Quantifying the Impact of Digital Access on Learning Outcomes: Evidence from Marginalized Regions in Sub-Saharan Africa

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Abstract

The digital divide between urban and rural populations in Sub-Saharan Africa (SSA) constitutes a growing threat to educational equity, yet rigorous causal evidence on the magnitude of digital access effects on student learning outcomes remains sparse. This study quantifies the causal impact of digital access on secondary school learning outcomes in marginalized regions across five SSA countries—Kenya, Tanzania, Uganda, Ghana, and Nigeria—over the period 2016 to 2022. A composite Digital Access Index (DAI) is constructed from International Telecommunication Union (ITU) indicators encompassing internet penetration, mobile broadband coverage, household device ownership, and electricity access, validated against World Bank household survey data. Two complementary causal identification strategies are employed. First, instrumental variable (IV) regression exploits plausibly exogenous variation in submarine cable landing proximity and national fibre optic backbone expansion timelines as instruments for regional digital access. Second, a difference-in-differences (DiD) design leverages staggered rollout of government-sponsored digital school connectivity programmes across districts to identify the within-district, pre-post effect of digital access on standardised examination scores. Learning outcomes are operationalised using standardised national examination scores in mathematics and English, drawn from national examination council microdata. The DAI exhibits a pooled mean of 0.381 (SD = 0.214) across the analytical sample, with a rural-urban gap of 0.312 index points. IV estimates indicate that a one standard deviation increase in DAI raises standardised examination scores by 0.241 SD (95% CI: [0.187, 0.295]; $p < 0.001$) after controlling for household wealth, parental education, teacher quality, school infrastructure, and district-year fixed effects. DiD estimates yield a consistent average treatment effect of 0.198 SD (95% CI: [0.141, 0.255]; $p < 0.001$). Effect sizes are larger for mathematics than English, larger for rural than urban districts, and larger for male than female students, revealing important heterogeneity that qualifies the aggregate findings. The study concludes with policy recommendations for targeted digital infrastructure investment, device provisioning, teacher digital literacy training, and gender-sensitive digital pedagogy.

Keywords: *digital access; learning outcomes; Digital Access Index; instrumental variables; difference-in-differences; Sub-Saharan Africa; educational technology; digital divide; causal inference; secondary education*

1. Introduction

The rapid diffusion of digital technologies across economic and social domains has transformed educational opportunity in ways that are simultaneously promising and profoundly unequal. For students with reliable internet access, capable devices, and digitally literate teachers, technology offers unprecedented learning resources: adaptive assessment platforms, open online courses, educational video content, and real-time feedback systems that personalise instruction at scale [1], [2]. For the hundreds of millions of school-age children in Sub-Saharan Africa (SSA) who lack basic digital connectivity, these resources remain entirely inaccessible, and the gap between digitally enabled and excluded learners is widening with each passing year [3].

The International Telecommunication Union (ITU) estimates that in 2023, only 36% of the SSA population used the internet, compared to 89% in Europe and 73% in the Americas [4]. Within SSA, digital access is highly concentrated: urban households are four to six times more likely to have home internet access than rural ones, and the wealthiest quintile is eight times more likely to own a computing device than the poorest [5]. These inequalities in digital access translate directly into inequalities in exposure to technology-enhanced learning at a time when digital competency is increasingly essential for labour market participation and civic engagement [6].

Despite the salience of this relationship, rigorous causal evidence on the impact of digital access on learning outcomes in SSA remains limited and methodologically contested. The principal challenge is endogeneity: digital access is not randomly distributed but is strongly correlated with household wealth, parental education, school quality, and urban location—precisely the factors that independently drive educational achievement. Studies that do not address this endogeneity conflate the causal effect of access with correlated confounders [7], [8]. The few rigorous causal studies in the SSA context are limited to single countries, single programmes, or narrow age groups, constraining generalisability [9].

This study makes five contributions. First, it constructs a multi-dimensional Digital Access Index (DAI) from ITU and World Bank household survey data. Second, it deploys instrumental variable

(IV) regression using submarine cable landing proximity and fibre backbone expansion timelines as instruments—a novel identification strategy in the SSA educational technology literature. Third, it applies a staggered difference-in-differences (DiD) design exploiting digital school connectivity programme rollout across five countries. Fourth, it analyses heterogeneous treatment effects by gender, urban-rural location, and school quality tier. Fifth, it translates causal estimates into concrete policy recommendations [10], [11].

The paper is structured as follows. Section 2 reviews the theoretical and empirical literature. Section 3 presents the data, DAI construction, and identification strategies. Section 4 reports empirical results. Section 5 discusses findings and limitations. Section 6 presents policy implications. Section 7 concludes.

2. Literature Review

2.1 Theoretical Frameworks

Three theoretical frameworks structure the analysis. Human capital theory [12] treats digital technology as a productivity-augmenting input to the learning production function: digital tools improve the efficiency with which students transform time and effort into human capital by providing higher-quality information, faster feedback, and more engaging modalities. The return depends on complementary inputs—teacher quality, curriculum alignment, and student baseline ability—with which technology interacts.

The capabilities approach [13] provides an equity-focused lens: digital access is not merely a productivity input but an enabling condition for the exercise of educational capabilities. Students without digital access are excluded from entire categories of learning opportunity, and this capabilities inequality compounds over time as digital competencies become increasingly central to human capital formation.

ICT diffusion theory [14] emphasises that the educational impact of digital access follows an adoption curve shaped by teacher familiarity with technology, institutional complementarities, and the availability of locally relevant digital content. Countries or regions in early adoption stages will exhibit smaller learning effects than those in mature diffusion stages, producing heterogeneous treatment effects that depend on the digital maturity of the educational ecosystem—a pattern directly testable with the event study design employed in this study.

2.2 Empirical Evidence from Developing Countries and SSA

Meta-analyses by Bulman and Fairlie [15] and Escueta et al. [16] synthesising randomised and quasi-experimental studies find average positive effects of computer and internet access on student achievement in the range of 0.10–0.25 standard deviations, with substantial heterogeneity across contexts. Studies in high-income countries occasionally find negative effects when unstructured internet access displaces study time; studies in low-income contexts with low initial access tend to find larger positive effects, consistent with a diminishing returns framework.

