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Investigating the Use of Information Technology in Learning English Technical Terminology among Students at the Higher Institute of Water Affairs, Ajilat, Libya: A Critical Secondary-Evidence Analysis

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Abstract

Technical terminology is a prerequisite for interpreting specialised texts, measurements and procedural guidance in water-related education, yet claims that digital tools automatically improve technical-vocabulary learning remain insufficiently tested in Libyan vocational institutions. This critical secondary-empirical analysis examines published evidence relevant to students at the Higher Institute of Water Affairs, Ajilat, without misrepresenting those studies as institute-based fieldwork. A purposive, source-traceable synthesis draws on recent Libyan vocabulary studies, a UK sector survey and established IT for Specific Purposes (ITSP) scholarship. A Libyan primary-school digital-storytelling study (n = 30 overall) reported an increase in vocabulary mean scores from 52% to 71%, a descriptive difference of 19 percentage points; a separate experimental university study (n = 60) reported a significant digital-intervention advantage without publishing a directly extractable effect estimate in its abstract. At the University of Zawia (n = 30), learners rated Quizlet's contribution to vocabulary and retention at 4.12/5, but this is an attitudinal rating rather than a measured learning gain. In the UK Jisc survey (n = 15,398), 60% reported wifi difficulties and 37% a period without a suitable device. These heterogeneous findings support a qualified model combining discipline-specific lexical input, retrieval practice, contextual application and equitable digital access, while precluding pooled effect estimation or site-specific conclusions. The study presents original cross-study analytical tables, source-labelled figures and a preregistration-ready protocol for future Ajilat data collection. Its principal contribution is to distinguish encouraging intervention evidence from weaker self-report and transferability claims.

Keywords: IT for Specific Purposes; technical vocabulary; digital literacy; technology-enhanced language learning; evidence synthesis; Libya; vocational education

1. Introduction and research problem

In water-sector education, English terminology mediates access to equipment specifications, aquifer surveys, pumping tests, environmental measurements and procedural documentation. A learner may recognise the everyday English word “head” yet misunderstand hydraulic head; may translate “yield” but not interpret

sustainable borehole yield; or may recognise “turbidity” while failing to explain its implications for water-quality monitoring. These are failures of situated technical meaning, not merely gaps in bilingual word recall. Consequently, digital-language interventions should be evaluated against disciplinary decisions and context-sensitive interpretation rather than vocabulary-test improvement alone.

Available digital resources include electronic dictionaries, specialist glossaries, captioned technical videos, flashcard applications, searchable corpora and AI-supported explanation systems. Their accessibility can expand repeated exposure, but abundance can also compound error. Public flashcard sets may contain incorrect senses, machine translation can select an inappropriate technical equivalent, and unrestricted search can privilege fluent but unreliable content. The correct empirical question is therefore conditional: for which learners, materials and tasks do digital resources provide learning benefits, at what cost and under what support conditions?

This paper uses the Higher Institute of Water Affairs, Ajilat, as a context for research questions and future study design. It does not claim that students from this institute were surveyed or taught by the authors as part of the reported evidence. Preserving this boundary is central to research integrity and to the article's contribution: a critical re-analysis of available evidence into a genuinely testable model for Libyan technical education.

2. Analytical framework and research questions

ITSP needs analysis holds that target language content should be derived from actual disciplinary communication rather than generic word frequency alone (Hyland, 2022). Nation (2013) distinguishes dimensions of lexical knowledge including form, meaning and use; Schmitt and Schmitt (2020) emphasise retrieval and repeated, contextual encounters. These concepts support a three-part assessment: receptive recognition of English terms, accurate explanation or recall, and productive interpretation in disciplinary contexts. Digital convenience is treated as an enabling condition, not as an educational outcome. Salem’s (2019) interview-and-observation study of Libyan EFL lecturers shows why the institutional and sociocultural setting of any educational technology matters to its practical use.

Mayer's (2009) multimedia-learning framework explains why diagrams and spoken or written explanation may complement one another, while also cautioning against excess information. For water-related examples, a labelled cross-section of an aquifer can connect “water table”, “recharge” and “permeability” to a physical relationship; an excessively animated clip may distract from it. The research logic therefore examines both accuracy and cognitive economy.

The questions guiding this secondary analysis are: (1) What measured and perceived vocabulary outcomes are reported in comparable Libyan digital-language studies? (2) Which methodological features constrain causal attribution and disciplinary transfer? (3) What does contemporary UK digital-access evidence imply about the practicality and equity of importing technology-rich models? (4) What Ajilat-specific study could evaluate genuine technical terminology learning?

