

When Alignment is not Enough: Cognitive Budget Framework for Game Design

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Abstract: The Learning Mechanics-Game Mechanics (LM-GM) model connects game mechanics with learning objectives. However, designers also need to consider the demands of learning to use those mechanics. This paper proposes the Cognitive Budget Framework to help designers assess these demands alongside learners' prior knowledge and interface familiarity. The framework draws on an online randomised controlled trial (RCT) of a scaffolded, card-based sales-training game and a content-matched conventional digital module. Both covered the same sales stages, types of scenarios and adaptive-selling content, while the game added card, token and progression mechanics. The primary analysis included 63 adult learners who completed their assigned condition (32 game, 31 control). Surveys measured sales self-efficacy and adaptive-selling beliefs before and after training. The latter were measured using the five-item short-form Adaptive Selling Scale (ADAPTS-SV). Post-test measures covered perceived cognitive load, engagement and transfer intention, while game telemetry recorded learner interaction. Participants in the game condition showed no statistically significant overall advantage over those in the control condition on the measured outcomes. They also reported higher perceived cognitive load ($d = 0.71$), while attrition was higher in the game condition (41.8%) than in the control condition (16.2%). However, a baseline-score interaction analysis showed that the condition difference varied with baseline ADAPTS-SV score, $F(1,59) = 11.65$, $p = .001$. The estimated difference ranged from a 15.17-point control advantage at a low baseline score to a 5.51-point game advantage at a high score. Among participants who completed the game condition, engagement was indirectly associated with adaptive-selling beliefs through self-efficacy (indirect effect = 1.10, 95% confidence interval [0.43, 2.24]). However, these associations do not establish causation. The proposed framework helps designers consider learner readiness when limiting the mechanics used or providing familiarisation before training. These design responses require direct testing.

Keywords: Game-based learning, Cognitive load, LM-GM, Learner readiness, Professional training

1. Introduction

Digital game-based learning (DGBL) remains uncommon in workplace settings. This limited adoption may be linked to a lack of suitable solutions and authoring expertise (Tay et al, 2022), as well as concerns about efficacy and negative perceptions of digital games (Bösche and Kattner, 2011). Yet, evidence suggests that DGBL can improve learning and retention. A meta-analysis by Wouters et al (2013), for example, found stronger effects when games were used across multiple training sessions and supplemented other forms of instruction. However, as most of this evidence comes from students in formal education, there is a need to develop DGBL tailored to workplace learning and test its effectiveness with workplace learners.

The Learning Mechanics-Game Mechanics (LM-GM) model provides a basis for this design by connecting learning mechanics with game mechanics that support the intended learning objectives (Arnab et al, 2015). This helps designers select game features for a clear learning purpose and avoid adding mechanics with little instructional value. Such alignment matters because DGBL outcomes vary with instructional design, feedback and game features (Tay et al, 2022). Supporting this emphasis on pedagogical design, Arnab et al's (2013) serious-game trial reported positive outcomes and recommended further comparison with non-game approaches.

However, a clear design rationale also needs empirical testing. All et al (2021) identified practical challenges in assessing DGBL effectiveness, including failed randomisation, the choice of control activity and how instructional time is compared. These issues require careful attention when designing a randomised controlled trial (RCT). A workplace DGBL intervention therefore needs both a clear design rationale and evaluation against a content-matched control in an RCT. Matching the instructional content helps assess whether the game design adds value beyond the material shared by both conditions.

To address these needs, this study developed an LM-GM-aligned DGBL intervention for sales training. Effective selling requires learners to recognise customer needs, select suitable sales strategies and adapt their approach to the customer's situation. Sales-education research therefore emphasises experiential learning alongside content knowledge (Cummins et al, 2013; 2020). Role-play and coached field practice can support these skills,

but they are resource-intensive and difficult to deliver consistently at scale. DGBL offers a way to provide repeated, low-risk scenario practice with feedback and decision consequences. However, the game must also remain manageable for adults with different levels of sales experience and familiarity with digital interaction.

