Measurement Without Validity: The Compounding Reliability Problem in Agentic AI Evaluation

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Abstract

Agentic AI evaluation pipelines produce benchmark scores that justify deployment decisions, safety certifications, and regulatory compliance claims. No formal framework has yet characterized how validity degrades across the stages of these pipelines. We present a three-layer compounding validity model, $V_{\text{total}} \leq V_1 \times V_2 \times V_3$, that captures multiplicative degradation across task generation (V_1), human-simulator calibration (V_2), and automated judgment (V_3). Under empirically grounded estimates, a pipeline retaining 70% validity at each stage is at most 34% valid against the intended construct (range 0.22–0.54).

We validate the model against a structured survey of 55 published agentic evaluation papers, finding that approximately 82% apply structurally mismatched, incomplete, or absent inter-rater reliability (IRR) metrics—a pattern consistent with systematic V_3 collapse. We further identify empirical evidence of V_1 failures (task validity flaws in 7 of 10 popular benchmarks) and V_2 miscalibration (up to 9 percentage points inter-simulator variance,

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with systematic demographic disparities for non-Standard American English speakers).

We derive eight prescriptions grounded in psychometric science and domain-stratified reliability thresholds (ICC≥0.70; $\alpha \geq 0.67/0.70/0.80$ by consequence level) that practitioners and benchmark authors can apply immediately. The framework provides a tractable knowledge-based tool for diagnosing and correcting evaluation pipeline validity before deployment decisions are made.
Keywords: inter-rater reliability, agentic AI evaluation, construct validity, LLM-as-a-Judge, knowledge-based evaluation framework, decision support, benchmark quality

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1. Introduction

Deployment decisions, safety certifications, and regulatory compliance claims for agentic AI systems are all downstream of a number that appears deceptively clean: a benchmark score. That score is the product of a pipeline (task generation, world simulation, rater judgment), and each stage of that pipeline is a potential source of systematic, non-random measurement error.

The measurement problem in agentic evaluation is not simply that individual components are imperfect. It is that the imperfections compound. A pipeline that loses 30% of valid signal at task generation, 20% at simulation, and 25% at the judgment layer does not produce evaluation that is 75% valid. Under the most optimistic independence assumption, it produces evaluation that retains less than 42% of valid signal against the intended construct. Under correlated failures (which are more likely), it is worse.

This paper makes three contributions. First, we document empirically that agentic evaluation fails three distinct psychometric validity tests simultaneously, and that the empirical evidence for each failure is published, peer-reviewed, and specific. Second, we formalize the three-layer compounding validity model and show that failures multiply rather than add—a relationship the field has not yet quantified. Third, we derive eight prescriptions from the psychometric literature that practitioners and benchmark authors can apply immediately.

We are not arguing that agentic evaluation is worthless. We are arguing that it is less valid than current practices acknowledge, that the mechanisms of invalidity are known, and that the standards for reporting and interpreting evaluation results should be substantially higher.

2. Background: The Psychometric Standard

This section introduces the two psychometric constructs that underpin the analysis: construct validity, which defines what a measurement system is supposed to capture, and inter-rater reliability, which quantifies the consistency of the scoring process. Both are necessary conditions for a valid benchmark score.

2.1. Construct Validity as the Organizing Concept

The foundational criterion for any measurement system is construct validity: whether the measurement instrument actually captures the phenomenon it claims to measure [1]. The measurement science tradition identifies three evaluable validity types that together constitute evidence for or against construct validity:

- **Content validity**: does the evaluation cover the full scope of the construct, or only an easily measurable subset? Does a benchmark described as "measuring agentic tool use" sample the full range of tool-use behaviors, or does it measure only the subset most amenable to automated scoring (e.g., API call success rate)?

- **Criterion validity** (concurrent and predictive): does the evaluation score correlate with an external criterion of the target construct? Predictive criterion validity specifically asks whether the score predicts real-world deployment performance.

- **Construct validity evidence** (convergent and discriminant): does the evaluation correlate with conceptually related measures (convergent evidence) and fail to correlate with conceptually unrelated ones (discriminant evidence)?

Despite its centrality to measurement science, construct validity has received limited attention in AI evaluation, even as benchmark creation involves numerous high-stakes design decisions including target user selection, construct-to-task operationalization, and scoring metric choice [2]. A systematic review of 84 academic and industry papers found that technical performance was represented in 83% of evaluated works, while human-centered evaluation appeared in only 30%, and both were combined in only 15% [3]. Benchmark success and deployment value remain systematically disconnected.

Content validity failures are measurable and consequential. A systematic analysis of 194,955 benchmark questions mapped against the EU AI Act's taxonomy of model capabilities found that current benchmarks devote 61.6%

of regulatory-relevant coverage to hallucination tendency and 31.2% to performance reliability, while capabilities central to loss-of-control scenarios (including evading human oversight, self-replication, and autonomous AI development) receive zero coverage across the entire public benchmark corpus [4].

2.2. Inter-Rater Reliability: Metrics and Their Conditions of Use

Inter-rater reliability (IRR) quantifies the degree to which raters (human annotators, LLM judges, or Agent-as-a-Judge systems) agree on the same outcome. The appropriate IRR metric is determined by the structure of the rating design, not by convention or familiarity.

Cohen's κ [5] is appropriate when exactly two fixed raters score every item using the same two-rater configuration across all items. It corrects for chance agreement using per-rater marginal distributions.

Fleiss' κ [6] generalizes to three or more raters, with the critical property that rater identity may vary per item provided the number of ratings per item is constant. It uses the pooled distribution across all raters for its chance-agreement correction. This is the correct metric for fixed-size judge panels where individual judge identity may rotate.

Krippendorff's α [7] is the most general metric: it handles any number of raters with varying identity, supports missing data natively, and accommodates nominal, ordinal, interval, and continuous measurement scales through interchangeable distance functions. For ordinal rubrics, it uses ranked distance rather than binary mismatch, correctly weighting the severity of disagreement.

Intraclass Correlation Coefficient (ICC) extends IRR concepts to continuous outcomes. The two-way mixed-effects absolute agreement form,

ICC$(A, 1)$, measures the degree to which scores from one rater (or measurement occasion) agree in absolute value with scores from another. Unlike κ-family metrics, ICC$(A, 1)$ is appropriate when outcomes are measured on a continuous or interval scale and when absolute agreement (not merely consistency of ranking) is the validity target. In the context of this paper, it is used to quantify simulation calibration: the agreement between agent success rates under simulated versus real user conditions (V_2, Section 5).

Table 1 summarizes metric selection by scenario.

Table 1: IRR metric selection by agentic evaluation scenario.

