

**GAME-BASED LEARNING AND FUNDAMENTAL BADMINTON
SKILLS IN YOUNG ATHLETES: A PRETEST-POSTTEST
COMPARISON USING SIMULATED DATA****Ade Zalindro¹, Rudyanto Rudyanto², Ardo Okilanda³**Universitas Negeri Padang^{1,2,3}
zalindro@fik.unp.ac.id**Abstract**

Fundamental skill acquisition in badminton requires coordinating stroke technique with perception, timing, and decision making. This manuscript demonstrates a pretest–posttest analysis pipeline for comparing a game-based learning condition with conventional instruction in athletes aged 11–13 years. The supplied dataset contains 30 simulated cases (game-based $n = 15$; conventional $n = 15$) with a composite basic badminton skill score (0–100) measured before and after an instructional period. Paired-samples t tests were used to describe within-condition change, while the primary between-condition contrast used gain scores (posttest minus pretest), supplemented by Welch and Mann–Whitney tests; ANCOVA was examined as a sensitivity analysis. In the simulated dataset, the mean gain was 15.27 points ($SD = 2.66$) in the game-based condition and 5.80 points ($SD = 1.37$) in the conventional condition, producing a between-condition gain difference of 9.47 points (95% CI [7.88, 11.05]). However, these values are generated from simulated data and therefore cannot establish efficacy, causal effects, clinical or practical significance, or generalisability. For an empirical submission, the present manuscript must be populated with verified participant-level observations, a prespecified allocation procedure, a fully documented intervention protocol, a validated and reliable outcome measure, prospective ethics approval, and complete participant-reporting information.

Keywords: Badminton; Game-Based Learning; Young Athletes; Motor Skill Acquisition; Physical Education.

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E-Mail: zalindro@fik.unp.ac.idDOI <http://dx.doi.org/10.31851/hon.v9i2.22621>Jurnal Laman Olahraga Nusantara licensed under a [Creative Commons Attribution-ShareAlike 4.0 International License](https://creativecommons.org/licenses/by-sa/4.0/)**INTRODUCTION**

Badminton is a technically and perceptually demanding racket sport in which performance depends on the coordinated execution of strokes, footwork, court positioning, and timing under continuously changing task constraints (Phomsoupha & Laffaye, 2015). Skilled play requires players to read early kinematic and contextual cues, anticipate the opponent's intentions, and select responses within very short time windows, a capacity that differentiates expertise even among trained players (De Waelle et al., 2023). For athletes aged 11–13 years, these abilities are

acquired within a broader period of motor development, and adequate movement competence at this stage is consistently associated with sustained physical activity and further skill learning (Bourke et al., 2025; Coe et al., 2024; Holfelder & Schott, 2014). Establishing sound foundational skills during early adolescence is therefore a meaningful objective for youth badminton programmes.

Traditional badminton instruction typically emphasises demonstration, repetition, and isolated technique drills. Such practice can be efficient for grooving a movement pattern, but it tends to decouple the technical action from the perceptual and tactical information that normally regulates it, offering limited decision making, limited variability, and limited ecological correspondence to the game (Pinder et al., 2011). When practice does not preserve the informational constraints of performance, the movement solutions that emerge may not transfer to competitive play, and syntheses of instructional research indicate that decontextualised drilling is not reliably superior for the outcomes that matter most in games (Manninen et al., 2025).

Game-based learning offers a contrasting logic. Rooted in Teaching Games for Understanding (Bunker & Thorpe, 1982) and, more recently, in ecological and constraints-led accounts of skill acquisition, it places learners in simplified or modified games in which the target skill is practised while a movement problem is being solved. By retaining representative information–movement couplings, manipulating task constraints, and requiring perception–action coupling and decision making, this approach is intended to make practice both more contextualised and more engaging (Myszka et al., 2023; Pinder et al., 2011). Rather than playing instead of training, well-designed game-based tasks embed a clear technical or tactical objective within each game.