Within this context, the study focuses on adaptive selling, defined as changing sales behaviour in response to customer and situational information (Spiro and Weitz, 1990). It measures adaptive-selling beliefs using the five-item short-form Adaptive Selling Scale (ADAPTS-SV; Robinson et al, 2002), alongside sales self-efficacy and transfer intention. These outcomes concern learners' beliefs, confidence and intentions rather than observed workplace sales performance. Perceived cognitive load and learner engagement describe the immediate training experience, while in-game behavioural telemetry records how learners interact with the game. Through an online RCT with adults interested in sales training, the study compares the intervention with a content-matched conventional digital module to test whether the aligned game improves adaptive-selling beliefs and sales self-efficacy.

2. Design of Game and Control Treatment

To provide repeated sales practice without requiring a facilitator for every attempt, a card-based digital game titled 'Improve Your Sales Game' was developed. It covered seven stages of the sales process: prospecting, pre-approach, needs analysis, presentation, objection handling, closing and follow-up. Learners first identified sales stages using scenario cards in Phase 1. They then responded to customer situations using event cards and action cards in Phase 2.

The design followed the LM-GM model described by Arnab et al (2015). Adaptive-selling objectives were mapped to learning mechanics and Bloom's cognitive levels, from remembering and understanding to applying, analysing and evaluating. Game mechanics were then selected to support these activities. The two phases moved from identifying sales stages to comparing possible responses, with feedback to help learners review their decisions. Successful attempts were intended to provide mastery experiences, or experiences of completing a task successfully, that could build sales self-efficacy (Bandura, 1977).

Before gameplay, onboarding screens explained the roles of scenario cards, event cards and action cards, as well as lead-token use and the progression system. This preparation introduced the game rules before learners began the sales tasks.

Phase 1 introduced the sales process through 10 scenario cards. Each card described a situation that corresponded to one sales stage, which learners had to identify. This direct matching task targeted remembering and understanding. Correct classifications earned lead tokens for use in Phase 2, while inaccurate classifications prompted an explanation of the appropriate sales stage. An end-of-phase summary allowed learners to review these distinctions before moving to more complex decisions.

Phase 2 built on this foundation through event cards and action cards. Learners spent their earned lead tokens to enter encounters involving customer needs, objections and decisions about whether a solution suited the customer. For each event card, they selected an available action card and observed the outcome. The task therefore shifted from identifying sales stages to applying knowledge: learners had to identify relevant customer cues and compare possible responses. The interface displayed available actions, token consequences and encounter outcomes, followed by a play-by-play summary.

As Phase 2 progressed, fewer action cards were available and the choices became less clear-cut. Unlike the direct correspondence between scenario cards and sales stages in Phase 1, event cards did not correspond to a single action card. More than one response could produce an outcome, including responses that were less suitable for the customer. Learners therefore had to judge trade-offs under uncertainty. Feedback on successful and failed attempts, together with the phase summary, was intended to help them reflect on the consequences of their choices.

Table 1 maps each adaptive-selling objective to its learning mechanic, Bloom's level, game mechanic and implementation.

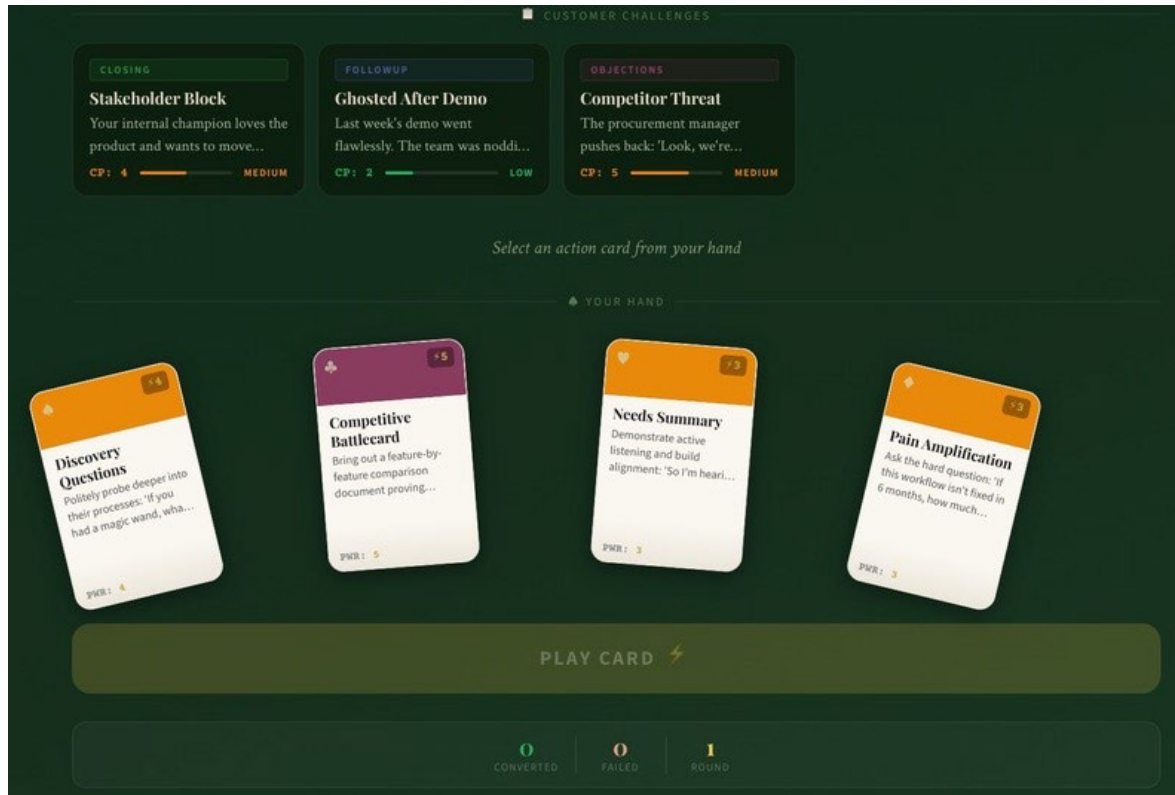
Table 1: LM-GM alignment of the sales game

