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Evaluating Universal Design for Learning and Differentiated Instruction for Improving Classroom Participation Among Learners with Intellectual Disabilities

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Abstract
Classroom enrolment does not guarantee meaningful instructional participation for learners with intellectual disabilities. This methodological article develops and evaluates a prespecified, factorial instructional framework combining universal design for learning (UDL) and differentiated instruction (DI) with an explicit measurement strategy for accessible classroom participation. A hypothetical 24-class, 240-learner, 13-observation-wave experiment was implemented in Python to evaluate analytical behaviour under transparent assumptions rather than to estimate an observed intervention effect. Outcomes were constructed as opportunities for academic response, supported communication, task initiation and collaborative engagement. A multilevel binomial model, latent participation construct, time-varying treatment specification, measurement-error adjustment and cluster bootstrap were proposed. In the simulation, mean baseline-to-week-12 change was −0.008 under usual practice, 0.085 under UDL, 0.073 under DI and 0.188 under the combined condition. These differences are generated consequences of the stated data-generating mechanism and cannot establish effectiveness in Ghanaian or other classrooms. The model illustrates how an ethically approved, adequately powered field trial could test whether the combined approach promotes participation, and identifies measurement, fidelity, spillover and accessibility issues requiring empirical attention.

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Keywords: intellectual disability, inclusive education, universal design for learning, differentiated instruction, classroom participation, cluster randomisation, educational measurement

1. Introduction

Inclusive education is often evaluated through enrolment, placement and attendance, yet these indicators overlook the quality of a learner's participation in ordinary classroom routines. A child can remain physically present during a lesson while having little opportunity to answer questions, select a learning activity, communicate an idea or contribute to peer work. For learners with intellectual disabilities, this distinction between access and participation is particularly important because classroom activities can impose simultaneous demands on working memory, expressive communication, attention, motor coordination and social interaction. The problem therefore concerns the alignment between instructional opportunities and an individual learner's mode of engagement, rather than a simple deficit attributed to the learner (United Nations, 2006; UNESCO, 2020) [41, 42].

Universal design for learning seeks to anticipate variation at the level of curriculum and instruction. The 2018 CAST guidelines organise planning around multiple means of engagement, representation, and action and expression. Teachers might offer equivalent concepts through demonstrations, concrete materials, spoken explanations, tactile examples or pictures, and permit a learner to demonstrate understanding through pointing, choice cards, assisted communication or short verbal contributions.

Differentiated instruction, by comparison, concerns responsive adjustments to task complexity, pacing, support, grouping and products informed by learner readiness and ongoing assessment (CAST, 2018; Tomlinson, 2014) [8, 40].

These approaches are related but conceptually distinguishable. UDL changes the architecture of classroom access before a particular lesson begins; DI adjusts the immediate instructional pathway as the teacher observes performance. Their combination may offer a more coherent opportunity structure than either approach alone, but this interaction cannot be assumed. A UDL classroom may present accessible materials yet provide insufficient individualized feedback, whereas a differentiated classroom may provide tailored worksheets without ensuring that learners can independently communicate or join peer discussion.

The existing evidence supports both promise and caution. Rao *et al*. (2014) [36] reported variation in how universal design models were operationalised; Ok *et al*. (2017) [35] found encouraging but mixed outcomes across school-level UDL studies; and Capp (2017) [7] found support for the teaching process while cautioning that strong learning-outcome claims were not demonstrated. Reviews of differentiation similarly highlight operational inconsistency and the centrality of teacher expertise (Deunk *et al*., 2018; Lindner & Schwab, 2020) [10, 27]. These findings justify a design in which the intervention components and participation outcomes are specified before analysis.

For a Ghanaian research setting, the classroom environment requires attention to teacher workload, instructional language, resource availability, teacher support and the experiences of families. These contextual features should not be treated as presumed characteristics of any particular school. Rather, site-level audits and co-designed implementation plans are necessary before a trial is launched. The University of Education, Winneba provides an appropriate academic affiliation for the present authorship, but no participating institution, ethical approval or recruitment record is claimed in this manuscript.

This article has a deliberately bounded purpose. It presents a 2022-era study design with a computational proof of concept, not a retrospective report of fieldwork. The experiment is reproducible, the assumed data-generating mechanism is public, and the interpretation explicitly separates design plausibility from clinical or educational effectiveness. Such transparency prevents illustrative numerical patterns from being mistaken for outcomes observed among real pupils.

1.1. Research Objectives and Hypotheses

The primary objective is to specify an accessible measure of active participation and an identification strategy for testing the incremental effect of UDL, DI and their combination. Secondary objectives are to examine heterogeneity across support needs, evaluate implementation fidelity, quantify cluster uncertainty and assess whether differential attrition or measurement differences could alter conclusions. The prospective hypotheses are that each strategy improves participation relative to usual practice and that the combined approach produces an additional effect beyond the sum of its components; the latter is a formal interaction hypothesis, not an assumed truth.

2. Literature Review and Conceptual Framework

2.1. Participation as an Educational Outcome

Participation is an interaction between the person, the activity and the immediate environment. Counting the total number of spoken answers can misclassify learners whose communication occurs through gestures or assistive devices. A defensible observation protocol therefore defines an opportunity as an instructional invitation to engage and defines a successful response using individualized but functionally equivalent modes. Observation should encompass lesson episodes rather than only a single teacher-led exchange.

