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Beyond aerobic intensity: Comparing the effects of cognitively engaging gross-motor coordination training and aerobic exercise on working memory and selective attention in school-age children: A randomized controlled trial

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Abstract

Background Working memory and selective attention develop rapidly during middle childhood and are sensitive to physical activity. Aerobic exercise is thought to act mainly through intensity-driven physiological pathways (cerebral perfusion, neurotrophic signalling and arousal), whereas gross-motor coordination training additionally imposes perceptual, decision-making, motor-planning and inhibitory demands that may provide stronger and more domain-specific cognitive stimulation. This study compared 14-week gross-motor coordination and aerobic exercise interventions, designed a priori to be matched for session frequency, duration and rated perceived exertion, on working memory and selective attention (indexed by processing volume on the d2 test) in children aged 10–12 years.

Methods The study employed a pre-test-post-test randomized controlled design. Participants ($n=78$) were recruited from a single state primary school and allocated by computer-generated stratified block randomisation (1:1:1) to an Aerobic Exercise Group (AEG; $n=26$), a Coordination Exercise Group (CEG; $n=26$), or a Control Group (CG; $n=26$). Intervention groups participated in structured exercise programmes consisting of 40-minute sessions three days per week for 14 weeks, delivered on school premises during the school day as an addition to the standard physical education curriculum. Working memory was assessed using the examiner-administered, paper-and-pencil Visual-Auditory Digit Span Task, and selective attention was measured with the paper-based d2 Test of Attention; the digit span was administered individually and the d2 in small groups, both in a quiet classroom and by assessors blinded to group allocation. No computerised platform was used. Data were analysed with aligned rank transform ANOVA (ART-ANOVA).

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Results The group × time interaction was statistically significant for both outcomes (working memory: F(2, 75) = 12.47, p < 0.001, partial $\eta^2 = 0.25$, 95% CI [0.09, 0.39]; selective attention: F(2, 75) = 8.23, p < 0.001, partial $\eta^2 = 0.18$, 95% CI [0.04, 0.32]). Both intervention groups demonstrated significant improvements in working memory and selective attention compared with the control group (all Bonferroni-adjusted $p \leq 0.018$). When the intervention groups were compared directly, the coordination-based exercise programme was significantly more effective than the aerobic exercise programme in improving working memory (adjusted $p = 0.045$; median change 5.2 [IQR 4.6–5.9] vs. 3.1 [2.2–3.9] points on the digit span total score; between-group effect size $r = 0.31$, 95% BCa CI [0.06, 0.52]; median difference 2.1 points, 95% BCa CI [1.44, 2.76]). No significant difference was found between the two intervention groups with respect to selective attention (adjusted $p = 0.320$; median change 29.0 [17–42] vs. 30.5 [25.0–40.0] d2 TN points; $r = 0.16$, 95% BCa CI [−0.10, 0.40]; median difference −1.5 points, 95% BCa CI [−10.48, 7.48]). The correlation between working memory and selective attention change scores was weak and statistically non-significant (Spearman's rho = 0.12, $p > 0.05$).

Conclusions Both aerobic and coordination-based exercise interventions may support working memory and selective attention in children. Coordination-based interventions that were designed a priori to impose a higher cognitive load — through motor planning, attentional switching, inhibition of prepotent movement responses, and progression from closed to open skills — may yield stronger gains in working memory specifically, although the between-intervention difference was modest and requires replication. Because selective attention was indexed by the number of items processed, the attentional findings should be read as evidence about processing volume rather than about attentional efficiency in the narrow sense. School-based physical activity programmes may therefore benefit from being structured not only around movement intensity but also around cognitive engagement and coordination components.

Registration was not completed before the first participant was enrolled, which the authors acknowledge as a departure from best practice. No changes were made to the eligibility criteria, outcomes, or planned analyses after enrolment began; the primary outcomes, group allocation and analysis plan reported here are identical to those documented in the ethics application dated 27 February 2026, which is available on request. The authors have confirmed that this journal permits retrospective registration provided it is declared.

Trial registration ClinicalTrials.gov, NCT07658404, registered 13 June 2026. Retrospectively registered.

Keywords Working Memory, Selective Attention, Aerobic Exercise, Gross-Motor Coordination Training, Cognitive Engagement, Children, Executive Functions

Background

Middle childhood (approximately 8–12 years) is a period of rapid maturation of the prefrontal cortex and of the fronto-parietal networks that support executive functions [1]. Because these networks remain experience-dependent throughout this window, middle childhood is also the period in which environmental interventions -- including structured physical activity -- have the greatest potential to shape executive function trajectories [1, 2]. This paper focuses on two outcomes within that window. Working memory is a core executive function, defined as the capacity to hold and manipulate information over short intervals. Selective attention is the capacity to prioritise task-relevant visual information while suppressing competing distractors; it is conventionally classified as an attentional rather than a strictly executive process, but it shares the fronto-parietal substrate of inhibitory control and is routinely examined alongside executive functions in paediatric exercise research [3, 4]. To avoid the terminological drift criticised in this literature, we use "selective attention" consistently throughout the manuscript (rather than "attentional control" or "attention"), and we reserve "executive function" for working memory. Both constructs predict academic achievement and classroom behaviour, which is what makes them meaningful targets for school-based intervention.

Systematic reviews have established that regular physical activity benefits children's cognition [5, 6], but the mechanisms invoked to explain this are frequently listed rather than distinguished. At least four partially overlapping pathways can be separated. (i) Neurophysiological and neurotrophic: exercise increases cerebral blood flow, upregulates brain-derived neurotrophic factor (BDNF) and insulin-like growth factor 1, and supports synaptic plasticity and the structural integrity of hippocampal and prefrontal regions [5, 7, 8]. (ii) Arousal-activation: acute increases in catecholaminergic tone transiently optimise the signal-to-noise ratio of prefrontal processing, producing short-lived performance gains that are intensity-dependent [7, 9]. (iii) Cognitive engagement: the executive demands embedded in the movement task itself -- motor planning, attentional switching, response inhibition, and rapid adaptation to unpredictable environmental constraints -- recruit and thereby

train the same fronto-parietal circuitry that supports executive functions [3, 4]. (iv) Neurodevelopmental and experience-dependent specialisation: repeated coordinated motor practice refines cerebellar-prefrontal loops that are subsequently co-opted for cognitive control, so motor and cognitive development are partly interdependent rather than parallel [2, 10]. Pathways (i) and (ii) are primarily intensity-driven and are therefore maximised by aerobic exercise; pathways (iii) and (iv) are primarily task-driven and are therefore maximised by coordinatively complex movement. This separation is the conceptual basis of the present comparison, and it generates a testable prediction: if the cognitive-engagement pathway is real, a coordination programme matched to an aerobic programme for frequency, duration and perceived exertion should still produce larger gains in the executive outcome most dependent on prefrontal maintenance and manipulation, namely working memory.

Gross-motor coordination training is defined here as the structured practice of whole-body movement tasks requiring the simultaneous regulation of dynamic balance, inter-limb (bilateral) coordination, rhythm, spatial orientation and rapid direction change. It is not fine-motor, manual-dexterity or hand-eye training, and the term is used in this restricted sense throughout. What distinguishes such tasks cognitively is not their metabolic cost but their information-processing load: the child must hold a movement goal in working memory, allocate attention selectively to relevant environmental cues, inhibit a prepotent but incorrect movement, and update the motor plan when task constraints change [4, 10]. School-based coordination programmes built on this principle have improved visuospatial working memory and gross motor skills in 9-10-year-old primary school children [11], attentional performance in adolescents following acute coordinative exercise [12], and both coordinative and executive skills in children completing a football-based programme [13]. This is consistent with meta-analytic evidence that motor competence and executive function are moderately associated across childhood and adolescence, with the strongest associations reported for inhibition and visuospatial working memory [14, 15, 16], and with recent cross-sectional evidence in preschool children reporting the same coupling between motor skills, inhibition and visuospatial working memory [17].

The literature nevertheless remains dominated by aerobic protocols, and the novelty of the present comparison lies in what is held constant rather than in the comparison itself. Most randomized trials in children have compared aerobic exercise against a passive, academic or usual-curriculum control [18, 19, 20]. The comparatively small number of trials that manipulate cognitive engagement have generally done so by adding cognitive tasks on top of an aerobic protocol, or by contrasting cognitively engaging activity with health-related fitness activity without equating perceived exertion [21, 19, 22, 23]. As a result, it is still unclear whether the reported advantage of cognitively engaging physical activity reflects the cognitive demand of the task itself or an uncontrolled difference in dose. The present trial was designed to isolate that question: session frequency, session duration, programme length and rated perceived exertion were equated across the two intervention arms, and only the coordinative-cognitive demand of the session content was varied.

Gross-motor coordination training, by contrast, involves complex whole-body skills that simultaneously engage motor planning, selective attention, dual-task performance and cognitive flexibility [24]. Experimental studies that separate the intensity-driven from the task-driven mechanisms within a single design, holding the exercise dose constant, remain scarce in children [25].

Cognitive responses to exercise are unlikely to be uniform across children. Sex, biological maturity, body composition, baseline motor competence and baseline cardiorespiratory fitness have all been proposed as moderators [26, 14]. Recent observational work in preschool children reports sex-specific associations between BMI category, 24-hour movement behaviours, motor skills and executive functions, with the pattern of association differing between girls and boys [17, 27]. Because pubertal timing differs systematically between girls and boys across the 10-12-year range, maturity must be controlled before any sex-related difference in cognitive response can be interpreted; we therefore used peak height velocity (PHV) offset rather than chronological age as the maturity reference, and stratified the allocation sequence on sex so that the arms would be balanced on it. Moderation by sex is not examined in the present trial. It was powered for the group × time interaction and not for a three-way group × time × sex term, and an analysis capable of detecting only a very large moderation effect would not have been informative in either direction. Establishing whether these interventions act differently in girls and boys requires a trial designed and powered a priori for that interaction, with maturity-matched rather than merely age-matched subgroups; we identify that as a direction for future work rather than as an aim of this study.

