What Cognitive Training Does and Does Not Improve: The Evidence on Transfer

An evidence synthesis · Holistic Quality LLC

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> **How this was produced.** AI-assisted literature review and drafting, then human-verified: each load-bearing figure and conclusion was checked against a fetched primary or authoritative source (peer-reviewed journals via PubMed/PMC and publisher pages; the FTC's own records; the two 2014 scientific statements), by independent per-claim verifiers that asserted nothing from memory. Any figure that could not be traced to such a source was reworded to what the source supports or removed. Where the evidence is contested, that is stated as contested.

Executive summary

"Cognitive training" — computerized brain-training games and structured cognitive exercises — reliably makes people better at **the specific tasks they practice.** Whether that improvement **transfers** to broader, everyday cognition is a different, and far weaker, claim. The distinction between **near transfer** (gains on the trained task and close variants) and **far transfer** (gains in general intelligence, memory, or real-world functioning) is the entire question, and the evidence divides sharply along it.

The largest single test — an online study of tens of thousands of adults (Owen et al., 2010) — found large improvements on trained tasks and **no transfer** to untrained cognition, even for closely related tasks. The most authoritative consensus review (Simons et al., 2016) found "extensive evidence" for trained-task gains, "less evidence" for closely related tasks, and "little evidence" that training improves everyday cognitive performance. Two meta-analyses of working-memory training (Melby-Lervåg and colleagues, 2013 and 2016) found reliable short-term gains on working-memory measures but **no convincing far transfer** to intelligence, reading, or arithmetic when tested against active controls. In 2016 the U.S.

Federal Trade Commission settled deceptive-advertising charges against the maker of Lumosity for exactly this overreach.

The honest frame is **contested, and asymmetrically so**: near transfer is well supported; far transfer to general cognition is weak, inconsistent, and in the strongest tests absent. Genuinely supportive evidence exists but is **bounded** — the ACTIVE trial shows durable, largely *domain-specific* gains in older adults, not broad transfer, and its downstream dementia-risk claim is statistically fragile and contested. And a measurement caveat sits underneath everything: because simply retaking a cognitive test raises the score, an uncontrolled before/after improvement is not, by itself, evidence of a cognitive gain.

1. The question is transfer, not practice

No one disputes that practice improves performance on the practiced task. If you do the same working-memory exercise for six weeks, you will get better at that exercise. The commercially and scientifically interesting claim is different: that training on a set of games improves **general** cognitive ability — attention, memory, reasoning, "brain fitness" — in a way that shows up in daily life, school, or work. Cognitive scientists call this **transfer**, and they grade it by distance:

- **Near transfer** — improvement on the trained task and closely related variants. Well supported.
- **Far transfer** — improvement on distantly related tasks, general intelligence, or everyday functioning. Weak, inconsistent, and in the strongest studies absent.

Every finding below is a statement about *where on this gradient* the evidence sits. Reading the literature without the near/far distinction is how "you got better at the game" becomes, in marketing, "you got smarter." [2]

2. The large-scale test: Owen et al., 2010

The single largest test of the transfer claim was an online, six-week randomized study run with the BBC (Owen et al., *Nature*, 2010). Of **52,617 participants who registered, 11,430 completed** enough of the protocol — both benchmark assessments and at least two full training sessions over the six weeks — to be analyzed. Participants were randomized to two training regimes (one emphasizing reasoning, planning, and problem-solving; one a broad set of memory, attention, visuospatial, and arithmetic tasks "similar to those commonly found in commercially available brain-training devices") or an active control group that answered general-knowledge questions. [1]

The training worked, in the narrow sense: improvements on the trained tasks were **large** (effect sizes roughly 0.72 to 1.63 — "even the smallest of these improvements would be considered large"). But on the four untrained benchmark tests of general cognition, the study found, in its own abstract's words, **"no evidence… for transfer effects to untrained tasks, even when those tasks were cognitively closely related."** The trained groups

did no better on the benchmarks than the control group that had only answered trivia questions. The paper's own illustration of the magnitude: the memory-training group improved its digit span by about three-hundredths of a digit — on that trajectory "it would take almost four years of training to remember one extra digit." [1]

The load-bearing point: the null is a null for **transfer**, not for learning. People genuinely improved at what they practiced. That improvement did not generalize.

