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# Building a Research Agenda for Cognitive Load Theory in Special Education

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**Abstract**

Interest in Cognitive Load Theory (CLT) within special education has grown steadily over the past several years, and the demand for evidence-based instruction has grown alongside it. Yet the research produced so far has emerged in a scattered way, without any shared sense of priority or direction. This paper proposes a coordinated research agenda meant to guide the field's development over the next decade. It summarizes the current state of the field, identifies the major gaps that still need attention, and organizes a set of research priorities into seven strategic pillars: theory development, intervention research, measurement and assessment, technology integration, teacher development, systems and policy, and evidence synthesis. It then lays out a three-phase timeline for pursuing these priorities in a realistic order, assigns responsibilities to the different groups who would need to act on this agenda, and names the practical challenges likely to slow progress along the way. The paper closes with a set of measurable indicators that would signal whether the field is actually maturing. Although studies applying Cognitive Load Theory in special education are increasing, the field lacks a coordinated research agenda capable of guiding cumulative scientific progress. This paper is an attempt to propose one.

**Keywords**: cognitive load theory, research agenda, special education, strategic planning, evidence-based practice

## Introduction

Cognitive Load Theory has quietly become one of the more active areas of interest connecting educational psychology to special education. Researchers have measured cognitive load directly in students with dyscalculia (Ahuja et al., 2021), traced working memory limitations across dyslexia, dysgraphia, and intellectual disability (Stein, 2023; Danna et al., 2023; Gupta, 2022), and begun testing whether AI-driven tools and adaptive software can be designed around load-sensitive principles from the outset (Almufareh et al., 2023; Dhingra et al., 2022). At the same time, the broader push for evidence-based instruction in special education has never been stronger, with journal editors, funding agencies, and school systems all asking for practices that rest on solid research rather than tradition or intuition (Cook & Cook, 2013).

These two trends should be reinforcing one another, and to some extent they are. But a closer look at how this research has actually developed reveals a field that is busy without being organized. A study on cognitive load in autism and a study on cognitive load in teacher training might never cite each other, despite drawing on the same underlying theory, simply because they were published in different journals aimed at different audiences. A researcher beginning a dissertation on CLT and dyslexia today has no obvious document to consult that tells them which questions the field considers most urgent, which methods are most needed, or which populations remain almost entirely unstudied. Individual researchers are making individually reasonable

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choices about what to study next, but nobody has stepped back to ask whether those choices, added together, are building toward anything coherent.

The practical cost of this fragmentation is easy to underestimate because it rarely shows up as a dramatic failure. It shows up instead as a slow accumulation of missed opportunities: a promising small study that nobody replicates, a measurement tool built for one disability population that a researcher working with a different population never discovers, a teacher training programme that reinvents load-sensitive guidance from scratch because nobody pointed its designers toward the relevant literature. None of these individual gaps looks catastrophic on its own. Together, over a decade, they add up to a field that has produced far less cumulative knowledge than the volume of individual publications would suggest, simply because so little of that work has been coordinated to build on what came before it.

This is precisely the situation a research agenda is meant to address. Although studies applying Cognitive Load Theory in special education are increasing, the field lacks a coordinated research agenda capable of guiding cumulative scientific progress. The remainder of this paper works toward building one. Section two explains why research agendas matter, drawing on examples from other fields that have benefited from exactly this kind of coordination. Section three summarizes the current state of CLT research in special education without repeating a full systematic review. Section four organizes the major gaps that still need attention. Section five presents the proposed research agenda itself, divided into seven strategic pillars. Section six lays out a three-phase timeline for pursuing these pillars in a realistic order. Section seven assigns responsibilities to the different stakeholders who would need to act on this agenda. Section eight names the practical challenges likely to slow progress and suggests ways to address them. Section nine proposes measurable indicators of whether the field is maturing. Section ten closes the paper.

**Why Research Agendas Matter**

A research agenda is, at its core, a shared statement of priorities, a document that tells a scattered community of researchers where the field's most pressing questions lie and roughly what order they should be tackled in. This might sound like a modest administrative exercise, but published research agendas have played a meaningful role in several fields that special education can learn from directly.

Reading research offers one of the clearest examples. In 2000, the National Reading Panel published a comprehensive evaluation of the scientific evidence on how children learn to read, organized around a small set of priority topics: phonemic awareness, phonics, fluency, vocabulary, and comprehension (National Reading Panel, 2000). That report did not just summarize existing evidence. It gave the field a shared vocabulary and a shared sense of which questions mattered most, and it shaped how reading research and reading instruction were funded and practiced for the following two decades. Autism research offers a more recent and more explicitly agenda-driven example. The Interagency Autism Coordinating Committee has published and regularly updated a formal strategic plan for autism research since 2009, most recently the 2021-2023 update, organizing federal research priorities around a consistent set of questions covering identification, underlying biology, services, and life course outcomes (Interagency Autism Coordinating Committee, 2023). That plan does not conduct research itself, but it shapes which studies get

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funded and gives individual researchers a way to position their work relative to a shared national priority list rather than working in isolation.

