

Comparison of the Effectiveness of Choice Theory-Based Education and Adolescent-Centered Mindfulness Training on Psychological Capital in Adolescent Football Players

Mohammad Ebrahim. Hellat Abadi Farahani¹, Mohammad Reza. Vajdian^{2*}, Mahdi. Davaei³

¹ Department of Psychology, Bo.C., Islamic Azad University, Borujerd, Iran

² Department of Educational Sciences, Shahid Mirshaki Aligudarz Higher Education Center, Farhangian University, Lorestan, Iran

³ Department of Educational Sciences, CT.C., Islamic Azad University, Tehran, Iran

* Corresponding author email address: mohammadrezavajdian22@gmail.com

E d i t o r	R e v i e w e r s
Thseen Nazir Professor of Psychology and Counseling Department, Ibn Haldun University, Istanbul, Turkey thseen.nazir@ihu.edu.tr	Reviewer 1: Mohammadreza Zarbakhsh Bahri Associate Professor, Department of Psychology, Tonekabon Branch, Islamic Azad University, Tonekabon, Iran. Email: M.Zarbakhsh@Toniau.ac.ir Reviewer 2: Seyed Ali Darbani Assistant Professor, Department of Psychology and Counseling, South Tehran Branch, Islamic Azad University, Tehran, Iran. Email: Ali.darbani@iau.ac.ir

1. Round 1

1.1. Reviewer 1

Reviewer:

The manuscript should clarify the distinction between random sampling and random assignment. In the Methods, the authors state that “Using purposive sampling, 45 eligible participants were selected and randomly assigned” to three groups. This is methodologically appropriate, but the implications should be explained. Because the initial sample was purposively recruited from football-related schools and programs in the Shemiranat region, the study is not a randomized population-based trial. Random assignment improves internal validity and group comparability, but purposive recruitment limits external validity. The terminology “quasi-experimental” should therefore be justified carefully, especially because participants were randomly allocated after eligibility screening. The authors should explain which element of the design necessitated the quasi-experimental classification rather than a randomized controlled experimental design.

The sample-size calculation requires substantially greater reporting detail. The Methods indicate that PASS 2023 was used with “a statistical power of 0.90 and an alpha level of 0.05” and that 13 participants per group were required. However, the assumed effect size, statistical model, number of groups, number of repeated measurements, within-subject correlation,

nonsphericity correction, and calculation module are not reported. These details are essential for reproducibility. The authors should specify the anticipated effect size and its justification, ideally with reference to prior intervention research, and explain whether the calculation was based on the primary group effect, group $\times$ time interaction, or pairwise group comparison. Reporting only power and alpha does not permit readers to evaluate whether the final sample of 45 participants was adequately powered for the reported repeated-measures analysis.

The eligibility and screening procedures require revision because the stated screening process may introduce selection bias. The manuscript indicates that adolescents were selected partly on the basis of responses to a researcher-developed screening form and that participants showing interest in improving psychological skills or experiencing certain performance difficulties were considered more suitable. Such a procedure can preferentially select participants who perceive a need for intervention and may therefore be especially responsive to psychological training. The authors should provide the exact decision rule used to determine eligibility, clarify whether every screening item was mandatory, and explain whether the form was validated. If the screening form was used solely for eligibility rather than measurement, this should be stated explicitly. Moreover, inclusion should ideally be based on predefined objective criteria rather than subjective judgments of who “needed” psychological intervention.

The demographic reporting contains an internal inconsistency that should be corrected. In the descriptive narrative for the adolescent-centered mindfulness group, the manuscript originally stated that “8 participants were enrolled in Grades 7–9 and 6 in Grades 10–12,” which sums to 14 rather than 15 participants, whereas the table indicates 8 and 7 participants, respectively. Although this was corrected in the later English findings, the authors should carefully verify all demographic values against the original dataset. Similar cross-checking should be performed for age categories, football experience, and group totals. All frequencies should sum correctly to $n = 15$ within each group, and percentages could be added to improve interpretability.

The reported chi-square analyses of baseline demographic comparability should be reconsidered. The manuscript reports, for example, $\chi^2 = 0.286$ with $df = 2$ for age and $\chi^2 = 1.95$ with $df = 4$ for football experience, although the presented demographic variables contain more than two categories and three groups. The reported degrees of freedom appear inconsistent with the contingency table structure. For a three-group comparison, the degrees of freedom should be determined by $(\text{rows} - 1) \times (\text{columns} - 1)$. Therefore, the authors should recalculate and verify the chi-square statistics, degrees of freedom, and p values. In addition, because several cells contain very small expected frequencies, the assumptions of Pearson's chi-square test may be violated; an exact test or category consolidation may be more appropriate.