Observed involvement should be distinguished from passive compliance. Remaining quiet, facing the board or copying another student may be relevant to classroom management but does not necessarily indicate purposeful participation (Bamidele, *et al*., 2025; Abolade, *et al*., 2024) [1, 5]. A participation measure can instead combine initiation, response to instruction, sustained task engagement and reciprocal peer interaction, provided each element is observable, culturally appropriate and accessible.

2.2. Universal Design and Access to Learning

CAST (2018) [8] specifies flexibility in how information is accessed, how learners are engaged and how understanding is expressed. This implies practical design features such as pictorial task sequences, multiple response routes, predictable routines, accessible materials and opportunities for choice. The intervention cannot be reduced to supplying a tablet or producing multiple worksheets; it must alter the expected opportunity to engage with substantive instructional content (Ogundola, 2022; Ijiga, & Akinleye, (2024); Akinleye, & Ijiga, 2025).

Rao *et al*. (2014) [36] emphasised that universal design studies differed substantially in intervention description. Ok *et al*. (2017) [35] similarly reported heterogeneity across the settings and outcome measures of school studies. For this reason, this design records component delivery by session, materials provided, learner choices and observable response opportunities, linking fidelity to the mechanism rather than to a generic endorsement of inclusive practice.

2.3. Differentiated Instruction and Tailored Support

Differentiated instruction is structured response to learner variability rather than a permanent division of pupils into high- and low-performing tracks. It can modify the content, process, product or environment of a lesson, while the learning objective remains coherent. Examples relevant to intellectual disability include task analysis, graduated prompting, reduced response complexity, shorter work intervals, peer-mediated support and differentiated performance demonstrations (Tomlinson, 2014) [40].

Deunk *et al*. (2018) [10] found that the meaning of differentiation was inconsistently specified in empirical work. Lindner and Schwab (2020) [27] reported the importance of collaboration, grouping, modified materials and feedback in inclusive settings. Accordingly, this paper treats responsiveness, teacher decision rules and support fading as measurable components rather than relying on a teacher's self-description as differentiated.

2.4. Why an Interaction Model Is Warranted

The mechanisms of UDL and DI may be complementary: universal options increase the number of accessible entry points, while differentiated support makes those options manageable for a particular learner. Complementarity would appear statistically as a positive interaction between experimental factors after accounting for baseline participation and cluster assignment (Doe Gadekah, & Enyejo, 2025) [11]. It would be inappropriate to infer complementarity merely because the combined group had the highest unadjusted mean.

Digital and multimedia resources may widen some learner response options but can also introduce accessibility and infrastructure barriers. The examination of low-bandwidth STEM learning by Ijiga *et al*. (2022) [24] is pertinent to implementation feasibility, not direct evidence of UDL or differentiated instruction effects in learners with intellectual disabilities.

An alternative is redundancy, whereby the first strategy resolves most barriers and the second adds little. Another possibility is burden: introducing many materials and individualized tasks may exceed teacher capacity and reduce implementation fidelity (Ogundola, 2020; Frank, 2026) [16, 31]. A factorial design is particularly informative because it can distinguish main effects from synergy and evaluate whether added complexity produces meaningful gains.

2.5. Evidence Gaps and Temporal Scope

The multilingual-classroom work by Ijiga *et al*. (2021a) [22] provides a related contextual argument for accessible and locally intelligible instruction, although its STEM setting and language emphasis differ from intellectual-disability-specific classroom participation. Their investigation of digital storytelling (Ijiga *et al*., 2021b) [23] points to additional means of representation and expression, but should not be interpreted as testing the present factorial intervention.

The relevant pre-2023 research leaves gaps in consistent operational definitions, blinded observation, cluster-aware inference, and disaggregation by communication mode and support needs. In a 2022 manuscript, it would be anachronistic to cite later UDL guideline revisions or post-2022 meta-analyses as though available at the time. Accordingly, the theoretical justification uses the 2018 CAST guidance alongside available reviews and established experimental-design literature.

Of the requested author network, three identifiable 2021–2022 education publications by Ijiga and collaborators are potentially pertinent to this study: multilingual classroom inclusion, digital storytelling, and accessibility in low-bandwidth learning environments. These publications inform implementation considerations rather than supplying evidence of treatment effectiveness for intellectual disability. Other works by the named researchers could not be incorporated without crossing the 2022 date boundary or introducing tangential evidence.

3. Materials and Methods

3.1. Design, Simulation Status and Allocation

The computational demonstration represents 24 classrooms, each containing 10 hypothetical learners, randomly assigned at classroom level to four conditions: usual instruction, UDL only, DI only and combined UDL plus DI. Six classrooms per condition are represented. The complete cohort includes 240 synthetic learner records followed from week 0 to week 12. All individual and classroom records are algorithmically generated; there are no human subjects, consent forms, diagnostic records or school permissions associated with this demonstration. An actual trial would require institutional ethics approval and school-level permissions before participant contact.