Scenario	Recommended metric	Reason
3+ LLM judges, binary, complete matrix	Fleiss' κ	Nominal data, complete, multi-rater
3+ LLM judges, 1–5 rubric, complete matrix	Krippendorff's α (ordinal)	Ordinal distance metric needed
Crowdsourced annotation with missing ratings	Krippendorff's α	Handles missingness natively
2 human annotators, categorical labels	Cohen's κ	Only valid two-rater case
Continuous scores from LLM judges	Krippendorff's α (interval)	Interval distance required
Rotating judge panel membership per task	Krippendorff's α	Varying rater identity; possible missingness

2.3. Reliability Thresholds and Their Origins

The Landis–Koch scale [8] is the most frequently cited threshold system in AI evaluation: $\kappa > 0.21$ as "fair," $\kappa > 0.41$ as "moderate," $\kappa > 0.61$ as "substantial." This scale was developed for behavioral science contexts with human raters and has no special authority in AI evaluation. Its widespread use as an acceptability benchmark for LLM judge agreement is a category error.

For agentic evaluation, domain-calibrated thresholds grounded in construct validity theory are more appropriate. We propose the following tiered thresholds, derived in Section 8:

- **Exploratory capability evaluation**: $\alpha \geq 0.67$ [7, pp. 241–243]
- **Safety, bias, fairness, and risk scoring**: $\alpha \geq 0.70$ (*proposed; see Prescription 5*)
- **Scoring that gates deployment, compliance, or regulatory reporting**: $\alpha \geq 0.80$ (*proposed; see Prescription 5*)

3. Related Work

3.1. Evaluation Documentation and Knowledge Frameworks

Structured knowledge frameworks for AI evaluation documentation have emerged as a response to inconsistent reporting practices. Mitchell et al. [9] introduced Model Cards, encoding expert knowledge about model behavior, intended use, and limitations into a standardized artifact that practitioners can apply without deep ML expertise. Dhar et al. [10] extended this to

Evaluation Disclosure Cards (EvalCards), providing a structured template that enforces reporting of evaluation methodology, including IRR and construct validity criteria. These frameworks follow the knowledge-based systems tradition of encoding expert domain knowledge into structured decision procedures that non-specialists can apply consistently [11, 12]: just as early medical expert systems (MYCIN, INTERNIST) encoded diagnostic knowledge into rule-based structures, documentation frameworks encode evaluation methodology expertise into required reporting fields. The compounding validity model ($V_{\text{total}} \leq V_1 \times V_2 \times V_3$) and the IRR metric selection decision tree proposed in this paper complement these documentation standards by providing the underlying validity model that determines *what* to report before documentation is written.

3.2. Validity Theory Applied to AI Evaluation

The foundational measurement science concept this paper applies is construct validity [1]: whether a measurement instrument captures the phenomenon it claims to measure. Despite its centrality, construct validity has received limited attention in AI evaluation practice [2]. Jacobs and Wallach [13] demonstrated that fairness metrics routinely measure constructs different from their stated targets—a specific instance of the construct validity failure this paper formalizes at the pipeline level. Meimandi et al. [3] provided empirical evidence that agentic AI evaluation systematically fails to support the productivity claims it is used to justify, a finding consistent with the V_1 failures documented in Section 4. The three-layer compounding model operationalizes construct validity theory for multi-stage automated pipelines, providing a tractable computational framework for what has previously been

treated as a qualitative concern.

3.3. Decision Support for Reliability Metric Selection

The IRR metric selection decision tree (Figure 1) follows the expert system tradition of encoding domain expertise into formal decision procedures that practitioners can apply without deep methodological background [7]. James [14] provided a systematic guide to inter-annotator agreement metric selection in NLP annotation, arguing against one-size-fits-all use of Cohen's κ—a position directly aligned with the prescriptions in Section 8. The decision tree proposed here extends this guidance to agentic AI evaluation pipelines, encoding the structural conditions (rater count, identity stability, measurement scale) that determine metric validity into a formal procedure. This knowledge representation is the framework's primary deployable artifact: it transforms implicit expert knowledge about IRR metric selection into an explicit, auditable decision structure.

4. Layer 1: Task Generation Validity

In traditional static benchmarks, tasks and ground truth are fixed. Reliability can be validated once against a stable reference. In agentic evaluation, tasks are often dynamically generated by language models, embedding systematic biases from the generator into the evaluation distribution before a single agent response is scored.

The construct validity problem at this layer is that dynamically generated tasks may not sample the intended construct uniformly. An LLM task generator that overrepresents certain task types, difficulty levels, or surface

forms creates a distribution that raters can agree on perfectly, while measuring something systematically different from the intended capability.

The empirical evidence is specific. A study evaluating 10 popular agentic benchmarks found task validity flaws in 7 of them and outcome validity flaws in 7 of them, with all 10 showing benchmark reporting gaps [15]. Documented failures include task validity failures where do-nothing agents passed 38% of airline booking tasks, and outcome validity failures where LLM judges made arithmetic errors. Every benchmark showed at least one validity failure category; none provided sufficient evidence to support unqualified capability claims.

Task generation validity must be assessed before any IRR analysis is meaningful. A rating matrix derived from systematically biased tasks cannot be rescued by metric sophistication at the judgment layer.

5. Layer 2: Simulation Calibration

Many agentic benchmarks evaluate agents in interaction with simulated users or simulated world environments rather than real humans and real environments. This design choice is operationally sensible (real human interaction at evaluation scale is expensive and slow), but it introduces a second, distinct validity problem: whether the simulated environment faithfully proxies the real deployment context.

5.1. Empirical Evidence of Simulation Miscalibration

The most direct empirical evidence comes from Seshadri et al. [16], which tested LLM user simulation against real human users on τ-Bench retail tasks across participants in the United States, India, Kenya, and Nigeria. The

findings establish simulation miscalibration as a measured fact rather than a theoretical concern:

- Agent success rates varied up to 9 percentage points across different LLM user simulators, demonstrating that simulation results are not robust to simulator choice.
- Evaluations using simulated users exhibited systematic directional miscalibration: underestimating agent performance on challenging tasks while overestimating it on moderately difficult ones.
- Simulated users introduced conversational artifacts absent from real interactions: elevated question-asking, heightened politeness markers, and artificial turn structures.

5.2. The Demographic Asymmetry Problem

The calibration failures documented by Seshadri et al. [16] are not uniformly distributed across user populations. African American Vernacular English (AAVE) speakers experienced consistently worse success rates and calibration errors than Standard American English (SAE) speakers, with disparities compounding with age. Indian English speakers showed similar patterns.

This is simultaneously a measurement validity failure and a fairness failure. An evaluation pipeline calibrated on SAE simulated users is not a valid measurement of agentic capability for AAVE-speaking or non-SAE users. As shown in Section 6, the same demographic asymmetry is compounded at the judgment layer by LLM judges that exhibit systematic miscalibration across dialect groups independently of simulation layer failures.

5.3. Why Agreement on a Distorted Signal Does Not Establish Measurement Validity

The key inferential step at this layer is often missed: high rater agreement on simulated interactions does not demonstrate evaluation reliability. It demonstrates that all raters agree on a distorted input.

A scoring panel that perfectly agrees on agent scores produced by an AAVE-miscalibrated simulator has achieved high IRR in the technical sense. It has also produced a high-reliability measurement of the wrong thing. Agreement on a distorted signal is agreement on distortion.

6. Layer 3: Metric Misspecification

The third layer of validity failure occurs at judgment: even when a human or LLM judge scores agent outputs, the inter-rater reliability metric used to validate that scoring is structurally mismatched to the pipeline design in the large majority of published evaluations.

