

The Role of Self-Awareness, Self-Control, and Spirituality in the Prevention of Social Harms Among Youth

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1. Round 1

1.1. Reviewer 1

Reviewer:

The manuscript frequently moves between the terms “adolescents,” “youth,” and “young people,” while the sample is defined as individuals aged 18 to 29 years. For example, the introduction cites many adolescent studies, whereas the method states that the statistical population consisted of “young people aged 18 to 29 years living in Birjand, Iran.” The authors should justify the developmental relevance of adolescent-based evidence for an 18–29-year-old sample. If the sample is legally and developmentally closer to emerging adulthood than adolescence, the theoretical framing should incorporate emerging adulthood, identity exploration, transition to adult roles, and autonomy-related risk.

In the introduction, the paragraph beginning “Self-awareness is one of the core psychological capacities that may protect youth against social harms” provides a strong conceptual basis, but the manuscript does not sufficiently distinguish self-awareness from self-regulation and self-control. Because self-awareness, self-control, and spirituality are positively correlated in the results, the authors should explicitly define the conceptual boundaries among these constructs. This is important to avoid construct overlap and to explain why each variable was expected to contribute unique variance to vulnerability to social harms.

The paragraph stating that “Spirituality is broader than formal religiosity and may include meaning in life, purposefulness, moral orientation, connection to the transcendent...” is appropriate, but the operational measure used is the Spiritual Well-

Being Scale, which includes religious well-being and existential well-being. The authors should more clearly explain whether “spirituality” in this study refers to general spirituality, spiritual well-being, religiosity, existential meaning, or a combined construct. This distinction matters because religious well-being and existential well-being may have different associations with youth risk behavior.

The manuscript reports that “For instruments with available validated Persian versions, the validated Persian forms were used,” but it does not specify which measures had validated Persian versions and which required translation and adaptation. This is a significant methodological ambiguity. The authors should list the exact version used for each questionnaire, cite validation studies where available, and describe the translation/adaptation process only for instruments that were actually adapted in the present study. Reporting all instruments together in a generic statement weakens reproducibility.

The data analysis section states that demographic variables were entered as controls “when they showed significant associations with the criterion variable,” while the results section states that age, gender, and educational level were entered in the first step “because of their theoretical relevance.” These are two different analytic justifications. The authors should make the procedure consistent: either demographic covariates were selected a priori based on theory, or they were selected empirically based on preliminary associations. The chosen strategy should be justified because covariate selection can influence regression coefficients and interpretation.

Authors uploaded the revised manuscript.

1.2. Reviewer 2

Reviewer:

The introduction would benefit from a clearer theoretical model before the aim statement. The paragraph beginning “The relationship among self-awareness, self-control, and spirituality can be explained through an integrated model of inner protective resources” is promising, but it remains descriptive rather than model-driven. The authors should specify whether they hypothesize independent direct effects only, or whether self-awareness may influence social harms indirectly through self-control, or spirituality may strengthen self-control through meaning and moral orientation. A diagram or a short theoretical hypothesis paragraph would strengthen the rationale for the structural equation model.

In the method section, the sentence “The present study used a descriptive-correlational design with a predictive modeling approach” should be expanded to clarify the epistemological and statistical meaning of “predictive.” In cross-sectional survey studies, prediction refers to statistical explanation of variance, not temporal prediction. The authors should explicitly state that predictors and outcome were measured simultaneously and that temporal or causal prediction cannot be inferred. This revision would make the methodological language more precise and reduce the risk of overstating the findings.

The sampling procedure is insufficiently detailed. The method states that “several educational, cultural, and community-based centers were selected from different urban districts,” but it does not specify how many districts, how many clusters, how clusters were randomly selected, how participants were recruited within clusters, or whether proportional allocation was used. Since the authors describe the method as multistage cluster sampling, the exact stages of sampling should be reported. Without this information, it is difficult to evaluate sample representativeness and potential selection bias.

The manuscript states that “The final sample included 350 young people” and that this sample size was appropriate because it exceeded the minimum recommended number for regression and structural equation modeling. This justification is too general. The authors should provide an a priori or post hoc power analysis, or at least a defensible sample-size rationale based on the number of parameters estimated in the structural model, anticipated effect sizes, desired power, and significance level. For SEM in particular, sample adequacy depends on model complexity, distributional assumptions, and number of indicators.

The inclusion and exclusion criteria need refinement. The exclusion criterion “reporting a severe psychological or cognitive condition that could interfere with accurate completion of the questionnaires” is clinically important, but the manuscript does not explain how this condition was assessed. If it was based on self-report, this should be stated. If participants were screened using clinical records or diagnostic interviews, the procedure must be described. The authors should also clarify whether

participants with current substance use, self-harm, or delinquent history were excluded, because excluding such participants could bias the outcome variable.

The measures section should provide more precise psychometric information for each instrument in the context of the present study. For example, the paragraph on the Self-Reflection and Insight Scale states that the scale has “acceptable psychometric properties,” but it does not report number of items, response range, subscale structure, sample items, scoring direction, or evidence for validity in Iranian populations. Similar detail is needed for the Brief Self-Control Scale, Spiritual Well-Being Scale, and Iranian Adolescents Risk-Taking Scale. Since measurement quality is central to the credibility of the model, this section requires expansion.

The use of the Iranian Adolescents Risk-Taking Scale for a sample aged 18 to 29 requires justification. The manuscript states that the scale was developed “to assess risk-taking tendencies and social harm-related behaviors among Iranian adolescents and youth,” but the title of the instrument explicitly refers to adolescents. The authors should explain whether the scale has been validated among young adults or emerging adults, and whether its domains are appropriate for participants up to age 29. If no validation exists for this age range, this should be acknowledged as a measurement limitation.

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2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.