

Effect of the Grasp and Grow Program on Fine Motor Development Among Preschool Children in Pathum Thani Province, Thailand

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ABSTRACT

Objective: This study aimed to evaluate the effect of the Grasp and Grow program on fine motor development among preschool children (Kindergarten 2) in Pathum Thani Province, Thailand, compared with routine art instruction.

Materials and Methods: A two-group pretest-posttest design was implemented, with one school allocated to each condition (single-cluster-per-arm) at two government primary schools in Pathum Thani, Thailand, between July and September 2025. Ninety-two Kindergarten 2 children (age 4-5 years) were allocated by school-level lottery to an experimental group (n=46), receiving an 8-week, classroom-delivered program of seven structured art-based activities grounded in Gesell's Maturation Theory, or a comparison group (n=46) receiving routine art instruction. Fine motor development was assessed using a 20-item structured observation instrument covering four domains; single-covariate ANCOVA was conducted post hoc as the primary between-group analysis, selected after baseline imbalance became apparent.

Results: Baseline fine motor scores were lower in the experimental group (28.63 vs. 33.63, $p < 0.001$). At posttest, the experimental group scored higher (48.43 vs. 37.00, Cohen $d = 2.14$, $p < 0.001$). The ANCOVA-adjusted between-group difference was 12.98 points (95% CI: 10.74-15.22), $F(1, 89) = 132.66$, Cohen $d = 2.59$, $p < 0.001$. Three analyses (the primary analysis and two additional sensitivity analyses) showed a consistent direction (Cohen's d range: 2.49-2.59), though this consistency does not establish a causal effect given shared design limitations.

Conclusion: In this exploratory, single-cluster-per-arm study, children receiving the Grasp and Grow program showed significantly greater fine motor improvement than those receiving routine art instruction. Given the unreplicated school-level allocation, this finding should be treated as hypothesis-generating rather than evidence of a causal program effect. Given its low cost, the program may offer schools in resource-limited settings a feasible option pending confirmation through a teacher-led, cluster-randomized trial.

Keywords: Motor skills; child; preschool; child development; hand strength (Siriraj Med J 2026;78(10):748-758)

INTRODUCTION

Fine motor delay is more than a classroom problem. Children who struggle with basic hand tasks such as dressing, using utensils, or holding a pencil often fall behind their peers in ways that affect confidence and emotional well-being. Lower self-esteem and anxiety have both been documented in children with poor fine motor proficiency.¹ Left unaddressed, these difficulties can persist into school-age and affect daily participation in ways that matter to health.

At kindergarten entry, fine motor skills predict academic achievement alongside executive function.² A review of 29 studies identified preschool-based programs, along with age, gender, and physical activity, as positive determinants of motor skill development in children aged 3 to 6.³ Schools may therefore serve as a practical setting for fine motor intervention, rather than only a place where delay is first noticed.

In Thailand, fine motor and gross motor skills are recognized as part of the physical movement and health competency that early childhood should acquire.⁴ According to the Director of the National Institute for Child and Family Development, approximately 47% of Thai preschoolers show suspected delay in the combined fine motor and intelligence domain under national developmental surveillance.⁵ However, province-specific

data for Pathum Thani were not identified, representing a gap that this study begins to address.

Despite a growing evidence base, several gaps remain. A scoping review mapping 45 motor skill intervention studies for preschool-aged children found that only two were conducted in low- and middle-income countries, while 24 were conducted in the United States alone.⁶ The same scoping review found that occupational therapists and physiotherapists delivered the majority of interventions, with classroom teachers serving as the primary facilitator in only seven of the 45 studies mapped.⁶ Iivonen and Sääkslahti (2014) identified parent- and family-related factors as one of four categories of potential determinants for fine motor development, although evidence for this category was limited to a small number of studies³, yet structured home-based components remain uncommon in published intervention programs. In Thailand, creative art activities have been shown to improve fine motor development among Kindergarten children⁷, but comparative evaluation of structured programs against such routine activities remains limited in this setting. A teacher-deliverable, low-cost program with evidence behind it could give schools a practical option for supporting fine motor development in typically developing children without relying on specialist services that are often unavailable.

