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QUASI-EXPERIMENTAL EVALUATION OF OFFLINE COMPUTER-BASED VIRTUAL MANIPULATIVES FOR CORRECTING PLACE-VALUE MISCONCEPTIONS AMONG PRIMARY PUPILS IN RURAL NIGERIAN SCHOOLS

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ABSTRACT
Persistent place-value misconceptions can limit pupils' interpretation of multidigit numbers and their use of base-ten regrouping. This quasi-experimental study evaluated an offline computer-based virtual-manipulative intervention in rural public primary schools in Edu and Patigi Local Government Areas, Kwara State, Nigeria. Sixteen schools formed eight matched pairs. A total of 320 Primary 4 pupils were enrolled and completed a diagnostic pre-test; 310 provided paired pre-test and post-test records, with 155 pupils in each study condition. Over eight weeks, intervention classes received 24 teacher-guided, 35-minute lessons using offline virtual base-ten blocks (ones, tens, and hundreds) and place-value charts. Comparison classes received the same scheduled instructional time and place-value content through usual board, textbook, and paper-based instruction. The 24-item two-tier diagnostic assessment produced a total misconception score from 0 to 24, with higher scores indicating more misconception-consistent reasoning. A baseline-adjusted model with matched-pair fixed effects and school-clustered CR2 standard errors estimated an adjusted post-test difference of −2.55 points for the intervention relative to comparison (95% CI [−3.79, −1.31], p = .002; Hedges' g = −0.61). Adjusted differences were −1.28 points for place-value principle items (95% CI [−1.94, −0.62], Holm-adjusted p = .006) and −1.20 for regrouping principle items (95% CI [−1.93, −0.47], Holm-adjusted p = .006). At post-test, pupils in intervention schools had lower adjusted misconception scores than pupils receiving usual instruction on the total measure and both domains. Because schools were not randomly assigned and only 16 schools in two local government areas participated, unmeasured differences between conditions may remain; a larger evaluation with a delayed assessment is needed.

Keywords: place value, mathematical misconceptions, virtual manipulatives, offline learning, quasi-experimental design, Nigeria

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Introduction

Understanding how a digit represents a quantity depends on more than reading a numeral or applying a written algorithm. A pupil who reads 407 as four hundred and seven may still treat the 4 as four units when explaining its value, or may omit the zero when composing the number. These responses reveal an incomplete connection between a digit's written position and the size of the unit it represents. Across the primary years, children's positional knowledge develops alongside number writing and multidigit calculation (Cheung & Ansari, 2021; Herzog & Fritz, 2022; Sari et al., 2021).

This study focuses on two related principles. The place-value principle requires a pupil to interpret a digit through both its face value and its position, while the regrouping principle requires recognition that ten units of one order can be exchanged for one unit of the next order. Jensen et al. (2024) show how these principles can be used to distinguish difficulties that appear similar in written work, including errors about digit position, zero as a placeholder, and equivalence across units. Mix et al. (2022) describe how children may use approximate number knowledge before they can explicitly coordinate the units and symbols in a multidigit numeral.

Place-value misconceptions can carry into written addition and subtraction when pupils do not understand why a digit changes place during an exchange. Vermeulen et al. (2020) show that an incorrect answer alone does not identify the reasoning behind a subtraction error; diagnostic items must elicit the rule a pupil is using. Recording both an answer and an explanation can distinguish a computational slip from a misconception about place or unit equivalence.

In rural Nigerian schools, device access and connectivity shape whether digital instruction can be delivered. National learning assessments link learning opportunity to household resources, location, and school type; studies of remote learning in Nigeria also document unequal access to devices and internet-based instruction (Adeniran et al., 2020; Azubuike et al., 2021; Olanrewaju et al., 2021). UNICEF Nigeria's education fact sheets report that access to computers and the internet at home is lower in rural and poorer households than in urban and wealthier households (UNICEF Nigeria, 2023). An offline application can reduce dependence on connectivity during lessons, but it does not remove the need for working devices, charging, teacher preparation, and curriculum fit.

Virtual manipulatives can make actions on a representation visible: a pupil can move ten ones into a ten, expand a numeral into units, or compare a written digit with the quantity represented by its position. Studies of virtual base-ten games and links between concrete objects and symbols point to a shared instructional requirement: teachers need to connect what pupils see on screen to mathematical language and notation (Donovan & Fyfe, 2022; Flevares et al., 2022; Yuan et al., 2021). Studies of fraction learning, online programs, and pupils receiving special education report varied outcomes, and many measure broad achievement rather than changes in place-value misconceptions (Ahmad & Siller, 2024; Bush, 2021; Park et al., 2022; Rich, 2021; Siller & Ahmad, 2024).

Technology-supported instruction depends on how a tool is used, the learning goal, and the conditions in which pupils and teachers work. Major et al. (2021) reported average positive effects across trials of technology-supported personalized learning, whereas Akın (2022) synthesized web-based mathematics instruction in connected settings rather than offline classrooms. Hennessy et al. (2022) and UNESCO (2023) discuss teacher capacity, access, relevance, and curriculum fit as conditions for implementation. Evaluations of rural technology-aided programs report different outcomes across settings and measures, so access to a screen alone cannot be treated as a mathematics intervention (de Hoop et al., 2024; Foster, 2024; World Bank et al., 2021).