The factorial assignment enables separate estimation of UDL, DI and their interaction. Classroom randomisation reduces cross-arm spillover within lessons, although teachers and pupils could still exchange materials across classes. Before any field recruitment, the allocation sequence, masking of observers, contamination monitoring and adverse-event procedures should be registered. The article models these protections but does not imply they were implemented.

$$A_c = (U_c, D_c) \in \{(0,0),(1,0),(0,1),(1,1)\}, \qquad \Pr(A_c = a) = \tfrac{1}{4} \qquad (1)$$

where A_c denotes classroom-level treatment; U_c is the UDL indicator; D_c is the DI indicator; a is any of the four allocation pairs; c indexes classrooms; and $\Pr$ denotes probability.

3.2. Participants, Inclusion and Accessibility

For a prospective study, eligible learners would attend participating inclusive classrooms, have documented support needs consistent with intellectual disability, and be able to take part in the observation protocol using their ordinary communication supports. Classification would rely on locally approved educational and clinical procedures rather than a one-off teacher judgment. Exclusion criteria must be narrow: difficulty with spoken language, assistive communication or complex behaviour should prompt an accommodation assessment rather than automatic exclusion.

Caregivers and learners should receive accessible information in the language and communication mode they use. The protocol should provide assent procedures, the right to withdraw, safeguarding procedures and secure pseudonymization. Where a learner communicates through gestures, pictures or a device, observers must code communicative acts according to their intended function and not treat absence of speech as absence of classroom participation.

3.3. Intervention Delivery and Treatment Differentiation

The UDL intervention uses a planning checklist for multiple means of representation, engagement and expression. Each lesson includes at least two accessible content presentations, two meaningful response formats and one supported choice opportunity. The DI intervention requires an initial task analysis, individualized prompting plan, flexible pacing and documented adjustment following formative observation. The combined arm implements both sets of procedures; the comparator continues established school practice without withdrawing ordinary accommodations.

Teachers would receive standardised professional learning, modelled practice and recurring feedback. Fidelity should be assessed by independent observations using component-specific rubrics. Common implementation barriers include

time pressure, competing curriculum requirements, limited tactile or visual materials, and inconsistent access to communication aids. Practical mitigation includes low-cost task cards, reusable visual schedules, co-planning templates and peer-supported classroom roles.

Table 1: Factorial intervention conditions and differentiating delivery components.

Arm	Universal lesson options	Responsive differentiation	Core implementation record
Usual practice	Existing provision	Existing provision	Routine lesson log
UDL	Multiple means of access, response and engagement	Not protocol-driven	UDL component checklist
DI	No new universal package	Adapt task, pace, grouping and prompt levels	Individual adjustment register
UDL + DI	Full UDL package	Full DI package	Combined adherence and burden log

3.4. Participation Indicator and Measurement Model

The primary outcome is a proportion of eligible instructional opportunities in which the learner demonstrates an active, purposeful response. Observation windows should sample teacher-directed instruction, independent work and peer exchange. Each eligible opportunity is defined in advance; denominator changes should be inspected because an intervention can increase the opportunity count as well as successful participation. The computational version fixes 20 opportunities per learner-week to isolate the response-rate mechanism.

$$Y_{ict} = \frac{\sum_{j=1}^{J_{ict}} w_j R_{icjt}}{\sum_{j=1}^{J_{ict}} w_j O_{icjt}}, \quad 0 \le Y_{ict} \le 1 \tag{2}$$

where Y_{ict} is participation for learner i in classroom c at time t; j indexes participation opportunity types; J_{ict} is their number; R_{icjt} counts successful responses; O_{icjt} counts eligible opportunities; and w_j is a nonnegative prespecified domain weight.

To evaluate whether a composite score represents a coherent construct, a confirmatory latent factor model may be fitted to observed domain indicators. Thresholds rather than raw total scores would be appropriate when an item is ordinal, and measurement invariance should be tested across communication modes and treatment groups.

$$Z^*_{icjt} = \lambda_j \eta_{ict} + \alpha_j + \varepsilon_{icjt}, \qquad Z_{icjt} = m \Leftrightarrow \tau_{j,m-1} < Z^*_{icjt} \le \tau_{jm} \tag{3}$$

where Z^*_{icjt} is a latent continuous response underlying observed ordinal category Z_{icjt}; λ_j is factor loading; η_{ict} is latent participation; α_j is an item intercept; ε_{icjt} is measurement error; m identifies a response category; and τ_{jm} are ordinal cut-points.

3.5. Binomial Hierarchical Outcome Model

A multilevel binomial likelihood is preferable to conventional analysis of untransformed percentages when denominators differ and rates approach zero or one. Classroom intercepts account for shared teaching conditions; learner intercepts capture repeated measurements, and the three factorial treatment terms distinguish main effects from complementarity. Time can be represented with a spline to avoid assuming a perfectly linear response trajectory. Equation (4) gives the observation distribution.

$$K_{ict} \mid p_{ict}, n_{ict} \sim \text{Binomial}(n_{ict}, p_{ict}), \quad \text{logit}(p_{ict}) = \log \frac{p_{ict}}{1-p_{ict}} \tag{4}$$

where K_{ict} is the count of active responses; n_{ict} is the number of opportunities; p_{ict} is the response probability; Binomial is the binomial distribution; and logit is the log-odds link.

