BenchBrowser: Retrieving Evidence for Evaluating Benchmark Validity

Harshita Diddee [1] **Gregory Yauney** **Swabha Swayamdipta** [2] **Daphne Ippolito** [1]

Abstract

Do language model benchmarks actually measure what practitioners intend them to? High-level metadata is too coarse to convey the granular reality of benchmarks: a "poetry" benchmark may never test for haikus, while "instruction-following" benchmarks will often test for an arbitrary mix of skills. This opacity makes verifying alignment with practitioner goals a laborious process, risking an illusion of competence even when models fail on untested facets of user interests. We introduce BENCHBROWSER, a retriever that surfaces evaluation items relevant to natural-language use cases across 20+ benchmark suites. Validated by a human study confirming high retrieval precision, BENCHBROWSER generates evidence to help practitioners diagnose low *content validity* (narrow coverage of a capability's facets) and low *convergent validity* (lack of stable rankings when measuring the same capability). BENCHBROWSER helps quantify a critical gap between practitioner intent and what benchmarks actually test. The tool and its code are publicly available.

1. Introduction

Practitioners often operate under a perilous assumption: that public benchmarks act as reliable proxies for their specific use-cases. Yet benchmarks differ widely in both scope and granularity. For instance, in some benchmarks each example probes a different capability, as with InfoBench (Qin et al., 2024) and AlpacaEval (Dubois et al., 2024). But in others, all examples test the same targeted capability, as in GSM8K (Cobbe et al., 2021) and NutriBench (Hua et al., 2024). This heterogeneity in design can compromise the *validity* of our evaluations, i.e., the degree to which the performance of a model on a benchmark reflects its true competence in the capability of interest (Bean et al., 2025; Ye et al., 2025).

[1]Language Technologies Institute, Carnegie Mellon University, Pittsburgh (PA), United States [2]Thomas Lord Department of Computer Science, University of Southern California, Los Angeles (CA), United States. Correspondence to: Harshita Diddee <hdiddee@andrew.cmu.edu>.

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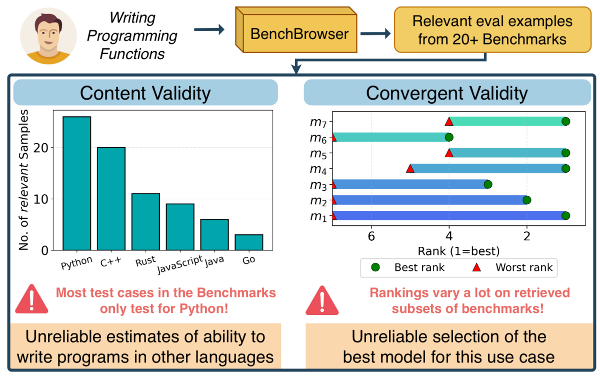

Figure 1. BENCHBROWSER retrieves use-case-relevant items from 20+ benchmarks to help diagnose validity failures. For a "Writing Programming Functions" use-case: Python-skewed coverage (left; content validity gap) and unstable model ranks across subsets (right; low convergent validity) make conclusions unreliable. m_1 to m_k are mid-sized decoder models. See Appendix A for details.

To confidently deploy a model, a practitioner must then answer a difficult question: *Does this benchmark provide comprehensive evidence of a model's competence for my specific use-case?*

Answering this question is difficult because the gap between a benchmark's description and its content creates two distinct validity pitfalls. First, evaluations can lack *content validity*: the extent to which the items in a test adequately represent the entire domain of the capability being measured. This can be compromised by *sparse coverage*, where a practitioner probing for a multi-faceted capability finds few relevant evaluation items in a broad evaluation suite, or by *scope-metadata mismatch*, where broad benchmark descriptions mislead. For example, BIG-bench's `Hindu_Knowledge` task (Srivastava et al., 2023) tests deity hierarchy but doesn't cover origins of spiritual texts—one of the many aspects that a practitioner might be interested in. Second, evaluations can often degrade *convergent validity*: this occurs when models evaluated on different benchmarks measuring the same capability produce conflicting rankings. A model might excel at "reasoning" on science questions (AI2 ARC, Clark et al. 2018) but could fail to "reason" about consistencies in narrative summarization (FlawedFictions, Ahuja et al. 2025). Confronted with these *divergent operationalizations* of reasoning competence, the practitioner may be left unsure of which signal to trust to choose a competent model for their use-case (Eriksson et al., 2025).

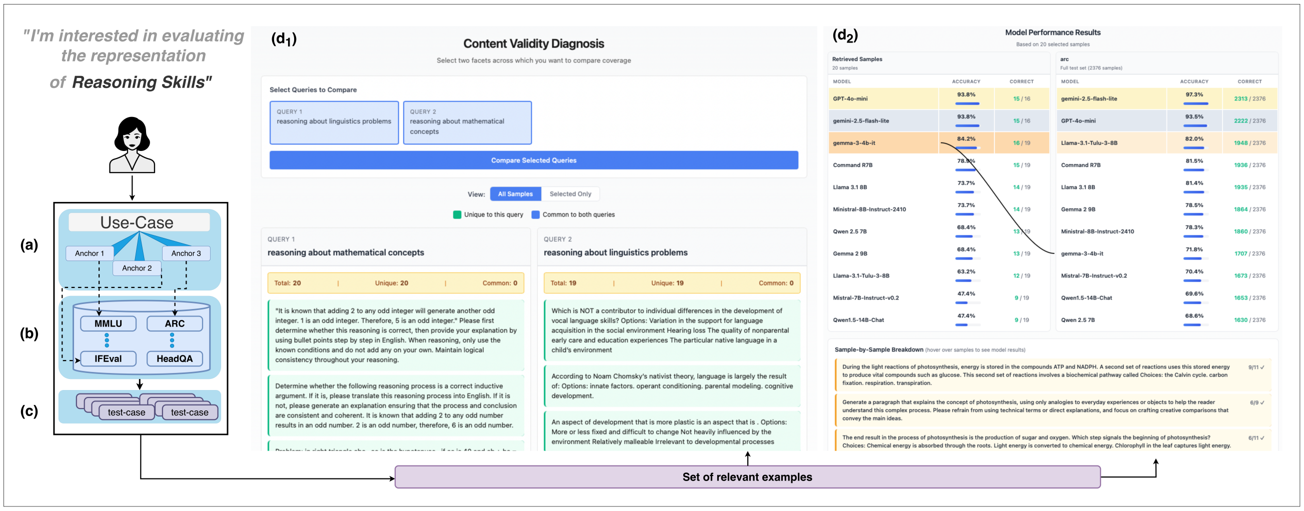

Figure 2. BENCHBROWSER pipeline: a practitioner submits a use-case of interest to BENCHBROWSER; the use-case is (a) rewritten into anchors for more diverse retrieval, which are then (b) embedded and used to retrieve (c) relevant examples from a suite of 20+ benchmarks. The retrieved evidence can be used by the practitioner in multiple ways (images show snapshots of BENCHBROWSER's website UI): (d_1) Comparing the representation of the use-case along different facets. For example, a user interested in assessing "reasoning skills" can observe the different ways (or lack thereof) in which reasoning competence is evaluated (linguistics fundamentals versus math problems) and (d_2) Compare the performance and stability of models evaluated on existing benchmarks (e.g., ARC AI2 Reasoning) for the task as well as subsets of the retrieved examples.

Diagnosing these failures is currently a prohibitively slow and expensive process. Existing methods such as manual auditing (Bean et al., 2025) or brittle keyword searches on dataset cards cannot scale to the tens of thousands of items in modern suites (Ma et al., 2025). Furthermore, validity is inherently stakeholder-dependent. One practitioner's definition of "coding ability" may focus on Python syntax, while another's might focus on Go concurrency. Static benchmark documentation cannot continuously capture these evolving definitions. To bridge this gap, we need tools that impose no assumptions about what a capability "should" mean, but instead empowers practitioners to retrieve operationalizations that match their own mental model (Wallach et al., 2025).

In this work, we introduce BENCHBROWSER, a retrieval tool that helps practitioners quickly gather evidence of how benchmarks represent their use-case (Figure 1 and §2). Practitioners can query BENCHBROWSER with a natural language use-case like "*ethical decision-making in AI art*" or "*organic chemistry*", and collect semantically related examples across tens of thousands of examples across popular benchmark suites in seconds. This speed enables interactive auditing, where practitioners can iteratively inspect which data points impact their use-case's evaluation.

We validate the reliability of the evidence generated by BENCHBROWSER in a user study with $N = 10$ NLP practitioners, finding that BENCHBROWSER returns examples that practitioners deem relevant for diverse use-cases (§4).

We then use this evidence to diagnose two risks to validity. First, we show how it can surface content validity failures, where minor facet changes expose stark representational disparities for use-case variants (§5). For instance, we show how swapping a single facet in a use-case like "programming in Python" to "programming in Go" results in a sharp drop in the number of available examples, demonstrating that competence estimates in "programming" may be disproportionately anchored to this specific language. Second, we show how evidence from BENCHBROWSER can uncover convergent validity failures when retrieved examples—even those judged as relevant by human annotators—induce model rankings that diverge from rankings derived from existing human-validated benchmarks for the same use-case (§6). For example, when evaluating models on their ability to answer questions about the Hindu religion, the top-ranked models on the multiple-choice BIG-bench `Hindu_Knowledge` task can drop to nearly the worst-ranked when instead evaluated on prompt-completion type examples from HellaSwag (Zellers et al., 2019).

Overall, BENCHBROWSER surfaces evidence for practitioners to directly assess validity rather than relying on rigid annotation schema, enabling interactive evaluation of benchmark validity for novel use-cases.

2. BENCHBROWSER

BENCHBROWSER is a tool that retrieves examples relevant to a natural-language use-case (Figure 2). Given a use-case and an integer k, we return the top-k most relevant examples from a large database spanning many benchmarks. Our method has three stages: (1) Anchor generation: rewrite the use-case into one or more retrieval anchors; (2) Retrieval: embed anchors and fetch nearest examples; (3) Filtering: an LLM judge removes obvious off-topic items.

2.1. Stage 1: Anchor Generation

Using practitioner use-cases directly for retrieval is insufficient: practitioner intents are often abstract (e.g., "instruction following") and benchmark examples are rarely annotated for such subjective, fine-grained intents. Moreover, use-cases omit format cues (e.g., multiple-choice vs. long-form) that are implicit in benchmark examples, and such mismatches can reduce retrieval effectiveness (Noël et al., 1997). Query-rewriting is a standard technique to bridge this gap between queries and document representations (Rizvi et al., 2004; Antonellis et al., 2008; Ma et al., 2023). We test four methods for generating rewritten *anchors* from queries, described below and summarized in Table 1. Full details for all methods are in Appendix B.

Original. As a no-rewrite baseline, we use the practitioner's use-case as an anchor.

Rephrasing. Practitioner use-cases can range from being multifaceted, broad topics to abstract capabilities which can attract a noisier set of examples with weak relevance (Krovetz & Croft, 1992; Chen et al., 2013). To make the anchors for such use-cases more specific, we first ask `gpt-5-mini-2025-08-07` to determine whether the practitioner's use-case is best interpreted as a topic or a capability. If it is a topic, the model decomposes it into $n = 3$ related sub-topics; if it is a capability, the model lists $n = 3$ skills needed to perform tasks associated with that capability.