Adaptive-selling objective	Learning mechanic	Bloom's level	Game mechanic	Implementation
Identify sales stages	Recognition and classification	Remember / Understand	Scenario cards	Phase 1: match each of 10 scenario cards to one sales stage.
Understand sales-stage distinctions	Feedback and reflection	Understand	Classification feedback	Phase 1: review corrections and the end-of-phase summary.
Build foundations before harder tasks	Practice and progression	Remember / Understand	Lead tokens	Earn tokens in Phase 1 to access Phase 2 encounters.
Choose suitable sales responses	Application and analysis	Apply / Analyse	Event cards and action cards	Phase 2: select an action card in response to an event card.
Compare possible responses	Comparison and judgement	Analyse / Evaluate	Limited action cards	Phase 2: weigh available actions against customer needs and remaining tokens.
Evaluate decisions and outcomes	Evaluation and reflection	Evaluate	Outcome feedback and summary	Phase 2: review which actions succeeded or failed as choices became less clear-cut.

The control condition received a conventional digital module covering the same sales stages, types of scenarios and core adaptive-selling content across 16 self-directed screens. Static visuals and explanatory text presented the sales process, while decision prompts asked learners to consider responses to customer situations. A menu allowed them to skip familiar sections and revisit material. The module retained learner choice and applied decision-making, but did not include card classification, lead tokens, gated phases or encounter rules.

Matching the instructional content allowed the study to examine whether the game improved outcomes beyond the material shared by both conditions. However, this comparison evaluated the game as a complete training design. It did not isolate the effect of individual mechanics or establish equal practice time. Figure 1 shows the two treatments as participants encountered them.