The conditional response probability is expressed in Equation (5). A learner-specific baseline term and classroom intercept reduce confounding from stable individual and class differences. Prespecified interaction between time and combined treatment tests the substantive claim. A flexible temporal basis is included for robustness, while the main simulation uses a transparent progression rule.

$$\text{logit}(p_{ict}) = \beta_0 + b_c + u_{ic} + \sum_{r=1}^{R} \gamma_r B_r(t) + \beta_U U_c + \beta_D D_c + \beta_{UD} U_c D_c + t(\delta_U U_c + \delta_D D_c + \delta_{UD} U_c D_c) + \theta^\top X_{ic} \tag{5}$$

where β_0 is the intercept; b_c and u_{ic} are classroom and learner random intercepts; $B_r(t)$ are temporal spline functions, with coefficients γ_r and R basis terms; t is time; β_U, β_D and β_{UD} are treatment contrasts; δ_U, δ_D and δ_{UD} are time interactions; and X_{ic} and θ are covariates and their coefficients.

3.6. Factorial Marginal Contrasts

Marginal estimands must be defined on an interpretable response-probability scale rather than equating odds ratios with probability differences. Standardised predictions can be averaged over the target learner distribution. The three contrasts include the UDL main effect, the DI main effect and the additional effect of delivering both programmes beyond additivity. Equation (6) defines the interaction in terms of counterfactual mean participation.

$$\Delta_{UD} = \mathbb{E}\{Y(1,1) - Y(1,0) - Y(0,1) + Y(0,0)\} \tag{6}$$

where Δ_{UD} is the factorial interaction; $Y(u,d)$ is the potential participation rate under UDL assignment u and DI assignment d; and $\mathbb{E}$ denotes expectation over the target learner population.

The cluster assignment, not the number of weekly observations, determines the independent units of treatment allocation. Treating each repeated record as independent would understate uncertainty. The random-effects framework

addresses within-class and within-learner dependence, while a cluster bootstrap provides a complementary uncertainty check.

3.7. Support-Needs Heterogeneity

Intellectual disability is not a unitary behavioural profile. The analysis should stratify, where feasible, by educational support needs, communication mode, age band and baseline participation without conflating these characteristics with immutable capacity. A partially pooled model is preferred over many small subgroup regressions because it stabilises estimates and makes uncertainty visible.

$$\delta_{UD,s} \mid \mu_{UD}, \sigma_{UD} \sim \mathcal{N}(\mu_{UD}, \sigma_{UD}^2), \qquad \hat{\delta}_{UD,s} = \omega_s \hat{\delta}_{UD,s}^{\text{raw}} + (1-\omega_s)\hat{\mu}_{UD} \tag{7}$$

where s indexes support-needs strata; $\delta_{UD,s}$ is a stratum-specific combined treatment effect; μ_{UD} and σ_{UD} are its mean and dispersion; $\mathcal{N}$ denotes a normal distribution; ω_s is a shrinkage weight; and hats mark estimates.

3.8. Fidelity, Mediation and Contamination

Teacher delivery is measured at classroom level, distinct from student participation. Fidelity items record whether response alternatives were offered, task steps were adjusted as specified, prompts were faded appropriately and feedback occurred. Fidelity should be treated primarily as a process measure because conditioning a causal analysis on post-randomisation delivery can introduce bias. A secondary descriptive dose-response analysis is useful but should not be described as a randomised estimate.

$$F_c = \frac{\sum_{h=1}^{H} \rho_h I_{ch}}{\sum_{h=1}^{H} \rho_h}, \qquad I_{ch} \in [0,1],\ \rho_h > 0 \tag{8}$$

where F_c is observed classroom fidelity; h indexes implementation components; H is the number of components; I_{ch} is the attained score for component h in classroom c; and ρ_h is its prespecified positive weight.

A prospective causal mediation analysis would additionally require assumptions about mediator-outcome confounding and temporal ordering that are not satisfied simply by observing a fidelity correlation. Therefore, the fidelity scatterplot below is a diagnostic illustration, not proof that fidelity mediates participation improvement.

3.9. Missingness, Selection and Sensitivity

Field observations can be missing because a learner is absent, an observer cannot access a classroom or an accommodation is temporarily unavailable. These mechanisms may be related to participation itself. A sensitivity analysis should examine multiple imputation, inverse-probability weighting and bounded missing-not-at-random scenarios. A complete-case analysis would be insufficient if absence was correlated with classroom difficulty or learner support needs.

$$\hat{\mu}_a^{\text{IPW}} = \frac{\sum_{i,c,t} \mathbf{1}(A_c = a)\, M_{ict} Y_{ict} / \hat{\pi}_{ict}}{\sum_{i,c,t} \mathbf{1}(A_c = a)\, M_{ict} / \hat{\pi}_{ict}} \tag{9}$$

where $\hat{\mu}_a^{\text{IPW}}$ is the inverse-probability-weighted arm mean; $\mathbf{1}$ is the indicator function; A_c is assignment; a is an arm; M_{ict} indicates observation; Y_{ict} is participation; and $\hat{\pi}_{ict}$ is the estimated probability of observation.