Example Synthesis. To overcome the lack of format cues in a use-case, we ask `gpt-5-mini-2025-08-07` to generate $n = 3$ synthetic examples that would be helpful in assessing the competence of AI models for the practitioner's use-case. We explicitly guide the model to generate such examples in diverse formats informed by our audit of the formats in which examples are represented in our benchmark database (long-form QA, MCQ, fill-in-the-blank). This rewrite aims to maximize the coverage of retrieved examples by using an anchor that is in-distribution with the forms in the database.

Shorthand. Most benchmarks do not include examples that explicitly include a natural language description of the capability or topic that the example is testing. For example, WinoGrande (Sakaguchi et al., 2020) examples do not explicitly state that they are testing "pronoun resolution". Since explicit annotations which include the practitioner's use-case can provide a stronger chance of alignment, we rewrite the query into a bounded-vocabulary *shorthand* representation which includes explicit annotation for these signals in the following form: $\langle\text{skill}\rangle$ & $\langle\text{key}_1\rangle$ & $\langle\text{key}_2\rangle$. Each Shorthand anchor includes the primary skill tested in the example in addition to most salient topical cues to avoid losing nuanced information from the example. Unlike other anchor methods that query raw examples, we first convert all examples in the benchmark database to the same Shorthand form and run retrieval in this space. This yields (a) a rewrite of the practitioner use-case into a vocabulary that is more in-distribution to the benchmark database and (b) embeddings where examples with the same skill or related skills cluster more tightly due to explicit skill annotations. We implement this cost-efficiently by finetuning a `Llama-3-8B-Instruct` model to rewrite the database at a nominal one-time cost.

Table 1. We compare four methods for producing retrieval anchors. Here we show how each handles an example practitioner use-case: "*understanding geopolitical tensions in Southeast Asia*".

Original: understanding geopolitical tensions in Southeast Asia
Rephrasing: South China Sea territorial disputes and maritime tensions • ASEAN diplomacy, regional security mechanisms, and conflict prevention • US–China strategic competition
Example Synthesis: Discuss the strategic factors driving contemporary geopolitical tensions in Southeast Asia • MCQ: Which factor most significantly contributes to territorial disputes in the South China Sea? [...] • Fill in the blank: ASEAN's principle of ________ often limits collective action [...]
Shorthand: geopolitical_analysis & southeast_asia & international_relations

2.2. Stage 2: Semantic Retrieval

We generate semantic embeddings for query anchors using an in-context learning (ICL) embedding model, `baai/bge-en-icl` (Xiao et al., 2024). We chose this model for its performance and speed after benchmarking a diverse set of embedding models, a neural re-ranker, an LLM embedder specifically designed for diverse RAG applications, and this ICL embedding model (details in Appendix C). Following this, we run an efficient similarity search using Faiss (Douze et al., 2024) and retrieve the top-k nearest neighbor examples for each anchor. We then combine and sort these examples by their similarity score and pick the top-k for the final filtering stage. We also compare to BM25, a classical lexical retrieval method (Robertson et al., 2009) and random retrieval to quantify gains using semantic retrieval.

2.3. Stage 3: Filtering

We then use an LLM judge (`gpt-5-mini-2025-08-07`) to further filter for relevance. The prompt for the LLM judge is designed to assess if the examples returned by BenchBrowser are related to the intent of the user's use-case (details in Appendix D). The judge assesses relevance along three dimensions: (a) the extent of topical overlap between the example and the use-case; (b) the degree of skill overlap between the competencies required to solve the example and those required for the use-case; and (c) whether success on the example provides credible evidence of competence with respect to the use-case. This step aims to eliminate only the most obviously unrelated examples and not to outright select the relevant examples, as the latter is a non-trivial aspect to judge and is ultimately the practitioner's prerogative.

Table 2. Use-case query sets for evaluating the retrieval quality of BenchBrowser. Automatically synthesized sets derive from benchmarks; manually curated sets are hand-crafted for validation.

Name	Origin	N	Examples
	Automatically synthesized		
Topics	BBH, MMLU	58	Marketing, Law, HS Maths
Skills	Clio	50	creative writing, navigation
Applications	Clio	50	Tips for Winter Gardening
	Manually curated		
Known Valid.	LM Eval	20	Riddles, Calorie Counting
Novel	Crowdsourced	21	Matplotlib for Research

3. Evaluation Setup

We evaluate BenchBrowser on five categories of use-case queries, which we design to mirror the diverse intents of real-world practitioners (summarized in Table 2). Full details and prompts are in Appendix E. The categories are:

Topics. Captures broad domains (e.g., "sports", "law") central to traditional suites like MMLU (Hendrycks et al., 2021) to reflect the intent of practitioners interested in auditing coverage of specific knowledge areas. We use 58 subjects from BBH (Suzgun et al., 2023) and MMLU.

Skills. Targets capabilities spanning multiple domains, such as "reasoning" or "creative writing." These queries represent intents for practitioners who design skill-focused evaluations or who assess how well a single skill (e.g., "critical thinking") is represented across multiple benchmarks. To construct this set, we start from the Clio collection of real-world AI use-cases (Tamkin et al., 2024) and use `claude-sonnet-4-5-20250929` to synthesize natural language use-case descriptions phrased as broad skills.

Applications. Focuses on the highly contextual, concrete tasks typically sought from deployed LLMs (e.g., "studying for the SAT"). Unlike the abstract *skills* set, this set mirrors the specific user requests common in instruction-following benchmarks (Qin et al., 2024; Zhou et al., 2023; Wen et al., 2024). These are also derived from Clio via `claude-sonnet-4-5-20250929`.

Novel. Because practitioners may also wish to audit new use-cases that are sparsely represented or entirely absent in existing benchmarks, we include a *novel* set. We crowdsource 20 novel queries from $N = 9$ NLP practitioners (e.g., "creating Matplotlib visualizations") to construct this set.

Known Validation. To assess if retrieved examples correlate with performance on established benchmarks, we construct a set of use-cases where test sets specifically designed for that use-case already exist. For example, we pair the query "Using meal descriptions to measure calorie intake" with NutriBench, allowing us to validate if BenchBrowser retrieves items that predict ground-truth performance. We call these *gold* test sets (see Appendix E.4 for the mapping from use-cases to tasks).

Automatic evaluation metrics. We grade relevance with `gpt-5-mini-2025-08-07`, labeling retrieved items *Relevant*, *Partially Relevant*, or *Irrelevant*. When computing metrics, we count both *Relevant* and *Partially Relevant* items as positive because the goal of BenchBrowser is to surface broadly relevant examples for multiple plausible interpretations of the query rather than enforcing a single definition of relevance. We report two precision metrics to separate retrieval quality from the effects of filtering: (a) Prec $_{\text{anchor}}@k$, which evaluates the unfiltered examples returned by each anchor method, and (b) Prec $_{\text{sys}}@k$, which evaluates the final examples returned by the full system. Prec $_{\text{sys}}@k$ is intended to track practitioner utility and hence, utilizes a stricter judge prompt (Appendix E.6), while Prec $_{\text{anchor}}@k$ is used only to select the anchor method for the final system's design. Additionally, we report ranking quality of the final retrieved set using NDCG$@k$. Finally, we also measure Recall$@k$ for the *topics* set; to avoid underestimating recall from relevant examples outside the designated gold set, we use a restricted 28k-example benchmark database containing only gold test sets for use-cases in the *topics* set. For all other sets, all metrics are reported over a database of 70k-examples (listed in Appendix E.1).

4. BenchBrowser Finds Relevant Examples

Table 3 compares how well different anchoring strategies retrieve relevant examples. Across both *topics* set and *skills* set, rewriting-based anchors (Rephrasing, Shorthand, Example Synthesis) substantially outperform non-neural baselines (Random, BM25) and the direct Original baseline, with gains of more than 10 points in Prec $_{\text{anchor}}@k$ and Prec $_{\text{sys}}@k$. Example Synthesis is consistently the best or second-best method on all three sets, and Shorthand performs comparably. NDCG$@k$ also improves, showing that rewriting im-

Table 3. Unified BENCHBROWSER retrieval performance with system and anchor generation evaluation metrics at $k = 20$. Example Synthesis and Shorthand have high Prec $_{\text{sys}}$@k. All methods have the least precision on the *applications* set, potentially due to the contextual nature of its queries. Best values per column are highlighted in **green**, with the second-best in gold. The horizontal line separates neural embedding-based methods (lower) from non-neural methods (upper).

	Topics				Skills			Applications		
Method	Prec $_{\text{sys}}$@k	Prec $_{\text{anchor}}$@k	NDCG@k	Recall@k	Prec $_{\text{sys}}$@k	Prec $_{\text{anchor}}$@k	NDCG@k	Prec $_{\text{sys}}$@k	Prec $_{\text{anchor}}$@k	NDCG@k
Random	0.304	0.478	0.702	0.029	0.310	0.396	0.628	0.045	0.051	0.231
BM25	0.529	0.748	0.850	0.191	0.654	0.780	0.823	0.320	0.309	0.648
Original	0.763	0.887	0.921	0.350	0.768	0.923	0.911	**0.572**	**0.722**	**0.870**
Rephrasing	0.812	0.896	0.923	0.345	0.775	**0.933**	0.908	0.563	0.687	0.849
Shorthand	0.884	**0.970**	0.955	0.414	0.815	0.925	0.916	**0.572**	0.611	0.808
Example Synthesis	**0.890**	0.968	**0.971**	**0.423**	**0.837**	0.926	**0.937**	0.560	0.687	0.869

proves the entire ranking of retrieved examples, not just the first few items. Recall@k is conservative for all methods on the *topics* set, which we attribute to the presence of multiple highly overlapping use-cases in this set (e.g., high-school vs. college chemistry). Such overlap hurts recall for a large proportion of use-cases because examples relevant to one use-case are counted as misses for a highly-related use-case.

Shorthand and Example Synthesis achieve the highest precision across diverse use-case sets in Table 3. We conduct two tie-breaking experiments: (i) we compare how well model performances on retrieved examples preserve the ranking of models on the gold test sets for the *topics* set (Appendix F.1). (ii) For *application* and *skills* set use-cases which do not have gold test sets, we measure the extent to which each method's retrieved examples overlap with the union of all examples judged relevant by any anchoring method (Appendix F.2). Example Synthesis performs best in both experiments; we therefore use Example Synthesis as the anchor generation method in all subsequent experiments.

We also study Prec $_{\text{sys}}$@k for Example Synthesis as we vary k (Appendix F.3). It is largely stable for the *topics* set and *skills* set as k grows, suggesting BENCHBROWSER can return larger pools of examples without rapidly flooding practitioners with irrelevant items. The *applications* set observes a sharper degradation in Prec $_{\text{sys}}$@k as k grows potentially due to the paucity of examples that are relevant to the highly contextual, nuanced use-cases in this set.

To also validate these judgments under another indepenent LLM judge, we further replicate these evaluations with `gemini-2.5-flash-lite` in Appendix G and observe strong, statistically significant correlations ($\rho = 0.795$ for Prec $_{\text{sys}}$@k) against our judge (Table 12 in the appendix).

BENCHBROWSER is fast. We report end-to-end user-perceived latency for the public BENCHBROWSER website over 210 requests (0 failures). Latency of the end-to-end warm-started system is 5.28 ± 1.48 s (mean and stdev). Detailed distribution statistics (tail, median and per-stage costs) and hosting infrastructure are reported in Appendix H. BENCHBROWSER enables iterative, diverse audits of practitioner use-cases at interactive speeds.