A final consideration is that a structured exercise session occupies a small fraction of a child's day. The 24-hour movement behaviour framework holds that physical activity, sedentary behaviour and sleep are co-dependent components of a fixed daily time budget, so the cognitive effect of an added exercise session depends partly on what that session displaces and partly on the child's habitual movement profile. SUNRISE-based

studies have documented substantial variation in these profiles between countries and between urban and rural settings, and have shown that a majority of young children fail to meet integrated movement guidelines [28, 29]. Sleep duration and quality in particular are associated with prefrontal-dependent cognitive performance and may either amplify or mask an intervention effect [29]. The present trial did not manipulate or objectively monitor habitual movement behaviours; we therefore treat the children's wider movement ecology as an explicit boundary condition on our findings rather than as an unmeasured nuisance, and we return to this point in the Limitations.

Accordingly, the primary aim of this study was to compare the effects of a 14-week gross-motor coordination programme and a dose-matched aerobic programme on working memory and selective attention in children aged 10–12 years, relative to a usual-curriculum control group. This was the sole aim of the trial. Moderation by sex, described as a secondary and exploratory aim in the previous version, has been withdrawn as an analytical objective for the reasons given above. Two hypotheses were pre-specified. H1: both intervention groups would outperform the control group on both outcomes. H2: because coordination training loads the cognitive-engagement and neurodevelopmental pathways in addition to the physiological pathway, the coordination group would show larger gains than the aerobic group in working memory, whereas the two intervention groups would not differ in selective attention, which is more responsive to intensity-driven arousal effects.

Methods

Ethical approval and consent

All procedures were carried out in accordance with the Declaration of Helsinki and were approved prospectively, before any participant was recruited or any measurement taken, by the Ethics Committee of Istanbul Rumeli University (Meeting No 2026/02, Decision No 08, approval date 27 February 2026). Written permission for school-based data collection was obtained from the relevant provincial education authority and from the school administration before recruitment began. Written informed consent was obtained from the parent or legal guardian of every child, and written informed assent was obtained from every child, after both had received a verbal and written explanation of the purpose, procedures, expected duration, foreseeable risks and benefits of the study. Participation was voluntary; families were told that they could withdraw at any point without consequence for the child's school standing, and no incentives were offered. All data were pseudonymised at source using a numeric code held separately from the key, and only the research team had access to identifiable records. Children allocated to the control group were offered the coordination programme after the post-test assessments were completed.

Study design

This study employed a pre-test-post-test randomized controlled design with three parallel arms: an Aerobic Exercise Group (AEG), a Coordination Exercise Group (CEG) and a Control Group (CG). The trial was conducted in a single state primary school in Istanbul, Turkiye, during the 2025–2026 academic year. All intervention sessions took place on school premises (the school sports hall and outdoor sports court) during the school day, immediately after the end of formal lessons, and were additional to -- not a replacement for -- the standard two weekly physical education lessons, which all three groups continued to receive. Sessions were delivered by two physical education specialists who followed a written session manual and were supervised by the first author. Reporting follows the CONSORT 2010 statement and its extension for non-pharmacological trials.

Participants

Sample size was determined via a priori power analysis using G*Power 3.1.9.7 software. Calculations indicated that a total of 60 participants ($n = 20$ per group) would be sufficient to detect the principal interaction effects. However, the sample was expanded and the study was conducted with 78 participants ($n = 26$ per group). Participants were aged 10–12 years (median 11.0) and comprised 41 girls and 37 boys. The participant selection criteria, group assignment processes, and flow of the experimental protocol are detailed in the CONSORT diagram presented in Fig. 1.

To control for inter-individual maturation differences, somatic maturity level was used as the reference measure rather than chronological age. Participants' Peak Height Velocity (PHV) offset was calculated using the non-invasive regression equation developed by Mirwald et al. [32]. PHV estimates indicated that all participants were in the early pubertal stage and that there was no statistically significant difference in maturity level among the three groups ($p > 0.05$), thereby confirming developmental homogeneity across groups prior to the intervention.

Participant background characteristics. In addition to age, sex and anthropometry, the following variables were recorded at baseline in order to characterise the sample and to check the comparability of the arms on factors known to influence executive function: habitual physical activity (parent-proxy report of days per week with at least 60 min of moderate-to-vigorous activity, together with average daily screen time and parent-reported sleep duration on school nights); organised sport participation during the preceding 12 months (yes/no, discipline, and

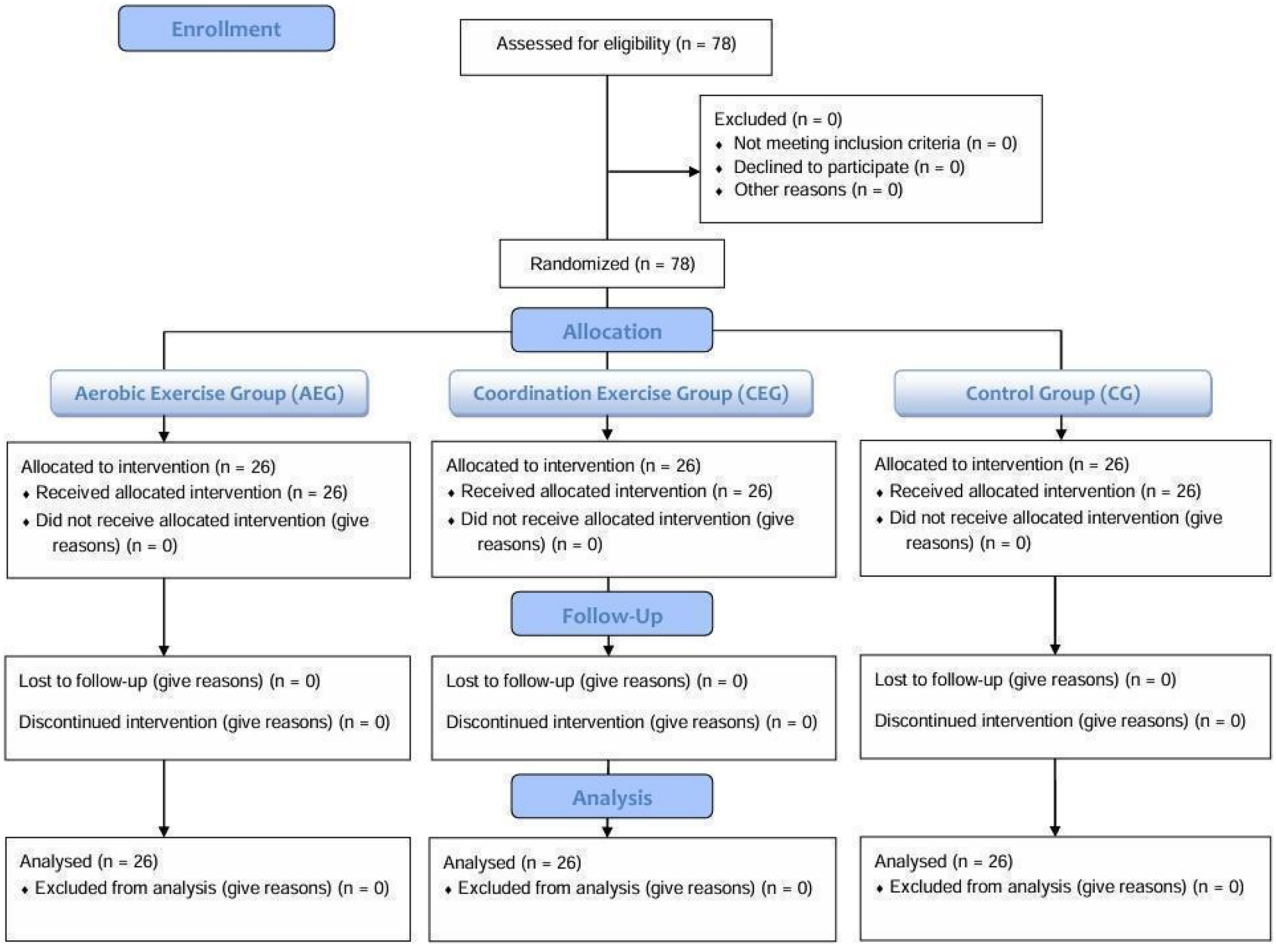


Fig. 1 CONSORT flow diagram of the study participants

years of practice); socioeconomic background (highest parental education level and household composition); and somatic maturity (PHV offset). These data were collected by a parent questionnaire completed at enrolment and were used to check the comparability of the arms. No objective (accelerometer-based) measure of habitual physical activity, sedentary behaviour or sleep was obtained, which is addressed in the Limitations.

Inclusion and exclusion criteria

Inclusion criteria were: typical development and the absence of any neurological, developmental, or physical disorder diagnosis. Exclusion criteria included: learning disability, ADHD diagnosis, chronic health conditions restricting physical activity, absence from more than 20% of exercise sessions, or unexpected health problems arising during the study period.

Randomization and group assignment

Allocation sequence generation. The allocation sequence was generated with Random Allocation Software v2.0 [33] using permuted blocks (block size 6, not disclosed to the personnel delivering the intervention) in a 1:1:1 ratio, stratified on three baseline variables: sex, cardiorespiratory fitness (median split of estimated VO2max) and executive function pre-test score (median split of the digit span total). Allocation concealment. The sequence was generated by an independent statistician who was not otherwise involved in the trial, had no contact with the participants, and had no role in recruitment or assessment. Group assignments were placed in sequentially numbered, opaque, sealed envelopes, which were opened only after a child's baseline assessment had been completed and eligibility confirmed. Implementation. Participants were enrolled by a member of the research team who had no part in generating the allocation sequence; the envelopes were opened, and participants assigned to groups, by a second member of the research team. Blinding. Because the interventions are behavioural, neither the children, their parents nor the instructors could be blinded to allocation. The assessors who administered and scored the digit span and d2 tests were blinded to group membership, as was the analyst who conducted the primary ART-ANOVA models; group

labels were replaced by arbitrary codes (A, B, C) until the analysis was finalised.

