

Investigating the Effectiveness of the RhythmoGame Method in Reducing Spelling Learning Disorders among First-Cycle Primary School Students

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E d i t o r	R e v i e w e r s
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1. Round 1

1.1. Reviewer 1

Reviewer:

In the Introduction paragraph discussing cognitive predictors, the manuscript argues that “spelling performance is influenced by both domain-specific language abilities and broader cognitive capacities.” Although this provides a reasonable rationale, the transition from these cognitive predictors to RhythmoGame is presently somewhat inferential. The authors should develop a clearer mechanistic model explaining why rhythmic training would be expected to affect spelling specifically. For example, the manuscript could explicitly connect temporal sequencing in rhythm to grapheme ordering, auditory discrimination to phoneme identification, response inhibition to error monitoring, and working memory to maintenance of phonological sequences during dictation. A more explicit theoretical pathway would substantially strengthen the study rationale.

In the Introduction paragraph beginning with the discussion of phonological processing, the manuscript cites evidence for direct instruction and phonological-awareness approaches but does not sufficiently explain how RhythmoGame differs from or complements these established treatments. The sentence “Although such interventions have demonstrated value, spelling performance is not determined exclusively by phonological competence” should be expanded to identify the specific unresolved treatment gap addressed by the present study. The authors should clarify whether RhythmoGame is proposed as an alternative

treatment, an adjunctive intervention, or a multimodal method intended primarily for students who show limited response to conventional phonological and orthographic instruction.

In the Introduction paragraph on multisensory approaches, the statement that “RhythmoGame similarly involves multisensory processing because children simultaneously hear rhythmic patterns, observe demonstrations, move their bodies, use tactile and kinesthetic feedback, and respond to visual or auditory cues” is theoretically plausible but should not be presented as sufficient evidence that spelling will improve. The manuscript should distinguish between a proposed mechanism and an empirically established mechanism. It would be preferable to state explicitly that multisensory engagement is hypothesized to facilitate attentional control, sequencing, and encoding and that the present study tests the behavioral consequence of this integrated stimulation rather than demonstrating those mediators directly.

In the same Data Collection Tools paragraph, the description of the Colorado Learning Difficulties Questionnaire is too brief relative to its importance in participant screening. The manuscript should report the number of items, subscales, response format, scoring procedure, cutoff score used to identify suspected learning difficulties, and psychometric characteristics of the version administered in the present population. The statement that it “was used in the present study as an additional instrument for recognizing students at risk of learning disorders” is ambiguous because being “at risk” is not equivalent to receiving a diagnosis. The authors should therefore explain exactly how CLDQ results contributed to final eligibility decisions.

In the description of the researcher-developed spelling assessment, the manuscript reports excellent test–retest correlations for seven dimensions, but several important psychometric issues remain unresolved. The authors should explain the number of items assigned to each dimension, scoring range, direction of scoring, item-development process, administration procedure, and whether subscale scores are based on independent item sets. Moreover, content validity being “confirmed by seven teachers specializing in learning difficulties” should be supported by a formal content-validity procedure if one was used, such as CVR or CVI. Inter-rater reliability should also be reported if scoring required judgment, particularly for dysgraphia-related errors.

Authors revised the manuscript and uploaded the document.

1.2. Reviewer 2

Reviewer:

In the final two paragraphs of the Introduction, the research gap should be stated more sharply. The manuscript currently notes that “comparatively limited attention has been given to the direct application of structured rhythm-game programs for reducing spelling learning disorders,” but it does not specify whether previous studies have directly tested rhythm-based interventions for spelling, whether evidence is entirely indirect, or whether such interventions have not been examined in Iranian primary-school populations. The authors should formulate a precise gap statement immediately before the study aim, identifying what is already known, what remains unknown, and how the present quasi-experimental study addresses that specific gap.

In the Study Design and Participants paragraph, the statement that “Eligible students were allocated to an intervention group receiving the RhythmoGame program and a control group” is methodologically insufficient. The authors must specify exactly how allocation occurred. If assignment was random, the randomization procedure, sequence generation, allocation ratio, and implementation should be reported. If assignment was nonrandom, the manuscript should identify the allocation mechanism and justify the quasi-experimental terminology. This information is essential for evaluating selection bias and baseline equivalence and cannot be replaced by a general statement that participants were “allocated.”

In the Study Design and Participants paragraph, the manuscript reports a total sample of 30 participants, with 15 students in each group, but no sample-size justification is provided. The authors should report whether an a priori power analysis was conducted and specify the expected effect size, α level, statistical power, number of groups, and number of covariates used in determining the required sample. With such a small sample, the very large observed effect could be unstable, and the precision of the estimate should be carefully considered. If no formal power calculation was performed, this should be acknowledged explicitly as a methodological limitation.

In the participant description, the manuscript indicates that students were “between 6 and 9 years of age” and were drawn from the first three grades, but inclusion and exclusion criteria are not sufficiently detailed. The authors should report criteria related to IQ range, hearing and visual functioning, neurological disorders, ADHD, developmental coordination disorder, medication use, prior remedial education, absenteeism, and participation in concurrent educational or psychological interventions. Given that the proposed mechanism of RhythmoGame involves attention, motor coordination, and auditory processing, uncontrolled comorbidities in these domains could substantially confound the observed treatment effect.

In the Data Collection Tools paragraph, the role of the WISC-IV needs clarification. The manuscript states that it was used “to assist in determining whether their academic difficulties were consistent with a specific learning disorder rather than a broader intellectual limitation,” but no cutoff criterion or decision rule is reported. The authors should specify which WISC-IV indices were administered, whether the full scale was used, the minimum IQ criterion for inclusion, who administered the test, and whether examiners were trained or licensed to administer it. The psychometric properties cited are insufficient unless the actual use of the instrument in participant selection is operationally described.

Authors revised the manuscript and uploaded the document.

2. Revised

Editor’s decision: Accepted.

Editor in Chief’s decision: Accepted.