3.10. Cluster Sample Size and Detectable Effects

Any real experiment must be sized to estimate the prespecified contrast after accounting for clustering. An initial design effect approximation is useful, although a factorial longitudinal design requires simulation-based power analysis informed by plausible class and learner variances. Without pilot data, a claimed minimum detectable educational effect would be speculative.

$$\text{DEFF} = 1 + (\bar{m} - 1)\rho, \qquad n_{\text{eff}} \approx \frac{n}{\text{DEFF}} \tag{10}$$

where DEFF is the cluster design effect; $\bar{m}$ is the mean learners per classroom; ρ is the intraclass correlation; n is total learners; and n_{eff} is an approximate independent-equivalent sample size.

For the planned combined-versus-control primary comparison, an approximate allocation-sensitive variance of the estimated contrast can be computed from classroom means. This approach makes the effective number of clusters explicit rather than relying on the much larger number of observation occasions.

$$\widehat{\text{Var}}(\hat{\Delta}) = \sum_{a \in \{00,10,01,11\}} \frac{s_a^2}{C_a}, \qquad \widehat{\text{SE}}(\hat{\Delta}) = \sqrt{\widehat{\text{Var}}(\hat{\Delta})} \tag{11}$$

where $\hat{\Delta}$ is a four-arm factorial contrast; s_a^2 is the sample variance of classroom-level outcome means in arm a; C_a is the classroom count in that arm; Var denotes variance; and SE denotes standard error.

3.11. Resampling and Robustness

A stratified cluster bootstrap resamples classrooms within randomisation arms and retains all learner observations belonging to the selected classroom. This preserves the experimental assignment unit and estimates the variability of treatment contrasts under clustering. The educational interpretation depends on the magnitude of a plausible change in participation, not merely on a nominal significance threshold.

$$\widehat{\text{CI}}_{1-\alpha} = [Q_{\alpha/2}(\hat{\Delta}^{*(b)}),\ Q_{1-\alpha/2}(\hat{\Delta}^{*(b)})]_{b=1}^{B} \tag{12}$$

where $\widehat{\text{CI}}_{1-\alpha}$ is a percentile confidence interval with coverage $1-\alpha$; Q_q is the q quantile of a bootstrap distribution; $\hat{\Delta}^{*(b)}$ is the contrast from resample b; and B is the number of resamples.

3.12. Reproducible Simulation Protocol

The reference implementation uses NumPy, pandas and Matplotlib with a fixed seed of 20221105. Twenty opportunities are generated per synthetic learner-week; responses arise from a binomial distribution with classroom offsets, support-needs variation, short-term fluctuation and an arm-specific time trend. The trends deliberately make the combined strategy most favourable. This construction is useful for testing whether tables and graphics detect the

intended pattern, but not for estimating causal effects from empirical educational data.

An audit record includes the full source code, raw synthetic panel, derived change scores and summary statistics. Regenerating the outputs requires Python and the named scientific packages; the code contains no hidden external data or participant records. Because the model is intentionally simple in its generation mechanism, the methodological equations above should be understood as an intended analytical strategy for a future real study, not all as fitted inferential models in this demonstration.

4. Computational Results (Synthetic Data Only)

All results in this section are outputs of the explicitly parameterised simulation. They are not observations from Ghanaian classrooms, and their confidence intervals describe synthetic resampling variation, not uncertainty about a true intervention effect in the population. Comparisons are presented to illustrate measurement and interpretation within the proposed four-arm design.

4.1. Baseline-To-Follow-Up Participation

Across 240 synthetic learners allocated to 24 classrooms, the mean simulated participation change was −0.008 for usual practice, 0.085 for UDL alone, 0.073 for DI alone and 0.188 for the combined condition. Figure 1 traces weekly group means, while Table 2 summarises their change distributions. The generated response surface reflects a greater programmed gain for the combined condition and a zero-drift comparator. No effect-size statement in this section should be interpreted as an observed pedagogical benefit.

Table 2: Descriptive synthetic participation changes from week 0 to week 12.

Simulation arm	Learners	Mean change	SD of change
Usual practice	60	-0.008	0.140
UDL	60	0.085	0.151
Differentiation	60	0.073	0.144
UDL + DI	60	0.188	0.152

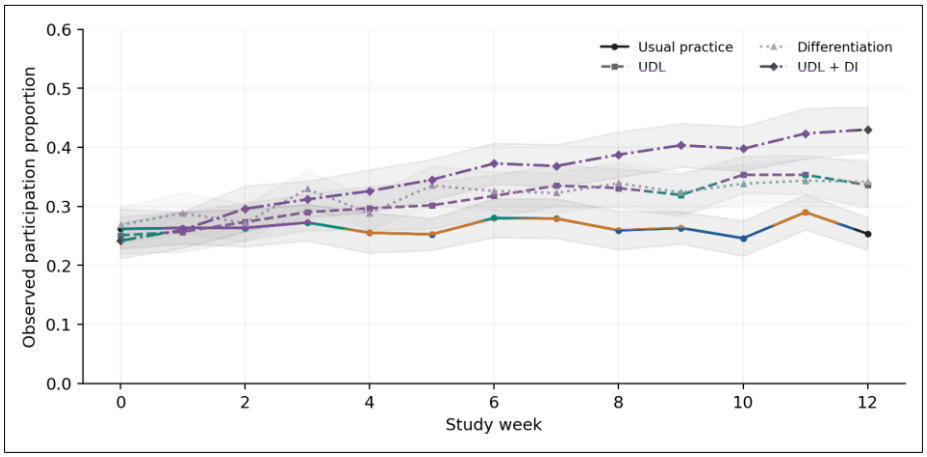


Fig 1: Average participation trajectories by experimental arm, with learner-level standard-error bands. Note. Values are generated from a synthetic teaching dataset; they are not observations from a school.