This study aimed to evaluate the effect of the Grasp

and Grow program on fine motor development among Kindergarten 2 children in Pathum Thani Province, Thailand. The program is a structured, classroom-delivered intervention grounded in Gesell's Maturation Theory, using low-cost everyday materials and sequencing seven activities by motor demand across four fine motor domains. To isolate the program's specific contribution, fine motor outcomes were compared against active art instruction rather than a non-active condition.

MATERIALS AND METHODS

Ethics statement

This study was approved by the Valaya Alongkorn Rajabhat University under the Royal Patronage Institutional Review Board (COA No. 0055/2568; REC No. 0043/2568; 21 July 2025) and conducted in accordance with the Declaration of Helsinki. Informed consent was obtained from all guardians prior to enrollment.

Study design

A two-group pretest-posttest design was employed, with allocation at the school level (one school per condition) to prevent within-school contamination. As this constitutes a single-cluster-per-arm design, the treatment condition is fully confounded with school; findings should be interpreted as hypothesis-generating rather than confirmatory of a causal program effect. Data were collected between 28 July and 25 September 2025 at two small government primary schools in Klongsong Subdistrict, Khlongluang District, Pathum Thani Province, Thailand. Formal school- or class-level characteristics (e.g., enrollment size, teacher-to-student ratio, resource level) were not systematically measured or compared; observed baseline differences in fine motor scores and family structure between groups (Table 2) suggest the two schools and their enrolled populations were not equivalent.

Sample size

Sample size was calculated using G*Power 3.1.⁸ An effect size of $d = 0.80$ was specified.⁹ Alpha was set at 0.05 and power = 0.95, yielding a minimum of 84 participants. A 10% allowance for attrition produced a final target of 92 participants (46 per group). This calculation was based on individual-level power analysis and did not account for the study's actual single-cluster-per-arm allocation; see Limitations

Participants

Children were eligible if enrolled in Kindergarten 2 (age 4–5 years), able to communicate in Thai, and

accompanied by a parent or guardian who provided written informed consent. Children were excluded if they had a diagnosed intellectual disability, previously identified developmental delay, attention-deficit/hyperactivity disorder, or any condition directly affecting fine motor function.

Sampling

A multi-stage sampling approach was used. Khlongluang District and Klongsong Subdistrict were purposively selected as a peri-urban area with multiple small government primary schools offering Kindergarten 2 classes. Two schools were then selected by simple random sampling (lottery) from seven eligible schools within the subdistrict (opaque-envelope procedure; allocation concealment maintained until draw). A second independent lottery assigned one school to the experimental condition and the other to the comparison condition. All Kindergarten 2 children enrolled at each allocated school who met the eligibility criteria were invited to participate, constituting a census of the eligible population at each site. At the comparison school, 48 children met eligibility criteria; two guardians declined participation, yielding 46 consenting children (identical to the experimental school's 46). The two non-consenting children remained present in the classroom throughout all activity sessions to avoid social exclusion but were directed to an unrelated activity during the Week 1 and Week 8 assessment sessions and contributed no outcome data. As only one school was allocated to each condition, the treatment condition is fully confounded with school; findings should be interpreted as hypothesis-generating rather than confirmatory of a causal program effect.

Intervention program

The program was developed through four stages: theoretical grounding, activity selection, expert validation, and pilot testing.

In Stage 1, Gesell's Maturation Theory was selected as the theoretical framework on the basis that it explicitly addresses the biologically sequenced progression of motor development in early childhood, predicting that fine motor control develops in a proximal-to-distal and simple-to-complex direction. This sequencing principle was used as a practical organizing framework for activity design rather than as a claim that the program accelerates innate maturation; consistent with contemporary motor-learning evidence that fine motor skills require active teaching and reinforcement rather than developing through maturation alone¹⁰, the program provided structured, progressively demanding practice. This progression

principle provided the organizing logic for sequencing program activities from lower to higher motor control demands across 8 weeks.

