

Learning Styles in Architecture and Interior Design Education: A Critical Review of Instruments, Studio Assessment and the Matching Claim

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DOI: <https://doi.org/10.47772/IJRISS.2026.1026EDU0609>

Received: 17 September 2026; Accepted: 23 September 2026; Published: 05 October 2026

ABSTRACT

Learning style inventories have been administered to architecture and interior design students since 2000, and their classifications are widely cited as grounds for tailoring studio instruction. This critical review asks what the instruments used in this literature were designed to do, whether the studies' claims follow from what they measured, and what survives once the prescriptive claim, sometimes called the matching hypothesis, is set aside. It also traces how the discipline received the psychometric critique of learning styles. A Scopus search, supplemented by semantic retrieval and backward citation searching, identified twenty-six empirical studies published between 2000 and 2026 for close analysis, each read in full and charted on instrument, sample, outcome, reliability, accreditation context and the relation between claims and results. The studies use nine instruments, none of which are validated against one another in this population, and several share samples. In-sample reliability of the predictor is reported in six studies, with values as low as 0.27; reliability of the outcome is reported in four, never for a studio grade. Findings do not converge. A frequently cited study reported group means running in opposite directions across two programmes at one university, and its chi-square statistics do not reach significance. None of the twenty-six studies allocated students to matched or mismatched instruction, so the prescriptive claim behind the field's recommendations remains untested in this corpus. The critique entered this literature in 2005, and eleven studies engaged with it, only one of which modified its instrument in response, before a 2026 study treated the question as open and accepted a null result. Descriptive use of the inventories remains defensible; prescriptive use does not. The review sets out four requirements for a study that could settle the question, with implications for studio teaching, assessment and accreditation.

Keywords: architectural education; design studio; interior design education; learning styles; studio assessment

INTRODUCTION

The design studio separates architectural and interior design education from most other undergraduate programmes. Over extended periods, students engage with open-ended design problems, present their work for individual critique before their peers, and are evaluated on the quality of their design output rather than through formal examinations. Performance within a single studio varies more widely than the shared brief, site, and tutor would seem to predict. Why some students flourish under these conditions while others do not is an old question in design pedagogy, and for the past quarter-century, one answer has dominated the empirical response.

That answer is the learning style. From Nussbaumer and Guerin (2000) onward, and in the template set by Demirbaş and Demirkan (2003) and Kvan and Jia (2005) in Design Studies, studies have administered an inventory, reported how the cohort distributes across its categories, tested the distribution against a performance measure, and recommended that teaching accommodate the categories found. The twenty-six studies reviewed here extend that template to cognitive-style instruments and to adjacent constructs: learning approaches, learner autonomy, and learning processes.

Educational research outside design moved in the opposite direction across the same period. Pashler et al. (2008) named the claim that carries the pedagogical weight: the meshing hypothesis, according to which instruction works best when its format matches the learner's stated preference. They also specified the experimental design that would validate it, set out below. Almost no published study met that design. Of the few that did, several produced results running against the hypothesis (Constantinidou & Baker, 2002, and Massa & Mayer, 2006, as cited in Pashler et al., 2008, p. 117). Coffield et al. (2004a, pp. 8–9, 138–139) had already documented, across seventy-one style models and thirteen examined in detail, that the psychometric properties of the major inventories were weak enough to make individual classification unsafe. Four of the nine instruments used in architecture and interior design are among those thirteen.

It would be easy to assume that design education has never encountered this argument. It did, and tracing what the discipline did with it is part of the work of this review. The psychometric objections entered the field's own reference lists in 2005, when Kvan and Jia reported Newstead's verdict that the reliability of the Kolb inventory was poor to moderate and its validity unproven. Over the twenty-one years since, twelve studies have engaged the critique, in ways that ranged from answering it, to acknowledging it and retaining the instrument, to modifying the instrument in response, and one further researcher reached the same scepticism from her own data. Only in 2026 did a study in this discipline treat the question as open, test whether style classifications related to an outcome, and accept a null result. Doubt arrived before evidence, and it arrived late.

That sequence is the finding, but it is not the whole. What none of the twenty-six studies examined here has done is allocate students to instruction matched or mismatched to their measured style. The prescriptive claim on which the field's recommendations rest has not been tested in any of them. Five further studies identified by the search could not be obtained in full, and although their abstracts describe no such allocation, this remains a finding about the corpus rather than a certainty about the field. Within that corpus, the discipline has moved from assuming the claim to doubting it without having examined it, which leaves the question open in the least useful way.

The general case against learning styles has been made outside design (Coffield et al., 2004a; Kirschner, 2017; Kirschner & van Merriënboer, 2013; Newton & Miah, 2017; Pashler et al., 2008), and this review does not restate it. Two earlier reviews have covered this literature. Kaba and Abdou (2022), reviewing sixteen Scopus-indexed studies that used the Kolb or Felder-Soloman instruments, concluded that identifying students' learning styles leads to better planning of teaching methods (p. 97). Mirmoradi (2023) systematically reviewed twelve studies relating learning styles to architectural design, drawn from Web of Science, Scopus, and Google Scholar; she rejected tailoring a curriculum to one dominant style but recommended identifying students' styles at the start of a programme to choose its educational approach (p. 83). Her review assessed the reliability of neither instruments nor outcomes, and its reference list cites none of the psychometric critique. Its contribution is specific to architecture and interior design education and has two parts: an audit of the discipline's own evidence, examining what each study measured, the reliability of the predictor and outcome, and whether its claims follow from its results; and an account of reception, tracing how the psychometric critique entered this literature's reference lists and what the citing studies did with it. A third review, by Kumar, Dhar and Kalita (2025), is known only from its abstract, which describes a concern with learning styles and the selection of design tools rather than with the evidence base examined here.

This review asks three things of those twenty-six studies. What were the instruments designed to do, what did their developers claim for them, and how have those claims fared under independent evaluation? What did the studies actually measure, as distinct from what has subsequently been claimed on their behalf? And what, once the prescriptive claim is set aside, remains defensible about attending to how individual students engage design problems?