6.1. Why Cohen's κ Is Structurally Wrong for Most Agentic Evaluation Designs

Cohen's κ requires exactly two raters scoring every item, with the same two-rater pair maintained across all items. Almost no agentic evaluation design satisfies this constraint:

- LLM-as-a-Judge pipelines typically use a rotating pool of judge models that vary across evaluation runs or item subsets.
- Agent-as-a-Judge frameworks [17] explicitly assign different agent-judges per task type, domain, or stakeholder persona.

- Human annotation at scale uses crowdsourcing pools where rater identity varies per item.
- Dynamic benchmark generation produces item batches scored by different annotator subsets.

Applying Cohen's κ to any of these designs produces a mathematically invalid result, because its chance-agreement correction assumes fixed rater identity. Using it anyway (common in practice) is a silent validity failure that produces an uncorrectable reliability estimate without warning.

6.2. The Literature Scan: IRR in Published Agentic Evaluation Papers

Methodology. We conducted a structured survey of 55 papers published or posted between 2022 and 2026, selected via a stratified purposive approach across nine topic categories: major agentic benchmarks; automated grader validity studies; benchmarks with correct IRR use; LLM-as-a-Judge studies; benchmarks with structural metric mismatch; long-horizon evaluation; safety and RLHF annotation; recent 2025–2026 evaluation papers; and safety-critical IRR failure cases. Within each category, papers were selected for citation prominence, venue tier (ACL, EMNLP, NeurIPS, ICLR), and direct relevance to evaluation methodology; arXiv preprints in cs.AI and cs.CL supplemented peer-reviewed coverage where thin. This is a purposive stratified survey, not a systematic review: the sample is designed to cover the main failure modes rather than to exhaust a keyword-defined corpus.

Coding was applied against four pre-specified dimensions: (1) the IRR metric reported or absent, (2) the rater design (number of raters, fixed or rotating identity), (3) the measurement scale (binary, nominal, ordinal,

or continuous), and (4) whether the metric's structural assumptions were satisfied by the pipeline design. The full coding criteria are documented in Appendix Appendix A.

Reliability validation. Coding was performed by the primary author. To assess coding reliability, a stratified random sample of $N = 20$ papers (36% of the corpus, proportionally allocated across the nine topic categories) was coded independently by three LLM raters from distinct model families: NVIDIA Nemotron-Ultra-550B, Google Gemma-4-31B, and Alibaba Qwen3-80B-A3B [18]. Each LLM rater received only the coding instrument and the paper's abstract and evaluation methodology passage, with no access to the primary-rater codes. Four-way Krippendorff's α across the author and all three LLM raters for the structural validity dimension was $\alpha = 0.89$ (pairwise range: 0.86–0.93), meeting the exploratory reliability threshold of $\alpha \geq 0.67$ adopted in the prescriptions of this paper.

Finding ($\alpha = 0.89$; four-rater validation). Across 55 coded papers, approximately 10 (18%) appear to use structurally correct IRR metrics with explicit rationale. The remaining 45 exhibit one of three failure modes:

Mode 1: Automated Grading, No IRR (11%).. Major benchmark papers—τ-bench [19], OSWorld [20], SWE-bench [21], GAIA [22], and WebArena [23]—use deterministic automated graders or unvalidated LLM judges with no IRR measurement against human annotation; τ^2-Bench [24], the quality-fix follow-up to τ-bench, takes the same approach: tighter automated grading rather than human IRR validation. Gurram [25] directly measures the gap: substring-based automated evaluation achieves $\kappa = 0.049$ against human annotation (essentially chance-level) while a three-LLM ensemble achieves

only $\kappa = 0.432$. The Verified re-releases of these benchmarks (WebArena Verified, El Hattami et al. 26: Cohen's $\kappa = 0.83$) demonstrate that rigorous human annotation produces fundamentally different reliability evidence.

Mode 2: Structural Metric Mismatch (16%).. Papers that do report IRR most commonly apply Cohen's κ to panels of three or more LLM judges, or report raw percentage agreement for ordinal and ternary scales without chance correction (Fan et al. 27 AgentProcessBench: 89.1% on ternary labels). In Han et al. [28] the mismatch is more subtle: Cohen's κ is applied as a series of pairwise comparisons (each of 54 LLMs vs. human ratings), each individually a valid two-rater design, but the results are then aggregated and interpreted as panel reliability across 54 configurations with varying rater compositions, a use case requiring Fleiss' κ or Krippendorff's α, not repeated two-rater κ.

Mode 3: Wrong Metric for the Measurement Construct (31%).. Safety, preference, and RLHF papers apply metrics appropriate for one statistical question to answer a different one: Pearson correlation for ordinal safety ratings from 112 demographically diverse annotators [29]; ELO ratings substituting for inter-annotator reliability in crowdsourced pairwise preference [30]; raw percent agreement without chance correction for 40 contractors ranking model outputs [31, 72–77%].

Mode 4: No Metric Reported (24%).. The largest single category by paper count (13 of 55) reports no IRR metric of any kind. This group includes RLHF annotation pipelines, red-teaming studies, and safety evaluation reports in which the absence of reliability reporting is treated as unremarkable. Unlike

Mode 1 (automated graders that could in principle be validated against human annotation), Mode 4 papers lack sufficient information to even assess which metric would be appropriate. The absence of any reliability report is a complete V_3 validity gap.

Table 2 summarizes the distribution.

Table 2: IRR failure modes across 55 coded papers. Only 10 (18%) use structurally correct metrics with explicit rationale.

Failure mode	Papers	%	Representative example
Automated grading, no IRR	6	11	τ-bench, OSWorld, SWE-bench
Structural metric mismatch	9	16	Cohen's κ for 54-LLM panel [28]
Wrong construct / % only	17	31	InstructGPT; Chatbot Arena (ELO)
No metric reported	13	24	RLHF pipelines, Red Teaming
Correct metric	**10**	**18**	WebArena Verified ($\kappa = 0.83$)

Structural contrast. The 10 papers using IRR correctly share a distinguishing behavior: they report multiple metrics explicitly and state their rationale. Papers using incorrect metrics report only Cohen's κ with no structural justification. The mismatch is not a deliberate simplification; it reflects unawareness of the structural conditions that govern metric validity.

6.3. Demographic Asymmetry in LLM Judge Calibration

The metric mismatch problem documented above is compounded by a systematic calibration asymmetry in LLM judges across linguistic varieties.

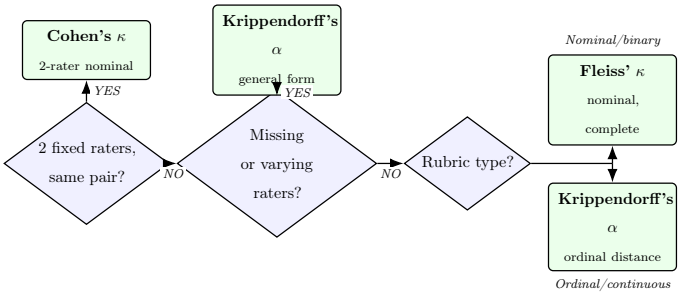
In a study evaluating three LLMs as toxicity judges across 60 language varieties spanning 10 language clusters, LLM–human agreement was the weakest dimension of consistency—weaker than cross-model or cross-dialect consistency—with systematic gaps across dialectal groups [32]. Judge miscalibration for non-standard dialect speakers persists in direct LLM judgment, independently of the simulation layer failures documented in Section 5.2.