Intervention Protocols

The intervention comprised a progressive programme of 40-minute sessions conducted three times per week over 14 weeks (42 sessions in total). Exercise intensity for the intervention groups was standardized based on the American College of Sports Medicine guidelines for children [34]. Both arms were delivered in the school sports hall by the same two instructors (a physical education teacher and a sport sciences graduate), who alternated arms weekly so that instructor identity was not confounded with group. Children were organised into four fixed stations of 6–7 children per station; every child completed every station in each session, and station order was rotated across the week. A representative single-session plan for each arm is given in Table 1 (AEG) and Table 2 (CEG), the full week-by-week progression in Table 3, and the difficulty-escalation criteria and fidelity procedures in Table 4. Together these four tables reproduce the session manual from which both programmes were delivered.

Aerobic Exercise Group (AEG) protocol

Every AEG session followed the same fixed four-part template so that the arm would not be a heterogeneous assortment of activities: (1) 5-minute standardised warm-up (jogging and dynamic mobility); (2) 25-minute main block; (3) 5-minute low-intensity cool-down; (4) 5 min of static stretching and RPE reporting. The main block drew on three activity families only -- high-intensity interval running (e.g. 8–12 × 20 s effort / 40 s active recovery), continuous and shuttle running games targeting large muscle groups, and orienteering-style pursuit tasks -- and each family was used in a fixed weekly rotation (family 1 on day 1, family 2 on day 2, family 3 on day 3) so that the weekly stimulus was identical for every participant. Within the 25-minute main block the AEG performed four work bouts of 5 min separated by 1 min of walking recovery (work-to-rest ratio 5:1); on interval days each 5-minute bout comprised 8–12 repetitions of 20 s effort and 40 s active recovery (work-to-rest ratio 1:2). Repetition number, not running speed, was the progressed variable. Crucially, all three families were deliberately selected to be motorically simple, repetitive and predictable: they impose a high metabolic load but a low decision-making, inhibition and motor-planning load. The AEG therefore operationalises the intensity-driven pathway with the cognitive-engagement pathway held to a minimum. A representative session plan is given in Table 1 and the full 14-week progression in Table 3. Exercise intensity was monitored in real time using chest-strap heart rate monitors (Polar H10, Polar Electro, Kempele, Finland). Data were tracked simultaneously using Polar Team Pro software, and participants were maintained within a target heart rate range of 65–85% HRmax. Intensity was progressively increased every three weeks by manipulating training volume and rest intervals in accordance with the overload principle [35–37]: interval repetitions rose from 8 in weeks 1–3 to 12 in weeks 10–14, and recovery between the 5-minute bouts was reduced from 60 s to 40 s over the same period. Progression to the next volume step required that at least 80% of the group had sustained the prescribed heart-rate band for at least 80% of the main block in the preceding week; if this criterion was not met the volume step was repeated for one further week.

Table 1 Representative Aerobic Exercise Group (AEG) session (week 8, day 2: interval-running family)

Block	Duration	Content	Structure (work : rest)	Prescribed intensity	Organisation
1. Warm-up	5 min	Continuous jogging (2 min) followed by dynamic mobility (leg swings, ankle bounces, arm circles, skipping)	Continuous	50–60% HRmax	Whole group, single file around the hall perimeter
2. Main block	25 min	High-intensity interval running: shuttle running between markers set 20 m apart, straight-line course, no direction cues or decisions required	4 bouts of 5 min separated by 1 min walking recovery (5:1). Within each bout: 10 repetitions of 20 s effort / 40 s active recovery (1:2)	65–85% HRmax; target RPE 13–16 (Borg 6–20)	4 stations of 6–7 children; children rotate stations after each 5-min bout; all stations run the same task
3. Cool-down	5 min	Low-intensity continuous jogging decreasing to walking	Continuous	< 50% HRmax	Whole group
4. Stretching and RPE	5 min	Standardised static stretching sequence (6 positions × 30 s), followed by individual Borg 6–20 rating collected privately by the instructor	6 × 30 s	n/a	Individual; ratings recorded on the session checklist

Session content on day 1 of each week used the continuous / shuttle-running family and on day 3 the orienteering-style pursuit family; the block structure, durations and work-to-rest ratios were identical across all three families. All three families were selected to be motorically simple, repetitive and predictable, so that the arm loads metabolic demand while holding decision-making, inhibition and motor-planning demand to a minimum

Table 2 Representative Coordination Exercise Group (CEG) session (week 8, day 2)

Block	Duration	Station / drill	Governing rule	Executive components loaded	Structure (work : rest)	Prescribed intensity
1. Warm-up	5 min	Rhythmic locomotion with tempo changes on a whistle cue	Change locomotion pattern on each whistle	Selective attention; inhibition	Continuous	50–60% HRmax
2. Main block, station A	5 min	Jump-rope sequence combinations	Reproduce a 6-element rope sequence demonstrated once at the start of the station and not shown again	Working memory; motor planning	7 sets × 30 s work / 15 s rest (2:1)	65–85% HRmax; RPE 13–16
2. Main block, station B	5 min	Dynamic balance on unstable surfaces with a colour-cue response	Respond to the colour of the instructor's marker and ignore the position in which it is held	Selective attention; perceptual processing; inhibition	7 sets × 30 s work / 15 s rest (2:1)	65–85% HRmax; RPE 13–16
2. Main block, station C	5 min	"Mirror" direction-change drill between four cones	Move in the direction opposite to the one demonstrated; on a second whistle the rule reverses for the remainder of the set	Inhibition of a prepotent response; cognitive flexibility	7 sets × 30 s work / 15 s rest (2:1)	65–85% HRmax; RPE 13–16
2. Main block, station D	5 min	Bilateral / contralateral step patterns with a concurrent secondary task	Perform the contralateral step pattern while counting backwards aloud in threes from a number given at the start of each set	Dual-tasking; working memory; cognitive flexibility	7 sets × 30 s work / 15 s rest (2:1)	65–85% HRmax; RPE 13–16
3. Cool-down	5 min	Slow rhythmic walking patterns with simple sequence recall	Reproduce a 3-element pattern	Working memory (low load)	Continuous	< 50% HRmax
4. Stretching and RPE	5 min	Standardised static stretching sequence identical to the AEG, followed by individual Borg 6–20 rating	n/a	n/a	6 × 30 s	n/a

Transitions between stations lasted 1 min and were used to deliver the governing rule for the next station, giving a station-level work-to-rest ratio of 5:1, identical to the AEG. The 15-second within-station rest interval was kept deliberately short so that the governing rule had to be maintained in working memory rather than re-demonstrated. Every main-block task was required by the manual to load at least two of the four pre-specified executive components. Equipment: ropes, floor markers, cones, balance pads and balls only

Coordination Exercise Group (CEG) protocol

CEG sessions used the identical four-part template, duration and weekly frequency as the AEG, so that the two arms differed only in the cognitive-coordinative demand of the main block. Content included jump-rope combinations, dynamic balance exercises, bilateral and contralateral motor coordination drills, rhythmic step patterns, and complex direction-change tasks [10]. The 25-minute main block was organised as four drill stations of 5 min each, separated by 1 min of transition and rule briefing (work-to-rest ratio 5:1, identical to the AEG). Within each 5-minute station the children performed 6–8 working sets of approximately 30 s separated by 15 s of rest (work-to-rest ratio 2:1) during which the instructor delivered or switched the governing rule; the 15 s interval was kept short deliberately so that the rule had to be held in working memory rather than re-demonstrated. Set number per station rose from 6 in weeks 1–4 to 8 in weeks 11–14. A representative session plan is given in Table 2. The cognitive demand of each task was not incidental but designed a priori: every main-block task was required to load at least two of four pre-specified components, and the session manual records which components each task loaded. (i) Working memory -- the child had to retain and reproduce a movement sequence of increasing length (e.g. a four- then six-element rope or step sequence recalled without demonstration). (ii) Selective attention and perceptual processing -- the correct response was cued by one feature of a multi-feature stimulus (e.g. respond to the colour of the coach's marker while ignoring its position). (iii) Inhibition of a prepotent response -- go/no-go and reversal rules were embedded in the movement task (e.g. "mirror" drills in which the child must perform the opposite of the demonstrated direction). (iv) Cognitive flexibility and dual-tasking -- the governing rule was switched mid-drill on an auditory signal, or a concurrent secondary task (ball handling, counting backwards) was added. Reaction and adaptation demands were therefore explicit design features rather than by-products of the movement. The progression from closed to open skills follows directly from this design. In weeks 1–4 the tasks were closed (self-paced, stable environment) so that the children could acquire the basic movement patterns; had the drills been unpredictable from the outset, performance would have been limited by motor execution rather than by cognitive control, and the intended executive load could not have been applied. From week 5 the environmental predictability was systematically reduced (externally paced signals, a moving partner, then two competing cues), so that the executive load increased while the motor complexity plateaued [38]. Task difficulty was advanced against a

Table 3 Week-by-week progression of both intervention arms

Week	AEG: interval repetitions per 5-min bout	AEG: recovery between bouts	CEG: sets per station	CEG: skill context	CEG: movement sequence length	CEG: competing cues	CEG: pacing
1	8	60 s	6	Closed (self-paced, stable environment)	3 elements	None	Self-paced
2	8	60 s	6	Closed	3 elements	None	Self-paced
3	8	60 s	6	Closed	4 elements	None	Self-paced
4	9	55 s	6	Closed	4 elements	None	Self-paced
5	9	55 s	6	Transitional: externally paced signals introduced	4 elements	One cue	Externally paced (whistle)
6	9	55 s	7	Transitional	5 elements	One cue	Externally paced
7	10	50 s	7	Transitional: moving partner introduced	5 elements	One cue	Externally paced
8	10	50 s	7	Open: moving partner	6 elements	One cue	Externally paced
9	10	50 s	7	Open	6 elements	One cue	Externally paced
10	11	45 s	7	Open: two competing cues introduced	6 elements	Two cues	Externally paced, variable interval
11	11	45 s	8	Open	7 elements	Two cues	Externally paced, variable interval
12	12	40 s	8	Open	7 elements	Two cues	Externally paced, variable interval
13	12	40 s	8	Open: mid-drill rule switching at unpredictable intervals	7 elements	Two cues	Externally paced, variable interval
14	12	40 s	8	Open: mid-drill rule switching	8 elements	Two cues	Externally paced, variable interval