METHODOLOGY

This is a critical review rather than a systematic or scoping review. Its aim is not to estimate an effect or map the extent of a field, but to evaluate whether the claims made in this literature follow from what its studies measured. That aim requires close reading of a defined set of studies rather than exhaustive coverage, and the selection process is outlined below so that readers can assess it.

The search ran in Scopus, using the string TITLE-ABS-KEY ((“learning style*” OR “cognitive style*” OR “learning approach*” OR “learning preference*” OR “learning process*”) AND (architect* OR “interior design” OR “design studio”)), limited to English. The run on 15 August 2026 returned 26,340 records. Most were irrelevant. The truncation architect* also retrieves “architecture” in its computing sense: 13,670 of the exported records (53.4%) carry machine-learning terms such as machine learning, deep learning, neural network, or transformer in their titles, and these were removed before screening.

A supplementary search on the same date, using Scopus’s semantic retrieval, returned a relevance-ranked set of the top 100 results. The remaining Boolean records were filtered for a style-construct term together with a term for architecture or interior design education in the title, abstract or keywords, combined with the semantic results, and screened on title and abstract, with candidate studies read in full. Twenty-six studies meeting the criteria below were selected for close analysis. The corpus is not claimed to be exhaustive. Its purpose is to allow the claims made in this literature to be tested against what its studies measured, and the review’s findings are stated for these studies rather than for the field as a whole. The Boolean search recovered all but one of them; Demirkan and Demirbaş (2008), whose abstract uses none of the discipline terms, was identified through the semantic search and independently through backward citation searching of the included studies.

Studies were included where they:

- reported primary empirical data;
- were published from 2000 onwards;
- sampled students enrolled in architecture, interior architecture or interior design programmes;
- administered a named learning style, cognitive style, learning approach or learning process instrument;
- appeared in a peer-reviewed journal or conference proceedings;
- were published in English, or reported results in English-language abstracts and tables.

The inclusion boundary is a sampling decision, not a claim that the constructs are equivalent. Instruments measuring learning style, cognitive style, thinking style, learning approach, learning process, and learner autonomy were all retrieved by the terms under which this literature is indexed. Within the corpus, studies of cognitive style and of learning style also cite one another as prior work on the same question. Whether that grouping holds is one of the things this review examines. Restricting the corpus to learning-style inventories in the narrow sense would have removed the studies against which the grouping can be tested, and would have built the review’s conclusion into its sample.

Three categories of exclusion should be stated. Studies whose sampled population was drawn from engineering, construction management, landscape architecture, naval architecture, or other design disciplines were excluded where architecture or interior design students were not reported separately. That removed two landscape architecture studies, one naval architecture study, a study of industrial design students (Yuan, Song, & He, 2018), and one otherwise relevant investigation of style-matched augmented reality instruction in structural engineering (Liu et al., 2025). Iyer and Roberts (2024) was excluded for administering no instrument: it is a phenomenographic analysis of first-year architecture students’ approaches to learning, and its method sits outside a criterion built around psychometric measurement.

Five identified studies could not be obtained in full within the review period: Ayalp and Erman (2019), Hosny et al. (2024), Izadpanah (2025), Kim and Choi (2026), and Kumar, Dhar and Kalita (2025). Because charting required full texts, the four empirical studies were not added to the corpus, and they are characterised here only from their abstracts. Ayalp and Erman related Kolb classifications of 55 architecture students to performance in construction management courses; Hosny et al. related Kolb classifications of 130 architecture students to preferences for biophilic design patterns; Izadpanah profiled interior architecture students with the Gregorc instrument; and Kim and Choi related cognitive style and perceived difficulty to improvement in sketching after

a workshop delivered to all 53 participants. None of the four abstracts describes allocating students to instruction by style. The fifth is a review, discussed in the introduction. A later check against the systematic review by Mirmoradi (2023) identified seven further empirical studies of learning style among architecture and interior design students. None was retrieved by the Scopus search, and they lie outside the corpus as defined here. The same check led to a full-text reading of two studies that the Boolean search had retrieved, Wang, Newton and Lowe (2015) and Hossain Maghool, Moeini and Arefazar (2018), both of which met the criteria and were added.

Each included study was read in full and charted against six headings: the instrument and its version; the sample composition, year of study and institution; the outcome measure and the procedure that produced it; reliability reported for the predictor, for the outcome, or for neither; the accreditation regime governing the programme; and the relation between the result obtained and the claim advanced in the abstract and discussion. Reference lists were checked separately for the presence of the critique literature. That last check is not conventional in a review of this kind. It is included because the argument developed here concerns not what this field found but what it did with what others found.

FINDINGS AND DISCUSSION

The Instruments and What They Were Built to Do

Kolb (1984) derived the Learning Style Inventory from experiential learning theory, which represents learning as a cycle of four modes; the instrument locates a respondent on two bipolar dimensions, and the coordinates fall into one of four quadrants labelled converging, diverging, assimilating and accommodating. De Ciantis and Kirton (1996, as cited in Coffield et al., 2004a, pp. 64–65) distinguish three things the inventory conflates: the process, meaning the four stages; the level, meaning how competently a learner performs at each; and the style, meaning how each is approached. Studies in architecture and interior design report the style almost exclusively and treat the quadrant as a property of the student, although Kolb himself warned against learning styles hardening into fixed traits used to pigeonhole individuals (as cited in Coffield et al., 2004a, pp. 62–63). Coffield et al. record a long and public dispute over the instrument's reliability and note that its construct validity has been challenged without resolution (pp. 62–65).

The Index of Learning Styles has a different history and more explicit warnings attached to it. Felder and Silverman (1988) proposed the model for engineering education, and Soloman and Felder built the 44-item instrument from it (Felder & Spurlin, 2005), assessing four dimensions: active and reflective, sensing and intuitive, visual and verbal, sequential and global. Felder and Spurlin (2005) set out qualifications intended to guard against misuse, and they read as a description of what this literature subsequently did: the dimensions are continua rather than categories, profiles indicate tendencies rather than predict behaviour, and preferences shift with educational experience. The plainest states that “the point of identifying learning styles is not to label individual students and modify instruction to fit their labels” (Felder & Spurlin, 2005, p. 105), and Felder and Spurlin add that their validity evidence assumes the instrument is used consistently with those observations.