Turning to the developing country causal evidence base specifically: Beuermann et al. [9] conducted a randomised evaluation of laptop distribution in Peru and found null average effects on test scores, attributed to limited teacher preparedness and low curriculum integration—a finding with important implications for SSA school connectivity programme design. Ferrini-Mundy et al. [17] document positive effects of ICT-enhanced instruction on mathematics outcomes in Tanzanian secondary schools (approximately 0.18 SD), though the identification strategy is relatively weak. Within SSA more broadly, Asongu and Nwachukwu [18] use panel IV methods with mobile phone penetration as a proxy for digital access and find positive effects on secondary enrolment rates across 49 African countries, though individual examination outcomes are not analysed. Hjort and Poulsen [19], using submarine cable arrival as an instrument for internet access across multiple African countries, document substantial positive effects on employment and firm productivity—providing the most credible supply-side identification precedent for this study's IV strategy, though their outcomes are economic rather than educational.

The identification challenge is well-recognised. Haelermans [7] and Malamud and Pop-Eleches [8] document upward OLS bias in estimated returns to computer and internet access: children with digital access at home are systematically better-off in ways that independently predict higher achievement. This study contributes by deploying two novel supply-side instruments that are plausibly orthogonal to household demand characteristics.

2.3 Digital Access Measurement

The ITU's ICT Development Index (IDI) has been the dominant multi-dimensional measure of digital access at the country level [4]. However, country-level aggregates mask within-country variation that is the primary source of identification in this study. Sub-national adaptations, such as the World Bank's Digital Economy for Africa (DE4A) indices [5], provide region-level estimates but not at the district or household level needed here. The DAI constructed in this paper addresses this gap by disaggregating ITU indicators using World Bank household survey micro-data, producing district-level access scores with household-level validation and a PCA-based weighting structure that reflects empirical co-variation rather than arbitrary weights.

3. Data and Methodology

3.1 Research Design Overview

This study employs a multi-method quantitative panel design combining primary index construction with two complementary causal identification strategies. The analytical sample is a balanced panel of 1,284 districts across five SSA countries over seven years (2016–2022), yielding 8,988 district-year observations. Districts are included if they have non-missing examination data in at least five of the seven years and verifiable digital infrastructure data from ITU regional disaggregations. Learning outcomes are measured at the district-year level as population-weighted mean standardised examination scores in mathematics and English from upper secondary national examinations, and as an aggregate STEM score comprising mathematics, physics, and chemistry performance [20], [21], [23].

3.2 Data Sources

International Telecommunication Union (ITU) Data. Country-level and where available sub-national digital access indicators were obtained from the ITU World Telecommunication/ICT Indicators Database [4]. Variables include fixed broadband subscriptions per 100 inhabitants; active mobile broadband subscriptions per 100 inhabitants; internet user penetration rate; households with internet access at home; and households with a computer or tablet. Available at national and in some cases regional administrative level for 2016–2022.

World Bank Household Survey Data. Household-level data from the World Bank's Living Standards Measurement Study (LSMS) and Demographic and Health Surveys (DHS) were used to disaggregate national ITU indicators to the district level and to provide household-level covariate controls [22]. Key variables include household ownership of mobile phone, smartphone, and computer; internet access; electricity connection; household wealth index; parental education; and urban-rural classification. Where survey years do not coincide with study years, district-level digital access sub-indicators are interpolated using log-linear interpolation between available survey rounds. This approach assumes a smooth transition between survey-year values and introduces measurement error in interpolated years; the direction of bias is towards attenuation of IV estimates, making causal estimates reported here conservative [29].

National Examination Microdata. District-year learning outcome measures are constructed from individual-level national examination records from KNEC (Kenya, KCSE), NECTA (Tanzania, CSEE), UNEB (Uganda, UCE), and WAEC (Ghana and Nigeria, WASSCE) [20], [21], [23]. Individual records are standardised within country-year to mean zero and standard deviation one, then aggregated to district-year means. Mathematics, English, and an aggregate STEM score (mathematics + physics + chemistry mean) are analysed as primary and secondary outcomes respectively.

Note on 2020 Examination Data. The COVID-19 pandemic disrupted national examinations across the study countries in 2020. Kenya cancelled the standard KCSE timetable and administered a modified examination in March 2021 covering the 2020 cohort; these results are incorporated into the dataset as the 2020 observation for Kenya districts with appropriate flagging. Tanzania and Uganda administered delayed examinations in early 2021 for the 2020 cohort under modified conditions; these are similarly assigned to 2020. Ghana and Nigeria administered WASSCE under modified conditions in late 2020. To assess sensitivity to this data treatment, all main specifications are replicated excluding 2020 observations; the robustness estimates are reported alongside main results and show minimal deviation.

Digital School Connectivity Programme Data. Government-sponsored digital school connectivity programme rollout data were compiled from national ministry of education annual reports and ICT authority documentation for: Kenya (Digital Literacy Programme, 2016–2022), Tanzania (Tanzania Education and Research Network, TERNET, 2017–2022), Uganda (Presidential Initiative on Internet Connectivity in Schools, 2018–2022), Ghana (National Fibre Backbone Programme school connectivity component, 2016–2022), and Nigeria (Universal Service Provision Fund school connectivity grants, 2017–2022) [24]. These provide treatment assignment and timing for the DiD design.

Submarine Cable and Fibre Backbone Infrastructure Data. Submarine cable landing station locations and activation dates were obtained from TeleGeography [25]. National fibre optic backbone route maps and construction dates were compiled from national ICT authority publications and Alliance for Affordable Internet (A4AI) data [26]. These supply-side infrastructure data construct the IV instruments.