In Stage 2, seven activities were selected through a narrative review to target all four fine motor domains using low-cost classroom materials without specialist equipment. Activities were sequenced from foundational control tasks (tracing) through grip modulation and bilateral coordination tasks (paper collage, nature printing, sensory bag filling, bead threading) to whole-hand integration tasks (drawing, creative cutting). A consolidated review session in Week 8 reinforced all four domains, consistent with the distributed practice principle in motor skill acquisition.¹¹ Full activity-level details and theoretical linkage are provided in Table 1.

In Stage 3, the program manual was submitted for content validation to three experts (early childhood education, public health, and pediatric occupational therapy). Content Validity Index was 0.89 (≥ 0.80 threshold).

Expert suggestions were incorporated prior to finalization.

In Stage 4, the program was piloted with 15 Kindergarten 2 children at a non-study school. All activities were confirmed executable within 60 minutes using low-cost materials. Minor bead size adjustments were made to reduce choking risk. No changes to the activity sequence or framework were required.

Design rationale

Activity design followed two criteria. First, sequencing and difficulty progression were grounded in Gesell's Maturational Theory, ordering activities from lower to higher motor control demand across the 8-week period (Table 1). Second, all materials were selected for low cost and ready availability in the Thai context (leaves, beads, rope, recycled paper, and similar everyday items), so that the program could be feasibly adopted by government schools operating with limited budgets.

TABLE 1. Grasp and grow program.

Week	Activity	Materials	Targeted Domain(s)	Gesell's Principle
1	Tracing tasks	Tracing sheets, pencils, crayons	Control; Hand-eye coordination	Proximal-to-distal; simple-to-complex
2	Paper collage (precision tearing)	Newspapers, coloured paper, glue sticks	Control	Bilateral-to-unilateral hand differentiation
3	Nature printing (Eco Print)	Leaves, flowers, ink pads, paper	Control <i>Flexibility*</i>	Proximal-to-distal grip modulation
4	Sensory bag filling	Zip-lock bags, sand, rice, beans, fabric	Dexterity; Hand-eye coordination <i>Flexibility*</i>	Progressive finger-wrist refinement
5	Bead threading/ stringing	Beads, pasta, string or shoelace	Control; Dexterity; Hand-eye coordination	Pincer grasp maturation; distal control
6	Drawing and painting	Paper, crayons, watercolors, brushes	Control; Dexterity; Hand-eye coordination	Proximal stability with distal dexterity
7	Creative cutting	Child-safe scissors, paper, cardboard	Control; Dexterity; Hand-eye coordination	Complex bilateral coordination
8	Consolidated review	All materials from Weeks 1–7	All four domains	Spiraling repetition; consolidation

Note: Activities are sequenced from Week 1 (lowest motor control demand) to Week 7 (highest motor control demand) in accordance with Gesell's Maturational Theory principle of simple-to-complex developmental progression. Week 8 is a consolidated review session covering all four domains.

*Domain inferred from the physical demands of the activity (e.g., grip-pressure modulation in Eco Print, finger flexion/extension in bag tying) rather than stated as an explicit objective in the original protocol.

Procedure: grasp and grow program

Activities were ordered by motor demand across all four domains (Table 1). Sessions were 60 minutes, delivered weekly for 8 weeks by research team members, with the classroom teacher present throughout. All materials were low-cost (leaves, beads, rope, paper). Attendance and fidelity (adherence to the designed activity protocol) were independently recorded in parallel by two research team members and cross-checked for agreement. All 46 children attended every session (fidelity: 100.0%). The Week 8 posttest assessment was conducted before the consolidated review session, to avoid inflated performance from immediate rehearsal of aligned tasks.

Program dissemination

Upon completion of the Week 8 session, a pre-planned dissemination phase was implemented at both schools. Each school received a teacher manual, and each enrolled family received a home exercise workbook and parent guide to support independent replication and continued home practice. Materials were distributed to both schools to ensure equity of access. No outcome data were collected after the Week 8 posttest.

Comparison group

The comparison group received routine art instruction (drawing, block assembly, clay modeling, cutting, collage) without structured sequencing, following the school's existing curriculum plan. Sessions were delivered weekly for 60 minutes by the two classroom teachers routinely present in each classroom, without additional training; the research team had gathered information on the school's existing art curriculum prior to intervention design but did not deliver or supervise comparison-group sessions directly. Attendance was recorded by the classroom teacher each session. Materials were comparable in type but not systematically quantified or matched in quantity to the experimental group. Researchers assessed at Week 1 and Week 8 only.