3. Method: structured critical analysis of published evidence

This is a purposive, structured secondary-data analysis rather than a systematic review, meta-analysis or newly collected institutional survey. Publicly accessible publisher and university records were consulted on 24 October 2026, prioritising Libyan empirical studies of technology-assisted English vocabulary and ESP, UK sector surveys supplying relevant access indicators, and established theoretical texts. Eligibility required an identifiable source, an educational context, an explicit design or dataset, and a result relevant to digital vocabulary or implementation. Studies without numerical estimates were retained for qualitative interpretation but not for plotted quantitative outcomes.

For every quantitative result, the extraction fields were author/year, population and setting, sample size where available, instrument, reported statistic, interpretation, and key threats to validity. Where publisher abstracts did not report test statistics, standard deviations, effect sizes or group-level results, these were not reconstructed. Narrative claims were differentiated as measured learning outcome, self-reported perception, or infrastructure experience. As the sources study different ages, subjects, technologies and questions, no weighted synthesis, confidence interval, cross-study significance test or formal pooled effect estimate was attempted.

Critical appraisal focused on four dimensions: internal validity (comparison group, allocation and testing), measurement validity (vocabulary knowledge versus attitudes), transferability to water-sector ITSP, and implementation equity (hardware, connectivity and teacher support). This pragmatic search is not exhaustive, no PRISMA flow is asserted, and publication or language bias may favour positive findings. Results below refer exclusively to the original studies identified by name.

Table 1. Source-level evidence matrix for digital vocabulary and access

Study/context	Design / sample	Extractable result	Analytical limitation
Abdullah (2026), Maslata	School comparison study; 30 pupils overall	Vocabulary mean: 52% pre; 71% post in experimental reporting	School-level general English; no extractable adjusted effect or variance
Hadaga & El-falfal (2026), Benghazi	Random allocation; 60 first-semester EFL undergraduates	Duolingo group outperformed conventional group (reported significant)	No effect size available from verified abstract; not technical ESP
Alqadhi et al. (2026), Zawia	Attitude survey; 30 EFL undergraduates	Vocabulary/retention mean 4.12/5; ease mean 3.29/5	Attitude ratings not achievement tests
Jalala (2023), Al-Jufra	Electronic dictionary questionnaire; 35 students	Students reported extensive dictionary reliance and context-selection difficulties	Self-report; no objective term-use scoring
Jisc (2025), UK HE	Cross-sectional sector survey; 15,398 students	60% wifi difficulties; 37% device shortfall at some point	UK sample cannot estimate Libyan prevalence
Dizon (2016), Japan	10-week Quizlet study; 9 university EFL students	Significant pre/post improvement reported	Very small sample; non-water-sector words

4. Results of the secondary empirical analysis

4.1 Vocabulary gains are encouraging but not uniformly identified as treatment effects. In Abdullah's (2026) Maslata investigation, the reported vocabulary score increased from 52% to 71%—an arithmetic difference of 19 percentage points. This is a transparent description of two reported figures, not evidence of a causal effect of 19 points attributable to the technology: confidence intervals, a comparator change and detailed baseline equivalence are not extractable from the public abstract. The intervention was conducted among school pupils learning general English, so educational stage and lexical demand differ substantially from interpreting water-sector documentation at a higher institute. Nevertheless, it establishes that an intervention-oriented rather than purely attitudinal design is feasible in a Libyan setting.

In a separate 2026 Benghazi university study, Hadaga and El-falfal described random assignment of 60 first-semester EFL students to Duolingo-assisted and conventional vocabulary instruction. The abstract reported superior vocabulary achievement for the digital condition following t-test and ANCOVA analyses, without providing the adjusted means or an effect-size estimate required for transparent comparative re-analysis. This is comparatively stronger causal evidence than cross-sectional attitudes, but its first-semester general-EFL target differs from specialist terminology.

4.2 Learner enthusiasm does not establish technical competence. Alqadhi et al. (2026) surveyed 30 University of Zawia students and reported a mean of 4.12/5 for perceived vocabulary learning and retention but 3.29/5 for ease of use. The 0.83 scale-point contrast reveals a perceived outcome–usability tension within one sample, not a measured 0.83-point learning improvement. Participants mentioned connectivity, inaccurate public sets and paywalled functions. Jalala (2023), from 35 Al-Jufra English-department respondents, similarly described students' dependence on electronic bilingual dictionaries despite persistent difficulties identifying the correct contextual sense. Jointly these studies suggest that access to a lexical answer can coexist with uncertainty about its specialist correctness.