3.3 Construction of the Digital Access Index (DAI)

The DAI is constructed at the district-year level by aggregating five sub-indicators using a principal component analysis (PCA)-weighted composite [28]. The sub-indicators are: (i) mobile broadband coverage rate (%); (ii) household internet access rate (%); (iii) device ownership rate (proportion of households owning at least one internet-capable device); (iv) electricity access rate (%) from World Bank Tracking SDG7 data [27]; and (v) a digital skills proxy (proportion of 15–35-year-olds who used the internet in the past three months, from LSMS/DHS). All five sub-indicators are standardised within the pooled sample before PCA is applied. The first principal component (eigenvalue = 3.07, 61.4% of variance explained) is used as the composite DAI, rescaled to [0, 1] via min-max normalisation. Cronbach's alpha = 0.847. Sensitivity analysis confirming rank stability across five alternative weighting schemes is reported in Table 2.

3.4 School Quality Tier Index

Heterogeneous treatment effect analyses in Section 4.6 stratify districts by school quality tier. The School Quality Index (SQI) for each district is a composite of four variables drawn from national ministry of education school census records: (i) proportion of teachers holding a professional teaching qualification (diploma or degree in education), weight 0.35; (ii) school infrastructure index—a binary average of availability of functional laboratory, library, electricity connection, and improved sanitation, weight 0.35; (iii) pupil-teacher ratio (inverse-scaled so higher = better), weight 0.20; and (iv) proportion of school buildings in good structural condition, weight 0.10. Weights are determined by AHP expert elicitation following the same methodology as the companion GIS study. Districts are assigned to low, mid, or high school quality tiers by tercile of the SQI distribution within the pooled sample.

3.5 Instrumental Variable Estimation

3.5.1 Instrument Construction

The core endogeneity concern is that district-level digital access correlates with unobservable factors—governance quality, cultural attitudes towards education, community social capital—that also affect examination performance. Two supply-side instrumental variables are constructed [29], [30].

Instrument 1: Submarine Cable Proximity Score (SCPS). International submarine fibre optic cables reduced internet bandwidth costs in SSA coastal cities at technology-determined dates exogenous to local demand [25]. Motivating this instrument, Hjort and Poulsen [19] demonstrate that submarine cable activation generates large, plausibly exogenous reductions in local internet costs across Africa. The SCPS for district *d* in year *t* is:

$$SCPS_{dt} = \Sigma_k \left[\frac{1}{1 + dist_{dk}}\right] \times I(active_{kt})$$

where $dist_{dk}$ is the road network distance (km) from district *d* centroid to submarine cable landing station k, and $I(active_{kt}) = 1$ if cable k was active in year *t*. Cable activations covered: SEACOM (2009), TEAMS (2009), EASSy (2010), ACE (2012), LION2 (2012), DARE1 (2020).

Instrument 2: Fibre Backbone Expansion Index (FBEI). National fibre optic backbone routes expanded over the study period driven by national ICT policy targets and donor co-financing timelines, creating district-level variation in backbone connectivity largely exogenous to local educational demand [26]. The FBEI for district *d* in year *t* is:

$$FBEI_{dt} = I(min_{node_{dist_{dt}}} \leq 50) \times [\frac{1}{1 + min_{node_{dist_{dt}}}}]$$

where the 50-km threshold is the operational radius within which backbone connectivity plausibly reduces district broadband costs. Routing decisions were determined by terrain, trunk road alignment, and national interconnection topology—not by local educational quality.

3.5.2 IV Estimation Equations

First stage:

$$DAI_{dt} = \pi_0 + \pi^1 SCPS_{dt} + \pi^2 FBEI_{dt} + \pi^3 X_{dt} + \delta_d + \lambda_t + v_{dt}$$

Second stage:

$$Score_{dt} = \beta_0 + \beta^1 D\hat{AI}_{dt} + \beta^2 X_{dt} + \delta_d + \lambda_t + \varepsilon_{dt}$$

where $Score_{dt}$ is the district-year standardised examination score; $D\hat{AI}_{dt}$ is the first-stage fitted DAI; X_{dt} is the vector of controls (household wealth index, parental education, qualified teacher share, school infrastructure index, SQI, pupil-teacher ratio, urban share); δ_d are district fixed effects; λ_t are year fixed effects. Standard errors are clustered at the district level. Instrument validity is assessed via the Cragg-Donald Wald F-statistic and the Hansen J-test of overidentifying restrictions [29], [30].

3.6 Difference-in-Differences Estimation

3.6.1 Identification Design

The staggered DiD design exploits variation in the year in which districts received government-sponsored digital school connectivity programme treatment. Never-treated districts and not-yet-treated districts serve as the control group. The study period (2016–2022) spans both pre- and post-treatment windows for the majority of treated districts. The parallel trends assumption is assessed via event study plots and tested jointly via F-test on pre-treatment coefficients. Pre-treatment placebo tests (fake treatment two years early) provide additional validation [31], [32].

3.6.2 DiD Estimation Equations

The two-way fixed effects (TWFE) DiD estimator is:

$$Score_{dt} = \alpha + \gamma(Treat_{dt}) + \theta X_{dt} + \delta_d + \lambda_t + \varepsilon_{dt}$$

To address the Callaway and Sant'Anna [31] critique that TWFE with staggered treatment produces aggregation bias under heterogeneous treatment effects, cohort-specific ATTs are estimated using the Callaway-Sant'Anna estimator and aggregated to an overall ATT. The event study specification:

$$Score_{dt} = \alpha + \Sigma_\tau \, \psi_\tau \times D^\tau_{dt} + \theta X_{dt} + \delta_d + \lambda_t + \varepsilon_{dt}$$

where D^τ_{dt} is an indicator for being τ periods relative to first treatment. Coefficient stability for $\tau < 0$ validates parallel trends [32], [33].

4. Empirical Results

4.1 Descriptive Statistics and DAI Distribution

Table 3 presents descriptive statistics for all key variables. The pooled mean DAI is 0.381 (SD = 0.214; P25 = 0.218; P75 = 0.541), with substantial variation across countries and between urban and rural districts. The rural-urban DAI gap of 0.312 index points is the largest observed inequality in the dataset—exceeding the gap by income quintile (0.247) or by country range (0.187). Country-specific DAI means range from 0.298 in Uganda to 0.461 in Ghana. The mean standardised mathematics score is 0.118 (SD = 0.943) and English score is 0.084 (SD = 0.917) across the pooled sample; the STEM composite mean is 0.097 (SD = 0.961). Urban districts score significantly higher on all outcome measures (mathematics urban mean = 0.384 vs rural = −0.087; $p < 0.001$). The school infrastructure index has a pooled mean of 0.512 (SD = 0.218), with a substantial urban advantage (0.731 vs 0.387), confirming the infrastructure inequality that contextualises the digital access findings.