Session duration (40 min), weekly frequency (3), block structure and work-to-rest ratios were held constant in both arms for all 14 weeks; only the variables tabulated above were progressed. AEG progression manipulated volume and recovery in accordance with the overload principle and was applied every three weeks. CEG progression manipulated executive load — sequence length, number of competing cues, environmental predictability and pacing — while motor complexity plateaued from week 5 onward, so that cognitive control rather than motor execution remained the limiting factor. Progression was applied only when the criteria in Table 4 were met

pre-specified criterion rather than by the calendar: a drill was escalated to the next level (sequence lengthened by one element, a competing cue added, or pacing transferred from the child to an external signal) only when at least 70% of the children at that station executed the current level correctly on at least 8 of 10 consecutive trials, and was stepped back one level if fewer than 40% did so. The four escalation ladders and their criteria are set out in Table 4, and the full 14-week progression in Table 3. This is the mechanism by which the programme was intended to act on working memory: sustained practice at the limit of the child's capacity to maintain, update and switch a movement rule. Because coordination tasks cannot be prescribed by heart rate in the same way as continuous running, internal load was prescribed to match the AEG using the Borg 6–20 rating of perceived exertion, collected individually at the end of each session and targeted at "hard" to "very hard" (13–16) [39]. Heart rate was recorded in the CEG with the same Polar H10 monitors as a verification measure. RPE was recorded individually after every session in both arms, and heart rate was recorded continuously in both arms, so that internal load could be compared between the two programmes throughout the intervention. This matters for interpretation: to the extent that the arms were equated for internal load, a between-arm difference in cognitive outcome cannot be attributed to a difference in exercise intensity. Both measures were used prospectively, during delivery, to hold each session within its prescribed target, and the session records confirm that no session was delivered outside the prescribed RPE and heart-rate bands. Session-level summaries were not compiled, however, so no aggregate RPE or heart-rate values and no between-arm statistical comparison of internal load are reported in this manuscript. The equating of internal load between the arms is therefore a property of the protocol and of the prescribed targets rather than a demonstrated result, and the two programmes are described throughout as having been designed to be matched, never as having been shown to be matched. The consequence for interpretation is stated in the Limitations, because it bears directly on the central contrast this trial was built to isolate. A written session manual recording the content of every session and the executive components loaded by each task was maintained throughout the intervention; Tables 1, 2, 3 and 4 are drawn directly from it, so that the programme can be reproduced from this article without recourse to the authors.

Table 4 Difficulty-escalation criteria, escalation ladders and intervention fidelity procedures

Domain	Escalation ladder (levels 1 → 5)	Criterion to advance one level	Criterion to step back one level
Movement sequence length (working memory load)	3 → 4 → 5 → 6 → 7/8 elements reproduced without re-demonstration	≥ 70% of children at the station reproduce the sequence correctly on ≥ 8 of 10 consecutive trials	< 40% of children correct on ≥ 8 of 10 consecutive trials
Environmental predictability (open-skill progression)	Closed and self-paced → externally paced signal → moving partner → moving partner with one cue → two competing cues	Same criterion as above, assessed over one full session	Same criterion as above
Inhibition load	No reversal rule → reversal rule fixed for the set → reversal rule switched mid-set on a signal → two alternating rules → rule switched at unpredictable intervals	Same criterion as above	Same criterion as above
Dual-task load	No secondary task → ball handling → counting forwards aloud → counting backwards in twos → counting backwards in threes	Same criterion as above	Same criterion as above
Fidelity procedure 1: instructor standardisation	8-hour pre-intervention workshop; every drill demonstrated, practised and rehearsed with its escalation ladder; scripted verbal instruction for each governing rule	Completed by both instructors before session 1	n/a
Fidelity procedure 2: session checklist	Completed by the instructor immediately after every session: drills delivered, escalation level per station, set and repetition counts, deviations and their reasons, individual attendance, individual Borg 6–20 rating	Applied to every session in both arms	n/a
Fidelity procedure 3: independent observation	12-item checklist covering template adherence, timing of each block, correct delivery of the governing rule, and correct application of the escalation criteria, scored by an observer not involved in delivery	Sessions observed in both arms across the intervention period; records reviewed with instructors and drift corrected before the next session. Not aggregated into a summary fidelity score	n/a
Instructor allocation	Two instructors (one physical education teacher, one sport sciences graduate) alternated arms weekly	Applied throughout, so that instructor identity was not confounded with group	n/a
Protocol deviations	Recorded prospectively on the session checklist	All recorded deviations involved substitution of an equivalent indoor running family for an outdoor one because of weather; no session shortened, cancelled or altered in its executive content	n/a

Control Group (CG) protocol

During the intervention period applied to the experimental groups, the CG continued with their routine school curriculum without participation in any additional structured programme.

Intervention fidelity and instructor standardisation

Three procedures were used to keep delivery consistent across instructors and across the 42 sessions. First, both instructors completed an 8-hour standardisation workshop before the intervention in which every drill in the manual was demonstrated, practised and rehearsed with the escalation criteria; the CEG manual specified, for each drill, the governing rule, the executive components loaded, the escalation ladder and the verbal instruction to be used. Second, a session checklist was completed by the instructor immediately after every session, recording the drills delivered, the escalation level reached at each station, set and repetition counts, any deviation from the manual and the reason for it, individual attendance, and each child's Borg 6–20 rating. Third, an independent observer, not involved in delivery, attended sessions in both arms across the intervention period and scored delivery against a 12-item fidelity checklist covering template adherence, timing of each block, correct delivery of the governing rule, and correct application of the escalation criteria; the observer's records were reviewed with the instructors, and any drift from the manual was corrected before the following session. These are reported as the procedures that were in place rather than as a quantified fidelity index, because the observation records were not aggregated into summary scores. The deviations from the manual that were recorded all involved substituting an equivalent indoor running family for an outdoor one because of weather; no session was shortened, cancelled or changed in its executive content. The checklist and the fidelity instrument are provided in Table 4.

Data collection procedure

All assessments were carried out at the school. Cognitive and anthropometric testing took place in a quiet, well-lit classroom reserved for the study; the cardiorespiratory fitness test and the motor coordination battery were conducted in the school sports hall, not in the classroom. To

prevent cognitive fatigue and preserve measurement precision, tests were administered across consecutive days in a standardized sequence, at the same time of day at both pre-test and post-test, at least 24 h after the most recent exercise session:

Day 1: Aerobic fitness test (Baseline).
Day 2: Anthropometric measurements (Baseline).
Day 3: Motor coordination test battery (Baseline).
Day 4: Working memory (Baseline and post-test).
Day 5: Selective attention (Baseline and post-test).

Measurement instruments

Cognitive and anthropometric measurements were conducted in a quiet classroom environment by certified researchers who were blinded to group membership; the cardiorespiratory fitness test and the KTK motor coordination battery were conducted in the school sports hall. Aerobic fitness, anthropometry and the KTK battery were administered at baseline only, in order to characterise the sample and to stratify randomisation, so no post-intervention fitness or motor coordination data are available (Table 5).

Working memory

Working memory was assessed with the Visual-Auditory Digit Span Task, a paper-and-pencil instrument administered individually by a trained examiner; no computer or tablet platform was used at any stage. The task measures the processes of holding, processing and recalling information in short-term memory across auditory-verbal, visual-verbal, auditory-written and visual-written presentation modalities [40]. Administration proceeded as follows. Each modality began with a two-digit sequence; digits were presented at a rate of one per second (auditory sequences read aloud by the examiner in a flat intonation, visual sequences shown on printed cards for one second each). Two trials were given at each sequence length. The sequence length increased by one digit after a correct trial and the modality was terminated after two consecutive failures at the same length. The score for a modality was the number of correctly reproduced sequences, and the total score used in the analyses was the sum of the four modality scores, reported throughout as the digit span total score. Testing took approximately 15–20 min per child. Two practice items preceded each modality and were not scored. Administration and scoring followed a written protocol, and the same examiner administered the task at both pre-test and post-test for a given child. Internal consistency and inter-rater agreement were not formally quantified in this sample, which is noted in the Limitations.