4.2. Individual Variability and Support Needs

Figure 2 demonstrates why a mean difference alone is inadequate. Within every arm, the distribution of learner changes overlaps zero and includes variable responses. In real classrooms, such heterogeneity could reflect changing learning tasks, communication accommodations, teacher relationships or episodic health and attendance effects. In the synthetic dataset it primarily reflects the binomial observation mechanism and randomly generated classroom-level offsets.

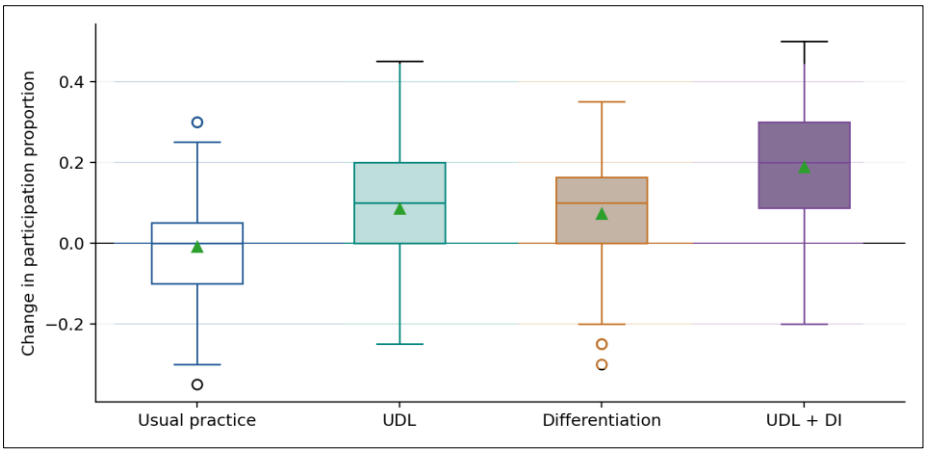


Fig 2: Distributions of within-learner change by intervention arm. Note. Values are generated from a synthetic teaching dataset; they are not observations from a school.

Stratification by support level in Figure 3 is illustrative. Because support needs alter the simulation slope, the expected gain decreases as support intensity increases. That assumption must not be treated as an established empirical relationship; a field study should specifically investigate whether adequately individualised supports eliminate, reverse or otherwise change such differences.

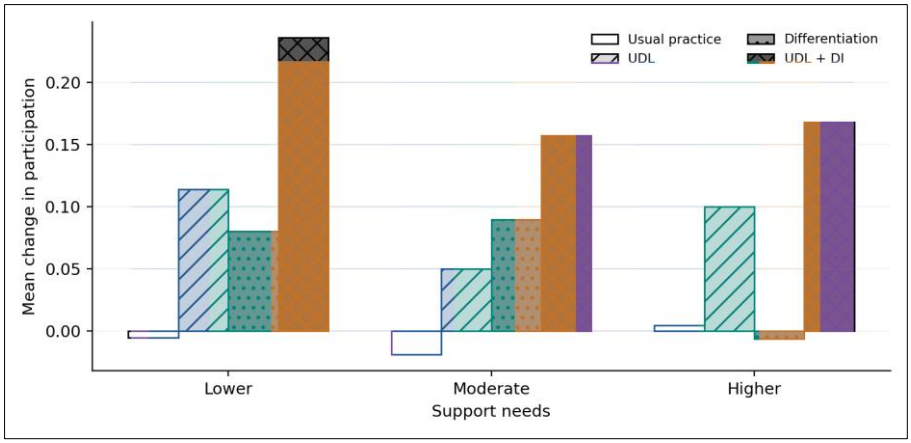


Fig 3: Simulated mean changes across three ordinal support-needs groups. Note. Values are generated from a synthetic teaching dataset; they are not observations from a school.

4.3. Implementation Fidelity and Apparent Dose Response

The simulated fidelity values are centred at a higher level for programme arms than for usual practice. Figure 4 depicts the association between classroom fidelity and participant change across active arms. A visible association does not separate teacher competence, implementation support or baseline classroom characteristics. Accordingly, it should guide the design of a fidelity audit rather than be interpreted as an unbiased mediated effect.