Evidence from physical education and sport pedagogy broadly supports this rationale. A multilevel meta-analysis reported that game-based approaches produce favourable effects on decision making, knowledge, and, more modestly, motor skill relative to technique-focused instruction (Manninen et al., 2025), and a separate review and meta-analysis found consistent improvements in decision making across

educational stages (González-Valero et al., 2024). A Bayesian network meta-analysis of pedagogical models ranked game-centred and hybrid models highly for tactical decision making, skill execution, and motivation, while cautioning that credible intervals overlapped (Guo et al., 2026). Meta-analyses of cooperative and other student-centred models likewise report positive effects across the physical, cognitive, affective, and social domains (Boke et al., 2025; García-Castro et al., 2026), and small-sided game formats have been associated with technical and tactical benefits in youth contexts (Fizi et al., 2023; Li et al., 2023). Developmentally, active-play and physical-activity interventions can enhance fundamental movement skills, although effects depend on dose, quality, and measurement (Ramos-Munell et al., 2025; Yang et al., 2025).

Badminton-specific evidence, while smaller, points in the same direction. A game-based training model produced significant fundamental-skill gains in young badminton athletes (Sari et al., 2025); a school-based badminton programme improved children's fundamental movement skills (Duncan et al., 2020); a modified, game-oriented badminton curriculum improved selected fitness and attention outcomes relative to a traditional curriculum (Wang et al., 2025); and technology- and game-supported approaches have improved badminton competence in physical education (González-Peño et al., 2024; Gustian et al., 2024). A bibliometric mapping confirms that badminton pedagogy is an expanding but still uneven field (Coenga et al., 2025).

Despite this convergence, the controlled evidence base for foundational badminton skills in young athletes remains limited. Studies differ in participant age, learning environment, intervention dose, and outcome instruments; many pool mixed age bands or focus on team sports; and few provide a clean, controlled contrast of game-based versus conventional instruction on basic badminton skills in the 11–13-year band specifically. There is, therefore, a need for controlled, transparently reported intervention evidence in this population that also models

rigorous statistical and reporting practice, including effect estimation and assumption checking rather than reliance on p-values alone (Xu & Dai, 2026).

The purpose of the planned empirical study is to compare changes in a primary, validated measure of fundamental badminton skill between athletes receiving game-based learning and athletes receiving conventional instruction. The intended estimand is the between-condition difference in change from pretest to posttest, with secondary analyses examining posttest performance adjusted for baseline and reporting effect sizes with confidence intervals. The hypothesis is directional: athletes exposed to game-based instruction are expected to show greater improvement than those receiving conventional instruction. For the present manuscript, this objective is demonstrated using simulated data only; no empirical conclusion is drawn.

METHOD

A two-group pretest-posttest comparison was specified to illustrate the planned empirical analysis. The supplied file contains simulated data rather than observations from recruited participants. Therefore, the manuscript does not claim that the allocation was randomised, quasi-experimental, or controlled in the empirical sense. The final empirical version must state the actual allocation mechanism (e.g., individual randomisation, cluster allocation, or intact-group assignment), whether matching or stratification was used, and whether outcome assessors were blinded. The title has consequently been revised to avoid implying a level of causal certainty that the present dataset cannot support.

The simulated analytic dataset comprised 30 cases aged 11–13 years, with 15 cases assigned to each condition and a mean age of 12.00 years ($SD = 0.85$) in both groups. The supplied file does not contain the empirical participant descriptors required for publication. The final manuscript must therefore report sex/gender distribution as collected, recruitment setting, sampling frame, previous badminton experience or training history, relevant anthropometric descriptors if collected, inclusion and exclusion criteria, recruitment procedures, attrition, and the number

screened, eligible, enrolled, allocated, and analysed. No participant characteristics should be fabricated or back-calculated from the simulated file.

The planned intervention contrast is pedagogical rather than exposure-based. In the game-based condition, athletes practise basic strokes and movement skills through modified game situations, progressing from brief technical orientation to constrained games, task variation, feedback, and short performance challenges. Examples include target games for accuracy, half-court rally challenges for consistency and placement, footwork-to-shot games, and scoring systems that reward execution and appropriate decisions. The conventional condition is intended to use demonstration and isolated technique drills. For the empirical trial, both conditions should be matched for contact time and overall training volume, while all essential intervention features are documented in a session-by-session protocol or supplementary table. The final manuscript must report programme duration, weekly frequency, session length, total dose, progression, coach qualifications, instructor preparation, adherence, treatment fidelity, contamination controls, and the exact content of the comparison condition.