Selective attention

Selective attention was assessed with the d2 Test of Attention [41], a standardised paper-based cancellation test administered in small groups in the same quiet classroom. The test evaluates visual scanning, rapid discrimination, sustained attention and accurate responding. The child is presented with 14 lines of 47 characters consisting of the letters d and p carrying one to four dashes, and must cancel every d bearing exactly two dashes while ignoring all other characters. Each line is worked under a 20-second time limit signalled by the examiner, giving a total working time of 4 min 40 s plus approximately 5 min of standardised instruction and practice. The total number of items processed (TN), error rates (E1 omissions, E2 commissions) and concentration performance (CP = correctly cancelled targets minus commission errors) were all computed. TN was pre-specified as the primary selective attention index and is the only d2 index analysed in this trial; every selective attention value reported in the Results and in Tables 5 and 6 is a TN score. CP and the error rates were computed during scoring but were not analysed, and no analysis of them is reported here. This constrains what the attentional findings can be taken to show, and we state the constraint rather than leaving it implicit. TN quantifies how much material a child works through, not how accurately they discriminate targets from distractors, so it is sensitive to a shift in the speed–accuracy trade-off: a child who scans faster and less carefully will raise TN without any gain in selective attention, and the present data cannot separate that from a genuine attentional improvement. Wherever this manuscript refers to selective attention, the claim is

Table 5 Baseline demographic and performance characteristics of participants across groups

Characteristic	Aerobic group (*n* = 26)	Coordination group (*n* = 26)	Control group (*n* = 26)	Test statistic	*p*-value
Age (years)	11.1 [10.5–11.8]	10.9 [10.2–11.5]	11.0 [10.4–11.6]	0.42	0.81
Sex (F / M)	14 / 12	14 / 12	13 / 13	0.08	0.96
BMI (kg/m^2)	19.4 [18.1–20.7]	19.6 [18.3-20.09]	19.3 [18.0-20.5]	0.31	0.85
Motor coordination	100 [95–106]	102 [96–108]	102 [95–107]	0.65	0.77
VO_2max (ml/kg/min)	37.1 [33.4–44.9]	37.8 [34.0-48.6]	38.4 [34.1–42.9]	0.13	0.93
Working memory	17 [15–19]	16 [15–18]	16 [15–18]	0.50	0.77
Selective attention	335 [294–382]	371 [317–392]	335 [301–388]	1.00	0.60

Data are presented as median [interquartile range] for continuous variables and as frequency for categorical variables. The Kruskal-Wallis H test was used for continuous variables; the chi-square (χ^2) test was applied for sex comparisons. Selective attention is the d2 total number of items processed (TN)

therefore a claim about processing volume on a cancellation task, and it is qualified as such in the Results, the Discussion and the Conclusions; the point is restated in the Limitations. Protocols were scored using the standard template by an assessor blinded to group and to test occasion.

Motor development

The Körperkoordinationstest für Kinder (KTK) test battery, a well-validated instrument for assessing motor coordination level in children, was used [42]. The battery comprises four subtests: walking backwards, hopping on one leg, jumping sideways, and moving sideways on platforms. Raw scores were normalized against age and sex norms to derive a comprehensive Motor Quotient.

Physical fitness

Cardiorespiratory fitness was assessed with the 20-metre shuttle run test, conducted in the school sports hall on a marked, non-slip 20-metre course, not in a classroom. The test has demonstrated validity and reliability in paediatric populations. Children ran in groups to a pre-recorded audio signal beginning at 8.5 km/h and increasing by 0.5 km/h each minute, and were withdrawn when they failed to reach the line on two consecutive signals or stopped voluntarily; strong standardised verbal encouragement was given. The final completed stage and shuttle number were recorded. Maximum oxygen uptake (VO2max) was estimated indirectly from the final running speed and the child's age and used as a quantitative indicator of physical fitness [43, 44]

Statistical analyses

All statistical analyses were conducted using IBM SPSS Statistics 29.0 software and appropriate supplementary modules for ART-ANOVA procedures. Prior to analysis, all variables were screened for outliers using the Median Absolute Deviation (MAD) method to ensure data reliability [45]. Normality checks performed with the Shapiro-Wilk test and variance homogeneity assessments conducted with the Levene test indicated that the majority of the data did not meet the assumptions of parametric tests ($p < 0.05$). Consequently, non-parametric approaches were adopted as the basis for the analytical process [46].

Descriptive statistics were reported as median and interquartile range (Med [IQR]) for continuous variables, and as frequency and percentage for the categorical variable (sex). Baseline homogeneity of groups was confirmed using the Kruskal-Wallis H test for continuous variables and the chi-square (χ^2) test for categorical data.

The Aligned Rank Transform ANOVA (ART-ANOVA) method was used to test the main effects of the intervention and group x time interactions within a non-parametric framework [47]. The primary model for each outcome was a 3 (group: AEG, CEG, CG) x 2 (time: pre, post) mixed ART-ANOVA; the group × time interaction from this model is the primary test for each outcome, and its F statistic, numerator and denominator degrees of freedom, exact p value and partial eta squared with confidence interval are reported in the Results for both outcomes. No further outcome model was fitted. Sex was not entered as a factor and no moderation analysis was conducted: a sensitivity power calculation in G*Power indicated that, with $n = 78$ and alpha = 0.05, the design had 80% power to detect a three-way group × time × sex interaction only at Cohen's f = 0.36 (partial eta squared = 0.114), a large effect by conventional criteria, so such an analysis could not have been informative in either direction and none is reported. Subsequent pairwise comparisons (post hoc) to identify the source of between-group differences were conducted with Bonferroni correction applied to minimize the risk of Type I error. Because two outcomes (working memory and selective attention) were each tested with three pairwise group contrasts, the familywise error rate was controlled at two levels: Bonferroni adjustment was applied within each outcome family (three contrasts per outcome; the Bonferroni-adjusted p-values reported in Table 7 are evaluated against alpha = 0.05), and the two outcome families were treated as co-primary and prespecified rather than as an exploratory multiple-outcome screen, so no further adjustment was applied across outcomes. The correlation analysis was not corrected and is reported as exploratory; its associated p-value should be interpreted descriptively. Spearman rank correlation (rho) was used to examine the association between the working memory and selective attention change scores, consistent with the non-parametric framework adopted throughout; no Pearson coefficients are reported anywhere in the manuscript.

Effect sizes were reported to evaluate clinical and practical significance. Within-group effect sizes were calculated as the r value (r = Z / sqrt(N)) and interpreted according to the criteria of 0.10 (small), 0.30 (medium) and 0.50 (large). For the ART-ANOVA models, partial eta squared was computed for the group × time term specifically as SS_effect / (SS_effect + SS_error), taking both sums of squares from the row of the aligned-rank ANOVA table corresponding to that term and not from the model as a whole. The two values reported in the originally submitted version had been taken from the omnibus model sums of squares in error; they were withdrawn in the previous revision and have now been recomputed by the stated method and independently recalculated by a second author from the same output. Partial eta squared values were evaluated against threshold values of 0.01 (small), 0.06 (medium) and 0.14 (large)

[48]. Between-group effect sizes for the three pairwise contrasts of interest, and in particular for the coordination versus aerobic comparison, were computed as the rank-biserial correlation on the change scores, which is the appropriate paired counterpart within a rank-based framework and is directly interpretable as the difference between the proportion of favourable and unfavourable pairs. Confidence intervals for every effect size reported in this manuscript -- partial eta squared for the interaction terms, within-group r, and between-group rank-biserial r -- were obtained by bias-corrected and accelerated (BCa) bootstrap resampling with 5,000 resamples, stratified by group, using the boot package in R 4.4.1; the same procedure was used to obtain 95% confidence intervals for the median change scores in each arm. Effect sizes derived from rank-transformed data are not directly comparable to those from raw-score parametric models, so they are interpreted here only in relative terms, and the practical magnitude of each change is conveyed by the median change and its interquartile range in the original score units rather than by the effect size alone. Statistical significance was set at $p < 0.05$ (two-tailed) for all analyses.

Box plots were used to present data distributions transparently, integrating median shifts, data density, and individual variation simultaneously.

Results

All 78 participants enrolled at baseline completed the 14-week intervention and post-test assessments; there was no attrition and no missing data, so all analyses were conducted on the full randomised sample. Attendance was recorded at every session in both intervention arms. No participant fell below the 80% attendance threshold specified in the exclusion criteria, and no participant was therefore excluded on this ground. No adverse events were recorded. No statistically significant differences were found among the AEG, CEG and CG groups with respect to baseline demographic, physical and cognitive characteristics (Table 5; all $p \geq 0.60$).

Significant improvements were recorded in both outcomes following the 14-week intervention (Table 6). Working memory rose from a median of 17.0 [IQR 15.0–19.0] to 20.1 [18.2–22.5] on the digit span total score in the AEG (median change + 3.1 [2.2–3.9]; $r = 0.62$, $p < 0.001$) and from 16.0 [15.0–18.0] to 21.2 [19.5–24.1] in the CEG (median change + 5.2 [4.6–5.9]; $r = 0.75$, $p < 0.001$), while the CG changed by only + 0.5 [0.2–0.7] points ($r = 0.12$, $p = 0.432$). For selective attention (d2 total number of items processed, TN), the AEG improved from 335.0 [294–382] to 365.5 [340–380] points (median change + 30.5 [25.0–40.0]; $r = 0.55$, $p < 0.001$) and the CEG from 371.0 [317–392] to 400.0 [380–425] (median change + 29.0 [17–42]; $r = 0.58$, $p < 0.001$), against + 5.0 [4.0–8.4] points in the CG ($r = 0.05$, $p = 0.518$). In relative terms this corresponds to gains of approximately 18% (AEG) and 33% (CEG) in working memory, and 9% (AEG) and 8% (CEG) in selective attention, versus 3% and 1% in the control group. The 3 (group) × 2 (time) mixed ART-ANOVA yielded a significant group × time interaction for working memory, $F(2, 75) = 12.47$, $p < 0.001$, partial $\eta^2 = 0.25$, 95% BCa CI [0.09, 0.39], alongside a significant main effect of time, $F(1, 75) = 82.16$, $p < 0.001$, partial $\eta^2 = 0.52$, 95% BCa CI [0.36, 0.63], and a significant main effect of group, $F(2, 75) = 4.64$, $p = 0.013$, partial $\eta^2 = 0.11$, 95% BCa CI [0.01, 0.24]. The corresponding model for selective attention (TN) also yielded a significant group × time interaction, $F(2, 75) = 8.23$, $p < 0.001$, partial $\eta^2 = 0.18$, 95% BCa CI [0.04, 0.32], with a significant main effect of time, $F(1, 75) = 58.90$, $p < 0.001$, partial $\eta^2 = 0.44$, 95% BCa CI [0.27, 0.56], and a significant main effect of group, $F(2, 75) = 5.60$, $p = 0.005$, partial $\eta^2 = 0.13$, 95% BCa CI [0.01, 0.26]. The source of the between-group differences underlying each interaction is identified by the Bonferroni-corrected pairwise contrasts in Table 7.

