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GeoGebra-Supported Mathematics Instruction and Academic Performance of Senior Secondary School Science Students in Ekiti State, Nigeria

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Abstract

Dynamic mathematics environments can help learners coordinate symbolic, graphical, and numerical representations, but their value depends on how the technology is integrated into instruction. This study examined the effect of GeoGebra-supported instruction on the mathematics performance of senior secondary school science students in Ekiti State, Nigeria. The source study used a quasi-experimental pre-test-post-test control-group design. The sampling section reports 128 SS I science students selected through a multistage procedure, whereas the outcome tables contain analyzable records for 89 students, comprising 41 students in the GeoGebra group and 48 in the conventional group. Because no participant-level dataset or flow log was supplied, the revised article preserves this discrepancy and bases all statistical interpretation on the 89 cases represented in the reported results. The Mathematics Performance Test was described as a 24-item multiple-choice instrument with test-retest reliability of 0.91. The treatment phase lasted four weeks, surrounded by one-week pre-treatment and post-treatment stages. Reported pre-test means were similar for the GeoGebra and control groups, $M = 52.10$ and $M = 51.80$, respectively. At post-test, the GeoGebra group achieved $M = 72.85$ ($SD = 7.90$), compared with $M = 61.20$ ($SD = 8.10$) for the conventional group. The source manuscript reported $t(87) = 6.63$, $p < .001$. Recalculation from the rounded group summaries produced a closely aligned but slightly larger estimate, $t = 6.85$, and a large standardized between-group effect, Hedges' $g = 1.44$, with an estimated mean difference of 11.65 points (95% CI [8.27, 15.03]). The findings support GeoGebra as a promising instructional support for the selected mathematics topics, while the sample-accounting discrepancy, unclear score scaling, absence of participant-level data, and limited treatment description constrain causal and generalizable claims.

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1. Introduction

Mathematics education has a central role in secondary schooling because it supports quantitative reasoning, problem solving, scientific modelling, and progression into many science, technology, engineering, and mathematics pathways. Yet the instructional challenge is not simply to increase the amount of mathematical content presented to students. Effective instruction must help learners connect procedures with concepts, move between representations, test conjectures, and explain why a mathematical relationship holds. These demands are particularly important in algebraic topics such as linear and quadratic equations and in sequences and series, where symbols can become detached from meaning when instruction is reduced to rule following. The source manuscript identifies this concern in the Nigerian secondary-school context and argues that technology-supported instruction may provide a practical route for making mathematical relationships more visible and manipulable.

Digital technology does not automatically improve mathematics learning. Its contribution depends on the representational opportunities it provides, the structure of the mathematical tasks, and the way teachers organize interaction around those tasks. GeoGebra is especially relevant because it connects algebra, geometry, graphing, tables, statistics, and calculus within a dynamic environment. The official platform describes GeoGebra as dynamic mathematics software in which multiple mathematical representations can be manipulated and linked. In a learning setting, this means that a student can alter a parameter and immediately observe how related symbolic and graphical objects change. The instructional value lies in the coordination of representations and the possibility of repeated exploration, not in the software as an isolated technological object.

A substantial empirical literature has examined GeoGebra-supported learning. Juandi et al. (2021) synthesized 36 effect sizes from 29 primary studies and reported a positive overall effect of GeoGebra-supported instruction on mathematical abilities. A later meta-analysis by Zhang et al. (2025) synthesized two decades of research and again found a positive effect on mathematics achievement, while also showing that topic, duration, and sample size can moderate outcomes. These results are important because they move the argument beyond isolated classroom anecdotes. At the same time, meta-analytic evidence does not imply that every GeoGebra intervention will succeed. The observed effects are produced within specific combinations of tasks, teacher guidance, learner readiness, access to devices, and content domains.

Individual studies illustrate these contextual differences. Zulnaidi et al. (2020) reported higher achievement among high-school students who learned functions and limits with GeoGebra than among students in a comparison condition. Birgin and Acar (2022) found that computer-supported collaborative learning with GeoGebra enhanced eleventh-grade students' achievement in exponential and logarithmic functions. Birgin and Topuz (2021) likewise reported gains in geometry achievement and retention within a GeoGebra-supported collaborative environment. Earlier work in Malaysia also showed improved achievement and positive learner perceptions when GeoGebra was used in mathematics instruction (Arbain & Shukor, 2015). These studies differ in age group, mathematical content, and instructional organization, but they converge on the importance of active interaction with mathematical representations.

Recent studies have extended the focus from test achievement to broader features of mathematical activity. Romero Albaladejo and García López (2024) showed that GeoGebra-supported work could foster perseverance, precision, and autonomy when students engaged with carefully designed secondary-school tasks. Chytas et al. (2024) reported that secondary students could use GeoGebra in calculus activities that integrated computational thinking, provided that appropriate scaffolding and prerequisite mathematical knowledge were available. Birgin and Uzun Yazıcı (2026) found that GeoGebra-supported instruction improved cognitive, affective, and social engagement while reducing mathematics anxiety among eighth-grade students. These findings suggest plausible pathways through which dynamic mathematics environments may influence achievement: students can inspect variation, receive rapid visual feedback, discuss emerging patterns, revisit failed attempts, and connect an algebraic expression with its graphical consequences.