3. The consensus reviews

Simons et al., 2016. The most authoritative assessment is a consensus review commissioned by the Association for Psychological Science (Simons et al., *Psychological Science in the Public Interest*, 2016), which examined the peer-reviewed studies that the leading brain-training companies themselves cite as their best evidence. Its verbatim bottom line: **"extensive evidence that brain-training interventions improve performance on the trained tasks, less evidence that such interventions improve performance on closely related tasks, and little evidence that training enhances performance on distantly related tasks or that training improves everyday cognitive performance."** The review further found that "many of the published intervention studies had major shortcomings in design or analysis" — missing or passive control groups, no correction for expectancy/placebo effects, no preregistration — and that **none** of the cited studies met all of the best-practice criteria the authors identify as essential for a credible efficacy claim. [2]

Melby-Lervåg and colleagues, 2013 and 2016. Two meta-analyses focused on working-memory training — the paradigm most often marketed as a route to general "brain power." The 2013 review (Melby-Lervåg & Hulme, *Developmental Psychology*; 23 studies) concluded that training programs "produce short-term, specific training effects that do not generalize." The larger 2016 review (Melby-Lervåg, Redick & Hulme, *Perspectives on Psychological Science*; 87 publications, 145 comparisons) is more precise: immediately after training there were **reliable but modest** improvements on working-memory measures (verbal working memory $g \approx 0.31$; visuospatial $g \approx 0.28$), but **"for measures of far transfer (nonverbal ability, verbal ability, word decoding, reading comprehension, arithmetic) there was no convincing evidence of any reliable improvements when working memory training was compared with a treated control condition"** (far-transfer effect sizes ≈ 0.05–0.06). Two methodological points carry the conclusion: the null is strongest against **active/treated** controls (apparent far transfer in the literature tracks weak, untreated control designs), and the size of a study's working-memory gain **did not predict** the size of its far-transfer effect — the mechanism the marketing claim depends on is not visible in the data. [3][4]

4. The 2014 dispute — represented from both sides

Cognitive-training efficacy is genuinely contested, and the clearest evidence of that is a pair of dueling 2014 statements.

In October 2014 roughly seventy scientists, organized through the **Stanford Center on Longevity and the Max Planck Institute for Human Development**, issued a statement objecting to commercial brain-training claims: **"We object to the claim that brain games offer consumers a scientifically grounded avenue to reduce or reverse cognitive decline when there is no compelling scientific evidence to date that they do,"** adding that task-specific gains are "small, narrow, and fleeting." [5]

A larger group of scientists and practitioners responded (coordinated at cognitivetrainingdata.org): **"a substantial and growing body of evidence shows that certain cognitive training regimens can significantly improve cognitive function, including in ways that generalize to everyday life,"** and disputed that the first letter represented any "consensus." [6]

Both statements are real and resolve at their primary URLs. The honest reading is not that one side won: it is that the field is contested, and the disagreement centers precisely on **how broadly narrow, task-specific gains generalize.** One caveat worth flagging for a reader weighing the two: the response was coordinated by figures with industry ties (it is associated with Michael Merzenich, co-founder of a brain-training company), whereas the skeptical statement came from academic centers. Neither letter is a scientific finding; together they establish that "scientific consensus supports brain training" is not a defensible claim. [5][6]

5. The regulatory line: FTC v. Lumos Labs (2016)

The dispute has a legal boundary. On January 5, 2016 the U.S. Federal Trade Commission settled deceptive-advertising charges against **Lumos Labs, the maker of Lumosity.** The order imposed a **$50 million judgment, suspended on the company's inability to pay after it paid $2 million** to the Commission — so the widely-cited "$2 million" is the amount actually paid, not the full judgment. [7]

The FTC's stated basis is the canonical map of the forbidden claims. It alleged the company had claimed that training would (1) improve performance in school, at work, in athletics, and on everyday tasks; (2) **delay age-related cognitive decline and protect against mild cognitive impairment, dementia, and Alzheimer's disease**; and (3) reduce cognitive impairment associated with health conditions including stroke, traumatic brain injury, PTSD, ADHD, chemotherapy side effects, and Turner syndrome — "and that scientific studies proved these benefits." In the Bureau of Consumer Protection director's words, the company "simply did not have the science to back up its ads." The order requires **competent and reliable scientific evidence before making future claims** of this kind. [7]

That standard — evidence *before* the claim — is the line any responsible cognitive-training product must stay behind.

6. What genuinely works, with its limits

The skeptical case is not that cognitive training does nothing. It is that the demonstrated benefits are **narrower** than the marketing.

Near transfer is real. Both Simons (2016) and the Melby-Lervåg (2016) meta-analysis agree that practice produces reliable gains on the trained task and on near variants. This is the honest "what works," and it is bounded to the trained and adjacent tasks. [2][4]

The ACTIVE trial — durable but domain-specific. The strongest positive RCT evidence comes from the ACTIVE trial (Advanced Cognitive Training for Independent and Vital Elderly), N = 2,832 older adults randomized to memory, reasoning, or speed-of-processing training or a no-contact control. Each training arm improved **its own targeted ability**, and the gains were durable: at two years all three trained abilities remained above baseline (Ball et al., *JAMA*, 2002), and at ten years the reasoning (effect size 0.23) and speed-of-processing (0.66) gains persisted, though the memory-training gain did not (Rebok et al., *JAGS*, 2014). Crucially, **there was no objective everyday-function benefit at two years**, and the ten-year everyday-function (instrumental activities of daily living) benefit was **self-reported**. ACTIVE is real evidence that specific, structured training produces durable, **trained-ability-specific** improvement in a defined older-adult population — it is *not* evidence of broad far transfer to general intelligence. [8][9]

