

**STATISTICAL ANALYSIS OF CHANGES IN INDICATORS OF
COLLECTIVE EFFICACY IN SPORTS, VOLITIONAL QUALITIES, AND
TEAM COHESION WITHIN THE EXPERIMENTAL AND CONTROL
GROUPS**

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ABSTRACT

Background. Collective efficacy, volitional self-regulation, and team cohesion are interrelated but non-identical components of coordinated athletic functioning. An intervention may improve these outcomes only if individual changes are interpreted in the context of team membership. **Objective.** To demonstrate a statistically transparent pretest–posttest comparison of experimental and control conditions on three normalized psychological indices. **Methods.** A synthetic dataset representing 218 university student-athletes nested in 24 teams (12 teams and 109 athletes per condition) was generated for an eight-week, four-stage team development protocol. Collective efficacy and cohesion were linked conceptually to the CEQS and GEQ; volitional qualities were represented by a proposed eight-item index requiring external validation. Outcome differences were evaluated using team-level Welch tests, Holm adjustment, and baseline-adjusted models. **Results.** Demonstration data favored the experimental condition for collective efficacy (between-group difference in change, 6.65 points, 95% CI 2.34–10.95), volitional qualities (10.79, 7.96–13.62), and cohesion (5.31, 1.82–8.80). These values are illustrative outputs, not observed treatment effects. **Conclusion.** The framework supports a

reproducible way to examine psychological change while protecting against individual-level pseudoreplication.

Keywords: collective efficacy; volition; team cohesion; student-athletes; cluster analysis; team building; change scores.

RESEARCH INTEGRITY NOTE. No original participant measurements were supplied. Every numerical outcome, uncertainty interval, and plotted trajectory in this manuscript is generated from simulated data and must be replaced before empirical submission.

1. INTRODUCTION

Competitive performance in team sports depends not only on motor skills, technical preparedness, and physiological capacity, but also on athletes' expectations about their collective competence, willingness to sustain effort under difficulty, and capacity to remain integrated around common objectives. Social cognitive theory positions collective efficacy as a shared judgment about what a group can accomplish through coordinated action [1]. In sports teams, this belief is distinguishable from individual confidence and may operate through preparation, persistence, and collective adjustment [2].

The central methodological question is whether improvements after a structured psychological intervention exceed changes attributable to routine training, maturation, repeated assessment, or contextual events. A credible answer requires consistent measurement, comparable groups, recognition that players share team-level experiences, and explicit reporting of effect sizes and uncertainty rather than reliance on statistical significance alone.

2. THEORETICAL BACKGROUND AND CONCEPTUAL MODEL

2.1. Collective efficacy in sports

The Collective Efficacy Questionnaire for Sports (CEQS) conceptualizes athletes' shared confidence through five related domains: ability, effort, preparation, persistence, and unity [2]. Its application is consistent with reviews identifying collective efficacy as an important correlate of team functioning, while also warning that instrument validation must be verified for the specific language, country, sport, and level of competition [3]. The present framework considers efficacy an emergent team-relevant perception rather than a simple synonym for positive mood or social harmony.

2.2. Volitional qualities and action regulation

Volitional qualities refer here to the capacity to translate intentions into sustained goal-directed behavior under fatigue, uncertainty, distraction, and interpersonal pressure. The theoretical distinction between self-control and self-regulation developed in volitional psychology [4] is especially useful for differentiating externally demanded persistence from flexible and self-endorsed regulation. In the proposed intervention, volitional components are trained through implementation intentions, pressure-specific response routines, progressive challenge, and post-task self-reflection. Because the adapted eight-item Volitional Qualities Index (VQI) is not a published validated scale, it should be regarded as a prototype and subjected to psychometric examination before field use.

2.3. Team cohesion as a multidimensional construct

The Group Environment Questionnaire (GEQ) distinguishes group integration and individual attraction to the group across task and social dimensions [5]. This distinction is decisive: a team may be socially close yet poorly coordinated around sporting objectives, or highly task-focused without

uniformly high interpersonal affiliation. Longitudinal research indicates that cohesion and collective efficacy may contribute to resilient team functioning, but observational associations do not establish a causal sequence [6].

2.4. Research logic and hypotheses



Figure 1. Conceptual and analytic pathway. The arrows denote a hypothesized intervention logic, not statistically proven mediation.

The first hypothesis (H1) proposes a larger increase in normalized collective efficacy in experimental teams compared with control teams. H2 predicts the same direction for the volitional index, and H3 for perceived team cohesion. The intervention is expected to support all three outcomes, but the estimated size of change may differ because belief, self-regulation, and team identification represent distinct processes. The hypotheses concern between-condition changes, not simply pretest–posttest improvement within the intervention group.