It remains unclear whether teacher-guided, fully offline virtual manipulatives can reduce place-value and regrouping misconceptions among rural Nigerian primary pupils. Place-value diagnostic studies distinguish these two domains, and Nigerian studies describe technology access and learning conditions, but none has tested an offline place-value manipulative against time-matched instruction. Nigerian classroom studies of primary mathematics include a quasi-experimental study in Osun State in which pupils taught with the Algorithm School Kit outperformed conventionally taught pupils in number recognition and basic arithmetic (Orisadare & Makinde, 2026), but that study reported early numeracy outcomes rather than place-value misconception scores and did not test an offline virtual manipulative. This evaluation compares schools using the offline lessons with schools receiving time-matched usual instruction, using the same diagnostic assessment in both conditions.

Purpose, research questions, and hypotheses

The purpose of the study was to estimate the association between participation in eight weeks of offline computer-based virtual-manipulative instruction and post-test place-value misconception scores among Primary 4 pupils, relative to time-matched usual instruction. The primary outcome was the total diagnostic misconception score. Domain scores for the place-value principle and regrouping principle were secondary outcomes.

Research Question 1. After adjustment for baseline score and pre-intervention characteristics, do pupils in schools using offline virtual manipulatives have lower post-test total misconception scores than pupils receiving usual instruction?

Research Question 2. Are adjusted post-test scores lower in the intervention than in the comparison condition for both the place-value principle and regrouping principle domains?

The primary alternative hypothesis was that the adjusted post-test total misconception score would be lower in the intervention condition. The secondary alternative hypotheses were that adjusted post-test scores would be lower in the intervention condition for both the place-value and regrouping domains. All scores were coded so that lower values indicated fewer misconception-consistent responses.

Literature review

Place-value knowledge and misconception patterns

Place-value knowledge involves coordinating a numeral's written structure with the quantities represented by its digits. Cheung and Ansari (2021), Herzog and Fritz (2022), and Mix et al. (2022) describe changes in children's number writing and place-value understanding, including cases where accurate procedures coexist with incomplete knowledge of base-ten relations. Jensen et al. (2024) and Sari et al. (2021) treat place identification and relations among bundling units as distinct but connected parts of a learner's understanding.

Place-value misconceptions can be identified by the rule a pupil applies. A pupil may treat digits as separate numbers, overlook the placeholder function of zero, read a digit's face value without multiplying by the unit specified by its position, or fail to recognize that ten units can be renamed as one unit of the next order. These patterns connect place-value understanding to arithmetic procedures, especially when addition or subtraction requires an exchange across columns (Vermeulen et al., 2020). Accordingly, a diagnostic instrument should include tasks requiring pupils to name, represent, decompose, compare, and regroup numbers rather than relying only on a total arithmetic score.

A wrong answer alone cannot show whether a pupil misunderstood digit position, the role of zero, or an exchange between units. Jensen et al. (2024) organize place-value difficulties through the place-value and regrouping principles, and Vermeulen et al. (2020) show that subtraction items differ in how clearly they elicit a targeted error. The assessment used here followed that distinction: an error counted as misconception-consistent only when the answer and explanation matched a predefined rule.

Virtual manipulatives and links between representations

Virtual manipulatives are interactive representations that allow learners to alter mathematical objects on a screen. Well-designed tools let pupils test relationships between quantities, diagrams, and written symbols. Flevares et al. (2022) describe how first graders' comments and strategies while using virtual base-ten materials were related to their emerging knowledge of tens. Donovan and Fyfe (2022) found that connecting concrete objects to abstract symbols supported children's place-value learning, and Yuan et al. (2021) report that physical models are useful when their visual structure foregrounds the relation a learner is ready to notice.

The effect of a manipulative depends on the information it makes visible and the instructional prompts that accompany it. Lafay et al. (2023) found that the conceptual transparency of manipulatives influenced how children at risk for mathematics learning difficulties represented and interpreted multidigit numbers. Rich's (2021) review of virtual manipulatives in elementary mathematics similarly emphasizes the need to situate digital tools within mathematical tasks and classroom discussion. Studies with learners with disabilities report benefits in some settings, but single-case designs and specialized samples limit direct transfer to whole-class rural instruction (Jimenez & Besaw, 2020; Park et al., 2022; Shin et al., 2021; Shurr et al., 2021).

Studies combining concrete and virtual manipulatives report mathematics achievement outcomes, but their measures concern broad achievement or fraction learning rather than diagnostic changes in place-value misconceptions (Ahmad & Siller, 2024; Siller & Ahmad, 2024). Bush (2021) examined a software-based intervention that used digital manipulatives to support conceptual understanding of fractions; Park et al. (2024) evaluated a synchronous online fraction intervention using virtual manipulatives for pupils with learning disabilities. Those studies concern different mathematical content and learner groups, so they do not establish the effect of an offline place-value program in rural Nigerian classrooms.

