Acta Psychologica 269 (2026) 107546

Contents lists available at ScienceDirect

Acta Psychologica

journal homepage: www.elsevier.com/locate/actpsy

Which practice makes perfect? The role of variability and agency in learning adaptive problem solving

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ARTICLE INFO

Keywords:
Adaptive problem solving
Variability
Active learning
Observational learning
Transfer
Physical reasoning

ABSTRACT

Adaptive problem solving—the ability to flexibly devise solutions to novel challenges—is a hallmark of human intelligence. Yet it remains unclear how best to cultivate this skill through training. Some researchers argue that variable practice promotes learning of generalisable rules, whereas others emphasize repetition and mastery of a single problem space before progressing to another. To address this question, 200 adults solved computerised physical reasoning problems under one of five practice regimens: active high-variable training (AHVT; many different problems), active low-variability training (ALVT; small set repeated), passive high-variable training (PHVT; watching complete trial-and-error attempts on many different problems), passive low-variable training (PLVT; watching complete trial-and-error attempts on a small set repeatedly), or no practice (control). All participants completed a common set of novel problems before and after practice, and we evaluated their performance and strategies. Our findings supported a strong effect of variability: high-variability training led to significantly greater improvements in problem-solving success and efficiency (fewer attempts), together with a more streamlined strategy search characterised by leaner tool use and lower switching between tools than low-variability training. By contrast, agency had minimal impact—participants who actively solved problems did not significantly outperform those who learned by observing, given equal exposure to the same problems, including full attempts, errors, feedback, and eventual successes. There were no reliable interactive effects of variability and agency. Findings suggest that practice variability, more than the physical act of performing each attempt, is key to mastering a set of skills that generalize to new problems. We discuss how variable experiences encourage abstract problem-solving strategies and "learning-to-learn," why observation can sometimes match active doing, and implications for cognitive theory, education, and AI.

1. Introduction

Every day, people encounter novel problems that demand going beyond rote solutions. Adaptive problem solving refers to the ability to learn solutions that extend to different problems within the same domain or problem space. Unlike routine learning of a single repeated task, adaptive learning entails mastering underlying principles that transfer to varied situations. A classic challenge in cognitive science and education has been how to train learners so that skills generalize rather than remain context-bound (Barnett & Ceci, 2002; Raviv et al., 2022; Singley & Anderson, 1989; Thorndike & Woodworth, 1901). Research shows that without specific interventions, people often struggle to apply what they learned in one context to even slightly different contexts—a gap between training and transfer (Singley & Anderson, 1989). Improving transfer is crucial for developing flexible expertise in fields ranging from mathematics and physics to everyday problem solving.

One promising route to foster transfer is increasing the variability of practice. The variability principle suggests that practising a wide range of examples or conditions can lead learners to abstract general rules and strategies, rather than overfit to the specifics of a single task (Ossmy et al., 2018; Ossmy et al., 2024; Schmidt, 1975; Schmidt & Bjork, 1992). In motor skill research, for instance, practising a movement under varied conditions produces more robust, generalisable performance on new variations of that movement (Adolph, 2005; Newell & Shapiro, 1976; Ranganathan & Newell, 2013; Shea & Morgan, 1979). Variable practice forces the learner to engage in deeper processing and to formulate a schema that captures the common structure across tasks (Schmidt, 1975). By contrast, repeating the exact same task may yield faster initial gains but often fails to transfer beyond the training scenario (Ossmy et al., 2018; Pacheco & Newell, 2018; Schmidt & Bjork, 1992).

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https://doi.org/10.1016/j.actpsy.2026.107546
Received 10 February 2026; Received in revised form 23 July 2026; Accepted 24 July 2026
Available online 31 July 2026

This trade-off between rapid acquisition and generalization is well documented as a “desirable difficulty”: conditions that challenge the learner (like variability or interleaving of problems) can enhance long-term retention and transfer, even if they slow performance during practice (Bjork, 1994; Kornell & Bjork, 2008; Raviv et al., 2022; Rohrer & Taylor, 2007). For example, students who study interleaved sets of diverse math problems or worked examples show better ability to solve new problems than those who practice the same type of problem repeatedly (Paas & Van Merriënboer, 1994; Rohrer & Taylor, 2007). Likewise, providing multiple distinct examples of a concept can facilitate extraction of the underlying principle, as demonstrated in studies of analogical reasoning (Gick & Holyoak, 1983). High variability in training appears to promote the kind of flexible knowledge representation that underlies adaptive problem solving (Barnett & Koslowski, 2002; Raviv et al., 2022).

In cognitive and educational contexts, the importance of variability has been highlighted in theories of adaptive expertise. Researchers distinguish routine experts who master efficient performance on familiar tasks from adaptive experts who can transfer and innovate on the fly (Branzetti et al., 2024; Brophy et al., 2004; Opre, 2015). A key factor that promotes adaptive expertise is exposure to variability during learning, combined with opportunities for mindful abstraction of principles (Adolph, 2005; Kua et al., 2021; Opre, 2015; Ossmy et al., 2024). Without diverse experiences, learners may become routine experts limited to their practiced routines. Consistent with this idea, studies have found that children and adults who encounter a broader range of problem types develop more generalized problem-solving skills (Chen & Mo, 2004; Kalyuga & Hanham, 2011). For instance, providing varied examples and encouraging comparison help learners notice structural commonalities and apply strategies to new problems (Gentner, 2003; Star & Rittle-Johnson, 2008). Even in perceptual-motor domains, varying training conditions (e.g., practising at multiple distances rather than one fixed distance) improves one's ability to handle novel conditions (Catalano & Kleiner, 1984; Kerr & Booth, 1978). Overall, there is substantial evidence that the practice we engage in—specifically, a variable practice that spans a spectrum of contexts—“makes perfect” in broadening our competence.

Another potential factor in learning is how we engage with practice—that is, whether we learn by actively doing or by observing others. Learning by observation is not simply “passive” because the learner may still predict outcomes, compare alternatives, monitor errors, and mentally simulate actions, even without direct control over the interface. Nevertheless, intuition and several theoretical perspectives suggest that active learning or learning by doing should yield superior outcomes compared with observation (Benware & Deci, 1984; Bruner, 1961; Koedinger et al., 2015). Active training allows learners to test ideas, receive environmental feedback, and revise strategies, which can deepen understanding and memory (Gureckis & Markant, 2012; Markant et al., 2016). In educational studies, students who actively solve problems or generate answers often retain information better than those who read, listen, or watch final solutions without feedback (Bertsch et al., 2007; Chi, 2009; Freeman et al., 2014). Allowing learners to control their experience can focus effort on knowledge gaps and expose otherwise hidden information (Gureckis & Markant, 2012).