Evidence from African settings is also relevant because infrastructure and teacher preparation can shape implementation. Baye et al. (2021) found that GeoGebra integrated with multiple teaching approaches supported university students' conceptual understanding of limits in Ethiopia. Batiibwe (2024), studying secondary mathematics classrooms in Uganda, emphasized that the usefulness of interactive software depends on teacher capability and institutional support. This point matters for the Nigerian context. A digital platform can reduce some representational barriers, but it can also introduce new constraints if teachers have insufficient preparation, computer laboratories are unavailable, power supply is unstable, or class time is consumed by technical troubleshooting. Technology should therefore be examined as part of an instructional system rather than as a stand-alone treatment.

The theoretical rationale for GeoGebra-supported instruction is consistent with constructivist and representation-oriented views of mathematics learning. Learners develop stronger mathematical understanding when they actively relate new information to prior knowledge and when they can test and revise their conceptions through meaningful activity. Dynamic visualization can support this process by making covariation visible. For example, when parameters in an equation change and the graph updates immediately, learners can compare cases and formulate generalizations. The software can also function as a feedback environment because an unexpected graph or construction signals a mismatch between the learner's intended mathematical relationship and the representation produced. This does not replace mathematical reasoning. Instead, it can create occasions for reasoning, provided the teacher asks students to explain the pattern rather than merely observe it.

The distinction between visualization and verification is crucial. A graph generated by software can support conjecture and conceptual interpretation, but visual agreement is not a substitute for mathematical proof or symbolic justification. Effective use therefore requires deliberate transitions among exploration, explanation, and formalization. Studies of technology-rich mathematics teaching repeatedly show that teacher orchestration remains central. Dynamic tools are most productive when students have a clear mathematical question, when the teacher directs attention to invariant and varying features, and when classroom discussion converts observations into explicit mathematical language (Romero Albaladejo & García López, 2024; Chytas et al., 2024).

The source manuscript positions the present study within this problem space. It focuses on Senior Secondary School One science students in public secondary schools in Ekiti State and on the topics of linear and quadratic equations and sequences and series. These topics are suitable for a dynamic approach because they involve relationships that can be represented symbolically, numerically, and graphically. The study compares a GeoGebra-supported group with a conventional instruction group in a pre-test-post-test quasi-experimental design. Such a design can provide useful evidence when random assignment is not feasible in school settings, but interpretation requires careful attention to baseline equivalence, intact-class effects, sample accounting, and the fidelity of treatment implementation.

The original manuscript contains several reporting issues that must be addressed before the study can be interpreted responsibly. First, the sampling section states that 128 students were included, while the results tables report 41 students in the GeoGebra group and 48 in the conventional group, for an analyzed total of 89. The source does not explain the difference of 39 students. Second, the Mathematics Performance Test is described as containing 24 multiple-choice items, yet group means are reported on a scale above 24. A score transformation may have been applied, but the transformation rule is not stated. Third, the treatment section identifies a four-week GeoGebra intervention but does not provide lesson-by-lesson activities, exposure time per lesson, or fidelity indicators. These gaps do not invalidate the reported results, but they limit the strength and reproducibility of the claims. The revision therefore separates what the source directly reports from what can be statistically reconstructed from the available summaries.

Against this background, the study addresses one research question: What is the performance of students in mathematics before and after exposure to GeoGebra-supported instruction or the conventional method? Two null hypotheses are evaluated: (H01) there is no significant difference between the experimental and control groups at pre-test; and (H02) there is no significant difference between students exposed to the GeoGebra platform and those exposed to the conventional method at post-test. The revised analysis also reports standardized effect size and confidence intervals calculated from the published summary statistics. These additional statistics do not replace the original analysis. They clarify the magnitude and precision of the reported group difference and support a more informative interpretation than statistical significance alone.

2. Methods

2.1 Research design. The study used a quasi-experimental pre-test-post-test control-group design. This design is appropriate when researchers compare an intervention group with a non-equivalent comparison group in naturally organized educational settings where individual randomization may be impractical. Both groups completed a baseline assessment before the instructional period and a post-test after treatment. The GeoGebra group received mathematics instruction supported by the GeoGebra platform, while the comparison group received the conventional method described in the source manuscript. Because students were sampled from intact classes and the source does not report individual random assignment, the design should be interpreted as quasi-experimental rather than as a randomized controlled trial. Baseline comparison is therefore an important diagnostic step, but similar pre-test means cannot eliminate all possible confounding.

2.2 Setting and population. The source manuscript locates the study in public secondary schools in Ekiti State, Nigeria. It identifies Senior Secondary School One science students as the target population and states that 206 public secondary schools served 6,868 SS I science students in 2025. The underlying administrative dataset was not supplied with the manuscript, so these population figures are retained as author-reported contextual information rather than independently verified statistics. SS I students were selected because they were not yet under the same external-examination pressure as students preparing for final senior secondary examinations, which was expected to make sustained participation in the intervention more feasible.

2.3 Sampling procedure and analytic sample. The sampling plan describes a multistage procedure. A senatorial district was selected from the three districts in the state using simple random sampling, followed by selection of one local government area by balloting. Three public secondary schools were then selected using stated eligibility criteria, including co-educational status, the presence of a regular mathematics teacher, and access to a computer laboratory. The source manuscript states that the study sample consisted of 128 SS I science students in intact classes. However, the results tables contain 41 students in the GeoGebra condition and 48 in the conventional condition, yielding N = 89. No attrition table, exclusion rule, absentee record, or participant-level dataset was provided to explain the 39-case difference. To avoid fabrication, the revised results use N = 89, because those are the cases represented in the inferential tables, while the discrepancy is reported explicitly as a limitation.