The planned primary outcome is a composite fundamental badminton skill score on a 0–100 scale, with higher values indicating better performance. In the simulated dataset, one pretest and one posttest value are available for each case. The empirical manuscript must identify the instrument by name or provide the complete test protocol, including domains/items, scoring rubric, administration sequence, standardised conditions, assessor training, assessor blinding where feasible, and the evidence supporting content/construct validity, test–retest reliability, and inter-rater reliability when applicable.

Power and sample-size planning. Because the dataset is simulated, no empirical power calculation can justify $n = 15$ per condition, and the observed simulated effect ($d = 4.47$) must not be used to plan the real trial. For the empirical study, an a priori power analysis should be based on a clinically or educationally meaningful smallest effect, the primary analysis, alpha level, desired power,

expected attrition, and the allocation ratio. The resulting target sample and all assumptions should be reported before data collection.

RESULT AND DISCUSSION

The analytic sample contained 30 simulated cases aged 11–13 years, equally distributed between conditions ($n = 15$ each). Descriptive statistics are presented in Table 1. At the simulated baseline, pretest scores were similar between conditions: game-based $M = 59.47$, $SD = 4.16$; conventional $M = 58.87$, $SD = 3.64$; $t(28) = 0.42$, $p = .677$, $d = 0.15$. Because these observations are simulated, the baseline comparison is descriptive of the generated dataset and should not be interpreted as evidence of successful randomisation or group equivalence in an empirical sample.

The analytic sample comprised 30 athletes aged 11–13 years, equally distributed between conditions ($n = 15$ each), with an identical mean age of 12.00 years ($SD = 0.85$). Descriptive characteristics and skill scores are presented in Table 1. At baseline, pretest skill scores were similar across groups (game-based $M = 59.47$, $SD = 4.16$; conventional $M = 58.87$, $SD = 3.64$); the difference was small and non-significant, $t(28) = 0.42$, $p = .677$, $d = 0.15$, with homogeneous variances (Levene's $p = .623$), indicating acceptable pre-intervention comparability.

Table 1. Participant characteristics and basic badminton skill scores by group

Group	n	Age M (SD)	Pretest M (SD)	Posttest M (SD)
Game-based	15	12.00 (0.85)	59.47 (4.16)	74.73 (2.25)
Conventional	15	12.00 (0.85)	58.87 (3.64)	64.67 (4.03)

Within the simulated dataset, both conditions showed statistically significant pretest-to-posttest changes. The game-based condition increased by 15.27 points, $t(14) = 22.24$, $p < .001$, 95% CI [13.79, 16.74], $dz = 5.74$; the conventional condition increased by 5.80 points, $t(14) = 16.36$, $p < .001$, 95% CI [5.04, 6.56], $dz = 4.22$. These inferential quantities describe the simulated values only.

Both groups improved significantly from pretest to posttest (see Table 2 and Figure 1). Within-group change was 15.27 points for the game-based group, $t(14) = 22.24$, $p < .001$, 95% CI [13.79, 16.74], $dz = 5.74$, and 5.80 points for the conventional group, $t(14) = 16.36$, $p < .001$, 95% CI [5.04, 6.56], $dz = 4.22$ (see Table 3).

Table 2. Descriptive and between-group inferential statistics

Outcome	Game-based M (SD)	Conventional (SD)	M	Mean diff.	95% CI	t (28)	p	Effect
Pretest	59.47 (4.16)	58.87 (3.64)		0.60	[-2.32, 3.52]	0.42	.677	d = 0.15
Posttest	74.73 (2.25)	64.67 (4.03)		10.07	[7.63, 12.51]	8.45	< .001	d = 3.08
Gain	15.27 (2.66)	5.80 (1.37)		9.47	[7.88, 11.05]	12.25	< .001	d = 4.47