4.1. Human Evaluation of BENCHBROWSER

We recruit $N = 10$ NLP practitioners to annotate retrieval relevance using the same three-point scale as our LLM judge. For all the use-cases we consider, every example in the top-20 examples retrieved by BENCHBROWSER is annotated by three annotators. To rigorously audit the judge, we employ two sampling strategies to select the use-cases for annotation. First, we ask annotators to annotate examples for <u>all</u> use-cases in the *novel* set and *known validation* set to establish a baseline of correlation between automatic and human judges. Through this we collect $n =$ 2,460 relevance judgments. Second, for the *topics* set, *skills* set and *applications* set, we take the 10 use-cases with the highest precision under the LLM judge and have their relevance judgments verified by the annotators. This setup allows us to determine if an automatic judge's high scores reflect genuine relevance or merely the systematic leniency often observed in LLM-based evaluation (Jain et al., 2025; Arabzadeh & Clarke, 2025). Through this we collect $n =$ 1,800 relevance judgments. In all, we collect $n =$ 4,260 judgments across 71 use-cases. Full details are in Appendix I.

Alignment between automatic and human judgments. We calculate mean relevance judgments by counting *Relevant* as 1, *Partially Relevant* as 0.5, and *Irrelevant* as 0. Table 4 compares human relevance judgments with automatic Prec $_{\text{sys}}$@k via paired t-tests on mean relevance (bias) and Spearman's ρ (rank agreement). For the *topics*, *skills*, and *applications* sets, mean relevance is closely matched (mean difference $\leq$0.1), indicating that higher automatic Prec $_{\text{sys}}$@k generally tracks higher human precision. Rank agreement is weak-to-moderate across sets ($\rho \in [0.15, 0.50]$). The *novel* set is moderately aligned (mean 0.67 vs. 0.70). The *known validation* set is the main outlier (mean gap 0.30); error analysis traces this to a few use-cases with simplified queries too broad for the esoteric suites they target (Appendix I.1). Overall, automatic judgments provide a reasonably calibrated proxy for practitioner-assessed precision, while occasionally misestimating absolute relevance for broad, domain-specific use-cases.

Inter-annotator agreement (IAA) and subjectivity. IAA for the relevance judgments ranges from slight to moderate (Table 4). Error analysis finds that disagreements are between *Relevant* vs. *Partially Relevant* rather than *Relevant* vs. *Irrelevant* (Figure 10 in Appendix I.1). This pattern suggests calibration differences rather than fundamental disagreement, consistent with prior work on the subjectivity of relevance labels (Adams et al., 2023; McDonnell et al., 2016; Shirani et al., 2021; El Dehaibi & MacDonald, 2020).

5. Measuring Content Validity

Having established that BenchBrowser retrieves relevant examples, we next demonstrate how BenchBrowser can help practitioners collect evidence for measuring *content validity*, the extent to which an evaluation comprehensively represents all facets of the ability it claims to measure (Strauss & Smith, 2009; Freiesleben & Zezulka, 2025; Wallach et al., 2025). In our setting, this means asking whether examples across benchmarks adequately cover both the breadth (different facets) and depth (difficulty or context) of capabilities that practitioners care about. Establishing the content validity of an assessment is crucial. If benchmarks collectively instantiate a capability narrowly, important facets can be left untested, and aggregate scores can systematically distort a model's competence. If examples for "code generation" mostly involve short Python snippets, then performance on those examples may tell us little about how a model fares on code generation for other languages.

For a practitioner, one way to assess content validity is to evaluate a capability across different facets: hold the broad capability fixed (e.g., "code generation") while varying a single facet (e.g., programming language) and checking benchmark coverage (Landsheer & Boeije, 2010). BenchBrowser makes coverage gaps directly inspectable by retrieving the relevant examples for each facet.

Experimental setup. To evaluate BenchBrowser in this setting, we need use-cases paired with relevant facets and explicit axes of variation. We use the *skills* set to seed `claude-sonnet-4-5-20250929` to produce 20 broad capabilities and six closely related variants for each, differing along a single, interpretable axis (120 total). For instance, "trip planning" varies by destination type (e.g., domestic vs. international). We manually verify that variants are lexically separable, ensuring that a purely lexical baseline could, in principle, distinguish them. We call the variants of a capability a *skill family*. Table 5 gives two example skill families.

Table 4. Mean relevance of retrieved examples by human annotators vs. the LLM judge across use-case query sets. Superscripts indicate significance: † $p < 0.05$; ‡ $p < 0.0001$. Markers on Mean_{LLM} are significance markers for paired t-test for the means. ρ is Spearman's rho and κ is Fleiss's kappa.

Use-case	Mean_{H}	Mean_{LLM}	ρ	κ
Topics	0.96	$0.92^{\ddagger}$	$0.15^{\dagger}$	0.10
Skills	0.80	$0.91^{\ddagger}$	$0.17^{\dagger}$	0.11
Applications	0.74	0.75	$0.50^{\ddagger}$	0.29
Known Valid.	0.58	$0.88^{\ddagger}$	$0.34^{\ddagger}$	0.29
Novel	0.70	0.67	$0.27^{\ddagger}$	0.13

Evaluation. Estimating content validity in practice is expensive because both (i) which facets matter and (ii) what counts as "relevant" are stakeholder-dependent (Strauss & Smith, 2009). Moreover, variants within a fixed skill can often retrieve overlapping examples (e.g., items relevant to "Java programming" may be partially relevant to "Python programming"), forcing annotators to repeatedly judge near-duplicates across facets—an especially high cognitive burden (Cartwright et al., 2019; Henley & Piorkowski, 2024). Accordingly, we use the LLM judge as a proxy for relevance judgments. We compute the percentage of examples retrieved by BenchBrowser deemed *Relevant* by the LLM judge (excluding *Partially Relevant* examples). Large differences between a skill family's variants signals low content validity (Landsheer & Boeije, 2010).

Findings. Figure 3 summarizes the percentage of *Relevant* examples for the best- and worst- represented facet of each skill family. Closely related use-case variations yield very different numbers of *Relevant* examples, indicating sizable spread in facet-level coverage in each skill family. Table 5 gives examples of this spread: dominant formulations such as "write a Python function" or "write a fantasy story" are associated with high yields of *Relevant* examples, whereas equally legitimate but less conventional facets such as "write code in Go" or "write a historical fiction story" often drop precipitously, despite testing the same underlying capability. Appendix J finds the same trends with a lexical BM25 baseline, suggesting that our findings are due to benchmark composition rather than an artifact of the semantic retriever.

The disparities uncovered here have direct consequences for how practitioners interpret benchmark scores; for example, if a benchmark reports a single number for debugging code and almost all retrieved examples concentrate on a single facet (e.g. syntax errors), then strong performance may say little about the model's behavior on other deployment-critical variants (e.g., security vulnerability or memory leaks). BenchBrowser can help practitioners uncover such disparities and can ground the construction of new benchmarks across under-explored facets of that skill.

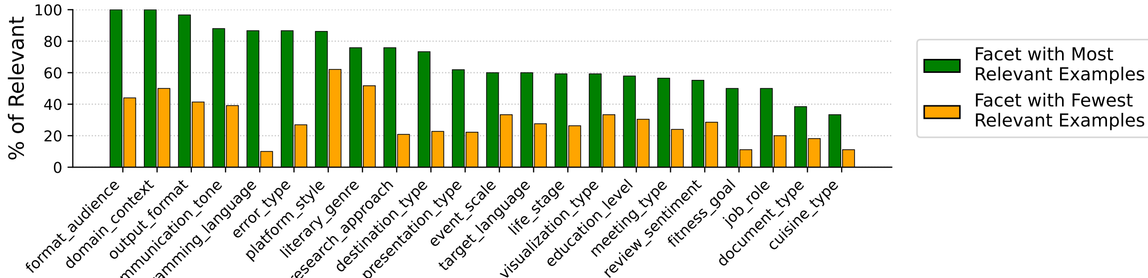

Figure 3. Variation in the percentage of *Relevant* examples across different facets of the same broad use-case. For each skill family, we show the facet for which the highest and lowest number of examples are retrieved. Most capabilities show sharp disparities: certain facets are heavily over-represented in our database of 70k benchmark examples, while others are nearly absent from benchmarks.

Table 5. Different facets of the same skill family can have very different Prec $_{\text{sys}}@k$ when using BENCHBROWSER with Example Synthesis anchors and the LLM judge (relevant examples only). Most skill families show substantial spread: some variants (green) have many *Relevant* examples, while others (red) are barely represented. For complete results for all skill families, see Appendix J.

Code generation **Programming language** "Write a function in . . ."		**Creative writing** **Literary genre** "Write a . . . story"	
Facet	Prec $_{\text{sys}}@k$	Facet	Prec $_{\text{sys}}@k$
Python	0.867	fantasy	0.759
C++	0.741	science fiction	0.679
Rust	0.478	mystery	0.654
JavaScript	0.346	romance	0.625
Java	0.286	horror	0.583
Go	0.100	historical fiction	0.517

6. Measuring Convergent Validity

With the rapid proliferation of benchmarks, it is now common to find multiple benchmarks that ostensibly evaluate the same capability (Sun et al., 2025; Zhang et al., 2025). This invites an implicit but often untested assumption: if two benchmarks truly measure the same underlying skill, they should lead to similar conclusions about which models are better for that skill. *Convergent validity* formalizes this expectation: model rankings on benchmarks that purport to test the same skill should be correlated (Strauss & Smith, 2009; Bean et al., 2025). When this assumption fails, benchmark results become hard to trust or act on, yielding contradictory takeaways (Diddee & Ippolito, 2024; Zhou et al., 2026). BENCHBROWSER helps practitioners diagnose these failures of convergence by retrieving relevant examples that enable the construction of alternate, use-case specific test sets. Practitioners can evaluate models on these alternate test sets and check whether specialized benchmark-derived rankings are preserved on them. As we demonstrate in this section, this exercise can reveal when "same capability" benchmarks do (or *do not*) support transferable conclusions.

Experimental setup. We use the *known validation* set of use-cases (§3). For each use-case u, we evaluate $n = 8$ models on (i) its gold test set G_u and (ii) the retrieved set R_u. For each set, we score examples using the source-benchmark metric, aggregate to model-level scores, and induce a ranking. Model and evaluation details are in Appendix K. For each benchmark, we compute the per-sample "success" of the model by using its designated success scoring metric (win rate, accuracy, exact match, etc.) and aggregate the per-sample "success" under the designated benchmark to compute the models' performance across the retrieved examples (see Appendix K for detailed metric computation). Then, we measure rank agreement via two forms of Kendall's τ (using the mean across 50 Monte Carlo trials):

1. $\tau_{\text{ret}}(u)$: shows the agreement between the ranking induced by R_u and a gold ranking estimated at matched cardinality. In each trial, for each model m, we subsample $|R_u|$ examples from G_u, induce a ranking, and report its mean Kendall's τ with the ranking from R_u.
2. $\tau_{\text{gold}}(u)$: a baseline of the correlation between model rankings on the full gold set with model rankings on a smaller subset of that gold set. Let k be the (average) retrieved size for u; in each trial we subsample k examples from G_u, induce a ranking, and compute Kendall's τ against the ranking on the full gold set. This accounts for the decrease in the maximum achievable Kendall's τ for smaller sets (Yauney et al., 2025).[1]

We define *rank divergence* as $\Delta(u) = \tau_{\text{gold}}(u) - \tau_{\text{ret}}(u)$. Small Δ indicates retrieved examples preserve rankings, while large Δ reflects divergence beyond finite-sample effects, indicative of low convergent validity.

