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Effectiveness of an Indigenous Life Skills Training Program on Cognitive Emotion Regulation Strategies and the Course of Interpersonal Relationships (Coach–Athlete–Parent) in Adolescent Football Players: A Pretest–Posttest Study with Follow-Up

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1. Round 1

1.1. Reviewer 1

Reviewer:

The participant description requires greater precision regarding sampling and participant flow. The manuscript states that “the life skills training program was delivered to all 25 players who were actively participating in the academy at the beginning of the study,” but only 22 were included in the final statistical analyses. The manuscript should explicitly report why each of the three participants was excluded, preferably according to the predefined exclusion criteria, such as excessive absence, injury, or incomplete questionnaire data. It should also indicate whether exclusions occurred before or after particular measurement occasions and whether any data were missing before exclusion. Because complete-case analysis can introduce attrition bias, the authors should report whether excluded participants differed from retained participants on available baseline characteristics or pretest scores.

The manuscript should provide a more complete demographic and sport-related description of the final sample. At present, participants are described primarily as "22 male adolescent football players" aged 14 years. For reproducibility and assessment of external validity, the authors should report the mean age and standard deviation if age varied within the 14-year category, years of formal football experience, weekly training frequency, competitive level, playing positions, and, where available, relevant educational or socioeconomic characteristics. The statement that inclusion required "at least two years of experience in structured sports participation" should also be supported by an actual descriptive statistic for sport experience in the analyzed sample.

The reported reliability coefficients should be tied explicitly to a measurement occasion. The manuscript states that "Cronbach's alpha coefficients calculated using the current sample were .86 for the total instrument, .83 for the positive processes dimension, and .79 for the negative processes dimension," with analogous values for the CERQ. In a six-wave longitudinal study, internal consistency may vary across measurement occasions. The authors should state whether these coefficients were calculated at pretest only, averaged across waves, or estimated using pooled observations. Preferably, reliability should be reported separately for each major measurement occasion or, at minimum, at baseline and final follow-up. Because the sample is small, confidence intervals around reliability estimates would also improve interpretive caution.

The normality testing strategy should be reconsidered and aligned with the Methods section. The Methods section specifies the "Shapiro–Wilk test," whereas Table 1 reports Kolmogorov–Smirnov statistics. This inconsistency must be corrected. Given the small sample size of n = 22, the Shapiro–Wilk test would generally be more appropriate than Kolmogorov–Smirnov for evaluating univariate normality. More importantly, significance tests of normality alone are insufficient for repeated-measures analysis. The authors should supplement them with graphical inspection, such as Q–Q plots, and assessment of extreme outliers. The manuscript should also avoid interpreting nonsignificant normality tests as proof that the distributions are normal; instead, state that no statistically significant deviations from normality were detected.

The statistical assumptions section contains conceptual terminology that should be corrected. The original methodology refers to "homogeneity of variance–covariance matrices (Box's test)," but Box's M is typically relevant to equality of covariance matrices across independent groups in multivariate analyses. This study has only one group measured repeatedly, so the relevance of Box's M is questionable. The manuscript should clearly distinguish multivariate normality, sphericity, and covariance structure assumptions. For the univariate repeated-measures analysis, Mauchly's test of sphericity is the key assumption test, and because it was violated for all outcomes, corrected degrees of freedom are appropriate. The statistical methods should therefore be rewritten to correspond precisely to the actual single-group repeated-measures model.

The multivariate test statistics in the Results section appear internally inconsistent and require verification against the original SPSS output. The manuscript reports, for example, "Pillai's trace was .882, F(5, 17) = 183.62," and then states that "Wilks' lambda tests yielded the same substantive conclusions." However, the supplied table lists Wilks' lambda values such as .018 and .013 together with identical F values to the Pillai statistics. These values may reflect transcription errors because the relationship between the reported multivariate coefficients and F statistics should be verified carefully. The authors should reproduce the exact SPSS multivariate output and confirm the values for Pillai's trace, Wilks' lambda, F, hypothesis degrees of freedom, error degrees of freedom, p values, and partial eta squared before publication.

Table 2 should report complete Greenhouse–Geisser-adjusted inferential information, including denominator degrees of freedom. At present, only the corrected numerator df is presented, as in "df = 2.15" for adaptive emotion regulation, while the denominator df is omitted. Reporting F without both numerator and denominator degrees of freedom is incomplete according to standard statistical reporting conventions. The authors should present results in a form such as F(df1, df2) = value, $p < .001$, partial η^2 = value. The Greenhouse–Geisser epsilon values should also be reported because they indicate the extent of the sphericity violation and explain the adjusted degrees of freedom.

Authors uploaded the revised manuscript.