2.4 Mathematics content and intervention period. The study was delimited to selected SS I mathematics concepts: linear equations, quadratic equations, sequences, and series. The experimental procedure is described as three stages. The pre-treatment stage lasted one week and included institutional access, briefing of school personnel and research assistants, and administration of the Mathematics Performance Test. The treatment stage lasted four weeks. During this period, the experimental group was taught with support from the GeoGebra platform, whereas the control group continued with the existing conventional method. The post-treatment stage lasted one week and included administration of a rearranged version of the same test. The total study cycle described in the source therefore covered approximately six weeks.

The source manuscript does not provide a sufficiently detailed intervention manual to reconstruct the exact GeoGebra activities. It states that the teacher used the platform to present concepts, explain mathematical relationships, guide students, and support problem solving. It does not identify the exact GeoGebra applets, whether students operated individual devices or worked in pairs, the number and duration of lessons each week, the balance between teacher demonstration and student manipulation, or the prompts used to connect dynamic representations with formal mathematical reasoning. These details are important because GeoGebra effects are sensitive to instructional design. Accordingly, this article does not invent lesson content that was absent from the source. The intervention is described at the level that the available manuscript supports.

2.5 Instrument. Academic performance was measured with a researcher-designed Mathematics Performance Test (MPT). The source describes two sections. Section A collected student background information, while Section B contained 24 multiple-choice items with response options. Items were drawn from past Senior Secondary School Certificate Examination material and aligned with the SS I curriculum topics addressed during the study. The same instrument was used at pre-test and post-test, with item order rearranged at post-test to reduce simple recall of the initial sequence. A methodological ambiguity remains because the reported group means range from approximately 52 to 73, which exceeds the stated maximum raw score of 24. This indicates that scores were likely transformed, scaled, or reported as percentages, but the manuscript does not state the conversion rule. The revised paper therefore refers to 'reported performance scores' rather than asserting a specific scale.

2.6 Validity and reliability. The MPT was subjected to face and content review by specialists in tests and measurement, curriculum and instruction, and mathematics education in the Faculty of Education at Ekiti State University, according to the source manuscript. The instrument was then administered to 10 students outside the main sampled coverage. The same instrument was re-administered after a two-week interval, and the two administrations were correlated using Pearson's product-moment correlation. The reported test-retest reliability coefficient was 0.91. This value indicates strong temporal stability for the pilot sample, although the very small pilot size means that the coefficient itself is imprecise. The source does not report item difficulty, discrimination indices, internal consistency, or a test specification table, so these properties cannot be evaluated from the available file.

2.7 Data collection and analysis. The source manuscript used means and standard deviations to answer the research question and independent-samples t tests to evaluate the two hypotheses at the .05 significance level. This revised report preserves those analyses and adds effect-size and confidence-interval calculations based only on the reported group sizes, means, and standard deviations. For pre-test and post-test comparisons, the pooled standard deviation was used to calculate Cohen's d, followed by the small-sample correction for Hedges' g. Approximate 95% confidence intervals for mean differences were calculated from the standard error derived from the two group standard deviations and sample sizes. These reconstructed statistics are clearly labelled because participant-level data were not available.

An important analytic distinction is made between the t statistic printed in the source and the t statistic recomputed from rounded summary data. At pre-test, the reconstruction produces t approximately 0.17, matching the reported value. At post-test, the source reports $t(87) = 6.63$, whereas calculation from the displayed means, standard deviations, and sample sizes produces t approximately 6.85. This small discrepancy may reflect rounding of displayed group summaries or calculation from unrounded raw data. Because the participant-level dataset was not provided, the original t statistic cannot be independently reproduced exactly. The revised article therefore reports the source result as primary and the reconstructed estimate as a sensitivity check, rather than replacing one with the other.

2.8 Research integrity and reporting decisions. No missing values, substitutions, subgroup results, or participant characteristics were invented during revision. The paper does not infer the sex distribution of the students, socioeconomic status, prior technology experience, or school-level assignment because these variables are not reported. Likewise, the revision does not claim that the intervention used particular GeoGebra features unless the source supports that statement. Discussion of sliders, dynamic graphs, linked representations, or immediate feedback refers to established affordances of GeoGebra in the literature and is used to interpret plausible mechanisms, not to assert that every feature was implemented in these classrooms.

2.9 Ethical information available in the source. The experimental procedure states that the researcher visited participating schools and obtained the principals' approval to use the schools, teachers, and students for the study. It also states that the study purpose and the need for cooperation were explained. However, the source file does not identify an institutional research ethics committee, approval number, date of ethical clearance, parental consent procedure, student assent procedure, data-protection protocol, or compensation arrangement. These omissions must be resolved by the author before formal publication if institutional or journal policy requires them. The Authors and Ethics Statement at the end of this manuscript therefore distinguishes documented permissions from information that remains to be confirmed.