However, the empirical evidence on active versus observational learning presents a more complex picture. In some domains, observation can be remarkably effective, sometimes nearly matching active practice under the right conditions. Classic social-learning research demonstrated that humans can acquire complex behaviours by watching others, without receiving direct reinforcement themselves (Bandura & Walters, 1977). For example, infants can learn novel object-directed actions after observing an adult demonstration (Meltzoff, 1988). In motor learning, observers can internalize coordination patterns and form motor memories by watching another person practise, as shown in studies of reaching adaptation (Mattar & Gribble, 2005). Crucially, observational learning should be strongest when learners see not only a correct answer but the evolving attempt, failure, feedback, and revision that make the eventual solution informative. Such sequences invite the observer to predict whether an action will work, diagnose why it failed, and consider what alternative might solve the problem. Thus, observation can provide substantial learning-relevant information when the learner is cognitively engaged with the unfolding action and its consequences.

On the other hand, there are cases where active experience has unique benefits that observation lacks. A prominent experiment by Held and Hein (1963) showed that self-produced movement was necessary for normal perceptual-motor development: kittens that were passively carried in a cart did not learn depth perception, whereas those that moved themselves did. In human infants, opportunities for active exploration (e.g., crawling or walking in various environments) appear to accelerate learning about spatial relations and risks, compared to infants with restricted mobility (Adolph, 1997, 2018). Active learners receive rich multimodal feedback (e.g., proprioceptive and tactile feedback when physically interacting with objects) and have agency to try different actions, which can lead to a deeper understanding of cause and effect. In problem-solving, actively generating solutions (even incorrect ones) may enhance later learning by making the learner more aware of the problem structure, a phenomenon sometimes called the “generation effect” in memory research (Ossmy et al., 2021; Slamecka & Graf, 1978). Additionally, active participation often increases motivation and attention—learners are generally more focused when they are doing something than when watching passively (Benware & Deci, 1984). Indeed, in educational settings, active learning methods (like interactive activities in class) have been shown to produce better test performance than traditional lectures, even though students feel they learn less during active engagement (Deslauriers et al., 2019). This underscores that active practice can impart benefits that learners might not immediately recognize, such as improved long-term understanding.

Given that both practice variability and active engagement have been advocated as ways to improve learning, an important question arises: which factor is more critical for promoting adaptive problem solving? Do they offer independent, additive benefits, or does one outweigh the other? Surprisingly, few studies have directly compared the relative contributions of variability and agency within the same learning paradigm. In theory, the ideal training might be actively practising a wide variety of problems, leveraging both factors. Many real-world learning contexts, however, involve trade-offs. Sometimes learners must observe worked examples or demonstrations due to safety, time, or resource constraints, and sometimes training content is limited in variety. Understanding whether cognitively engaged observation can substitute for active doing, and whether variety can compensate for lack of direct agency (or vice versa), has practical significance for designing curricula and training programmes.

There are reasons to expect either an interaction between variability and agency or a dominant effect of one factor. Diverse examples may be most useful when learners can actively test and revise their own hypotheses. Conversely, motivated observers who see the same varied examples, failures, and outcomes may extract much of the same causal structure. Theories of “learning to learn” propose that repeated experience across different problems can produce a general learning set (Adolph, 2005; Harlow, 1949), and observational exposure to multiple problem forms may similarly reveal a range of solutions and support analogical transfer (Catrambone & Holyoak, 1989; Gick & Holyoak, 1983). By contrast, active practice with a narrow set may cultivate routine rather than adaptive expertise (Branzetti et al., 2024). These alternatives predict either synergy between agency and variability or a dominant contribution of variability, motivating a direct comparison of both factors within one paradigm.

In this preregistered study, we tested the effects of training variability and agency on adaptive problem solving. We implemented a controlled experiment using a within-domain physical reasoning task known as the Virtual Tools puzzle (Fig. 1; Allen et al., 2024, 2020; Cheng

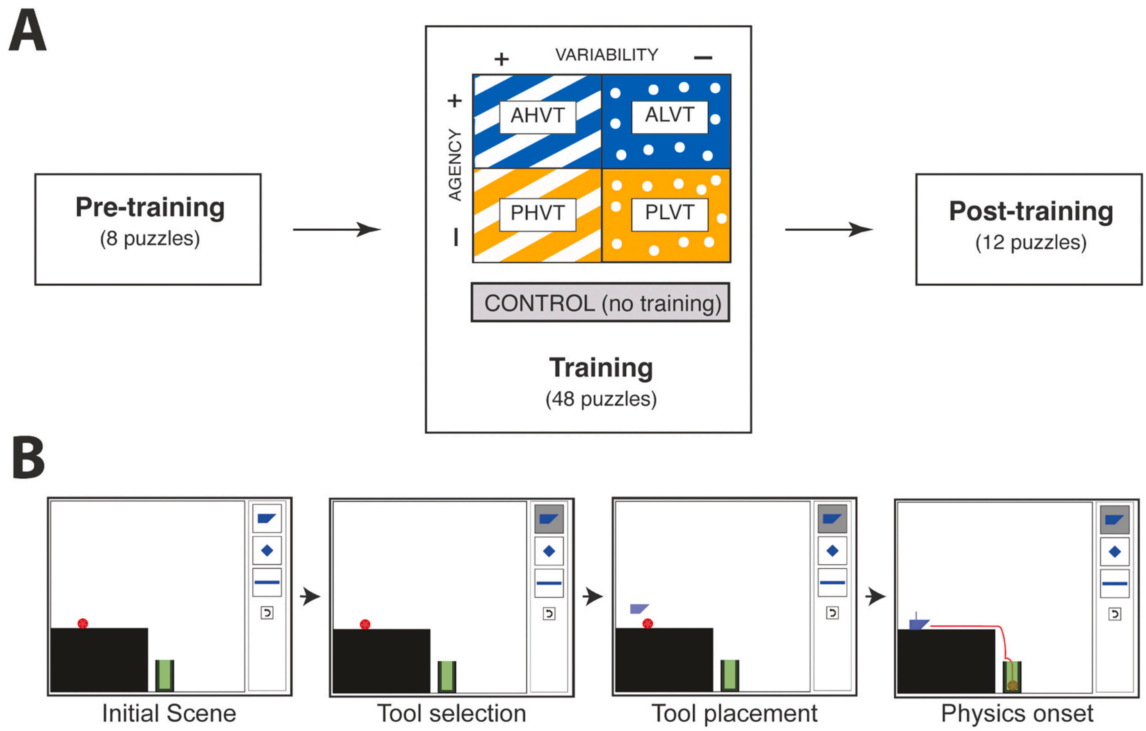

Fig. 1. Experimental design. (A) All participants completed a pre-training test (8 novel Virtual Tools puzzles; Fig. S1) and were then randomly assigned to one of five conditions: Active High-Variability Training (AHVT), Active Low-Variability Training (ALVT), Passive High-Variability Training (PHVT; observational-replay condition), Passive Low-Variability Training (PLVT; observational-replay condition), or a no-training control. The four training groups followed a 2 × 2 factorial manipulation of Training Variability (high: many distinct puzzles; low: a small set repeated) and Training Agency (active: participants solved puzzles; observational: participants watched yoked replays of an active participant's trial-and-error attempts), followed by a post-training test (12 novel puzzles; Fig. S2). (B) Example Virtual Tools puzzle. The goal is to move the target (red) into the goal region (green) by selecting and placing one of the available tools (blue). (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