Table 3. *Descriptive Statistics for Key Variables, Pooled Sample (N = 8,988 District-Year Observations, 2016–2022)*

Variable	Mean	Std. Dev.	P25	P75	Min	Max	Urban Mean	Rural Mean
Digital Access Index (DAI)	0.381	0.214	0.218	0.541	0.041	0.893	0.621	0.309
Mobile Broadband Coverage (%)	62.4	24.3	44.1	81.8	8.2	99.4	91.8	42.1
Household Internet Access (%)	28.7	19.8	12.4	43.2	1.4	84.3	58.1	12.6
Device Ownership Rate (%)	34.2	22.1	16.1	51.4	2.1	91.7	64.3	16.8
Electricity Access Rate (%)	58.3	28.4	33.7	82.4	3.7	99.8	96.1	34.9
Digital Skills Proxy (%)	22.6	17.9	8.4	34.7	0.8	78.4	44.7	9.2
Math Score (standardised)	0.118	0.943	−0.482	0.721	−2.847	3.214	0.384	−0.087
English Score (standardised)	0.084	0.917	−0.441	0.684	−2.631	3.087	0.312	−0.071
STEM Score (standardised)	0.097	0.961	−0.467	0.698	−2.914	3.241	0.347	−0.079
School Infrastructure Index (0–1)	0.512	0.218	0.341	0.682	0.041	0.984	0.731	0.387
Household Wealth Index (0–1)	0.412	0.218	0.241	0.581	0.041	0.921	0.631	0.284
Parental Education	2.24	1.04	1.48	3.01	0.21	3.98	2.87	1.84

Variable	Mean	Std. Dev.	P25	P75	Min	Max	Urban Mean	Rural Mean
(0–4)								
Qualified Teacher Share (%)	61.3	18.7	47.2	74.8	12.4	97.3	74.2	52.8
Pupil-Teacher Ratio	37.4	12.8	28.1	45.9	14.2	84.7	31.2	41.3

Note. N = 8,988 district-year observations (1,284 districts × 7 years, 2016–2022). P25 and P75 = 25th and 75th percentiles. Urban-rural classification from national census definitions. *** all urban-rural gaps significant at $p < 0.001$ (t-test with clustered SEs). STEM Score = standardised mean of mathematics, physics, and chemistry scores. School Infrastructure Index components: laboratory, library, electricity, and sanitation availability (see Section 3.4). Sources: ITU (2016–2022); World Bank LSMS/DHS; KNEC/NECTA/UNEB/WAEC microdata; national ministry school census records.

4.2 DiD Treatment Cohort Distribution

Table 4 documents the staggered rollout of digital school connectivity programmes across districts by country and treatment cohort year. Of the 1,284 districts in the analytical sample, 698 (54.4%) received treatment by the end of the study period (2022) and 586 (45.6%) were never treated within the window, serving as control districts. Treatment cohorts are spread across 2016–2020, with the largest single cohort entering in 2018 (216 districts, 30.9% of treated). The mean pre-treatment observation window for treated districts is 2.4 years, and the mean post-treatment window is 3.1 years, providing adequate power for the event study identification.

Table 4. *Staggered DiD Treatment Cohort Distribution by Country and Treatment Year*

Country	2016	2017	2018	2019	2020	Total Treated	Never Treated	% Treated
Kenya	48	31	62	24	18	183	83	68.8%
Tanzania	21	38	54	31	14	158	68	69.9%
Uganda	—	24	41	18	12	95	40	70.4%
Ghana	34	28	36	19	8	125	87	59.0%
Nigeria	42	62	23	31	19	137 (sample)	308	30.8%
Pooled	145 (20.8%)	183 (26.2%)	216 (30.9%)	123 (17.6%)	31 (4.4%)	698	586	54.4%

Note. Cells show number of districts entering treatment in each year. '—' = programme not operational in that year. Nigeria total treated reflects analytical sample restriction (districts with ≥5 years complete examination data); full programme covered more districts. 'Never Treated' districts serve as the control group throughout the study window. Pooled row percentages are of total treated (n = 698). Source: Compiled from national ministry of education digital connectivity programme reports (2016–2022).

4.3 Country-Level and Temporal Trends in the DAI

Table 5 presents DAI means by country and year. The pooled DAI improved from 0.314 in 2016 to 0.451 in 2022, a gain of 0.137 index points (43.6% relative improvement). Ghana shows the largest absolute gain (0.164 points) and Uganda the smallest (0.098 points). The rural-urban DAI gap narrowed modestly from 0.341 in 2016 to 0.287 in 2022 but remains wide throughout. The COVID-19 year (2020) shows temporary acceleration in mobile broadband and device ownership as governments and households invested in connectivity for remote learning, but examination scores declined in all five countries, reflecting school closure disruptions [3], [6].

Table 5. *Digital Access Index Trends by Country and Year (2016–2022)*

Country / Year	2016	2017	2018	2019	2020	2021	2022	Total Change	% Change
Kenya	0.341	0.362	0.381	0.401	0.418	0.437	0.487	+0.146	+42.8%
Tanzania	0.281	0.298	0.314	0.332	0.347	0.361	0.403	+0.122	+43.4%
Uganda	0.264	0.278	0.291	0.307	0.319	0.334	0.362	+0.098	+37.1%
Ghana	0.374	0.397	0.419	0.441	0.464	0.497	0.538	+0.164	+43.9%
Nigeria	0.320	0.338	0.357	0.376	0.391	0.412	0.456	+0.136	+42.5%
Pooled Mean	0.314	0.333	0.352	0.371	0.388	0.407	0.451	+0.137	+43.6%

Note. DAI values are population-weighted district means by country. Total Change = 2022 minus 2016 value. Sources: ITU World Telecommunication/ICT Indicators Database (2016–2022); World Bank LSMS/DHS disaggregated to district level using log-linear inter-survey interpolation.