Measurement tool

The primary outcome was the total fine motor score at posttest (maximum 60 points). Fine motor development was assessed using a 20-item structured observation instrument adapted from Noiniam¹² used in this study in its original form, without modification, under the developer's CC BY-NC-ND 4.0 open-access license with full attribution, covering four domains (5 items each). Each item was scored 0–3 based on the quantity of the task completed within a fixed time limit specified for that item: 3 = full quantity completed within the time limit;

2 = more than half completed; 1 = less than half completed; 0 = unable to complete or did not attempt (maximum total: 60). The complete instrument, including item-level time limits and materials, is provided in Supplementary Table S1. Content validity was confirmed by three experts (CVI = 0.89), and internal consistency tested with a pilot sample of 30 children (Cronbach's α = 0.87). A comparable content-validation and reliability-testing approach has been applied in developing motor-function assessment tools for Thai children in other clinical contexts.¹³

Assessment was conducted by research team members, independent of the classroom teacher. A pre-collection calibration session standardized scoring among assessors. Assessors were not blinded to group allocation, and the same research team members who delivered the intervention also conducted the outcome assessments; this overlap in role represents a substantial source of potential detection bias that could not be mitigated through role separation. Inter-rater reliability was good (ICC = 0.88, 95% CI: 0.82–0.93).¹⁴ Each child was independently scored by two research team assessors; when scores diverged, the child was asked to repeat the task so both assessors could re-observe and reach an agreed score. Assessments were not video-recorded, precluding independent blinded re-rating.

Bias

Potential biases were addressed systematically. Selection bias was partially mitigated through school-level lottery and predefined eligibility criteria; residual baseline imbalances were addressed via post hoc ANCOVA sensitivity analyses. Performance bias was unavoidable given the visible intervention. Detection bias was substantial: assessors were not blinded, and the same research team members who delivered the intervention also conducted assessments, with role overlap only partially offset by a pre-collection calibration session. Attrition bias was absent (100% retention). Criterion contamination is expected to have favored the experimental group to some degree: the intervention's activities were organized around the same four domain labels used by the outcome instrument, and at least one activity (bead threading, Week 5) closely resembled a specific instrument item (macaroni threading), whereas the comparison group's routine activities overlapped with instrument items only incidentally and to a lesser extent. This asymmetry is likely to have inflated the between-group effect size in a specific, non-neutral direction, though the degree of item-level overlap varied across the program's activities and was not uniform, motivating substantial caution in interpreting the effect size's magnitude and the use of standardized, non-aligned instruments in future studies.

Statistical analysis

Descriptive statistics (frequency, percentage, mean $\pm$ SD) were computed for baseline characteristics. Baseline comparability was examined using chi-square and independent t-tests. Within-group change was assessed by paired t-test; between-group posttest comparison used independent t-test. Cohen's d^9 was reported as the effect size index, interpreted as: small (≥ 0.20), medium (≥ 0.50), large (≥ 0.80), and very large (≥ 1.20).¹⁵ Change scores (post-pre) were calculated to supplement between-group interpretation.

Given the significant baseline difference in fine motor scores, single-covariate ANCOVA (pretest score as covariate) was conducted post hoc as the primary between-group analysis, selected after baseline imbalance became apparent. An independent t-test on posttest scores was retained for descriptive comparison. Additional sensitivity analyses comprised: (i) an independent t-test on change scores; and (ii) two-covariate ANCOVA (pretest score and family structure). Statistical significance was set at $\alpha = 0.05$. (Data were analyzed using SPSS version 29.0.1 [IBM Corp.].) Levene's test indicated unequal variances between groups at baseline ($p < 0.001$) and posttest ($p = 0.037$); given equal group sizes ($n = 46$ per group), this is expected to have minimal impact on the validity of ANCOVA and t-test results. Reported t-tests used pooled variance; Welch's correction yielded an essentially identical result ($t = 10.248$, $p < 0.001$ for both). ANCOVA assumption checks indicated adequate homogeneity of regression slopes ($p = 0.211$), normally distributed residuals (Shapiro-Wilk $p = 0.988$), and 8 of 92 observations (8.7%) exceeding the conventional Cook's distance threshold. Linearity was weaker in the experimental group ($r = 0.10$, ns) than the comparison group ($r = 0.46$, $p = 0.001$). Heteroscedasticity-robust (HC3) standard errors yielded a similar 95% CI (10.60–15.37) to the original (10.74–15.22).