What both of these examples show is that research agendas serve several distinct functions at once. They prevent fragmented research by giving individual researchers a shared reference point, reducing the odds that valuable effort gets spent answering questions the field has already answered elsewhere. They help avoid unnecessary duplication, since a visible agenda makes it easier to see who else is already working on a given question. They identify priority questions explicitly, rather than leaving that judgment to each researcher's individual guess about what matters. They guide funding agencies, who need some basis for deciding which grant proposals to support when resources are limited. They inform doctoral research, giving graduate students a menu of genuinely useful dissertation topics rather than leaving them to stumble onto a worthwhile question through trial and error. And they support interdisciplinary collaboration, since a shared agenda gives researchers from cognitive psychology, special education, computer science, and policy studies a common document to organize around, even when their individual training and vocabulary differ considerably.

CLT research in special education sits at almost exactly the stage where a research agenda tends to matter most. The field is past its earliest years, when a handful of scattered papers would not have justified the effort of formal coordination. But it has not yet reached the kind of natural maturity where shared priorities have emerged through decades of accumulated convention, the way they eventually did in reading research after the National Reading Panel's work. This in-between stage, active enough to need direction but not yet organized enough to have found that direction on its own, is exactly when a proposed agenda has the best chance of actually shaping how the field grows, rather than simply describing growth that has already happened and calcified into habit.

It is worth adding a note of realism here as well. A research agenda does not guarantee coordination on its own, and both examples above illustrate this. The National Reading Panel's influence depended heavily on institutional backing, congressional funding, and a receptive policy environment that gave its recommendations teeth. The Interagency Autism Coordinating Committee's strategic plan works similarly, tied to specific federal legislation and a dedicated funding stream. CLT research in special education currently has no equivalent institutional home, no single agency charged with coordinating it, and no dedicated funding line tied explicitly to its priorities. This paper's proposed agenda is offered in that more modest spirit: not as a document with institutional authority behind it, but as a starting point that individual researchers, journal editors, and funding programmes could choose to adopt voluntarily, in the hope that enough of the field's participants doing so would produce something close to the coordination effect a more formally backed agenda might achieve.

## Current State of the Field

Rather than repeating a full systematic review, this section identifies the broad patterns visible in the existing body of CLT research connected to special education. Intervention studies remain few and mostly small in scale. Ahuja et al. (2021) tested cognitive load differences between dyscalculic and non-dyscalculic learners working with pedagogical agents, offering one of the more

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direct experimental examples currently available, but studies of this kind remain the exception rather than the norm across most disability categories.

Disability coverage is uneven. Dyslexia, dyscalculia, autism, and ADHD have each attracted a modest but growing body of work connecting cognitive processing, including working memory, to instructional needs (Stein, 2023; Mishra & Khan, 2022; Alnasser, 2023; Rajaprakash & Leppert, 2022; Le Cunff et al., 2023). Intellectual disability, hearing impairment, visual impairment, and multiple disabilities have received comparatively little attention through a CLT lens specifically, even though the underlying rationale, working memory constraints shaping how much instructional demand a learner can absorb at once, applies just as strongly to these populations.

Research methods lean heavily toward small, cross-sectional designs. Controlled experiments exist but remain rare (Ahuja et al., 2021), and qualitative or mixed-methods studies capturing how teachers and students actually experience cognitive load in real classrooms are almost absent. Systematic reviews and methodological critiques exist in adjacent fields, most notably computing education, where reviewers have found that many studies citing CLT reference outdated versions of the theory and apply it only superficially (Duran et al., 2022; Berssanette & de Francisco, 2022). If a field with strong methodological infrastructure struggles this much to apply CLT rigorously, special education, with fewer dedicated CLT researchers, likely faces the same problem to an even greater degree.

Geographic distribution skews heavily toward North America, Europe, and parts of East Asia. Studies from South Asia, the Middle East, and Sub-Saharan Africa remain rare, despite these regions serving enormous numbers of children with disabilities, often in classrooms where teachers have fewer resources available to compensate for poorly designed instruction. This geographic imbalance limits how confidently existing findings can be assumed to generalize outside the specific educational systems where most of the research has been conducted.

Common strengths of the existing literature include a consistent finding that reducing unnecessary extraneous load tends to help learners across nearly every disability category studied so far, even in work that never explicitly uses CLT vocabulary (Bishara, 2021; Skulmowski & Xu, 2021). Assistive technology built around load-sensitive design from the start, rather than added after the fact, also shows encouraging early results, as seen in tools like Mathlete for dyscalculia (Dhingra et al., 2022). Major weaknesses mirror what a strategic agenda needs to address directly: small samples, short intervention windows, almost no replication, heavy reliance on self-report measures not validated for special education populations, and very little research connecting cognitive load findings to actual classroom implementation or policy decisions.

Taken together, these patterns describe a field with real substance but no real spine. There is enough here, a consistent core finding about extraneous load, some promising early technology, a handful of methodologically sound experiments, to justify serious investment. There is not yet enough coordination, replication, geographic breadth, or population coverage to call this a mature research area. That gap between substance and structure is precisely what the rest of this paper tries to address.