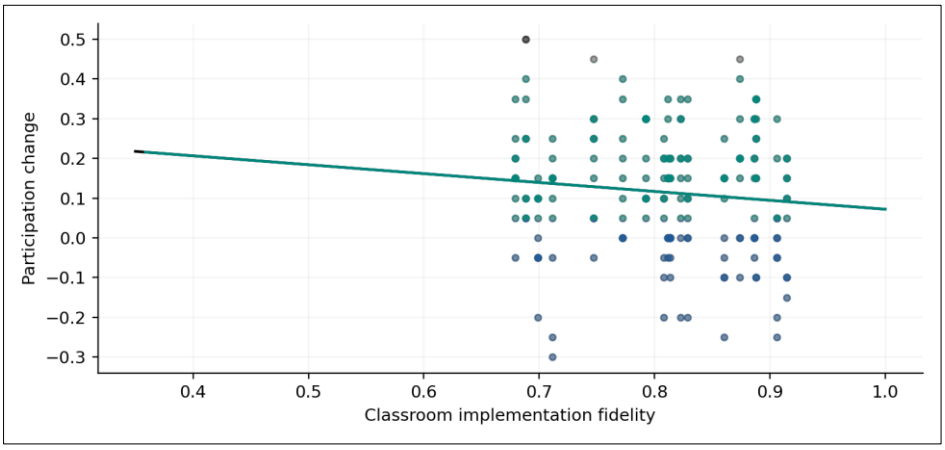


Fig 4: Classroom fidelity and synthetic learner-level participation change across active interventions. Note. Values are generated from a synthetic teaching dataset; they are not observations from a school.

4.4. Comparative Contrasts and Uncertainty

Figure 5 reports bootstrap contrasts in the illustrative data-generating environment. The bootstrapping of learner change scores in this figure provides a descriptive demonstration only. For a real trial, classrooms rather than pupils must be resampled to honour assignment and shared environments, as specified in Section 3.11. This difference between the demonstration graphic and the inferential plan is explicit to prevent a misleading impression of field-level precision.

4.5. Observation Schedule and Interpretability

Figure 6 displays mean participation at weeks 0, 4, 8 and 12. In a genuine trial, the observation schedule should be aligned with teaching terms and subjected to a prespecified protocol for holidays, absences and changes in instructional content. The synthetic trajectories show how repeated outcomes can support assessment of temporal variation, but the direction and size of their movement are imposed through the model inputs.

5. Discussion

5.1. Educational Interpretation of the Proposed Intervention

The central conceptual contribution is the separation of accessible classroom opportunity from differentiated support and from participation itself. UDL is not a substitute for identifying particular learners' instructional needs, and DI is not necessarily accessible if it relies on one response channel. The factorial design makes this separation observable by requiring teachers to document features delivered in each condition and by specifying an interaction estimand rather than assuming that combining approaches is always superior. The synthetic dataset is consistent with an illustrative scenario in which complementary supports increase

successful response opportunities. Its generated advantages are neither a finding about learners with intellectual disabilities nor proof of programme efficacy. Empirical confirmation would require masked observation, adequate cluster replication, representative schools and meaningful estimation of the expected interaction on a probability scale. The field study should also report absolute opportunity counts because a high response percentage can coexist with very few invitations to participate.

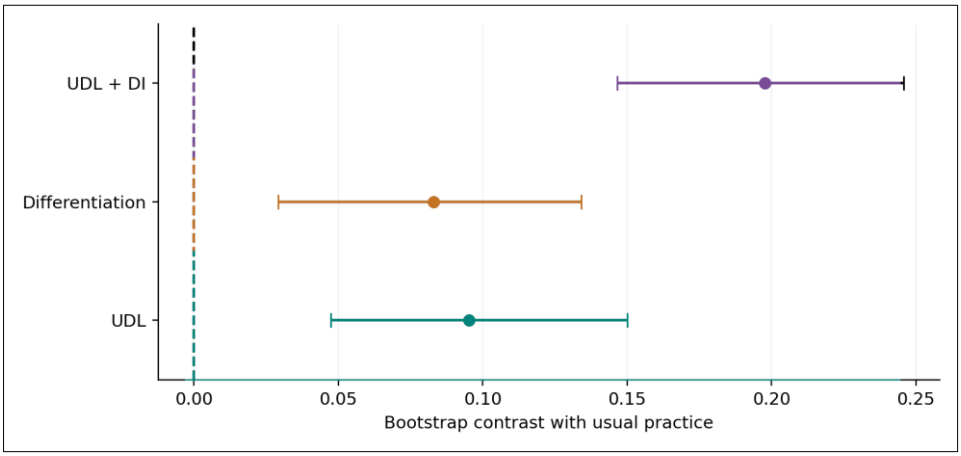


Fig 5: Synthetic bootstrap contrasts of change scores against usual practice. Note. Values are generated from a synthetic teaching dataset; they are not observations from a school.