Table 1. Source-grounded study design and reporting profile

Component	Source manuscript report	Revision treatment	Editorial implication
Design	Quasi-experimental pre-test-post-test control group	Preserved	Causal claims remain qualified because allocation was not reported as individual randomization.
Planned/reported sample	128 SS I science students	Preserved as reported	Does not match N = 89 in outcome tables.
Analyzed sample	41 GeoGebra; 48 conventional; total 89	Used for all reconstructed statistics	39-case difference requires author clarification.
Treatment duration	Four weeks, plus one-week pre- and post-treatment stages	Preserved	Exact lesson count and exposure time are not supplied.
Content	Linear and quadratic equations; sequences and series	Preserved	No additional topics were invented.
Instrument	24-item Mathematics Performance Test	Preserved	Reported means exceed 24, so score scaling must be clarified.

Component	Source manuscript report	Revision treatment	Editorial implication
Reliability	Test-retest r = .91 using 10 pilot students	Preserved	Strong reported stability but small pilot sample.
Primary analysis	Means, SDs, independent t tests at alpha = .05	Preserved and supplemented	Effect size and CI reconstructed from displayed summaries only.
Ethics information	Principal approval and school cooperation described	Reported transparently	No committee name, approval number, consent, or assent details supplied.

Source: Editorial synthesis of the supplied Oniya Paper 3 manuscript; no missing study details were invented.

3. Results

3.1 Participant accounting. The first result that must be reported is the difference between the stated sample and the analyzable sample. The methods section states that 128 SS I science students were sampled. The outcome tables, however, report 41 students in the GeoGebra group and 48 in the conventional group, for a total of 89 analyzed cases. The difference is 39 students, equivalent to 30.5% of the stated initial sample. The source manuscript does not say whether these students were absent at one testing occasion, excluded because of incomplete data, assigned to another condition, or lost for another reason. No conclusion should therefore be drawn about attrition mechanisms. The inferential results below are restricted to the 89 cases explicitly represented in the source tables.

3.2 Descriptive performance before and after treatment. The two groups had almost identical mean scores before the intervention. The GeoGebra group recorded a pre-test mean of 52.10 (SD = 8.45), whereas the conventional group recorded a mean of 51.80 (SD = 7.95). The raw mean difference was only 0.30 points. At post-test, both groups improved, but the improvement was larger in the GeoGebra group. The experimental mean increased to 72.85 (SD = 7.90), a reported pre-post gain of 20.75 points. The conventional group increased to 61.20 (SD = 8.10), a gain of 9.40 points. The difference between the two simple gain values was 11.35 points. Because the source does not provide standard deviations for individual gain scores or the pre-post correlation, no inferential test of gain scores is reconstructed here.

Figure 1 visualizes the reported group means at the two measurement occasions. The lines are based only on the four means in the source manuscript. No intermediate time points have been estimated. The graph shows the near-overlap at baseline and the wider separation after the four-week treatment period. This visual pattern is consistent with the inferential results but should not be interpreted as a longitudinal trajectory beyond the two observed testing occasions.

3.3 Baseline equivalence. The source reports an independent-samples t test comparing the pre-test scores of the GeoGebra and conventional groups. The reported result was $t(87) = 0.17$, $p = .8654$, indicating no statistically significant mean difference at baseline. Reconstruction from the displayed group summaries yields $t = 0.172$ and an estimated mean difference of 0.30 points with an approximate 95% confidence interval from -3.18 to 3.78. The standardized pre-test difference was negligible, Hedges' $g = 0.04$. These statistics indicate that the measured baseline performance of the two analyzed groups was similar. Nevertheless, baseline equivalence on one achievement measure does not establish equivalence on unmeasured learner, teacher, or school characteristics.

3.4 Post-test group difference. The source manuscript reports that the post-test difference was statistically significant, $t(87) = 6.63$, $p < .001$. The GeoGebra group mean of 72.85 exceeded the conventional group mean of 61.20 by 11.65 points. When the t statistic is independently reconstructed from the rounded summary values, the result is $t = 6.85$. The difference between the printed and reconstructed statistics is modest and does not alter the conclusion at conventional significance levels. The approximate 95% confidence interval for the mean difference, using the displayed summaries, is 8.27 to 15.03 points. Thus, the available summary data are consistent with a substantial post-test advantage for the GeoGebra-supported group.

3.5 Effect magnitude. The pooled post-test standard deviation calculated from the reported group summaries is approximately 8.01. Dividing the 11.65-point mean difference by this pooled variability yields Cohen's d = 1.45. After small-sample correction, Hedges' g is approximately 1.44. By conventional standardized-effect benchmarks, this is a large between-group difference. Effect-size labels should not be treated as universal measures of educational importance, especially when the score scale and minimal educationally important difference are not specified. However, the effect is large relative to the observed within-group dispersion and is substantially greater than the negligible pre-test standardized difference.

3.6 Summary of hypothesis tests. H01 was not rejected because the analyzed groups did not differ significantly at pre-test. H02 was rejected because the GeoGebra-supported group achieved a significantly higher post-test mean than the conventional group. The pattern therefore supports an association between exposure to the GeoGebra-supported instructional condition and higher post-intervention mathematics performance among the analyzed students. Because the design used intact classes and incomplete reporting prevents evaluation of school-level clustering, treatment fidelity, and unexplained case loss, the result should be described as strong quasi-experimental evidence within the study sample rather than definitive proof that GeoGebra alone caused the difference.