Albilaezi (2026) also reported a positive association between AI technology use and ESP learning-efficiency ratings among 65 graduate students at the Libyan Academy’s Derna Branch. Because the publicly available summary does not supply an interpretable correlation coefficient or objective before-and-after ESP scores, this association cannot be treated as an independent effect estimate. A further quasi-experimental report by Hmouma and Benarose (2026) at Zawia University describes an advantage for AI-supported vocabulary learning, but its public summary likewise does not provide the numerical quantities needed for pooling. These studies reinforce the feasibility of university-level inquiry while underscoring the distinction between general EFL improvement and discipline-specific water terminology.

4.3 Access and skills constitute plausible moderators. Jisc's 2024/25 UK survey of 15,398 students found 86% rated the overall digital learning environment above average, but 60% reported wifi problems, 37% had lacked a suitable device at some point, and 55% received guidance on course-related digital skills (Jisc, 2025a). These percentages address different questions and do not define mutually exclusive respondent categories. They illustrate why satisfaction should not be used as a proxy for equal opportunity to participate. The precise prevalence at Ajilat is unknown and must be measured locally.

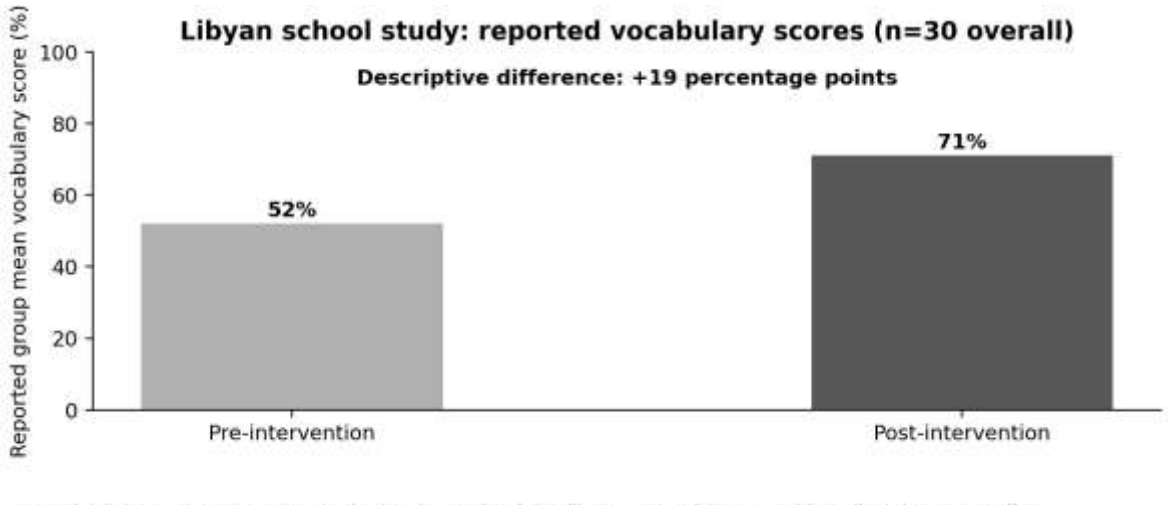


Figure 1. Published Libyan pre-/post-intervention scores; from Abdullah (2026). The 19-percentage-point change is descriptive and is not an Ajilat finding.

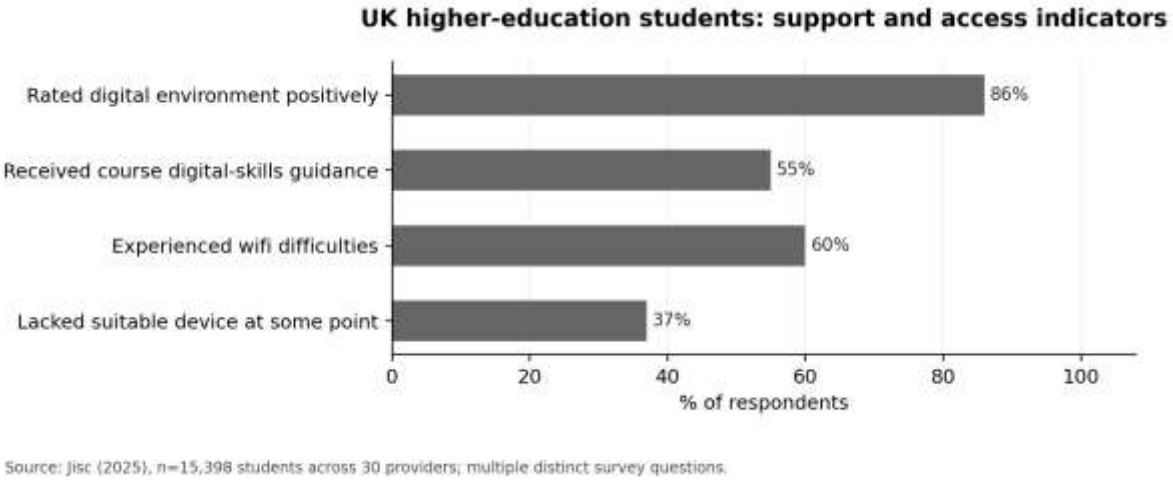


Figure 2. Source-derived percentages from Jisc (2025). These are UK student responses, not estimates for Libyan students.