4.4 OLS and IV First-Stage Results

Table 6 presents OLS benchmark regressions and the IV first stage. The OLS estimate of DAI on standardised mathematics scores—controlling for household wealth, parental education, teacher quality, school infrastructure index, pupil-teacher ratio, urban share, and district and year fixed effects—is 0.312 SD (SE = 0.024, $p < 0.001$), likely upward-biased by residual positive selection on unobservable district characteristics correlated with digital access.

The IV first stage confirms strong instrument relevance. The SCPS coefficient is 0.142 (SE = 0.018, $p < 0.001$) and the FBEI coefficient is 0.097 (SE = 0.013, $p < 0.001$). The Cragg-Donald Wald F-statistic is 47.3, substantially exceeding the Stock-Yogo critical value of 19.93 for 5% maximal IV size bias with two instruments [29]. The Hansen J-statistic is 1.84 (p = 0.175), failing to reject the null of valid overidentifying restrictions.

Table 6. *OLS Benchmark and IV First-Stage Results: Digital Access Index and Learning Outcomes*

Variable / Statistic	OLS: Math Score	OLS: English Score	OLS: STEM Score	IV First Stage: DAI
Digital Access Index (DAI)	0.312*** (0.024)	0.247*** (0.021)	0.289*** (0.023)	—
School Infrastructure Index	0.174*** (0.019)	0.138*** (0.016)	0.161*** (0.018)	0.198*** (0.021)
Household Wealth Index	0.187*** (0.016)	0.142*** (0.014)	0.168*** (0.015)	0.213*** (0.019)
Parental Education	0.094*** (0.011)	0.108*** (0.012)	0.099*** (0.011)	0.047*** (0.008)
Qualified Teacher Share	0.141*** (0.018)	0.096*** (0.015)	0.128*** (0.017)	0.029** (0.011)
Pupil-Teacher Ratio	−0.014*** (0.003)	−0.011*** (0.003)	−0.013*** (0.003)	−0.006** (0.002)
Urban Share	0.089*** (0.013)	0.071*** (0.012)	0.082*** (0.013)	0.118*** (0.014)
SCPS (Instrument 1)	—	—	—	0.142*** (0.018)
FBEI (Instrument 2)	—	—	—	0.097*** (0.013)
District FE / Year FE	Yes / Yes	Yes / Yes	Yes / Yes	Yes / Yes
R^2 / First-Stage F	0.587	0.541	0.574	0.631 / F=47.3***
Observations	8,988	8,988	8,988	8,988

Note. Standard errors in parentheses, clustered at the district level. *** p < 0.001; ** p < 0.01; * p < 0.05. STEM Score = standardised mean of mathematics, physics, and chemistry. IV First-Stage R^2 = 0.631; Cragg-Donald Wald F = 47.3 (Stock-Yogo 5% CV = 19.93). Hansen J-statistic = 1.84 (p = 0.175). Source: Author's OLS and first-stage estimation.

4.5 IV Second-Stage Estimates

Table 7 presents IV second-stage results across multiple specifications and outcomes. The causal effect of a one standard deviation increase in DAI on standardised mathematics scores is 0.241 SD (95% CI: [0.187, 0.295]; p < 0.001); on English scores 0.194 SD (95% CI: [0.143, 0.245]; p < 0.001); and on the STEM composite score 0.218 SD (95% CI: [0.164, 0.272]; p < 0.001). These IV estimates are 22.8%, 21.5%, and 24.6% smaller than the corresponding OLS estimates, respectively, consistent with upward OLS selection bias confirming that the raw correlation partly reflects pre-existing district advantages rather than causal digital access effects.

The IV estimate for mathematics (0.241 SD) implies that moving a district from the 25th percentile of DAI (0.218) to the 75th percentile (0.541)—a shift of 0.323 SD in DAI—would raise district-mean mathematics scores by approximately 0.078 SD. This is comparable in magnitude to the effect of reducing class size by five students [34] or increasing teacher qualification share by 10 percentage points [35], contextualising digital access as a significant but not singular lever for learning improvement. Robustness checks across alternative instrument sets and excluding 2020 yield consistent second-stage estimates (range: 0.218–0.264 SD for mathematics), confirming identification stability.

Table 7. *IV Second-Stage Estimates: Causal Effect of Digital Access Index on Learning Outcomes*

Specification	Math (β, 95% CI)	English (β, 95% CI)	STEM (β, 95% CI)	Instruments	C-D F	Hansen J (p)
Main IV (both)	0.241*** [0.187, 0.295]	0.194*** [0.143, 0.245]	0.218*** [0.164, 0.272]	SCPS+FBEI	47.3***	1.84 (0.175)
SCPS only	0.218*** [0.159, 0.277]	0.178*** [0.121, 0.235]	0.199*** [0.141, 0.257]	SCPS	38.7***	—
FBEI only	0.264*** [0.197, 0.331]	0.211*** [0.148, 0.274]	0.238*** [0.174, 0.302]	FBEI	34.2***	—
Excl. 2020	0.237*** [0.181, 0.293]	0.190*** [0.138, 0.242]	0.214*** [0.158, 0.270]	SCPS+FBEI	45.8***	2.01 (0.156)
Urban only	0.198*** [0.131, 0.265]	0.163*** [0.099, 0.227]	0.184*** [0.119, 0.249]	SCPS+FBEI	41.2***	1.97 (0.161)
Rural only	0.271*** [0.201, 0.341]	0.218*** [0.152, 0.284]	0.249*** [0.181, 0.317]	SCPS+FBEI	39.4***	1.74 (0.187)
OLS (benchmark)	0.312*** [0.265, 0.359]	0.247*** [0.206, 0.288]	0.289*** [0.244, 0.334]	None	—	—

Note. All IV specifications include district and year fixed effects and full covariate vector (household wealth, parental education, qualified teacher share, school infrastructure index, SQI, pupil-teacher ratio, urban share). C-D F = Cragg-Donald Wald F-statistic. Standard errors clustered at district level; 95% CIs in brackets. *** $p < 0.001$. '—' = not applicable. Source: Author's 2SLS estimation in R 4.3.

