Can Dance Education Support Students' Social–Emotional Competence? A Systematic Review and Meta-Analysis

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ABSTRACT

Background and purpose: Structured dance education combines embodied expression, interpersonal coordination, and guided participation, creating pedagogical opportunities to develop social–emotional competence (SEC). However, prior syntheses have combined educational dance with therapeutic or health-oriented programmes, leaving its pedagogical contribution unclear. This systematic review and meta-analysis mapped how SEC has been conceptualised and measured in structured, nonclinical dance education, estimated its average effect, and examined descriptive variation across study and programme characteristics.

Methods: Five databases were searched on 18 July 2026. Thirty-eight reports representing 35 independent study families met the review criteria. Risk of bias was assessed using RoB 2 or ROBINS-I. Nine independent families providing direct dance-versus-non-dance comparisons (N = 1,067) entered a REML random-effects meta-analysis with modified Hartung–Knapp inference. Family-level effects avoided double-counting companion reports and correlated outcomes.

Results: The evidence spanned all educational stages but varied substantially in SEC domains, measures, dance forms, and comparators; 14 of the 35 families explicitly identified a theoretical or conceptual framework. The primary synthesis showed a positive average effect in favour of dance education (Hedges' g = 0.43, 95% CI [0.13, 0.73], p = .011). Heterogeneity was moderate to substantial (I^2 = 64.3%), and the prediction interval indicated meaningful variation across settings (95% PI [−0.32, 1.17]). Alternative outcome-aggregation procedures yielded comparable estimates (g = 0.38–0.40). None of the primary families provided low-risk evidence, and available data did not establish moderation by design, comparator, dance type, duration, or educational stage.

Conclusions: Structured dance education may support students' SEC, although outcomes are likely to depend on how programmes organise movement, interaction, and reflection within particular educational contexts. The synthesis shifts the question from whether dance works in general to which pedagogical features may support particular competencies, for whom, and under what conditions.

Impact statement: By separating structured, nonclinical dance education from therapeutic and non-isolable multicomponent evidence and linking companion reports into independent study families, this review provides a pedagogically focused evidence map and pooled estimate. It offers researchers a framework for examining programme features, implementation, and measurement. For educational practice, it highlights aligning movement tasks, interaction structures, and reflection with explicit SEC goals. Together, the findings position dance education as a promising, context-sensitive pedagogical resource and focus attention on how embodied participation may develop into transferable social–emotional competence.

Keywords: dance education; social–emotional competence; systematic review; meta-analysis; embodied learning; students

INTRODUCTION

Social–Emotional Competence and Dance Education

Social–emotional competence (SEC) refers to the capacities used to understand and regulate emotion, relate to others, and act effectively in social contexts. SEC supports participation in learning and longer-term adjustment. Early social competence predicts later educational, occupational, mental-health, and behavioural outcomes (Jones, Greenberg, and Crowley 2015). Meta-analyses of universal social and emotional learning programmes report gains in social–emotional skills and in broader outcomes, including behaviour, well-being, and academic achievement, with some effects sustained at follow-up (Cipriano et al. 2023; Corcoran et al. 2018; Durlak et al. 2011; Sklad et al. 2012; Taylor et al. 2017). These effects nevertheless vary with implementation and context (Durlak and DuPre 2008; Wigelsworth et al. 2016). Because these effects are context-sensitive, attention must also be given to the learning environments in which SEC is developed.

Dance education is one such environment because it combines bodily action, expression, attention, and interaction within sustained learning. It may involve learning and performing movement, improvising or composing, coordinating with others, and reflecting on expressive choices (Hanna 2008). The present review defines dance education as instructor-organised activity in which dance is the central content and participants are actively engaged in learning, creating, performing, or reflecting on movement. It excludes dance/movement therapy, general exercise, passive viewing, and one-off movement inductions. Dance-centred multicomponent programmes in which the contribution of dance could not be isolated were retained for RQ1 evidence mapping but were ineligible for the strict RQ2 meta-analysis. These distinctions matter because educational dance, therapeutic dance, and general physical activity differ in purpose and delivery (Koch et al. 2014; Koch et al. 2019). Comparator choice also determines the intervention contrast: no-treatment or usual-practice comparators estimate the effect of the dance-education package, whereas active comparators estimate its incremental effect relative to another structured activity (Fong Yan et al. 2024).

Theoretical Pathways and Pedagogical Conditions

Dance may contribute to SEC through the way it engages the body, organises interaction, and structures participation. Embodied accounts suggest that emotional and social processes draw on perceptual, motor, and bodily states (Barsalou 2008; Niedenthal 2007). Dance repeatedly links the observation and production of movement with intention and affect. Research using dance as a model has connected perception–action coupling with emotion and social understanding (Sevdalis and Keller 2011). Attending to bodily sensation and translating affect into movement may support the recognition and regulation of internal states, while interpreting another person's movement may support emotion recognition and social awareness.

Many forms of dance also require interpersonal coordination. Synchronous action and joint music-making have increased cooperation and helping in adults and children (Cirelli, Einarson, and Trainor 2014; Kirschner and Tomasello 2010; Wiltermuth and Heath 2009), particularly when coordination involves shared intention and manageable demands (Kokal et al. 2011; Reddish, Fischer, and Bulbulia 2013). Dance-specific studies further associate synchrony with bonding within and across groups (Tarr et al. 2015; Tunçgenç and Cohen 2016), consistent with meta-analytic evidence linking synchrony to prosocial outcomes (Mogan, Fischer, and Bulbulia 2017). Mirroring, shared rhythm, and coordinated movement may therefore support empathy, cooperation, and relationship skills by aligning attention and action around a common goal (Behrends, Müller, and Dziobek 2012).

These possibilities depend on how dance is taught. Creative and cooperative programmes can provide opportunities for choice, feedback, persistence, and negotiation, whereas technique-centred programmes may place greater emphasis on accuracy, evaluation, and individual performance. However, appearance-focused and highly evaluative practices may increase self-scrutiny or body objectification (Radell et al. 2014). Dance participation should therefore not be assumed to produce uniformly positive outcomes. Moreover, some benefits may arise from physical activity rather than dance-specific learning (Lubans et al. 2016; Rodriguez-Ayllon et al. 2019). Accordingly, dance type, programme duration, educational stage, and comparator choice may contribute to variation in observed effects.

Existing Evidence and the Need for a Focused Synthesis

The evidence base is growing but remains fragmented. Broad reviews have examined recreational dance and youth well-being (Burkhardt and Brennan 2012), dance and dance/movement therapy across psychological outcomes (Koch et al. 2014; Koch et al. 2019), self-related outcomes (Schwender et al. 2018), physiological and psychological outcomes in young people (Tao et al. 2022), and dance relative to other forms of physical activity (Fong Yan et al. 2024). More closely related syntheses have addressed emotional intelligence and dance (San-Juan-Ferrer and Hípola 2020), dance as a vehicle for social and emotional learning in primary-school populations (Calçada and Gilham 2022), embodied approaches to dance education (Chai et al. 2026), and dance interventions for adolescent prosocial behaviour (Brown et al. 2026).