Evaluation. Because convergence claims require that retrieved sets reflect the intended use-case, we strictly restrict

[1]Appendix K reports correlations between same-cardinality subsets as a sanity check.

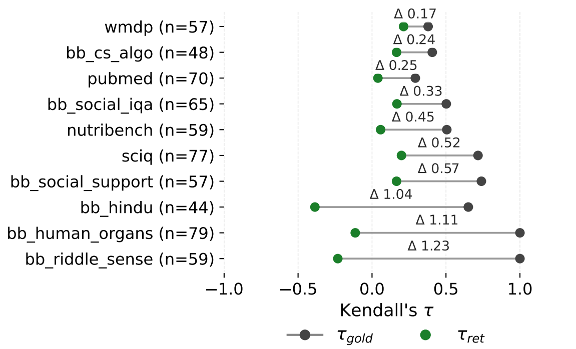

Figure 4. Kendall's τ correlations of model rankings on gold test sets vs. retrieved sets for *known validation* set use-cases with mean human relevance ≥ 0.5. Wider bars correspond to greater rank divergence $\Delta = \tau_{\text{gold}} - \tau_{\text{ret}}$. n: number of retrieved examples.

analysis to use-cases whose retrieved examples are verified as relevant during our human evaluation. Concretely, we report results only for use-cases where the mean human relevance over examples is at least *Partially Relevant*.

Findings. Figure 4 shows rank divergence varies sharply across use-cases Some exhibit low divergence, indicating that the retrieved sets preserve benchmark-induced model comparisons. Others show large divergence, even including ranking inversions, suggesting that high topical relevance may not be sufficient for establishing convergent validity.

Manual inspection of the retrieved examples suggests three recurring drivers of ranking inversions (Table 6). Collectively, they reflect two practical ways in which practitioners are likely to encounter low convergent validity.

First, retrieved examples often vary the *format or scoring rubric* of the evaluation (e.g., short-form factual questions vs. longer, constraint-heavy, or open-ended prompts). Hence, even when an example's topic matches the use-case, such shifts change what it means to succeed (Root Cause 1 in Table 6). This creates a measurement mismatch: different metric and rubrics can systematically alter observed model rankings for the same nominal capability due to inherent differences in what the measurements reward (Campbell & Fiske, 1959). For instance, win-rate is a popular metric for generative tasks; it is known to be sensitive to calibration inconsistencies (Dubois et al., 2024; Gao et al., 2024b; Zheng et al., 2025), which can propagate into rank differences and ultimately yield divergent conclusions.

Second, for over-represented capabilities with dedicated benchmarks, retrieval may concentrate on one source whose *operationalization* of the advertised skill is narrower or differently scoped (e.g., two "puzzle-solving" benchmarks emphasizing distinct task families). In this case, benchmarks that appear to target the same capability may in fact measure different underlying competencies, yielding *construct non-equivalence* (Root Cause 2 in Table 6). Scoping differences can also appear as a shift from definitional recall to applying a concept in context, effectively redefining the construct under evaluation (Root Cause 3 in Table 6).

Table 6. Root causes of ranking inversions in BENCHBROWSER; gold highlight indicates example is from the gold set, and blue indicates it is from the retrieved set.

Root Cause 1: Rubric Shift ***Example Query:*** *"Hindu Religion"*
HellaSwag: Philosophy and Religion: How to learn about ancient hinduism ...
BIG-bench Hard (Hindu Knowledge): Which of the following gods are associated with a flying elephant
Root Cause 2: Divergent Operationalization ***Example Query:*** *"Solving Puzzles"*
BIG-bench Hard (Colored Objects): On the desk, you see items in a row: a gold textbook, a purple puzzle, a teal necklace, and a silver pencil. How many non-gold items do you see to the right of the pencil?
AlpacaEval: Hi, I'm trying to solve a crossword puzzle, but I've never done one of these before. Can you help me out?
Root Cause 3: Context Shift (Recall → Applied) ***Example Query:*** *"Writing Programs"*
HumanEval: Check if in given list of numbers, are any two numbers closer to each other than given threshold.
InfoBench: ... you've came up with an algorithm for solving the coding question your interviewer gave you but ...

BENCHBROWSER helps diagnose validity threats by generating clear evidence for an observed rank instability: when benchmarks expected to measuring the same "capability" produce divergent model rankings, surfacing the retrieved examples and their source benchmarks can help practitioners manually inspect whether the inconsistency reflects construct non-equivalence or divergent operationalization.

7. Related Work

Customizable benchmark selection and construction. Customizable benchmarking also addresses the sparse coverage and narrow operationalizations of existing benchmarks. These approaches synthetically generate test sets that tailor evaluation to a practitioner's task or domain from a seed corpus (Shashidhar et al., 2025) or in a zero-shot manner (Pombal et al., 2025). Others re-structure existing evaluations to enable targeted subset selection, e.g., capability meta-descriptions (Zeng et al., 2025), description-driven retrieval of difficult items (Li et al., 2025), and large benchmark repositories with fixed taxonomies (Kim et al., 2025). In contrast, we retrieve and organize *vetted, labeled* benchmark items for a practitioner's free-form intent, avoiding both synthetic labels and reliance on predefined schemas. This preserves the manual supervision that was applied to the test set curation originally, reducing sensitivity to artifacts from LLM-generated evaluations (Huang et al., 2024; Chen et al., 2024). This choice aligns with evidence-centered benchmark design, which posits that capability claims should be grounded in item-level evidence (Liu et al., 2024).

Infrastructure for validity-oriented evaluation. Benchmark audits emphasize that modern evaluation practice often under-specifies validity (Dietz et al., 2025; Reuel-Lamparth et al., 2024). Yet establishing validity typically requires construct-sensitive measurement tools (Salaudeen et al., 2025; Ye et al., 2025) and expensive evidence collection: expert annotation (Bean et al., 2025), explicit enumeration of operationalizations (Wallach et al., 2025), and stakeholder-aware construct definitions (Alaa et al., 2025). While psychometrics-inspired approaches, such as IRT-based fluid benchmarking (Hofmann et al., 2025), improve test composition by modeling item properties, practitioners lack scalable support for assembling and inspecting the item-level evidence needed to surface missing coverage and conflicting operationalizations, a gap our retrieval-based interface fills.

8. Discussion and Conclusion

We present BENCHBROWSER, a meta-evaluation tool for *cost-effective, transparent* validity assessment of LLM benchmarks under practitioner-defined use-cases. Rather than treating benchmark scores as self-explanatory, BENCHBROWSER surfaces the underlying items that instantiate a use-case, enabling practitioners to directly audit *what* is being measured and *how*. Human evaluations, consistent with LLM-judge results, show that BENCHBROWSER retrieves relevant evidence for assessing coverage and representation. By making sources, rubrics, and framing differences inspectable, BENCHBROWSER supports rapid diagnosis of low content validity and convergent validity.

BENCHBROWSER helps audit the validity of LLM evaluations by surfacing evidence *across* benchmarks rather than relying on any single suite (Veuthey et al., 2025; Son et al., 2024; Chern et al., 2024). Because capabilities admit multiple operationalizations, this cross-benchmark view makes it practical to inspect the most atomic evidence for a use-case —the evaluation items themselves—revealing which subsets, outliers, or framings drive ranking inversions and where facets are underrepresented. In doing so, BENCHBROWSER supports more stable, well-contextualized conclusions and directly guides where new benchmarks (or slices of existing ones) should focus to better measure the intended capability.

Impact Statement

Systems are only as good as the yardsticks we measure them with. We take a first step toward practitioner-centered validation by offering a fast, low-cost, end-to-end workflow for auditing benchmark validity under user-specified intent. Rather than treating disagreement across benchmarks as noise, we expose the underlying measurement choices—rubrics, framing, and construct scope—that can produce conflicting rankings, allowing practitioners to interpret competence claims more cautiously and to diagnose where (and why) conclusions diverge.

Our work contributes a core ingredient for trustworthy benchmarking by making capability claims auditable (Cheng et al., 2025). When user-centric capabilities—rather than metric dominance on traditional tasks—drive utility, practitioners need direct access to the evaluation items and rubrics that underwrite system-selection decisions. We do not anticipate any negative ethical impacts of this work.

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A. Motivation Experiment Details for Figure 1

We evaluate a set of seven instruction-tuned models, denoted as $\mathcal{M} = \{m_i\}_{i=1}^{7}$. Specifically, we define $m_1, \ldots, m_7$ as `Qwen2.5-7B-Instruct` (Yang et al., 2024), `Ministral-8B-Instruct-2410`, `Llama-3.1-Tulu-3-8B` (Lambert et al., 2024), `c4ai-command-r7b-12-2024` (Cohere et al., 2025), `DeepSeek-R1-Distill-Llama-8B` (Guo et al., 2025), `Mistral-7B-Instruct-v0.2` (Jiang et al., 2023), and `Meta-Llama-3-8B-Instruct` (Grattafiori et al., 2024), respectively.

B. Anchor Generation Methods

B.1. Shorthand Anchor

Many benchmarks (e.g., WINOGRANDE, BIG-bench) lack fine-grained capability annotations, making it difficult to identify which skills are being tested at the datapoint level. To ensure semantic proximity among functionally similar samples across diverse formats, we design structured textual representations called *shorthands*.

Shorthand format. A shorthand follows the template $\langle\text{skill}\rangle$ & $\langle\text{key}_1\rangle$ & $\langle\text{key}_2\rangle$ & $\langle\text{key}_3\rangle$, where $\langle\text{skill}\rangle$ denotes the primary cognitive ability required, and keys encode salient lexical or contextual attributes that enhance retrieval precision.

Pipeline.

1. **Seed Data construction.** To train a model to efficiently translate our index into the Shorthand representation, we needed to generate a small, high quality translation dataset where a example corresponds to its equivalent shorthand. To achieve a diverse set of examples to act as this set - we first embed 10 random subsets of our entire index using the `BAAI bge-en-icl embedding model`. Each subset contains 1000 examples (without replacement). We, then, run t-SNE on each subset separately and k-means per batch (with $k = 10$). Finally, we pick, both, the $n = 250$ nearest-to-centroid and $n = 250$ farthest-from-centroid examples in each cluster. Choosing these allows us to cover both, the prototypical and well represented tasks as well as the least represented tasks for the initial seed data construction. This allows us to generate a dataset consisting of 5000 examples.

2. **Finetuning Data Synthesis.** We then use `gpt-5-mini-2025-08-07` to produce shorthands for the 5000 examples. We use the following prompt for this conversion.