The measurement section concerning the Psychological Capital Questionnaire requires clarification regarding the instrument's origin, scoring, and construct validity in adolescent athletes. The sentence stating that the questionnaire was “developed by McGee and colleagues” should be supported by a precise bibliographic citation, and the authors should verify whether the instrument was originally developed for employees or another adult population. If so, the use of this instrument among 12- to 18-year-old football players requires stronger justification. The manuscript should provide evidence that the language and factor structure are developmentally appropriate for adolescents and, ideally, report reliability coefficients calculated from the present sample. Reliabilities reported from previous Iranian studies are useful but cannot substitute for internal consistency estimates in the current dataset.

A major psychometric issue concerns the magnitude of the reported psychological capital scores. According to the Methods, the questionnaire contains 26 items scored from 1 to 6, producing a theoretical total score of 26–156. However, the means reported in Table 1 range from approximately 26.80 to 42.20, which are very low for a 26-item six-point scale and are difficult to reconcile with the stated scoring procedure. The authors must verify whether Table 1 presents total scores, transformed scores, mean subscale scores, or scores from a different instrument version. If the total psychological capital score was calculated as described, a pretest mean of 26.80 would imply an average response extremely close to the minimum possible value on nearly every item. This apparent scoring inconsistency is substantial and should be resolved before the results can be interpreted confidently.

The intervention protocols differ in dosage, which creates a potentially important confounding factor. The Choice Theory group received eight 90-minute sessions, while the adolescent-centered mindfulness group received ten 90-minute sessions. Nevertheless, the manuscript interprets the direct between-group difference as evidence that Choice Theory-based education was intrinsically more effective. Because treatment exposure was not matched, differences in duration, content density, homework requirements, therapeutic alliance, and participant engagement may influence the comparative result. The authors

should acknowledge this design issue more prominently and avoid attributing the superiority exclusively to theoretical mechanisms. Future research should use interventions matched for total contact time, frequency, facilitator exposure, and homework demands.

The manuscript does not provide adequate information about intervention fidelity. For both intervention paragraphs, detailed session content is presented, but there is no information regarding who delivered the interventions, the facilitator's professional qualifications, whether the same facilitator administered both programs, whether a treatment manual was used, or whether adherence to the protocol was monitored. These factors are essential in intervention research because differences in facilitator competence or enthusiasm can create systematic group effects. The authors should describe interventionist training, supervision, fidelity checklists, session attendance, homework adherence, and any procedures used to ensure that the two intervention conditions were implemented as intended.

The control condition is described simply as receiving “no intervention during the study period.” The authors should specify whether this was a passive control, wait-list control, treatment-as-usual condition, or no-contact control. A passive control does not control for expectancy effects, attention from facilitators, group interaction, social support, or repeated engagement with psychological content. Consequently, the superiority of both active interventions over the control condition cannot be attributed exclusively to their specific theoretical components. The discussion should acknowledge nonspecific intervention effects, and the methods should state whether control participants were offered one of the programs after completion of the follow-up assessment.

The statistical analysis should be reconsidered with respect to the structure of the design. The manuscript uses repeated-measures ANOVA across pretest, posttest, and follow-up and reports both within-subject and between-subject effects. This is reasonable, but the Methods also refer to multivariate covariance-based analyses, while the Findings ultimately emphasize repeated-measures ANOVA. The authors should ensure that the statistical plan is described consistently and only analyses actually conducted are reported. Given the modest sample size, three repeated measurements, and potential baseline variation, a linear mixed-effects model or ANCOVA-based posttest comparison controlling for baseline may offer greater flexibility and robustness. At minimum, the authors should explain why repeated-measures ANOVA was selected as the primary model.

The manuscript should strengthen its reporting of ethical procedures. The Methods state that “written informed consent from the participants' parents” was obtained, but because participants were minors aged 12–18 years, the authors should also state whether adolescent assent was obtained. The manuscript should report the name of the approving institutional ethics committee, ethics approval code, approval date if required by the journal, procedures for confidentiality and data protection, and any trial or study registration if applicable. It should also clarify whether participants were informed of their right to withdraw without consequences and whether any adverse psychological responses during the interventions were monitored.