These reviews establish the relevance of the topic but address different questions. They vary in population, dance practice, intervention purpose, comparator, and outcome scope. Some combine educational dance with therapeutic or health-oriented activity; others focus on a single developmental stage or outcome domain and provide descriptive rather than quantitative synthesis. To our knowledge, no previous review has simultaneously isolated instructor-organised dance education, examined multiple domains of SEC across educational stages, linked multiple reports to their underlying studies, and estimated a pooled effect from direct comparisons.

The evidence also presents three synthesis challenges. Relevant outcomes appear under different labels and vary in manifestation, reporter, and measurement approach. Educational, therapeutic, exercise-based, and multicomponent interventions do not estimate the same effect, nor do active and inactive comparators. In addition, multiple reports, outcomes, or time points may be statistically dependent or belong to the same underlying study family. Treating them as independent would give some studies disproportionate influence and overstate precision. A defensible synthesis must therefore preserve the breadth of the literature while distinguishing the evidence that can support a quantitative estimate.

The Present Review: Aims and Research Questions

This systematic review and meta-analysis therefore maps how dance education and SEC have been conceptualised and studied, estimates the average effect among independent study families providing direct comparisons, and examines descriptive variation across substantive and methodological characteristics. It addresses three research questions:

RQ1: What are the study, participant, intervention, comparator, and outcome characteristics of the evidence base on dance education and SEC, and how has SEC been conceptualised and measured?

RQ2: Among independent study families providing direct comparisons, what is the average effect of dance education on SEC, and how precise and heterogeneous is that estimate?

RQ3: How do effect estimates vary descriptively by study design, dance type, comparator, programme duration, and educational stage?

METHOD

This review followed PRISMA 2020 and PRISMA-S (Page et al. 2021; Rethlefsen et al. 2021) and was not prospectively registered. Initial PICOS criteria and the report–family distinction were documented before full-text assessment. Effect-construction and analysis rules were formalised after selection and pilot extraction but before formal synthesis; later amendments appear in Supplementary Table S13.

Eligibility Criteria

Eligibility criteria were based on the PICOS framework. Studies were eligible if they included: (1) Population—learners in preschool, primary, secondary, higher, or vocational education, including special education settings; (2) Intervention—structured, nonclinical programmes centred on active dance learning in curricular, extracurricular, or community settings, including creative dance, traditional or folk dance, ballet, and educational Biodanza; (3) Comparator—no intervention, waitlist, usual education, a non-dance alternative, or another dance

programme; (4) Outcomes—overall social–emotional competence or at least one domain specified before full-text screening: emotion knowledge or expression, self-regulation, empathy or perspective-taking, social competence or communication, relationships, belonging, or social connectedness, or prosocial behaviour, cooperation, teamwork, conflict resolution, or responsible decision-making; and (5) Study design—individually randomised trials, cluster-randomised trials, or non-randomised studies with a concurrent comparison group, including controlled pre–post designs. Mixed-methods studies were eligible when they reported comparative quantitative data. Journal articles, theses, full-text conference papers, and research reports were considered without restrictions on publication year or language.

The exclusion criteria were as follows: (1) Non-student populations and ineligible interventions: studies involving non-student participants or focusing on dance/movement therapy, clinical treatment, rehabilitation, general exercise, or passive exposure to dance; (2) Ineligible outcomes: studies that did not assess an eligible social–emotional outcome; self-esteem, confidence, motivation, general well-being, creativity, academic performance, and physical outcomes were not eligible when reported alone; (3) Insufficient comparative evidence: studies lacking a concurrent comparison group or comparative quantitative data; and (4) Non-original reports: reviews, theoretical papers, protocols, and other publications without original empirical findings.

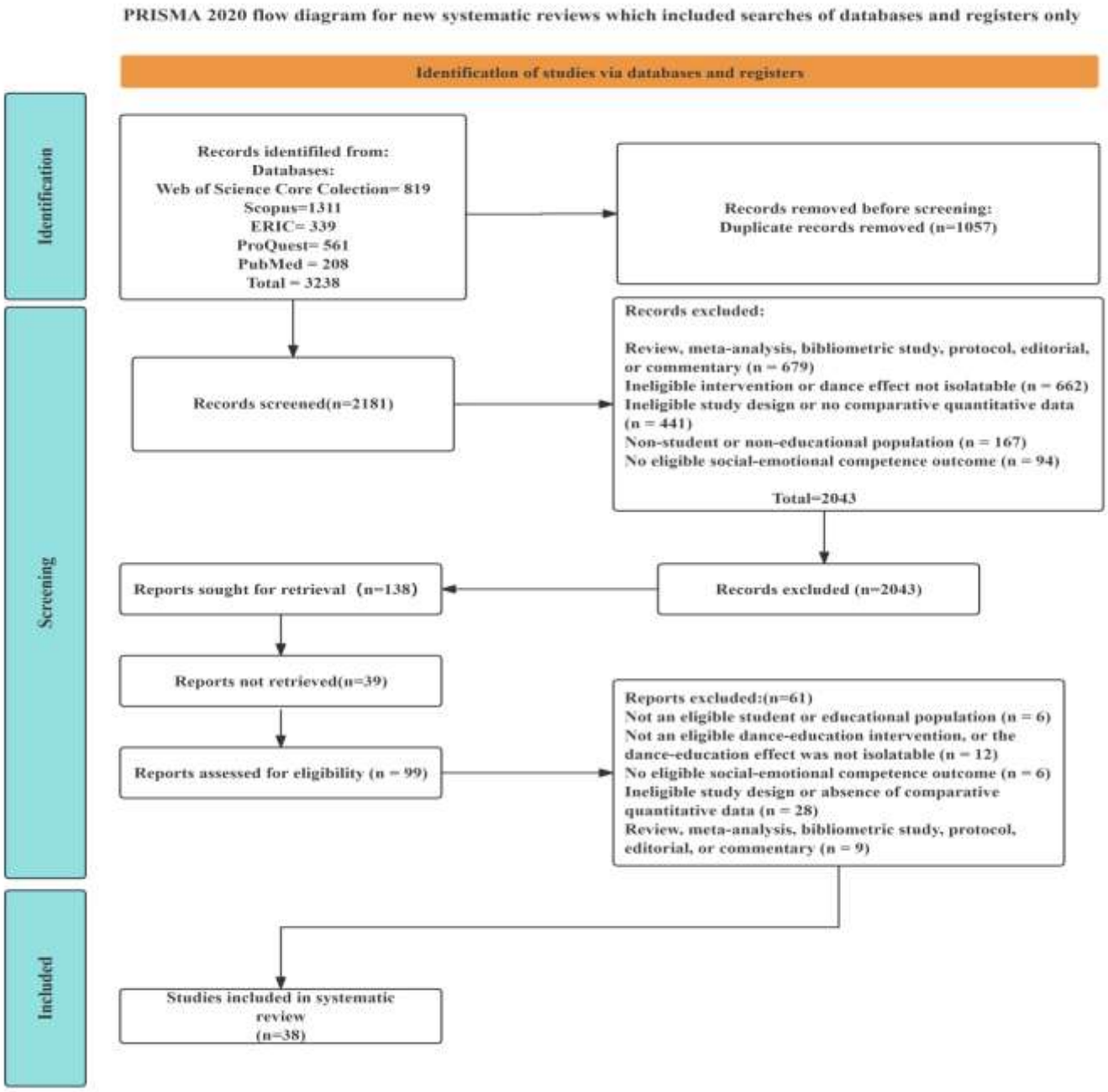
Additionally, for inclusion in the primary meta-analysis, studies had to compare a dance intervention with a non-dance comparator, isolate the contribution of dance, and provide sufficient statistical data to calculate or reconstruct an effect size. Studies not meeting these additional requirements were excluded from the meta-analysis but retained in the systematic review when otherwise eligible.