> Shorthand Generation Prompt ”””You are tasked with converting natural language text into a concise shorthand format which can be used to identify other datapoints having similar capabilities. The format for creating this shorthand is: $< skill >$ & <key1> & <key2> & <key3> where:
> - <skill> encodes the primary cognitive/technical ability needed to accomplish the task underlying the text. Ask yourself: ”What is the primary cognitive/technical ability needed to accomplish the task underlying the text?” Some examples (NOT EXHAUSTIVE): creative_writing, coding, equation_solving, coreference_resolution, strategy_planning, logical_reasoning, factual_recall, reading_comprehension, scientific_visualization, graphics, social_interaction, movie_trivia, statistical_analysis, etc.
> - <key1>,<key2>, <key3> are 1-3 SPECIFIC and DISTINCTIVE features that maximize retrieval precision; For adding each feature/key, ask yourself: - ”What are the specific document types, topics, and/or entities in this text” that need to be preserved for retrieving similar datapoints? - Is there any specific topic underlying being asked by the user query: ”machine learning” ”statistics” ”poetry” etc - include that as a key. - Is there any specific output format mentioned in the query: ”research papers”, ”clinical cases”, ”financial reports” ”emails” etc - include that as a key. - Is the the query about a specific person, organization, or entity - include that as a key. If there are multiple entities, or placeholders like ”X”, ”Y”, ”Z” - include the broad category of the entity (e.g., ”actor”, ”writer”, ”sportsperson” ”female”) and number of entitites as a key. - Is the user looking for a specific task like: ”sorting_ascending”, ”counting_3_objects”, ”implementing_neural_network”- include that as a key. - If the text includes a mathematical formula: include the concept type of the formula (e.g., ”differential_equations”, ”linear_algebra”, ”probability_distribution”) - If the user has only specificed a single topic - use that as the key. DO NOT ADD A SKILL UNLESS ITS SPECIFIED. - If the user has only specified a single skill/task - use that skill/task as the key. DO NOT ADD A DIFFERENT TASK UNLESS ITS SPECIFIED. - Use underscores for multi-word concepts (e.g., ”machine_learning”, ”social_skills”, ”new_york”) - Make sure that no key adds a new generic domain/skill/topic to the query. STRICTLY FOLLOW THIS RULE. - Separate each key with an ampersand.

- Return exactly 1 shorthand for a user query wrapped in XML tags: <shorthand¿¡/shorthand>. - IMPORTANT: Only include keys, if they add unique, searchable value. If there are no more distinctive features, USE FEWER KEYS rather than padding with generic terms.Your final outputs should be of the form: <shorthand¿ ¡/shorthand> Here are some examples of shorthands:

3. **Model Finetuning.** We then finetune `Llama-3-8B-Instruct` via on (example, Shorthand) pairs. We fine-tuned the base model using 4-bit quantization (QLoRA) with a maximum sequence length of 4096 tokens. Parameter Efficient Fine-Tuning (PEFT) was applied via Low-Rank Adaptation (LoRA) with a rank of $r = 64$, a scaling factor $\alpha = 32$, and a dropout rate of 0. The LoRA adapters targeted the query, key, value, output, gate, up, and down projection modules. Training was performed for 2 epochs using the 8-bit AdamW optimizer with a learning rate of 1×10^{-4} and a linear decay schedule with a warmup ratio of 0.03. We utilized a per-device batch size of 2 with 8 gradient accumulation steps, resulting in an effective batch size of 16. To ensure reproducibility, the random seed was set to 3407.

4. **Index translation.** We use our finetuned version of the model to translate all benchmark examples from our benchmark database into their respective shorthand notation. This step yields cost-efficient annotation, constrains the vocabulary, and enforces consistent skill verbalization across all the examples in the transformed benchmark database. Some examples from the final benchmark database are provided below:

ID	Test Case	Shorthand
mathqa	if x is a number such that $x^2 + 2x - 24 = 0$ and $x^2 - 5x + 4 = 0$, then $x =$? Options: a) 4, b) -4, c) -3, d) -6, e) 1	equation_solving & quadratic_equations & solution_id
triviaqa	Who invaded Europe from Mongolia and Turkey over 300 years, beginning in the 13th century?	factual_recall & histori-cal_figures & mongols
alpacaeval	Provide a formula for computing the nth term in the given sequence: 5, 14, 23, 32, 41, ...	equation_solving & sequence_formula & nth_term
arc_c	Scientists often change conclusions when new information becomes available. Which statement was changed? Options: Moon is in outer space / Plants make own food / Sun revolves around Earth	reading_comp & analysis & scientific_conclusions

Once this translated benchmark database is generated, we create the FAISS (Douze et al., 2024) index for this benchmark database to use for retrieval when Shorthand is used as an anchor generation strategy.

B.2. Anchor Generation Prompts

Rephrasing Baseline.

Segment Generation Prompt

”””Given a user query, create **three brief refinements** of that query that are appropriate for retrieving examples representing the interest highlighted in the query. Use the following rules:
- If the query is compositional, i.e., it contains multiple concepts, make different operationalizations of each concept. For example: if the query is ‘scientific visualizations for scientific papers‘ - each refinement should focus on a different sub-part of this query: like scientific visualizations, visualizations for scientific papers and tools for visualizations. - If the query is about some broad capability, enlist different sub-topics that test for that capability. For example: if the query is ‘trivia‘ - each refinement can include common topics included in trivia questions like ‘celebrity history‘, ‘geography‘, ‘world history‘, etc. - If the query is about some broad topic, enlist different related domains and sub-topics that are related to that topic. For example: if the query is ‘biology‘ - each refinement can include different related domains like ‘genetics‘, ‘ecology‘, ‘dermatology‘, etc. - If the query is about a specific task, generate different contextual forms of that task. For example: if the query is ‘Creative Writing‘ - each refinement can include different types of creative writing like ‘poetry‘, ‘short stories‘, ‘novels‘, etc. - If the query describes a scenario - ‘Cooking indian food‘ - list different activities that can be performed in that scenario. For example: ‘indian spices‘, ‘indian food recipes‘ and ‘indian desserts‘. - If the query is a proper Noun - ‘Harry Potter‘ - generate at least one query that highlights the broad task associated with that proper noun, for example: ‘fantasy literature‘ or ‘magic‘, and one that lists the most popular form of that proper noun, for

example, ‘Harry Potter Books‘. - If the query is a question - ‘What is the Capital of France ¿ - generate at least one query that highlights the topic of that question. For example: ‘Capitals of different countries‘, ‘Facts about France‘ and ‘Paris‘. - If it falls under neither of these categories, for example: ‘Can Elephants fly ¿ - generate queries that cover reasonable aspects of that query: For example: ‘Animals that can fly‘, ‘Elephant facts‘ and ‘Flying facts‘.
* Make sure that one refinement is not a subset of another refinement. * Make sure that at least one refinement highlights the broad domain of the query. For example, if the query is ‘chess‘ - one refinement should include ‘board games‘. * Avoid making overly complicated refinements by using simple words and phrases. * Focus on what would attract different types of relevant test cases or documents from existing test benchmarks. ALWAYS Return your responses in the following XML format; each refinement should be in a separate <refinement> tag: <refinements> ¡refinement¿ ¡/refinement¿ ¡/refinements¿ DO NOT INCLUDE ANY OTHER TEXT IN YOUR RESPONSE or any reasoning in your response. The user query is: query

Example Synthesis Baseline.

Testcase Generation Prompt
”””Given a user query, generate 3 testcases that is representative of the user’s interest that can help evaluate an AI Model on its competence on the users intent. The rules for generating the testcases are as follows: - The testcase should be STRICTLY between 1-2 lines and not very verbose. - Each testcase can be of a different format (Long Form, MCQ, Fill in the blank, etc.) - Only include the question/input for a testcase and not the expected output. - Return each testcase wrapped in a separate <testcase> </testcase> tag. Your final outputs should be of the form: <testcases> <testcase> Some question related to the query </testcase> <testcase> Some question related to the query </testcase> <testcase> Some question related to the query </testcase> </testcases> DO NOT INCLUDE ANY OTHER TEXT IN YOUR RESPONSE or any reasoning in your response. - The user query is: {query} ”””

C. Embedding-Based Semantic Retrieval Details

C.1. In Context Examples for the Embedding Models (used during Original, Example Synthesis and Rephrasing Index Creation)

use-case	Relevant example
Verify scientific claims	Claim: AdaBERT achieves inferior performance while significantly worsening the effiency by 12.7x. Evidence: The dataset contains 3772 word pairs. The accuracy of ConVecs 70.02% is not significantly different from the accuracy of SimDiffs (72.4%). Label: Negative/Refuted.
Blogging	Write a blog announcing the opening of a new mall in the town of Serenity.
Unit Testing Code	Question: An **anagram** is the result of rearranging the letters of a word to produce a new word. **Note:** anagrams are case insensitive Complete the function to return `true` if the two arguments given are anagrams of each other; return `false` otherwise. **Examples:** foefet is an anagram of toffee. ut_id: 0 code - import unittest class TestAreAnagramsFunction(unittest.TestCase): This class contains unit tests for the are_anagrams function. def test_anagram(self): Test that two anagrams return True. self.assertTrue(are_anagrams(”foefet”, ”toffee”)) def test_not_anagram(self): Test that two non-anagrams return False.
Star Gazing	The Big Dipper is a ______ constellation.
Resume for Machine Learning Engineer	I am a software engineer with 5 years of experience in the industry. Can you help me write a resume?

Note: All records share the instruction: ”Given an activity, retrieve relevant records that have the same underlying concepts and topics as the given activity”

C.2. Comparing Embedding-Models and Retriever Configurations for the Retriever Backbone

We measure the $\text{Prec}_{\text{sys}}@k$ and $\text{Recall}@k$ on the raw user use-cases from the *topics* set on several widely used dense retriever backbones spanning (i) standard BGE bi-encoders: BAAI/bge-base-en-v1.5 and BAAI/bge-large-en-v1.5 which are popular semantic embedding construction models; (ii) BAAI/llm-embedder, a popular instruction-tuned/LLM-derived embedding model suited for diverse retrieval augmented generation applications especially involving long, abstract queries (Zhang et al., 2024); and (iii) a cross-encoder reranker BAAI/bge-reranker-base, which jointly models query–example interactions rather than independent embeddings. The reranker's inclusion also allows us to test whether higher compute per query can compensate for the representation gap induced by broad, under-specified practitioner use-cases. Finally, we include the (iv) BAAI/bge-en-icl, an embedding construction model that is designed to better encode instructional intent and task structure using guidance from In-Context Learning examples demonstrating ideal recall. We leverage the latter to diversify our retriever's recall of diversely formatted examples by including the same in the in-context learning examples we generate for the case. §C.1 outlines the in-context examples used when generating the embeddings for the `BAAI/bge-en-icl` model. All values reported here at $k = 20$.

Retriever Type	$\text{Prec}_{\text{sys}}@k$	$\text{Recall}@k$	NDCG@10 / MRR	Processing Time (s)
`bge-base-V1.5`	0.208	0.004	0.291	0.048
`bge-large-V1.5`	0.332	0.004	0.353	0.044
`llm-embedder`	0.488	0.012	0.537	0.029
`bge-en-icl`	**0.694**	**0.218**	**0.684**	1.040
`bge-reranker`	0.664	0.176	0.632	14.030

Table 9. Retriever Performance Comparison: `bge-en-icl` achieves the highest $\text{Prec}_{\text{sys}}@k$, $\text{Recall}@k$ and $\text{NDCG}@k$ with reasonable latency.