(a) Game condition: event cards and action cards



(b) Control condition: sales situation and response feedback

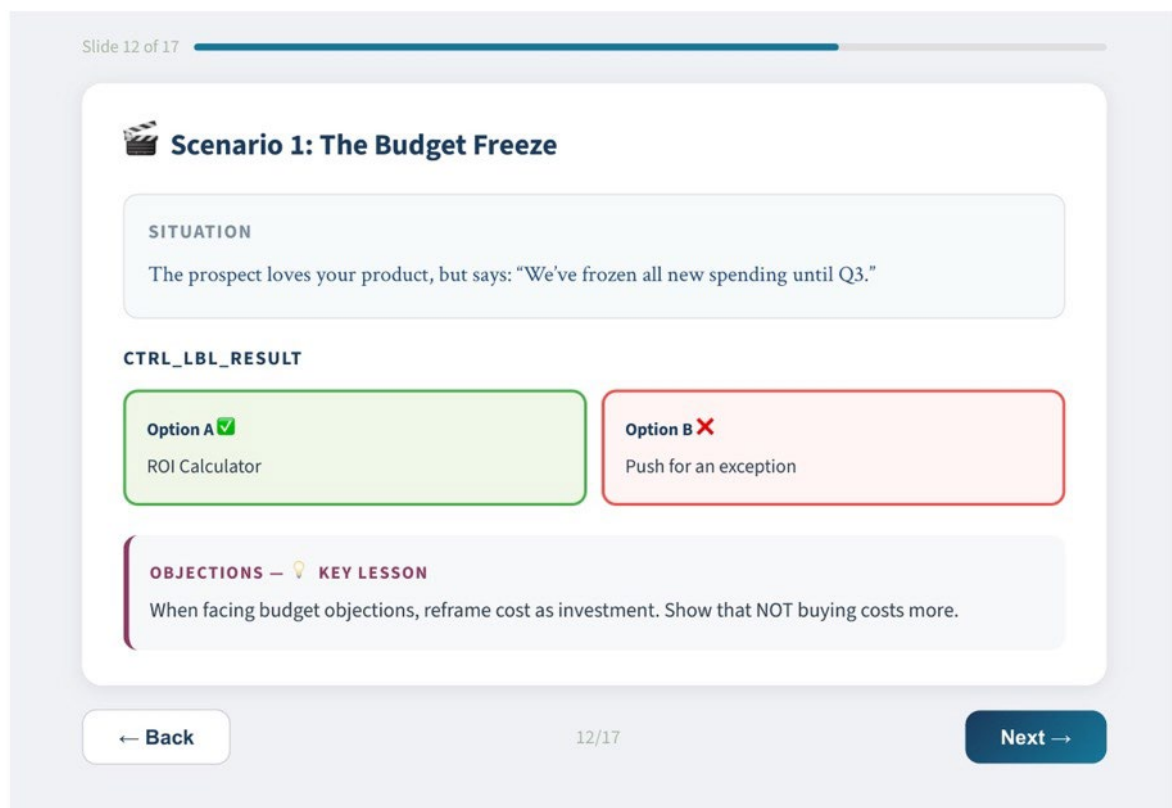


Figure 1: Details of the game and control interfaces. Panel (a) shows Phase 2 event cards and action cards; panel (b) shows a control-module decision prompt. Both panels are cropped from the original screenshots

3. Method

The study used a pre-test-post-test randomised controlled trial (RCT), delivered entirely online between 31 March and 15 April 2026. Recruitment took place through professional networking sites, online forums and digital communities related to sales and professional development. The public study website was open to consenting adults interested in sales training, including sales professionals and people aspiring to sales roles. Participation was voluntary and unpaid, apart from access to the training. Recruitment therefore produced a convenience sample of adults interested in sales training.

Participants reviewed the study information, provided digital informed consent, and completed demographic questions and pre-test measures. Allocation then occurred before either treatment was displayed. The server automatically assigned each eligible session to the next condition in a pre-blocked sequence; neither participants nor researchers could choose the condition. As enrolment continued, the server could direct subsequent assignments towards the condition with fewer usable completions. This adjustment used recorded assignment and completion status rather than participants' post-test scores.

The platform recorded participant flow from initial access through consent, pre-test, allocation, treatment progression and post-test. Assigned participants without usable post-test data were recorded as non-completers in their condition. Completion and attrition were therefore reported for all assigned participants, while the primary outcome analyses included only those with the required pre-test and post-test scores. These analyses estimate differences among completers and may be affected by differential attrition.

Sales self-efficacy, or confidence in performing sales tasks, was measured with 10 items developed for this study, drawing on Bandura (1977) and Knight et al (2014). Adaptive-selling beliefs were measured using the five-item ADAPTS-SV (Robinson et al, 2002). Both were administered before and immediately after training and are reported on a 0–100 scale. Post-test measures included four cognitive-load items covering mental effort, difficulty, complexity and strain, informed by subjective load measurement (Paas et al, 2003; 2008; Sweller et al, 2011). Six engagement items covered behavioural, emotional and cognitive engagement (Fredricks et al, 2004; Woo, 2014). Three transfer-intention items were adapted for the sales context from workplace knowledge-transfer research (Nguyen and Nguyen, 2015). Table 2 reports item counts, response scales and internal consistency using Cronbach's alpha. Platform logs separately recorded allocation, completion, card attempts, classification accuracy, phase progression, response timing and encounter outcomes.

