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Designing a Quantitative Model for the Development of Sports for Children with Special Needs in Iran

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Christian Wiesner Professor at University College of Teacher Education in Lower Austria christian.wiesner@ph-noe.ac.at	**Reviewer 1:** Hooman Namvar Assisstant Professor, Department of Psychology, Saveh Branch, Islamic Azad University, Saveh, Iran. Email: hnamvar@iau-saveh.ac.ir **Reviewer 2:** Stephen C. L. Lau Professor (Assistant) at Washington University in St, Louis, United States. Email: lauc@wustl.edu

1. Round 1

1.1. Reviewer 1

Reviewer:

The terminology used for the target population should be standardized throughout the manuscript. The title refers to “children with special needs,” while several passages refer to “children with special needs,” “children with disabilities,” “students with disabilities,” and the underlying Persian terminology appears to correspond to “exceptional children.” These expressions are not necessarily interchangeable from scientific, educational, or policy perspectives. The authors should provide an operational definition specifying which groups are included—for example, children with physical disabilities, intellectual disability, autism spectrum disorder, learning disabilities, sensory impairments, developmental coordination disorder, or multiple disabilities—and should explain whether the model is intended to apply uniformly across these heterogeneous populations. Without such clarification, the scope and external validity of the proposed model remain ambiguous.

The Results section states that “the partial least squares algorithm (PLS Algorithm) was implemented to estimate path coefficients, factor loadings, the proportion of explained variance in endogenous constructs, and the direct, indirect, and total effects among the research variables.” The authors should justify the choice of PLS-SEM rather than covariance-based SEM. A methodological justification should address whether the study was primarily prediction-oriented, whether the model contained formative constructs, whether distributional considerations favored PLS, and whether model complexity relative to sample size motivated the choice. Merely using SmartPLS does not establish that PLS-SEM is the appropriate estimator. The manuscript should also specify the weighting scheme, maximum iterations, stopping criterion, bootstrapping procedure, number of bootstrap subsamples, sign-change option, and whether bias-corrected confidence intervals were calculated.

The structural model should be assessed for multicollinearity before the path coefficients are interpreted. In Table 4-8, strategies are predicted simultaneously by the central phenomenon, contextual conditions, and intervening conditions, with path coefficients of 0.47, 0.51, and 0.53. Given the substantial correlations among several higher-order constructs reported in Table 4-6—for example, 0.851 between the central phenomenon and contextual conditions and approximately 0.77–0.78 involving intervening conditions—there is a meaningful possibility of predictor collinearity. The authors should report inner VIF values for all predictor sets and discuss whether the estimated path coefficients are stable. Without collinearity diagnostics, the relative importance attributed to contextual, intervening, and central-phenomenon effects cannot be evaluated confidently.

Authors revised the manuscript and uploaded the document.

1.2. Reviewer 2

Reviewer:

Table 4-1 requires careful rechecking because several reported AVE values appear inconsistent with the later Fornell–Larcker matrix. For example, Table 4-1 reports an AVE of 0.59 for “Causal Conditions,” whereas Table 4-6 gives a diagonal Fornell–Larcker value of 0.968. The square root of 0.59 is approximately 0.768, not 0.968. Similarly, the central phenomenon has AVE = 0.60 in Table 4-1 but a diagonal value of 0.961 in Table 4-6, whereas $\sqrt{0.60}$ is approximately 0.775. These discrepancies are mathematically substantial and raise concern regarding either transcription errors or incorrect computation of discriminant validity. The authors should regenerate the AVE and Fornell–Larcker outputs directly from SmartPLS, ensure that the diagonal values represent $\sqrt{AVE}$, and revise all corresponding interpretations before the model can be considered psychometrically validated.

The paragraph interpreting Table 4-2 contains a conceptual error when it states that “the SSO/SSE-based cross-validated communality criterion likewise reflects predictive relevance.” In blindfolding procedures, Q^2 is calculated as $1 - SSE/SSO$, not SSO/SSE. Furthermore, SSO and SSE being positive does not itself demonstrate acceptable model quality; the relevant criterion is whether Q^2 is greater than zero for an endogenous construct, with larger positive values indicating greater predictive relevance. The same conceptual problem appears in the interpretation of Table 4-3. The authors should rewrite these sections using correct PLS-SEM terminology, distinguish cross-validated communality from cross-validated redundancy, and interpret the Q^2 values rather than asserting that positive SSO and SSE values confirm model adequacy.

Several numerical entries in Tables 4-2 and 4-3 should be independently verified because the displayed Q^2 values do not always appear to correspond to the stated formula $Q^2 = 1 - SSE/SSO$. For example, the entry for “Sports Empowerment of Children” in Table 4-3 reports SSO = 137.75 and SSE = 22.05 with $Q^2 = 0.53$; however, $1 - 22.05/137.75$ is approximately 0.84, not 0.53. Comparable discrepancies may exist elsewhere. Because Q^2 is a core criterion used to support predictive relevance, arithmetic inconsistencies undermine confidence in the statistical results. The authors should recalculate every Q^2 entry from the original SmartPLS output, preferably reproduce the relevant software output in supplementary material, and ensure that no values were shifted between rows during table preparation.

The section titled “Overall Model Quality Test (GoF)” should be reconsidered methodologically. The manuscript reports “$GoF = \sqrt{(0.672 \times 0.908)} \approx 0.781$” and concludes that the model has strong overall fit because the value exceeds 0.36. In contemporary PLS-SEM methodology, the classical Tenenhaus GoF index is generally not regarded as a reliable global model-fit criterion because it cannot adequately distinguish valid from invalid models and is not recommended as a principal basis for model acceptance. The authors should avoid presenting GoF = 0.781 as decisive evidence of model validity. More appropriate evaluation should include measurement-model quality, structural-model explanatory power, predictive relevance, collinearity assessment, effect sizes, and, where justified, indices such as SRMR. If GoF is retained because of disciplinary convention, its limitations should be acknowledged explicitly rather than interpreting it as definitive global fit.

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The manuscript states, “Because the t-coefficients obtained for all hypotheses were greater than ±2.58, their statistical significance was confirmed at the 99% confidence level.” This section should report bootstrap-derived p values and preferably 95% or 99% confidence intervals for each structural path, rather than relying exclusively on threshold comparisons of t statistics. More importantly, several t statistics are extraordinarily large, including values above 100, 200, and even 300 for measurement relationships. Although large t values are possible when standard errors are extremely small, the magnitude observed here warrants verification of the bootstrapping settings and data structure. The authors should confirm that the reported column indeed represents bootstrap t statistics, rather than another SmartPLS output statistic, and provide bootstrap standard errors and confidence intervals for the five principal structural hypotheses.

Authors revised the manuscript and uploaded the document.

2. Revised

Editor’s decision: Accepted.
Editor in Chief’s decision: Accepted.

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