Table 9 shows that `bge-en-icl` provides the best overall trade-off: it achieves the highest system precision ($\text{Prec}_{\text{sys}}@k$ =0.694), $\text{Recall}@k$ (0.218), and ranking quality $\text{NDCG}@k$, substantially outperforming both generic BGE encoders and the stronger `llm-embedder` baseline ($\text{Prec}_{\text{sys}}@k$ =0.488, $\text{Recall}@k$ =0.012). Notably, while the `bge-reranker` is competitive in accuracy ($\text{Prec}_{\text{sys}}@k$ =0.664, recall=0.176), its latency is an order of magnitude higher (14.03s vs. 1.04s), making it impractical as a default backbone for the interactive needs for our tool. In contrast, `en-icl` yields the strongest effectiveness metrics while remaining within a reasonable per-query compute budget, and we therefore adopt `bge-en-icl` as our default retriever backbone in BENCHBROWSER.

D. Filtering selection prompt

SELECTION PROMPT: You are evaluating whether a test case is relevant for assessing an AI model's ability to help with a specific user task. You are given: - <user_intent> Some topic, a description of some scenario, a capability or an application that some user is interested in <user_intent> - <test_case> Some question/test case that might be used to evaluate that system. </test_case>.
A test case is RELEVANT if correctly answering it would demonstrate capabilities that directly support the user's intent. Consider the following:

- **Knowledge Overlap**: Does the test require knowledge domains that overlap with what the user needs?
- **Skill Transfer**: Would the reasoning, analysis, or problem-solving skills needed for this test directly apply to the user's task?
- **Practical Utility** If an AI can handle this test case well, would that increase confidence it can help the user with their specific goal?

Rate the relevance as one of the following:

- 1: Test case is relevant to the user's intent.
- 0: Test case maybe relevant to the user's intent but not directly relevant.
- -1: Test case is not relevant to the user's intent

Provide your rating (only 1,0 or -1) wrapped in the <score> and </score> tags.

E. Evaluation Setup and Datasets

E.1. Benchmarks Included in BenchBrowser

Table 10. Benchmarks Used for Example Retrieval

Benchmark	Citation	Benchmark	Citation
IFEval	Zhou et al. (2023)	logiqa	Liu et al. (2021)
MMLU	Hendrycks et al. (2021)	mathqa	Amini et al. (2019)
TruthfulQA	Lin et al. (2022)	openbookqa	Mihaylov et al. (2018)
HellaSwag	Zellers et al. (2019)	qasper	Dasigi et al. (2021)
ARC	Chollet (2019)	triviaqa	Joshi et al. (2017)
Winogrande	Sakaguchi et al. (2020)	webqs	Berant et al. (2013)
BBH	Suzgun et al. (2023)	AlpacaEval	Dubois et al. (2024)
gsm8k	Cobbe et al. (2021)	ComplexBench	Wen et al. (2024)
headqa	Vilares & Gómez-Rodríguez (2019)	Infobench	Qin et al. (2024)
humaneval	Chen et al. (2021)		

E.2. Evaluation Categories

Motivation Skill-based testing is directly aligned with the traditional motivation for constructing LLM evaluation benchmarks, so practitioners directly benefit from being able to use BenchBrowser's analysis to anticipate the need to construct a new skill-oriented benchmark. Topics and application-oriented testing is more implicitly relevant to discovering new facets where evaluating LLMs could help discover new strengths and weaknesses. These set of queries can help practitioners decide new contexts to include in future benchmarks which are increasingly focusing on maximizing coverage and topical diversity of real-world user intents.

***Topics* use-cases.** An index is constructed using only BIG-bench Hard (BBH) and MMLU test sets, covering 58 use-cases (tasks and subjects). MMLU college-level subsets are excluded due to topic redundancy (e.g., "Human Aging", "High School Biology", "Clinical Knowledge"). Each topic-based query tests for focused conceptual alignment on a single subject area, enabling controlled measurement of retriever precision on narrow topical domains.

***Skills* use-cases.** To represent practitioner-facing evaluation intents, we derive 50 skill-based use-cases from the Clio analysis of real-world model usage (Tamkin et al., 2024). High-level skills (e.g., "instruction-following", "critical thinking", "creative writing") are prompted to Claude-3-7-Sonnet for naturalistic phrasing. The evaluation index aggregates $\sim$67k examples from 20 modern benchmarks such as IFEval (Zhou et al., 2023), InfoBench (Qin et al., 2024), MMLU, and BBH, enabling retrieval testing over both long-form and MCQ styles. This setup evaluates whether the retriever can generalize across benchmark formats and domains.

***Applications* use-cases.** These 50 Clio-seeded synthetic prompts simulate concrete tasks like "studying for the SAT" or "debugging HP printer errors." They assess how well retrievers locate evaluation items relevant to practical scenarios. Precision@20, NDCG@20, and yield are reported per application, using the same 20-benchmark index as the skills setup. Such testing is crucial for understanding real-world generalization and identifying missing evaluation coverage across practical tasks.

***Novel* use-cases.** We circulated a Google Form surveying NLP practitioners (graduate students within AI-focused field) to seek novel use-cases that they were interested in evaluating LLMs on. The participants were allowed to make more than one submission and up to 3 submissions. Each participant was requested to provide at least one peer-reviewed research paper that demonstrates the common utility of the use-case of their interest to the broader scientific community, ensuring the relevance and validity of our evaluation scenarios. This generated a set of 21 use-cases. Some use-cases were processed to omit personal identifiers - for example,'recommend me food that can satisfy my dietary constraints' was modified to 'recommending food that satisfies dietery restrictions'.

E.3. Prompts for Generating Usecases

We use the Clio (Tamkin et al., 2024) paper to seed the usecases for which we test BENCHBROWSER. This is done to ensure that we don't pick usecases that are too disconnected from real-world needs.

Generating Usecases for the *skills* set For generating the skill set we use the high-level task categories emerging from a random sample of 1M Claude.ai conversations as described in the paper (Table 7, Appendix F in (Tamkin et al., 2024)).

You are given a list of high-level use case categories for which Claude is used by real-world users. Your task is to identify the underlying fundamental skills that users need when engaging in tasks in these categories.
For each category, think about:

- What are the core cognitive abilities required? (e.g., reasoning, analysis, creativity)
- What technical competencies are involved? (e.g., coding, data manipulation, system design)
- What interpersonal or communication skills are needed? (e.g., persuasion, empathy, cultural awareness)
- What metacognitive or strategic capabilities are at play? (e.g., planning, decision-making, problem diagnosis)

Generate a list of 50 broad, transferable skills that represent these fundamental capabilities. These skills should be:

- **Atomic**: Each skill should represent a single, measurable capability
- **Cross-domain**: Skills should be applicable across multiple use case categories
- **Evaluable**: Each skill should be something that can be tested or benchmarked
- **Granular enough to be distinctive**: Avoid overly generic terms like "intelligence" or "ability"

Return your answer wrapped in XML tags.
Example: `<skill>cultural adaptation</skill>`
Input - Use Case Categories:

- Web and Mobile App Development
- Content Creation and Communication

- Academic Research and Writing
- Education and Career Development
- Advanced AI/ML Applications
- Business Strategy and Operations
- Language Translation
- DevOps and Cloud Infrastructure
- Digital Marketing and SEO
- Data Analysis and Visualization

Generating Usecases for the *applications* set For the *applications* set, we use the ground-truth categories from an evaluation dataset of 19,476 synthetic chat transcripts that outline more specific contexts under which Claude is utilized. We ask `Claude-3.4-Sonnet` to generate applications on the basis of the provided categories using the following prompt.

You are given a list of broad categories of tasks for which Claude is used by real-world users. Create a list of 50 applications based on the given categories. Do not make the compositional applications i.e., applications that combine too many tasks. Return your answer wrapped in XML tags ”usecase” ”usecase” Example: ”usecase” improving home energy efficiency ”usecase” Example: ”usecase” recommending Netflix series for me ”usecase”

- Software development questions
- Elementary school homework help
- Technology troubleshooting
- Health and fitness advice
- Questions about geopolitics
- Parenting and childcare tips
- Language learning and translation help
- Financial planning and investment
- Theological and philosophical questions
- Environmental science and sustainability
- Book discussions and literary analysis
- Sports rules and strategy questions
- Cooking and recipe inquiries
- Job application questions
- Home improvement and DIY projects
- Pet care and animal behavior
- Romantic relationship advice
- Movie and TV show recommendations
- Music theory and instrument learning
- Tourism and travel questions

E.4. *known validation* set use-cases and their corresponding Gold Test Sets

Table 11 specifies the use-cases designed for the *known validation* set.

Usecase	Gold Test Set
Learn how to measure my carbohydrate intake and make my meals more balanced	nutribench (Hua et al., 2024)
Test my knowledge of different sports and their respective players	bb_sports
Help me learn more about the Hindu religion	bb_hindu
Test me on different topics in science	sciq (Welbl et al., 2017)
Help me answer questions for my biomedical research	pubmed (Jin et al., 2019)
Help me mitigate different biological, cyber, and chemical attacks	wmdp (Li et al., 2024)
Explain the ethically right choice to make under different kind of situations	moral_stories (Emelin et al., 2021)
Can help me write one-line python codes	bb_code_line
Helps me find common substrings between strings	bb_cs_algo
Assists me in fact checking my claims	bb_fact_checker
Tests me on the properties of human organs	bb_human_organs
Teaches me how to use metaphors in daily language	bb_identify_odd
Learn how to identify common misconceptions	bb_misconceptions
Helps me resolve moral dilemmas	bb_moral_permissibility
Teaches me how to identify the underlying concept or theme among a few things	bb_novel_concepts
Can solve riddles for me	bb_riddle_sense
Can help me understand the social implications of my actions	bb_social_iqa
Help me figure out if someone supports my argument	bb_social_support
Tests my emotional intelligence	bb_strange_stories

Table 11. Prompts with Corresponding Gold Datasets used for the *Known Validation* Set. All datasets prefixed with "bb" are included in the Big Bench Dataset (Srivastava et al., 2023).

E.5. Evaluation Metrics

We use the following metrics for evaluation:

(a) MethodPrecision@k$_t = \frac{|\{d_i : i \leq k \wedge S_t(d_i) \in \{1,0\}\}|}{k}$ - This quantifies the precision of the ranking methods i.e., the number of relevant or partially relevant retrieved examples before the relatedness filtering within BenchBrowser.

(b) SystemPrecision@k$_t = \frac{|\{d_i : i \leq k \wedge L_t(d_i) \in \{\text{REL,PART}\}\}|}{k}$ - This quantifies the precision of BenchBrowser i.e., the number of relevant or partially retrieved examples by BenchBrowser after the relatedness filtering.

(c) Intersection@k$_t = \frac{\text{hits}_t(k)}{k}$ For the tie-breaking experiment, we also report the Intersection@k$_t$ which helps identify which method recalls the highest number of examples among all the examples returned by all the methods.

(d) **Recall@k**: In the Bounded Recall setup, measures how many of the returned examples actually belong to the expected gold test set for that use case:

$$\text{Recall@k} = \frac{|\{d \in \mathcal{R}_k \cap \mathcal{G}\}|}{|\mathcal{G}|} \tag{1}$$

where $\mathcal{G}$ is the gold test set for the given use case.

(e) **Common cutoff:** $K_c = \max\{k : |T_k| \geq 0.9 \cdot |T|\}$

(f) **Correlation with Model Ranking**: Wherever gold sets are available compute the correlation between the rankings of models when evaluated on a validation set (gold test set) versus our retrieved test set of the same advertised intent.

