community, and alignment with organizational values—but does not state whether workplace spirituality was modeled as a first-order latent construct, a second-order construct, or a total observed score. The figure appears to model workplace spirituality as a latent factor reflected by three indicators, which should be explained explicitly. Furthermore, the authors should report current-sample standardized factor loadings, composite reliability, average variance extracted, and evidence of discriminant validity rather than relying primarily on historical Cronbach’s alpha coefficients.

The Spiritual Well-Being Scale section uses “spiritual health” as the study construct but describes an instrument conventionally labeled the “Spiritual Well-Being Scale,” consisting of existential and religious well-being. The manuscript should justify the conceptual equivalence between “spiritual health” and “spiritual well-being” rather than treating the terms as automatically interchangeable. If the latent variable is operationally defined by the two SWBS dimensions, the construct should be named consistently throughout the title, abstract, hypotheses, figure, tables, and discussion. A clear operational definition is especially important because the introduction distinguishes spiritual health from workplace spirituality, and conceptual overlap between these constructs could otherwise inflate associations.

The model-fit strategy relies on several older fit indices and relatively permissive thresholds. The statement that GFI, AGFI, CFI, and IFI were considered acceptable when “greater than .80” should be reconsidered, particularly for CFI and IFI, for which contemporary SEM practice generally expects substantially higher values. A CFI of .90 should therefore be characterized as acceptable or marginal rather than necessarily strong. The authors should justify their cutoff criteria with methodological references and add SRMR if available. Confidence intervals for RMSEA should also be reported. This would provide readers with a more transparent evaluation of whether the model is genuinely well fitting rather than simply meeting selected minimum thresholds.

The structural findings require reconciliation with an inconsistency in the narrative immediately following Figure 1: “the direct effect of self-differentiation on workplace spirituality was not significant; however, the direct effects of spiritual health, meaning in life, self-differentiation, and job crafting on workplace spirituality were significant.” Self-differentiation is simultaneously described as nonsignificant and significant in the same passage, and moral identity appears to be omitted from the list despite its significant coefficient of $\beta = .426$. This sentence should be corrected to state that spiritual health, meaning in life, job crafting, and moral identity had significant positive direct effects, whereas self-differentiation did not. Similar consistency checks should be conducted across the Abstract, Results, Discussion, tables, and figure.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

The Meaning in Life Questionnaire paragraph contains psychometric statements that require correction or clarification, particularly the sentence reporting “convergent validity coefficients of .46 with life satisfaction and .32 with positive affect, as well as divergent validity coefficients of .48 with depression, .28 with negative affect, and $-.23$ with neuroticism.” The signs of the reported associations should be verified against the original source, because positive correlations with depression and negative affect would not ordinarily be presented as evidence of divergent validity without explanation. More importantly, the manuscript should report the psychometric performance of the MLQ in the present sample, including factor loadings for Presence of Meaning and Search for Meaning. Given that these two dimensions can be theoretically distinct and not necessarily interchangeable, the rationale for aggregating them into a single “meaning in life” latent factor should be defended.

The Differentiation of Self Inventory section reports a current Cronbach’s alpha of .95, which is exceptionally high for a multidimensional 46-item scale. The authors should examine whether this coefficient reflects item redundancy or whether reporting a single total-scale alpha is psychometrically appropriate for a construct comprising emotional reactivity, I-position, emotional cutoff, and fusion with others. Because the SEM treats self-differentiation as a latent construct indicated by these four subdimensions, reliability should preferably be reported for each dimension and for the higher-order construct using omega or composite reliability in addition to alpha. The direction of scoring should also be explained because some DSI dimensions typically require reverse coding before higher scores can consistently represent greater differentiation.

The Job Crafting Questionnaire description requires source verification and conceptual precision. The sentence “The Job Crafting Questionnaire was developed by Laurence (2010)” should specify the exact instrument and its theoretical basis, because multiple job-crafting measures exist with different dimensions. The manuscript uses “changing task boundaries,” “changing relational boundaries,” and “changing cognitive boundaries,” which corresponds to a specific conceptualization of job crafting and should be explicitly linked to that framework. More importantly, the reported statement that discriminant validity was established through a “significant negative correlation of $-.12$ with job satisfaction” should be checked carefully, because the Fornell–Larcker criterion is not established by a simple correlation with job satisfaction. The psychometric terminology should therefore be corrected, and current-sample CFA evidence should be provided.

The Moral Identity Questionnaire section should be revised for both naming and measurement accuracy. The sentence “Moral identity was assessed using the Moral Identity Questionnaire developed by Aquino and Reed (2002)” should acknowledge that the original measure is commonly conceptualized as a moral identity scale based on internalization and symbolization. The authors should explain whether the original trait-listing instructions were preserved in the Persian version and whether the two-factor structure was replicated in the present faculty sample. Because internalization and symbolization can have different psychological implications, the appropriateness of combining them into one latent moral-identity construct should be supported with CFA results, inter-factor correlations, and second-order-model evidence rather than only an overall alpha of .86.

The manuscript states that “The skewness and kurtosis coefficients for all main variables fell within the range of -2 to $+2$, indicating univariate normality,” and then uses a multivariate critical ratio of 2.35 to confirm multivariate normality. This section requires greater statistical precision. The authors should identify the specific AMOS statistic used, most likely the multivariate kurtosis critical ratio, and cite the criterion applied. They should also report whether normality was examined at the item level, indicator level, or composite-score level. Because SEM estimation is sensitive to multivariate non-normality, the estimator used—presumably maximum likelihood—must be explicitly stated, and robust or bootstrap procedures should be considered if distributional assumptions were not fully satisfied.

The fit criteria and reported results contain an internal inconsistency that should be addressed. The Methods section states that “the chi-square to degrees-of-freedom ratio (χ^2/df) with a value less than 3” was used as an acceptance criterion, whereas Table 3 reports $\chi^2/df = 3.04$ and the text subsequently claims that “all calculated goodness-of-fit indices were within the

acceptable ranges.” By the authors’ own predefined criterion, 3.04 exceeds the cutoff. This should not be described as satisfying all criteria. The authors should report the absolute χ^2 , degrees of freedom, and exact p value, acknowledge that χ^2/df was marginally above the specified threshold, and interpret overall model fit using the complete pattern of RMSEA, CFI, IFI, GFI, AGFI, and preferably SRMR rather than retroactively changing the threshold.

Response: Revised and uploaded the manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.