The Discussion should more explicitly address the generalizability of results to the athletic context. The present sample appears to consist of adolescent football players recruited from schools or training settings in one geographical region, but the manuscript does not report competitive level, weekly training volume, playing position, team status, or whether participants were recreational, academy-level, or competitive athletes. These characteristics could influence both baseline psychological capital and responsiveness to psychological intervention. The authors should either provide these data or acknowledge their absence as a limitation, because the term “adolescent football players” encompasses a highly heterogeneous population.

Authors uploaded the revised manuscript.

1.2. Reviewer 2

Reviewer:

The assumptions section should be streamlined and statistically refined. The manuscript states that “the Shapiro–Wilk test showed no significant deviation from normality” and then reports Levene's test, Box's M test, polynomial trends, Mauchly's test, and Wilks' Lambda. Several of these checks are presented as prerequisites when they are not all required in the manner described. For repeated-measures ANOVA, normality of residuals, absence of influential outliers, homogeneity of between-group variances, and sphericity are central assumptions. The manuscript should also report whether extreme outliers were

examined. In addition, significant polynomial trends are not an assumption of repeated-measures ANOVA but rather a form of analysis of change patterns; this should not be presented as an assumption test.

The interpretation of partial eta-squared requires greater precision. The manuscript states that “approximately 78.7% of the variance associated with within-participant changes over time [was] attributable to the time effect” and that “49.2% of the between-group variance in psychological capital was associated with group membership.” Partial η^2 should not be interpreted straightforwardly as the percentage of total observed variance explained in the same manner as R^2 . It represents the proportion of effect-plus-error variance attributable to a specific effect after excluding variance associated with other effects in the model. The authors should revise these statements and describe .787, .663, and .492 as very large effect sizes without translating them into potentially misleading percentages of total variance.

The post-hoc comparison strategy should be explained more clearly. Table 3 combines Scheffé-adjusted between-group comparisons with Bonferroni-adjusted between-time comparisons, but the rationale for using two different adjustment methods is not provided. The manuscript should specify in the Data Analysis section which post-hoc procedure was planned for each effect and why. More importantly, the between-group mean differences appear to be based on marginal means averaged across measurement occasions, but this is not explicitly stated. Given the significant time $\times$ group interaction, averaging group differences across all time points can be difficult to interpret because pretest values were similar while posttest and follow-up values diverged markedly. Simple-effects comparisons at posttest and follow-up, preferably adjusted for baseline, would provide a clearer assessment of intervention superiority.

The interpretation of the significant difference between Choice Theory-based education and adolescent-centered mindfulness training should be made more cautious. The Discussion currently states that “Choice Theory-based education appears to produce a greater improvement” and subsequently attributes this to closer conceptual correspondence between Choice Theory and psychological capital. Although this is a plausible explanation, the observed mean difference was relatively modest and yielded $p = .044$, very close to the conventional .05 threshold. Given the small sample size and multiple statistical comparisons, the authors should avoid presenting this result as definitive evidence of superiority. Reporting confidence intervals for the between-intervention contrast and considering the clinical or practical magnitude of the difference would substantially strengthen the interpretation.

The discussion paragraph beginning “One possible explanation for the greater effectiveness of Choice Theory-based education lies in its emphasis on personal responsibility, internal control, purposeful choice, and behavioral evaluation” is conceptually coherent, but the explanation remains largely inferential because the proposed mechanisms were not directly measured. Perceived control, agency, goal orientation, behavioral responsibility, and internal locus of control were not assessed as mediators. Similarly, the mindfulness mechanisms proposed later, including attentional flexibility and present-moment awareness, were not measured. The authors should explicitly distinguish empirical findings from theoretical explanations and state that these mechanisms remain hypotheses requiring mediation analysis in future research.

The interpretation of maintenance effects requires qualification. The sentence “the absence of a significant posttest-to-follow-up decline indicates that the improvements in psychological capital achieved following the interventions were maintained over the two-month follow-up period” is reasonable, but failure to detect a significant difference does not necessarily establish statistical equivalence or noninferiority between posttest and follow-up. With only 15 participants per group, the study may have limited power to detect modest deterioration. The authors should describe the scores as showing no statistically detectable decline rather than definitively demonstrating complete maintenance. A longer follow-up and equivalence-based analysis would provide stronger evidence of sustained effects.

Authors uploaded the revised manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.