The implication for the compounding model is asymmetric V_3: benchmarks using LLM judges without dialect-stratified calibration validation will produce V_3 values that overstate validity for SAE-speaker user populations and understate it for non-SAE populations. This is an IRR validity failure that structurally correct metric selection cannot by itself correct, because the miscalibration occurs within the judge's learned behavior, not in the choice of metric.

6.4. Ordinal Rubrics and the Kappa Penalty

Most agentic evaluation rubrics use ordinal scoring (e.g., 1–5 on helpfulness, safety, coherence). Fleiss' κ treats all disagreements as categorically equal: a rater scoring 1 when others score 5 is penalized identically to a rater scoring 4 when others score 5. This is wrong for ordinal data.

The consequence is bidirectional distortion: Cohen's κ and Fleiss' κ artificially inflate apparent disagreement when raters are closely aligned on an ordinal scale (one point apart) and can underestimate disagreement when raters are at opposite ends of the scale. For safety evaluation, this matters: a safety rubric where judges disagree by 1 point appears as unreliable as one where they disagree by 4 points.



Selection rules:

Cohen's κ
Exactly 2 fixed raters, same pair, every item.

Fleiss' κ
$\geq$3 raters, complete matrix, nominal/binary rubric.

Krippendorff's α (general)
Missing ratings or rater identity varies.

Krippendorff's α (ordinal)
Complete matrix; ordinal or continuous rubric.

Using Cohen's κ outside the 2-rater fixed-pair case produces an invalid reliability estimate without warning.

Figure 1: IRR metric selection. Left: decision tree based on rater design, matrix completeness, and rubric scale. Right: selection rules summary. Using Cohen's κ outside the two-rater fixed-pair case is a silent validity failure.

6.5. Metric Selection Decision Tree

Figure 1 provides a decision tree for selecting the structurally correct IRR metric based on rater design, data completeness, and measurement scale.

Figure 1 is a knowledge-based decision support system in the expert systems tradition [14]: it encodes domain expert knowledge about IRR metric structural assumptions into a formal decision procedure that practitioners can apply without background in psychometrics. The four decision nodes correspond to the four structural conditions that govern metric validity; the terminal nodes are executable recommendations. A practitioner who provides

the rater count, identity stability, and scale type obtains a structurally correct metric recommendation without needing to understand the mathematical foundations of chance-agreement correction. This procedural knowledge representation is the framework's primary deployable artifact, complementing the governance prescriptions in Section 8.

7. The Compounding Argument

The full mathematical derivation, including the correlated-failure extension and a practical estimation protocol, is in Section 9 and Appendix Appendix B.

7.1. Formal Statement

Let $V_1 \in [0, 1]$ denote the task generation validity: the degree to which the generated task distribution matches the ideal construct distribution, defined as one minus the total variation distance between them. Because the ideal construct distribution $D_{\mathcal{C}^*}$ is a theoretical object and not directly observable, V_1 cannot be computed from first principles; in practice it is estimated by comparing generated tasks against a curated expert reference set (Appendix Appendix B, §B.6). Let $V_2 \in [0, 1]$ denote the simulation calibration validity, defined as the ICC$(A, 1)$ between simulated and real agent outcomes on a matched task set, estimated separately per user population group. Let $V_3 \in [0, 1]$ denote the judgment validity: the appropriate IRR metric value (Krippendorff's α or Fleiss' κ, per the pipeline's measurement scale) for the rating design.

The overall construct validity of the evaluation pipeline is bounded above by:

$$V_{\text{total}} \leq V_1 \times V_2 \times V_3 \tag{1}$$

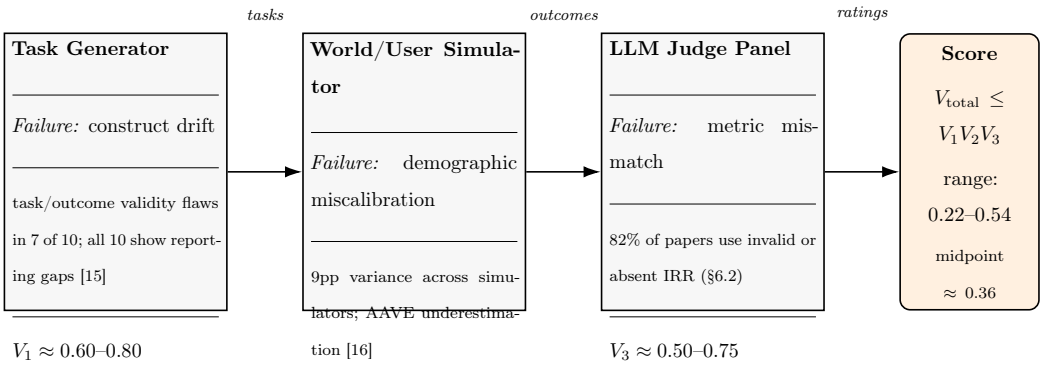


Figure 2: Three-layer agentic evaluation pipeline with validity estimates. Each layer introduces a distinct failure; failures compound multiplicatively under the independence bound ($V_{\text{total}} \leq V_1 \times V_2 \times V_3$, range 0.22–0.54, midpoint ≈ 0.36). The bound tightens further under correlated failures when the same model family operates at all three layers.

The bound holds under the assumption of independence between layer failures. When layer failures are positively correlated (as they are likely to be when the same provider family operates at all three layers), V_{total} falls below this bound. The independence assumption is therefore optimistic; the correlated-failure bound is tighter (Appendix Appendix B, §B.4).

Figure 2 illustrates the three-layer pipeline with its validity estimates and the compounding formula.

7.2. Applying the Model to Published Benchmarks

Table 3 applies the compounding model to six widely-used agentic benchmarks using values drawn directly from the cited literature.

V_1 **sources**: Zhu et al. [15] identified task validity failures per benchmark: do-nothing agents passed 38% of τ-bench airline tasks ($V_1 = 0.62$); stale CSS caused 28% performance underestimation in OSWorld ($V_1 = 0.72$); augmented test cases changed 41% of SWE-bench rankings ($V_1 = 0.59$); WebArena's LLM judge introduced 1.6–5.2% misestimation ($V_1 = 0.97$).

V_2 **sources**: OSWorld, SWE-bench, and WebArena use real computer or web environments without LLM user simulators; $V_2 = 1.00$ for these. For τ-bench, Seshadri et al. [16] measured Expected Calibration Error ($\text{ECE}_{\text{Human-LLM}}$) between simulated and real user outcomes. We operationalize $V_2 \approx 1 - \text{ECE}/100$: 11.7% for Standard American English speakers ($V_2 = 0.88$) and 20.3% for AAVE speakers ($V_2 = 0.80$).