3. MATERIALS AND METHODS

3.1. Design, unit of allocation, and illustrative sample

The demonstration uses a parallel, two-condition, pretest–posttest structure with existing sports teams treated as assignment clusters. The synthetic sample comprises 218 student-athletes, approximately 18–22 years of age, distributed across 24 teams. Twelve teams form the experimental condition and 12 the control condition; each arm contains 109 athletes (one team of 10 and 11 teams of 9). The data do not correspond to recruited persons or actual

institutions, and the simulation does not establish that random assignment, consent, or measurement occurred.

Table 1. *Analytic design and simulated participant distribution.*

Design component	Experimental condition	Control condition
Teams (clusters)	12	12
Athletes	109	109
Team size	9–10	9–10
Observation points	Baseline; week 8	Baseline; week 8
Training exposure	Usual sport practice + protocol	Usual sport practice only
Outcome coverage	Three indices, 0–100	Three indices, 0–100

Note. The two observations per athlete are paired. Team membership is maintained throughout the analysis, and all 218 synthetic athlete records are complete by construction.

3.2. Eight-week, four-stage experimental program

Stage I (weeks 1–2): psychological diagnosis, discussion of collective standards, shared goal formulation, and role mapping. Stage II (weeks 3–4): structured cooperative assignments, communication exercises, explicit task dependencies, and mutually agreed performance indicators. Stage III (weeks 5–6): high-pressure tactical scenarios, self-regulatory cue routines, error recovery, persistence drills, and supportive peer feedback. Stage IV (weeks 7–8): competition-like integration tasks, reflection on successes and failures, team feedback, and transfer planning. The comparison condition continues ordinary technical, tactical, and physical training without additional structured psychological sessions.

3.3. Implementation standards for a real trial

In an actual study, eligibility criteria, sport type, years of practice, baseline performance, sex/gender distribution, coaching arrangements, and team scheduling should be prospectively recorded. Team assignment should be randomized or, if randomization is infeasible, the quasi-experimental allocation rule and likely confounding should be explained. Sessions should follow a manual and include attendance and fidelity checklists. Evaluators should be blinded where practical; preregistration should specify the primary outcome and planned statistical contrasts [7].

4. PSYCHOMETRIC AND STATISTICAL PROCEDURES

4.1. Operationalization of outcome variables

Collective efficacy is operationalized by the CEQS conceptual structure (20 items; ability, effort, preparation, persistence, unity) [2]. Team cohesion draws on the GEQ framework (18 items; individual attractions–task/social and group integration–task/social) [5]. For this methodological demonstration, score direction is harmonized so that higher values consistently reflect stronger levels of the target construct. The VQI is a proposed eight-item combination of goal maintenance, effort mobilization, perseverance under setbacks, and emotional-volitional control; the index is not claimed to possess established reliability or construct validity.

For a response scale with theoretical lower bound L and upper bound U , normalized score = $100 \times (\text{mean item response} - L) / (U - L)$. Thus, a 1–9 CEQS or GEQ mean maps using $L = 1$ and $U = 9$, while the proposed 1–5 VQI uses $L = 1$ and $U = 5$. Raw subscale results must be retained in empirical work and reverse-coded items handled exactly as prescribed in their manuals. A normalized composite facilitates graphical comparison but does not equate the constructs psychometrically.

4.2. Outcome estimation and control of dependence

For athlete i in team j , change is $\Delta_{ij} = Y_{ij, \text{week8}} - Y_{ij, \text{baseline}}$. Athlete-level means and standard deviations summarize distributions; inferential contrasts use each team's mean change. The primary estimand is the difference between the 12 experimental team-change means and the 12 control team-change means. Welch's unequal-variance t statistic and corresponding 95% confidence interval quantify this contrast, avoiding the false precision that would result from incorrectly treating 218 participants as 218 independent experimental units. Holm's family-wise procedure adjusts the three primary hypothesis tests.

As a complementary analysis, each team's posttest mean is regressed on condition and its baseline team mean. The experimental coefficient is reported with HC3 heteroskedasticity-robust standard errors and a two-sided confidence interval. Hedges' g standardizes the primary difference using the pooled standard deviation of team mean changes. This cluster-level standardized effect is not directly interchangeable with an individual-level standardized mean difference. Analyses follow a two-sided $\alpha = .05$ threshold.

4.3. Assumptions, missing data, and safeguards

With only 12 clusters per arm, normality and unequal team-level variances deserve attention; confidence intervals and raw cluster distributions are more informative than a p -value alone. The synthetic dataset contains no missing observations, but a real trial must describe nonresponse by arm, reasons for attrition, missing-data assumptions, and a prespecified sensitivity analysis. Statistical analysis should use reproducible syntax, versioned code, unique cluster IDs, an item scoring key, and clear records of all deviations. If allocation is not randomized, baseline-adjusted differences remain associations conditional on measured covariates and cannot automatically establish causality [7,8].