Table 6 Within- and between-group comparisons of executive function subdomains from baseline to post-test

Executive subdomains	Group	Baseline Med [IQR]	Post-test Med [IQR]	Change Med [IQR]	Effect Size (*r*)	*p*-value	95% CI for *r*	Median change, 95% CI
Working memory	Aerobic	17.0 [15.0–19.0]	20.1 [18.2–22.5]	3.1 [2.2–3.9]	0.62	**< 0.001***	[0.24, 0.83]	3.1 [2.50, 3.70]
	Coordination	16.0 [15.0–18.0]	21.2 [19.5–24.1]	5.2 [4.6–5.9]	0.75	**< 0.001***	[0.46, 0.90]	5.2 [4.74, 5.66]
	Control	16.0 [15.0–18.0]	16.5 [15.2–17.8]	0.5 [0.2–0.7]	0.12	0.432	[-0.34, 0.53]	0.5 [0.32, 0.68]
Selective attention	Aerobic	335.0 [294–382]	365.5 [340–380]	30.5 [25.0–40.0]	0.55	**< 0.001***	[0.14, 0.80]	30.5 [25.23, 35.77]
	Coordination	371.0 [317–392]	400.0 [380–425]	29.0 [17–42]	0.58	**< 0.001***	[0.18, 0.85]	29.0 [20.22, 37.78]
	Control	335.0 [301–388]	340.0 [315–361]	5.0 [4.0-8.4]	0.05	0.518	[-0.40, 0.48]	5.0 [3.46, 6.54]

Data are presented as median [IQR]. Within-group effect sizes are reported as r ($r = Z/\sqrt{N}$) with bias-corrected and accelerated bootstrap 95% confidence intervals (5,000 resamples)

* $p < 0.05$. Selective attention is the d2 total number of items processed (TN); concentration performance and the error rates were computed during scoring but were not analysed, and the interpretation of TN is qualified accordingly throughout. The partial eta squared values of 0.89 and 0.91 reported for the group × time interaction in the originally submitted version were withdrawn because they had been taken from the omnibus model sums of squares rather than from the group × time term. They have been recomputed by the method set out in the Statistical Analyses section and are now reported, with their confidence intervals, in the text immediately below this table and in the Abstract. Between-group effect sizes for the three pairwise contrasts are given in Table 7

Table 7 Post-hoc pairwise comparisons of executive function subdomains using Bonferroni correction

Executive Function	Comparison	Mean Rank Difference	SE	Adjusted *p*	Result	Effect size (rank-biserial *r*)	95% BCa CI
Working memory	CEG vs. CG	5.12	0.82	**< 0.001***	CEG > CG	0.81	[0.69, 0.88]
	AEG vs. CG	3.06	0.91	**0.018***	AEG > CG	0.43	[0.20, 0.62]
	CEG vs. AEG	2.11	0.88	**0.045***	CEG > AEG	0.31	[0.06, 0.52]
Selective attention	CEG vs. CG	4.95	0.79	**< 0.001***	CEG > CG	0.81	[0.70, 0.88]
	AEG vs. CG	3.87	0.85	**0.007***	AEG > CG	0.59	[0.39, 0.73]
	CEG vs. AEG	1.12	0.92	0.320	NS	0.16	[-0.10, 0.40]

CEG Coordination Exercise Group, *AEG* Aerobic Exercise Group, *CG* Control Group, *SE* Standard Error, *NS* Non-significant

* $p < 0.05$. Between-group effect sizes are rank-biserial correlations computed on the change scores, with bias-corrected and accelerated bootstrap 95% confidence intervals (5,000 resamples). Effect sizes derived from rank-transformed data are interpreted in relative terms only. *p*-values are Bonferroni-adjusted

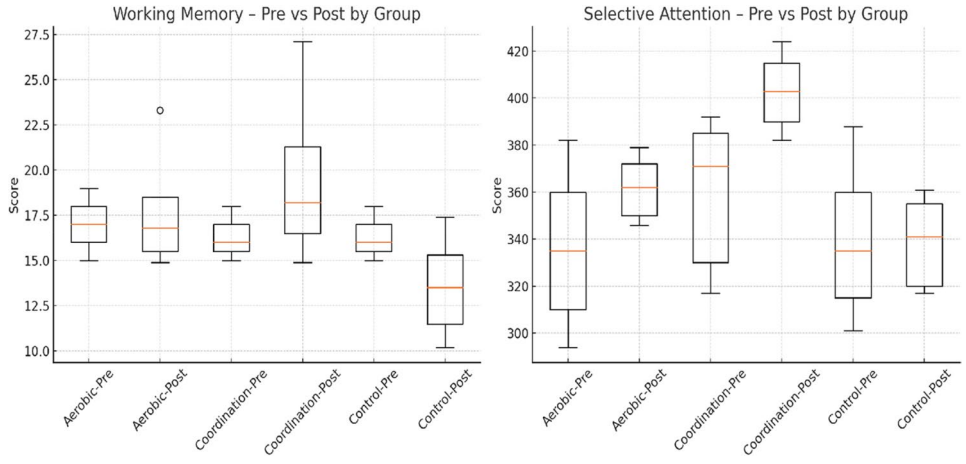


Fig. 2 Changes in working memory (digit span total score) and selective attention (d2 total number of items processed, TN) from baseline to post-test across the aerobic, coordination and control groups. Boxes show the median and interquartile range, whiskers the 1.5 × IQR range, and points individual participants. Note that the two panels use different y-axis scales

Bonferroni-corrected post hoc analyses (Table 7) confirmed that both experimental groups performed significantly better than the control group on both outcomes (all adjusted $p \leq 0.018$). For working memory the coordination group also outperformed the aerobic group (mean rank difference 2.11, SE 0.88, adjusted $p = 0.045$; rank-biserial $r = 0.31$, 95% BCa CI [0.06, 0.52]), a difference of approximately 2.1 points in median change (Hodges–Lehmann median difference 2.1 points, 95% BCa CI [1.44, 2.76]); for selective attention the two intervention groups did not differ (mean rank difference 1.12, SE 0.92, adjusted $p = 0.320$; rank-biserial $r = 0.16$, 95% BCa CI [– 0.10, 0.40]), the median changes being 29.0 and 30.5 d2 points respectively (Hodges–Lehmann median difference – 1.5 points, 95% BCa CI [– 10.48, 7.48]). The confidence interval for the coordination versus aerobic contrast in working memory excludes zero but extends close to it, so the direction of the effect is supported while its magnitude remains imprecisely estimated; the corresponding interval for selective attention spans zero comfortably. Examination of the box plots in Fig. 2 shows that the CEG had the highest median improvement in working memory and the narrowest interquartile range, whereas in selective attention the two intervention groups overlapped substantially and the CEG showed the wider dispersion (IQR 17–42 vs. 25.0–40.0).

Discussion

The purpose of this study was to compare the effects of a dose-matched aerobic and gross-motor coordination programme on working memory and selective attention in 10-12-year-old children. Both hypotheses concerning the group comparisons were supported. Both protocols improved both outcomes relative to the usual-curriculum control (H1), and the coordination programme produced a larger working memory gain than the aerobic programme (median change + 5.2 vs. + 3.1 points, adjusted $p = 0.045$) while the two did not differ in selective attention (+ 29.0 vs. + 30.5 d2 points, adjusted $p = 0.320$) (H2). Because the two arms were prescribed the same session frequency, duration, programme length and rated perceived exertion, the dissociation between the two outcomes is the most informative result of the trial: on the assumption that those prescriptions were met, which the Limitations examine, it is difficult to attribute the working memory advantage to a difference in exercise dose, and correspondingly easier to attribute it to the cognitive-coordinative content of the sessions. This is precisely the

pattern predicted by separating the intensity-driven from the task-driven mechanistic pathways in the Background.

The most informative finding was the superiority of the CEG over the AEG in working memory (median change + 5.2 [4.6–5.9] vs. + 3.1 [2.2–3.9] points; adjusted $p = 0.045$). Throughout this manuscript, "cognitive engagement" is used in the specific sense defined in the Methods: the extent to which the movement task itself requires the child to maintain a goal in working memory, allocate selective attention, inhibit a prepotent response, and switch rules under time pressure. It does not refer to enjoyment, motivation, effort or the addition of academic content to a physical activity session. Read in this sense, our result converges with three lines of evidence rather than merely sitting alongside them. First, Schmidt and colleagues [21] found that cognitively engaging chronic physical activity, but not aerobic exercise, affected executive functions in primary school children -- the same dissociation we observe, obtained with a different task family and a different age band, which argues that the effect is carried by cognitive demand rather than by any particular movement form. Second, the school-based coordination programme reported by Forte et al. [11] improved visuospatial working memory in 9-10-year-olds; our finding extends that result to verbal-auditory working memory and, because our comparator was an active dose-matched aerobic arm rather than a usual-curriculum control, it removes the ambiguity about whether the gain simply reflected added activity. Third, neuroimaging work showing increased prefrontal activation and perfusion during cognitively loaded exercise [49, 50] supplies a plausible proximate mechanism for why a task that repeatedly taxes maintenance and updating should improve a maintenance-and-updating outcome. Taken together, these three strands support a specificity account: the executive process that is loaded during training is the executive process that improves. The size of the advantage should nonetheless be kept in proportion. Two points of median change on a summed multimodality digit span, with an adjusted p of 0.045 in a sample of 52 exercising children, is a modest and fragile result that requires independent replication before it is translated into curriculum recommendations.

Although the AEG improved less than the CEG in working memory, it achieved a significant improvement compared with the CG ($p = 0.018$). Research has consistently shown that moderate-to-vigorous physical activity is associated with improvements in executive functions such as working memory, cognitive flexibility, and inhibitory control [51, 52]. For instance, a meta-analysis by Vazou et al. [53] demonstrated that aerobic exercise positively influenced cognitive performance in young individuals. It has additionally been reported that HIIT not only enhances physical fitness but also challenges cognitive processes, thereby supporting the development of executive functions [54, 55]. Consistent with this literature, the significant improvement observed in the AEG relative to the CG indicates that aerobic-based interventions can also confer cognitive benefit, and the present comparison suggests, without establishing, that such gains may be larger when cognitive engagement is increased.