Table 2. Reported descriptive statistics for mathematics performance

Group	N	Pre-test M	Pre-test SD	Post-test M	Post-test SD	Simple mean gain
GeoGebra platform	41	52.10	8.45	72.85	7.90	20.75
Conventional method	48	51.80	7.95	61.20	8.10	9.40

Source: Values transcribed from Table 1 of the supplied manuscript.

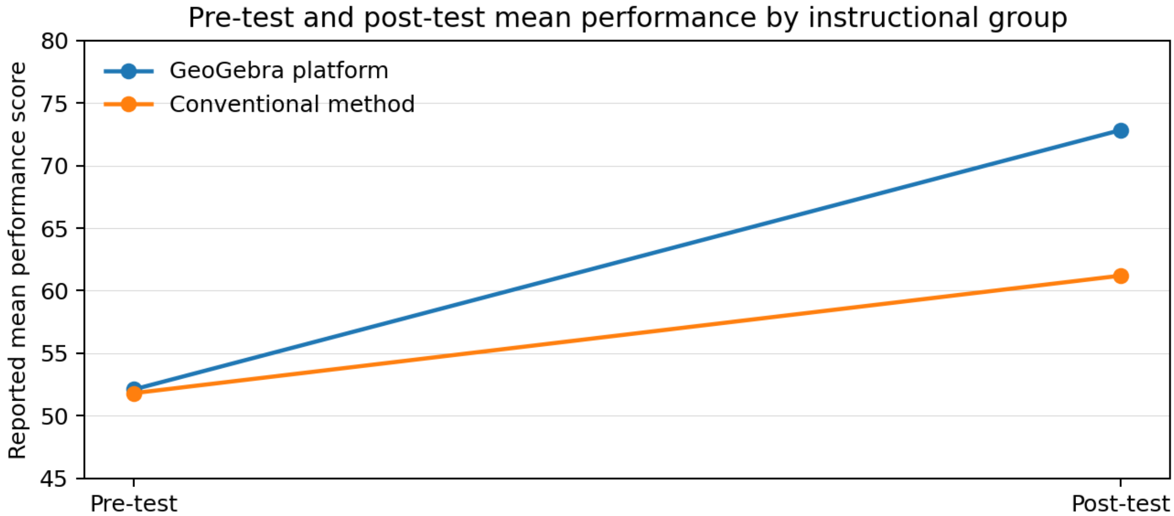

Figure 1. Pre-test and post-test mean performance by instructional group.

Source: Author-reported group means. The figure contains only the two observed testing occasions.

Table 3. Inferential results and reconstructed effect-size estimates

Comparison	Mean diff.	df	Source t	Source p	Recomputed t	95% CI for diff.	Cohen d	Hedges g
Pre-test GeoGebra vs control	0.30	87	0.17	.8654	0.17	[-3.18, 3.78]	0.04	0.04
Post-test GeoGebra vs control	11.65	87	6.63	< .001	6.85	[8.27, 15.03]	1.45	1.44

Note: Source t and p values are reproduced from the manuscript. Recomputed statistics use the displayed rounded N, means, and SDs; small differences may reflect rounding or use of unrounded raw data.

Table 4. Participant and measurement reporting issues requiring author confirmation

Issue	What is reported	What is missing	Required clarification before final publication
Sample accounting	128 sampled; 89 represented in result tables	Reason for 39-case difference	Provide participant-flow counts and exclusion/missing-data rules.
MPT score scale	24 multiple-choice items; means > 50	Score conversion formula	State raw maximum, transformation, and interpretation of reported points.
Treatment fidelity	Four-week GeoGebra treatment	Lesson frequency, duration, applets, device ratio, fidelity checks	Provide intervention schedule or supplementary protocol.
Assignment structure	Experimental and conventional groups	Whether allocation occurred by student, class, or school	State assignment unit and whether the same teachers taught both groups.
Ethics	Principal approval described	Committee approval, consent/assent, confidentiality procedure	Insert verified ethics and consent information or an explicit institutional exemption if applicable.

Source: Reporting audit of the supplied manuscript.

4. Discussion

The principal finding is that students in the GeoGebra-supported condition achieved substantially higher mathematics performance after the intervention than students taught with the conventional method, while the two analyzed groups had similar baseline means. The source manuscript's post-test test statistic is highly significant, and the reconstructed standardized effect is large. This pattern is consistent with the broad direction of prior GeoGebra research. Juandi et al. (2021) reported a positive overall effect across a large body of GeoGebra studies, while Zhang et al. (2025) found a medium-to-large pooled effect across two decades of research. The present effect estimate is larger than those meta-analytic averages, which makes the finding potentially important but also calls for careful examination of design features, score scaling, and sample accounting before generalizing it beyond the study context.

One plausible explanation is the representational affordance of dynamic mathematics software. Algebraic learning often requires students to coordinate equations, numerical patterns, and graphs. Conventional instruction can present these representations sequentially, leaving students to infer the relationships among them. In a dynamic environment, a change in one representation can be connected immediately with changes in another. This can reduce the delay between action and consequence and create opportunities for students to compare cases. Zulnaidi et al. (2020) similarly reported advantages when GeoGebra was used to teach functions and limit functions, where graphical and symbolic relationships are central. Baye et al. (2021) also emphasized visualization when explaining improved conceptual understanding of limits.