Information Sources, Search Strategies, and Study Selection

The search strategy comprised three concept blocks covering dance, social–emotional outcomes, and learners or educational settings, and was adapted to the indexing fields and syntax of each database. To address conceptual overlap and inconsistent terminology in the field, the outcome block included both umbrella terms, such as “social–emotional competence” and “social–emotional learning,” and related constructs, including “emotion regulation,” “empathy,” “communication,” “peer relationships,” “prosocial behaviour,” “cooperation,” and “teamwork,” which represent commonly recognised emotional and interpersonal domains of social–emotional competence (CASEL 2020). Broader dance- and education-related terms were used to maximise retrieval; however, eligibility remained governed by the definitions of structured, nonclinical dance education and social–emotional competence documented before full-text screening. Searches were conducted on 18 July 2026 in Scopus, ERIC, the Web of Science Core Collection, ProQuest, and PubMed. No date or language restrictions were applied. The ProQuest search was limited to scholarly journals and dissertations. Complete database search strategies are provided in the Supplementary Materials.

All retrieved records were imported into Rayyan and deduplicated before screening (Ouzzani et al. 2016). After removing 1,057 duplicates, two reviewers independently screened 2,181 titles and abstracts and excluded 2,043. Full texts were sought for 138 reports; 39 could not be retrieved despite online and library searches and, where possible, author requests. The remaining 99 reports were independently assessed, with disagreements resolved through discussion using documented decision rules. Sixty-one reports were excluded at full text because of ineligible populations, interventions, outcomes, study designs, or publication types, leaving 38 reports representing 35 independent study families (Figure 1).

Figure 1 PRISMA 2020 Flow Diagram for Study Selection



Note. The 38 included reports represented 35 independent study families.

Data Extraction and Risk-of-Bias Assessment

Study characteristics and quantitative data were extracted using a documented codebook finalised before formal synthesis and checked against the source reports. Risk of bias was assessed using RoB 2, including the cluster-randomised version where applicable, and ROBINS-I for non-randomised studies (Sterne et al. 2016; Sterne et al. 2019). Result-level judgements were retained, and the family-level rating reflected the most serious judgement among the components contributing to each family effect. The tools were not converted into numerical quality scores. Ambiguous cases were reconsidered against the source reports and decision rules.

Narrative Synthesis

Families not entering a pooled model were synthesised using a structured framework. We grouped families by educational stage, dance form, comparator, social–emotional domain, measurement source, and reason for non-pooling. For direct RQ2 comparisons, findings were classified as generally favourable, mixed, null, or unclear from the reported comparative pattern, without counting statistical significance as evidence strength. Interpretation prioritised design, measurement quality, and consistency across eligible outcomes. RQ1-only active-dance and inseparable multicomponent contrasts were described separately from direct non-dance comparisons.

Effect-Size Calculation and Dependence

Continuous outcomes were expressed as Hedges' g, oriented so that positive values favoured dance education; lower-is-better scales were multiplied by −1. Standard deviations were obtained from reported SDs or reconstructed from SEs, confidence intervals, test statistics, or individual data. No missing SD was imputed in the strict k = 9 analysis. For controlled pre–post designs, change-score effects used the pooled pretest SD and a within-person pre–post correlation documented before formal synthesis (Borenstein et al. 2009; Hedges 1981; Morris 2008).

For clustered designs, author-adjusted estimates were preferred; otherwise, participant-level variances were inflated by $1 + (m - 1)ICC$. A study with one cluster in either arm was not meta-analysed. Vasilopoulos and Dumontheil (2025) and Peng et al. (2026) each contributed two observed classes per arm and were included only in an exploratory low-cluster sensitivity analysis. The observed ICC was used for Peng et al.; ICC = .10 was used for Vasilopoulos and Dumontheil. For Tunçgenç, Greig, and Cohen (2024), estimates from 10 imputed datasets were combined using Rubin's rules.

Each independent family contributed one effect to the primary synthesis. A validated global social–emotional score was prioritised and never combined with nested subscales. Otherwise, effects within a domain and time point were combined by generalised least squares under $\rho = .60$, varied to .30 and .90. Domain estimates were then averaged with equal domain weights so that a family's conceptual contribution did not depend on how many measures it reported; the composite variance retained cross-domain covariance. Fifteen component estimates contributed to nine family effects.

Two aggregation sensitivities were conducted retrospectively: covariance-aware precision weighting across domains, and one result-blind rule-selected outcome or scale per family. The latter prioritised a validated global score, then an author-foregrounded construct, and then a blinded aggregated teacher rating when multiple informants measured the same construct. Multilevel and robust-variance models were not used as primary estimators because nine independent families provided too little information for additional variance components.

Statistical Analysis

The strict RQ2 synthesis comprised nine independent families. A random-effects model used restricted maximum likelihood and modified Hartung–Knapp inference with a t distribution and $q = \max[1, Q_e/(k - 1)]$ (Knapp and Hartung 2003; Partlett and Riley 2017; Röver, Knapp, and Friede 2015). We report 95% confidence intervals, Q, τ^2, τ, I^2, and a 95% prediction interval (Higgins and Thompson 2002; IntHout et al. 2016). No family was excluded solely because of an influence diagnostic.

Sensitivity analyses examined alternative family-effect constructions, risk-of-bias exclusions, omission of the multiply imputed study, design-specific subsets, a study-specific sensitivity varying Fakiola et al.'s assumed inter-domain correlation ($\rho = .30$, .60, and .90), and leave-one-family-out estimates. The two trials with exactly two clusters per arm were analysed only in an exploratory low-cluster model and were not used to support a robustness claim; results are provided in the Supplementary Materials.

Comparator conditions were coded as passive (no intervention or waitlist), usual education (standard teaching, physical education, or routine curricular provision), or active structured non-dance controls. RQ3 stratified

estimates descriptively by design, dance type, comparator, programme duration, and educational stage. Confirmatory subgroup tests and meta-regression were not conducted because only nine families were available and categories were sparse and correlated (Hedges and Pigott 2004). A contour-enhanced funnel plot was descriptive; formal asymmetry tests and trim-and-fill were not applied at k = 9 (Sterne et al. 2011). Certainty for overall social–emotional competence was appraised with GRADE. Final pooled models and figures were produced in R 4.6.1.