In the simulated dataset, the primary gain-score contrast favoured the game-based condition by 9.47 points, 95% CI [7.88, 11.05], $t(28) = 12.25$, $p < .001$, Cohen's $d = 4.47$ and Hedges' $g = 4.35$. Because gain-score variances were unequal (Levene's $p = .029$), Welch's $t(20.98) = 12.25$, $p < .001$, and Mann–Whitney $U = 225.0$, $p < .001$ were used as robustness checks. The simulated posttest difference was 10.07 points, 95% CI [7.63, 12.51], $t(28) = 8.45$, $p < .001$. These estimates are included to document the intended statistical workflow and must not be interpreted as empirical evidence of intervention efficacy.

The primary between-group contrast on gain scores showed a substantially larger improvement for the game-based group. The mean difference was 9.47 points, 95% CI [7.88, 11.05], $t(28) = 12.25$, $p < .001$, with a very large standardised effect (Cohen's $d = 4.47$; Hedges' $g = 4.35$). Because variances were unequal for gain scores (Levene's $p = .029$), robustness checks were consistent: Welch's $t(20.98) = 12.25$, $p < .001$, and Mann–Whitney $U = 225.0$, $p < .001$. A parallel advantage was present at posttest (mean difference 10.07, 95% CI [7.63, 12.51], $t(28) = 8.45$, $p < .001$). Shapiro–Wilk tests did not indicate departures from normality for gain scores (game-based $W = 0.975$, $p = .927$; conventional $W = 0.913$, $p = .149$).

Table 3. Within-group change from pretest to posttest

Group	Pretest M (SD)	Posttest M (SD)	Mean change	95% CI	t (14)	p	dz
Game-based	59.47 (4.16)	74.73 (2.25)	+15.27	[13.79, 16.74]	22.24	< .001	5.74
Conventional	58.87 (3.64)	64.67 (4.03)	+5.80	[5.04, 6.56]	16.36	< .001	4.22

Note. Mean change = posttest – pretest with its 95% CI. dz = standardised mean change for paired data. Values are from the supplied simulated dataset.

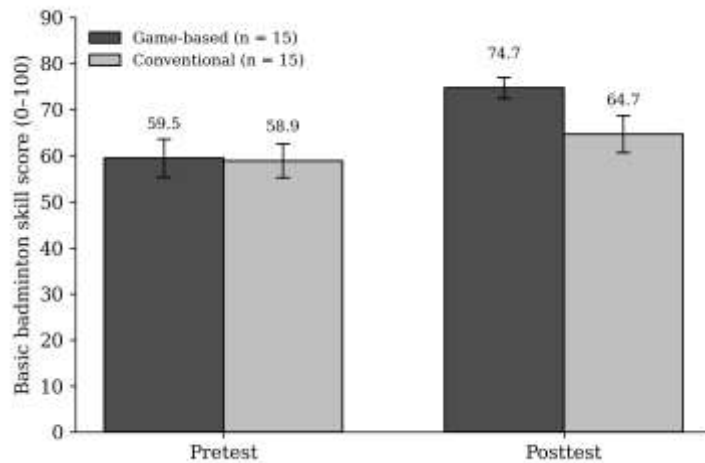


Figure 1. Mean basic badminton skill scores at pretest and posttest by group. Error bars represent ± 1 standard deviation ($N = 30$; $n = 15$ per group). Data are simulated.

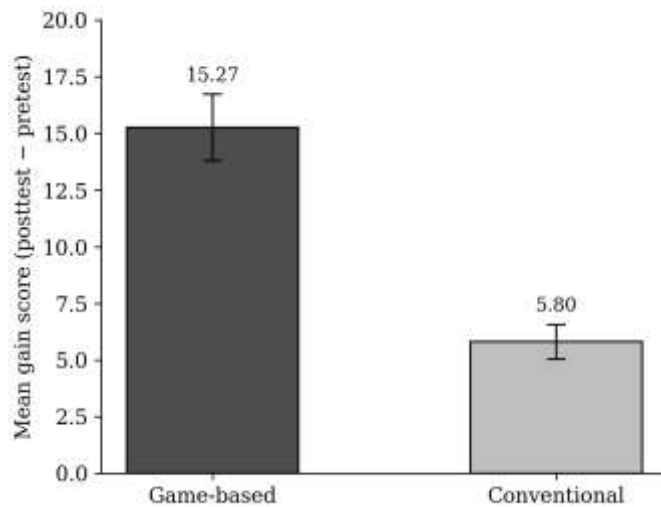


Figure 2. Mean gain score (posttest – pretest) by group. Error bars represent 95% confidence intervals of the mean. Data are simulated.