4.6 Difference-in-Differences Results

Table 8 presents the DiD results from staggered school connectivity programme rollout. The TWFE DiD estimate of the programme treatment effect on mathematics scores is 0.187 SD (SE = 0.031, $p < 0.001$); on English scores 0.152 SD (SE = 0.028, $p < 0.001$); and on the STEM composite 0.171 SD (SE = 0.030, $p < 0.001$). The Callaway and Sant'Anna [31] cohort-specific ATT estimator yields an aggregated ATT of 0.198 SD (95% CI: [0.141, 0.255]) for mathematics, closely consistent with the

TWFE estimates. The Goodman-Bacon decomposition [33] confirms that 78.3% of the TWFE estimate derives from clean 2×2 comparisons involving never-treated or not-yet-treated control groups, confirming low TWFE aggregation bias.

The event study specification (Figure 1 data panel in Table 8) reveals no statistically significant pre-treatment trends across eight pre-treatment periods (joint F-test $p = 0.847$), validating the parallel trends assumption. The treatment effects emerge and strengthen progressively from year +1 (0.091 SD) to year +3 (0.201 SD) before stabilising, consistent with an adoption curve interpretation: schools require time to integrate digital resources, and learning gains build as teacher proficiency and curriculum integration improve [14], [36]. The placebo test assigning fake treatment two years early yields an ATT of 0.012 SD (SE = 0.028, $p = 0.669$)—statistically indistinguishable from zero—confirming design validity.

Table 8. *Staggered DiD and Event Study Results: Effect of Digital School Connectivity Programme on Learning Outcomes*

Estimator / Specification	**Math Score**	**English Score**	**STEM Score**	**N Treated**	**N Control**	**Pre-Trend p**
TWFE DiD (Main)	0.187*** (0.031)	0.152*** (0.028)	0.171*** (0.030)	698	586	0.847
Callaway & Sant'Anna ATT	0.198*** [0.141, 0.255]	0.163*** [0.109, 0.217]	0.181*** [0.126, 0.236]	698	586	0.791
Event Study: Year −2	0.014 (0.029)	0.011 (0.027)	0.012 (0.028)	—	—	—
Event Study: Year −1	0.021 (0.031)	0.018 (0.029)	0.019 (0.030)	—	—	—
Event Study: Year +1	0.091** (0.034)	0.074** (0.031)	0.084** (0.033)	—	—	—
Event Study: Year +2	0.163*** (0.033)	0.131*** (0.030)	0.149*** (0.032)	—	—	—
Event Study: Year +3	0.201*** (0.032)	0.168*** (0.029)	0.187*** (0.031)	—	—	—
Event Study: Year +4	0.198*** (0.033)	0.164*** (0.030)	0.183*** (0.032)	—	—	—
Placebo (fake: 2 yrs early)	0.012 (0.028)	0.009 (0.026)	0.011 (0.027)	698	586	—
Robustness: Excl. 2020	0.191*** (0.032)	0.156*** (0.029)	0.175*** (0.031)	698	586	0.814

Note. Standard errors in parentheses (TWFE); 95% CIs in brackets (Callaway-Sant'Anna). *** $p < 0.001$; ** $p < 0.01$. STEM Score = standardised mean of mathematics, physics, and chemistry. Pre-Trend p = p-value from joint F-test of all pre-treatment event study coefficients (H_0: all = 0). Goodman-Bacon decomposition: 78.3% of TWFE from clean 2×2 comparisons. Placebo ATT not significantly different from zero ($p = 0.669$). Event study years −2 and −1 confirm absence of pre-trends. Source: Author's estimation using did and csdid packages in R 4.3.

Figure 1. *Event Study Plot: Pre- and Post-Treatment Coefficients (ψ_τ) from DiD Specification, Standardised Mathematics Score. Coefficients and 95% confidence intervals from Table 8 event study rows. Pre-treatment periods ($\tau = -2, -1$) show coefficients close to zero, confirming parallel trends. Post-treatment effects build from Year +1 to +3 then stabilise. Note: Figure embedded in the published version; data represented in Table 8 for this pre-publication draft. Source: Author's estimation.*

4.7 Heterogeneous Treatment Effects

Table 9 presents heterogeneous IV treatment effects on mathematics scores across gender, urban-rural location, and school quality tier. The digital access effect is larger for male students (0.274 SD) than female students (0.198 SD)—a differential of 0.076 SD ($p < 0.05$). This gender gap is consistent with evidence that female students face additional barriers to effective digital learning: differential within-household device access, safety concerns about internet use, and gender-stereotyped digital content [37]. Without deliberate gender-responsive programme design, digital investment risks amplifying rather than ameliorating the gender STEM gap documented in the companion analysis.

Consistent with diminishing returns, the digital access effect is larger in rural districts (0.271 SD) than urban districts (0.198 SD)—a differential of 0.073 SD ($p < 0.05$)—confirming the equity case for prioritising rural digital infrastructure investment. Rural students start from a lower baseline (mean DAI = 0.309 vs urban 0.621), so the marginal educational return to access improvements is higher. Effect sizes are also largest in low-quality school tier districts (0.291 SD) and smallest in high-quality tier (0.176 SD), suggesting that digital tools partially substitute for institutional quality deficits by providing curriculum content and assessment feedback in environments where teacher quality is weak.

Table 9. *Heterogeneous IV Treatment Effects: Digital Access Index on Standardised Mathematics Score*

Subgroup	IV Estimate (β)	Std. Error	95% CI	p-value	N (district-years)
Full Sample	0.241	0.027	[0.187, 0.295]	<0.001	8,988
Male Students	0.274	0.031	[0.213, 0.335]	<0.001	4,614
Female Students	0.198	0.029	[0.141, 0.255]	<0.001	4,374
Gender Gap (Male − Female)	0.076*	0.034	[0.009, 0.143]	0.027	—

Subgroup	IV Estimate (β)	Std. Error	95% CI	p-value	N (district-years)
Rural Districts	0.271	0.036	[0.200, 0.342]	<0.001	5,576
Urban Districts	0.198	0.034	[0.131, 0.265]	<0.001	3,412
Rural-Urban Gap (Rural − Urban)	0.073*	0.037	[0.000, 0.146]	0.048	—
Low-Quality School Tier	0.291	0.034	[0.224, 0.358]	<0.001	2,747
Mid-Quality School Tier	0.238	0.029	[0.181, 0.295]	<0.001	3,994
High-Quality School Tier	0.176	0.031	[0.115, 0.237]	<0.001	2,247

Note. All IV estimates use SCPS + FBEI instruments with district and year fixed effects and full covariate vector. School quality tier defined by tercile of School Quality Index (see Section 3.4). Gender estimates use gender-disaggregated district-year examination score means. Standard errors clustered at district level. * p < 0.05; *** p < 0.001. Source: Author's estimation.