V_3 **sources**: For benchmarks with no reported human IRR, we use the empirically measured automated grader validity from Gurram [25]: substring matching achieves $\kappa = 0.049$ against human annotation on comparable tasks (chance-level). This is the empirical V3 floor for automated-only evaluation. For WebArena Verified, $V_3 = 0.83$ from the reported Cohen's κ between two fixed human annotators [26].

Table 3: Compounding validity bounds for published agentic benchmarks, using values derived from the cited literature. V_3^*: no IRR reported; value estimated from automated grader validity against human annotation (Gurram 2026, $\kappa = 0.049$). $V_2^\dagger$: real environment, no LLM user simulator.

Benchmark	V_1	V_2	V_3	$V_{\text{total}} \leq$
τ-bench retail (SAE users)	0.62	0.88	0.05*	**0.027**
τ-bench retail (AAVE users)	0.62	0.80	0.05*	**0.025**
OSWorld	0.72	1.00†	0.05*	**0.036**
SWE-bench	0.59	1.00†	0.05*	**0.030**
WebArena (original)	0.97	1.00†	0.05*	**0.049**
WebArena Verified	0.97	1.00†	0.83	**0.805**

Three findings emerge. First, every benchmark without human IRR ($V_3 \approx 0.05$) produces $V_{\text{total}} < 0.05$ regardless of V_1 and V_2: less than 5% of

valid signal survives the judgment layer alone. The automated grader is the binding constraint.

Second, correcting V_3 through rigorous human annotation increases V_{total} by approximately 16×: WebArena original ($V_{\text{total}} \leq 0.049$) versus WebArena Verified ($V_{\text{total}} \leq 0.805$). The only difference is the addition of two fixed human annotators with $\kappa = 0.83$. This demonstrates empirically that Prescription 2 (metric selection) and Prescription 8 (IRR as a required reporting field) are the highest-leverage interventions.

Third, the demographic disparity documented by Seshadri et al. [16] appears in V_2 (SAE: 0.88 vs. AAVE: 0.80) but produces similarly low V_{total} for both groups (0.027 vs. 0.025) because $V_3 = 0.05$ dominates. For populations where V_3 is corrected, the V_2 gap becomes the binding constraint and the fairness implications are larger.

7.3. Implications for Published Benchmark Claims

When a paper reports that "Agent X achieves 82% on [benchmark]," the epistemic weight of that claim is conditioned on the validity of the three pipeline layers. Under the compounding model, an 82% score from a benchmark with $V_{\text{total}} \leq 0.049$ (Table 3, WebArena original) does not tell us that the agent achieves 82% of the intended capability. It tells us that the agent achieves some unknown capability level, measured by an instrument that retains less than 5% of valid signal against the intended construct.

This does not mean the score is uninformative. It means its interpretation requires the three validity layers to be assessed and reported alongside the score. The WebArena Verified result ($V_{\text{total}} \leq 0.805$) demonstrates that this standard is achievable at the cost of adding rigorous human IRR to the

evaluation design.

8. Prescriptions for Valid Agentic Evaluation

Items supported by existing literature are stated as requirements; items that extend beyond current literature are marked *we propose*.

1. **Validate simulation calibration before trusting evaluation results.** Use a held-out set of real human interactions to measure the ICC$(A, 1)$ between simulated and real agent outcomes. If calibration is not validated, the evaluation signal is of unknown reliability regardless of how precisely the judging rubric is specified [16]. Report the calibration ICC alongside any benchmark result that uses user simulation. *We propose ICC$(A, 1) \geq 0.70$ as a minimum threshold for simulation calibration, set 3pp above Krippendorff [7]'s general content analysis minimum of 0.67 because LLM simulators exhibit systematic behavioral artifacts absent from trained-human rater contexts [16].*
2. **Select the IRR metric based on pipeline structure, not convention.** Apply Cohen's κ only when exactly two fixed raters score every item. Apply Fleiss' κ when a fixed-size panel of three or more raters scores nominal or categorical outcomes on a complete matrix. Apply Krippendorff's α when the rubric is ordinal or continuous, when missing ratings are possible, or when rater panel membership varies. Using Cohen's κ outside its structural assumptions is a silent validity failure (Section 6).
3. **Report both Fleiss' κ and Krippendorff's α for any rubric using ordinal or interval scales.** When the two metrics diverge, the

divergence is itself diagnostic: $\kappa < \alpha$ suggests raters are frequently one scale point apart rather than at opposite ends of the range, a much healthier disagreement pattern than the κ score alone would imply.

4. **Use cross-family judge ensembles.** Self-preference and family-level bias in LLM judges [33, 34] are large enough to distort comparative agent rankings. Evaluation pipelines should use judges from at least two distinct provider families and report inter-judge agreement across families. Single-family judge panels produce spuriously inflated within-family agreement.

5. **Apply domain-appropriate reliability thresholds.** For exploratory capability evaluation, $\alpha \geq 0.67$ is the minimum consistent with Krippendorff [7, pp. 241–243]'s own recommendation. For safety, bias, fairness, and risk scoring, *we propose* $\alpha \geq 0.70$. For scoring that gates model deployment, compliance certification, or regulatory reporting, *we propose* $\alpha \geq 0.80$. When these thresholds cannot be met, as Jafari et al. [35] demonstrate for LLM mental health response evaluation (three certified psychiatrists produced ICC = 0.087–0.295 and $\alpha = -0.203$), the correct response is not to lower the threshold but to recognize that the construct is insufficiently specified for reliable measurement.

6. **Validate dynamically generated task distributions against curated reference sets.** When tasks are generated by an LLM, validate that the generated distribution samples the intended construct by testing a sample of generated tasks against domain-expert judgment: present tasks to experts who rate each on whether it faithfully instantiates the target construct, and compute V_1 as the proportion rated as construct-

valid [15]. Where a curated expert reference set exists, total variation distance between the generated and reference distributions provides a more formal measure; in practice, expert-rated proportions are the tractable operationalization (Section 9, Step 1).

7. **Stratify evaluation by demographic and linguistic group.** Simulation calibration failures are not uniformly distributed across user populations [16], and LLM judge calibration failures show the same pattern across dialect groups [32]. AAVE speakers, Indian English speakers, and other non-SAE populations must be included in calibration validation before benchmark results are considered representative.
8. **Report IRR as a required field in evaluation documentation.** Standardized reporting frameworks such as EvalCards [10] provide the infrastructure to enforce consistent disclosure across papers and model releases. Including IRR metric, threshold, and metric-selection rationale as required fields in evaluation reporting cards would institutionalize the prescriptions above at the submission level.

9. Applying the Framework: A Pipeline Assessment Procedure

The following procedure operationalizes the compounding validity model as a knowledge-based pipeline assessment tool. Pipeline operators can apply it at evaluation design time to estimate V_{total} and identify which layer requires the most attention before results are reported.

1. **Estimate V_1 (task generation validity).** Generate a sample of 100 tasks from the pipeline's task generator G. Present them to domain experts who score each task on whether it faithfully samples the intended

construct. Compute V_1 as the proportion rated as construct-valid. If a curated reference set is available, measure total variation distance between generated and reference distributions instead.