Results

Study Selection

The database searches identified 3,238 records. After 1,057 duplicates were removed, 2,181 records were screened and 2,043 were excluded. Full texts were sought for 138 reports; 39 could not be retrieved, leaving 99 reports for eligibility assessment. Sixty-one reports were excluded, and 38 reports representing 35 independent study families were included in the systematic review (Figure 1).

Of the 35 families, 21 directly addressed RQ2: nine entered the primary meta-analysis, two were included only in an exploratory low-cluster sensitivity analysis, and 10 were synthesised narratively. Fourteen additional families informed RQ1 through active-dance comparisons, inseparable multicomponent interventions, or other contrasts that did not isolate the effect of dance.

Study Characteristics and Evidence Tiers

For RQ1, the 35 families involved preschool (k = 12), primary (k = 6), secondary (k = 6), higher-education (k = 7), and mixed-stage (k = 4) learners. Twenty-one families evaluated creative or educational dance, 10 traditional or social dance, two street or hip-hop dance, and two technology- or delivery-focused forms. Comparators were usual education or no added dance (k = 15), another dance mode or teaching approach (k = 10), passive controls (k = 7), and active structured non-dance activities (k = 3; Supplementary Table S5a).

The primary outcome profile comprised comprehensive social–emotional competence (k = 5), self-regulation or dysregulation (k = 4), empathy or social cognition (k = 1), social or communication skills (k = 8), belonging or peer relationships (k = 2), prosocial behaviour, cooperation, or social problem solving (k = 5), and multiple eligible domains (k = 10). Principal measurement sources were student self-report (k = 13), teacher report (k = 6), parent report (k = 3), child tasks or independent observation (k = 6), peer nomination (k = 1), mixed reporters (k = 5), and one unreported source. Fourteen families explicitly named a theoretical or conceptual framework (Supplementary Table S5b); reporting was absent or unclear for the other 21. Measures ranged from global social–emotional scales to construct-specific ratings, performance tasks, and peer-network nominations. Detailed report- and family-level information is provided in the Supplementary Materials.

The strict primary synthesis comprised nine families and 1,067 selected participants from five countries. Three families were randomised and six non-randomised; two involved preschool, six school-age or secondary, and one university participants. Four evaluated traditional or social dance and five creative, educational, or urban dance (Table 1).

Table 1 Characteristics of Families Included in the Strict Primary Meta-Analysis (k = 9)

Study	Population	Design	Dance intervention/dose	Comparator	SEC outcome/ reporter	N	Overall RoB
Lobo and Winsler (2006) (USA)	Preschool; 39–62 months	Individual RCT	Creative dance/movement; 8 weeks; 16 sessions	Attention-control play/activity	SCBE social competence (Parent + teacher)	40	Some concerns
Demirağ (2010) (Türkiye)	High school; 15–18 years	Non-randomised matched	Argentine tango; 24×90 min; ~10.5 weeks	No tango	Communication Skills Inventory (Self-report)	32	Critical

Kotaman, İnceoğlu, and Kotaman (2024) (Türkiye)	Preschool	Non-randomised cluster; 3 vs 5 classes	Dance activities; 8 weeks; 2×20–25 min/week	Usual classroom	Prosocial behaviour (Teacher)	107	Critical
Soykan (2019) (Türkiye)	University; 18–24 years	Non-randomised	Folk dance; 12 weeks; 3 days/week; 3 h/day	No intervention	Social Skills Inventory (Self-report)	60	Serious
White (2014) (USA)	Grades 9–12	Non-randomised; 9 classes	Dance-integrated academic instruction (JP-CIM); 12 school days	Subject-matched usual academic instruction	BRIEF Behavioral Regulation Index (self-report)	62	Serious
Kreutzmann et al. (2018) family (Germany)	Grades 5–9; mean age 11.4	Non-randomised; 30 classes	14/28 weekly 90-min sessions; primary report: two semesters	Regular/alternative activities	Belonging and peer networks (Peer/self)	606	Serious
Fakiola et al. (2025) (Greece)	Ages 10–13 years	Non-randomised/unclear	Greek traditional dance; 12 weeks; 2×60 min/week	No systematic activity	SSBS: cooperation, empathy, dysregulation (Self-report)	94	Serious
Tunçgenç, Greig, and Cohen (2024) (United Kingdom)	Ages 11–16 years	Individual RCT	Online hip-hop; 5 weeks; 1×60 min/week	Waitlist	Peer social bonding (Self-report)	58	High
Marouli et al. (2021) (Greece)	ASD; ages 6–14 years	Individual RCT	Greek traditional dance; 8 weeks; 2×40 min/week	Usual PE	Teacher-rated social skills (Teacher)	8	High

Note. ASD = autism spectrum disorder; BRIEF = Behavior Rating Inventory of Executive Function; JP-CIM = Jacob's Pillow Curriculum in Motion; N = selected participants; PE = physical education; RCT = randomised controlled trial; RoB = risk of bias; SCBE = Social Competence and Behavior Evaluation; SEC = social–emotional competence; SSBS = School Social Behavior Scales. The Kreutzmann et al. (2018) family includes Zander et al. (2014).

Narrative Synthesis of Non-Pooled Evidence

Ten direct-comparison families were not pooled because of unit-of-analysis constraints, unavailable variances, or unresolved comparative data. Six were coded as generally favourable, three as mixed/favourable, and one as unclear because no separable contrast could be reconstructed (Supplementary Table S6). Outcomes covered social–emotional adjustment, prosocial behaviour, social cognition, theory of mind, communication, social skills, and peer relationships.