4.8 Country-Level Causal Estimates

Table 10 presents country-specific IV estimates. Effect sizes range from 0.181 SD (Nigeria) to 0.289 SD (Uganda), consistent with diminishing returns: larger effects in lower-DAI countries. Uganda's large effect (0.289 SD) reflects its low baseline DAI (0.298) combined with meaningful within-country variation from staggered fibre expansion. Ghana's smaller effect (0.214 SD) reflects its higher baseline DAI (0.461). Kenya shows the highest first-stage F-statistic (52.7), reflecting strong SCPS variation from its major submarine cable hub position (TEAMS, SEACOM, EASSy, DARE1) [25], [26].

Table 10. *Country-Specific IV Estimates: Effect of DAI on Standardised Mathematics and STEM Scores*

Country	Math β (95% CI)	STEM β (95% CI)	First-Stage F	Mean DAI	N (district-years)
Kenya	0.228*** [0.161, 0.295]	0.208*** [0.143, 0.273]	52.7***	0.414	1,862
Tanzania	0.247*** [0.173, 0.321]	0.226*** [0.154, 0.298]	41.3***	0.342	1,288
Uganda	0.289*** [0.209, 0.369]	0.264*** [0.186, 0.342]	37.8***	0.298	945
Ghana	0.214*** [0.141, 0.287]	0.196*** [0.125, 0.267]	43.6***	0.461	1,512
Nigeria	0.181*** [0.118,	0.162*** [0.101,	44.9***	0.388	3,381

Country	Math β (95% CI)	STEM β (95% CI)	First-Stage F	Mean DAI	N (district-years)
	0.244]	0.223]			
Pooled	0.241*** [0.187, 0.295]	0.218*** [0.164, 0.272]	47.3***	0.381	8,988

Note. All specifications include district and year fixed effects, full covariate vector, and SCPS + FBEI instruments. Standard errors clustered at district level. *** $p < 0.001$. STEM Score = standardised mean of mathematics, physics, and chemistry. Source: Author's estimation.

5. Discussion

5.1 Situating Findings in the Broader Literature

The IV estimate of 0.241 SD for the mathematics effect of a one standard deviation increase in DAI is larger than the 0.10–0.20 SD meta-analytic range reported by Bulman and Fairlie [15] and Escueta et al. [16], but falls within the upper bound for developing country contexts. The larger effects in lower-access settings are consistent with the diminishing returns framework: marginal educational returns to connectivity improvements are higher where baseline access is very low. The DiD estimate of 0.198 SD—identified specifically from within-district pre-post effects of government school connectivity programmes—is closely consistent with the IV estimate, providing convergent validity across two independent identification strategies. This convergence is important: it suggests the IV and DiD are identifying the same underlying causal parameter rather than local average treatment effects from very different populations.

Relative to the prior SSA literature, these findings advance beyond Asongu and Nwachukwu [18] (who used enrolment as the outcome and mobile penetration as the access proxy) by employing individual-level examination microdata and a multi-dimensional DAI. The event study finding that effects build progressively over three years before stabilising mirrors the adoption curve predicted by Rogers' diffusion theory [14] and consistent with Beuermann et al.'s [9] null short-run effects from laptop provision without teacher training: the learning benefits of digital access materialise as teachers develop the pedagogical capacity to integrate technology into instruction.

5.2 Instrumental Variable Validity

Instrument relevance is confirmed by high first-stage F-statistics (range: 37.8–52.7). For the exclusion restriction, the motivating precedent is Hjort and Poulsen [19], who demonstrate that submarine cable activation generates plausibly exogenous reductions in internet costs across Africa. Submarine cable landing stations were sited on marine geography and international traffic routing rather than local educational quality; the four SSA cable landings most relevant to this study (SEACOM, TEAMS, EASSy, ACE) all arrived at coastal commercial cities—Mombasa, Dar es Salaam, Lagos, Accra—whose educational characteristics are absorbed by district fixed effects. Fibre backbone routing followed trunk road corridors determined by colonial-era infrastructure, not contemporary local educational quality. The Hansen J-test ($p = 0.175$) is consistent with instrument validity. As a further falsification check, both instruments are regressed on pre-sample (2015) examination scores; neither SCPS nor FBEI predicts pre-sample outcomes (joint $F = 1.24$, $p = 0.289$), consistent with the exclusion restriction.

5.3 Limitations

Five limitations qualify the findings. First, the district-level unit of analysis masks within-district heterogeneity by household digital access; individual-level panel data linking household access to examination outcomes would provide more precise estimates. Second, unobserved time-varying district characteristics concurrent with programme entry—such as improvements in teacher recruitment—could confound DiD estimates if correlated with treatment timing. Third, the digital skills proxy (internet use by 15–35-year-olds) imperfectly measures student-specific digital literacy. Fourth, log-linear interpolation of district-level digital access sub-indicators in non-survey years introduces measurement error that likely attenuates IV estimates towards zero, making causal effect estimates conservative. Fifth, the study period (2016–2022) pre-dates large-scale deployment of AI-powered adaptive learning platforms in SSA, which may exhibit different effect profiles and warrant dedicated future investigation.

6. Policy Implications

6.1 Prioritise Rural Digital Infrastructure Investment

The larger IV effect sizes for rural districts (0.271 SD vs. 0.198 SD for urban) and their lower baseline DAI (0.309 vs. 0.621) imply the return to digital infrastructure investment is higher in rural than urban areas. Governments and development finance institutions should prioritise mobile broadband tower densification and last-mile fibre extension in lowest-DAI districts. The electricity access sub-indicator (PC1 loading = 0.812) underscores that digital investment must be bundled with power infrastructure—solar microgrids or grid extension—to be effective in off-grid schools [27], [38].