et al., 2024; Dexter et al., 2025; Grandchamp des Raux et al., 2024) to provide a rich problem-solving environment. The Virtual Tools puzzle requires players to solve physics-based puzzles by choosing and placing virtual objects (tools) in a scene to achieve a goal (for example, knocking a target off a platform or guiding a ball into a container). It offers a large space of possible actions and solutions, making it an ideal model system for adaptive problem solving: participants can approach each puzzle in multiple ways and must discover what works through trial and error (Allen et al., 2020). The task, therefore, captures core components of adaptive physical problem solving, including affordance detection, means-end planning, intuitive physics prediction, causal inference from feedback, inhibition of ineffective repetitions, and flexible revision of strategy. This task domain allowed us to track not only whether participants succeeded in solving novel problems, but also how they attempted solutions, including measures of tool selection, switching between tools or strategies, and adjustment of actions based on feedback. Using this paradigm, we designed a training intervention with two key manipulations: Training Variability (High vs. Low) and Training Agency (Active vs. Observational), along with a no-training control.

At the same time, Virtual Tools is deliberately narrower than real-world adaptive problem solving. It does not capture collaborative reasoning, explicit metacognitive explanation, extended multi-step project planning, or the motivational and classroom-management features of instruction. It also reduces embodied motor demands to a two-dimensional point-and-click action. This constraint was useful for isolating variability and agency, but it limits generalization to domains in which bodily coordination, social interaction, or symbolic reasoning are central.

All participants, including an untrained control group, completed the same pre-training test and post-training test phases consisting of solving a set of novel Virtual Tools puzzles without assistance. We first hypothesized that training variability would enhance adaptive problem solving: participants trained on a wide variety of problems were expected to show greater gains in post-training test performance (e.g., higher success rates on new puzzles, requiring fewer attempts to solve them) and more flexible strategy use (e.g., readily switching tools or approaches when needed) compared to those trained on low-variety problems. This hypothesis was grounded in the literature on variable practice and transfer (Gick & Holyoak, 1983; Ossmy et al., 2018; Schmidt, 1975). Second, we hypothesized that training agency (active involvement) would improve learning relative to observational learning. Active participants were expected to outperform yoked observers on post-training test measures, even if they saw the same problems, due to the presumed benefits of hands-on problem solving (Chi, 2009). Finally, our preregistered predictions leaned towards a model where each factor contributes positively to learning, but with an interaction between the two factors. Yet, we did not make a specific prediction, as it is plausible that active high-variability training might yield especially great improvements, or conversely, that high variability might benefit even observational learners so much that agency differences would be small.

2. Methods

The experiment was preregistered on the Open Science Framework (https://osf.io/wq8bf/overview).

2.1. Participants

Two-hundred volunteers (159 females; Mage = 38.9 years; range 20–51) completed the online study. Participants were recruited using Prolific (prolific.co/), an outsourced method of crowdsourcing. All participants provided informed consent to participate and were compensated (depending on the duration of the experiment; the rate was £5.01 per hour of participation). The study was approved by the ethics committee at Birkbeck, University of London. This study aimed to test variability and agency in a stable learner population before translating the paradigm to developmental or classroom samples. Therefore, we used adult online participants who can follow complex task instructions, complete a relatively long factorial training protocol, and provide sufficient statistical power for isolating the two experimental manipulations. We did not select participants on the basis of physics, engineering, video-game, or educational background, and these characteristics were not measured; random assignment should distribute such experience across conditions, but future work should collect these data explicitly when testing educational generalisability.

All participants provided informed consent in accordance with the local research ethics committee. Participants were randomly assigned to one of five conditions: Active High-Variability Training (AHVT), Active Low-Variability Training (ALVT), Observational High-Variability Training (PHVT; passive-replay label retained for consistency with preregistration), Observational Low-Variability Training (PLVT; passive-replay label retained for consistency with preregistration), or control. We aimed to assign participants in equal numbers, having 40 participants per group. However, 18 participants from the control group were excluded due to a technical malfunction in our task and their data were not saved properly. Thus, data from 182 participants were included in analyses.

2.2. Task

We used the Virtual Tools platform as our problem-solving task (Fig. 1B; Allen et al., 2020). Virtual Tools is a computer-based two-dimensional physics puzzle environment. In each puzzle level, the player is presented with a scene containing stationary objects (e.g., platforms, walls, ramps) and a goal (such as a target object that must be moved or a ball that must reach a certain location). The player is provided with a set of three moveable "tool" objects (for example, planks, balls, or bars) that they can use to interact with the scene. To attempt a solution, the player selects one of the available tools and places it at a desired position within the scene, then releases it to let physics take its course. Once the tool is placed and released, gravity and collisions are simulated: the tool falls or moves as it normally would, potentially hitting other objects in the scene.

The objective of each puzzle is typically to get the scene to a certain end state (for instance, to knock the target off its perch or get the ball into a container) using the tool. If the attempt achieves the goal, the level is solved. If not (e.g., the tool misses the target or nothing happens), the player can try again. They may either reposition the same tool and attempt a different placement or choose a different tool for the next attempt. Each puzzle thus allows multiple attempts, and participants can learn from the outcome of each attempt to adjust their strategy.

For our experiment, we created a set of puzzle levels within the Virtual Tools framework, covering a range of problem types (see Fig. S1). All puzzles were designed such that they were solvable with at least one of the provided tools, but the appropriate choice of tool and placement varied by puzzle. For example, one puzzle might require using a long plank as a bridge to cross a gap, another puzzle might require dropping a ball to knock over a tower, and another might involve using a bar to prop an object up. These levels were based on those used by Allen et al. (2020) and were piloted to ensure broadly comparable difficulty range (not too trivial, but solvable within a few attempts by naïve participants). Virtual Tools puzzles equalize action possibilities: all participants have the same three tools and can place them anywhere on a 600 × 600-pixel canvas, yielding a large action space. This feature captures the open-ended nature of the problem-solving task, requiring participants to strategically narrow down possible actions. The software recorded each attempt's details, including which tool was chosen, where it was placed, and whether the attempt succeeded. This enabled us to derive performance metrics (like success or number of attempts) as well as strategy metrics (like whether the participant switched tool types between attempts or how they adjusted placement). Because every failed attempt changes what the participant knows about the scene, the task also requires monitoring feedback, updating a causal model of the puzzle, and deciding whether to refine a current strategy or abandon it for a different tool or placement.

2.3. Design & procedure

The experiment consisted of three phases for each participant: a Pre-training test, a Training phase, and a Post-training test (Fig. 1A). We had a between-subject design, and we manipulated two factors—Training Variability (High vs. Low) and Training Agency (Active vs. Observational)—plus a control condition with no training. Participants in the four training groups experienced different versions of the training phase as described below, while the pre-training and post-training tests were identical for all groups. The study was conducted individually on a computer, with instructions and tasks presented via a custom interface for the Virtual Tools platform (the experiment is available at: https://psyc.bbk.ac.uk/ex/241/258/). Participants in all conditions were given the same general instructions about the task.