Table 2: Measures and internal consistency in the completer sample

Measure	Items and response scale	Cronbach's alpha
Sales self-efficacy	10 items; 1–5; pre-test and post-test	Pre .93; post .96
Adaptive-selling beliefs (ADAPTS-SV)	5 items; 1–7; pre-test and post-test	Pre .91; post .92
Perceived cognitive load	4 items; 1–7; post-test	.77
Learner engagement	6 items; 1–5; post-test	.88
Transfer intention	3 items; 1–5; post-test	.85

The analysis first tested whether outcomes differed between conditions. Analysis of covariance (ANCOVA) compared post-test sales self-efficacy and ADAPTS-SV scores while accounting for the corresponding pre-test score. Welch tests compared perceived cognitive load, engagement and transfer intention, which were measured only at post-test. Moderation analyses then examined whether the condition difference varied with learners' starting scores. Each model included an interaction between condition and baseline score, with baseline scores centred on the sample mean. Pre-test ADAPTS-SV and sales self-efficacy scores were treated as indicators of baseline readiness. Finally, exploratory correlation and mediation analyses examined relationships among engagement, sales self-efficacy and adaptive-selling beliefs in the game condition, while telemetry provided supporting evidence about how learners interacted with the training.

4. Results

As shown in Figure 2, among the 92 assigned participants, 23 of 55 in the game condition and six of 37 in the control condition did not complete the study. This left 63 completers for the primary analysis: 32 in the game condition and 31 in the control condition. Of these completers, 32 (50.8%) reported 0–1 year of sales experience and 41 (65.1%) worked in the education industry. The sample therefore included adults with varied sales experience and substantial representation from education.

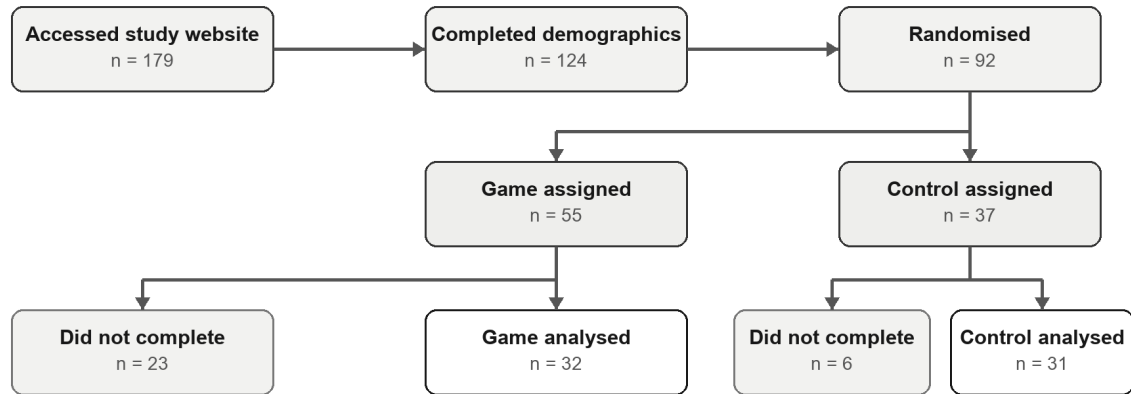


Figure 2: Participant flow

The overall results in Table 3 show that, after accounting for pre-test scores, adjusted post-test sales self-efficacy was 69.9 in the game condition and 75.5 in the control condition. The difference favoured the control condition but was not statistically significant, $F(1,60) = 3.56$, $p = .064$. Adjusted ADAPTS-SV scores were 67.7 and 72.5, respectively, and also did not differ significantly, $F(1,60) = 2.22$, $p = .141$. However, participants in the game condition reported higher perceived cognitive load: 4.81 versus 4.19, $t(59.8) = 2.83$, $p = .006$, $d = 0.71$. Learner engagement and transfer intention did not differ significantly.

Table 3: Key between-condition outcomes

Outcome	Game	Control	Test	Effect / p
Self-efficacy post-test (adjusted, 0-100)	69.9	75.5	$F(1,60) = 3.56$	$\eta_p^2 = .056$; $p = .064$
ADAPTS-SV adaptive-selling beliefs post-test (adjusted, 0-100)	67.7	72.5	$F(1,60) = 2.22$	$\eta_p^2 = .036$; $p = .141$
Cognitive load (1-7)	4.81 (0.83)	4.19 (0.93)	$t(59.8) = 2.83$	$d = 0.71$; $p = .006$
Engagement (1-5)	3.79 (0.61)	4.03 (0.61)	$t = -1.54$	$d = -0.39$; $p = .128$
Transfer intention (1-5)	3.80 (0.69)	4.01 (0.62)	$t = -1.27$	$d = -0.32$; $p = .209$

To understand these overall results, further analyses examined who completed each condition and whether the condition differences varied with baseline scores.