RESULTS

Participant flow

Ninety-two children were enrolled (experimental: $n = 46$; comparison: $n = 46$), following school-level (two-cluster) allocation in which one entire school was assigned to each condition; guardian consent was obtained prior to this allocation, so recruitment was not influenced by knowledge of group assignment. No withdrawals occurred; all completed both assessments. Intervention fidelity was complete: all eight sessions were delivered as planned with all 46 experimental group children attending every session (100.0%). No adverse events were recorded in either group. (Fig 1)

Baseline characteristics

Table 2 presents baseline characteristics. The groups did not differ significantly in sex, age, or screen time (all $p > 0.05$). Statistically significant differences were observed in family structure ($p = 0.029$), with more intact two-parent families in the experimental group (97.8% vs. 78.3%). Baseline fine motor scores differed significantly between groups (28.63 vs. 33.63, $p < 0.001$), an imbalance addressed through post hoc sensitivity analyses.

Within-group comparisons

The experimental group improved from 28.63 ± 4.07 to 48.43 ± 4.23 (+19.80 points, +69.2%; $d = 3.56$). The comparison group improved from 33.63 ± 7.88 to 37.00 ± 6.28 (+3.37 points, +10.0%; $d = 0.45$).

Between-group comparison

The primary analysis (single-covariate ANCOVA) yielded adjusted posttest means of 49.21 (experimental) and 36.23 (comparison), a between-group difference of 12.98 points (95% CI: 10.74–15.22), $F(1, 89) = 132.66$, $d = 2.59$, $p < 0.001$, calculated as the adjusted mean difference divided by the residual standard deviation (5.01). These estimates assume independence among children and do not account for school-level clustering, given the single-cluster-per-arm design. The unadjusted posttest difference was also significant (48.43 vs. 37.00; $d = 2.14$). The experimental group's change score (+19.80) was 5.9 times greater than the comparison group's (+3.37), despite commencing from a lower baseline.

Domain-level analysis

All four domains showed significant between-group differences at posttest (all $p < 0.001$; Cohen's d range: 1.34–1.56), consistent with the primary outcome. These analyses were exploratory, not corrected for multiplicity, and subject to the same design limitations (single-cluster-per-arm, criterion contamination) as the primary outcome.

Sensitivity analysis

Additional sensitivity analyses are presented in Table 4. The two-covariate ANCOVA (pretest score and family structure) yielded $d = 2.55$ ($p < 0.001$), and the change score t-test yielded $d = 2.49$ ($p < 0.001$). All estimates exceeded the unadjusted $d = 2.14$. However, this consistency does not confirm a causal intervention effect, given the shared limitations of a single school per condition, criterion contamination favoring the experimental group, and non-blinded assessment. These estimates are compatible with, but do not establish, the