2.3.1. Pre-training phase

At the start, all participants (including control) completed a set of baseline puzzles without prior exposure to the puzzle. The pre-training test consisted of 8 puzzle levels that sampled the range of problem types in the domain (see games in Fig. S1). These baseline puzzles were distinct from the training and post-training test puzzles to avoid overlap. Participants attempted to solve each pre-training test puzzle with the tools available. They had a maximum of 6 attempts per puzzle to find a solution before the puzzle automatically moved on. During this pre-training test, no explicit performance feedback or instruction was given beyond the natural feedback of seeing the result of their attempts. We measured participants' performance and strategy in this phase (see details in Performance and strategy measures), and those later served as covariates and baseline indicators.

After the pre-training test, participants in training groups proceeded to the training phase, while control group participants took a break of equivalent duration (approximately 15–20 min, matching the training time) that did not involve problem-solving practice.

2.3.2. Training phase

The four training conditions differed in two aspects (Fig. 1A): Variability of the training problems (High vs. Low) and Agency of the participant (Active vs. Observational).

2.3.2.1. High-variability training (HVT). Participants in the high-variability conditions (AHVT and PHVT) completed 48 distinct training trials. The number 48 equated total exposure with the low-variability conditions while allowing broad sampling of the Virtual Tools problem space. The set was selected to maximise coverage across four intersecting dimensions: (i) scene geometry, including ramps, gaps, platforms, barriers, containers, and moving versus stationary elements; (ii) tool affordances, including supporting or bridging, blocking, striking or pushing, redirecting, propping, and counterweighting; (iii) causal solution principles, including gravity-driven falling, collision and force transfer, rolling or sliding, leverage or catapulting, support, and containment; and (iv) the relation between the available tools and the goal, such that the useful tool and its placement differed across trials. Thus, diversity reflected both surface-level changes in layout and changes in the underlying physical principle required for solution. Each training puzzle could be attempted once before the task advanced, and puzzle order was pseudo-randomised. Active participants solved the trials themselves; observational participants watched yoked replays as described below.

2.3.2.2. Low-variability training (LVT). Participants in the low-variability conditions (ALVT and PLVT) experienced a narrower sample of the same problem space, emphasizing repetition and refinement within a limited set. Specifically, they trained on 12 distinct puzzle configurations, selected from a restricted subset of the layout, affordance, and causal-solution categories represented in the high-variability set, and each configuration was repeated four times in separately randomised blocks. This 12 × 4 structure equated the total number of training trials across variability conditions (48 trials) while contrasting breadth of experience with repeated practice. Repetition gave LVT participants multiple opportunities to refine solutions to recurring configurations, potentially supporting mastery of that narrower subset.

2.3.3. Active vs. observational agency

In Active training conditions (AHVT and ALVT), participants had full control during the training puzzles. They actively selected tools, placed them, and attempted to solve each training puzzle just as they did in the pre-training test. They received feedback through the outcome of each attempt (e.g., seeing the tool fall and whether it achieved the goal). They could plan their actions and learn by active trial and error. In Observational training conditions (PHVT and PLVT), participants did not interact with the puzzles directly. Instead, they watched a series of recorded demonstrations corresponding to the training puzzles. For each training puzzle that their active counterparts attempted, observational participants viewed a screen-capture replay of an active participant attempting that puzzle. These replays included the full process rather than only final solutions: failed attempts, corrective adjustments, and eventual successes were all visible. This feature is central to our interpretation because it made observation informationally rich and likely cognitively active. Essentially, a participant in PHVT saw videos of an AHVT participant attempting the 48 puzzles one by one; a participant in PLVT saw videos of an ALVT participant working through the 12 puzzles, each repeated 4 times. This yoked design ensured that observational trainees observed the same sequence of problems, tools chosen, and outcomes that an active learner experienced, providing identical visual information about which actions worked or failed. Observational participants were instructed to pay close attention to the screen during these videos and to try to understand how each solution worked. They did not receive any additional commentary—they learned purely by observation of behaviour and outcomes.

Throughout training, the interface for active participants provided immediate visual feedback (the physics simulation). Observational participants saw the same visual events via replay. Neither group received explicit verbal instruction or coaching on strategy; any learning had to come from the task experience or observation alone. Thus, the agency manipulation isolated control over action selection and placement, not access to visual feedback about attempts and outcomes. The training phase lasted approximately 15–20 min for all groups. To keep timing consistent, any minor downtime was filled with a waiting screen so that the post-training test did not begin early for some participants.

2.3.4. Post-training phase

After training, all participants (including the control group, who had just waited) completed the post-training test, which was the critical assessment of adaptive problem solving. The post-training test consisted of a new set of 12 puzzle levels that had *not been* seen previously in the pre-training test or training (see Fig. S2 for all games played in the post-training phase). These post-training test puzzles were of similar difficulty to the pre-training test, but included different problems and required different solutions. Importantly, some post-training test puzzles were designed to be conceptually similar to one or two of the training puzzles, while others were different, to examine both near and far transfer within the domain. Similar to the pre-training test, participants had a maximum of 6 attempts to succeed. We calculated the same performance and strategy measures as in the pre-training phase.

2.4. Performance and strategy measures

To assess how participants completed the puzzles during the pre-training and post-training tests, we measured their performance and strategy. Performance in each phase was calculated by Success Rate (the number or proportion of puzzles solved successfully within the attempt limit) and Attempts per Puzzle (the average number of attempts participants took to solve each puzzle, with unsolved puzzles counted as 8 attempts by default).

In addition, we captured strategy measures by analysing how participants attempted to solve each puzzle. We calculated (i) the average number of distinct tools used for each puzzle, (ii) the percentage of tool-switching across attempts, and (iii) the average tool-positioning distance across attempts. In this analysis, we focused only on puzzles in which the participant failed to solve the puzzle on the first try. These measures index how learners searched the available action space after failure: whether they broadened the search to additional tools, switched to a different tool, or refined the spatial implementation of the current approach.

To calculate the percentage of tool switching, for each puzzle and each attempt (starting from the second attempt), we compared whether the selected tool was identical to the one used in the previous attempt. For each puzzle, we evaluated the percentage of attempts in which the selected tool changed, and then averaged this percentage across all puzzles. This metric reflects one form of exploratory flexibility: a higher switching rate indicates willingness to try an alternate approach when one attempt fails, whereas a low switching rate might indicate perseveration on one method. However, switching is not intrinsically good or bad. When paired with success and fewer attempts, switching can indicate adaptive revision; when paired with many unsuccessful attempts, it can indicate inefficient trial-and-error. We therefore interpret switching jointly with success, attempts, and tool-use measures rather than as a standalone marker of skill.