2. **Estimate V_2 (simulation calibration).** Run a matched subset of tasks (minimum 50) with both the LLM simulator S and real human users. Compute ICC$(A, 1)$ between simulated and real agent success rates, separately for at least two user population groups (e.g., SAE and one non-SAE group). Use the population-weighted average as V_2.
3. **Estimate V_3 (judgment validity).** Apply the IRR metric selection procedure (Figure 1) to identify the structurally correct metric for the pipeline's rater design. Compute that metric on a sample of ratings. This value is V_3.
4. **Compute the bound.** $V_{\text{total}} \leq V_1 \times V_2 \times V_3$. Report this bound alongside any benchmark result derived from the pipeline.

Interpretation guideline. If $V_{\text{total}} \leq 0.50$, benchmark scores should not be used for consequential deployment decisions without independent validation. If $V_{\text{total}} \leq 0.30$, the pipeline should be redesigned before any deployment claim is made. The formal derivation and correlated-failure extension are in Appendix Appendix B.

10. Threats to Validity

We assess threats to the validity of this study following the construct, internal, external, and conclusion validity framework standard in empirical research methodology [36, 37].

10.1. Construct Validity

Operationalization of V_1, V_2, V_3. The three validity measures are operationalizations of theoretical constructs. The ideal construct distribution $D_{\mathcal{C}^*}$ (Section 7.1) is a theoretical object; V_1 cannot be computed from first principles and is estimated here from published benchmark failure rates [15] rather than direct total variation distance measurement. V_2 is operationalized via Expected Calibration Error (ECE) rather than $\text{ICC}(A, 1)$: specifically, $V_2 \approx 1 - \text{ECE}_{\text{Human-LLM}}/100$ from Seshadri et al. [16]. This is an approximation; the equivalence holds only when ECE measures uniform miscalibration, which is not guaranteed. V_3 for benchmarks without human IRR uses the automated grader validity floor ($\kappa = 0.049$) from Gurram [25] rather than a direct IRR measurement for each specific benchmark. Readers should treat the values in Table 3 as derived estimates rather than direct measurements of each construct.

IRR coding taxonomy. The four-dimension coding scheme (Section 6.2) may not capture all relevant failure modes. In particular, papers that apply the structurally correct metric but with an incorrect threshold, or that report IRR on a subset of items not representative of the full evaluation, fall outside the coding scheme and may be classified as "correct" when they should not be. Appendix Appendix A provides the full coding table to support independent assessment.

Compounding model. The multiplicative validity bound (Equation 1) is presented as a conceptual model, explicitly not a proved theorem (Appendix Appendix B, §B.3). The motivating argument assumes layer independence and treats each V_i as a normalized signal-to-noise ratio; both assumptions

are approximations. The bound should be read as a formalization of the compounding intuition, not a mathematical guarantee.

10.2. Internal Validity

Coding reliability. The literature scan (Section 6.2) was coded by the primary author. To address the potential self-referential tension (a paper about IRR using single-rater coding), we conducted a four-rater reliability study on a stratified $N = 20$ subsample. Three LLMs from distinct model families (NVIDIA Nemotron-Ultra-550B, Google Gemma-4-31B, Alibaba Qwen3-80B-A3B; OpenRouter 18) independently applied the coding instrument to the same excerpts, with no access to the primary-rater codes. Four-way Krippendorff's $\alpha = 0.89$ on the structural validity dimension meets the paper's own exploratory threshold ($\alpha \geq 0.67$), supporting the 82% IRR misuse prevalence as an IRR-validated finding rather than a structured estimate.

A secondary tension is worth naming explicitly: this paper argues that LLMs exhibit systematic miscalibration as evaluative raters (Sections 5.2 and 6), yet uses LLM raters as a validation mechanism here. These are not equivalent roles. The $\alpha = 0.89$ result does not claim that LLMs are valid substitutes for domain-expert human raters; it demonstrates that the coding instrument is sufficiently unambiguous to be applied consistently by raters from different model families, none of whom had access to the author's codes. Instrument clarity (can independent agents apply the same decision rules?) is a more limited claim than rater validity (do the raters' judgments track the true construct?), and it is the former that the four-rater study supports. The pre-specified coding criteria and full coding table in Appendix Appendix A make individual coding decisions verifiable independently by human readers.

Purposive sampling and confirmation bias. The 55-paper sample was selected purposively to cover nine topic categories, not to exhaust a keyword-defined corpus. This exposes the scan to selection bias: papers demonstrating failures may be more visible (cited more, identified more easily) than papers with correct but unreported IRR practices. The author's prior belief that IRR is systematically misused may have influenced which papers were included in each category or how ambiguous cases were coded. The structured category definitions and transparent category membership (Appendix Appendix A) mitigate but do not eliminate this threat.

10.3. External Validity

Sample scope. The 55 papers were drawn primarily from English-language venues (ACL, EMNLP, NeurIPS, ICLR) and arXiv preprints in cs.AI and cs.CL. Papers from other geographic regions, language communities, or non-English venues may show different IRR reporting patterns. The temporal scope (2022–2026) means that earlier work and work after the scan cutoff is not captured; IRR practices are evolving and the field may improve.

Domain of the compounding model. The three-layer model was constructed for agentic evaluation pipelines involving task generation, user simulation, and LLM judgment. Static benchmarks with fixed tasks, no simulation layer, and human-only raters present a different threat model where only Layer 3 (IRR metric selection) directly applies. The prescriptions in Section 8 are intended to generalize across evaluation pipeline designs, but their applicability to static benchmarks should be assessed separately.

10.4. Conclusion Validity

Independence assumption. The primary conclusion of Section 7 is that validity failures compound multiplicatively. This conclusion depends on the independence assumption: that layer failures are statistically unrelated. When the same underlying mechanism (e.g., the same LLM provider family) operates at all three layers, failures are positively correlated and V_{total} falls below the independence bound. The conclusion is therefore stated conservatively: the independence bound is presented as optimistic, and correlated-failure scenarios are shown to produce lower V_{total} (Appendix Appendix B, §B.4). The qualitative conclusion (failures multiply rather than add) is robust to the independence assumption because the bound holds as an upper limit regardless of correlation structure.

Derived estimates in Table 3. The V_i values in the benchmark table (Section 7.2) are derived from published measurements using approximations documented in the construct validity subsection above. The V_3 floor ($\kappa = 0.049$) is drawn from a single study [25] on one task type; whether it generalizes to deterministic automated graders (e.g., test-suite execution in SWE-bench) or other evaluation designs is an open empirical question.

To bound the sensitivity of the primary conclusion to this assumption, Table 4 shows V_{total} across three V_3 scenarios for the τ-bench and SWE-bench cases.

Even under the most optimistic $V_3 = 0.50$ scenario, V_{total} remains below 0.30 for both benchmarks, falling below the pipeline-redesign threshold proposed in Section 9. The qualitative conclusion (benchmarks without human IRR validation retain less than 30% of valid signal even under generous as-

Table 4: Sensitivity of V_{total} to V_3 assumption for two representative benchmarks. V_1 and V_2 values are held fixed at the values in Table 3. $V_3 = 0.05$ is the automated grader floor from Gurram [25]; $V_3 = 0.30$ and $V_3 = 0.50$ represent plausible scenarios for higher-quality automated graders.