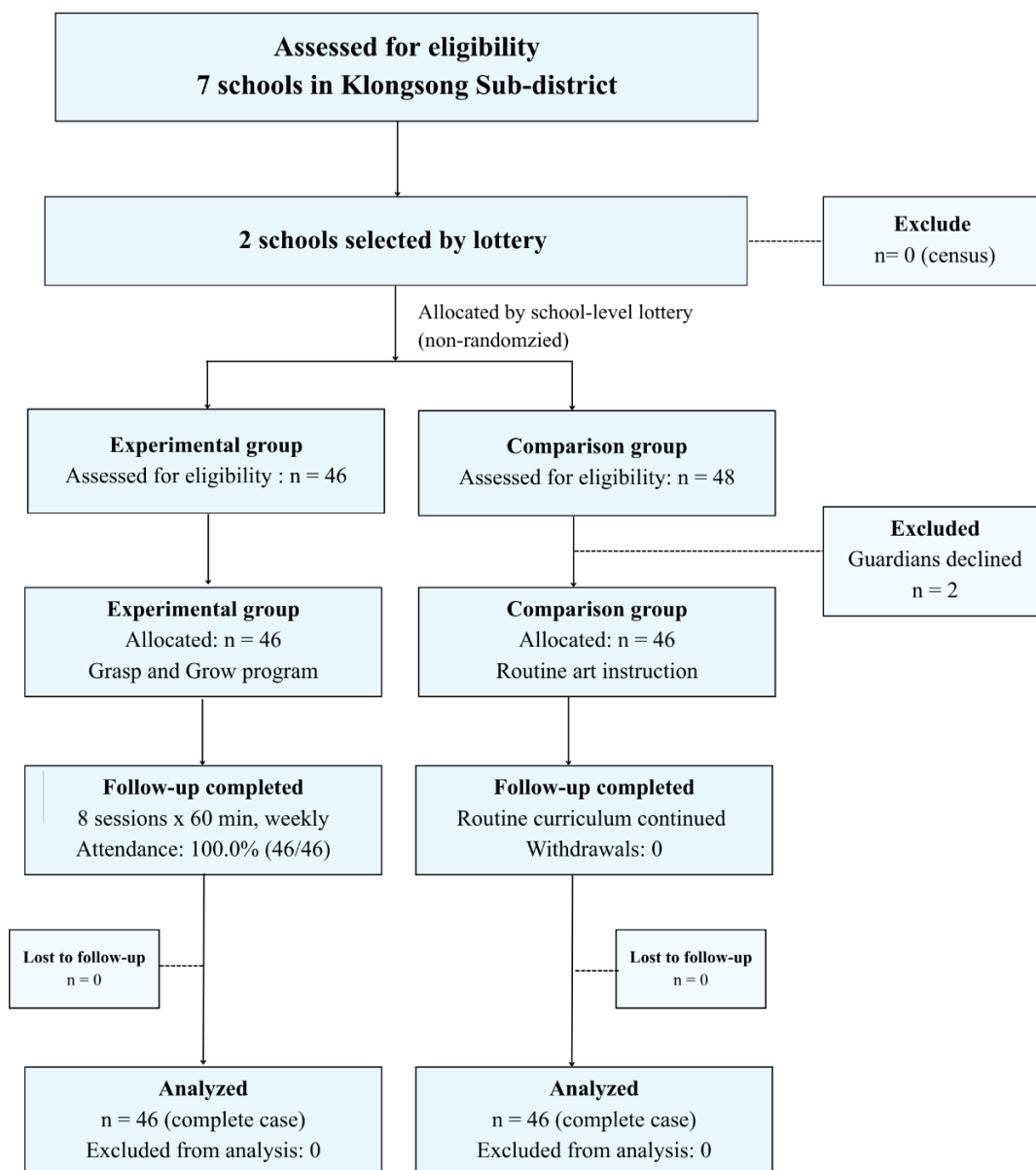


Fig 1. Participant flow diagram (TREND format for non-randomized designs).

Allocation occurred at the school level (two-cluster allocation); one entire school was assigned to each condition, and individual children were not independently randomized. Two schools were selected by lottery from seven eligible school; a second lottery assigned one school to each condition. Of 94 eligible children across both schools, 92 were enrolled (46 at the experimental school; 46 of 48 eligible at the comparison school, where 2 guardians declined participation). No withdrawals or losses to follow-up occurred among enrolled participants; all 92 were included in the final analysis.

TABLE 2. Baseline characteristics and group comparability (n = 92).

Characteristic	Total (n=92)	Experimental (n=46)	Comparison (n=46)	p-value
Sex: male, n (%)	48 (52.2)	23 (50.0)	25 (54.3)	0.68
Age (years), mean ± SD	4.71 ± 0.46	4.72 ± 0.45	4.69 ± 0.47	0.762
Screen time >2 h/day, n (%)	56 (60.9)	31 (67.4)	25 (54.3)	0.187
Two-parent family, n (%)	81 (88.0)	45 (97.8)	36 (78.3)	0.029*
Baseline fine motor score, mean ± SD	31.13 ± 6.72	28.63 ± 4.07	33.63 ± 7.88	<0.001***

* $p < 0.05$.

