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Comparing Sleep Disorders, Procrastination, and Cognitive Intelligence in Male and Female Upper Secondary Students in Rudsar

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1. Round 1

1.1. Reviewer 1

Reviewer:

The literature review on sleep disorders is comprehensive; however, the authors do not sufficiently operationalize "sleep disorders" within the context of their measurement instrument. The term "sleep disorders" conventionally refers to clinically diagnosable conditions, while the employed questionnaire appears to assess sleep-related symptoms or disturbances. The manuscript should therefore clarify whether the study evaluates clinical sleep disorders or subjective sleep disturbance severity to avoid overstatement of findings.

In the paragraph beginning with "Procrastination is another major behavioral and educational variable…", the authors cite multiple studies linking procrastination to motivation and self-regulation, but they do not clearly explain why procrastination was expected to differ by gender in this specific sociocultural context. Since prior findings are described as inconsistent, the study rationale should include a more explicit argument regarding the expected direction of gender differences among Iranian adolescents specifically.

Table 3 introduces "Psychosomatic Problems" as a dependent variable, although this construct is never described in the introduction, methods, measures, or study aims. This is a major inconsistency in the manuscript. The inclusion of an undefined

variable raises concerns about possible editing errors, data misreporting, or table misplacement. The authors must correct this issue immediately.

The reporting of the ANOVA results for sleep disorders appears statistically inconsistent. Specifically, the manuscript reports $F = 30.54$ with $p = 0.040$, which is unlikely given the magnitude of the F statistic. The reported p-value should be rechecked because an F value of this size would typically correspond to $p < 0.001$. This discrepancy affects the credibility of the statistical reporting.

The interpretation of effect sizes requires greater precision. For example, the manuscript describes $\eta^2 = 0.290$ as a "large effect size according to Cohen's criteria," yet no citation for the interpretation standard is provided. Additionally, effect sizes for other variables (e.g., $\eta^2 = 0.071$ and $\eta^2 = 0.001$) should also be interpreted systematically to provide readers with a more nuanced understanding of practical significance.

Authors uploaded the revised manuscript.

1.2. Reviewer 2

Reviewer:

The discussion of cognitive intelligence relies heavily on older debates surrounding sex differences in intelligence, including references to controversial work such as Lynn (1994). The manuscript should contextualize these studies more critically and balance them with contemporary intelligence research emphasizing sociocultural influences, neuroplasticity, educational opportunity, and measurement bias. Otherwise, the discussion risks appearing theoretically outdated and insufficiently nuanced.

In the Methods section under "Study Design and Participants," the authors describe the design as "descriptive-analytical causal-comparative." However, the term "causal-comparative" may imply causal inference despite the cross-sectional observational nature of the study. The authors should revise this terminology and explicitly acknowledge that causal conclusions cannot be drawn from the present design.

The sampling procedure requires further clarification. The authors mention "two-stage cluster random sampling," but they do not explain what constituted the clusters (e.g., schools, classrooms, districts), how clusters were selected, or whether proportional allocation procedures were used. Without these details, the reproducibility and representativeness of the sampling method remain unclear.

The justification for the sample size is methodologically weak. The manuscript states that Cochran's formula was used and that "200 participants were considered for each group," yet no parameters for the calculation (confidence level, margin of error, estimated variance, or effect size assumptions) are reported. The authors should provide the exact formula inputs and resulting calculations.

In the description of Lynch et al.'s Sleep Disorders Questionnaire, the reported content validity coefficient of "0.35" appears unusually low for acceptable validity. The authors should verify whether this value is correct, clarify which type of content validity index was used, and explain how such a value was interpreted as acceptable. This issue raises concerns regarding the psychometric adequacy of the instrument.

The manuscript reports Cronbach's alpha coefficients for all instruments, but no evidence of construct validity within the present sample is provided. Because these measures were applied to adolescents in a specific cultural and educational setting, the authors should report confirmatory factor analysis results or at least provide evidence supporting the dimensional structure of the scales in the current sample.

The rationale for using nonparametric tests for cognitive intelligence is insufficiently detailed. The authors state that IQ scores were non-normally distributed but do not report the Kolmogorov–Smirnov statistics, skewness/kurtosis values, or visual inspection results. Furthermore, given the large sample size ($n = 400$), the authors should justify why MANOVA robustness assumptions were considered inadequate for this variable.

Table 2 presents a multivariate analysis including six dependent variables, yet the manuscript conceptually introduces only three main variables. It appears that sleep disorder subscales were entered separately, but this analytical decision is not explicitly

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justified in the Methods section. The authors should clarify the exact structure of the MANOVA model and explain why both total sleep disorder scores and subscales were simultaneously analyzed.

Authors uploaded the revised manuscript.

2. Revised

Editor's decision after revisions: Accepted.
Editor in Chief's decision: Accepted.

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