For the positioning distance, we calculated for each puzzle the average Euclidean distance in pixels between the positioning of the tool in each attempt and the previous attempt. For each puzzle, we averaged this Euclidean distance across all attempts (starting from the second attempt), yielding an average tool-use positioning distance per puzzle. Then, we averaged the positioning distance across all puzzles. This is complementary to switching; participants could either adjust a current strategy or switch to a new one after failures. Positioning distance was used as an implementation-precision measure: smaller successive movements suggest finer spatial calibration of a chosen strategy, whereas larger movements suggest broader relocation within the problem space. Number of tools used was interpreted as the breadth of action search or redundant exploration. Together, number of tools, tool-switching, and tool-positioning distance provide a behavioural profile of adaptive problem solving, yet they are not intended to directly measure metacognition, explicit planning, motivation, or learners' verbal explanations of their strategy. All these measures were computed from the detailed log data of the pre- and post-training tests.

2.5. Statistical analyses

All analyses were conducted using R and SPSS software. We analysed the effects of training condition on post-training performance and strategy measures using between-groups comparisons and controlling for baseline skills. Given that random assignment should equate groups, but small baseline differences could add noise, our preregistered plan was to use analysis of covariance (ANCOVA) for each key dependent measure. The ANCOVAs included training condition (5 levels: AHVT, ALVT, PHVT, PLVT, control) as a fixed factor and the corresponding pre-training test measure as a covariate (to control for any pre-existing differences in cognitive skills). For example, for post-training success rate we included pre-training success rate as a covariate; for post-training test average attempts we included pre-training test attempts, and so on. This approach adjusts the post-training test scores for where participants started, providing a more sensitive test of training effects. Preliminary checks confirmed that groups did not significantly differ at baseline on any measure (all $p > .20$ for one-way ANOVAs on pre-training performance across groups), supporting the validity of the ANCOVA approach. We also evaluated standard ANCOVA assumptions before interpreting the models: residual distributions were inspected for major departures from normality, Levene-type checks were used to evaluate homogeneity of variance, and group × covariate terms were examined to assess homogeneity of regression slopes. These checks did not reveal violations that altered the interpretation of the reported effects. Covariate-adjusted means reported in the figures were estimated at the sample mean of the corresponding pre-training covariate; because baseline group differences were small, the adjustment primarily reduced residual error rather than changing the qualitative ordering of groups. To make the magnitude and uncertainty of the primary post-training test effects directly evaluable, Supplemental Table S1 reports group sample sizes, covariate-adjusted means, and 95% confidence intervals for post-training test success and attempts.

Our main hypotheses concerned the two training factors: Variability and Agency. Although we had five groups including control, we carried out planned contrasts and secondary analyses to interpret effects. In addition, we conducted follow-up 2 × 2 factorial ANCOVAs on the four training groups (omitting control) as a complementary analysis to directly test for main effects of Variability (High vs Low) and Agency (Active vs Observational), and their interaction. Key comparisons of interest (specified a priori) were: (a) High-variability vs Low-variability (averaging over agency), to test the effect of variability; (b) Active vs Observational (averaging over variability), to test the effect of agency; and (c) Each of the four training groups vs control, to see which training conditions outperformed having no training. The control group was used primarily to test whether training in general led to improvement relative to no training.

3. Results

3.1. *Training variability, but not agency, improved problem solving*

We first compared the five groups on post-training success rate (percentage of puzzles solved), controlling for pre-training success (Fig. 2A). The ANCOVA revealed a significant effect of group, F(4,176) = 10.02, $p < .001$, partial $\eta^2 = 0.19$, indicating reliable differences in adjusted success across training conditions. This effect size indicates that the training-condition differences were substantial relative to the residual within-group variability shown by the error bars in Fig. 2A. Sidak-corrected pairwise comparisons showed that all four training groups outperformed the control group ($ps < .046$). Within the training conditions, the only significant difference was between the two active groups: AHVT achieved higher adjusted success than ALVT (estimate = 13.85 percentage points, $p = .044$). The two high-variability groups (AHVT, PHVT) did not differ from each other ($p \approx 1.00$), and the two low-variability groups (ALVT, PLVT) were statistically indistinguishable ($p = 1.00$). Thus, all forms of training increased success relative to no training, and active high-variability practice yielded a modest advantage over active repetitive practice.

To test the specific contributions of variability and agency, we then re-analysed the four training groups in a 2 × 2 ANCOVA with factors Variability (high vs. low) and Agency (active vs. observational), again controlling for pre-training performance. Using the number of puzzles solved (success counts) as the dependent variable, the model confirmed a significant main effect of variability, $F(1,156) = 13.38$, $p < .001$, but no main effect of agency, $F(1,156) = 0.16$, $p = .69$, and no Variability × Agency interaction, $F(1,156) = 0.18$, $p = .67$. Collapsing across agency, high-variability groups solved more puzzles than low-variability groups. Collapsing across variability, active and observational groups showed virtually identical adjusted success. These analyses confirm that variability, not agency, drove the improvements in problem-solving success.

We found a parallel pattern in the average number of attempts per puzzle (Fig. 2B). A five-group ANCOVA on the average number of attempts pre/post-training puzzle, controlling for pre-training attempts, yielded a significant group effect, F(4,173) = 38.39, $p < .001$, partial $\eta^2 = 0.47$. The magnitude of this effect shows that the improvement in efficiency was large relative to natural variability among learners. Sidak post-hoc tests showed that all four training groups required fewer attempts than the control group ($ps < .001$). Within the training conditions, both high-variability groups were more efficient than the active low-variability group: AHVT vs ALVT, $p = .042$; PHVT vs ALVT, $p = .032$. The two high-variability groups did not differ from one another ($p = 1.00$), and ALVT and PLVT also showed no reliable difference ($p = .99$). The corresponding 2 × 2 ANCOVA on training groups confirmed a main effect of variability, F(1,156) = 25.38, $p < .001$, with high-