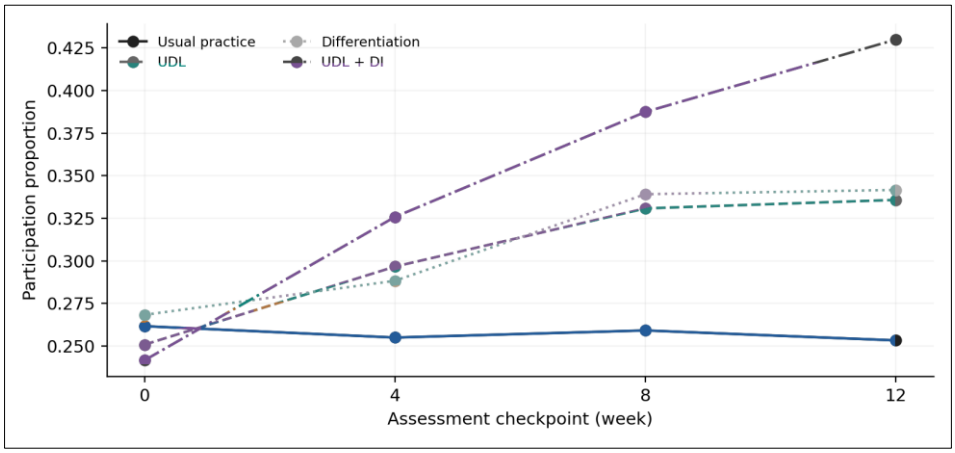


Fig 6: Simulated group means at baseline and three follow-up checkpoints. Note. Values are generated from a synthetic teaching dataset; they are not observations from a school.

5.2. Relation to pre-2023 Evidence

The argument aligns with the emphasis on curriculum accessibility in the CAST (2018) [8] framework and with the incomplete but promising evidence base reported by Rao *et al.* (2014) [36], Capp (2017) [7] and Ok *et al.* (2017) [35]. Consistent with the differentiation literature, a detailed implementation record is necessary because generic programme labels conceal differences in instruction (Deunk *et al.*, 2018; Lindner & Schwab, 2020) [10, 27]. The literature does not justify predicting a universal effect, and this article does not assert one.

Related work on cross-cultural inclusion and multimodal educational participation (Ijiga *et al.*, 2021a, 2021b) [22, 23] reinforces the need to specify what constitutes an accessible classroom response. The 2022 analysis of remote STEM learning environments (Ijiga *et al.*, 2022) [24] further draws attention to resource constraints. These connections concern design considerations; they do not validate the simulated effects reported in Section 4.

It is particularly important to distinguish classroom participation from standardised attainment. Greater response frequency may create conditions for learning, social belonging and self-determination, but it does not guarantee improved curriculum mastery. A future trial should independently assess learning outcomes, communication autonomy, peer inclusion and pupil satisfaction. An excessive focus on the quantity of responses might inadvertently encourage superficial turn-taking while neglecting quality and independence.

5.3. Implications for Ghanaian Inclusive Classrooms

A locally implemented programme would need collaborative planning with specialist teachers, classroom teachers, caregivers and learners using accessible consultation methods. Instructional examples should be selected from locally taught content and classroom languages, and low-cost, reusable learning supports should be preferred where materials are constrained. Local adaptation should be documented systematically instead of being inferred from broad claims about national educational conditions.

Teacher professional development should include live modelling, observation-based feedback, practice with

augmentative communication and a method for checking whether choices remain genuine rather than tokenistic. Classroom participation also depends on peer interaction. Therefore, the intervention should examine cooperative tasks, peer mentoring safeguards, and opportunities to make decisions without creating dependence on a single supporting adult.

5.4. Interpretation and Methodological Limitations

The most consequential limitation is the absence of field data. Neither the assumed trends nor the apparent interaction can be described as experimentally verified. The fixed number of response opportunities, simplified support-needs categories and high-fidelity simulation do not reflect the full complexity of actual classrooms. Classroom randomisation can control assignment-related confounding, but noncompliance, assessor bias and spillover remain possible in any field implementation.

Another limitation is the difference between the rich multilevel statistical specification and the relatively simple synthetic generation process. That distinction is intentional: the equations articulate candidate analyses while the Python code validates basic data structure, graphical outputs and certain estimands. Bayesian estimation, measurement invariance and missing-not-at-random sensitivity were not fitted here. They remain planned analyses requiring prespecified priors, model checking and adequate observed sample size.

5.5. Next Empirical Steps

Before recruitment, investigators should pilot the opportunity coding system using video examples and independent observers. Agreement should be evaluated across communication modalities and classroom activities; ambiguous categories should be revised with teacher and learner input. A feasibility phase should record consent acceptability, observation burden, fidelity stability and attrition so that the eventual cluster sample-size simulation has credible inputs.

An adequately powered trial should then register the primary contrast, record implementation deviations, use independently audited outcome coding and present estimates with cluster-aware intervals. Findings ought to be disseminated in accessible formats for participating schools and families. Positive results would support a context-specific claim about measured participation under the implemented conditions, not a claim that all learners with intellectual disabilities respond identically.

6. Conclusions and Recommendations

UDL and differentiated instruction offer complementary design principles for addressing the gap between physical placement and meaningful classroom participation. A credible evaluation must measure accessible response opportunities, document intervention fidelity and estimate factorial effects using classrooms as the units of assignment. The proposed analysis combines repeated-measures binomial modelling, multilevel heterogeneity, cluster-aware uncertainty, missingness sensitivity and explicit interpretation of individual variability.

The reproducible demonstration shows how a research team can organise these steps, but its generated group differences provide no evidence that a real 2022 intervention was conducted or that either approach improved outcomes for actual pupils. Priority should be given to ethically approved pilot work, validated classroom participation observation, local co-design and prospective randomised evaluation. The most defensible contribution of this manuscript is a transparent study architecture and a reusable computational test bench.

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