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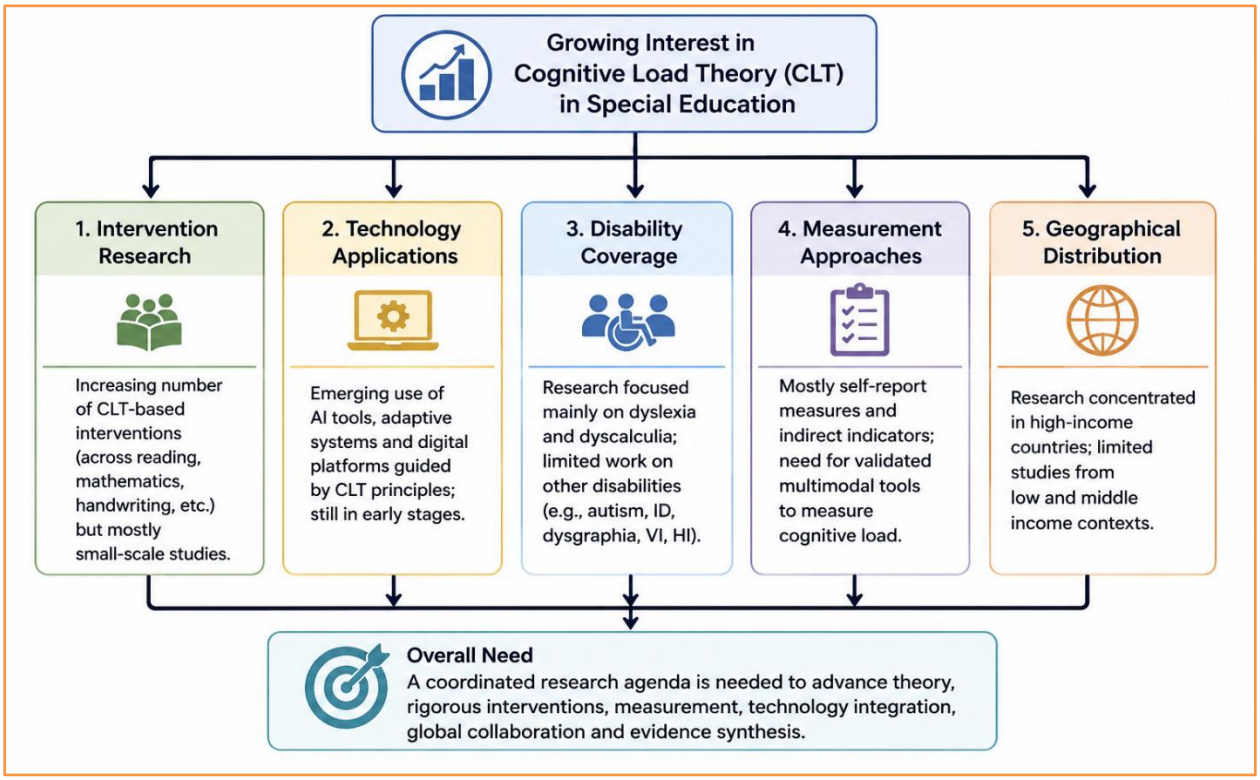
**Figure 1: Current State of CLT Research in Special Education**




## Major Gaps Requiring Attention

Organizing these weaknesses into distinct categories makes the scope of what remains to be done considerably clearer. Some of these gaps are already familiar from earlier conceptual work in this broader research programme, but naming them again here, specifically as gaps a research agenda needs to close rather than as observations about the current literature, changes what they call for. A gap noted in passing invites acknowledgment. A gap named as part of an agenda invites a plan.

**Theoretical gaps.** The field currently has limited conceptual models connecting CLT specifically to special education populations, and what models do exist are rarely integrated with the frameworks special educators already use daily, such as Universal Design for Learning or Response to Intervention. Weak theoretical integration means researchers studying cognitive load in one disability category often work in near-complete isolation from researchers studying the same underlying construct in a different disability category.

**Methodological gaps.** Small samples remain the norm rather than the exception, which is understandable given how heterogeneous and often small special education populations are, but it still limits how confidently findings can be generalized. Randomized trials are rare, and where they exist, they usually involve a narrow population and a short intervention window. Longitudinal studies tracking whether load-sensitive instruction produces lasting benefit are essentially absent. And replication, one of the most basic requirements for treating any single finding as reliable, has barely been attempted anywhere in this literature.

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**Measurement gaps.** Cognitive load measurement tools, including recently developed and validated questionnaires (Krieglstein et al., 2023) and broader surveys of workload measurement methods spanning physiological and behavioral approaches (Kosch et al., 2023), have mostly been built and tested with general education or adult populations. Very few of these instruments have been validated specifically for special education populations, whose communication styles, attention patterns, and physiological baselines may differ enough from typically developing samples that borrowed instruments do not transfer cleanly.