Attrition was higher in the game condition: 41.8% of assigned participants did not complete, compared with 16.2% in the control condition. The voluntary, online and unsupervised nature of the intervention makes it difficult to identify why participants left. As post-test outcomes were available only for participants who completed their assigned condition, the between-condition analysis applies only to these completers.

The difference between conditions varied with learners' starting scores. The condition-by-baseline ADAPTS-SV interaction was significant, $F(1,59) = 11.65$, $p = .001$. At one standard deviation below the mean baseline score, the estimated difference favoured the control condition by 15.17 percentage points. At one standard deviation above the mean, it favoured the game condition by 5.51 points. The interaction for baseline sales self-efficacy was also significant, $F(1,59) = 5.52$, $p = .022$. The fitted lines in Figure 3 show how the relative outcomes varied

across baseline scores, taking into account the absence of an overall advantage for the game condition. Evidence was stronger for a disadvantage in the game condition at lower baseline readiness than for a benefit at higher readiness levels.

Among participants who completed the game condition, mediation analysis identified an indirect association between engagement and ADAPTS-SV scores through sales self-efficacy, with a bootstrapped indirect effect of 1.10 and a 95% confidence interval of [0.43, 2.24]. Once self-efficacy was included in the model, the direct engagement-ADAPTS-SV association was not significant. The association between self-efficacy and ADAPTS-SV was stronger in the game condition than in the control condition ($r = .877$ versus $.532$, $p = .004$). Among these completers, perceived cognitive load was not significantly related to self-efficacy, engagement or ADAPTS-SV scores. These analyses describe relationships among the reported beliefs and experiences; they do not establish that the game increased self-efficacy independently of engagement. The strong self-efficacy-ADAPTS-SV association may also partly reflect overlapping beliefs and the use of self-report measures in the same post-test.

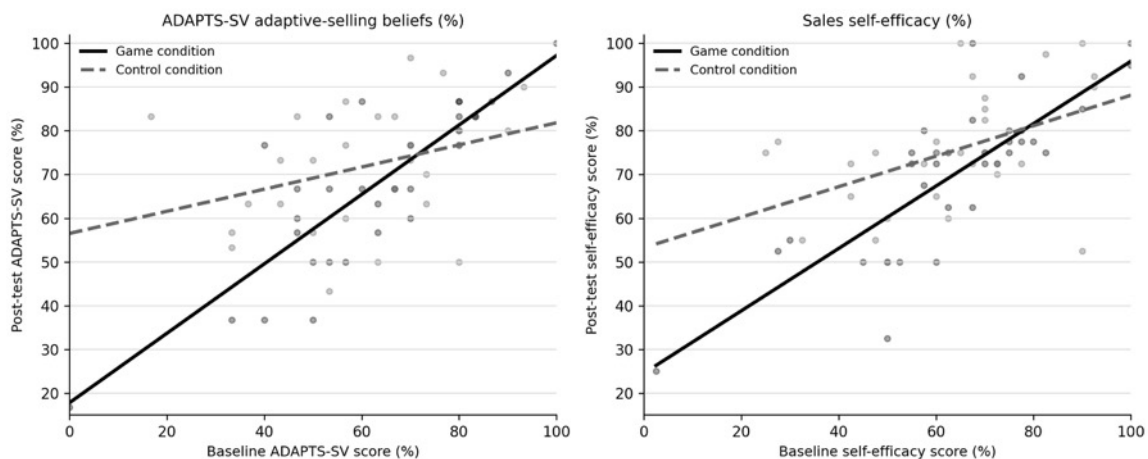


Figure 3: Fitted condition-specific lines from the baseline-score interaction models

5. Learner Demand and the Cognitive Budget Framework

Although the game used LM-GM for pedagogical alignment, alongside scaffolding and feedback, participants in the game condition showed no statistically significant overall advantage over those in the control condition. Participants assigned to the game condition were also less likely to complete, while completers reported higher perceived cognitive load. Furthermore, the comparison favoured the control condition more strongly among learners with lower baseline adaptive-selling beliefs. Together, these findings raise a practical question for DGBL development: how can designers account for differences in learner readiness when selecting and introducing game mechanics?