Selective attention improved substantially in both intervention groups relative to control (+ 30.5 and + 29.0 d2 TN points versus + 5.0 in the CG), but the two intervention groups did not differ from one another (adjusted $p = 0.320$). This null result is theoretically informative rather than merely negative. If cognitive engagement acted as a general, domain-nonspecific enhancer of cognition, the CEG should have led on both outcomes; it did not. The pattern is instead consistent with selective attention being predominantly sensitive to the arousal-activation and cerebral-perfusion pathways, which both protocols loaded equally because they were designed to be matched for perceived exertion, whereas working memory is additionally sensitive to the task-driven cognitive-engagement pathway, which only the CEG loaded. Two qualifications restrict how far this reading can be taken. First, the attentional outcome is the number of items processed, so what the null result strictly shows is that the two programmes did not differ in the volume of material children worked through on a cancellation task. Because concentration performance and the error rates were computed but not analysed, we cannot establish that the two arms were also equivalent in the accuracy of that processing, and the inference is confined accordingly. Second, a ceiling consideration also applies: the d2 is a speeded cancellation task on which practice effects and the sheer act of being tested twice contribute to improvement, which compresses the room for a between-intervention difference to emerge.

The comparison literature supports this reading rather than contradicting it, and it divides along the same line. On one side, studies that vary session content report content effects: programmes of many formats improve selective attention [56, 57], a combined aerobic and coordination intervention delivered twice weekly for eight weeks raised attention in preschool children [58], and a ten-week jump-rope programme improved selective attention accuracy [59]. Where a direct contrast has been drawn, tasks demanding temporal precision, rapid response and coordinative processing have related more closely to attentional performance than continuous repetitive running [60], game-based instruction has outperformed direct instruction [61], and cognitively loaded coordination work has outperformed conventional exercise [62, 21]. On the other side, studies that vary dose report dose effects: children exercising twice weekly outperformed those exercising once [63], and

two daily 20-minute bouts of moderate activity improved selective attention [64]. Our null between-arm result sits with the second group. Both arms were delivered three times weekly at a matched perceived exertion, so on a dose account they should produce equivalent attentional gains — which is what we observed — while content differentiated them only on working memory. Read together with the working memory result, this suggests that selective attention in this age range responds primarily to how much children move and how often, whereas working memory responds additionally to what the movement demands of them.

A recent trial in a younger population bears directly on this account and qualifies it in a useful way. Başarır et al. [89] delivered an eight-week, twice-weekly, 25-minute coordination-based training programme in a game format to 51 preschool children aged 5–6 years, and assessed physical fitness, motor competence (KTK3+) and inhibitory control (Go/No-Go). The exercise group improved significantly in vertical jump and in the KTK backward-balance item relative to controls, but the programme produced no significant improvement in static balance, dynamic balance or agility, and its effects on inhibitory control were limited. Read alongside the present findings, that pattern is informative in three respects. First, it converges with our result in showing that coordination-based content reliably shifts motor outcomes; the divergence concerns the cognitive side. Second, the dose differed substantially — 16 sessions of 25 min against our 42 sessions of 40 min — and the executive benefit of cognitively engaging movement appears in meta-analysis to be dose-dependent [20], so the limited inhibitory-control effect in an eight-week programme does not contradict a working memory effect in a fourteen-week one. Third, and most usefully, the two studies differ in the executive domain assessed: Başarır et al. indexed inhibition, whereas the present trial indexed working memory and selective attention. Our own data show that the coordination advantage is domain-specific rather than general, appearing in working memory and not in selective attention, and the absence of a clear inhibition effect in preschoolers is consistent with that specificity rather than with a failure of the approach as such. Because inhibition was explicitly loaded by our own protocol but was not assessed, their finding also identifies a concrete gap in the present design, and the comparison strengthens the case that future trials of coordination training should assess inhibition, working memory and flexibility together rather than relying on a single executive index.

The mechanism assumed throughout this discussion — that adding a cognitive demand to a movement task changes what the task requires of the performer — has been demonstrated directly in dual-task experiments, although in populations distant from the present one. Jouira et al. [90] examined postural balance in 20 adolescents with mild to moderate intellectual disability across five vision conditions crossed with single-task and dual-task settings, and found that adding a concurrent cognitive task increased postural instability, that the effect was amplified when the cognitive task was made more demanding, and that cognitive performance itself degraded when the postural requirement was hardest. In a companion study, the same group [91] compared trained with untrained adolescents with intellectual disability under visual and auditory cognitive tasks and reported that the interaction between cognitive load and postural control differed according to physical activity history. These findings support two premises on which the present interpretation rests: that cognitive and motor demands compete for shared control resources, so that a movement task with an added rule is not simply the same task performed with an extra step; and that habitual physical activity modifies how that competition resolves, which is what a fourteen-week cognitively loaded programme would be expected to exploit. The evidential weight of this support is limited and we do not overstate it. Both studies were acute, cross-sectional dual-task experiments rather than training trials; both used postural balance as the motor outcome rather than an executive-function test; and both were conducted in adolescents with intellectual disability, a population whose baseline motor and executive capacity, and whose dual-task cost, differ systematically from those of typically developing school-age children. They are therefore cited here as theoretical corroboration for the cognitive-motor interaction that the coordination protocol was designed to exploit, and not as evidence for the magnitude or the transfer of any training effect in the present sample.

Positive effects of high-intensity exercise programmes on selective attention have been reported [65], and a single high-intensity exercise session has been shown to improve neural processing related to selective attention [66]. Similar improvements in selective attention were observed following moderate-to-high intensity endurance, strength, and coordination exercises of 25 min each, without coordination exercises demonstrating a clear advantage [67]. High-intensity interventions have been found to produce improvements in attention [68, 69], and HIIT programmes have been reported to increase selective attention [70]. Brief aerobic exercise participation may generate an acute positive effect on selective attention [71], and aerobic fitness during childhood has been proposed as particularly beneficial for selective attention and inhibition [72]. Acute aerobic exercise sessions have been shown to improve selective attention performance in children [73] and to enhance executive function skills [74, 75]. It has also been noted that moderate-intensity aerobic exercise may represent the optimal level for

improving cognitive processing efficiency, particularly in terms of attentional allocation [76]. Therefore, more standardised intervention studies comparing intensity, duration, and frequency variables in a controlled manner are needed to determine the exercise dose that produces the strongest effect on selective attention.

Coordination and aerobic exercise interventions in this study were maintained for 14 weeks. Hillman et al. [77] observed that a 9-month intervention significantly improved executive functions, particularly working memory. Conversely, interventions shorter than 6 weeks typically yield inconsistent results due to insufficient exposure [78]. This is consistent with the findings of Pesce et al. [79], who reported that physically active interventions sustained over several months with cognitive engagement provide more pronounced cognitive benefits. Within this scope, intervention duration is considered a determinant factor in the process of improvement in cognitive functions.

The exercise intervention protocols in this study were administered in three sessions per week. A review of the literature indicates that interventions applied more than twice per week yield better results, suggesting that intervention frequency also exerts an important effect. Ludyga et al. [30] proposed that regular participation in cognitively challenging physical activities supports sustained cognitive engagement and neural adaptation processes. Frequent and prolonged exposure to such activities is thought to support cognitive development [80]. Therefore, physical activity interventions should be designed to be applied more frequently to achieve meaningful cognitive gains.

The exercise session duration applied to the AEG and CEG in this study was planned as 40 min. Sessions lasting more than 20 min have been shown to be more beneficial for cognitive outcomes compared with shorter sessions [31]. Longer sessions allow more complex cognitive tasks to be implemented and greater mental effort to be expended, thereby yielding better outcomes in executive functions [81, 80]. Furthermore, as noted by de Greeff et al. [82], longer sessions may increase the cardiovascular and metabolic responses necessary for cognitive benefits. This is further supported by research demonstrating that longer physical activity sessions lead to significant improvements in cognitive performance [83]. Therefore, physical activity sessions should be of sufficient length to ensure adequate cognitive and physical engagement in order to maximise cognitive benefits.

Sex was not examined as a moderator in this trial, and the reason is worth stating explicitly rather than leaving to the Limitations. With 12–14 children of each sex per arm, 80% power was available only for a three-way group × time × sex interaction of partial eta squared ≥ 0.11, a large effect by conventional criteria. An analysis of that kind would have been uninformative in both directions: a null result would not have been evidence of equivalence, and a positive result would have been unstable. We have therefore withdrawn the moderation aim and the sex-stratified subgroup table that appeared in the original submission, together with the claim that girls benefited more. The existing literature is itself inconsistent on this question — some studies report a female advantage on verbal and attentional tasks in the prepubertal period [15, 20], others a male advantage on visuospatial tasks during childhood [84, 85, 86, 16] — and resolving it will require trials designed and powered a priori for the interaction, with maturity-matched rather than merely age-matched subgroups. Nothing in the present data speaks to it either way. The correlation between the working memory and selective attention change scores was weak and non-significant (Spearman's rho = 0.12, $p > 0.05$). This is consistent with the two constructs being dissociable, and with the differential intervention effect described above: children who improved most in working memory were not systematically those who improved most in selective attention.

Developmental considerations. The 10-12-year window is not neutral with respect to these findings. Working memory capacity and the fronto-parietal networks that support it are still on a steep developmental trajectory at this age, whereas basic visual search and cancellation performance, which the d2 indexes, matures earlier; a training stimulus applied during a period of rapid change in one system and relative stability in another would be expected to produce exactly the outcome-specific pattern we observed. Motor learning capacity is also high in this band, so the closed-to-open skill progression could be advanced quickly enough for the cognitive load, rather than the motor load, to remain the limiting factor. Evidence from younger populations is informative here: in preschool children, motor skills, inhibition and visuospatial working memory are already coupled [17], and sex-specific associations between BMI category, movement behaviours and executive functions are detectable well before puberty [27]. Whether the coordination advantage we report would be larger in younger children -- who have more headroom in motor competence -- or smaller -- because their executive systems are less able to exploit the task's demands -- cannot be determined from the present design and is a clear direction for future work.