Gregorc's Style Delineator, used by Watson and Thompson (2001) and by Albadi and Zollinger (2021), asks respondents to rank ten sets of four words, yielding four categories: concrete sequential, concrete random, abstract sequential and abstract random. Its scoring, like Kolb's, is ipsative, so the four scores cannot vary independently. Gregorc presented the styles as natural abilities not amenable to change and reported high reliability (as cited in Coffield et al., 2004a, pp. 18–19). Watson and Thompson restate that claim without citing figures. Coffield et al. concluded that the instrument's internal consistency and factorial validity raise serious doubts about its psychometric properties and that its use could not be recommended (pp. 14–17). The architecture literature has followed the first assessment.

Riding's Cognitive Styles Analysis, used by Roberts (2006) and Pektaş (2007, 2010), is the only instrument in this corpus not based on self-report. It presents verbal and visual stimuli by computer and derives wholist-analytic and verbaliser-imager ratios from response latencies, within a tradition that treats cognitive style as relatively permanent, largely innate, and independent of intelligence, a framing Pektaş (2007) adopts directly (Riding & Cheema, 1991, as cited in Pektaş, 2007). Roberts chose it because many cognitive style tests lacked validity and reliability, because self-report items were remote from design project work, and because little

evidence suggested it was less reliable than the alternatives. It was the best available, not an instrument whose properties had been established. It is among Coffield's thirteen and met none of their four criteria (Coffield et al., 2004a, pp. 138–139).

Sternberg and Wagner's Thinking Styles Inventory, used by Erkan Yazici (2013) in its Turkish adaptation on its global and local subscales, derives from the theory of mental self-government. Sternberg described styles as varying across tasks, situations, and stages of life (as cited in Coffield et al., 2004a, pp. 112–113), and Coffield et al. found that the model met none of their four criteria (pp. 138–139). The remaining four instruments each appear once: the revised two-factor Study Process Questionnaire (Biggs, Kember, & Leung, 2001), Schmeck, Ribich and Ramanaiah's Learning Processes Inventory, Macaskill and Taylor's Autonomous Learning Scale, and the Object-Spatial Imagery and Verbal Questionnaire, which measure approaches, processes, autonomy, and imagery rather than learning style as such.

The psychometric record is the second constraint, and the architecture literature's own figures are the relevant evidence. Litzinger, Lee, Wise and Felder (2007), validating the Index of Learning Styles, found internal consistency from 0.55 to 0.77 across its four scales. Demirbaş and Demirkan reported Kolb alphas of 0.57 to 0.73 in 2003 and 0.51 to 0.73 in 2007, and Demirkan and Demirbaş reported 0.54 to 0.69 in 2008. The 2007 paper attributed its weakest values to the few accommodating and diverging learners in its sample (p. 353). The lower values meet the 0.50 minimum that Litzinger et al. cite for preference measures but fall below the 0.70 that Coffield et al. (2004a, pp. 28–29) treat as the usual standard for a subscale, the more relevant benchmark where scores classify individual students. Demirkan (2016), using the Index on a later Bilkent cohort, reported 0.47, 0.66, 0.52 and 0.27; for an eleven-item scale, 0.27 implies a mean inter-item correlation of about .03. Başçı and Türkmen report no in-sample reliability for either instrument, citing only a Turkish adaptation of the creativity test, whose own Cronbach's alpha for university students ranged from .56 to .84 (Aslan, 2001, p. 29), and twenty of the twenty-six studies report none at all.

Two claims have to be separated before any of this evidence can be assessed. The descriptive claim holds that students differ in how they report preferring to engage with material. That is close to uncontroversial, and the instruments, whatever their reliability, are attempts to capture it. The prescriptive claim holds that matching instruction to a measured preference produces better learning than mismatching it. Pashler et al. (2008) called this the meshing hypothesis and specified what would establish it: students grouped by style, randomly allocated across instructional methods, assessed on a common measure, producing a crossover interaction in which each group does best under its matched method. The two claims are independent, and the first can be true while the second is false, as the evidence outside design indicates. What follows asks which of them the studies in this field actually tested.

What the Twenty-six Studies Measured

Distribution, Association, and the Two Cases That Carry the Argument

A walk through each study would be a catalogue rather than an argument, and Table 1 carries the enumeration so that this section can do something else. The studies are sorted by what sits on the right-hand side of the equation. The sorting is the finding: five report a distribution and stop; seven test a classification against academic performance; seven test it against a measured task or skill; three substitute a second construct for any performance outcome; one manipulates instruction; and three examine change over time. A reader meeting these papers under the heading of learning styles would not know which kind they were reading, because the heading covers all of them.

The distribution studies are the simplest to characterise and the most frequently over-read. Watson and Thompson (2001), Demirkan and Demirbaş (2008), Hossain Maghool, Moeini, and Arefazar (2018), Labib et al. (2019), and Albadi and Zollinger (2021) profiled cohorts using the Gregorc, Kolb, and Felder-Soloman instruments (Table 1). These are legitimate descriptive exercises, but none collected an outcome, and none can support a claim about consequences. Hossain Maghool et al. nonetheless drew one, concluding from an Index of Learning Styles distribution of 60 students that about half of architecture students are not taught in the way they learn best (p. 267).

Of the seven performance studies, three come from the Bilkent department, and one is Tucker's (2009) at Deakin; the others are Casakin and Gigi (2016), Sen et al. (2021) and Kvan and Jia (2005) (Table 1). Tucker's (2009) reported advantage for Southerner learners rested on a one-tailed test, $t(106) = 1.827$, $p = .0355$, with a two-tailed probability of .071. Although Tucker stated the direction in advance, the same paper set a Bonferroni-adjusted alpha of .003 for its other comparisons, and this result does not meet it. Kvan and Jia's study, with 120 Scopus citations as of 17 September 2026, is the most cited performance study outside Bilkent and the third most cited in the corpus, after Demirbaş and Demirkan (2007; 157) and Demirbaş and Demirkan (2003; 147), and it requires examination in detail.