Benchmark	V_1	V_2	V_3	$V_{\text{total}} \leq$
τ-bench retail (SAE)	0.62	0.88	0.05	**0.027**
τ-bench retail (SAE)	0.62	0.88	0.30	**0.164**
τ-bench retail (SAE)	0.62	0.88	0.50	**0.274**
SWE-bench	0.59	1.00	0.05	**0.030**
SWE-bench	0.59	1.00	0.30	**0.177**
SWE-bench	0.59	1.00	0.50	**0.295**

sumptions) is robust across the full V_3 sensitivity range. A reader who assigns $V_3 > 0.50$ to automated graders is implicitly claiming that automated grading achieves moderate reliability against human annotation on these specific benchmark tasks; that claim should be supported by direct grader-vs-human measurement analogous to Gurram [25], which has not been published for these benchmarks.

11. Discussion

11.1. What This Paper Does Not Claim

We do not claim that current agentic evaluations are worthless. They provide directional signal. We claim that the field's current practices overstate the validity and precision of that signal, and that the standards for reporting should be substantially higher. A benchmark result reported without IRR

metrics, simulation calibration evidence, and metric selection justification is incomplete, in the same way that a clinical measurement reported without instrument validity data is incomplete.

11.2. The Governance Stakes

For academic benchmark comparisons, validity limitations are a scientific quality issue. For deployment decisions, safety certifications, and regulatory claims, they are a governance failure. Agentic AI systems are increasingly being deployed in consequential contexts. The evaluation apparatus used to justify those deployments must meet the same validity standards applied to any measurement-driven governance process.

The regulatory dimension makes this concrete. Emerging frameworks such as the EU AI Act require evidence of comprehensive risk assessment for general-purpose AI models. A systematic analysis of 194,955 benchmark questions found that capabilities central to loss-of-control (including evading human oversight, self-replication, and autonomous AI development) receive zero benchmark coverage [4]. If the construct coverage gap means that no existing benchmark can provide evidence of regulatory compliance, then the IRR validity of those benchmarks is a secondary concern: the primary problem is that they do not measure what regulations require. Both failures must be addressed together.

11.3. Broader Impact

This work raises the evidential bar for deployment certifications, surfaces demographic calibration failures that existing evaluation practice obscures,

and gives practitioners eight actionable criteria they can apply using existing psychometric tools.

Three misuse risks follow from this work. First, the proposed reliability thresholds ($\alpha \geq 0.70$, $\alpha \geq 0.80$) may become compliance checkboxes rather than genuine validity indicators. Meeting a threshold does not guarantee valid measurement; it establishes the minimum condition for a score to be interpretable. Second, stratified calibration across demographic and linguistic groups increases evaluation cost; benchmark maintainers and shared evaluation platforms should bear primary responsibility for calibration coverage, not individual research teams. Third, the finding that three certified psychiatrists produced ICC = 0.087–0.295 on LLM mental health safety evaluation [35] should not discourage research in high-stakes AI domains. It means the construct is underspecified for reliable measurement, which calls for more rigorous construct development, not withdrawal.

12. Conclusion

The move from static to dynamic agentic evaluation does not reduce the importance of inter-rater reliability; it multiplies the surfaces on which reliability can fail and introduces new systematic biases at each layer: task generation, world simulation, and judgment. The compounding effect means that an evaluation pipeline with moderate validity problems at each of three layers can produce an evaluation signal that is far less valid than any single-layer estimate would suggest.

The science of measurement was not built for static benchmarks alone. It was built for situations where the measurement apparatus is a source of

variance, where raters disagree in structured rather than random ways, and where the construct must be separated from the instrument measuring it. That is agentic evaluation.

These standards are achievable. El Hattami et al. [26] (WebArena Verified) demonstrates this directly: adding rigorous human annotation with two fixed annotators and reporting Cohen's $\kappa = 0.83$ [0.81, 0.85] satisfies the metric selection requirement (Prescription 2), exceeds the exploratory reliability threshold (Prescription 5), and treats IRR as a required reporting field (Prescription 8). WebArena Verified meets three of the eight prescriptions through a design choice the field already knows how to make.

Fleiss' κ, Krippendorff's α, ICC, and construct validity frameworks are available, validated, and applicable. The field's task is to use them, to report IRR as a first-class metric alongside every benchmark result, and to recognize that a score without validity evidence is not a measurement. It is a number.

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CRediT Authorship Contribution Statement

William Caban: Conceptualization, Methodology, Formal Analysis, Investigation, Writing – Original Draft, Writing – Review & Editing, Visualization.

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The author is employed by Red Hat, Inc. This research was conducted independently as part of the author's doctoral program at Alma Mater Europaea University. The views expressed are the author's own and do not represent the official position of Red Hat, Inc. The author declares no financial competing interests.

Declaration of Generative AI and AI-Assisted Technologies

During preparation of this work the author used Claude (Anthropic) to assist with converting the LaTeX template to elsarticle format and editing text for length. After using this tool, the author reviewed and edited the content as needed, and takes full responsibility for the content of the publication. Claude is not listed as an author.

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This study is based entirely on analysis of publicly available published research. No human participants, personal data, or animal subjects were involved.

Data Availability

The data and code supporting this study are openly available. The second-rater IRR validation experiment—including the 20-paper stratified

subsample coding results (`results.csv`), author codes (`author_codes.csv`), paper excerpts used as rater input (`excerpts.json`), and the Python scripts for running the LLM raters and computing Krippendorff's α—are deposited at:

`https://github.com/williamcaban/`
`experiment-measurement-without-validity`

The repository is released under the Apache 2.0 license. The literature scan coding table (Appendix Appendix A) is included in full within this paper; all 55 papers in the scan are publicly available at the venues and arXiv identifiers cited. No proprietary datasets, model weights, or benchmark systems were created or used. Code for the compounding model (Appendix Appendix B) requires no implementation beyond the equations stated.

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Appendix A. Literature Scan: Coding Criteria and Category Summary

This appendix documents the methodology and results of the structured literature scan reported in Section 6.2. It presents the pre-specified coding criteria, a category summary of the 55 coded papers, and notable individual cases referenced in the main text.

A.1 Pre-Specified Coding Criteria

Each of the 55 papers was coded on four dimensions, applied uniformly before reading the paper's results section:

1. **IRR metric reported**: Cohen's κ, Fleiss' κ, Krippendorff's α, ICC (form specified or unspecified), percentage agreement only, or no metric reported.
2. **Rater design**: number of raters; whether rater identity was fixed across items or varied (rotating pool, crowdsourced, or multiple independent runs of the same model).
3. **Measurement scale**: binary, nominal (unordered categories), ordinal or ternary (ordered but discrete), or continuous.
4. **Structural validity**: whether the reported metric's structural assumptions were satisfied by the rater design and measurement scale. Coding

applied the following rules: Cohen's κ for > 2 raters or rotating identity $\rightarrow$ MISMATCH; Fleiss' κ for ordinal or continuous scale $\rightarrow$ PARTIAL MISMATCH; percentage agreement without chance correction $\rightarrow$ INCOMPLETE; no IRR with automated grader of unvalidated accuracy $\rightarrow$ LAYER-3 VALIDITY FAILURE.