Because the two conditions shared the same sales content, the additional demands of operating the game offer one possible explanation for the perceived-load difference. Working memory, which holds and processes information during a task, has limited capacity for unfamiliar information (Sweller et al, 2011). In the game condition, learners had to understand card roles, lead tokens and progression rules while also interpreting sales situations. The effort required to learn these rules and interface actions is described here as mechanics-acquisition cost. This cost may compete with the attention needed for sales reasoning, particularly when both the content and the mechanics are unfamiliar. Bloom's cognitive levels provide the demand scores used in the illustrative worked example below.

The demands learners experience may also depend on what they already know. Research on expertise reversal shows that instructional methods can affect learners differently depending on their prior knowledge (Kalyuga, 2007). For example, a learner who readily recognises a sales stage may have more attention available to understand the card interface. A learner who is still learning the sales stages must manage both tasks together. The framework describes this potential relief from prior knowledge as an expertise offset. Familiarity with similar game interfaces may provide further relief, although it is distinct from knowledge of sales. In this study, the more favourable game-control comparison at higher baseline adaptive-selling belief scores is consistent with this explanation.

Together, mechanics-acquisition cost and expertise offset inform effective load: the demand expected to remain when a learner's existing knowledge and familiarity are taken into account. A game mechanic may support a learning objective yet require more effort to acquire than some learners can comfortably manage alongside the content. The proposed Cognitive Budget Framework treats this remaining demand as a budget to be managed when selecting mechanics and preparing learners to use them. The framework extends LM-GM by asking designers to consider both the learning purpose of a mechanic and the demands it places on the intended learners.

This question becomes particularly relevant when a design combines several mechanics. For example, Callaghan et al (2016) describe an LM-GM-informed engineering game that includes cut scenes, tutorials, time constraints, leaderboards and achievements. These features may support instruction or motivation, but their combined demands also deserve attention. This example illustrates why designers need to consider what learners must manage when several mechanics are combined. The Cognitive Budget Framework encourages designers to select a manageable set of mechanics that supports the intended learning objectives.

This framework also responds to a limitation identified by Arnab et al (2015): LM-GM did not yet include a player model, although the authors recognised the importance of adaptation and personalisation. The Cognitive Budget Framework addresses part of this gap by connecting learners' prior knowledge and interface familiarity to the demands of acquiring game mechanics. These characteristics could inform which mechanics are included and how much familiarisation learners need before training. For example, learners with the same adaptive-selling objective may need different preparation because one already recognises sales stages while another is still learning them. The framework therefore offers a way to incorporate learner readiness into LM-GM design decisions.

The LM-GM model asks whether a mechanic is aligned to a learning function. The Cognitive Budget Framework adds a learner-readiness question: what will this aligned mechanic cost the learner to acquire, and which learners can absorb that cost? Mechanics-acquisition cost can be considered at design time by extending the LM-GM table: for each mechanic, what must the learner understand before the mechanic teaches anything? Table 4 shows this format as a worked example for the sales game.

The illustrative worked example scores Bloom-level demand as +1 (retention or understanding), +2 (application or analysis), or +3 (evaluation or strategic judgement), then applies an expertise offset of 0 for low schema, -1 for moderate schema, and -2 for strong schema. Here, schema refers to organised prior knowledge of the relevant sales situations. The resulting budget is interpreted as easy (≤ 0), acceptable (+1), scaffolded (+2), or excessive ($> +2$).