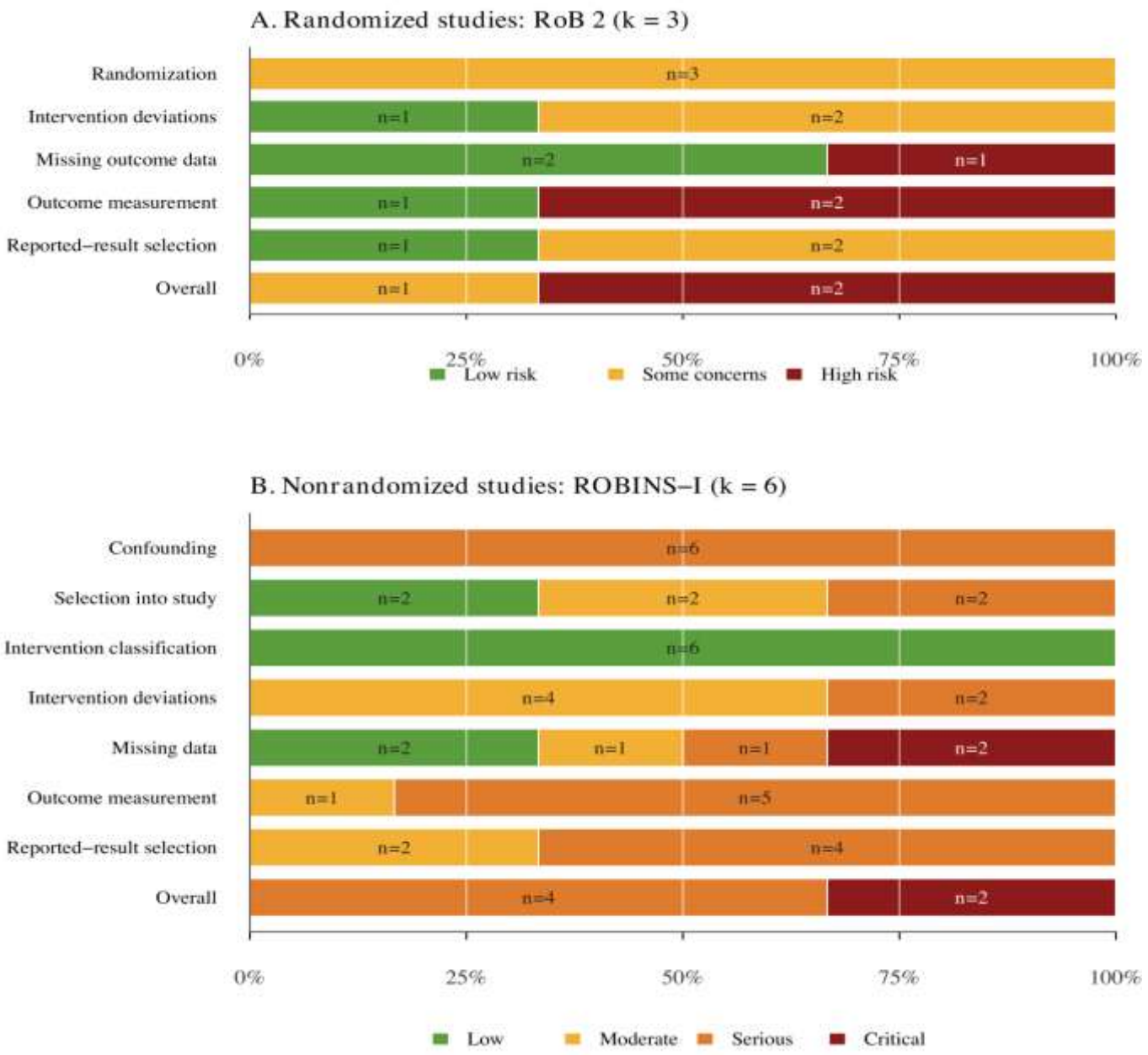
These findings were not interpreted as an alternative estimate of the average effect. Their causal value was limited by small intact-class samples, incomplete variance reporting, subjective measurement, and non-random allocation. The 14 RQ1-only families were described separately because active-dance comparisons and inseparable multicomponent interventions addressed different estimands from direct dance-versus-non-dance comparisons.

Risk of Bias

Risk-of-bias findings were reported separately by study design (Figure 2). Among the three randomised families, one raised some concerns and two were at high risk of bias, primarily because of missing outcome data, unblinded subjective measurement, and selective reporting. Among the six non-randomised families, four were

at serious and two at critical risk, mainly because of confounding or self-selection, attrition, intervention-aware measurement, and incomplete reporting. None of the nine families provided low-risk evidence.

Figure 2 Risk-of-Bias Summary for the Strict Primary Meta-Analysis



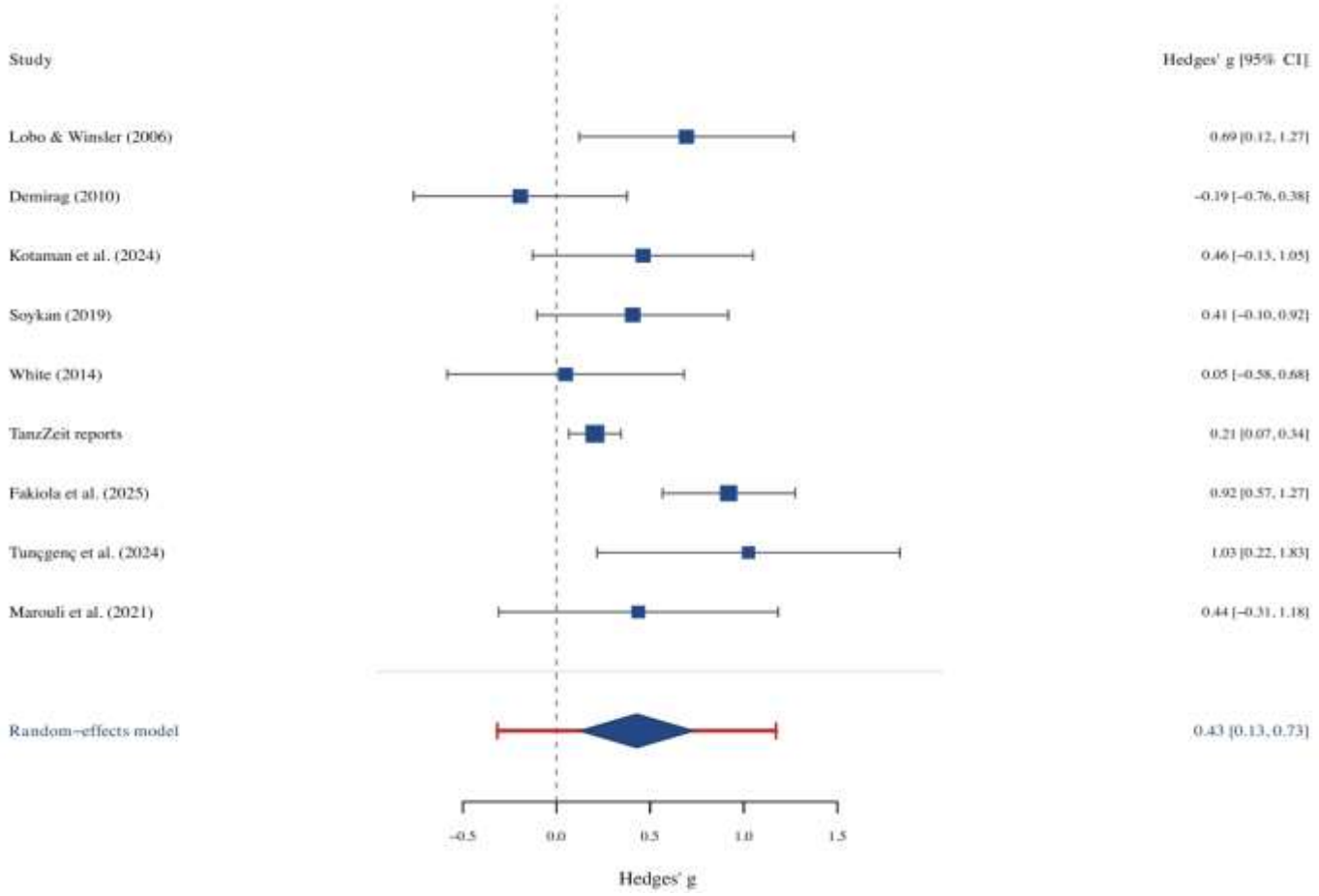
Note. Bars show the percentage of families receiving each domain-level judgment; labels give counts. RoB 2 and ROBINS-I are displayed separately because their domains and judgment categories are not interchangeable.