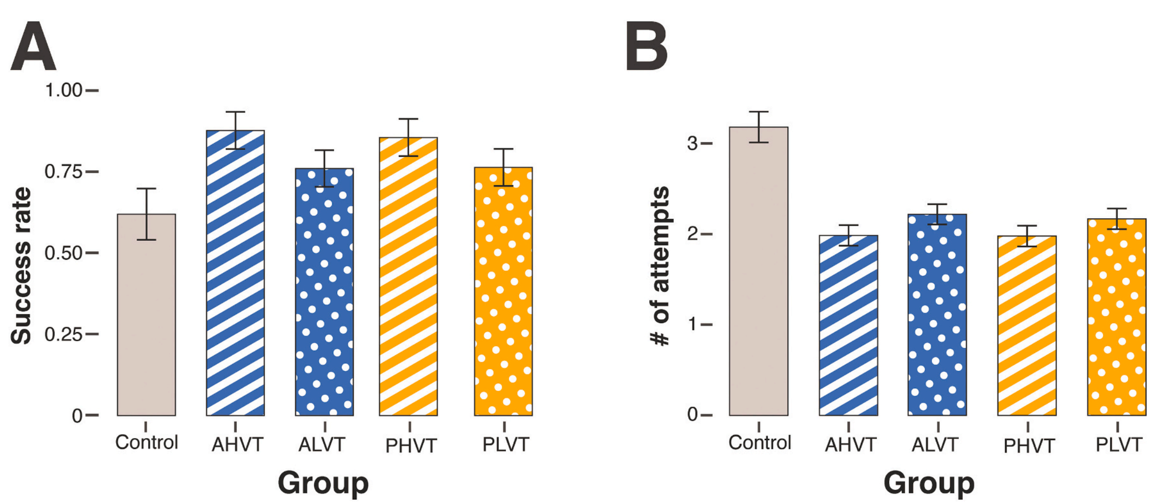

Fig. 2. Group differences in performance. (A) Post-training test success (percentage of puzzles solved) by condition, controlling for pre-training test success. (B) Post-training test efficiency (mean attempts per puzzle; lower values indicate more efficient solutions) by condition, controlling for pre-training test efficiency. Values depict covariate-adjusted group means estimated at the sample mean of the relevant pre-training test covariate; error bars show ±1 SE. High-variability training produced higher success and greater efficiency than low-variability training, with no reliable differences between active and observational training. Because pre-training test group differences were small, covariate adjustment was primarily used to improve precision rather than to correct large baseline imbalance.

variability learners using fewer attempts on average than low-variability learners. Agency did not significantly influence attempts, $F(1,156) = 1.12$, $p = .29$, and the interaction was negligible, $F(1,156) = 0.06$, $p = .80$. Thus, training on a broader set of problems yielded more accurate and more efficient solutions on new puzzles, regardless of whether participants learned by doing or by observing.

3.2. *Training variability streamlined strategy search, whereas agency improved spatial precision*

We next analysed strategy measures derived from the detailed tool-use logs: the number of tools used per puzzle, switching rate (proportion of tool changes between attempts; see Methods), and tool-positioning distance (average Euclidean distance between successive placements; see Methods). For the number of tools used per puzzle (Fig. 3A), a five-group ANCOVA with pre-training tool use as covariate indicated a robust group effect, $F(4,176) = 36.03$, $p < .001$. Sidak-corrected comparisons showed that all four training groups used fewer distinct tools per puzzle than the control group (*ps* $< .001$), suggesting that training led to more streamlined solutions. Differences among the four training groups were small and not reliable (*ps* $\geq .56$). The 2×2 ANCOVA restricted to training conditions revealed a significant main effect of variability, $F(1,156) = 11.18$, $p = .001$, with low-variability learners using more tools per puzzle than high-variability learners. Agency again had no significant effect, $F(1,156) = 0.13$, $p = .72$, and there was no interaction, $F(1,156) = 0.57$, $p = .45$. Together, these findings indicate that any training reduced redundant tool use relative to no training, and high-variability practice yielded slightly leaner tool use than repetitive practice.

For tool switching rate (Fig. 3B), we computed, for each participant, the mean proportion of attempts in which the chosen tool differed from the previous attempt. A five-group ANCOVA with pre-training switching as covariate yielded a significant effect of group, $F(4,173) = 18.47$, $p < .001$. According to Sidak-corrected post-hoc tests, controls showed substantially higher switching than every training group (all control vs training comparisons, $p < .001$), consistent with more trial-and-error behaviour in the absence of prior practice. Differences among the four training groups were not reliable (*ps* $\geq .50$). In the 2×2 ANCOVA on training groups, variability produced a small but significant effect, $F(1,156) = 6.47$, $p = .012$: low-variability groups switched tools slightly more often than high-variability groups. Agency did not affect switching, $F(1,156) = 0.02$, $p = .88$, and the interaction was negligible, $F(1,156) = 0.09$, $p = .76$. Thus, training in any form reduced "flailing" relative to control, and repetitive practice prompted slightly more tool changes than variable practice, likely reflecting the need for additional attempts to reach a solution.

Finally, for tool-positioning distance (Fig. 3C), we computed for each participant the mean Euclidean distance between successive placements of the tool within a puzzle. The five-group ANCOVA (controlling for pre-training distance) revealed a significant group effect, $F(4,176) = 4.00$, $p = .004$. Pairwise post-hoc tests indicated that AHVT participants adjusted tool positions over shorter distances than some other groups (*ps* $< .05$), including control and PHVT. The 2×2 ANCOVA on training groups showed no main effect of variability, $F(1,156) = 0.00$, $p = .95$, but a clear main effect of agency, $F(1,156) = 11.63$, $p = .001$: active trainees made smaller spatial adjustments between attempts than observational trainees, suggesting more precise placement. Variability $\times$ Agency did not interact, $F(1,156) = 0.54$, $p = .46$. Thus, if spatial precision of placement is treated as an outcome in its own right, active learners did show an advantage. This agency effect was specific to fine-grained implementation of a chosen action and did not translate into higher overall success or fewer attempts.

4. Discussion

This study tested which aspect of practice is critical in "making perfect" adaptive problem solving: the variability of training experiences, the agency of the trainee, or an interaction between the two. Our experiment, using physical reasoning puzzles (Virtual Tools; Allen et al., 2020) and a factorial training design, provides clear evidence that variability was the stronger determinant of transfer to new challenges within the same problem space. High-variability training yielded greater success and efficiency than low-variability training in the factorial analyses. Importantly, however, all four training groups outperformed the no-training control group on success, attempts, number of tools used, and switching rate; the low-variability conditions therefore produced meaningful benefits, but smaller gains than high-variability training. Participants who actively practised did not significantly outperform those who observed the same practice on success or efficiency when observational learners were exposed to the full trial-and-error process. Indeed, observational high-variability training showed comparable success and required fewer attempts than active low-variability training. This finding—that what learners practise (varied versus repetitive scenarios) can matter more for transfer than whether they physically perform each attempt—has important theoretical and practical implications. However, the result should not be read as evidence that passive exposure is generally sufficient. The observational condition in this study was unusually informative: learners saw attempts unfold, saw failures, and saw the consequences that made later adjustments meaningful.

Our hypotheses anticipated that both factors would aid learning, but the results showed one factor dominated. Variability was the driving