Table 1. Characteristics of the Twenty-six Included Studies

Study	Category	n	Instrument	Outcome measure	Reliability reported
Nussbaumer & Guerin (2000)	Task	578	Kolb LSI 2 (1985)	Isham visualisation skills test	Not reported
Watson & Thompson (2001)	Distribution	147	Gregorc GSD	None (distribution only)	Asserted high; no figures given
Demirbaş & Demirkan (2003)	Task	88	Kolb LSI 3 (method); LSI 2 in lit. review	Four-stage staircase exercise, two assessors	α 0.57–0.73; inter-rater r 0.87–0.98
Kvan & Jia (2005)	Performance	91 / 75	Kolb K-LSI (II), 1985	Studio jury grade; A and F grades capped at 10% each	Not reported
Roberts (2006)	Change	190 / 120	Riding CSA	Rank on design project work, years 1–3	Not reported
Demirbaş & Demirkan (2007)	Performance	273	Kolb LSI 3 (1999)	Course scores and GPA	α 0.51–0.73 across three groups
Pektaş (2007)	Task	46	Riding CSA	CAAD drafting and creativity scores, 3 expert judges (CAT)	Inter-rater r 0.66–0.81 (same data as Pektaş, 2010)
Tucker (2007)	Change	152	Kolb LSI 2 (1985)	None (year-group comparison)	Not reported
Demirkan & Demirbaş (2008)	Distribution	286	Kolb LSI 3 (1999), Turkish	None (PCA of distribution)	α 0.54–0.69
Tucker (2009)	Performance	152 / 108	Kolb LSI 2 (1985)	Design assignment and portfolio marks	Kolb (1984) manual figures, not in-sample
Demirkan & Demirbaş (2010)	Performance	100	Felder-Soloman ILS	CGPA, dichotomised at 2.00	Not reported

Pektaş (2010)	Task	46	Riding CSA	2D drafting and creativity scores, 3 expert judges (CAT)	Inter-rater r 0.66–0.81 across three stages
Carmel-Gilfilen (2012)	Second construct	139	Kolb LSI 3.1 with MID and MOD	Perry intellectual development position	κ 0.90 for protocol coding only
Erkan Yazici (2013)	Task	20	Sternberg TSI (Turkish adaptation)	Design solution process, protocol analysis of video	Not reported
Wang, Newton & Lowe (2015)	Change	245 (seven groups)	Kolb LSI, four scrambled forms	None (LSI repeated three times in one semester; VR task)	Not reported
Casakin & Gigi (2016)	Performance	100	VSCS with OSIVQ	Course score average, first-year students	R 0.38 between the two measures
Demirkan (2016)	Performance	218	Felder-Soloman ILS	Studio grade, institutional letter grades	α 0.47 / 0.66 / 0.52 / 0.27
Hossain Maghool et al. (2018)	Distribution	82 / 60	Felder-Soloman ILS	None (distribution only)	Not reported
Labib et al. (2019)	Distribution	92 (women)	Felder-Soloman ILS	None (distribution only)	Not reported
Albadi & Zollinger (2021)	Distribution	603 / 466	Gregorc GSD	None (distribution only)	Not reported
Sen et al. (2021)	Performance	349	R-SPQ-2F	Academic performance	α 0.781 / 0.70 (predictor)
Al Maani (2022)	Second construct	127	Felder-Soloman ILS with ALS	Learner autonomy	Not reported
Al-Shdaifat et al. (2023)	Intervention	26 (13 + 13)	Kolb LSI 3.1	Design performance scores (intervention)	Not reported
Taneja et al. (2023)	Task	33 (women)	Kolb LSI 3	Baqer Mehdi Nonverbal Test of Creative Thinking	Not reported
Ketizmen (2024)	Second construct	150	Learning Processes Inventory with BFI	None (personality and motivation)	α .869 LPI / .779 JPI / .730 BFI

Başçı Türkmen (2026)	&	Task	50	Kolb KLSI 3.1	Torrance Tests of Creative Thinking	None in-sample; TTCT Turkish adaptation cited
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Note: Category = primary category by design (Distribution = no outcome; Performance = academic performance; Task = scored task or test; Second construct; Intervention; Change = change over time). LSI = Learning Style Inventory; GSD = Gregorc Style Delineator; CSA = Cognitive Styles Analysis; ILS = Index of Learning Styles; R-SPQ-2F = revised two-factor Study Process Questionnaire; ALS = Autonomous Learning Scale; VSCS = computerised visual-spatial cognitive style test; OSIVQ = Object-Spatial Imagery and Verbal Questionnaire; TSI = Sternberg and Wagner Thinking Styles Inventory; MID = Measure of Intellectual Development; MOD = Measure of Designing; BFI = Big Five Inventory. Kolb inventory versions are given where the study states them; LSI 2 dates from 1985, LSI 3 from 1999 and LSI 3.1 from 2005. Where two figures are given for n, the first is the number recruited or returned and the second the number entering the reported analysis. All reliability figures are as reported by the studies themselves.

Kvan and Jia's abstract reports a statistically significant correlation between learning styles and academic performance. The supporting statistics, 19.929 on 18 degrees of freedom in Programme 1 and 24.715 on 21 in Programme 2, are printed with the symbol "a" (p. 27). Read as Pearson chi-square values, neither reaches significance at $\alpha = .05$ (critical values 28.869 and 32.671; upper-tail probabilities .337 and .260), and the paper states no significance level. The test was also unsuited to the data: 32 students across 28 cells and 43 across 32 give mean expected frequencies of 1.14 and 1.34, against a conventional minimum of 5. The abstract's second claim, that convergers achieved significantly lower marks in one studio while assimilators succeeded in the other, is also unsupported. The authors note that no t-test was possible for the single converger in Programme 1, and in Programme 2, the reported means place assimilators lowest of the four groups.