The dementia-risk claim is contested. A later analysis (Edwards et al., 2017) reported that the speed-of-processing arm was associated with a lower ten-year dementia risk (hazard ratio 0.71). This finding should be labeled contested, not established: it was statistically borderline (95% CI 0.50–0.998, p = .049), the memory and reasoning arms were not significant, dementia was not biomarker-confirmed, dropout was not fully modeled, and the lead author has a **disclosed financial relationship with the training vendor.** It does not license a dementia-prevention claim. [10]

7. The measurement caveat: practice effects

Underneath every efficacy claim sits a measurement problem that also governs how any product may report its own results. **Simply retaking a cognitive test raises the score.** A meta-analysis of retest effects (Hausknecht et al., *Journal of Applied Psychology*, 2007; 107 samples, 134,436 participants) found a mean retest gain of about **d ≈ 0.26** — larger when identical forms are reused and when coaching is added — with no change in underlying ability. [11]

The consequence is methodological and non-negotiable: a before/after score increase, on its own, cannot be attributed to an intervention. It must be netted against a **control group and/or alternate test forms** — which is exactly the design standard the credible meta-analyses require (only treated-control studies count as evidence of specific benefit). [4][11] Any pre/post "you improved X%" claim that lacks a control condition is measuring practice, not cognition.

8. What the evidence supports — and what it does not

Established. Practice reliably improves performance on the **trained task and near variants** (near transfer). Specific, structured training can produce **durable,**

domain-specific gains in defined populations (the ACTIVE reasoning and speed arms in older adults).

Not established. That cognitive-training games improve **general intelligence, everyday cognition, or real-world performance** (far transfer). The largest test found none; the consensus review found "little evidence"; the working-memory meta-analyses found far-transfer effects near zero against active controls.

Contested / not established. That cognitive training **prevents or delays age-related decline, dementia, or Alzheimer's**, or reduces impairment from named health conditions. This is the exact class the FTC found Lumosity could not substantiate; the one supportive dementia-risk signal (Edwards 2017) is statistically fragile and conflicted.

The defensible synthesis: cognitive training **works for what you train, in the population and on the ability trained, and does not reliably generalize beyond it.** Overstating the general-cognition claim does not strengthen the honest one — it is precisely the overreach that drew regulatory action.

9. What this evidence licenses a product to claim

Read as an operating boundary for any cognitive-training product, the record supports a clean allow/deny line:

Defensible. That practicing the product's exercises improves performance on **those exercises and closely related tasks**; that specific structured protocols have produced **durable, domain-specific** gains in **defined populations** (stated with the population and the specific ability, not generalized); and — with disclosure — that whether such training transfers to broad everyday cognition is **scientifically contested**, citing this synthesis.

Not defensible without competent, reliable scientific evidence specific to the product. Any claim that the product improves **general intelligence, IQ, memory, "brain fitness," or everyday/real-world cognition**; any claim that it **delays or prevents cognitive decline, dementia, or Alzheimer's**, or reduces impairment from a health condition; any assertion that "**studies prove**" such benefits; any appeal to a "**scientific consensus**" in support; and any **uncontrolled before/after** result presented as a cognitive gain.

Key gaps

- **Individual differences.** Most transfer studies report averages; whether specific subgroups benefit more is under-characterized and often confounded with baseline ability and expectancy.
- **Dose and design.** Which training schedules, intensities, and adaptivity (if any) produce even near-transfer durably is not settled.

- **Everyday-function measurement.** The everyday-function signals that exist (ACTIVE ten-year) are largely self-reported; objective, real-world outcome measurement is the central missing piece.
- **Active-control discipline.** Much of the apparent far-transfer literature rests on weak (untreated/passive) controls; the field's own strongest reviews attribute the positive signal to that design flaw.

Priority research directions

- **Active-controlled, preregistered** transfer studies with objective everyday-function outcomes, powered for far transfer rather than near.
- **Population-specific** trials (as ACTIVE was) that state the trained ability and the population rather than generalizing.
- **Observed-outcome** (not modeled-risk) tests of the decline/dementia claims, with vendor-independent analysis and biomarker confirmation.
- **Measurement-first** reporting standards: alternate forms and control conditions as a precondition for any efficacy statement.

How to cite

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A summary of this work is published at holisticquality.io/research/cognitive-training-efficacy.

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Disclosures

Competing interests. The author is the founder and principal of Holistic Quality LLC, the commercial publisher of this report, which develops regulator-facing safety-data and compliance products; a sibling property (the Institute for Cognitive Sovereignty) and a planned cognitive-training surface may cite this work. Because this synthesis defines the evidentiary boundary for a product line the author has an interest in, the competing interest is direct and material. Mitigation: every claim and citation was independently source-verified fetch-no-memory by verifiers blind to any product framing; the limits of the evidence are stated throughout; the genuinely-supportive evidence is represented with its boundary conditions rather than dismissed, and the unsupported far-transfer claims are represented as unsupported rather than softened; and the author retained sole editorial control.

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