Across mathematics topics, Schönherr et al. (2024) found a positive overall effect of visualization interventions, although effects varied with the features of the display and the learning context. For that reason, the on-screen objects in this evaluation were linked to the place-value chart and written numeral, and teacher prompts asked pupils to explain what an exchange changed and what remained equal.

Offline technology in rural instruction

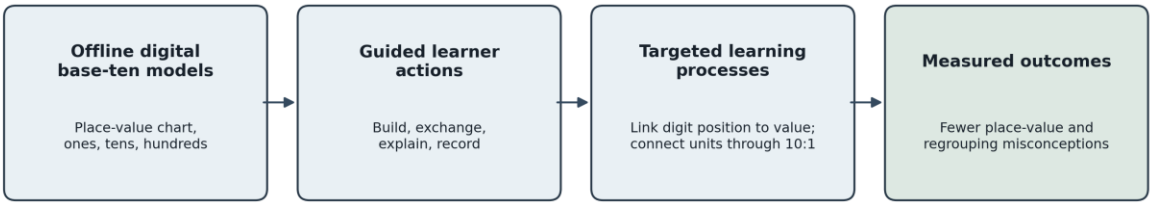
An offline program removes the need for a school internet connection during a lesson, but device access and maintenance remain necessary. UNICEF's Nigeria analyses and studies of pandemic-era remote learning document differences in access to computers, internet, and learning opportunities by location and socioeconomic status (Azubuike et al., 2021; Olanrewaju et al., 2021; UNICEF Nigeria, 2023). UNESCO (2023) cautions that technology policy should consider how devices are supplied, maintained, used by teachers, and connected to curriculum goals. The evaluation therefore used locally stored software, shared classroom devices, rechargeable power packs, and teacher-led activities; it did not require home access or online accounts.

Hennessy et al. (2022) review technology use for teacher professional development in low- and middle-income countries, while Major et al. (2021) synthesize technology-supported personalized learning. Their focus on teacher preparation and program design matters because digital instruction varies by learning domain, intensity, personalization, and teacher role. The rural Zambia program evaluated by de Hoop et al. (2024) combined technology with activity-based learning, and Foster (2024) examined a conceptual replication of supplemental computer-assisted mathematics instruction. These are instructional packages distinct from the offline place-value lessons evaluated here, and they targeted different outcomes. The present program must therefore be judged by its own instructional components and diagnostic outcomes.

Conceptual rationale

The intervention was organized around a short learning pathway: pupils manipulate base-ten units, connect each action to a place-value chart and numeral, explain the equivalence represented by an exchange, and then solve a parallel paper item. Teacher questions made the relation between face value and unit explicit and asked pupils to justify why ten ones can be represented as one ten or ten tens as one hundred. The proposed pathway is consistent with place-value research that treats positional meaning and regrouping as linked but distinct principles (Jensen et al., 2024; Sari et al., 2021). Figure 1 presents the proposed instructional pathway and measured outcomes.

Figure 1. *Proposed instructional pathway from offline manipulation to diagnostic outcomes*



Note. Teacher prompts and visual feedback are instructional supports. The study measured misconception scores and did not test mediation.

The lesson sequence linked pupils' on-screen actions to the diagnostic blueprint. Teachers asked pupils to explain each manipulation and then to solve a comparable paper item. Both conditions covered the same objectives and scheduled time, so the comparison estimates the difference between the two instructional approaches as delivered.