5. Critical interpretation: mechanisms, rival explanations and external validity

The most defensible cross-study finding is a pattern of conditional promise, not proof that technology itself improves technical terminology at Ajilat. Intervention studies permit limited inferences about improvement, but the sources differ in age, educational subject, instructional intensity and assessment. Questionnaire studies document acceptability and perceived usefulness, yet are vulnerable to novelty, social desirability, common-method variance and a lack of independent vocabulary checks. Comparing these designs as though they estimate a common "digital effectiveness" effect would conceal rather than resolve heterogeneity.

A first rival explanation is additional time-on-task: learners using digital flashcards may simply practise more frequently. A second is intervention novelty and teacher attention. A third is differential prior English proficiency or device ownership. A fourth concerns outcome proximity: immediate recognition of trained

flashcards may improve while students remain unable to interpret a water-quality report. To test instructional benefit, Ajilat assessments should therefore contain both taught and untrained but related terms, blind-rated contextual uses, and a delayed retention test.

Specialist terminology adds a distinct construct-validity problem. “Flow”, “head”, “discharge”, “recharge”, “drawdown” and “saturation” can have general and technical meanings. Acceptable responses must reflect water-science interpretation rather than an English–Arabic dictionary match. Domain lecturers should validate the glossary and scoring rubric independently of the language instructor, ideally recording disagreements and consensus rules. Accessibility also matters: offline-capable term banks may outperform sophisticated cloud-only resources where connectivity is unpredictable. This is a hypothesis for local testing, not a claim that Ajilat students currently experience specified barriers.

The UK comparison is deliberately used as a counterexample to technologically deterministic assumptions. Even systems with largely positive student evaluations exhibit access disparities. The UK figures cannot be transferred as Libyan base rates; they justify measuring access and planning an equitable, low-bandwidth option before running the intervention.

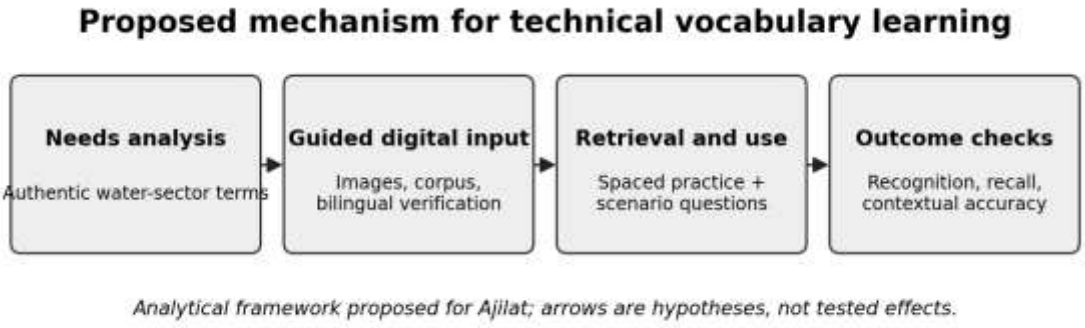


Figure 3. Author-developed conceptual programme theory for an Ajilat intervention. Relationships are hypotheses that require testing.

6. Ajilat-specific empirical protocol: how the claim can be tested

A defensible follow-on design would start with a student and lecturer needs analysis using authentic teaching materials, technical manuals, laboratory sheets and assessment questions. Two subject specialists and an ESP lecturer would curate a list of approximately 60–80 representative target expressions; the final item pool should be selected empirically rather than asserted in advance. The technical glossary would record definition, Arabic equivalent where appropriate, diagram, common collocations, example sentence and source of verification. Pilot think-aloud interviews would determine whether the test distinguishes linguistic recognition from technical reasoning.