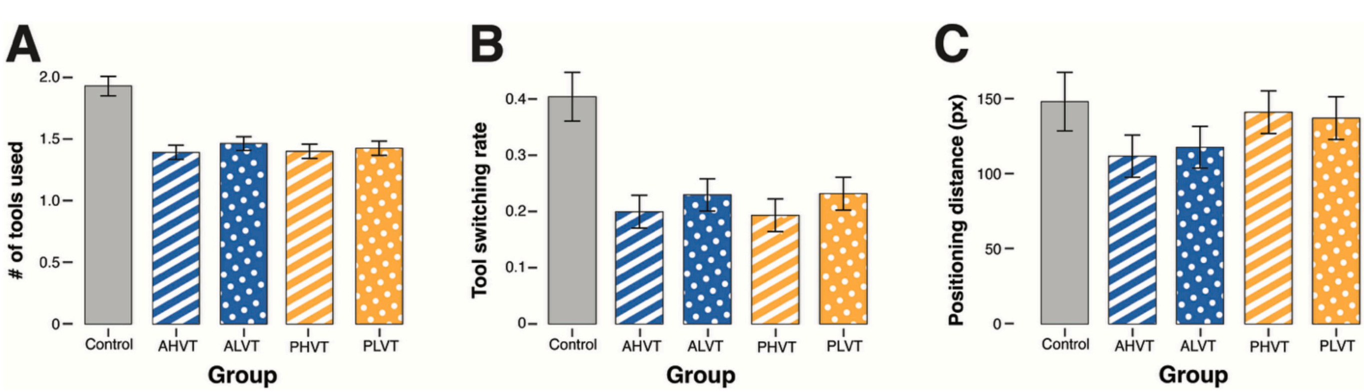

Fig. 3. Group differences in strategy. (A) Number of distinct tools used per puzzle. (B) Tool switching rate (proportion of successive attempts involving a change of tool). (C) Positioning distance (mean Euclidean distance between successive placements). Strategy measures were computed for puzzles requiring more than one attempt and adjusted for the corresponding pre-training test measures; error bars show ±1 SE. Relative to Control, all training conditions reduced redundant tool use and switching. In the factorial analyses of the four training conditions, high-variability training yielded leaner tool use and lower switching than low-variability training, whereas active training yielded smaller, more fine-grained spatial adjustments than observational training.

force behind adaptive learning in this context, aligning with a rich literature emphasizing the role of broad experience in developing transferable skills (Catrambone & Holyoak, 1989; Gentner et al., 2003; Ossmy, 2026; Ossmy et al., 2018; Paas & Van Merriënboer, 1994; Schmidt, 1975; Schmidt & Bjork, 1992; Serino & Ossmy, 2025). Conversely, the negligible role of agency in post-training success and efficiency was initially surprising given common assumptions about active learning (Bruner, 1961; Chi, 2009), but it resonates with research showing observational learning can be powerful under conducive conditions (Bandura & Walters, 1977; Mattar & Gribble, 2005).

Why does high variability training enhance adaptive problem solving? Our results confirm that exposure to a wide range of problem instances is a potent catalyst for "learning to learn" within a specific problem space. High variability training likely works by forcing learners to abstract general principles and recognize deeper commonalities across problems. When each task is different, learners cannot rely on rote memory of a single solution; instead, they must grasp why a particular solution works for one puzzle and what might work for another. This fosters the formation of a more general mental model of the problem space (Newell & Simon, 1972). In our experiment, high-variability participants experienced many distinct physics scenarios—some requiring pushing, some blocking, some bridging, and so on. Over time, they may have extracted rules like "a plank can support or transfer force" or "a heavy ball can knock things over" and learned when to apply each tool. This idea is consistent with schema theory from motor learning (Schmidt, 1975; Van Rossum, 1990): practising varied movements allows the learner to develop a schema (general rule) relating actions to outcomes, which can be applied flexibly to new movements. Analogously, varied puzzle practice builds a schema of cause–effect relations in the puzzle domain, supporting transfer to new puzzles.

These results are align with the concept of adaptive expertise: varied experiences combined with mindful engagement can produce learners who perform effectively while retaining the capacity to adapt (Barnett & Koslowski, 2002; Branzetti et al., 2024; Ossmy, 2026). In the present study, high-variability learners showed higher success, greater efficiency, and a leaner search across tools than low-variability learners in the factorial analyses. This pattern reinforces the recommendation that education and training should incorporate structured variability rather than rely exclusively on narrow drilling (Paas & Van Merriënboer, 1994). The effects emerged after a brief training period; longer-term work is needed to determine whether the advantage widens, stabilises, or changes with extended practice.

Our findings also resonate with developmental observations. Infants and children naturally encounter variability in their environment and through self-motivated play they experience a range of scenarios, which is thought to be crucial for developing robust cognitive and motor skills. Adolph and colleagues have shown that infants who experience varied motor challenges (like navigating different slopes, surfaces, and gaps) learn more flexible locomotor strategies and are better at gauging their abilities on new challenges, compared to infants with less varied experience (Adolph, 1997; Adolph & Avolio, 2000). In our adult analogue, varied training puzzles served as diverse challenges that encouraged the exploration of the solution space. Just as infants benefit from "variable practice" in learning to walk adaptively (Karbach & Kray, 2009; Mulavara et al., 2009), adults benefit from variable practice in a cognitive-physical task to learn adaptive problem-solving strategies (Siegler, 2007).

Why did active training confer no significant advantage over observational training on success or efficiency? The observational condition was not a conventional passive-learning condition: participants watched complete trial-and-error sequences, including mistakes, corrective adjustments, and successes. This likely engaged prediction and diagnostic reasoning, allowing observers to compare expected and observed outcomes and infer why an action should be modified. In this respect, the condition resembled cognitively engaged example-based learning more than passive listening or exposure to final answers. Social-learning accounts establish that observation can support acquisition of complex behaviour (Bandura & Walters, 1977), and motor-learning studies show that observing another person practise can produce measurable learning (Mattar & Gribble, 2005). Where the principal challenge is selecting a causal strategy rather than calibrating a demanding motor action, rich observation may therefore provide much of the information required for transfer.

It is also important to acknowledge that active control can have benefits that our measurements may not have captured. For instance, being active could increase intrinsic motivation or confidence (Boot et al., 2013; Rhodes & Courneya, 2004). We did not explicitly measure motivation or long-term retention; it is possible that active participants might retain the skills longer or feel more confident tackling new tasks beyond the immediate post-training test (though we did not see an immediate performance difference). Moreover, if the task had a stronger sensorimotor component—for example, if precise motor skill or timing was crucial—active practice might have yielded an advantage due to motor learning. In the Virtual Tools problem, the motor demands are low (point-and-click interface) and mostly a cognitive strategy is needed, which likely levels the field between doing and watching. In tasks that require bodily coordination or calibration (like riding a bicycle or playing a musical instrument), observation alone might not suffice; physical practice is required to calibrate performance using sensory feedback. Indeed, previous research shows clear benefits of active experience (Held & Hein, 1963). Our results, therefore, should be interpreted in context: for cognitive/analytical problem solving with observable feedback, observational learning can be almost as effective as direct experience.

These findings also contrast with some educational studies. In typical classroom settings, active participation (working through problems, discussions) often improves understanding compared to passive listening (Deslauriers et al., 2019; Freeman et al., 2014; Peleg et al., 2026). However, in those situations, the passive condition often lacks engagement or feedback—students might 'zone out' during lectures. In our study, observational participants still received frequent feedback (seeing what happened after each attempt) and likely maintained focus as they performed better than the control group. Thus, our observational condition might be more akin to guided observation. This suggests that if done properly, observational learning can be a viable mode of instruction, especially when physical practice is impractical or when one can demonstrate many examples efficiently via video or demonstration. Our observational high-variability group, for instance, effectively "learned from examples"—they watched example problems being solved. Example-based learning is known to be effective for novices as long as they process the examples deeply (Atkinson & Renkl, 2007). The present findings suggest that examples may be especially powerful when they include the learner-relevant path to a solution, including errors and revisions, rather than only final procedures.