A second mechanism concerns active mathematical exploration. The educational value of GeoGebra is not that it produces correct graphs faster than hand calculation. Its stronger contribution is that it can support controlled variation. Students can alter values, inspect resulting patterns, and test whether a conjecture remains true across cases. Such activity can shift classroom work from receiving completed procedures toward investigating mathematical structure. Romero Albaladejo and García López (2024) found that GeoGebra-supported secondary-school work was associated with stronger perseverance, precision, and autonomy. Chytas et al. (2024) showed that carefully scaffolded GeoGebra activities can place students in an active computational and mathematical role. If the present intervention gave students genuine opportunities to manipulate and reason with representations, those processes could help explain the achievement difference.

A third mechanism is feedback. In paper-and-pencil mathematics, an error can remain hidden until a teacher checks the work. Dynamic software can expose inconsistencies more rapidly. A graph that does not match an expected form, a point that moves unexpectedly, or a sequence that fails to follow the predicted pattern can prompt revision. This feedback is useful only when students interpret it mathematically. The teacher therefore remains essential. Batiibwe (2024) stresses that successful classroom use of interactive software depends on teacher capability and support. The same point appears in recent professional-development literature, where teachers require both technical knowledge and pedagogical strategies for using dynamic visualizations effectively. Technology can make feedback available, but teachers and tasks determine whether students transform that feedback into conceptual learning.

The present study's four-week treatment period is also noteworthy. Juandi et al. (2021) found that intervention duration moderated effect sizes in their synthesis, and Zhang et al. (2025) likewise reported moderation by treatment duration and sample size. A relatively short, focused intervention may produce a strong contrast when the digital activities are tightly aligned with the assessed content. The selected topics in the source study, including equations and sequences, are amenable to visual and pattern-based exploration. However, a short treatment also raises questions about durability. The study did not report a delayed post-test. Consequently, the data show an immediate post-intervention difference but do not establish retention weeks or months later. Birgin and Topuz (2021), by including delayed measurement, provide a useful model for future Nigerian studies that seek to determine whether GeoGebra-supported learning persists after the intervention ends.

The finding also fits research that links GeoGebra with learner engagement. Birgin and Uzun Yazıcı (2026) reported gains in cognitive, affective, and social engagement and reductions in mathematics anxiety. These outcomes are not measured in the present study, so they cannot be claimed as mediators. They do, however, offer plausible hypotheses for future work. Students who can interact with visual representations, compare solutions with peers, and revisit mathematical objects may be more willing to persist. At the same time, digital environments can create frustration when students lack software familiarity or when device access is constrained. Future studies should therefore measure engagement, mathematics anxiety, technology self-efficacy, and time-on-task alongside achievement so that mechanisms can be evaluated rather than inferred.

The baseline result strengthens the interpretation of the post-test difference to a limited extent. The analyzed groups began with nearly identical mean performance, and the standardized baseline difference was close to zero. In quasi-experimental research, this is preferable to beginning with a large measured imbalance. Still, equal pre-test means do not make intact groups equivalent in every respect. The schools may differ in teacher experience, peer climate, scheduling, computer access, or prior exposure to digital tools. The source does not specify whether the 41 and 48 analyzed students came from the same schools, whether treatment was assigned at the school or class level, or whether the same teacher taught both conditions. These design details are needed to judge the extent to which the instructional platform, rather than contextual differences, produced the observed gap.

The sample discrepancy is the most important reporting limitation. A study that describes 128 sampled students but analyzes 89 must account for the remaining 39. If the missing students were absent randomly, the effect on bias might be limited, though statistical power would decline. If loss differed by group or was related to prior achievement, technology access, attendance, or motivation, the post-test comparison could be biased. The source provides no information that allows these possibilities to be distinguished. The correct editorial response is not to infer a reason. It is to report the discrepancy, request a participant-flow explanation from the author, and base current calculations on the 89 cases that appear in the result tables. A final publication should ideally include a flow diagram or concise accounting table showing enrollment, allocation, pre-test completion, post-test completion, exclusions, and analyzed cases.

The score-scale ambiguity creates a second limitation. The MPT is described as a 24-item test, but reported means are between 51.80 and 72.85. These values could represent percentages or another transformed score, but the transformation is not described. Without that information, readers cannot translate the 11.65-point post-test difference back into raw items or evaluate the practical meaning of a one-point change. This also limits comparisons with other studies. Before publication, the author should specify the scoring procedure, including the raw score range, any multiplication or percentage conversion, treatment of omitted responses, and whether the same scale was used at both testing occasions. If scores were converted to percentages, this should be stated explicitly in the table headings and methods.

The inferential strategy can also be improved in future research. Independent-samples t tests at pre-test and post-test are understandable and correspond to the original study plan, but a stronger approach would adjust post-test performance for baseline performance using analysis of covariance or a regression model, provided assumptions are satisfied and the design structure is adequately represented. If treatment assignment occurs at class or school level, multilevel modelling or cluster-robust standard errors may be needed because students within the same class are not statistically independent. With participant-level data, researchers could also inspect score distributions, missingness, influential observations, and the correlation between pre-test and post-test scores. None of these analyses can be responsibly reconstructed from group summaries alone.

The large Hedges' g estimate should therefore be interpreted as descriptive evidence of the observed post-test separation rather than as a transportable causal parameter. Standardized effects can appear larger in small samples, tightly controlled interventions, or instruments closely aligned with the taught content. Zhang et al. (2025) found that sample size and treatment characteristics moderated GeoGebra effects across studies. The present analyzed sample is below 100 and the treatment focused on a defined set of topics over four weeks. These features may support a strong local effect. Replication across more schools, larger samples, and multiple mathematics topics would show whether the magnitude remains stable under ordinary classroom conditions.