A.2 Category Summary

Table A.5 summarizes the 55 papers by topic category.

A.3 Notable Individual Cases

- Jafari et al. [35]: Three certified psychiatrists, correct metrics, ICC $= 0.087$–0.295 and $\alpha = -0.203$ on LLM mental health response safety: highest-stakes domain, most disagreement.
- Ouyang et al. [31]: 40 rotating contractors, 4-way preference ranking. Reports 72–77% raw percent agreement with no chance correction; requires Krippendorff's α for rotating-rater ordinal design.
- Gurram [25]: Direct grader-vs-human comparison; substring grading achieves $\kappa = 0.049$ (chance-level), 3-LLM ensemble $\kappa = 0.432$.
- El Hattami et al. [26] (WEBARENA VERIFIED): Two annotators per task (primary + verifier), binary success/fail labels, Cohen's $\kappa = 0.83$ [0.81, 0.85] across all 812 tasks: correct metric, threshold exceeded, reporting complete. The strongest positive example in the scan.

Appendix B. Formal Derivation of the Compounding Validity Model

This appendix provides the mathematical treatment supporting Section 7.

B.1 Definitions

Let an agentic evaluation pipeline Π consist of three sequential components:

- G: a task generation function that samples tasks t from a distribution D_G over task space $\mathcal{T}$
- S: a simulation function that, given a task t and an agent A, generates an interaction trajectory $\tau = S(t, A)$ drawn from a distribution $D_S(t)$
- J: a judgment function that, given a trajectory τ, assigns a score $s = J(\tau) \in \mathbb{R}$

Let $\mathcal{C}^*$ denote the target construct: the latent capability the evaluation is designed to measure. The construct $\mathcal{C}^*$ induces an ideal task distribution $D_{\mathcal{C}^*}$ over $\mathcal{T}$ and an ideal scoring function J^* that maps agent behavior to true capability.

B.2 Layer-Specific Validity Measures

V_1: Task Generation Validity..

$$V_1 = 1 - d_{\text{TV}}(D_G,\, D_{\mathcal{C}^*})$$

where d_{TV} denotes total variation distance. $V_1 = 1$ when $D_G = D_{\mathcal{C}^*}$ exactly; $V_1 = 0$ when the supports are disjoint.

V_2: Simulation Calibration Validity..

$$V_2 = \text{ICC}(\text{outcomes}_{\text{sim}},\ \text{outcomes}_{\text{real}})$$

where ICC is computed using the two-way mixed-effects model for absolute agreement (ICC$(A, 1)$). V_2 must be estimated separately per demographic and linguistic user group:

$$V_2 = \mathbb{E}_{g \sim P_{\text{users}}}[V_2^{(g)}]$$

V_3: Judgment Validity.. V_3 measures the degree to which the rating protocol produces valid inter-rater agreement, using the appropriate IRR metric for the pipeline's measurement scale.

B.3 The Multiplicative Validity Bound

Conceptual bound. We state the following as a conceptual model, not a proved mathematical proposition. Under the assumption that V_1, V_2, V_3 are independently determined:

$$V_{\text{total}}(\Pi) \leq V_1 \cdot V_2 \cdot V_3$$

Motivating argument. Let q denote an agent query drawn from the evaluation pipeline. Under independence, the signal-to-noise ratio of the pipeline degrades multiplicatively across layers:

$$\text{SNR}(\Pi) \leq \text{SNR}(G) \cdot \text{SNR}(S) \cdot \text{SNR}(J)$$

Treating each V_i as the normalized SNR of the corresponding component then yields the bound. The motivating argument is that layer-wise validity losses multiply; this intuition is well-grounded even if the full formal derivation is not supplied here.

Remark 1. *Because this is a conceptual bound rather than a proved proposition, equality in the inequality is not established. The bound becomes more*

informative (closer to the true V_{total}) when layer failures are less correlated, and less informative when positive correlation drives V_{total} below the product.

B.4 The Correlated-Failure Extension

Let $\epsilon_i = 1 - V_i$ denote the error at layer i, and let $\rho_{ij} = \text{Corr}(\epsilon_i, \epsilon_j)$. When $\rho_{ij} > 0$ (positively correlated failures):

$$V_{\text{total}} < V_1 \cdot V_2 \cdot V_3 \qquad (\text{when } \rho_{ij} > 0)$$

The most common source of positive correlation: all three pipeline components use LLMs from the same provider family, causing systematic tendencies (verbosity preference, formatting bias, positional sensitivity) to manifest consistently across G, S, and J.

Mitigation. Cross-provider pipeline design reduces ρ_{ij} toward zero, making the independence bound more accurate and V_{total} higher.

B.5 Worked Numerical Example

Cross-provider design: $V_{\text{total}} \leq 0.70 \times 0.75 \times 0.75 = 0.394$

Within-family design with metric mismatch: $V_{\text{total}} \leq 0.70 \times 0.75 \times 0.58 = 0.305$

An evaluation score reported to two decimal places from a pipeline in the 0.30–0.40 validity range is precise to a degree of resolution the underlying measurement cannot support.

B.6 Estimation Protocol

The practical estimation protocol for V_1, V_2, V_3, and V_{total} is presented in full as Section 9 in the main body. The steps in that section implement the

formal definitions in B.1–B.2 using tractable approximations: expert-rated sampling for V_1, ICC$(A, 1)$ between simulated and real user outcomes for V_2 (estimated separately per demographic group), and the structurally correct IRR metric from Figure 1 for V_3. The interpretation guideline ($V_{\text{total}} \leq 0.50$: no consequential decisions without independent validation; $V_{\text{total}} \leq 0.30$: pipeline redesign required) accompanies the protocol in Section 9.

Table A.5: Literature scan category summary. "Correct" = structurally valid metric with explicit rationale. "Failure mode" = dominant pattern among incorrect papers.

Cat.	Topic	Papers	Correct	Dominant failure mode
A	Major agentic benchmarks (automated grading)	6	0	No IRR; automated grader validity assumed
B	Automated grader validity studies	4	4	— (all correct; measuring the gap)
C	Benchmarks with formal IRR	4	4	— (all correct)
D	LLM-as-a-Judge studies	6	0	Cohen's κ for multi-model panels
E	Benchmarks with structural metric mismatch	7	0	Cohen's κ for ordinal rubrics; % only
F	Long-horizon and capability benchmarks	6	0	No metric or % agreement only
G	Safety, RLHF, and preference evaluation	6	0	Pearson/ELO for ordinal IRR problems
H	Recent 2025–2026 evaluation papers	5	2	Metric stated; structural rationale absent
I	Safety-critical IRR failures	11	0*	Raw % agreement; no metric; wrong metric
Total		**55**	**10 (18%)**	

* Category I includes one paper (Jafari et al. 35) that uses the correct metrics but reports catastrophically low values (ICC = 0.087–0.295, $\alpha = -0.203$).