As a sensitivity analysis, ANCOVA controlling for the simulated pretest score produced a significant group term, $F(1, 27) = 214.09$, $p < .001$, partial $\eta^2 = .888$, and a significant covariate, $F(1, 27) = 65.10$, $p < .001$, partial $\eta^2 = .707$. The adjusted between-condition difference was 9.65 points, 95% CI [8.29, 11.00]. However, the group $\times$ pretest interaction was significant ($p = .0001$), violating the

homogeneity-of-regression-slopes assumption. Accordingly, ANCOVA is treated only as a sensitivity analysis of the simulated dataset, not as a definitive adjusted treatment effect.

As a sensitivity analysis controlling for baseline scores, ANCOVA showed that group remained a significant predictor of posttest score, $F(1, 27) = 214.09$, $p < .001$, partial eta squared = .888, with a significant covariate, $F(1, 27) = 65.10$, $p < .001$, partial eta squared = .707 (see Table 4). The adjusted between-group difference was 9.65 points, 95% CI [8.29, 11.00]. Importantly, the homogeneity-of-regression-slopes assumption was violated (group by pretest interaction $p = .0001$); the ANCOVA is therefore reported as convergent support for the gain-score analysis rather than as a definitive adjusted estimate.

Table 4. Analysis of covariance for posttest score (covariate: pretest)

Source	SS	df	MS	F	p	η^2p
Group	693.36	1	693.36	214.09	< .001	.888
Pretest (covariate)	210.82	1	210.82	65.10	< .001	.707
Error	87.44	27	3.24	—	—	—

Note. Adjusted between-group difference = 9.65, 95% CI [8.29, 11.00].

The homogeneity-of-regression-slopes assumption was violated (group by pretest interaction $p = .0001$); the model is reported as convergent, not definitive. SS = sum of squares; MS = mean square; η^2p = partial eta squared. Values are from the supplied simulated dataset.

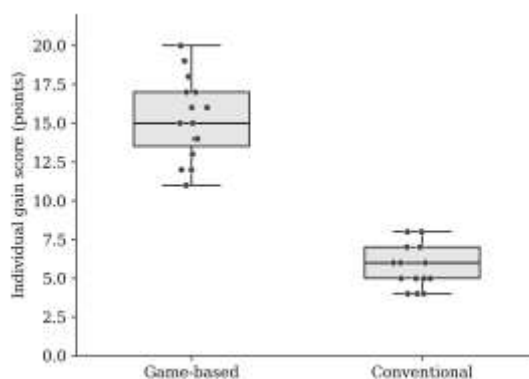


Figure 3. Distribution of individual gain scores by group (box plots with individual data points; whiskers extend to the most extreme values within $1.5 \times$

IQR). Because the data are simulated, individual points illustrate dispersion only and should not be read as empirical participant-level evidence.

Interpretation must remain strictly limited because the analysed values are simulated. The simulated dataset produces a larger gain in the game-based condition than in the conventional condition, and the statistical procedures behave as expected under the specified analysis pipeline. Nevertheless, no empirical inference about the effectiveness of game-based learning can be drawn from these values. In particular, statistical significance in generated data does not demonstrate efficacy, feasibility, acceptability, transfer to gameplay, or superiority in real young athletes.

The direction of the simulated contrast is compatible with the broader literature on game-based approaches, but it should not be treated as replication. A badminton-specific study by Sari et al. (2025) reported improvement after a game-based training model in young athletes, but used a one-group pretest–posttest design rather than a controlled comparison. More broadly, Manninen et al. (2025) synthesised 28 studies ($n = 1,600$) and found positive effects for game-based approaches on motor skill measured in skill tests, while noting heterogeneity and reporting limitations; González-Valero et al. (2024) likewise synthesised evidence for improved decision making under Teaching Games for Understanding. These studies support the rationale for a prospective trial, not validation of the simulated result.