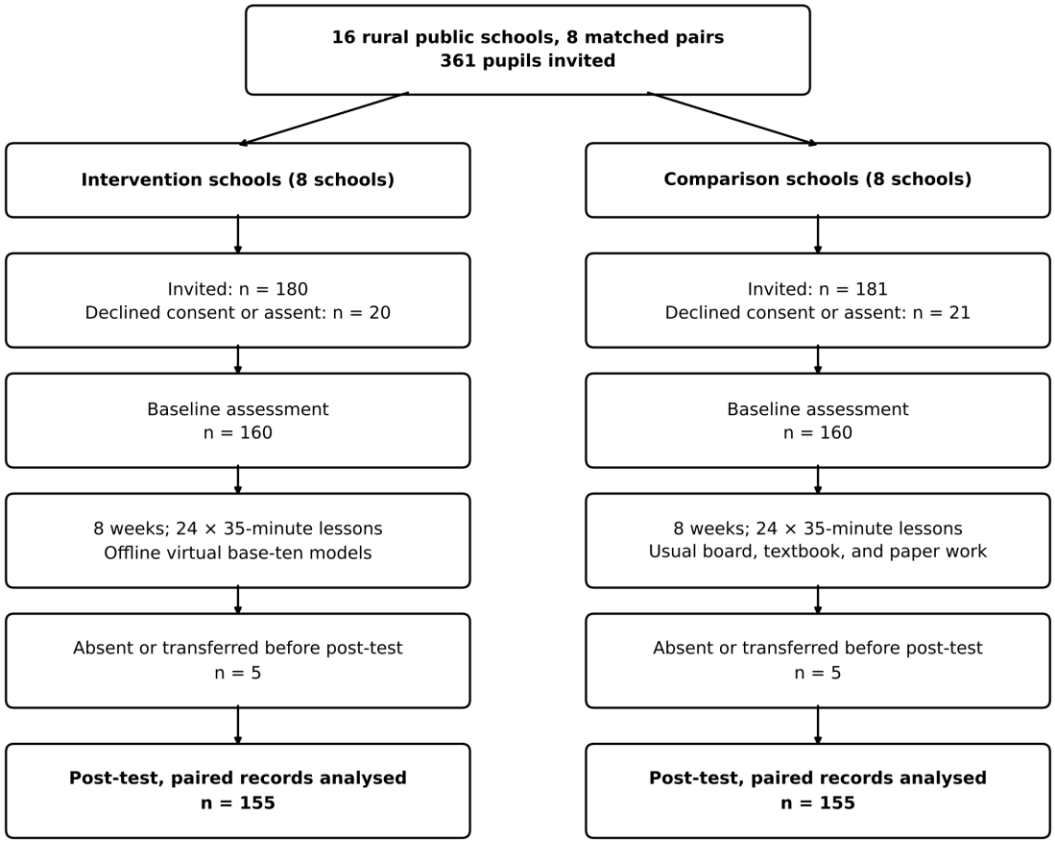
Methodology

Design and setting

A non-equivalent comparison-group pre-test and post-test quasi-experimental design was used. Sixteen public primary schools in rural wards of Edu and Patigi Local Government Areas, Kwara State, were purposively selected from eligible schools with an active Primary 4 class and an instructional timetable that could accommodate the study. The schools were organized into eight matched pairs. Within each pair, one school implemented the offline virtual-manipulative lessons and the other continued with usual place-value instruction. School condition was not randomly assigned because local education authorities used existing timetables and staffing arrangements to determine which schools could host the intervention during the study term. The design therefore supports an adjusted comparison but does not remove all risk of selection bias.

Matching was completed before instruction using local government area, Primary 4 class size, prior-term mathematics performance, and available teaching time. Schools within each pair were located in the same local government area and served communities classified as rural by the participating education authorities. Each school contributed one Primary 4 class. Figure 2 shows the study sequence and participant flow.

Figure 2. *School-level design, instructional period, and participant flow*



Note. School assignment was nonrandom. Of 361 pupils invited, 41 declined consent or assent, 320 were enrolled at baseline, and 310 had paired valid pre-test and post-test records.

Participants and recruitment

The baseline cohort included 320 Primary 4 pupils, with 160 pupils in the eight intervention schools and 160 in the eight comparison schools. Pupils were drawn in random order from each class register and invited in sequence until 20 pupils per class had consented; 361 pupils were invited in total, and 41 declined caregiver consent or withheld assent (20 in the intervention schools and 21 in the comparison schools), so that 320 pupils, 20 per class, completed the baseline assessment. Ten pupils, five in each condition, were absent for the post-test or had transferred before the assessment and did not provide paired records. No school withdrew. The analytic sample comprised 310 pupils, 155 per condition. Mean age at baseline was 9.42 years (SD = 0.83) in intervention schools and 9.45 years (SD = 0.79) in comparison schools. Girls represented 51.0% of the intervention group and 49.7% of the comparison group.

Caregivers received an information sheet and provided written consent; pupils provided age-appropriate assent. Participation did not affect school grades or access to regular lessons. Ethical clearance was granted by the Kwara State Ministry of Education Research Ethics Committee (reference KWSME/REC/2025/041, approved 15 September 2025), and administrative permission was obtained from the Edu and Patigi local education authorities before recruitment. Pupil records were assigned study codes, stored without names, and used only for the analysis described here.

Table 1: *Baseline characteristics of pupils with paired assessment records*

Characteristic	Offline virtual manipulatives (n = 155)	Usual instruction (n = 155)	SMD
Age in years, M (SD)	9.42 (0.83)	9.45 (0.79)	−0.04
Girls, n (%)	79 (51.0)	77 (49.7)	0.03
Prior-term mathematics score, M (SD), 0–100	48.6 (11.1)	47.8 (11.4)	0.07
Place-value diagnostic score, M (SD), 0–12	7.31 (2.38)	7.38 (2.31)	−0.03
Regrouping diagnostic score, M (SD), 0–12	6.79 (2.28)	6.83 (2.31)	−0.02
Total misconception score, M (SD), 0–24	14.10 (4.20)	14.21 (4.15)	−0.03

Note. SMD = standardized mean difference. Scores are higher when more misconception-consistent responses are observed. Baseline differences are described with standardized differences rather than tests of statistical significance.

Instructional conditions

The intervention was delivered during the first term of the 2025/2026 academic session over eight instructional weeks. Teachers in intervention schools used Place Value Blocks Offline, a locally stored, browser-based program that ran without internet access, developed by the research team as an HTML5 adaptation of open base-ten block activities and available free of charge for non-commercial classroom use on request from the corresponding author. Each class had ten school laptops or tablets for paired pupil work. Rechargeable power packs were provided where mains power was not available during the scheduled lesson. The program presented single cubes as ones, rods as tens, flats as hundreds, a place-value chart, and a linked numeral display.