Roberts (2006) is the methodological counter-case. He administered Riding's Cognitive Styles Analysis once, to 190 students at the Welsh School of Architecture, and followed two cohorts of 120 students across three years by their rank on assessed design project work. Against Riding's norms for school pupils, 57.9% were classed as analytic, where a third would be expected. In both cohorts, the mean rank of analytic students fell significantly between the first and third years; in the first cohort only, the style groups also differed significantly and over half of the wholists left. Roberts attributed the early analytic advantage partly to smaller, more sequential first-year projects and concluded that by graduation cognitive style may have little effect on performance in design project work (p. 179). His is the only study in the corpus to follow the same individual students' performance across years, to document attrition by style and to show an intake skewed before teaching begins. Because it measured style once, it cannot show whether style changes, but it shows that differences between year groups may reflect who arrived and who left.

Measured Outcomes, Second Constructs, Intervention, and the Count

Seven studies score a task instead of a studio grade: a visualisation test (Nussbaumer & Guerin, 2000), a staircase exercise (Demirbaş & Demirkan, 2003), drafting and creativity scores on which Imagers outperformed Verbalisers (Pektaş, 2007, 2010), a design solution process (Erkan Yazici, 2013) and published creativity tests (Taneja et al., 2023; Başçı & Türkmen, 2026). Whether these scores were themselves reliable is taken up below.

Casakin and Gigi (2016) are the only study to address selection rather than instruction, relating a computerised visual-spatial measure taken at admission to first-year course averages.

Three studies replace the performance outcome with a second construct, and in each the second construct overlaps with or displaces style. Carmel-Gilfilen (2012) found Kolb classifications unrelated to Perry positions ($p = .772$ and $p = .8053$); Ketizmen (2024) found learning processes associated with Big Five personality traits; and Al Maani (2022), pairing the Index of Learning Styles with an autonomy scale, concluded that searching for style differences in the design studio is redundant.

One study manipulates anything. Al-Shdaifat et al. (2023) gave two non-randomly assigned groups of thirteen first-year students either video or face-to-face feedback. The medium was manipulated, not its fit to any

classification, and neither group contained a diverging student. The authors report that video feedback improved performance regardless of learning style.

Three studies examine change over time, and none analysed style change within individual students. Tucker (2007) compared first- and third-year students at Deakin in one year, which is a cross-section rather than a follow-up. Roberts (2006), described above, followed the same students' performance but measured their style once, at entry. Wang, Newton and Lowe (2015) administered the Kolb inventory three times across one semester to a first-year architecture cohort whose exposure to virtual reality was increasing, but participation was voluntary at each point, and the results were reported only as group balance points and pooled individual plots.

Seven of the twenty-six studies come from one Bilkent department and two more are Tucker's. Several are not independent samples: Demirbaş and Demirkan (2007) and Demirkan and Demirbaş (2008) both report freshman cohorts of 111 and 88 students, and the 2003 study's 88 freshmen carry the same reliability coefficients as the 2007 paper's second cohort; Pektaş's two papers report the same 46 sophomores with identical inter-rater tables; and Tucker's two papers come from one research project. The number of independent samples is therefore smaller than twenty-six. None of the studies reports reliability for a studio grade, and none has allocated students to instruction matched or mismatched to their measured style.

Nine Instruments, One Literature

No two of the nine instruments measure the same thing, and their developers did not present them as variants of one another. Within this corpus, two of the traditions are nonetheless treated as one. Roberts (2006), measuring cognitive style, cites the Kolb studies of Demirbaş and Demirkan (2003) and Kvan and Jia (2005) as prior work, and Tucker (2009), measuring learning style, reviews Roberts's findings as evidence on style and performance before turning to the matching hypothesis for cognitive style. A wholist-analytic ratio and a converging quadrant are thereby treated as comparable measures of one quantity. The approach, process and autonomy studies do not enter this exchange of citations, but they are indexed, retrieved and reviewed under the same heading. The consequence is that findings here cannot accumulate: what looks like a converging body of evidence is a set of results about different variables.

No study in twenty-six validates any of its measures against another. Casakin and Gigi come closest, and they report that the validity of their computerised test was established in a separate study on a different sample. No study has asked whether a student classified as converging by Kolb is classified as analytic by Riding, or as sequential by Felder-Soloman, and the field has therefore never established that its nine instruments are measuring anything in common.

The count of nine also understates the variation, because one of the nine is not a single instrument. Kolb's inventory has been revised repeatedly since its first issue, and the twelve studies that use it span at least three versions, from 1985, 1999 and 2005, with Demirbaş and Demirkan (2003) naming two in the same paper and Wang, Newton and Lowe (2015) administering four locally scrambled forms; the literature compares their results without remark. At Bilkent, the switch from Kolb's inventory to the Index of Learning Styles, which Demirkan (2016) acknowledges while drawing her findings into parallel with Demirkan and Demirbaş (2008), took internal consistency from 0.54–0.69 to a low of 0.27.

The distinction that matters most is between style and approach. Biggs, Kember and Leung are explicit that a trait framework would be inappropriate for what their instrument measures, because strategy use depends on a student's motives, on how they read the demands of the task, on the teaching and assessment methods in front of them, and on the climate of the classroom. A deep approach is something a student adopts and can stop adopting when the assessment rewards something else. A learning style, in the Kolb and Felder traditions, is presented as a preference stable enough to justify designing instruction around it. The pedagogical implications diverge accordingly. If approach is what matters, the lever is assessment design. If style is what matters, the lever is differentiated instruction. The field advances both as though they were one recommendation.

Sen et al. (2021) is the only study in that line within the corpus. Their finding that a deep approach predicted performance positively is a finding about approach, produced by an approach instrument with reported reliability

of 0.781 and 0.70, in a two-institution sample. Their abstract adds that surface approaches influenced performance negatively, but the regression they report does not support it ($\beta = -0.134$, $p = .773$). It is among the strongest pieces of evidence in this corpus, and it supports no claim about learning styles. It supports a claim about how architecture students engage their coursework, and about how little of their attainment that engagement explains: 1.5 percentage points of variance beyond demographics.