Several mechanisms are theoretically plausible: modified games can preserve representative information–movement couplings, require perception–action coordination, and place technique within decision contexts (Myszka et al., 2023; Pinder et al., 2011). However, those mechanisms remain hypotheses for the present project because the simulated dataset contains no measures of decision making, motivation, fidelity, engagement, or transfer. The empirical trial should therefore collect outcome measures aligned with the proposed mechanism, rather than infer mechanisms from a single composite score.

The simulated effect size is exceptionally large (Cohen's $d = 4.47$) and substantially exceeds the magnitude commonly reported in educational intervention research. It is therefore a validity warning, not a result to be celebrated. Plausible explanations include the low noise and regularity of the simulated values, but the present file cannot distinguish simulation characteristics from measurement sensitivity, practice effects, instructor effects, novelty/Hawthorne effects, or allocation bias. In the empirical study, these threats should be addressed prospectively through a documented allocation procedure, standardised assessment, assessor blinding where feasible, fidelity monitoring, comparable contact time, contamination controls, and an adequately powered sample.

The magnitude of the present effect nonetheless requires caution. A Cohen's d near 4.5 implies almost complete separation between groups and is far larger than the small-to-moderate effects typical of educational interventions. Recalculation from the raw scores confirmed that the value is arithmetically correct; it is best explained by the high regularity and low noise of the simulated scores rather than by a genuinely enormous instructional effect. In real athletes, a considerably smaller and more variable effect should be expected, and observed effects should be reported with their confidence intervals without adjusting observations to reach a preferred result. A further caution is that the ANCOVA's homogeneity-of-slopes assumption was violated, so its adjusted estimate is convergent support rather than a definitive adjusted effect.

Limitations and implications

First, the current analysis uses simulated rather than participant data; consequently, it cannot establish intervention efficacy or support generalisable conclusions. Second, the empirical source file does not document allocation, recruitment, eligibility criteria, intervention dose, adherence, fidelity, assessor blinding, or the psychometric properties of the composite outcome. These omissions create risks of selection bias, instructor bias, expectancy/Hawthorne effects, testing effects, maturation effects in 11–13-year-olds, contamination

between conditions, and measurement error. Third, the single composite score prevents component-level interpretation of serving, stroke execution, footwork, placement, and game-performance outcomes. Fourth, the small simulated sample is not a basis for empirical generalisation, and its very large standardised effect should not be used for power planning. Fifth, the ANCOVA slopes assumption was violated, so adjusted estimates require caution even within the simulation. The empirical study should use a preregistered protocol, a validated multi-component assessment, transparent allocation and recruitment, fidelity checks, an a priori power analysis, assessor blinding where feasible, attrition reporting, and a retention/follow-up assessment when appropriate.

For coaches and researchers, the present manuscript offers a plausible intervention framework rather than evidence of effectiveness. The future intervention can use target-hit games, half-court rally challenges, footwork-to-shot games, and decision-oriented scoring systems, with difficulty regulated through court size, shuttle speed, target area, rules, and opponent constraints. These recommendations should be tested prospectively with transparent fidelity and outcome measurement rather than presented as effects demonstrated by the current simulated data. The limitations above define the minimum methodological corrections required before the study can be presented as empirical evidence.

CONCLUSION

No empirical conclusion about the effectiveness of game-based learning can be drawn from the simulated dataset used in this manuscript. The manuscript instead demonstrates a reproducible analysis framework for a future two-group pretest–posttest study, including baseline description, gain-score comparison, robust variance checks, effect-size estimation, confidence intervals, and sensitivity analysis. Before empirical publication, the simulated values must be replaced with verified participant observations and the manuscript must report the actual allocation mechanism, participant recruitment and eligibility, intervention and comparison protocols, validated and reliable outcome measurement, a priori

sample-size justification, treatment fidelity, ethics approval, consent and assent, attrition, data availability, and complete author/declaration information.

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