**Population gaps.** Several disability groups remain notably underrepresented in this literature, including intellectual disability, multiple disabilities, and hearing and visual impairment, despite each carrying a clear theoretical rationale for why cognitive load should matter (Gupta, 2022; Bishop & Gregory, 2022; Muradyan, 2023). Adult learners with disabilities are almost entirely absent from this research, even though many of the same cognitive constraints continue to shape adult education and vocational training. Early childhood populations are similarly underrepresented, despite early intervention being exactly the window where load-sensitive instruction could plausibly have the largest long-term impact.

**Contextual gaps.** Almost no research has examined how CLT principles function specifically within inclusive classrooms, where a single lesson must serve both disabled and non-disabled learners simultaneously. Low-resource settings, including many public sector special education systems in South Asia and other under-resourced regions, remain almost entirely outside the current evidence base, despite serving a large share of the world's children with disabilities. Cross-cultural evidence more broadly is thin, since most existing studies come from a narrow set of well-resourced countries.

**Technology gaps.** Artificial intelligence and adaptive learning systems represent one of the fastest-growing application areas (Almufareh et al., 2023; Dhingra et al., 2022), but rigorous evaluation of whether these tools actually reduce cognitive load, rather than simply appearing to because they are digital and adaptive, remains limited. Learning analytics that could infer cognitive load indirectly from digital behavior are still mostly theoretical possibilities rather than validated tools. Assistive technology built specifically around load-sensitive principles, as opposed to technology that happens to incorporate load-reduction as an incidental feature, remains a relatively small niche within a much larger and faster-growing technology market.

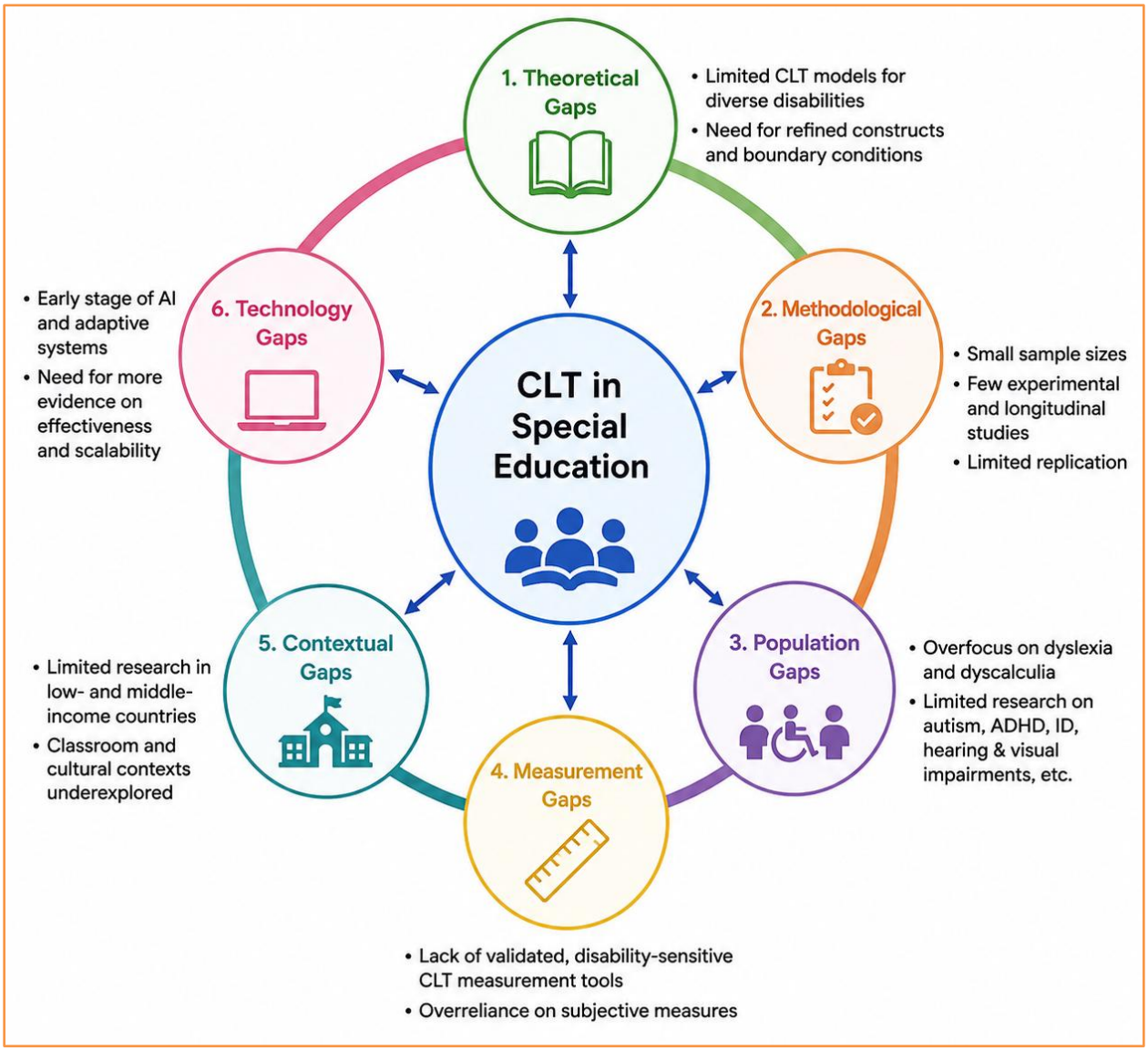
Table 1 summarizes the field's current strengths and limitations side by side, offering a compact reference for the state of the field before the proposed agenda is introduced.