TABLE 3. Between-group comparison at posttest and change score analysis.

Outcome	Total (n=92)	Experimental (n=46)	Comparison (n=46)	t	95% CI	p	d
Posttest score, mean $\pm$ SD	42.72 $\pm$ 7.83	48.43 $\pm$ 4.23	37.00 $\pm$ 6.28	10.25	9.22, 13.65	<0.001***	2.14 (95% CI: 1.62, 2.65)
Change score, mean	11.59 $\pm$ 10.55	+19.80 $\pm$ 5.56	+3.37 $\pm$ 7.50	-	-	-	-

t-test used pooled variance (Welch's *t* = 10.248, *p* < 0.001, nearly identical).
Exact *p*-values reported; no asterisks.

TABLE 4. Sensitivity analyses: change score comparison and ANCOVA-adjusted posttest means.

Analysis	Experimental	Comparison	t(90)	p-value	Cohen's d
Change score (mean $\pm$ SD)	+19.80 $\pm$ 5.56	+3.37 $\pm$ 7.50	11.94	<0.001***	2.49
ANCOVA-adjusted posttest mean ¹	49.21	36.23	-	<0.001***	2.59
Two-covariate ANCOVA ²	49.12	36.32	-	<0.001***	2.55

¹ Post hoc primary analysis (pretest as covariate), conducted after baseline imbalance was identified.

² Pretest and family structure as covariates. **p* < 0.05

interpretation that baseline imbalance suppressed rather than inflated the apparent effect.

DISCUSSION

Summary of main findings

This study found that the Grasp and Grow program produced a substantially larger improvement in fine motor skills than routine art instruction, an effect that was consistent across multiple sensitivity analyses accounting for baseline imbalances between groups. Improvement was observed across all four assessed fine motor domains. The comparison group also improved, though to a smaller degree, indicating that routine classroom art activities are not without benefit, but that structured, theory-grounded sequencing provided meaningfully more support for fine motor development.

Interpretation and relevance

The program was grounded in Gesell's Maturational Theory¹⁶, with activities sequenced from foundational wrist and finger control through grip modulation and bilateral coordination to tool-guided precision cutting,

an ordering absent from the comparison group's routine curriculum. The program also incorporated progressive difficulty, drawn directly from this sequencing logic, and distributed exposure across varied activity contexts in the Week 8 review session, consistent with literature on the benefits of spaced over massed practice.¹¹ This theoretical grounding offers one plausible explanation for why a structured, sequenced program outperformed unstructured routine art activities covering similar materials.

These findings carry practical relevance for early childhood settings, particularly in resource-limited government schools, where access to specialist occupational therapy is often limited. A structured but low-cost, classroom-deliverable program of this kind offers a feasible option for supporting fine motor development in typically developing children, addressing a concern flagged at the national level for several consecutive years. Beyond the immediate skill gain, fine motor proficiency in early childhood has downstream relevance: it has been linked to later academic performance^{17,18} and, in at least one large cohort, to behavioral health outcomes extending well beyond the skill domain itself.¹⁹

This suggests that early, low-cost fine motor support may carry value beyond the immediate outcome measured here, though this study was not designed to test those downstream pathways directly.

Comparison with other studies

The direction of this study's findings is broadly consistent with that study's conclusion that structured motor skill interventions are, on average, moderately effective for improving fine motor outcomes in young children²⁰, and with another meta-analysis concluding that fundamental movement skills require active teaching and reinforcement rather than developing through maturation alone.¹⁰ However, the magnitude of effect observed in this study was considerably larger than what is typically reported in that body of literature. Several factors plausibly explain this gap: the present comparison group received an active alternative (routine, unstructured art instruction) rather than no intervention, which should, if anything, narrow rather than widen an observed difference; conversely, the experimental group's low starting score left more room for improvement, and the assessment instrument's close alignment with the trained activities raises the possibility of criterion contamination, which other studies using more generic standardized instruments would not share to the same degree.