4.4. Simulation disclosure

Numbers were generated with a fixed random seed (20261022), correlated baseline components and team-specific change components. Both conditions were assigned modest positive expected trends; the experimental arm was assigned larger changes. Therefore, favorable outcomes were embedded in the data-generation mechanism and should never be interpreted as independent verification of the intervention. Computed confidence intervals represent repeated-sampling uncertainty under this invented model only.

5. RESULTS: DESCRIPTIVE CHANGE

5.1. Baseline and post-intervention levels

Table 2 provides individual-level descriptive statistics for three normalized outcomes. Baseline values are broadly similar, although the control arm begins somewhat higher on team cohesion and the experimental arm slightly higher on volitional qualities. These imbalances illustrate why the primary interpretation should focus on between-condition change rather than posttest rankings alone.

Table 2. Descriptive means and standard deviations at baseline and week 8 (simulated values).

Indicator	Group	Baseline, M $\pm$ SD	Week 8, M $\pm$ SD	Mean Δ
Collective efficacy	Exp.	52.23 $\pm$ 7.09	62.76 $\pm$ 10.83	+10.52
Collective efficacy	Ctrl.	52.49 $\pm$ 7.09	56.48 $\pm$ 9.69	+3.98
Volitional qualities	Exp.	55.38 $\pm$ 8.03	68.44 $\pm$ 10.65	+13.06
Volitional qualities	Ctrl.	53.76 $\pm$ 7.64	56.07 $\pm$ 10.36	+2.31
Team cohesion	Exp.	51.62 $\pm$ 7.26	60.98 $\pm$ 9.78	+9.35
Team cohesion	Ctrl.	53.65 $\pm$ 7.68	57.73 $\pm$ 10.51	+4.08

M = individual-level arithmetic mean; SD = individual-level standard deviation; Δ = posttest minus baseline; Exp. = experimental; Ctrl. = control. Each group contains $n = 109$ athletes. Descriptive SDs do not account for team clustering.

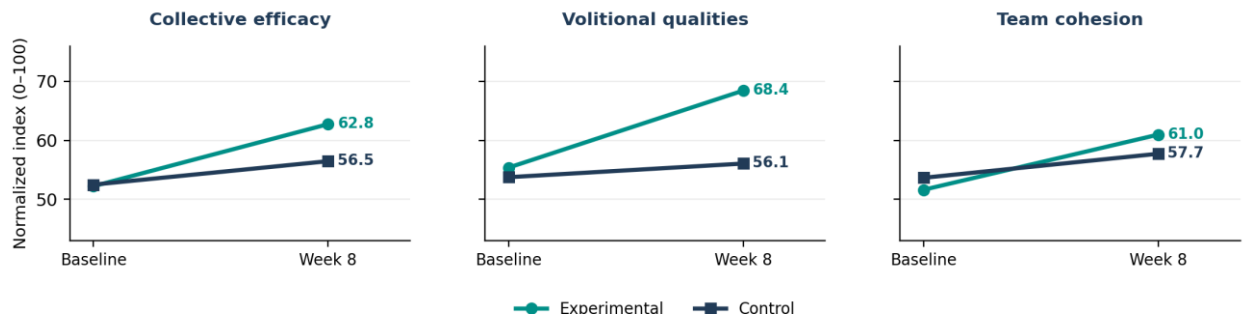


Figure 2. Mean trajectories from baseline to week 8. Values on the right represent posttest means; no real athlete outcomes are depicted.

5.2. Interpretation of descriptive patterns

The experimental sample increases from 52.24 to 62.76 points on collective efficacy (+10.52), while the control sample increases from 52.49 to 56.48 (+3.98). Volitional qualities show the largest experimental increase, from 55.38 to 68.44 (+13.06), compared with 53.76 to 56.07 (+2.31) in the control arm. Team cohesion moves from 51.63 to 60.98 (+9.35) in the experimental arm and from 53.65 to 57.73 (+4.08) in the control arm. These patterns are consistent with an intervention hypothesis only within the simulated example; alternative explanations have not been

Conclusions

First, a cluster-sensitive pretest–posttest framework can compare changes in collective efficacy, volitional qualities, and team cohesion without conflating descriptive athlete counts with independent experimental units. Second, the worked example yields simulated differences favoring the intervention in all three indices, with confidence intervals and multiplicity-adjusted tests reported transparently. Third, baseline-adjusted cluster models provide a useful complementary check but do not remove the need for credible assignment and

valid measurement. Fourth, the theoretical distinction between shared efficacy beliefs, individual volition, and cohesion should be preserved in both intervention design and statistical interpretation. The present manuscript is suitable as a methodological draft only; empirical publication requires replacement of every synthetic result by verified research data.

Data-availability and authorship statement

Data availability: reproducible synthetic scores and a statistics script were generated for the demonstration and contain no personal records. Funding, authorship, institutional affiliation, and ethics approval have not been assigned or claimed. The figures and tables are original analytical illustrations created for this draft.

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