Lessons followed a common routine. The teacher introduced a number or problem, pupils built or decomposed the quantity on-screen, pairs explained the relation between the digital blocks and written digits, and pupils solved a similar item on paper. Feedback showed whether the displayed amount matched the target number and prompted a retry; it did not provide a full solution before pupils explained their choice. Tasks progressed from identifying digit value and the zero placeholder to composing and decomposing numbers, comparing magnitudes, exchanging ten ones for one ten, exchanging ten tens for one hundred, and applying exchanges in addition and subtraction.

Comparison teachers used the usual board, textbook, place-value chart, and paper exercises. Both conditions covered the same curriculum objectives and scheduled 24 lessons of 35 minutes each, three times per week. Teachers in both conditions received a six-hour orientation. Intervention teachers practiced the offline program and the common lesson routine; comparison teachers reviewed the matched content sequence and lesson schedule. The design holds curriculum objectives and scheduled instructional time constant while comparing the two instructional approaches. Table 2 summarizes the planned activities and exposure in each condition.

Table 2: *Instructional conditions and planned implementation*

Element	Offline virtual-manipulative condition	Usual-instruction comparison
Representation	On-screen ones, tens, hundreds, place-value chart, linked numeral display	Teacher-drawn place-value chart, textbook examples, written numerals
Pupil activity	Build, decompose, exchange, compare, explain, then solve a paper transfer item	Follow teacher modeling, discuss examples, solve matched paper exercises
Teacher prompts	Ask which unit each digit represents; ask what changed and what remained equal after exchange	Ask pupils to name place, digit value, and steps in written examples
Feedback	Immediate visual match or mismatch cue followed by explanation and retry	Teacher checks responses and gives oral or board-based correction
Access and storage	Local application on shared classroom devices; no internet or learner account required	Printed textbook, board, paper, and classroom place-value chart
Planned exposure	8 weeks; 3 lessons per week; 35 minutes per lesson; 24 lessons	8 weeks; 3 lessons per week; 35 minutes per lesson; 24 lessons

Note. Both conditions addressed the same Primary 4 place-value and regrouping objectives. Intervention teachers did not use the program outside scheduled lessons during the evaluation period.

Diagnostic assessment

The Place-Value Misconception Diagnostic Assessment contained 24 two-tier items. Each item required a numerical response and a brief explanation, representation choice, or written working. Lessons and the assessment were delivered in English, the language of instruction from Primary 4 under the national curriculum, and teachers and assessors were permitted to give standardized oral clarifications of task wording in Nupe or Yoruba, the languages most commonly spoken in the two local government areas, without translating item content. Twelve items assessed the place-value principle, including digit value, zero as a placeholder, expanded form, and comparison of multidigit numerals. Twelve assessed the regrouping principle, including unit equivalence, noncanonical representations, and exchanges during addition and subtraction.

An item received one misconception point only when the pupil's answer and second-tier response matched a predefined misconception rule. Correct responses and isolated arithmetic slips without the target reasoning received zero. Blank responses were coded as missing rather than incorrect. Total and domain scores were calculated only for pupils with complete item responses; all pupils retained in the paired analysis had complete item-level records at both assessments, so no score prorating or item-level imputation was used. The total score ranged from 0 to 24; each domain score ranged from 0 to 12. Higher scores indicated more misconception-consistent responses. The item blueprint followed the distinction between positional meaning and unit regrouping used in recent place-value diagnosis research (Jensen et al., 2024; Vermeulen et al., 2020). Table 3 summarizes the domains and scoring rules.

Six mathematics education specialists reviewed the item-to-construct mapping and scoring rules. The assessment was piloted with 42 Primary 4 pupils in schools outside the evaluation sample. Internal consistency estimates (McDonald's omega) were .86 for the total score, .79 for the place-value domain, and .81 for the regrouping domain. Two trained scorers independently coded a random 20% of response booklets; the scorers were blind to school condition, because booklets carried pupil codes only, and to assessment occasion, because pre-test and post-test booklets were pooled and shuffled before scoring; agreement for misconception classifications was Cohen's kappa = .88. Disagreements were resolved against the scoring manual before the remaining scripts were coded.

Table 3: *Assessment blueprint and scoring rules*

Domain	Item content	Misconception evidence coded	Items / score range
Place-value principle	Digit value and position; zero placeholder; expanded form; numeral comparison	Treating a digit as its face value regardless of position; omitting or misusing zero; reading digits as a string	12 / 0–12
Regrouping principle	Compose and decompose units; noncanonical forms; addition and subtraction exchanges	Rejecting equivalence across units; treating a ten as one unit; carrying or borrowing without exchange meaning	12 / 0–12
Total	Both domains	One point only when the answer and explanation fit a predefined misconception rule	24 / 0–24

Note. A higher score indicates more misconception-consistent responses. A computational error is not scored as a misconception unless it reflects the target rule.