Table 4: Worked cognitive-budget accounting for LM-GM-aligned game mechanics

Game mechanic	Bloom cost	Expertise-offset accounting	Budget interpretation	Design response
Scenario-card classification	+1 understanding	Low: $+1+0=+1$ moderate: $+1-1=0$ strong: $+1-2=-1$	Acceptable for novices; easy once basic sales-stage schema exists.	Add worked examples or practice rounds before scored classification.
Immediate accuracy feedback	+1 understanding	Low: $+1+0=+1$ moderate: $+1-1=0$ strong: $+1-2=-1$	Acceptable when feedback is explanatory; easy for learners who already know the stages.	Make feedback stage-specific rather than only game-state based.
Lead-token economy	+2 application	Low: $+2+0=+2$ moderate: $+2-1=+1$ strong: $+2-2=0$	High for novices; acceptable or easy when learners have enough schema to see why tokens matter.	Introduce token logic separately through a low-stakes tutorial gate.
Event-card encounters	+2 application	Low: $+2+0=+2$ moderate: $+2-1=+1$ strong: $+2-2=0$	High for low-readiness learners because sales cues and game rules must be coordinated.	Route low-readiness learners to simpler scenarios or direct instruction first.
Action-card choice under constraints	+2 analysis	Low: $+2+0=+2$ moderate: $+2-1=+1$ strong: $+2-2=0$	High when learners must compare actions while tracking customer context and constraints.	Reduce early choice volume; increase branching complexity after demonstrated mastery.

Game mechanic	Bloom cost	Expertise-offset accounting	Budget interpretation	Design response
Difficult event cards	+3 evaluation	Low: $+3+0=+3$ moderate: $+3-1=+2$ strong: $+3-2=+1$	Too high for unsupported novices; acceptable mainly for learners with stronger prior schema.	Reserve difficult event cards for learners who pass an onboarding/readiness threshold.

This worked example makes the design choices concrete. The lead-token economy receives an illustrative demand score of +2: with an offset of 0, the budget remains +2 for learners with little relevant prior knowledge, whereas an offset of -2 brings it to 0 for learners with strong prior knowledge. Its design response is therefore to introduce token logic separately through a low-stakes tutorial. For more difficult event cards, the +3 demand remains excessive for unsupported beginners, suggesting that the full set of difficult situations should be limited to learners who have completed the necessary preparation.

LM-GM can align learning and game mechanics while lower-level mechanics still need to be learned before higher-order learning through those mechanics can begin. For learners with stronger prior knowledge, this onboarding cost may be more manageable. For beginners, it may consume the working memory needed for domain learning. The Cognitive Budget Framework can therefore inform two design decisions: limit the mechanics learners must manage, or familiarise them with those mechanics before they learn the sales content. In practice, this could mean a game with fewer rules and constraints, separate practice with the card and token systems, or direct instruction before the full game.

A direct test of the framework would vary the mechanics used while holding sales content constant. A separate comparison could retain the mechanics but provide familiarisation before training. Measuring prior sales knowledge and interface familiarity separately would help determine which learners benefit from each approach. Completion, perceived cognitive load and observed sales decisions could then show whether these changes improve participation and learning.

Several limitations should guide interpretation of the framework. The study did not isolate mechanics-acquisition cost or establish that higher cognitive load caused poorer outcomes, and its brief load measure cannot distinguish effort spent learning sales content from effort spent operating the game. ADAPTS-SV measures adaptive-selling beliefs rather than expertise directly, so the baseline interaction provides only indirect support for the proposed expertise offset. The modest completer sample and differential attrition limit generalisation, while immediate self-report outcomes cannot establish changes in workplace performance. Further trials should therefore test the proposed components directly and include observed sales tasks and delayed follow-up.

6. Conclusion

This content-matched RCT found no statistically significant overall advantage for participants who completed the LM-GM-aligned sales game over those who completed the conventional digital module. Game-condition completers reported higher perceived cognitive load, while the comparison favoured the control condition more strongly at lower baseline adaptive-selling belief scores. These findings motivated the Cognitive Budget Framework, which extends LM-GM's pedagogical alignment by considering the effort required to acquire game mechanics alongside learners' prior knowledge and familiarity. In doing so, it addresses part of the player-model gap identified by Arnab et al (2015) and supports two design responses: limiting the mechanics used or familiarising learners with them before training. A larger trial, stratified by prior sales knowledge, should compare these approaches while holding instructional content constant. Observed sales tasks and delayed follow-up would then help establish whether the resulting designs improve workplace performance and transfer.

Ethics Declaration: The study involved voluntary adult online participants, who received study information and provided digital informed consent before data collection. This paper reports no personally identifying information. No separate formal ethics clearance was required for this voluntary, non-clinical online educational study.

AI Declaration: Generative AI tools supported language editing, structural revision and formatting. The authors reviewed all material and accept responsibility for the study design, analysis, interpretation, claims and references.

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