Two further separations follow the same pattern. Ketizmen's learning-process scores were associated with personality: elaborative processing with conscientiousness and enjoyment, synthesis and analysis with extraversion ($r = .876$, high enough to raise the question of overlapping item content). Personality has an established measurement literature and reasonable stability over time, which is what style inventories claim and do not demonstrate; a learning-process score that correlates with conscientiousness may be capturing conscientiousness. Carmel-Gilfilen's finding that Kolb classifications were independent of intellectual development, reported above, points the same way: whatever develops in design students across a programme, the style inventory is not measuring it.

The third separation concerns change over time. Claims that styles shift across a design programme are common, and almost all of them rest on comparing different students at different stages: Tucker's year-group comparisons, the Bilkent cohort series, and Al Maani's five-year cross-section. Cross-sectional comparison cannot distinguish change within individuals from differences between intakes or from selective attrition. Roberts's longitudinal study, described above, shows that both confounds are present in this setting: an intake that differs from a general population norm and, in one of his two cohorts, disproportionate attrition by style. Tucker (2009) acknowledged that his own year-group differences might reflect attrition rather than change, but Al Maani (2022), working later from a cross-section, interpreted differences between year groups as development. Wang, Newton and Lowe (2015) came closest, measuring one cohort's style three times in a semester, but no study in the corpus analysed style change within the same individual students.

Taken together, these separations narrow what the corpus can show: approach effects that are real but small, process scores that point toward personality, one study finding style unrelated to intellectual development, and one longitudinal study that finds selection and attrition where others infer learning.

The Outcome Variable and Its Context

Every association reported above has two halves. The literature has scrutinised one of them at length, debating which inventory to use, which version, how to translate it and how to interpret the scores. The other half has received far less attention. In most of these studies, the dependent measure is a studio grade, taken from institutional records and entered into the analysis as though it were a reading from an instrument. It is not. It is the output of a social process whose properties the field has largely not examined.

The studies that describe their assessment procedures show what that process involves. Kvan and Jia report marks set at a public jury against four weighted criteria, divided 40% to the instructor and 60% to three external critics, under an institutional rule capping A and F grades at 10% each, and attending to final products rather than process. Tucker (2009) reports marking by three teachers with moderation and describes it as subjective; Roberts (2006) and Demirkan (2016) describe panel moderation and institutional letter grades. These are ordinary and defensible ways to assess design work, but none yield a measurement with known error properties.

Reliability is reported in this corpus, but never for the grades, and only where a study built or adopted the assessment for research. Pektaş's three expert judges, using Amabile's Consensual Assessment Technique (Amabile, 1996, as cited in Pektaş, 2010), reached inter-rater reliabilities of 0.66 to 0.81, reported identically in 2007 and 2010, although both papers describe every value as above 0.70; Demirbaş and Demirkan's (2003) two instructors correlated at 0.87 to 0.98; and Carmel-Gilfilen's raters reached a kappa of 0.90 in coding intellectual development protocols, though not in marking design. Building an assessment did not guarantee checking it: Nussbaumer and Guerin, Erkan Yazici, Taneja et al. and Başçı and Türkmen report no in-sample outcome reliability. Two datasets, reported in three papers, show that a design outcome can be measured with reported agreement, and the field has not followed. The more a study relies on routine studio assessment, the less is known about the reliability of what it measures.

This bears directly on how the association findings should be read. Random measurement error in either variable attenuates an observed association towards zero, so unreliable predictors and outcomes make real relationships harder to detect. Demirkan (2016) reports a significant difference in studio grades across the sequential and global scales, with an internal consistency of 0.27. The outcome has no reported reliability and is produced by multiple assessors whose agreement was not measured. In Kvan and Jia's case, it is also capped by limits on the proportion of A and F grades. Two features prevent these results from being read simply as underestimates. Error in studio grading need not be random: if assessors reward qualities of presentation that covary with a style classification, an observed association can be inflated rather than attenuated. And capping the grade distribution restricts its range, with effects that depend on where the compressed cases fall. The coefficients are therefore weakly informative about any underlying relationship, in either direction.

Anthony (1987) identified the vagueness of jury criticism and the absence of criteria agreed in advance as central problems in design assessment. If the criteria that produce a grade are not settled before the work is judged, correlating that grade with anything assumes a stability the assessment does not have. Roberts (2006) is the one study in the corpus to respond. Citing doubts about the reliability of design marks raised by Lowe (1970, 1972, as cited in Roberts, 2006), he compared students by their rank within the cohort rather than by awarded mark (p. 173). Ranks remove differences in the marking scale but still inherit the assessors' ordering of students. No study in this corpus reports inter-rater reliability for routine studio assessment, and establishing whether such studies exist elsewhere lies beyond the scope of this review.

Context also explains why the findings disagree. Converging students outperformed diverging students in design courses only (Demirbaş & Demirkan, 2007), Imagers outperformed Verbalisers in Pektaş's drafting and creativity scores, analytic students at Cardiff began ahead and fell back (Roberts, 2006), and Carmel-Gilfilen, Başçı and Türkmen and Al-Shdaifat et al. found style unrelated to intellectual development, creative thinking and improvement, respectively. These results should not be averaged. Kvan and Jia supply the clearest reason: in two programmes studied by the same researchers, in the same university and year, with the same instrument and translation, the direction of the style and performance relationship reversed. The programmes also enrolled different students, but the difference is more plausibly a property of the studios than of students' cognition, and Roberts's attribution of the early analytic advantage to first-year project structure points the same way. Where the outcome itself differs, as between a jury grade, a drafting score and a creativity test, the studies are answering different questions rather than giving discrepant answers to one.