Procedure and fidelity

External assessors administered the baseline diagnostic assessment during the week before instruction and the post-test within five school days after the final lesson. The same item order and standardized directions were used at both occasions, with parallel numerical values where an item required substitution to limit direct recall. Assessors did not deliver lessons and used pupil codes rather than group labels on assessment booklets. Teacher observation and implementation logs documented lesson completion, attendance, device availability, and adherence to the common content sequence.

Thirty-two lessons were selected for structured observation across the intervention schools. Observers used a 10-item checklist covering use of the intended representation, pupil manipulation, explanation prompts, and paper transfer tasks. The comparison checklist recorded lesson duration, content coverage, teacher modeling, and pupil practice. Fidelity scores were summarized descriptively and were not used to select pupils for the outcome analysis.

Analysis plan

The primary estimand was the adjusted mean difference in post-test total misconception score between pupils in intervention and comparison schools. The primary analysis used a linear ANCOVA estimated in R (version 4.3.2) with the clubSandwich package for CR2 variance estimation, using two-sided tests at the .05 level. A nonsignificant baseline-by-condition interaction ($p = .41$) supported the homogeneity-of-regression-slopes assumption. The model specified the pupil's post-test total score as the outcome, intervention status as the exposure, the baseline total score as a covariate, and fixed effects for matched school pair. Age, sex, and prior-term mathematics score were included as prespecified pre-intervention covariates. The model coefficient for intervention represented the adjusted difference in post-test score, with negative values favoring the intervention under the scoring direction defined above.

Because pupils were nested within schools and treatment status varied at the school level, standard errors were clustered by school using the CR2 small-sample correction with Satterthwaite degrees of freedom. This choice addresses dependence within school and the limited number of clusters; ordinary pupil-level standard errors would treat correlated observations as independent (Huang & Li, 2022; Thompson et al., 2023). The matched-pair design and baseline adjustment were specified before outcome comparisons, consistent with guidance to document baseline equivalence and covariate adjustment in nonrandomized education studies (Cunningham, 2021; What Works Clearinghouse, 2022).

The two domain scores were analyzed with the same model structure, replacing the total baseline score with the corresponding baseline domain score. Holm adjustment controlled the familywise error rate across the two secondary domain tests. Effect sizes were reported as Hedges' g using the pooled baseline standard deviation as the standardizer. All estimates are accompanied by 95% confidence intervals. Results include the pre-test and post-test means, analytic sample, attrition, fidelity, and adjusted estimates so that the model-based findings can be interpreted alongside the observed scores.

Results

Participant flow and baseline characteristics

All 16 schools completed the evaluation period. Of 320 pupils assessed at baseline, 310 completed both assessments and entered the paired-record analysis. Five pupils in each condition lacked a post-test because of absence or transfer. Paired records were available for 155 of 160 pupils in the intervention condition and 155 of 160 pupils in the comparison condition, an attrition rate of 3.1% in each condition.

Baseline characteristics for the analytic sample are reported in Table 1. Standardized mean differences ranged from −0.04 to 0.07 across the listed characteristics. At baseline, the intervention group had a mean total diagnostic score of 14.10 (SD = 4.20), compared with 14.21 (SD = 4.15) in the comparison group.

Implementation and assessment performance

Intervention pupils attended a mean of 21.1 of 24 scheduled lessons (SD = 2.4). Observed adherence to the intervention checklist averaged 91.4% (SD = 5.8) across 32 observed lessons. Devices were functional for 93.8% of scheduled intervention sessions; teachers used paper versions of the same tasks when a device was temporarily unavailable. Comparison pupils attended a mean of 22.0 of 24 scheduled lessons (SD = 2.1). Observed adherence in the comparison condition was assessed in 32 lessons, which averaged 34.6 minutes (SD = 1.8), covered 93.2% (SD = 6.1) of the planned content sequence, and showed teacher modeling and pupil practice in 31 of 32 lessons (96.9%); no comparison teacher used the offline program or any other digital tool during scheduled lessons. In the analytic sample, McDonald's omega for the total score was .85 at pre-test and .87 at post-test, with domain estimates of .78 and .80 for the place-value domain and .80 and .82 for the regrouping domain at the two occasions.

Diagnostic error patterns

Table 4 shows the proportion of pupils with at least one response matching each listed error pattern. At baseline, the most frequent listed pattern in each condition was digit-only carrying or borrowing. At post-test, the proportions were lower in the intervention condition than in the comparison condition for all four patterns. Among intervention pupils, the largest absolute reduction was in digit-only carrying or borrowing, from 55.5% to 29.0%. Among comparison pupils, the corresponding proportion changed from 54.8% to 41.9%.