**Table 1:** ***Current Strengths and Limitations of the Field***

| Dimension | Strength | Limitation |
|---|---|---|
| Intervention evidence | Consistent benefit from reducing extraneous load | Very few controlled experiments |
| Disability coverage | Growing work on dyslexia, dyscalculia, ASD, ADHD | Intellectual disability and sensory impairment underrepresented |
| Methods | Some rigorous experimental designs exist | Small samples, short windows, no replication culture |
| Geography | Active research communities in several countries | Heavy concentration in well-resourced regions |

| | | |
|---|---|---|
| Technology | Promising early load-sensitive tools | Limited rigorous evaluation of actual load reduction |
| Measurement | New validated questionnaires now exist | Not yet validated for special education populations |

**Figure 2:** Research Gaps Framework




## Proposed Research Agenda

Building directly on the gaps identified above, this paper organizes its proposed agenda into seven strategic pillars. Each pillar names a set of priorities rather than a rigid checklist, since different research groups will naturally emphasize different priorities depending on their expertise and resources.

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The seven pillars are not ranked in order of importance, and none of them can realistically advance in isolation from the others for very long. Progress in Pillar III, measurement, depends on progress in Pillar I, theory, since instruments need clear constructs to measure. Progress in Pillar IV, technology, depends on progress in Pillar III, since a genuinely load-sensitive tool needs validated ways to confirm it is actually reducing load rather than merely appearing to. Progress in Pillar VI, policy, depends on progress in Pillars II and VII, since policymakers need a solid evidence base and synthesized findings before they can reasonably act. Readers should keep these dependencies in mind while working through the pillars below, since presenting them as seven separate headings, for clarity, risks understating how tightly interconnected they actually are in practice.

**Pillar I: Theory Development.** Priorities here include building conceptual frameworks that connect CLT specifically to special education populations, developing shared taxonomies that organize the field's scattered applications into a common structure, pursuing cross-theory integration that connects CLT to frameworks special educators already use, and refining understanding of cognitive architecture as it applies specifically to disability populations rather than assuming general education findings transfer automatically. This pillar carries outsized importance relative to its modest cost, since conceptual and taxonomic work requires far less funding than large intervention trials, yet it shapes how efficiently every later pillar can proceed.

**Pillar II: Intervention Research.** Priorities include well-designed experimental studies testing specific CLT-informed techniques, quasi-experimental studies where randomization is not feasible, single-case experimental designs suited to the small, heterogeneous populations common in special education, dedicated replication studies rather than only original research, and longitudinal interventions that track whether benefits persist once explicit support is withdrawn. This pillar is likely to require the largest share of funding and time of any of the seven, since rigorous intervention research, done properly, is inherently slow and resource-intensive, particularly when working with small and diagnostically diverse populations.

**Pillar III: Measurement and Assessment.** Priorities include developing new cognitive load instruments specifically for special education populations, validating existing tools such as Krieglstein et al.'s (2023) questionnaire with these populations rather than assuming they transfer, building cognitive load measurement into everyday classroom assessment rather than treating it as a separate research-only activity, and developing digital assessment approaches that can track load indirectly through behavioral indicators such as time on task or error patterns. Without progress here, every other pillar is forced to rely on indirect or borrowed measures, which weakens the strength of any claim the field wants to make about whether a given intervention or technology actually works.

**Pillar IV: Technology Integration.** Priorities include artificial intelligence systems that adapt pacing and content based on genuinely measured, rather than assumed, cognitive load (Almufareh et al., 2023), adaptive learning systems tested rigorously against load-reduction outcomes rather than only engagement metrics, assistive technology built from the ground up around load-sensitive principles as seen in early examples like Mathlete (Dhingra et al., 2022), intelligent tutoring systems capable of adjusting instruction in real time, and virtual and augmented reality applications whose modality and split-attention effects have already been studied in general education contexts (Albus & Seufert, 2023) and could extend directly into special ed-

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ucation settings. This pillar is likely to attract the most commercial and public interest of the seven, which makes rigorous evaluation especially important, since market enthusiasm for adaptive technology can easily outpace the evidence needed to confirm that a given tool genuinely reduces load rather than simply appearing sophisticated.

**Pillar V: Teacher Development.** Priorities include research into what special educators currently understand about cognitive load and where their knowledge gaps lie, professional development programs that embed CLT explicitly into ongoing teacher training rather than treating it as an optional add-on, classroom implementation research that tracks how CLT-informed practices actually function once they leave controlled research settings, and fidelity of implementation research that examines whether teachers apply these practices as intended and what happens to outcomes when they do not. Of all seven pillars, this one has arguably the most direct path to real classroom impact, since even well-validated interventions and well-designed technology accomplish little if the teachers expected to use them never receive adequate training or support.