Primary Meta-Analysis and Heterogeneity (RQ2)

The primary equal-domain model included one effect from each of nine independent families (N = 1,067) and yielded a positive pooled effect in favour of dance education, g = 0.43, SE = 0.13, 95% CI [0.13, 0.73], t(8) = 3.27, p = .011 (Figure 3; Table 2).

Effects showed moderate-to-substantial heterogeneity, Q(8) = 22.42, p = .004, $\tau^2 = 0.082$, $\tau = 0.286$, and $I^2 = 64.3\%$. The 95% prediction interval ranged from −0.32 to 1.17, indicating that effects in comparable settings may range from negative to substantially positive despite the positive mean estimate.

Figure 3 Forest Plot for the Strict Primary Random-Effects Model (k = 9)



Note. Squares are family-level Hedges' g values, sized by random-effects weight; horizontal lines are 95% confidence intervals. The diamond is the pooled estimate and the red line is the 95% prediction interval. Positive values favour dance education.

Table 2 Primary and Sensitivity Meta-Analysis Models

ID	Analytic set	k	Hedges' g [95% CI]	τ^2	I^2 (%)	95% prediction interval
M1	Equal-domain primary model	9	0.43 [0.13, 0.73]	0.082	64.3	[−0.32, 1.17]
M2	Covariance-aware precision weighting	9	0.40 [0.13, 0.67]	0.055	54.8	[−0.22, 1.02]
M3	One rule-selected outcome per family	9	0.38 [0.10, 0.67]	0.065	54.2	[−0.29, 1.06]
M4	Exclude High RoB randomised studies	7	0.38 [0.02, 0.74]	0.090	68.9	[−0.48, 1.24]
M5	Exclude Critical ROBINS-I studies	7	0.50 [0.15, 0.85]	0.076	68.5	[−0.30, 1.30]
M6	Exclude both High RCT and Critical NRSI	5	0.46 [−0.001, 0.92]	0.088	75.1	[−0.62, 1.54]
M7	RCTs only (descriptive)	3	0.70 [−0.17, 1.57]	0.000	0.0	[−1.87, 3.27]
M8	NRSIs only (descriptive)	6	0.34 [−0.08, 0.75]	0.100	71.2	[−0.65, 1.32]

Note. Positive values favour dance education. Models used REML random effects and Hartung–Knapp confidence intervals. M1 is primary. M2 and M3 were specified retrospectively. RCT = randomised controlled trial; NRSI = non-randomised study of intervention.

Descriptive Variation Across Study Characteristics (RQ3)

Because only nine families were available and subgroup categories were sparse and correlated, analyses for RQ3 were descriptive. Estimates varied across comparator conditions, from a pooled g = 0.22 for usual education to g = 0.69 for the single active structured-control family; however, confidence intervals were generally wide, and no between-category tests were conducted (Table 3). Estimates stratified by design, dance type, programme duration, and educational stage were similarly imprecise and should not be interpreted as evidence of moderation.

Table 3 Descriptive Stratified Estimates for RQ3

Study characteristic	Category	k	Hedges' g [95% CI]	τ^2	I^2 (%)
Design	RCT	3	0.70 [−0.17, 1.57]	0.000	0.0
Design	NRSI	6	0.34 [−0.08, 0.75]	0.100	71.2
Dance type	Traditional/social dance	4	0.42 [−0.36, 1.21]	0.166	73.0
Dance type	Creative/educational/urban	5	0.38 [−0.03, 0.79]	0.042	43.8
Comparator	Passive control	4	0.53 [−0.34, 1.40]	0.218	75.4
Comparator	Usual education	4	0.22 [0.01, 0.43]	0.000	0.0
Comparator	Active structured control	1	0.69 [0.12, 1.27]	—	—
Duration	≤8 weeks	5	0.50 [0.09, 0.92]	0.000	1.6
Duration	>8 weeks	4	0.36 [−0.36, 1.08]	0.157	81.8
Education stage	Preschool	2	0.58 [−2.08, 3.24]	0.000	0.0
Education stage	School-age/secondary	6	0.39 [−0.11, 0.89]	0.149	75.2
Education stage	University	1	0.41 [−0.10, 0.92]	—	—

Note. Rows with k ≥ 2 are random-effects syntheses; k = 1 rows are single-family estimates, not pooled results. Positive values favour dance education. No between-category tests were performed; categories overlap. RCT = randomised controlled trial; NRSI = non-randomised study of intervention.

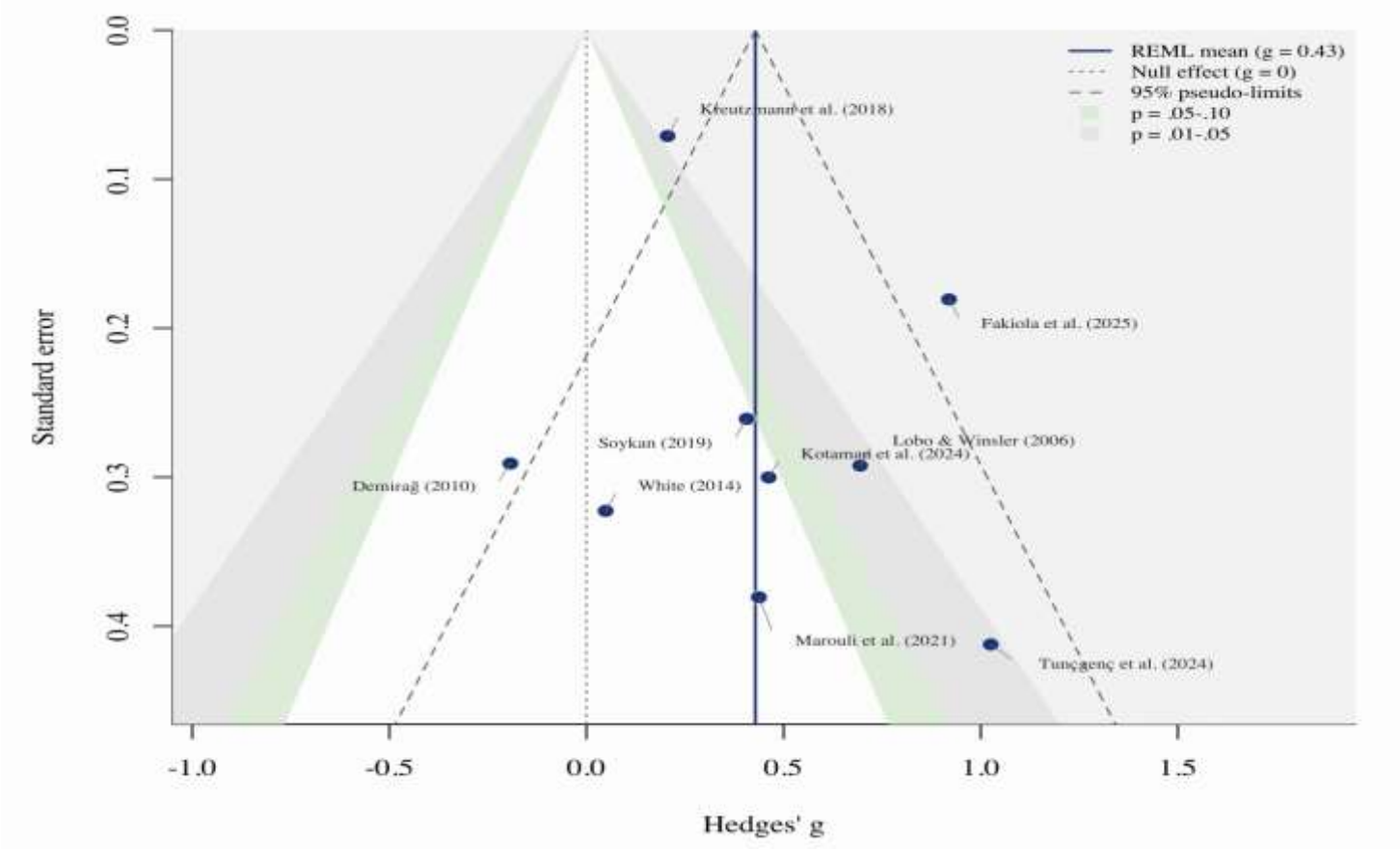
Sensitivity, Influence, and Small-Study Effects

