



Effects of Eight Weeks of Integrated Neuromuscular Training on Static and Dynamic Balance, Flexibility, and Landing Errors in Male Football Players Aged 18–20 Years

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1. Round 1

1.1 Reviewer 1

Reviewer:

In the Introduction, the paragraph beginning “Non-contact lower-limb injuries commonly occur during landing, rapid deceleration, or change-of-direction tasks” appropriately identifies hazardous movement patterns, but it should distinguish more clearly between general lower-limb injury risk and anterior cruciate ligament-specific mechanisms. The current wording moves from broad lower-limb injury mechanisms to knee valgus and trunk control, which are especially relevant to ACL injury. Please specify whether the study is theoretically anchored in ACL injury risk reduction, general landing mechanics, or overall movement-quality enhancement. This distinction matters because LESS scores, balance measures, and flexibility do not capture all lower-limb injury mechanisms equally.

The Introduction paragraph stating “Flexibility may also influence landing and change-of-direction mechanics” is conceptually reasonable, but the theoretical pathway linking flexibility to LESS performance is underdeveloped. Please explain which specific flexibility limitations are expected to affect landing errors: hamstring flexibility, hip flexor mobility, ankle

dorsiflexion, posterior-chain extensibility, or lumbopelvic range of motion. Since the study used the sit-and-reach test, the manuscript should justify why this test was selected as the flexibility outcome and how it is biomechanically relevant to jump-landing mechanics in football players.

The final Introduction paragraph states that “Evidence remains limited regarding the simultaneous effects of integrated neuromuscular training on static balance, dynamic balance, flexibility, and landing errors in male football players of this age group.” This is an appropriate gap statement, but it should be sharpened by explicitly summarizing what previous studies have already examined and what they have not examined. For example, the authors should clarify whether previous research has focused mostly on female athletes, adolescent players, injury incidence, FIFA 11+, or isolated proprioceptive training. A stronger gap statement would make the novelty of examining all four outcomes in 18–20-year-old male football players more explicit.

In the Methods, the sentence “A two-group, parallel, randomized controlled design with pretest and posttest assessments was used” is clear, but the manuscript does not provide sufficient information about the randomization sequence. Please specify who generated the random sequence, which method was used, whether allocation was concealed, and whether the researcher enrolling participants was independent from the researcher assigning participants. Without these details, the term “randomized controlled” is not fully supported methodologically, and readers cannot assess whether allocation bias was adequately minimized.

In the Participants section, the manuscript reports that “Thirty-four male football players from a competitive football academy were initially randomized” and that four withdrew due to minor injuries unrelated to the intervention. Please provide a CONSORT-style flow diagram or a more detailed participant flow description, including the number assessed for eligibility, excluded before randomization, randomized to each group, lost to follow-up per group, and analyzed per group. The current description does not indicate whether attrition was balanced between groups, which is important because even small differential dropout can influence results in a sample of only 30 participants.

In the Eligibility Criteria section, the exclusion criterion “participation in additional strength or conditioning programs outside the team schedule” is appropriate, but the manuscript should describe how this was monitored. Please clarify whether participants kept training diaries, whether coaches confirmed compliance, or whether self-report was used. Since the outcomes are highly responsive to balance, strength, plyometric, and flexibility training, uncontrolled external training could substantially confound the intervention effect.

In the Landing Mechanics section, the authors report that LESS was scored by “a blinded assessor” and that intra-rater reliability was confirmed with “an intraclass correlation coefficient greater than 0.90.” Please report the exact ICC value, confidence interval, ICC model, number of videos rescored, and time interval between scoring sessions. In addition, the manuscript should clarify whether inter-rater reliability was assessed. Because LESS scoring involves subjective visual judgment, reliability information is central to the credibility of the landing-mechanics outcome.

In the Statistical Analysis section, the authors state that “One-way analysis of covariance was used to compare posttest values between groups, with the corresponding baseline value entered as a covariate.” This approach is appropriate, but the manuscript should present evidence that ANCOVA assumptions were met for each outcome, including linearity, homogeneity of regression slopes, normality of residuals, and absence of influential outliers. The current Results section only generally states that residuals were normally distributed and Levene’s test supported homogeneity. More precise reporting is needed, ideally including assumption-test statistics or a statement that all outcome-specific models satisfied assumptions.

Authors revised the manuscript and uploaded the updated document.

1.2 Reviewer 2

Reviewer:

In the Sample Size section, the manuscript states that “Assuming a medium effect size of 0.25, alpha of .05, and power of .80, the minimum required sample was 28 participants.” Please clarify the statistical model used in G*Power, the outcome selected for sample-size estimation, the assumed correlation between repeated measures if applicable, and whether the effect size was based on previous studies or convention. Since the final analysis used ANCOVA rather than repeated-measures ANOVA, the sample-size calculation should correspond to the actual primary analysis model or be justified clearly.

In the Intervention section, the authors state that the experimental group performed “balance, core-stability, strength, plyometric, landing-control, unilateral and bilateral movement, and reactive-agility exercises.” This description is useful but still insufficient for replication. Please provide a detailed intervention table showing exercises, sets, repetitions, duration, rest intervals, progression criteria, intensity, feedback cues, and weekly changes across the three phases. Without this level of detail, practitioners and researchers cannot reproduce the program, and the intervention cannot be evaluated adequately for dose, progression, or fidelity.

In the Intervention section, the control group is described as continuing “usual football training, including technical and tactical practice, small-sided games, general physical preparation, and conventional warm-up activities.” This control condition needs more detail. Please describe the content, duration, frequency, and intensity of the usual warm-up and general physical preparation. If the control warm-up included stretching, balance drills, agility, or plyometrics, this could reduce the contrast between groups; if it did not, the intervention effect may partly reflect additional structured attention rather than integrated neuromuscular training specifically.

In the Outcome Measures section, the Dynamic Balance paragraph states that reach distances were normalized to lower-limb length and a composite score was calculated, but the Results table reports dynamic balance in centimeters. Please resolve this inconsistency. If normalized composite scores were used, the unit should be percentage of limb length or normalized score; if raw reach distance was used, the normalization procedure should be removed or explained separately. This issue is important because a reported adjusted difference of +35.9 cm appears unusually large and may reflect either a summed composite score or a unit-labeling problem.

In the Static Balance section, the manuscript states that the “best of three trials was recorded in seconds.” Please justify the use of the Stork Balance Stand Test as a static balance measure in competitive football players and report its reliability in the present study or cite established reliability data. Because field balance tests may show learning effects and ceiling effects in trained athletes, it would be useful to explain whether familiarization trials were sufficient and whether test administrators were blinded to group allocation.

In the Flexibility section, the sit-and-reach test is used to assess “posterior thigh and lumbopelvic region” flexibility, but the Results section does not present the actual pretest and posttest flexibility values in the visible tables. Please include descriptive statistics and ANCOVA results for flexibility, including group means, standard deviations, adjusted mean difference, confidence interval, F statistic, p value, and partial eta squared. Since flexibility is included in the title, objective, conclusion, and discussion, its statistical reporting must be as complete as that of dynamic balance and LESS.

Authors revised the manuscript and uploaded the updated document.

2. Revised

Editor's decision after revisions: Accepted.

Editor in Chief's decision: Accepted.