**Pillar VI: Systems and Policy.** Priorities include curriculum development that embeds load-sensitive design principles into materials from the outset rather than retrofitting them later, research connecting CLT evidence to inclusive education policy and practice, work exploring how national standards might incorporate cognitive load considerations, formal policy integration efforts that translate accumulated evidence into actionable guidance, and reform of teacher education programmes so that future special educators encounter CLT as a foundational concept rather than an elective topic. As noted above, this pillar depends heavily on evidence maturing through Pillars II and VII first, and premature policy recommendations risk undermining the field's credibility if they later prove to rest on thinner evidence than they claimed.

**Pillar VII: Evidence Synthesis.** Priorities include systematic reviews that consolidate accumulating primary research, meta-analyses that pool effect sizes once enough comparable studies exist, umbrella reviews that synthesize across multiple existing reviews once the review literature itself grows large enough to warrant this, bibliometric analyses that map the shape and growth of the field itself, similar to bibliometric work already conducted in adjacent literatures such as dyslexia research (Wu et al., 2022), and living reviews that update continuously as new evidence accumulates rather than becoming outdated the moment they are published. This pillar functions as the field's memory, ensuring that Pillar II's accumulating intervention studies actually get synthesized into usable conclusions rather than sitting as an ever-growing but never-summarized pile of individual findings.

Table 2 translates these seven pillars into sample priority research questions, giving a more concrete sense of what work under each pillar might actually look like.

**Table 2** ***Priority Research Questions for Each Strategic Pillar***

| Pillar | Sample priority research question |
|---|---|
| I. Theory Development | What disability-specific taxonomy of CLT applications would the field find most useful? |
| II. Intervention Research | Do worked-example interventions replicate across independent samples of students with dyscalculia? |
| III. Measurement and Assessment | Can existing cognitive load questionnaires be validated for students with intellectual disability? |

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| | |
|---|---|
| IV. Technology Integration | Do AI-adaptive tools measurably reduce load compared with non-adaptive equivalents? |
| V. Teacher Development | What do special educators currently understand about cognitive load, and what training changes that understanding? |
| VI. Systems and Policy | How could national curriculum standards incorporate cognitive load principles for students with disabilities? |
| VII. Evidence Synthesis | What does a bibliometric map of CLT and special education research reveal about its growth and gaps? |

**Figure 3:** Seven Strategic Pillars of the Research Agenda


## Proposed Timeline

Naming priorities is only half of building a usable agenda. The other half is sequencing them realistically, since not every priority can or should be pursued at once. This paper proposes a three-phase timeline spanning roughly a decade, organized by research maturity rather than by calendar precision. A decade is a deliberate choice rather than an arbitrary one. Shorter horizons tend to produce agendas that overpromise, treating early conceptual work as though it could quickly translate into policy. Longer horizons risk feeling too abstract to guide any specific researcher's

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next grant application or dissertation proposal. Ten years is roughly the span over which a field like reading research moved from active but scattered inquiry toward genuine consensus and policy influence, and it offers a similarly realistic window for CLT research in special education to do the same.

**Phase I (Years 1-3).** This phase focuses on conceptual papers that build shared theoretical ground, similar in spirit to the taxonomy and integration work this broader research programme has already begun. It includes initial intervention studies, small in scale but carefully designed, aimed at generating preliminary evidence rather than definitive conclusions. Framework development work, connecting CLT to existing special education frameworks such as Response to Intervention and Universal Design for Learning, belongs here as well, since later empirical work will be more useful if it is grounded in shared conceptual language from the start. Pilot testing of new measurement instruments also belongs in this phase, since instrument development needs to happen before larger studies can use those instruments with confidence.

**Phase II (Years 4-6).** With conceptual groundwork in place, this phase shifts toward large-scale experiments with adequate sample sizes and proper controls, something the field has mostly lacked so far. Replication becomes a priority here rather than an afterthought, deliberately testing whether Phase I's preliminary findings hold up in different settings and with different research teams. Instrument validation work matures during this phase too, moving from pilot testing toward full psychometric validation with special education populations specifically. Technology development accelerates as well, building on Phase I's conceptual groundwork to produce genuinely load-sensitive tools rather than tools that merely claim to be adaptive.

**Phase III (Years 7-10).** By this stage, enough primary research should exist to support longitudinal research tracking whether benefits from earlier interventions persist over time. International collaborations should be well underway, extending the field's evidence base beyond the narrow set of well-resourced countries where most current research originates. Meta-analyses become possible once enough comparable studies have accumulated, allowing the field to speak with more confidence about effect sizes and boundary conditions than any single study could support alone. Guidelines translating accumulated evidence into practical recommendations for teachers and schools should emerge during this phase, followed by policy work that embeds those guidelines into curriculum standards and teacher preparation requirements. Implementation science, the study of how well-supported practices actually spread and take hold in real schools, becomes a legitimate priority only once there is enough validated practice worth implementing at scale.