For classroom practice, the findings support a cautious recommendation to integrate GeoGebra as a pedagogical resource rather than to replace mathematical teaching with software use. Teachers can use dynamic graphs and linked representations to prompt prediction, comparison, explanation, and generalization. A productive sequence might ask students to make a prediction before manipulating an object, observe what changes and what remains invariant, record the pattern, and then express the relationship symbolically. Such a sequence aligns the technology with mathematical reasoning. The teacher should also require students to justify conclusions because dynamic visual evidence can suggest a relationship without establishing a proof. This balance between exploration and formalization is essential for maintaining mathematical rigor.

For schools and education authorities, implementation requires more than installing software. GeoGebra is accessible as software and through online resources, but meaningful use still depends on device availability, reliable power, appropriate scheduling, and teacher preparation. In resource-constrained settings, pair work or projected whole-class demonstrations may provide partial access, but these arrangements create different learning opportunities than individual interaction. Professional development should therefore address both technical operation and didactic design. Teachers need to know how to create or adapt tasks, anticipate student misconceptions, manage the transition from screen observations to symbolic reasoning, and assess learning produced through dynamic exploration.

For curriculum planning, the study suggests that technology integration should be aligned with specific mathematical goals. Digital tools are most defensible when they make a representation, transformation, or pattern accessible in ways that support reasoning. The curriculum need not require GeoGebra for every topic. Instead, it can identify areas where dynamic visualization adds particular value, such as functions, equations, transformations, geometry, calculus, and sequences. This principle reduces the risk of using technology merely for novelty. It also allows teachers to combine paper-and-pencil methods, mental mathematics, discussion, and digital exploration in a coherent sequence.

For research, future Nigerian studies should prioritize transparent intervention documentation. A replicable protocol would state the exact number of lessons, lesson duration, teacher training, device-to-student ratio, GeoGebra files or links used, student tasks, homework requirements, and procedures for the control condition. Fidelity should be monitored so that researchers can distinguish an ineffective tool from weak implementation. Reporting should also include a participant flow, a clear scoring rule for the achievement test, full psychometric information, and an analysis plan that reflects the level at which treatment was assigned. These steps would make the evidence more useful for both local policy and international comparison.

The study nevertheless contributes a relevant piece of evidence. It examines a dynamic mathematics platform in a public-secondary-school context where conventional instruction remains common and where researchers are seeking affordable ways to strengthen mathematics learning. The direction of the finding is consistent with international research, and the observed post-test separation is large. The strongest editorial conclusion is therefore neither to dismiss the study because of reporting gaps nor to present it as definitive proof. Rather, the results justify further use and rigorous evaluation of GeoGebra-supported instruction, while the documented limitations define the reporting and design improvements required for stronger evidence.

A further implication concerns the distinction between technological access and pedagogical opportunity. Counting computers or confirming that software is installed does not show how students actually use the tool. Two classrooms can both be described as GeoGebra-supported while offering very different forms of learning. In one classroom, students may watch a teacher demonstrate completed graphs. In another, learners may generate cases, drag parameters, record observations, compare explanations, and revise conjectures. The second environment provides more opportunities for mathematical agency, but it also requires more planning and classroom management. Future evaluations in Ekiti State should therefore document the enacted pedagogy, not only the technology label. Observation schedules, teacher logs, screenshots of student work, and archived GeoGebra files could provide evidence of how the digital environment was used and whether implementation was consistent across lessons and schools.

The distinction also matters for equity. A technology intervention can improve average achievement while still producing uneven opportunities if some students have more prior experience with computers, more reliable access to devices, or stronger support outside school. The present source manuscript does not report prior digital experience, household access, disability status, or other learner characteristics that might affect participation. These variables should not be added retrospectively. They should instead inform the design of subsequent studies. Researchers can assess whether students begin with comparable confidence in using digital tools, whether interface difficulties consume instructional time, and whether pair or group arrangements distribute control of the device equitably. Such evidence would help determine whether GeoGebra narrows or reproduces existing differences in access to mathematically rich learning experiences.

Assessment alignment deserves similar attention. When an intervention emphasizes dynamic visualization and conceptual exploration, the achievement test should capture more than routine procedural recall. The source states that MPT items were drawn from past examination questions and aligned with the SS I curriculum, which supports curricular relevance. However, the manuscript does not provide an item blueprint showing the balance among procedural fluency, conceptual understanding, application, and reasoning. A highly treatment-aligned test can legitimately measure the intended learning outcomes, but it may also produce a larger effect than a broader standardized assessment. Future studies should publish a table of specifications and, where feasible, include both a curriculum-aligned researcher test and an external or delayed measure. This would clarify whether benefits extend beyond tasks closely resembling the treatment activities.

The role of the teacher is another priority for interpretation. If different teachers taught the experimental and comparison classes, teacher effects may be confounded with the platform effect. If the same teacher taught both groups, contamination may occur because instructional strategies learned while preparing the GeoGebra lessons could influence conventional lessons. The source does not provide enough information to determine which situation applied. A stronger design would document teacher assignment, teaching experience, prior GeoGebra competence, and training received for the intervention. Where several teachers participate, researchers can standardize core instructional objectives while allowing normal pedagogical variation, then measure fidelity. This approach would make the resulting evidence more representative of real implementation and would show whether the effect depends on one particularly skilled teacher.