Table 4: *Pupils showing at least one response matching selected diagnostic error patterns*

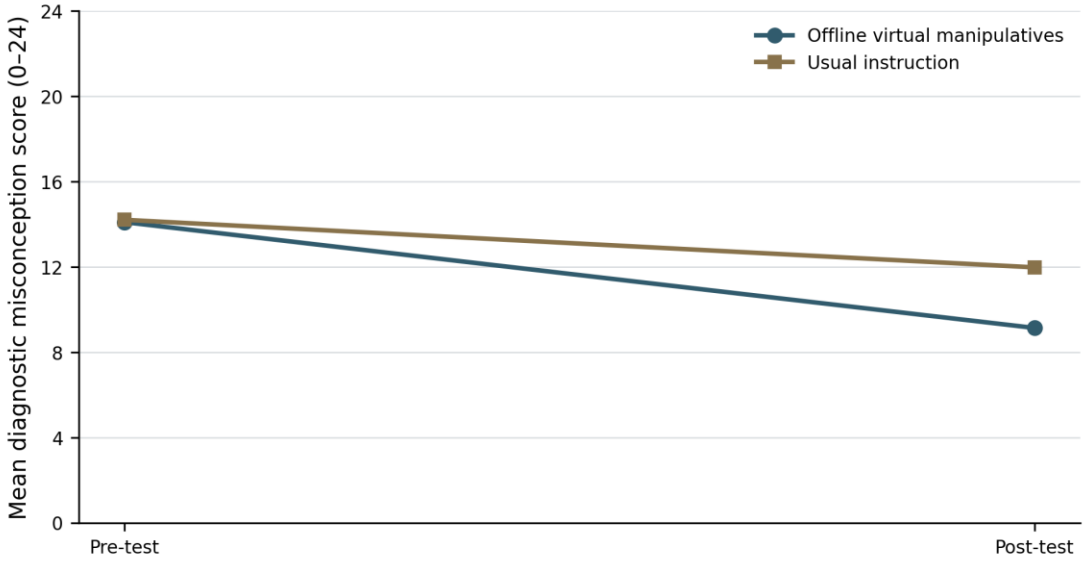
Error pattern	Intervention Pre n (%)	Intervention Post n (%)	Comparison Pre n (%)	Comparison Post n (%)
Face value used without place value	68 (43.9)	34 (21.9)	70 (45.2)	55 (35.5)
Zero placeholder omitted or misused	61 (39.4)	29 (18.7)	62 (40.0)	42 (27.1)
Ten-to-one unit equivalence rejected	78 (50.3)	39 (25.2)	80 (51.6)	58 (37.4)
Carrying or borrowing treated as digit-only procedure	86 (55.5)	45 (29.0)	85 (54.8)	65 (41.9)

Note. The analytic sample was 155 pupils per condition at each time point. A pupil could show more than one pattern. Percentages use the number of pupils with paired records as the denominator.

Pre-test and post-test scores

Unadjusted mean total scores declined from 14.10 to 9.14 in the intervention condition and from 14.21 to 11.98 in the comparison condition. Mean place-value scores declined from 7.31 to 4.56 in the intervention condition and from 7.38 to 5.98 in the comparison condition. Mean regrouping scores declined from 6.79 to 4.58 in the intervention condition and from 6.83 to 6.00 in the comparison condition. Figure 3 displays the unadjusted total-score means. Table 5 reports the observed scores and adjusted between-condition estimates.

Figure 3. *Unadjusted total misconception scores before and after instruction*



Note. Means are unadjusted. The score ranges from 0 to 24, with higher scores indicating more misconception-consistent responses.

Table 5: *Observed scores and adjusted between-condition estimates*

Outcome	Intervention Pre M (SD)	Intervention Post M (SD)	Comparison Pre M (SD)	Comparison Post M (SD)	Adjusted difference [95% CI]	p	Holm p
Total, 0–24	14.10 (4.20)	9.14 (4.01)	14.21 (4.15)	11.98 (4.08)	−2.55 [−3.79, −1.31]	.002	N/A
Place-value, 0–12	7.31 (2.38)	4.56 (2.42)	7.38 (2.31)	5.98 (2.40)	−1.28 [−1.94, −0.62]	.003	.006
Regrouping, 0–12	6.79 (2.28)	4.58 (2.35)	6.83 (2.31)	6.00 (2.34)	−1.20 [−1.93, −0.47]	.006	.006

Note. Analytic n = 155 per condition. Adjusted differences are intervention minus comparison from baseline-adjusted ANCOVA with matched-pair fixed effects and CR2 school-clustered standard errors. Models adjust for baseline score, age, sex, and prior-term mathematics score. Negative estimates favor the intervention. Holm adjustment applies to the two secondary domain tests; the primary test is not adjusted.

Adjusted intervention estimates

The adjusted post-test total misconception score was 2.55 points lower in the intervention condition than in the comparison condition, 95% CI [−3.79, −1.31], p = .002. The standardized effect was Hedges' g = −0.61 (95% CI [−0.91, −0.31]). For place-value principle items, the adjusted difference was −1.28 points, 95% CI [−1.94, −0.62], raw p = .003 and Holm-adjusted p = .006; Hedges' g was −0.55 (95% CI [−0.82, −0.27]). For regrouping principle items, the adjusted difference was −1.20 points, 95% CI [−1.93, −0.47], raw p = .006 and Holm-adjusted p = .006; Hedges' g was −0.52 (95% CI [−0.84, −0.20]).

All three adjusted estimates were negative under the prespecified scoring direction. The total-score and both domain estimates had 95% confidence intervals that did not include zero. These estimates use the same analytic sample, prespecified covariates, matched-pair structure, and school-level small-sample correction.