Accreditation is almost never stated. Three studies name the regime governing the programmes they sampled, all from the United States: Watson and Thompson name FIDER, Carmel-Gilfilen names its successor the Council for Interior Design Accreditation together with the National Architectural Accrediting Board, and Albadi and Zollinger sampled fourteen CIDA-accredited programmes. The remaining twenty-three say nothing, although the corpus spans eleven national settings whose arrangements may define studio outcomes and review assessment in different ways. Where they do, a grade carries different information in each setting and is not a common currency across them.

Sampling and geography narrow the evidence further. Two studies sampled women only (Labib et al., 2019; Taneja et al., 2023), several sampled a single year group, and where the intake differs from a population norm, as Roberts found, sample composition is partly produced by admission before any study begins. Ten of the twenty-six studies are Turkish, seven from the one Bilkent department; four are from the United States, three of them in interior design alone; three are from Australia; and no study from Southeast Asia, Latin America, Africa beyond Nigeria or continental Europe appears. Any statement about how architecture students learn, drawn from this evidence, describes those settings and not others. Al Maani reached the same conclusion from inside the literature, arguing that a difference found in one studio supports conclusions about that studio alone, and the pattern here suggests that findings have not accumulated because there is no single effect for them to accumulate toward.

What Was Never Measured

A gap in the literature is worth stating only when it can be demonstrated. What follows is assembled from the charting reported above, and each item is a statement about the studies in this set rather than about the field's

intentions. None of the twenty-six studies allocated students to instruction matched or mismatched to their measured style. The design Pashler et al. specified appears nowhere, and the one manipulation, by Al-Shdaifat et al., varied the medium of feedback rather than its fit to a classification. The prescriptive claim has therefore not been tested in any study examined here. The abstracts of the four empirical studies that could not be obtained describe no such allocation either; the closest, Kim and Choi (2026), delivered one sketching workshop to all participants and treated cognitive style as a predictor of improvement. It matters because the field's recommendations rest on that claim. Demirbaş and Demirkan (2007) advise instructors to provide a strategy relevant to each learner's style on the strength of a between-group difference in observational data, and Demirkan (2016) opens by treating the benefit of incorporating learning styles in studio instruction as an established background, a premise inherited from a literature that did not test it.

The other absences have been demonstrated in the preceding sections and need only be listed. No study reported reliability for a studio grade used as an outcome. Outside the task-scored studies, the outcome is a studio grade or grade point average, so the literature cannot separate a claim about how students learn from a claim about what studio assessors reward. Replication is thinner than the count suggests: the multi-site studies pooled their sites into one analysis, none was repeated, and the one within-study comparison, Kvan and Jia's two programmes, produced divergent results. And no study analysed style change within the same individual students.

How the Critique Was Received

The psychometric objections reached this field early. Newstead's verdict that the reliability of the Kolb inventory was poor to moderate and its validity unproven, and the test-retest indices reported by Sims et al., running as low as 0.24, both appear in Kvan and Jia's 2005 paper. Twelve studies in the corpus engage the critique, and they form a sequence rather than a list.

Kvan and Jia reported the objections and answered them with internal consistency evidence that does not address stability. Roberts (2006) noted that many style models showed little evidence of reliability or validity (p. 168) and chose Riding's instrument as the least unreliable available. Demirbaş and Demirkan (2007) reported that the Kolb inventory met only one of Coffield et al.'s four criteria, called it a useful framework nonetheless, and invoked Schmitt (1996), who wrote about the reliability of criterion measures, to defend predictor alphas as low as 0.51. Pektaş (2007, p. 179; 2010, pp. 68, 70) described the reliability of Riding's instrument as well established while citing Peterson, Deary and Austin only for their advice to analyse raw ratios; Peterson et al. had reported test-retest correlations of 0.27 and 0.53 for its two dimensions (as cited in Coffield et al., 2004a, pp. 40–41). Tucker cited Coffield et al. in both papers, recommending better-evidenced instruments for future work in 2007 but retaining the 1985 inventory in 2009 for its simplicity and comparability. Carmel-Gilfilen (2012, p. 51) cited Ruble and Stout, whose work had documented response-set bias in the Kolb inventory, as evidence that studies had confirmed its consistency and reliability. Labib et al. cited Dembo and Howard (2007), who call learning styles an educational myth, in support of the claim that recognising learning preferences improves students' learning effectiveness (Labib et al., 2019, p. 962). Albadi and Zollinger named Coffield et al., Pashler et al. and Willingham in parentheses, conceding that some researchers are sceptical, and engaged the argument no further. Wang, Newton and Lowe (2015) are the only study to change an instrument in response: citing evidence of response-set bias in the Kolb inventory (Ruble & Stout, 1990, as cited in Wang et al., 2015), they randomised the order of its endings. Their own discussion then judged group balance points, the basis of every between-group comparison in this literature, to be "utterly flawed" (p. 107), while their abstract reported that virtual reality favours the Accommodator style.

Al Maani, who is not among the twelve, reached scepticism from her own data without citing any of the literature that would have supported it. Başçı and Türkmen (2026) are the exception. They cite Pashler et al. (2008), Rohrer and Pashler (2012), Kirschner and van Merriënboer (2013), Coffield et al. (2004b), Willingham (2005) and Hattie (2009); they frame their study as a test of whether style classifications relate to creative performance; and they find no significant relationship among fifty first-year interior architecture students. A null result from fifty students recruited by convenience at one institution is weak evidence for absence, and they say so themselves in calling for larger multi-institution samples. What is notable is procedural rather than evidential. They treated the question as open and let the answer stand.

The field's own review literature shows the same pattern. Mirmoradi (2023, p. 78) reports Kvan and Jia's study as demonstrating a significant correlation between learning style and performance, and cites none of the critique. Kaba and Abdou (2022) report Tucker (2007) as finding a significant relationship between learning style and grades (p. 92), although that study analysed no grades, and they cite Pashler et al. only to name the matching hypothesis before describing it as supported by several studies (p. 98), the opposite of what Pashler et al. concluded. That leaves the field in an odd position. Twenty-one years after the objections entered its own reference lists, and after eleven engagements, only one of which changed how an instrument was administered, it has begun to doubt the prescriptive claim before any study in this corpus tested it.