This phased structure is not meant to suggest that all research must wait for its designated phase before beginning. Some Phase III work, particularly international collaboration, can and should start early, since building the relationships and infrastructure for cross-national research takes years regardless of when the underlying studies begin. What the phasing does provide is a rough sense of what the field can realistically expect to accomplish at different points along a ten-year horizon, which matters for funding agencies deciding what to support now versus what to hold in reserve, and for doctoral students deciding which kind of contribution their own work is best positioned to make.

Table 3 pairs each phase with the methodologies most appropriate to it, offering researchers a practical reference for matching their own work to the field's current stage of development.

Online ISSN: 3006-8886
Print ISSN: 3006-8878

**Table 3:** ***Recommended Methodologies for Future Studies***

| Phase | Recommended methodologies |
|---|---|
| Phase I (Years 1-3) | Conceptual papers, small pilot studies, framework development, instrument piloting |
| Phase II (Years 4-6) | Randomized and quasi-experimental trials, single-case designs, replication studies, instrument validation |
| Phase III (Years 7-10) | Longitudinal designs, cross-national collaborative studies, meta-analyses, implementation trials |

## Stakeholder Responsibilities

A research agenda only works if the people who need to act on it understand their own role within it clearly. Seven groups carry distinct, though overlapping, responsibilities for turning the pillars and timeline proposed above from a document into an actual field.

**Researchers** carry the most direct responsibility: generating high-quality evidence, which means committing to the methodological rigor, sample sizes, and replication culture the field has lacked so far, rather than continuing to produce isolated, small-scale studies that never connect to a larger cumulative effort.

**Universities** need to support doctoral training and interdisciplinary centres capable of housing this kind of work, since CLT and special education research genuinely spans cognitive psychology, education, and increasingly computer science, and few existing academic structures are built to support that combination naturally.

**Schools** provide the implementation sites without which none of this research can move beyond controlled laboratory settings, and their willingness to participate in intervention and implementation research is not a minor logistical detail but a genuine precondition for the field's later phases.

**Teachers** are positioned to co-design and evaluate interventions directly, not merely to implement whatever researchers hand them. Teachers who have spent years working with a specific disability population often notice practical constraints and opportunities that a researcher without that daily classroom experience would miss entirely.

**Journal editors** can encourage methodological diversity and replication by actively valuing these contributions in what they choose to publish, since a publishing culture that rewards only novel findings, while treating replication and null results as unpublishable, actively discourages the kind of rigor this field needs most.

**Funding agencies** need to prioritize strategic research themes rather than funding proposals purely on individual merit without reference to how well they fit into a larger coordinated effort, since funding is one of the few levers capable of actually redirecting where a scattered field's energy goes.

**Policymakers** are responsible for translating evidence into educational policy once that evidence reaches sufficient maturity, a responsibility that requires resisting the temptation to act on Phase I conceptual work as though it already carried the weight of Phase III validated practice.

Online ISSN: 3006-8886
Print ISSN: 3006-8878

Table 4 summarizes these seven stakeholder roles alongside their primary contribution to the agenda, offering a compact reference for how responsibility is distributed across the field.

**Table 4:** ***Stakeholder Roles in Advancing the Field***

| Stakeholder | Primary contribution |
|---|---|
| Researchers | Generate rigorous, cumulative evidence |
| Universities | Support doctoral training and interdisciplinary infrastructure |
| Schools | Provide real-world implementation sites |
| Teachers | Co-design and evaluate interventions from practical experience |
| Journal editors | Value methodological diversity and replication in publishing decisions |
| Funding agencies | Direct resources toward strategic, coordinated priorities |
| Policymakers | Translate mature evidence into policy at the appropriate stage |

## Challenges to Building the Field

Building this kind of coordinated field will not happen automatically, and it is worth naming the obstacles honestly rather than assuming good intentions alone will be enough. Every challenge described below has already slowed progress in adjacent fields facing similar coordination problems, and there is no reason to expect CLT research in special education to be exempt from any of them.

**Limited funding** is probably the most obvious constraint. Special education research already competes for scarce resources against many other priorities, and CLT-specific research within special education is a narrower slice still. A practical response is for funding agencies to create dedicated funding streams or special calls tied explicitly to this agenda's priority pillars, making the case for investment easier to defend within larger, more general funding competitions. Smaller, targeted seed grants aimed specifically at Phase I conceptual and pilot work could also help, since this early-stage work is comparatively inexpensive and could generate the preliminary evidence needed to compete more successfully for larger Phase II funding later.

**Limited expertise** compounds the funding problem, since relatively few researchers currently combine deep knowledge of both cognitive load theory and special education practice. Cross-training programmes, joint doctoral supervision between cognitive psychology and special education departments, and dedicated postdoctoral fellowships could help grow this expertise base over time rather than waiting for it to emerge on its own. Universities in countries with fewer existing specialists in this intersection, including many public sector systems in South Asia, could benefit particularly from partnerships that bring visiting scholars or joint supervision arrangements rather than waiting for local expertise to develop entirely independently.

**Interdisciplinary barriers** show up in less obvious ways too, including different publishing norms, different methodological standards, and even different vocabulary between cognitive psychology and special education research communities. Deliberately convened working groups and joint conference tracks could help bridge this gap, giving researchers from both traditions regular opportunities to align their language and expectations.