An ethically approved quasi-experiment, or randomised allocation where operationally feasible, could compare a structured digital condition (offline glossary, spaced retrieval, captioned visual tasks and instructor feedback) with an active comparison condition receiving the same terminology, teaching time and

instructor attention in a non-digital format. Record baseline proficiency and device access; include pre-test, immediate post-test and delayed post-test, with blinded scoring of contextual tasks. Report group sizes, attrition and missing data, intervention fidelity, score distributions and a pre-specified adjusted effect with 95% confidence intervals. Where classes are allocated rather than individuals, account for clustering instead of assuming observations are independent.

Qualitative learner diaries and purposive interviews can identify when technology supported comprehension or created uncertainty. A descriptive access audit would record device ownership, reliable internet access, data costs and opportunities for offline work, without presuming levels. Reporting should distinguish recognition, meaning explanation, context-sensitive application and retention; present negative and null findings as well as gains. The study should not be described as completed until participants, ethics documentation and records exist.

7. Evidence quality, model sensitivity and reporting safeguards

An evidence-to-claim audit helps prevent attractive visualisations from being mistaken for stronger findings than their sources permit. The Abdullah (2026) 19-point change is an observed pre-/post-score contrast. Without a published comparator change and measures of sampling variability, it cannot be translated into a causal treatment effect, an equivalent months-of-learning figure or a confidence interval. The Hadaga and El-falfal (2026) study is reported as randomised and statistically significant, but the lack of accessible adjusted differences and uncertainty estimates prevents us from calculating a robust pooled standardized mean difference. A synthetic average of these interventions would create false precision.

Attitudinal data constitute a different evidence class. Alqadhi et al. (2026) reported scale ratings, not vocabulary-test outcomes. The perceived learning and retention score was 4.12/5, but high mean agreement does not establish retention at four weeks, nor does the 0.83-point within-survey contrast with ease of use warrant treating ease as a statistically significant predictor. Jalala's (2023) dictionary findings indicate a meaningful gap between ready access to lexical equivalents and ability to discriminate context. Neither survey resolves whether greater use caused higher vocabulary knowledge or whether stronger learners were more likely to choose digital resources.

A future Ajilat experiment should commit to intention-to-treat estimates where assignment allows it, pre-specified missing-data handling, and a multi-rater marking procedure with blinded scoring. For a 0–20 performance rubric, publish item distributions, inter-rater agreement, adjusted mean differences and 95% confidence intervals. Separate immediate post-test benefit from a delayed retention effect and include a “transfer” subset of authentic unseen water-sector terms. A null result could mean no instructional advantage, insufficient implementation, a ceiling effect, or a poor outcome measure; interpretation should inspect these alternatives rather than retroactively select only favourable measures.

Finally, the project should register an analysis plan and make anonymised instruments, scoring keys and de-identified aggregate outcomes accessible wherever consent and policy permit. This is particularly important

for local conference research, where modest samples and highly specific institutional contexts can otherwise encourage over-generalisation. The original published percentages and the figures in this article remain descriptive secondary evidence only.

Table 2. Inferential strength by type of available evidence

Evidence category	Supported claim	Unsupported leap
Pre-/post-intervention score	Mean changed across measured occasions	Technology caused all of the observed change
Randomised comparison, summary only	Reported between-group direction and significance	Specific effect magnitude or lasting transfer without data
Five-point attitude item	Respondents endorsed a statement	Respondents objectively mastered target vocabulary
UK access survey	Reported barriers in the surveyed UK group	Same percentage of learners at Ajilat face the barrier

8. Implications, limitations and conclusion

For the Higher Institute of Water Affairs, a defensible first step is to build a modest, verified, bilingual, offline-capable terminology bank and couple every digital item with a technically authentic question. Procurement should follow measured needs, not precede them. Lecturers and students must be taught how to challenge ambiguous translations and to confirm high-consequence terms against approved disciplinary sources. Assessment should award credit for precise usage in context rather than for memorised labels alone.

This synthesis is restricted to public accounts, several of which provide incomplete statistical details; it uses purposive rather than exhaustive source selection, lacks formal risk-of-bias ratings, and cannot estimate local prevalence or effect sizes. Studies of school learners, university EFL students and UK higher education are informative but not interchangeable with Ajilat's technical cohorts. The contribution is therefore analytical and design-oriented: published intervention results justify investigation, whereas perception and access data show why positive attitudes or easy application access should not be treated as sufficient evidence of learning. Only future institute-based evaluation can adjudicate its substantive effectiveness.

Author declarations for completion before journal submission

Ethics: This document reports analysis of already published, publicly accessible studies and introduces no new human-participant data. Funding, competing interests, correspondence details, author contributions, affiliation approval and final manuscript approval must be completed and verified by the named authors before submission. Data availability: Numerical values used in the figures are transcribed from the cited public reports; no original institutional participant-level dataset exists for this article.

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