The mean was similar when domains were combined using covariance-aware precision weights, g = 0.40, 95% CI [0.13, 0.67], and when one result-blind rule-selected outcome or scale was retained per family, g = 0.38, 95% CI [0.10, 0.67] (Table 2). Prediction intervals for both models included zero and adverse effects. Equal domain weighting therefore did not determine the positive average.

Point estimates remained positive after excluding high-risk randomised families (g = 0.38, 95% CI [0.02, 0.74]), critical-risk non-randomised families (g = 0.50, 95% CI [0.15, 0.85]), or both (g = 0.46, 95% CI [−0.001, 0.92]); the last interval included zero. Leave-one-family-out means ranged from 0.30 to 0.49. Removing Fakiola et al. produced the largest attenuation, g = 0.30, 95% CI [0.05, 0.55]. The exploratory model adding the two trials with exactly two clusters per arm yielded g = 0.46, 95% CI [0.19, 0.72] and was treated as hypothesis-generating.

The contour-enhanced funnel plot was dispersed around the pooled estimate (Figure 4). Because $k = 9$ and heterogeneity was moderate, formal asymmetry tests were not conducted. The plot is therefore a visual diagnostic; small-study effects and selective reporting cannot be excluded.

Figure 4 Contour-Enhanced Funnel Plot for the Strict Primary Model (k = 9)



Note. The solid blue line marks the REML pooled estimate (g = 0.43), and the vertical dotted grey line marks the null effect (g = 0), around which the two-sided significance contours are defined. Dashed lines show 95% pseudo-limits centered on the pooled estimate. Labels identify study families by the primary report's first author and publication year. The Kreutzmann et al. (2018) label denotes the Berlin TanzZeit 2009–2010 family, which also includes the linked companion report by Zander et al. (2014). The plot is descriptive because only nine effects were available.

Certainty of Evidence

Certainty for overall social–emotional competence was very low. Risk of bias was rated very serious because no family provided low-risk evidence; inconsistency was serious because I^2 was 64.3% and the prediction interval crossed zero; and imprecision was serious because only nine families were available and the most restrictive risk-of-bias sensitivity interval included zero. Indirectness was not downgraded, and publication bias could not be assessed reliably at k = 9 (Supplementary Table S12).

DISCUSSION

Interpreting the Average Effect in Context

The average effect estimate favoured structured, nonclinical dance education, and convergence across alternative aggregation rules makes it unlikely that this direction was created by one outcome-selection decision. That analytic stability does not change the very-low GRADE rating: no contributing family was at low risk of bias, the most restrictive sensitivity analysis was compatible with no effect, and the prediction interval allowed favourable or unfavourable effects in a new setting. The non-pooled comparisons were generally favourable, mixed/favourable, or unresolved. As IntHout et al. (2016) emphasised, a pooled mean and the range of plausible setting-specific effects answer different questions. The result is therefore a positive but fragile signal, not a secure causal or universal effect.

The direction is consistent with meta-analyses of universal school-based SEL (Durlak et al. 2011; Sklad et al. 2012; Taylor et al. 2017) and with broader dance reviews reporting generally favourable psychological outcomes (Burkhardt and Brennan 2012; Koch et al. 2019; Tao et al. 2022). The agreement is limited, however, because their populations, purposes, and outcome scopes differ from the present educational estimand. More pointedly, Fong Yan et al. (2024) found dance generally comparable, and only occasionally superior, to other structured physical activities, with some outcomes favouring the comparator. This is not a simple contradiction: passive and usual-practice controls estimate the value of adding a dance programme, whereas active controls better isolate value beyond physical activity, attention, and group contact. With only one active structured non-dance comparison in the strict synthesis, a dance-specific advantage remains untested.

From Dance Affordances to Transferable Competence

The review did not test mediators, so mechanism claims must remain hypotheses. Grounded-cognition accounts locate emotion and social understanding partly in perceptual–motor states (Barsalou 2008; Niedenthal 2007), and dance repeatedly couples enacted affect and intention with observation of others (Sevdalis and Keller 2011). Mirroring, improvisation, and expressive movement can therefore create opportunities for emotion recognition and perspective-taking, as Behrends, Müller, and Dziobek (2012) and Hanna (2008) propose. These accounts establish plausibility, not transfer: interpreting emotion during a dance task is not evidence that a learner can regulate emotion or take another's perspective outside it.

Interpersonal coordination offers a second pathway, but the evidence challenges a simple "synchrony causes competence" account. Mogan, Fischer, and Bulbulia's (2017) meta-analysis linked synchrony with prosocial behaviour, bonding, and social cognition, yet effects differed by outcome and comparison condition. Reddish, Fischer, and Bulbulia (2013) showed that shared intentionality strengthened cooperation, Tarr et al. (2015) separated the contributions of synchrony and exertion, and Kokal et al. (2011) found prosocial effects when rhythmic coordination was readily mastered. Dance-related change may therefore arise from timing, common goals, exertion, mastery, or their interaction. Any resulting affiliation may also remain immediate and group-bound; synchrony is a candidate component, not an established active ingredient.