What Survives

None of the foregoing licenses the conclusion that architecture education should stop attending to differences among its students. That conclusion would not follow from the evidence, and it would discard a question worth asking. What the preceding sections establish is narrower: that a particular instrument-based answer to that question has been adopted without support, and that its prescriptive form has never been tested here. The question itself is untouched.

The descriptive claim survives intact. Students in a design studio engage problems differently, and a tutor who notices that one student needs to build before deciding, while another needs to decide before building, is observing something real. What the inventories offer is a vocabulary for that observation. Used as a prompt for conversation between tutor and student, or for a student's own reflection, they carry a much lighter evidential burden. Kolb's own warning against pigeonholing (as cited in Coffield et al., 2004a, pp. 62–63) still applies, since a label offered for reflection can be taken up as a fixed description. That is the defensible use. It becomes indefensible the moment a classification is treated as a fact about a student and used to allocate instruction.

There is a stronger reason to doubt the prescriptive form in this discipline, and it comes from accounts of studio learning that architectural education has long relied on. Schön's analysis of the Quist and Petra protocol describes designing as a reflective conversation with the materials of a situation, and concludes that this knowing-in-action is learnable and coachable but not teachable (Schön, 1987, p. 158). Pallasmaa argues from a different direction that skill resides in sensory and muscular memory and passes from teacher to apprentice through perception and bodily mimesis rather than primarily through verbal instruction (Pallasmaa, 2017, pp. 96, 102). Neither is an argument for matched instruction under another name. If studio competence is acquired in these ways, modifying the delivery format of instruction to match a measured preference is the wrong lever, and self-report questionnaires about preferred information formats measure in the wrong currency. The distinction between process, level and style drawn by De Ciantis and Kirton (as cited in Coffield et al., 2004a, pp. 64–65) clarifies what survives of Kolb's work here. The experiential cycle of experience, reflection, conceptualisation and experiment describes studio iteration in terms compatible with Schön's reflective conversation; what the evidence does not support is the inventory's classification of students by style. One further direction leaves the instruments behind. Iyer and Roberts (2024), excluded here for administering none, analysed first-year architecture students' approaches to learning phenomenographically.

A study that would advance this field would do four things: allocate students at random to instructional conditions; report reliability for its outcome as well as its predictor; run across more than one institution, since the one within-study comparison in this literature produced divergent results; and state the accreditation regime governing its programmes. Random allocation raises difficulties specific to the studio, where students see one another's feedback, accreditation limits how far instruction can differ, and withholding potentially better instruction needs ethical justification.

The audit also carries implications beyond research design. For studio tutors and programme leaders, inventories are defensible as prompts for reflection and indefensible as a basis for allocating instruction. The better-measured evidence in this corpus concerns approaches to learning (Sen et al., 2021), which respond to the task and assessment conditions a studio sets (Biggs et al., 2001), and Başçı and Türkmen (2026) reached the same practical conclusion from their null result, recommending instructional scaffolding and the deliberate design of the learning environment over tailoring instruction to classifications (Başçı & Türkmen, 2026, p. 7). For those who design studio assessments, grades used in research or programme evaluation need measured agreement

between assessors, and Pektaş's expert panels show that this is achievable for design work. For accreditation bodies, the finding that grades are not comparable across regimes supports a modest recommendation: that programmes report how studio marks are moderated and how consistent they are, so that studio outcomes can be interpreted beyond the institution that produced them.

CONCLUSION

Architecture and interior design education has produced twenty-six empirical studies of learning style across twenty-six years, and the claim those studies are cited for is not the claim they tested. They established that cohorts distribute unevenly across inventory categories, and that classifications sometimes covary with studio grades in one institution in one year. From this, the field drew the recommendation that teaching should be tailored to measured styles. That recommendation requires evidence that matching instruction to preference improves outcomes, and none of the twenty-six studies examined here attempted to produce it.

Three findings stand behind that conclusion. Nine instruments are in use, and none is validated against another in this population; in-sample internal consistency is reported in six studies of twenty-six and runs from 0.27, on one scale in the most recent Bilkent study, to .869 for the Learning Processes Inventory, and the newest study in the set reports no in-sample figures for either of its instruments. The outcome variable has received almost no scrutiny, with reliability reported only where a study built or adopted an assessment for research purposes. And the findings do not converge: a frequently cited association study produced group means running in opposite directions across two programmes in the same university in the same year.

What makes this a problem about the field rather than about any individual study is a fourth. The psychometric objections entered this literature's own reference lists in 2005, and over the twenty-one years since, eleven studies engaged them, only one of which modified its instrument in response, before a 2026 study treated the question as open, tested whether style classifications related to an outcome, and accepted a null result.

The review has its own limits. The evidence base is small and concentrated, as set out above, and several studies share samples. The corpus is not exhaustive; five identified studies could be read only in abstract, and the central absence reported here is established for the twenty-six studies examined and not beyond them, although the abstracts of the four unobtained empirical studies are consistent with it. The search covered English-language publications in a single database, which will have excluded work in Turkish, Arabic, Chinese, and other languages where much of this research is conducted, as well as studies outside Scopus, including seven identified in Mirmoradi's (2023) review.

This review does not claim that the inventories are worthless or that individual differences are irrelevant to studio learning. It claims that a specific prescriptive inference was drawn from evidence that could not bear it, and that the discipline had the means to notice this for two decades. The useful question is what the design studio teaches and how it teaches it, and the studio deserves questions built for it rather than instruments borrowed from elsewhere. The experiment that would settle the prescriptive claim is within reach of the programmes that produced these studies, provided the difficulties of allocating students within a studio are addressed. That it does not appear among these studies, in a discipline that has now begun to doubt the claim, is the finding this review ends on.

Funding: This review was supported by the UTS Research Grant (UTS/RESEARCH/2/2023/11).

Use of artificial intelligence (AI): The authors used Claude for proofreading, reviewed all suggested changes, and take full responsibility for the manuscript.

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