The wider movement ecology. Three 40-minute sessions per week represent roughly 2 h of a child's 168-hour week, and the cognitive effect of that addition necessarily depends on the remaining 166. Within the 24-hour movement behaviour framework, physical activity, sedentary behaviour and sleep compete for a fixed time budget, so an added session may displace sedentary screen time in one child and sleep in another, with

opposite implications for executive function. SUNRISE-based studies have shown both that a minority of young children meet integrated movement guidelines and that profiles differ markedly between urban and rural settings [28, 29], which means that habitual movement behaviour is a plausible source of the substantial within-group variability visible in our box plots (Fig. 2). Sleep is the component most directly implicated in prefrontal-dependent performance: short or fragmented sleep degrades working memory and sustained attention on the following day, so unmeasured variation in sleep between children -- and any change in sleep induced by an afternoon exercise programme -- could either amplify or mask a true intervention effect [29]. Environmental and contextual factors act on the same chain. Access to safe outdoor space, seasonal and weather constraints, school facilities, and family socioeconomic circumstances all shape how much and what kind of activity a child accumulates outside the intervention [29, 27]. Our participants were drawn from a single urban school, which limits this variability but also limits generalisability: the same programme delivered in a rural or a lower-resource setting, where baseline habitual activity and sleep profiles differ, might not produce the same effect. Future trials in this area should measure the full 24-hour movement profile with accelerometry alongside the intervention, and treat it as a covariate or moderator rather than ignoring it.

The practical implication of these findings is narrow and specific, and we state it that way deliberately. This trial does not speak to the relative merits of team sports, exergaming, active academic lessons, martial arts or open-skill versus closed-skill sport in general; it compared two structured school-based programmes that were identical in dose and differed only in coordinative-cognitive demand. Within that narrow comparison, the result suggests that when a school has already committed a fixed amount of curricular time to additional physical activity, filling that time with tasks that require children to remember, monitor, inhibit and switch movement rules may yield a working memory benefit over filling it with motorically simple high-intensity activity — at no cost to the volume of material processed on a cancellation test of selective attention, which improved comparably under both. The practical requirements are correspondingly modest: no equipment beyond ropes, markers and balls, no additional curricular time, and a session manual that specifies which executive component each task loads. One mediator we did not measure deserves mention, because it offers a competing explanation: children who find an activity enjoyable practise more and persist longer, and enjoyment is generally higher in varied, rule-based tasks than in repetitive running, so part of the coordination advantage may reflect engagement rather than cognitive load as such [87, 88]. Distinguishing the two would require measuring situational interest alongside the cognitive outcomes. What the finding does not license, in any case, is the substitution of coordination work for cardiorespiratory training, whose health benefits are established and lie outside the scope of this trial [34].

Limitations

Several limitations qualify these findings. First, the trial cannot speak to moderation by sex. It was powered for the group × time interaction and not for a three-way group × time × sex interaction; with 12–14 children of each sex per arm, 80% power was available only for a moderation effect of partial eta squared ≥ 0.11, that is, only for a very large one. Sex has therefore been withdrawn as an analytical aim in this revision, along with the sex-stratified subgroup table and the claim that girls benefited more, both of which appeared in the original submission. Nothing in these data should be read as evidence that the intervention effects do, or do not, differ by sex. Second, the total sample ($n = 78$) and the narrow age range (10–12 years) limit generalisability to other age groups and school contexts, and all participants were recruited from a single urban state school, so school-level and contextual factors are entirely confounded with the sample. Third, the 14-week duration, while longer than most published trials, is short relative to the timescale over which executive functions consolidate; no follow-up assessment was conducted, so it is unknown whether the working memory advantage persists after the programme ends; cardiorespiratory fitness and motor coordination were also not reassessed at post-test, so no manipulation check confirms that the two programmes produced their intended physiological and motor adaptations. Fourth, biological maturation was estimated non-invasively from PHV offset [32] rather than by a direct method such as skeletal age or hormonal assay; PHV offset carries appreciable individual error, particularly around the onset of the growth spurt, and residual maturity confounding cannot be excluded. Fifth, habitual physical activity, sedentary behaviour and sleep were assessed only by parent-proxy report and were not monitored objectively during the intervention; because these behaviours share a fixed 24-hour time budget with the intervention and independently predict executive function, unmeasured variation in them is a plausible source of the residual within-group variability, and it is unknown whether the programme displaced sedentary time, sleep, or other activity [28, 29]. Sixth, no measure of diet, screen exposure or academic workload was obtained. Seventh, internal consistency and inter-rater agreement for the two cognitive instruments were not formally quantified in this sample, so measurement error cannot be estimated directly. Eighth, executive function was operationalised with a single

task per construct -- digit span for working memory and the d2 for selective attention -- so task-specific practice effects cannot be separated from construct-level improvement, and inhibition and cognitive flexibility were not assessed at all despite being explicitly loaded by the coordination programme. Ninth, the children and instructors could not be blinded, so expectancy effects in the intervention arms cannot be excluded, although assessors and the primary analyst were blinded. Tenth, the control group received no attention-matched placebo activity, so the difference between each intervention and the control group reflects the combined effect of exercise, adult attention and group participation. Eleventh, the trial was registered retrospectively, which is a reporting limitation notwithstanding the pre-existing ethics-approved protocol. Twelfth, selective attention was indexed primarily by the d2 total number of items processed (TN), which reflects processing volume and is therefore sensitive to a speed-accuracy trade-off; concentration performance (CP) and the error rates were computed during scoring but were not analysed, so the possibility that the observed gain in TN partly reflects a shift in response criterion — children scanning faster and less carefully — rather than an improvement in selective attention itself cannot be excluded. Every attentional claim in this manuscript is qualified accordingly and should be read as a claim about processing volume on a cancellation task. Finally, two features of the analysis should be weighed by the reader, and they are the two on which we would most welcome the reader's scepticism. First, and most important for the central interpretation of this trial, the equating of internal load between the two intervention arms is a property of the protocol rather than a demonstrated result. Both arms were prescribed the same rated perceived exertion range and the same heart-rate band, both were monitored continuously against those targets during delivery, and no session was delivered outside them; but session-level summaries were not compiled, so no aggregate RPE or heart-rate values and no between-arm comparison of measured internal load are reported here. The two programmes are accordingly described throughout as having been designed to be matched, never as having been shown to be matched. The consequence is material: a residual between-arm difference in exercise intensity cannot be excluded, and if one existed it would be confounded with the difference in cognitive-coordinative content, which is the very contrast the study was built to isolate. The working memory result should be read with that possibility open. Second, effect sizes derived from rank-transformed data are not directly comparable with those from raw-score parametric models, and the bootstrap confidence intervals around the between-group effect sizes are wide, so the magnitude of the coordination advantage in working memory should be treated as imprecisely estimated even though its direction is supported.

Conclusions

In this randomized controlled trial, 14-week aerobic and gross-motor coordination programmes, designed a priori to be matched for frequency, duration and rated perceived exertion, were compared in children aged 10–12 years. Both programmes were associated with improvements in working memory and selective attention relative to a usual-curriculum control group, the latter indexed by the volume of material processed on a cancellation test rather than by an accuracy-corrected measure, and the coordination programme was associated with a modestly larger working memory gain than the aerobic programme, while the two did not differ in selective attention. The findings suggest that the cognitive-coordinative demand of a session may contribute to its working memory benefit over and above its metabolic intensity, but the between-intervention difference was small, its confidence interval wide, and was obtained in a single school with a modest sample, so it should be regarded as preliminary. Whether the interventions affect girls and boys differently remains open; the trial was not powered to answer it. These results may inform the design of school-based physical activity programmes, and indicate that structuring sessions to include coordinative and cognitively demanding tasks is a potentially worthwhile addition rather than an established requirement. Adequately powered, longer, multi-school trials that assess a broader battery of executive functions — including inhibition and cognitive flexibility, which the coordination protocol loaded but which were not measured here —, include follow-up measurement, and monitor 24-hour movement behaviours objectively are needed before firmer recommendations can be made.

Abbreviations

AEG	Aerobic Exercise Group
CEG	Coordination Exercise Group
CG	Control Group
BDNF	Brain-Derived Neurotrophic Factor
HIIT	High-Intensity Interval Training
PHV	Peak Height Velocity
BMI	Body Mass Index
VO_2max	Maximal Oxygen Uptake
RPE	Rating of Perceived Exertion
MAD	Median Absolute Deviation
IQR	Interquartile Range
CI	Confidence Interval
ART-ANOVA	Aligned Rank Transform Analysis of Variance

Supplementary Information

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Supplementary Material 1

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Authors' contributions
SB: Writing – original draft, Writing – review and editing, Investigation, Formal Analysis, Methodology. MSC: Formal Analysis, Investigation, Writing – original draft, Data curation, Methodology, Supervision, Investigation, Visualization. GA: Formal Analysis, Writing – original draft, Writing – review and editing, Investigation, Methodology, Supervision.

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Data availability
The datasets generated and/or analysed during the current study are not publicly available due to ethical and privacy restrictions involving minor participants. However, anonymized data are available from the corresponding author upon reasonable request and subject to approval by the relevant Ethics Committee.

Declarations

Ethics approval and consent to participate
All study procedures were conducted in accordance with the principles of the Declaration of Helsinki and were approved by the Ethics Committee of Istanbul Rumeli University (Meeting No: 2026/02, Decision No: 08, Approval Date: 27 February 2026). Written informed consent was obtained from the parents or legal guardians of all participants, and written informed assent was obtained from the children prior to participation. Prior to participation, the children and their parents or legal guardians were informed about the purpose, procedures, potential risks and benefits of the study. Written informed consent was obtained from the parent or legal guardian of every child, and written informed assent was obtained from every child. Participation was voluntary, and participants were free to withdraw from the study at any time without any consequences.

Consent for publication
Not applicable.

Competing interests
The authors declare no competing interests.

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