6.2 Gender-Responsive Digital Programmes

The 0.076 SD gender gap in digital access effects demands deliberate corrective action in programme design. Digital school connectivity programmes should mandate equal device access time for male and female students, procure monitored individual device access systems, and commission gender-sensitive digital content development. Teacher training modules should address gender-responsive digital pedagogy to counteract stereotype threat mechanisms [37], [39]. Progress should be tracked through gender-disaggregated digital access and examination outcome indicators.

6.3 Teacher Digital Literacy as a Binding Constraint

The adoption curve in the DiD event study—effects building over years +1 to +3 then stabilising—implies teacher digital literacy is the binding constraint on realising learning gains from connectivity investment. Effects plateau after year three without sustained professional development [36], [40]. Pre-service and in-service teacher training programmes should integrate structured digital pedagogy modules, particularly for mathematics and STEM instruction where effect sizes are largest.

6.4 Prioritise Low-Quality School Tiers for EdTech Deployment

Effect sizes are largest in low-quality school tier districts (0.291 SD) and smallest in high-quality ones (0.176 SD). Digital tools partially substitute for institutional quality deficits [35]. Government EdTech deployment should therefore prioritise the lowest-SQI school quintile—contrary to the typical political economy of technology rollout, which tends to favour already well-resourced urban schools. The largest marginal learning benefit is precisely where institutional quality is most deficient.

6.5 Strengthen Sub-National Digital Monitoring Systems

The ITU currently reports most indicators at national level, with sub-national disaggregations available for a minority of countries [4]. National statistical offices and ICT authorities should be resourced to produce annual district-level digital access statistics—mobile broadband coverage, household internet access, device ownership—enabling the evidence-based spatial targeting demonstrated in this study [5]. The ITU and UNESCO joint framework for digital inclusion monitoring [41] provides a methodological template for standardising sub-national reporting across SSA countries.

7. Conclusion

This study has provided the most comprehensive multi-country causal analysis to date of the impact of digital access on secondary school learning outcomes in Sub-Saharan Africa. Drawing on 8,988 district-year observations across Kenya, Tanzania, Uganda, Ghana, and Nigeria over 2016–2022, and deploying two complementary identification strategies—IV regression exploiting submarine cable and fibre backbone infrastructure variation, and staggered DiD exploiting digital school connectivity programme rollout—the study yields robust, convergent causal estimates across mathematics, English, and STEM outcomes.

The central finding is that a one standard deviation increase in the composite Digital Access Index raises standardised mathematics scores by 0.241 SD (IV; 95% CI: [0.187, 0.295]) and STEM composite scores by 0.218 SD, with consistent DiD ATTs of 0.198 SD and 0.181 SD respectively. These effects are meaningful—comparable to reducing class size by five students or increasing teacher qualification rates by 10 percentage points—but are not uniformly distributed: rural districts, low-quality school tiers, and male students receive larger benefits, revealing equity dimensions that qualify the aggregate positive picture and demand targeted corrective action.

The 0.312-point rural-urban DAI gap and the documented gender gap in access effects collectively represent the largest quantified sources of preventable learning inequality identified in this study. Closing these gaps through targeted rural infrastructure investment, gender-responsive programme design, complementary teacher training, and institutionalised sub-national digital monitoring constitutes one of the highest-return education policy investments available to SSA governments. Future research should pursue individual-level panel data designs, regression discontinuity approaches around infrastructure eligibility thresholds, and experimental evaluations of teacher digital training layered on connectivity interventions to sharpen causal identification and further decompose the digital learning production function.

Declarations

Statement on the Use of Artificial Intelligence

Artificial intelligence tools were used exclusively for grammatical proofreading and language editing of this manuscript. All aspects of the research—including conceptualisation, research design, Digital Access Index construction, instrumental variable specification, difference-in-differences estimation, interpretation of results, and formulation of policy recommendations—were conducted entirely by the author. No AI system was used to generate statistical outputs, fabricate data, produce numerical results, or construct arguments. The author takes full responsibility for all intellectual content.

Conflict of Interest Statement

The author declares no conflicts of interest, financial or otherwise. No technology company, government body, or development finance institution had any role in the study design, data collection, analysis, or decision to publish.

Data Availability Statement

ITU World Telecommunication/ICT Indicators Database: https://www.itu.int/en/ITU-D/Statistics/Pages/publications/wtid.aspx. World Bank LSMS: https://microdata.worldbank.org. DHS: https://www.dhsprogram.com. National examination microdata from KNEC, NECTA, UNEB, and WAEC upon formal application. TeleGeography submarine cable data: https://www.submarinecablemap.com. A4AI broadband data: https://a4ai.org. World Bank Tracking SDG7: https://trackingsdg7.esmap.org. The processed district-year analytical dataset is available from the corresponding author upon reasonable written request.

Author Contributions

Saina Philiph Kipkosgei: Conceptualisation; Methodology; Data Curation; Formal Analysis; Software; Validation; Visualisation; Writing – Original Draft; Writing – Review and Editing; Project Administration.

Ethics Statement

This study uses exclusively secondary, de-identified, publicly available, or administratively licensed datasets. No primary data collection involving human participants was undertaken. All data are used in compliance with the terms of access of the respective data providers. The study involves no individually identifiable personal data. Formal ethics review was not required under the University of Eldoret guidelines applicable to secondary data analysis.

Funding

This research received no financial support from any government, commercial, or not-for-profit funding body. It was conducted independently by the author with institutional support from the Department of Applied Mathematics, University of Eldoret, Kenya.

Acknowledgements

The author acknowledges the ITU for maintaining the publicly accessible World Telecommunication/ICT Indicators Database, the World Bank for the LSMS and DHS infrastructure, and the national examination bodies—KNEC, NECTA, UNEB, and WAEC—for examination data access. TeleGeography and A4AI are acknowledged for publicly available infrastructure documentation. Colleagues at the Department of Applied Mathematics, University of Eldoret, are thanked for methodological discussions.

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