(g) **NDCG@k (Normalized Discounted Cumulative Gain)**: Measures the quality of ranking by considering both relevance and position, with higher-ranked relevant documents contributing more to the score:

$$\text{NDCG@k} = \frac{\text{DCG@k}}{\text{IDCG@k}} \tag{2}$$

where DCG@k (Discounted Cumulative Gain) is defined as:

$$\text{DCG@k} = \sum_{i=1}^{k} \frac{2^{\text{rel}(d_i)} - 1}{\log_2(i+1)} \tag{3}$$

and IDCG@k (Ideal Discounted Cumulative Gain) is the DCG@k score of the perfect ranking. Here, $\text{rel}(d_i)$ is the relevance score of document d_i at position i, and the logarithmic discount factor $\log_2(i+1)$ reduces the contribution of lower-ranked documents.

E.6. Evaluation Prompts

The key distinction between our Prec $_{\text{sys}}@k$ and Prec $_{\text{anchor}}@k$ is guided by the distinction between our evaluation prompt used by the LLM Judge for the Filtering Stage (referred to as the Selection Prompt) and the LLM Judge used for the final evaluation. While the Prec $_{\text{anchor}}@k$ evaluation aims at ascertaining which proportion of examples ranked by any anchoring method (Example Synthesis, Shorthand, etc) are related to the user's intent, the Prec $_{\text{sys}}@k$ evaluation prompt is strongly situated in a practitioner's intent of actually using the retrieved examples for a downstream task. We make this distinction assuming that practitioners might be lenient in claiming relatedness but not necessarily find a testcase useful for their specific intent if the test case is only broadly related to their task.

We corroborate this assumption with annotators during our human evaluation. For most comparisons, we stick to Prec $_{\text{sys}}@k$, as that is the practically useful metric that practitioners would expect maximized.

EVALUATION PROMPT: """"You are evaluating whether a test case is relevant for assessing an AI model's ability to help with a specific user task. You are given: - <user_intent> Some topic, a description of some scenario, a capability or an application that some user is interested in </user_intent> - <test_case> Some question/test case that might be used to evaluate that system. </test_case> Your evaluation criteria is: A test case is RELEVANT if successfully answering it would demonstrate capabilities that directly support the user's intent. Consider the following:

- **Knowledge Overlap**: Does the test require knowledge domains that overlap with what the user needs?
- **Skill Transfer**: Would the reasoning, analysis, or problem-solving skills needed for this test directly apply to the user's task?
- **Practical Utility** If an AI can handle this test case well, would that increase confidence it can help the user with their specific goal?

Rate the relevance as one of the following:

- RELEVANT: If an AI can answer this test case correctly, it would strongly imply that it can help the user with their specific goal.
- PARTIAL RELEVANT: If an AI can answer this test case correctly, it would somewhat imply that it can help the user with their broad goal i.e., with the topic or the skill needed by the user.
- IRRELEVANT: Answering this question, would not provide any useful indication about if the AI can fulfill the user's intent.

Provide your rating and wrapped in the <label> and </label> tags.

F. Additional Evaluation of Anchor Generation Methods for Retrieval

F.1. Tie-breaking: Computing Correlation between Model Performances on Retrieved Set and Gold Test Set for *topics* set

Measuring the correlation between the performance of a model on a gold test set that is explicitly designed for a use-case with the performance of the model on the set of retrieved examples can be a strong sanity check on the utility of the retrieved examples. Accordingly, for the *topics* set, we compute the performance of $N = 8$ models on the 58 BBH and MMLU testsets and then compute their correlation with the performance of the models on the sets of retrieved

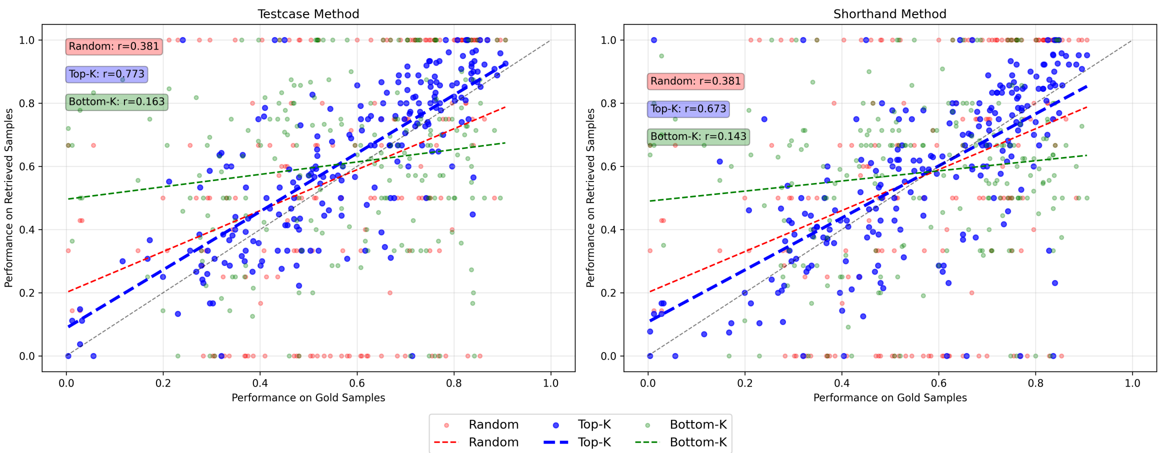


Figure 5. We observe that performance on the retrieved testset and the gold test set (BBH and MMLU for the *topics* set set) show reasonable to high correlation.

examples for the same use-cases. We use the following models for our evaluation: We use the following eight models for our evaluation: `Llama-3.1-8B-Instruct`, `Llama-3.1-Tulu-3-8B`, `Mistral-7B-Instruct-v0.2`, `Qwen1.5-14B-Chat`, `Qwen2.5-7B-Instruct`, `c4ai-command-r7b-12-2024`, `gemma-2-9b-it`, and `gemma-3-4b-it`. Since all the benchmarks in this set use either accuracy or exact string match as their metric for reporting accuracy, we aggregate the per-sample accuracy or exact-string match ($\in 0, 1$) and then normalize by the number of retrieved examples to obtain each model's performance on the retrieved set. As a baseline, we also plot the correlation between the performance of the models on the bottom-k and a random slice of the benchmark index as a way of calibrating random correlation.

Figure 5 demonstrates that the performance of the models on the retrieved sets and gold sets have high correlations: highest for the Example Synthesis anchor method.

F.2. Tie-breaking: Estimating Overlap with Approximated Gold Set for *applications* set and *skills* set

Use-cases in the *application* and *skill* sets do not have pre-defined gold sets of human-curated examples. Therefore, we cannot calculate the recall@k metric for these use-cases. Instead, we compare the different retrieval methods using a metric we call *intersection*@k. This metric measures the overlap between a method's relevant examples and the full set of examples that are deemed relevant by any method. To do this, we first run each retrieval method for a relatively large value of $k = 80$ and then take the union of all the examples that are judged as relevant by any method. Then, for a method m with ranked list $R_m(q) = [d_1, d_2, \dots]$, we compute the intersection of the examples up to rank k retrieved by BENCHBROWSER with the union set and divide by k.

Figure 6 shows that the *testcase* method consistently has higher intersection@k values across all three sets of use-cases.

To investigate the cause of the spike observed at the tail end of the intersection (Figure 6), we compute the support fraction across k which denotes what fraction of the queries actually contribute to the precision at a particular value of k. Formally, for method m, the *support* is $s_m(k) = |\{q : \ |G(q)| > 0 \ \wedge \ |R_m(q)| \geq k\}|$, and the support fraction is $\hat{s}_m(k) = s_m(k)/Q$. We define $K_{\text{common}}(m) = \max\{k : \ \hat{s}_m(k) \geq 0.9\}$, so points beyond K_{common} are backed by fewer than 90% of queries. In Figure 6: the green area hence marks the common area versus the greyed out region indicates the region where intersection@k is contributed by less than 10% of the queries; Through this we yield a more stable comparison between the macro averages at large k by avoiding dominance by a small, unrepresentative subset of easy queries where achieving high precision is naively possible. Ultimately, this experiment further provides evidence of Example Synthesis' reasonable proficiency and generality in being able to (a) retrieve relevant examples and (b) retrieve the largest set of relevant examples for any use-case.

F.3. Precision of BENCHBROWSER as K is increased

Figure 7 shows Prec$_{\text{sys}}$@k for the best-performing anchor (Example Synthesis) as we vary the number of retrieved examples, k. For *topics* set and *skills* set, precision remains relatively stable as k grows, indicating that BENCHBROWSER can return

Figure 6. intersection@k in approximated recall setup: Gray region indicates precision for fewer than 90% of queries. For each method we compute K_{common}: the largest k for which $\geq$ 90% of tasks contribute a value at every position $\leq$ k (i.e., their lists are at least that long). The area up to K_{common} under that method's curve is filled green (trusted, high task support); the tail beyond K_{common} is filled grey (lower support). A dot marks (K_{common}, Precision at K common.)

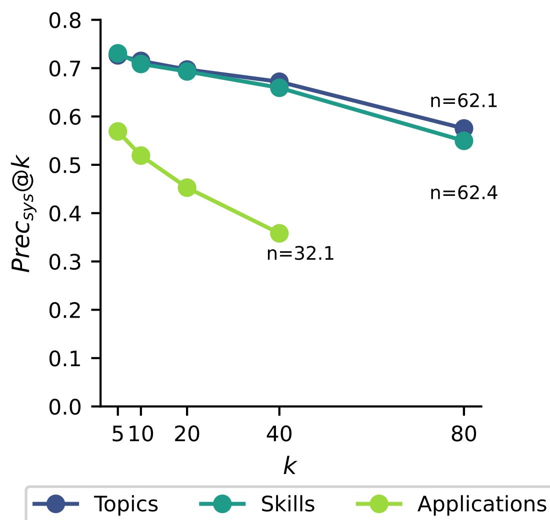


Figure 7. System precision (Prec $_{sys}$@k) as a function of k for *topics* set, *skills* set, and *applications* set. For *topics* set and *skills* set, precision remains high as k increases, while *applications* set exhibits a sharper decline.

larger pools of examples without rapidly flooding practitioners with irrelevant items. This may be especially useful when practitioners want to sample a broader, larger set of examples for using doing fine-grained selection over or using as an alternate test set for custom use-cases for which gold test sets don't exist.

In both experiments, performance on the *applications* set set is noticeably lower across all methods (Table 3, Figure 7), reflecting the difficulty of retrieving examples for highly contextual use-cases from existing benchmarks. This drop in precision is not surprising since *applications* set use-cases often bundle multiple skills and entities (e.g., domain, user role, format, constraints), only some of which may be followed or *available* in the examples included in the benchmarks. Despite this, Example Synthesis has high performance in this set too where the non-rewrite based Original baseline actually becomes the most competitive retrieval method. In alignment with prior work, this parity between the Original and Example Synthesis based anchoring highlights the complexity in devising and choosing a single rewrite method that can maintain retrieval performance across a wide variety of practitioner use-cases (Kalra et al., 2025; Salemi et al., 2024).