1.2. Reviewer 2

Reviewer:

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The absence of a control or comparison condition needs to be handled more carefully throughout the manuscript. Several passages use causal language, including the conclusion that the intervention "was associated with substantial increases" and earlier formulations referring to "the effectiveness of" the program. With a single-group repeated-measures design, observed changes may reflect intervention effects, but alternative explanations such as maturation, normal team development, concurrent coaching influences, repeated questionnaire exposure, seasonal changes, regression to the mean, or other academy experiences cannot be excluded. The authors should consistently use appropriately qualified language such as "changes following the intervention" or "changes associated with participation in the program" and explicitly acknowledge that causal effectiveness cannot be established as strongly as in a controlled trial.

The intervention protocol is conceptually strong but insufficiently reproducible in several practical respects. The manuscript reports that sessions were "conducted in a workshop-oriented format incorporating brief instruction, guided discussion, behavioral rehearsal, role-play, and practical simulations," but it does not specify who delivered the intervention, the facilitator's qualifications, whether the facilitator was also a team coach or researcher, whether a treatment manual was used, and how intervention fidelity was monitored. The authors should add information regarding facilitator training, session standardization, adherence checks, attendance recording, supervision, and any procedures used to ensure that the intended content was delivered consistently across all 12 sessions. Without these details, replication and evaluation of implementation fidelity remain difficult.

The justification for describing the intervention as "indigenous" or "culturally adapted" should be elaborated. The manuscript indicates that the program "had previously been developed through a qualitative participatory action research approach based on a field-based needs assessment involving adolescent football players, coaches, and parents." However, the qualitative development process is not sufficiently described to establish how cultural adaptation occurred. The authors should specify the participants involved in the formative phase, sampling strategy, number and type of interviews or focus groups, analytic procedure, principal culturally specific themes identified, and concrete examples showing how these findings modified intervention content. The term "indigenous" may otherwise appear broader than the evidence supports; "locally developed," "contextually adapted," or "culturally adapted" may be more precise depending on the actual development process.

The manuscript reports that "the methodological and content quality of the intervention package was evaluated using the Appraisal of Guidelines for Research and Evaluation II (AGREE II) framework, with scores exceeding 75% across all six quality domains." This application requires stronger justification because AGREE II was developed primarily for assessing clinical practice guidelines rather than behavioral intervention packages. The authors should explain why AGREE II was selected, who performed the ratings, how many raters participated, whether raters were independent, how domain scores were calculated, and whether inter-rater agreement was assessed. If AGREE II was adapted for the intervention-development process, the nature and rationale of those adaptations should be reported. Otherwise, a more suitable intervention-content validation procedure may need to be described.

The description of the Cognitive Emotion Regulation Questionnaire requires correction and clarification regarding scoring. The manuscript describes the CERQ as containing nine strategies categorized into "adaptive" and "maladaptive" domains, while the descriptive scores presented in Table 1 appear unexpectedly low for a five-strategy adaptive composite if all 20 adaptive items were summed using the standard 1–5 scoring. For example, an adaptive score of $M = 21.54$ at pretest would be extremely close to the theoretical minimum if calculated from 20 items. The authors should therefore state exactly how adaptive and maladaptive scores were calculated, including the number of items contributing to each composite, theoretical score ranges, whether mean scores or sums were used, whether any items were reverse scored, and whether a shortened or modified version of the CERQ was administered. The descriptive values should be checked carefully against the scoring protocol.

The psychometric description of the CERQ contains a potentially inaccurate attribution that should be verified. The Methods section states that "previous research has supported the psychometric adequacy of the questionnaire and its translated versions," whereas the original Persian text attributes validation of the Persian version to Garnefski and Kraaij (2006). Garnefski and Kraaij are associated with the original CERQ literature rather than necessarily with a Persian-language validation. The authors should cite the actual Persian validation study used in their population and provide relevant evidence for construct validity and reliability. If no appropriate Persian validation study was used, this should be acknowledged, and the translation/adaptation procedure applied in the present research should be described.

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The manuscript should provide substantially more psychometric information for the Positive and Negative Processes in the Coach–Athlete–Parent Relationships Questionnaire. The Methods section states that the Persian version was validated by "Eghbali and Badami (2021)" and reports Cronbach's alpha coefficients of .86, .83, and .79. The authors should provide the full bibliographic reference for this Persian validation and specify whether the factor structure reported in that study corresponds to the scoring approach used here. Because this instrument assesses a specialized triadic construct, it would be useful to report whether positive and negative process scores were calculated as total sums or averages, the theoretical ranges of these scores, and whether any confirmatory or exploratory evidence supports use of the instrument specifically among 14-year-old football players.

Authors uploaded the revised manuscript.

2. Revised

Editor's decision after revisions: Accepted.
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