Pedagogy is the likely conversion point between such experiences and transferable competence. In general SEL research, active practice is more effective when learning is sequenced, focused, explicit, and well implemented (Durlak and DuPre 2008; Durlak et al. 2011). Hanna (2008) similarly tied transfer from dance to depth and reflection, while Behrends, Müller, and Dziobek (2012) acknowledged that movement-based empathy had not yet been shown to generalise. Fong Yan et al. (2024) further noted that dance studies often failed to report whether classes were intentionally adapted to psychological aims. These literatures therefore suggest a conditional hypothesis: feedback, reflection, progression, and practice across partners and contexts may help convert embodied and relational experiences into generalisable skill.

Sources of Variation and Boundary Conditions

Some heterogeneity is built into the comparison. A waitlist asks what happens when dance is added; usual education compares dance with routine provision; an active structured control asks what dance adds beyond another organised experience. Durlak and DuPre (2008) warned that school comparison groups often receive alternative services, while Fong Yan et al. (2024) showed how active controls change the interpretation of dance effects. The descriptive differences observed here cannot establish comparator moderation, especially because only one strict family used an active structured control. They do show that comparator content is part of the estimand and must be reported, not treated as a nuisance label.

Descriptive stratification provided no credible basis for attributing variation to broad genre or programme length. Schwender et al. (2018) found that dance studies frequently omitted teaching content and method, and Fong Yan et al. (2024) documented substantial variation in both dance and comparator dose. Implementation research distinguishes fidelity, dosage, delivery quality, participant responsiveness, programme differentiation, and adaptation (Durlak and DuPre 2008); none is captured adequately by weeks or genre. Two programmes carrying the same "creative dance" label may provide very different opportunities if one centres reciprocal composition

and reflection while another centres technical reproduction. Pedagogical function and the experience actually received are therefore more credible candidates for future moderator tests.

The evidence map identifies a further source of variation: construct and method. Fewer than half of the study families named a theoretical framework, and SEC was represented by multiple domains and reporters. Koch et al. (2019) found substantially larger effects for observational ratings than for less reactive measures, illustrating how measurement can contribute to apparent intervention differences; Schwender et al. (2018) likewise reported inconsistent constructs and instruments. Context is no simpler. Wigelsworth et al. (2016) found that trial stage and international transfer did not influence all SEL outcomes in the same direction, arguing for attention to local fit and mutual adaptation rather than simple country effects. Because the present subgroups were sparse and correlated, developmental stage, culture, reporter, and dance type remain hypotheses—not demonstrated moderators.

Educational and Research Implications

These findings do not warrant dance as a stand-alone SEL solution, but they suggest a testable design principle: begin with the intended competence, then align the movement task, social organisation, feedback, and reflection with that target. Mirroring and perspective exchange may be relevant to empathy; interdependent composition and rotating leadership may be more relevant to cooperation. Such examples extend the active, focused, and explicit logic supported in SEL research (Durlak et al. 2011), but they are not validated prescriptions. Challenge should remain manageable, because Kokal et al. (2011) observed prosocial commitment when rhythmic coordination was readily mastered, and implementation should track responsiveness and adaptation rather than attendance alone (Durlak and DuPre 2008).

Future studies should first strengthen identification. Prospective, adequately replicated cluster-randomised or well-controlled designs should compare dance with conditions matched on physical exertion, music, group contact, instructor attention, and expectancy; Fong Yan et al. (2024) illustrate why an active comparator is essential for separating dance-specific from common activity effects. Allocation, clustering, attrition, assessor blinding, intervention content, fidelity, adaptation, and adverse experiences should be reported, with descriptions sufficient for replication (Schwender et al. 2018). Mechanism studies should then manipulate or measure synchrony, shared goals, creative agency, exertion, and reflection, distinguishing immediate experience, competence acquisition, and later transfer. Lubans et al. (2016) found that physical-activity mechanisms were rarely tested formally, and the longer-term SEL evidence synthesised by Taylor et al. (2017) cannot substitute for dance-specific follow-up. Prospectively specified domain measures, independent observations, behavioural tasks, and multiple informants would make both mediation and durability more credible.

Strengths and Limitations

Several design features strengthen the review. The 38 included reports were linked to 35 independent families before synthesis, keeping companion reports and correlated outcomes from masquerading as independent evidence. All families remained visible for RQ1, whereas the primary model was restricted to nine direct-comparison families meeting the final quantitative criteria. Alternative family-effect constructions, risk-of-bias exclusions, the study-specific correlation check for Fakiola et al., and leave-one-family-out analyses separated sensitivity to analytic decisions from uncertainty in the studies themselves. Modified Hartung–Knapp inference and a prediction interval were also appropriate safeguards for a small, heterogeneous evidence base (Partlett and Riley 2017; Röver, Knapp, and Friede 2015).

The main limitations remain evidential rather than computational. The strict synthesis was small, relied heavily on non-randomised studies, and contained no low-risk family; subjective or intervention-aware measurement may have inflated or obscured effects, a concern also evident in prior dance research (Koch et al. 2019). With few effects and correlated study characteristics, moderator tests would have had low power (Hedges and Pigott 2004), while funnel-plot asymmetry could not be interpreted reliably (Sterne et al. 2011). Unretrieved full texts may have introduced availability bias, some effect calculations required correlation or ICC assumptions, and retrospective protocol finalisation leaves room for analytic flexibility despite documented amendments. Finally, sparse follow-up and absent mediator tests prevent conclusions about durability or the proposed embodied,

relational, and pedagogical pathways. Sensitivity analyses reduce uncertainty about modelling choices; they cannot correct design bias or supply missing evidence.

CONCLUSION

This systematic review and meta-analysis mapped how social–emotional competence has been conceptualised and measured in research on structured, nonclinical dance education and evaluated its effects among independent study families. Across 38 reports representing 35 families, social–emotional competence was assessed using diverse domains and measurement sources, while the non-pooled comparisons were generally favourable, mixed/favourable, or unclear. Among the nine families included in the primary meta-analysis, the random-effects model indicated a positive average effect in favour of dance education ($g = 0.43$). However, effects varied substantially across settings, the prediction interval included null and adverse effects, and descriptive differences by design, comparator, dance type, duration, and educational stage were insufficient to establish moderation. Similar estimates across alternative outcome-aggregation procedures reduced concern that the positive mean depended on a single aggregation rule, but they could not overcome the absence of low-risk evidence; consequently, GRADE certainty remained very low. Taken together, dance education should be viewed as a promising but unconfirmed and context-dependent pedagogical resource rather than an established stand-alone intervention. Progress requires prospectively registered, adequately replicated controlled studies with credible active comparators, validated multi-informant outcomes, implementation and mechanism measures, and follow-up assessments.

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Competing Interests

The authors declare that they have no competing interests.

Ethics Approval

Not applicable. This systematic review and meta-analysis synthesised data from previously conducted studies and did not involve the collection of new data from human participants or animals; institutional ethics approval was therefore not required.

Consent to Participate

Not applicable.

Consent for Publication

Not applicable.

Welfare of Animals

Not applicable.

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