**Research capacity** in many parts of the world, particularly the low-resource settings identified as a contextual gap in section four, remains limited by more basic constraints: fewer doctoral

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programmes, less research funding, and heavier teaching loads for the researchers who might otherwise contribute. International partnerships that pair well-resourced research groups with researchers in under-resourced settings, treating the latter as genuine co-investigators rather than data collection sites, could help address this without simply reproducing the geographic imbalance the field already has.

**Ethical considerations** deserve explicit attention as well, since special education research involves a genuinely vulnerable population, and cognitive load research in particular sometimes requires exposing students to deliberately more or less demanding instructional conditions. Careful institutional review, informed consent processes appropriate to learners with communication differences, and a strong preference for interventions that carry clear potential benefit rather than only research value, are not optional extras but core requirements for any agenda built around this population.

**International collaboration challenges** extend beyond funding and capacity into more practical territory: differences in special education classification systems, language barriers in both the literal and the technical, terminological sense, and varying ethical review requirements across countries all slow down genuinely international research. None of these challenges are unique to CLT research, but a field trying to build cross-national evidence intentionally, as this agenda proposes, needs to plan for them rather than treating them as unexpected obstacles.

## Indicators of Progress

A research agenda is easier to hold accountable when it names concrete, measurable signs of progress rather than only aspirational goals. Several indicators would suggest, a decade from now, that this agenda succeeded rather than simply being another document nobody acted on.

The number of intervention studies meeting reasonable methodological standards, adequate sample sizes, appropriate controls, and clear reporting, would be one of the clearest indicators, since this remains one of the field's thinnest areas today. The number of validated measurement tools built specifically for special education populations would indicate whether Pillar III's priorities have actually been pursued, since this gap currently limits nearly every other kind of research in the field. The number of countries represented in published research, and the number of genuine international collaborations rather than single-country studies published in international journals, would indicate whether the geographic concentration identified in section three has actually started to shift.

Special journal issues dedicated to CLT and special education would signal that editors recognize the field as a coherent area worth featuring prominently, rather than scattering relevant work across many different, loosely connected outlets. Clinical or educational guidelines translating accumulated evidence into practical recommendations would indicate that the field has moved past Phase I and II research into genuine Phase III translation. And the number of teacher training programmes that embed CLT explicitly into their special education coursework, rather than treating it as an optional elective, would indicate whether Pillar V's teacher development priorities have actually reached the people who need them most.

None of these indicators is meant to function as a rigid pass or fail test. A field can be making genuine, healthy progress along several of these dimensions while still lagging on others, and

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progress in different pillars will naturally happen at different speeds. What matters is that these indicators exist at all, giving the field, and the funding agencies and institutions supporting it, something concrete to check progress against rather than relying on a vague, unverifiable sense that things seem to be moving in the right direction.

There is also a more immediate use for these indicators, beyond the ten-year horizon this paper has mostly focused on. A funding agency deciding between two competing grant proposals could reasonably ask which one moves one of these indicators forward more directly. A doctoral student choosing between two possible dissertation topics could use the same question to weigh which contribution the field currently needs more. Framed this way, these indicators are not just a way of checking on progress after the fact. They are also a practical decision-making tool that individual researchers and institutions can apply right now, long before the ten-year mark this paper has used as its planning horizon actually arrives.

## Conclusion

The scattered, energetic growth of CLT research in special education described throughout this paper is worth celebrating, not treating as a problem in itself. Researchers across several countries have independently recognized that working memory limitations sit close to the center of many learning difficulties, and that recognition is exactly the kind of intellectual momentum a coordinated agenda needs to work with rather than create from scratch. What this paper has tried to add is direction: a shared sense of which gaps matter most, which methods the field urgently needs more of, and roughly what order these priorities should be pursued in over a realistic ten-year horizon.

The future development of Cognitive Load Theory in special education will depend not only on producing additional intervention studies but on pursuing a coordinated programme of theory development, methodological innovation, technological advancement, implementation research, and evidence translation. No single research group, however well-funded or well-positioned, can pursue all seven pillars proposed in this paper simultaneously, and that is precisely why coordination matters more than individual effort here. A shared agenda does not tell any one researcher exactly what to study. It gives every researcher a way to see how their own work connects to a larger, cumulative effort, and it gives the field as a whole a much better chance of looking back a decade from now at a body of knowledge that adds up to something coherent, rather than a pile of individually interesting studies that never quite became a field.

This paper is best read alongside the three companion papers that precede it in this broader research programme, the rationale paper making the case for why CLT deserves attention in special education, the taxonomy paper proposing a shared structure for organizing its applications, and the integration paper connecting CLT to the frameworks special educators already trust. Each of those papers addresses a different piece of the same underlying problem, that a genuinely promising theoretical connection has grown without the coordination it needs to reach its full potential. This paper's contribution is the most forward-looking of the four: not a description of where the field stands or how its pieces fit together, but a proposal for where it should go next, and a rough map of how to get there.

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