Strengths

This study's strengths include a statistical analysis plan that appropriately addressed the observed baseline imbalance between groups (though selected post hoc rather than pre-specified, as noted in the Limitations), an active comparison condition rather than a no-treatment control (allowing isolation of the program's specific contribution beyond general classroom engagement), complete retention with no missing data, and a structured four-stage intervention development process including expert content validation and pilot testing prior to the main trial.

Limitations

Several limitations require acknowledgement. Criterion contamination is expected to have favored the experimental group specifically, given that the intervention's activities were organized around the same four domain labels as the outcome instrument, whereas the comparison group's routine activities overlapped only incidentally; this asymmetry likely inflated the between-group effect in a specific, non-neutral direction rather

than merely inflating within-group change. Assessors were not blinded to group allocation, and the same research team members who delivered the intervention also conducted assessments, representing a substantial source of potential detection bias; assessments were not video-recorded, precluding independent blinded re-rating. Additionally, the experimental group's sessions involved research team members delivering the program with the classroom teacher present, while the comparison group's sessions were delivered by classroom teachers alone; this difference in provider composition introduces a potential attention/provider confound that cannot be separated from the program's specific content using the current design. The baseline fine motor imbalance and the family structure difference between groups represent threats to internal validity that were addressed but not eliminated by sensitivity analyses; while all adjusted estimates exceeded the unadjusted estimate, this pattern is compatible with, but does not establish, suppression of the effect by baseline imbalance, given the limitations noted above. The study was confined to two schools in one sub-district, limiting generalizability, and sessions were delivered by research team members rather than classroom teachers, which limits inference about real-world scalability under ordinary school conditions.

Future research

A cluster-randomized trial with baseline-matched groups and blinded assessors would address the principal limitations identified here. A fully teacher-led effectiveness trial under ordinary classroom conditions, without research-team delivery, is needed to establish real-world scalability. Future work should also incorporate a standardized, independently validated fine motor instrument not aligned with the specific trained activities, to rule out criterion contamination as an explanation for effect size.

CONCLUSIONS

The Grasp and Grow program produced significantly greater fine motor improvement than routine art instruction in this exploratory, single-cluster-per-arm study of Thai preschool children. Given its low cost and minimal personnel requirements, the program may offer schools in resource-limited settings a feasible option for supporting fine motor development, pending confirmation through a teacher-led, cluster-randomized trial.

Data Availability Statement

The datasets are not publicly available but are available from the corresponding author upon reasonable request.

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DECLARATIONS

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This study received no external funding.

Conflict of Interest

The authors declare no conflicts of interest related to the content of this manuscript.

Registration Number of Clinical Trial

The study was retrospectively registered with the Thai Clinical Trials Registry (TCTR20260409005; 9 April 2026).

Author Contributions

Conceptualization: P.C., P.D., N.T., R.S., A.Se., and A.Sa.; Methodology: all authors; Software: P.D., A.J., and R.A.; Validation: P.D., A.J., and R.A.; Formal analysis and data interpretation: all authors; Investigation: P.C., P.D., N.T., R.S., A.Se., A.Sa., and A.J.; Resources: all authors; Data Curation: P.C., P.D., N.T., R.S., A.Se., and A.Sa.; Writing – original draft: P.C., P.D., and N.T.; Writing – review & editing: P.D.; Visualization: P.D.; Project administration: P.D. All authors read and approved the final manuscript.

Use of Artificial Intelligence

The authors used AI-assisted tools (Claude Sonnet 4.6, Anthropic) to support language editing and manuscript formatting. All intellectual content, research design, data collection, analysis, and interpretation were conducted by the authors. The authors take full responsibility for the accuracy and integrity of the work.

Institutional Review Board Statement

This study was approved by the Valaya Alongkorn Rajabhat University under the Royal Patronage Institutional Review Board (COA No. 0055/2568; REC No. 0043/2568; July 21, 2025) and conducted in accordance with the Declaration of Helsinki. Written informed consent was obtained from all guardians prior to enrollment. Given the young age of participants (4–5 years), separate child assent was not obtained; guardian consent served as the

basis for enrollment, consistent with standard practice for research involving preschool-aged children.

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