The statistical interpretation should also move beyond a binary significant or non-significant framework. The post-test result is clearly inconsistent with a null hypothesis of equal means under the assumptions of the reported t test, but the practical meaning of the difference depends on the score scale and educational context. Confidence intervals are useful because they show a range of values compatible with the rounded summaries. The estimated interval of about 8.27 to 15.03 points indicates that even the lower bound represents a noticeable separation on the reported scale. Yet without the score transformation and without an externally defined proficiency threshold, the study cannot state how many additional curriculum objectives the average student mastered. Future reporting should connect score differences to interpretable benchmarks such as raw items, proficiency categories, or criterion-referenced learning objectives.

Finally, replication should be treated as part of evidence building rather than as a test of whether the original study was 'right' or 'wrong.' The international literature indicates positive average effects of GeoGebra, but it also shows heterogeneity. Replications across urban and rural schools, different senatorial districts, varied teacher experience levels, and additional mathematical topics would help identify the conditions under which the platform is most useful. A coordinated series of studies could use a common core achievement measure and intervention reporting framework while allowing local adaptation. Such a program would provide stronger evidence for curriculum or procurement decisions than a single experiment. It would also position Nigerian mathematics education research to contribute contextually rich evidence to the broader international literature on dynamic mathematics technologies.

5. Conclusion and Recommendations

This quasi-experimental study compared GeoGebra-supported mathematics instruction with a conventional method among SS I science students in Ekiti State, Nigeria. Based on the 89 students represented in the results tables, the two groups had nearly equivalent pre-test means. After the intervention, the GeoGebra group achieved a substantially higher mean than the conventional group. The source reported $t(87) = 6.63$, $p < .001$, and reconstruction from the displayed summaries produced a large standardized post-test difference, Hedges' g approximately 1.44. The evidence therefore supports GeoGebra-supported instruction as a promising approach for the mathematics topics addressed in the study.

The conclusion must be read alongside the study's reporting constraints. The manuscript states that 128 students were sampled, but the outcome tables contain 89 analyzed cases and do not explain the 39-case difference. The MPT is described as a 24-item instrument, but the scoring transformation that produced means above 24 is not reported. The intervention is described at a general level without sufficient detail to reproduce the exact classroom activities. In addition, no participant-level dataset was provided for independent checking of assumptions, missingness, score distributions, or adjusted analyses.

These issues limit generalizability and prevent stronger causal claims.

Several recommendations follow. Mathematics teachers should consider using GeoGebra where dynamic representation can deepen understanding, especially when students are asked to predict, manipulate, explain, and formalize mathematical relationships. Schools should support teachers with access to functioning devices, electricity, technical assistance, and sustained professional development. Teacher preparation should combine software skills with pedagogical knowledge so that GeoGebra activities remain focused on mathematical reasoning. Curriculum planners can identify topics where linked graphical, algebraic, and numerical representations are educationally valuable rather than requiring technology use uniformly across all content.

Future studies should use larger and more diverse samples, document participant flow from enrollment to analysis, and report the exact scoring scale for achievement measures. Researchers should retain participant-level pre-test and post-test data and analyze post-test outcomes with appropriate baseline adjustment. When treatment is assigned by class or school, the analysis should account for clustering. A delayed post-test should be added to evaluate retention. Studies should also measure engagement, mathematics anxiety, technology familiarity, and treatment fidelity so that the mechanisms behind achievement differences can be tested. Finally, all intervention materials, including GeoGebra files and teacher guides, should be archived or made available when possible to strengthen transparency and replication.

6. Authors and Ethics Statement

Authorship and contributions. The metadata of the supplied manuscript identifies Oniya Toluwani (Dr.) as the author. No additional author list or contributorship statement was provided in the source file. For publication, the submitting author should confirm the final authorship order, institutional affiliation, email address, and ORCID information, and should ensure that every listed author meets the journal's authorship criteria. On the basis of the available file, the author is responsible for the study conception, field coordination, instrument development, data collection, analysis reported in the original manuscript, and preparation of the manuscript. The present editorial revision reorganizes and clarifies the reporting but does not create new participant data.

Ethics and permissions. The source manuscript states that the researcher visited participating schools, sought the principals' approval, explained the focus and importance of the study, and requested cooperation from principals, teachers, and students before the experiment. The file does not report the name of an institutional ethics committee, an ethics approval number, the date of approval, procedures for parental or guardian consent, student assent, or specific confidentiality and data-protection procedures. These items should be supplied by the author before publication where required by institutional and journal policy. In the absence of documentary information, this article does not claim an ethics approval that is not evidenced in the source.

Conflict of interest and funding. No conflict-of-interest declaration or external funding information was supplied in the source manuscript. The author should confirm whether any financial, institutional, commercial, or personal interests could reasonably be perceived to influence the work. If no such interests exist, the final publication may state that the author declares no competing interests. Similarly, funding should be identified by funder name and grant number when applicable; otherwise, the author should confirm that the study received no specific external funding. Data availability should also be stated accurately. Participant-level data were not supplied with the manuscript used for this revision, so no public availability claim is made here.

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