Discussion

Pupils in schools using the offline virtual-manipulative lessons had lower adjusted post-test scores than pupils receiving time-matched usual instruction. The estimated differences were −2.55 points for the total score, −1.28 for place-value items, and −1.20 for regrouping items. Since higher scores represented more misconception-consistent reasoning, all three estimates favored the intervention and were consistent with the study hypotheses.

The lower scores in both domains align with the instructional sequence. The application repeatedly paired the written digit with its represented unit and showed the exchange between ten smaller units and one larger unit. That pairing may have supported pupils in coordinating positional meaning with unit equivalence, the two principles used to organize the assessment (Jensen et al., 2024). Donovan and Fyfe (2022), Lafay et al. (2023), and Mix et al. (2022) describe the need for pupils to interpret how a representation maps onto symbols and quantities. Teacher prompts and paper transfer items were integral to the lessons because they asked pupils to explain the link between each digital move and the written number.

The outcome used here differs from the measures in much of the virtual-manipulative literature. Studies of virtual base-ten games, place-value representation, and elementary manipulatives have reported learning processes or achievement outcomes rather than paired diagnostic scores for misconception patterns (Flevares et al., 2022; Rich, 2021; Siller & Ahmad, 2024). Studies of fractions and specialized learner groups examine different content and classroom arrangements from whole-class place-value instruction in Kwara State (Ahmad & Siller, 2024; Bush, 2021; Park et al., 2022; Park et al., 2024; Shin et al., 2021). The adjusted differences reported here apply to this eight-week instructional package in the participating schools; they do not establish that virtual manipulatives generally correct place-value misconceptions.

Local storage allowed lessons to run without school internet, which addressed one documented constraint in Nigeria: unequal access to computers and connectivity (Azubuike et al., 2021; Olanrewaju et al., 2021; UNICEF Nigeria, 2023). However, the program still depended on functioning devices, charging, a suitable classroom schedule, and teachers able to guide pupil explanations. UNESCO (2023) and Hennessy et al. (2022) discuss device maintenance, teacher preparation, and curriculum alignment as implementation conditions. The school-level device arrangement and rechargeable packs used here may be feasible in some communities, but the evaluation did not estimate procurement, maintenance, charging, or teacher-time costs.

Major et al. (2021) reported average gains across trials of technology-supported personalized learning, although those programs differed in their features and delivery arrangements. Akın's (2022) synthesis concerns web-based mathematics instruction, while de Hoop et al. (2024) and Foster (2024) evaluated other technology-assisted programs; none tests this offline lesson package. The score differences observed here followed lessons combining a defined place-value objective, teacher-guided manipulation, feedback, and paper practice. The design cannot isolate the contribution of any one component.

The quasi-experimental design limits causal certainty. Schools were matched but not randomly assigned, and administrative feasibility may have been associated with unmeasured differences in teacher confidence, leadership, attendance, or prior classroom practice. The models accounted for pre-test score, age, sex, prior mathematics score, and matched pair, but they cannot control unmeasured differences between schools (Cunningham, 2021; What Works Clearinghouse, 2022). Implementation was school-level, and there were only 16 schools. Because each school contributed one class taught by one teacher, teacher effects could not be separated from the intervention, and the novelty of classroom devices may also have contributed to the observed differences. CR2 standard errors with a small-sample degrees-of-freedom correction address one source of overprecision, but the limited number of school units still constrains generalization and subgroup analysis (Huang & Li, 2022; Thompson et al., 2023).

The diagnostic instrument was designed for this evaluation, and its local pilot reliability estimates do not establish validity across languages, states, or grade levels. The two-tier scoring rule improves separation between computational slips and misconception-consistent reasoning, but pupils' written explanations may not capture every reasoning step. The assessment followed the intervention immediately; no delayed test established whether the changes persisted or transferred to unfamiliar tasks. The 16 participating schools were located in two local government areas, so the sample cannot represent rural Nigerian schools as a whole.

Future evaluations should use a larger number of schools and randomize within matched pairs where authorities permit, or strengthen a nonrandomized design with repeated pre-intervention measures and a prespecified analysis plan. A delayed assessment would test retention, while interviews or think-aloud tasks could clarify how pupils interpret exchanges and zero placeholders. Implementation studies should report device uptime, charging arrangements, teacher preparation, pupil access time, and costs alongside learning results. Teacher development should focus on connecting digital actions to mathematical explanations and written strategies, an area addressed in Hennessy et al.'s (2022) review of teacher learning in lower-resource settings. Measurement should be examined for language accessibility and differential item functioning before use across other Nigerian states.

Conclusion

Among the 310 Primary 4 pupils with paired assessment records, the eight-week offline virtual-manipulative package was associated with lower adjusted post-test total, place-value, and regrouping misconception scores than time-matched usual instruction. Because schools were not randomly assigned and came from two local government areas, these estimates describe an adjusted association rather than a definitive causal effect. Offline base-ten tools may support targeted place-value instruction when teachers connect each digital action to unit meaning, written notation, and paper-based transfer tasks. Larger evaluations with more schools, delayed outcomes, and implementation-cost measures are needed before the approach is generalized.

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