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Effects of a Trauma-Informed Mindfulness-Based Stress Reduction Program on Social Anxiety and Sleep Quality Among War-Exposed Secondary-School Students

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1. Round 1

1.1. Reviewer 1

Reviewer:

The paragraph describing recruitment across "six secondary schools and their affiliated counseling services in Tehran, Iran" requires considerably more information about the sampling frame and recruitment procedure. Please explain how the six schools were selected, whether they represented particular districts, public or private schools, girls' schools, boys' schools, or mixed educational settings, and how potentially eligible students were initially identified. The manuscript should also report the number of students screened, the number excluded at each eligibility stage, the number declining participation, and the reasons for exclusion or refusal. A CONSORT-style participant flow description is needed because reporting only that "a total of 120 eligible students were randomized" does not permit assessment of recruitment bias or external validity.

The eligibility statement that participants "reported clinically meaningful symptoms of social anxiety and/or poor subjective sleep on a screening assessment" is insufficiently operationalized. The authors should specify the exact screening instruments,

cut-off scores, and decision rules used to determine eligibility. In particular, it is unclear whether students could enter the trial solely because of elevated social anxiety, solely because of poor sleep, or because of both, which may create substantial baseline heterogeneity in the two co-primary outcomes. Please report how many participants qualified through social anxiety only, sleep disturbance only, and both criteria, and consider whether eligibility status should be included in sensitivity or subgroup analyses.

The manuscript reports that intervention groups contained "approximately eight to ten students" and that sessions were led by clinicians trained in mindfulness and trauma-responsive work, yet therapist effects and group clustering are not adequately addressed. Students receiving the intervention were nested within therapy groups and likely within facilitators, creating non-independence beyond repeated observations within individuals. The statistical plan states only that clustering by school and intervention group was considered in sensitivity analyses. Given the design, this issue should be treated as a primary analytical concern rather than an optional sensitivity analysis. Please report the number of intervention and control groups, number of facilitators, allocation of facilitators across groups, intraclass correlation coefficients where estimable, and analyses accounting for school and treatment-group clustering.

The active control condition is described as including "balanced nutrition, everyday physical activity, digital citizenship, creative interests, healthy routines, communication with school resources, and general well-being." The authors should provide greater evidence that the comparator was structurally equivalent to the experimental intervention. In particular, please report facilitator qualifications, group size, session attendance, homework expectations, use of discussion or experiential exercises, and whether expectancy and credibility were measured in both conditions. The phrase "healthy routines" also raises the possibility that the control program indirectly addressed sleep behavior. The authors should clarify the exact content to establish that the comparator did not contain elements likely to directly improve PSQI scores.

In the measures section, the authors correctly acknowledge that the Persian SPIN evidence cited previously was not adolescent-specific; however, the statement that "item wording was reviewed with a small group of students" is methodologically vague. Please report the number and characteristics of adolescents involved in cognitive interviewing, the procedure used, whether any wording was changed, and whether those students subsequently entered the main trial. Most importantly, provide the actual internal-consistency estimates from the present sample, including Cronbach's alpha and McDonald's omega with confidence intervals where possible. Since social anxiety is a co-primary outcome, evidence that the instrument functioned adequately in this specific population is essential.

The PSQI paragraph requires further clarification regarding its use in adolescents and the interpretation of change scores. The manuscript states that the adolescent-appropriate Persian version was used "without changing the standard one-month reference period," yet the intervention lasted eight weeks and the postintervention assessment therefore reflects sleep during approximately the final month of treatment rather than necessarily the full intervention period. This temporal relationship should be discussed explicitly. Please also report reliability coefficients for the PSQI in the present sample, clarify whether the standard global score was calculated exactly according to the original scoring algorithm, and state whether use of sleep medication contributed meaningfully to the global score in this adolescent sample.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

In the paragraph defining war exposure, the manuscript states that qualifying experiences included "displacement, nearby explosions or military activity, damage to the family home or other property, physical injury, injury or death of a close family member or significant person, and prolonged exposure to perceived threat or insecurity." This definition is very broad and potentially combines qualitatively different exposures. Please provide the actual content, response format, scoring procedure, and development process for the "brief structured event-history checklist." The authors should also explain whether the checklist underwent expert review, pilot testing, or content-validity assessment, and how "prolonged perceived threat" was

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distinguished from general anxiety. Without such detail, the trauma-burden variable used as a covariate is difficult to interpret or reproduce.

The safeguarding paragraph is commendably detailed, but the criterion stating that "a licensed mental-health professional" conducted screening for imminent self-harm, dissociation, psychosis, and mania needs greater methodological precision. Please specify the professional qualifications of the assessors, the structured or semi-structured instruments used to evaluate these risks, the thresholds triggering exclusion or referral, and the procedures followed when risk was identified. Given that participants were minors and trauma-exposed, the paper should also identify the ethics committee that approved the study, provide the approval code, describe parental consent and adolescent assent procedures, and explain how confidentiality was handled when a student disclosed acute risk.

The sample-size paragraph explicitly notes that "120 randomized students (60 per condition)" represented a feasible target rather than a confirmed power calculation. This is a major issue for an efficacy trial and should be revised substantially. A completed manuscript reporting inferential results should present an a priori sample-size calculation based on the intended primary analysis, anticipated effect size, alpha level, statistical power, repeated-measures correlation, expected attrition, and any design effect arising from students being treated in intervention groups and recruited from schools. If no formal a priori calculation was performed, the authors should state this clearly and avoid implying that the sample was established on statistical grounds. A post hoc power calculation would not resolve this limitation and should not be substituted for an appropriate design justification.

The randomization description states that allocation used "permuted random blocks of undisclosed variable sizes, stratified by school and baseline social-anxiety severity," but the manuscript does not define the severity strata or provide enough detail to evaluate allocation integrity. Please specify the cut-points used to stratify SPIN severity, who generated the random sequence, who enrolled participants, who accessed the allocation system, and whether assignment could be predicted or overridden. Because school was included as a stratification factor, the authors should also report whether approximately equal numbers were allocated within each school and whether any school contributed disproportionately to one condition.

The intervention paragraph describes the program as "an eight-week trauma-informed adaptation of Mindfulness-Based Stress Reduction," but the distinction between standard MBSR and the adapted program should be made more explicit. Please identify which components were retained from conventional MBSR, which were removed, which were shortened, and which trauma-informed elements were newly added. The theoretical rationale for eliminating the all-day retreat, prolonged sitting, and mandatory body scan should also be described. Because intervention reproducibility is essential, the authors should indicate whether a treatment manual exists, whether it can be obtained, and whether the intervention was developed through consultation with adolescent trauma specialists, mindfulness trainers, or students with lived experience.

Response: Revised and uploaded the manuscript.

2. Revised

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

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