The findings contribute to theories of skill acquisition and transfer by highlighting the importance of content diversity relative to physical interactivity in this learning context. They support an emergentist view of strategy learning (Ossmy & Adolph, 2020): learners improve by encountering a repertoire of approaches and progressively selecting those suited to each situation. Even observers could build such a repertoire by seeing varied attempts and their consequences. The high-variability advantage was expressed as higher success and efficiency together with leaner tool use and lower switching, not as indiscriminate switching between alternatives. Thus, the information extracted from diverse experience may be more important for building a broad problem-solving schema than direct control over each exploratory action, provided that observation includes informative attempts, feedback, and revision (Adolph et al., 2020; Gibson, 1979).

A distinct pattern emerged for tool-positioning distance: agency, but not variability, shaped how precisely participants adjusted placements between attempts. Active trainees made smaller spatial adjustments than observational trainees, suggesting that placing the tools themselves calibrated a fine-grained mapping between cursor movement and the virtual environment. Sensorimotor accounts propose that self-generated movement provides action-contingent error signals that can tune internal predictions (Wolpert et al., 2011). Observers saw the same outcomes but did not generate the corresponding motor commands, and may therefore have formed a coarser spatial policy when later implementing their own solutions (Mattar & Gribble, 2005; Williams & Gribble, 2012). Agency therefore mattered for the precision with which a chosen strategy was implemented, even though it did not alter which strategy was selected or the broader outcomes of success and efficiency.

From an applied perspective, our findings suggest that the priority for training programs, education, and even everyday learning, should be to ensure variability in practice examples if the goal is transferable problem-solving skill. Whether the learner actively performs or observes may be a secondary consideration—which is not to say it never matters, but that providing diverse experience is paramount. For example, in teaching science or engineering problem solving, instructors might focus on giving students a range of problem scenarios to work through (or watch demonstrations of) rather than having them repeatedly practice the exact same problem type. In an undergraduate engineering mechanics course, for example, this could mean varying the surface structure and solution principle across problems: students might compare beam-support problems with different constraints, bridge-building examples with different load distributions, and collision or momentum problems that require different representations. Rather than presenting only a canonical solved example, instructors could ask students to predict whether an attempted solution will work, show a failed attempt, discuss why it failed, and then contrast it with a successful variant. The educational translation of "variability" is therefore not simply more practice, but structured contrast among problems that differ in relevant causal features while sharing deeper principles. A student could learn a broad strategy set by watching an expert solve many different kinds of problems, possibly almost as well as by solving them personally, as long as the student is cognitively engaged (Atkinson & Renkl, 2007; Booth et al., 2015; Peleg et al., 2026). This could inform blended learning approaches: videos of varied problem-solving demonstrations could supplement hands-on exercises to cover more ground efficiently. In scenarios where hands-on training is costly or risky (e.g., medical or aviation training), our results are encouraging in that observation-based training (e.g., simulation videos or virtual reality observation) can provide substantial learning benefits if it includes high variability of scenarios.

That said, one should be cautious about generalizing to all domains. In tasks requiring physical skill or implicit learning, active practice might prove more important. Additionally, motivation and attentional factors should be considered: in a less controlled environment than a lab setting, observational learners might not pay full attention to demonstrations, whereas active involvement naturally enforces attention. Our study reduced that factor by instructing observational participants to attend to the videos, but in real classrooms or online learning, maintaining engagement for observers is a challenge. Therefore, while one might leverage observational learning, it should be implemented in a way that keeps learners mentally active (e.g., by prompting them with questions during observation, or having them predict outcomes). The implications for school, undergraduate, and professional training populations should also be tested directly: younger learners may depend more on embodied exploration, whereas experienced adults may be better able to extract abstract structure from observation.

This study also has several limitations that suggest avenues for future research. First, our task was a specific physics-based puzzle game with adult participants; the extent to which these findings generalize to other domains (e.g., verbal problem solving, mathematical reasoning, or physical motor skills) is an open question. We chose a within-domain transfer paradigm—new problems from the same puzzle domain—to have a controlled measure of adaptive problem solving. It would be valuable to examine whether high-variability training also helps in farther transfer, such as applying learned principles to qualitatively different tasks. We suspect it might because abstracted principles could, in some cases, transfer out of the original domain, but our study does not directly show far transfer. Second, because we did not measure education level, physics background, engineering experience, or familiarity with similar computer games, we cannot determine whether prior knowledge moderated the effects. These characteristics should be measured in future studies, especially when extending the paradigm to high-school, undergraduate, or professional STEM education. Third, the observational training condition in our design was idealised: participants watched full, clear demonstrations with feedback. We did not compare this to other forms of observational learning (like reading solutions or only seeing end results without the process). It is likely that seeing the process (including failures) was important. If observational learners only saw final solutions, they would likely learn less. Future research could isolate what aspects of observational experience are critical—for instance, do observers need to see mistakes to learn the value of switching strategies, or is seeing just the correct solution each time enough? Observing failures might have taught learners "what not to do" and underscored the need for strategy changes, contributing to their flexibility (Kapur, 2008). Fourth, our ability to detect agency effect might have been limited by the nature of the task. As discussed, the minimal motor demands might have made active and observational learning nearly equivalent. If the task had required fine motor adjustments (e.g., a timing-based game or one that involves muscle memory), the active group may have had an edge. Also, our active vs observational manipulation did not incorporate differences in motivation—all participants knew they would be tested later, so observers had reason to pay attention. In more self-directed settings, active learners might intrinsically be more involved than observers, potentially leading to differences.

5. Conclusions

The current study provides evidence that, when training is intended to support adaptive problem solving, variety is central. Practising a diverse sample of problems enabled learners to solve novel challenges more successfully and efficiently than repeatedly practising a narrow set, and produced a more streamlined search across tools. Nevertheless, every training condition outperformed no training on the principal performance and strategy measures. The expected advantage of learning by doing over learning by watching was largely absent when observers had access to rich, varied, trial-and-error experiences, although active practice improved precision-sensitive spatial implementation. These findings suggest that teachers and trainers seeking transferable skills should prioritise structured variation in the problems and causal principles learners encounter, while preserving active practice where sensorimotor calibration, motivation, or embodied skill is itself a learning objective.

CRediT authorship contribution statement

Hannah Planells: Writing – review & editing, Validation, Project administration, Methodology. **Giulia Serino:** Writing – review & editing, Writing – original draft, Visualization, Validation, Methodology, Formal analysis. **Ori Ossmy:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors declare no competing interests in relation to the manuscript "Which practice makes perfect? The role of variability and agency in learning adaptive problem solving".

Acknowledgements

This work was supported by the ESRC New Investigator grant ES/W009242/1, the BA Talent Award TDA21\210038, the Waterloo Foundation grant 917-4975, and the Birkbeck/Wellcome Trust Institutional Strategic Support Fund to Ori Ossmy.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.actpsy.2026.107546.

Data availability

Data will be made available on request.

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