G. Automatic Judge Calibration with Another Automatic Judge

To further validate the sanity of our automatic relevance judgments we also replicate the automatic precision computation in Table 3 with another automatic judge i.e., `gemini-2.5-flash-lite` and report the spearman correlation between the scores

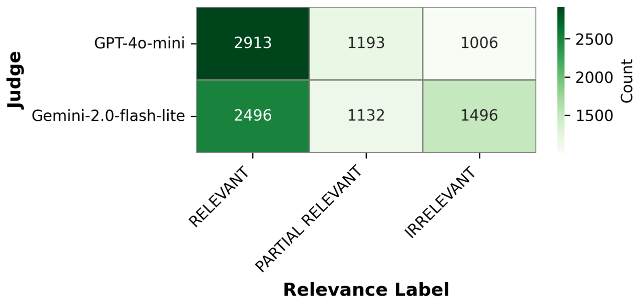


Figure 8. Marginal relevance-label distributions assigned by GPT-5-mini (designated judge) and Gemini-2.5-flash-lite over the same evaluated top-k retrieved items.

enlisted by this model and our designated judge, `gpt-5-mini`.

Category	Score	Prec $_{\text{sys}}@k$ [k=10]	Prec $_{\text{sys}}@k$ [k=20]
Overall	$0.629^{\ddagger}$	$0.700^{\ddagger}$	$0.795^{\ddagger}$
Applications	$0.613^{\ddagger}$	$0.738^{\ddagger}$	$0.725^{\ddagger}$
Known Validation	$0.549^{\ddagger}$	$0.485^{\ddagger}$	$0.604^{\ddagger}$
Skills	$0.603^{\ddagger}$	$0.626^{\ddagger}$	$0.747^{\ddagger}$
Topics	$0.576^{\ddagger}$	$0.562^{\ddagger}$	$0.622^{\ddagger}$

Table 12. Spearman correlation coefficients. ‡ indicates $p < 0.001$; † indicates $p < 0.05$.

The resulting Spearman correlations are consistently positive and statistically significant (Table 12), indicating that items judged as more relevant by our designated judge also tend to be judged as more relevant by the alternative judge. This provides evidence that the relative relevance ordering induced by our automatic evaluation is stable under judge substitution, supporting the robustness of our aggregate retrieval comparisons. Figure 8 also reports the marginal label distributions assigned by each judge across all evaluated items. Both judges use all three labels and share the same qualitative pattern—RELEVANT is most frequent, followed by PARTIAL RELEVANT but they differ in strictness: `gpt-5-mini` assigns RELEVANT more often (≈57%) and *gemini-2.5-flash-lite* assigns fewer RELEVANT labels (≈49%). Together with the strong item-level rank agreement, this suggests that the judges largely agree on which items are comparatively more vs. less relevant, while disagreeing primarily in the decision boundary between RELEVANT and IRRELEVANT (i.e., Gemini is more conservative). This pattern is consistent with *reasonable calibration* for our use case as aggregate precision trends seem stable (at least across method) under an alternate judge.

H. System Infrastructure and Profiling

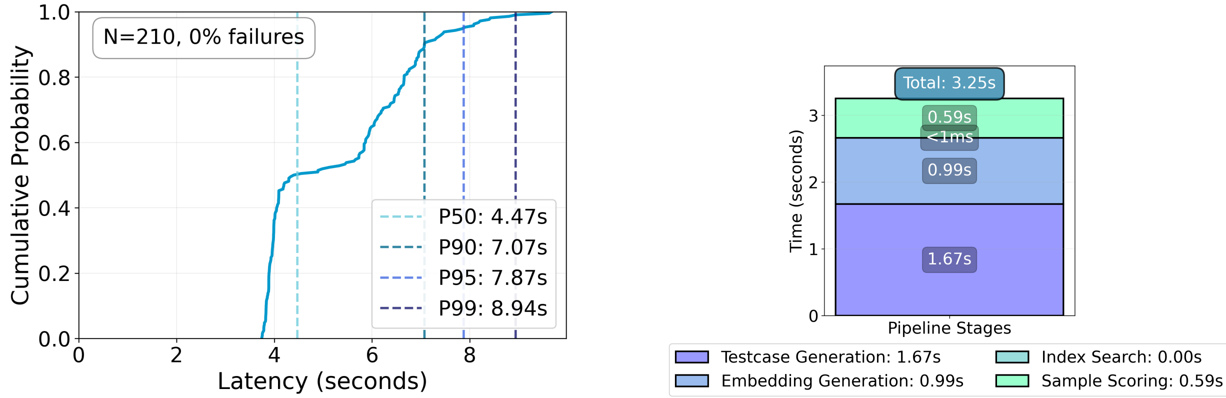
H.1. Hosting Infrastructure

BENCHBROWSER is deployed as a Google Cloud Run service in region `northamerica-northeast1`, provisioned with **4 GiB RAM** and **1 vCPU**. To reduce cost, we set the minimum number of instances to **0**, so requests may incur cold-start overhead; each request has a **120 s** deadline (requests exceeding this limit are terminated). Cloud Run request concurrency is set to 80.

To avoid GPU-backed embedding computation, we outsource embeddings to the DeepInfra API, using `BAAI/bge-en-icl` at a rate of **$0.0100/Mtoken**. Our reported end-to-end latency includes the external embedding API call (network + inference) in addition to backend processing.

H.2. Latency Profiling

We report end-to-end user-perceived latency for the public BENCHBROWSER demo over 210 requests (1 iteration per query; no failures). Latency is 5.28 ± 1.48 s (mean ± stdev), with p50 = 4.47 s, p90 = 7.08 s, p95 = 7.90 s, and p99 = 8.95 s (min 3.75 s, max 9.68 s). A stage-level breakdown attributes 1.67 s (31.7%) to testcase generation, 0.99 s (18.8%)



(a) **End-to-end latency distribution.**

(b) **Mean stage-time breakdown.**

Figure 9. **Latency of the BENCHBROWSER web demo.** (Left) Empirical CDF of request latency over 210 runs of the public BENCHBROWSER demo (k=35), with percentile markers (p50/p90/p95/p99). Median latency is 4.47s; tail latency remains below 8.95s at p99 (0 failures). (Right) Average time spent in each pipeline stage, showing testcase generation (1.67s; 31.7%), embedding generation (0.99s; 18.8%), and sample scoring (0.59s; 11.2%) as the dominant contributors; the remaining time corresponds to end-to-end overhead.

to embedding generation, and 0.59 s (11.2%) to sample scoring; the remaining 2.02 s (38%) is unattributed overhead (primarily request/response handling, serialization, networking, and other pipeline glue), which we include in the end-to-end measurement.

I. Human Evaluation of BENCHBROWSER

We obtained an Institutional Review Board (IRB) exemption under the 2018 Common Rule (45 CFR § 46.104(d)) to conduct our study with $N = 10$ annotators. Annotators were compensated with a DoorDash food order (up to $20 USD) for approximately one hour of participation. Eligibility criteria required that participants be at least 18 years old, fluent English speakers, currently residing in the United States, and have prior experience working on at least one NLP project.

I.1. Detailed Analysis of Human–Automatic Disagreement

Known Validation outliers A closer analysis of the `Known Validation` set reveals that the large mean difference between human and automatic relevance scores (Table 4) is driven by six use-cases with unusually large per-use-case deltas (0.40–0.75), with humans consistently assigning lower relevance. These use-cases are drawn from benchmarks that test abstract or esoteric capabilities, making it difficult to "retrofit" a concise practitioner-style use-case. For instance, the most extreme deviation (mean difference 0.75) arises from BIG-bench's `novel_concepts` task (Srivastava et al., 2023), whose original goal is to *"measure the ability of models to uncover an underlying concept that unites several ostensibly disparate entities, which hopefully would not co-occur frequently."* We simplify this to a retrieval use-case of *"identifying the underlying concept for a given set of items."* Annotator comments (Table 13) indicate that, without the full benchmark context, this simplified use-case encourages annotators to form narrow, idiosyncratic interpretations. examples that are consistent with the original benchmark designer's intent, but not with the annotator's inferred interpretation, are then marked as *Irrelevant*, increasing the human–automatic gap. We observe similar issues for other abstract tasks in this set, suggesting that retrofitting highly specialized benchmarks into short use-cases can systematically bias human judgments downward.

Low inter-annotator agreement We also investigate the relatively low Fleiss's κ for several sets (Table 4). Figure 10 plots confusion matrices for (a) the LLM judge versus the majority human label and (b) pairs of human annotators, restricted to examples from the *topics* set, *skills* set, and *applications* set sets. In both cases, most disagreements occur between *Relevant* and *Partially Relevant* relevant labels rather than between *Relevant* and *Irrelevant*, indicating that annotators and the LLM generally agree on whether an item is "in the ballpark" and differ mainly on how strict to be. Table 13 illustrates this with example comments from annotators on disputed items: both annotators often articulate plausible but different interpretations of what counts as "relevant" to a given use-case (e.g., whether sentiment classification suffices for "detecting emotional support in conversations"). This pattern matches prior findings that inter-annotator agreement is modest for inherently subjective constructs such as quality, relevance, or interest (Adams et al., 2023; McDonnell et al., 2016; Shirani et al., 2021),

Table 13. Comments from Annotators over samples where we observe disagreement: Annotator A is always the annotator assigning a high relevance (1) and Annotator B is always the annotator assigning a low relevance (0).

Sample	Annotator Comment
use-case: Learn Differential Equations	
Prompt: Antiderivar la siguiente función f(x)=(x+1)(2x-1)	A says: “solving DEs requires computing antiderivatives;” B says: “The question is testing both translation and maths;within maths, differential equation is not explicitly tested so on average I this isn’t relevant”.
use-case: Detecting emotional support in conversations	
Group chat excerpt: Owner: @Team - Aren’t you too unpunctual? We agreed on 10 o’clock yesterday ..... **Prompt**: Determine the emotion of the owner’s last message. Choose from [negative, neutral, positive].	A says : The task is sentiment classification which is relevant to detecting emotions. B says: The task description is specifically asking for cases that test emotional support, while the question tests emotion classification with predefined categories which aren’t relevant to emotional support.
use-case: Identifying the underlying concept for a given set of items	
All of the following support the endosymbiotic theory .... *EXCEPT* • Mitochondria and chloroplasts divide ... • Mitochondria and chloroplasts have ribosomes	Annotator B: Given the broad nature of ”concepts, factual knowledge,” technically anything in a domain could share underlying concepts. This seems too abstract and insufficiently related to the specific task so I can consider this partially relevant at best”. *Annotator A marks as ’relevant’ but does not justify in the comments.*

and suggests that our IAA values mainly reflect the intrinsic ambiguity of the task rather than poorly specified instructions.

(a) LLM vs Human

(b) Human vs Human

Figure 10. Intra and Inter-Label distributions between LLM and human annotators on the examples evaluated from the *topics, skills and applications* set. Most disagreements occur between *partially relevant* and *relevant* categories rather than polar opposites, implying calibration rather than conceptual divergence.

J. Additional Details for Content Validity

We manually verify that the 120 use-cases for demonstrating the utility of BENCHBROWSER in §5 are constructed such that induced variations are lexically distinct. Assuming a very rigid relevance criterion of which deems that “a example is relevant only if it explicitly states the induced variation” (following which it is already checked for semantic relevance at the filtering stage) - even a lexical baseline is expected to give similar trends in the variation of the spread of relevant examples across variations of the same usecase.

Accordingly, we also construct a lexical BM25 baseline which only retrieves an example if it explicitly mentions the variation’s keyword. Though stringent, this has been shown to be a realistic baseline of an annotator judging relevance for use-cases that they are either not interested in or are not experts in (Voorhees, 1998; Saracevic, 2007). Figure 11 confirms that the qualitative trends from Figure 3 hold under this lexical baseline, suggesting that our findings are due to benchmark composition rather than an artifact of the semantic retriever.

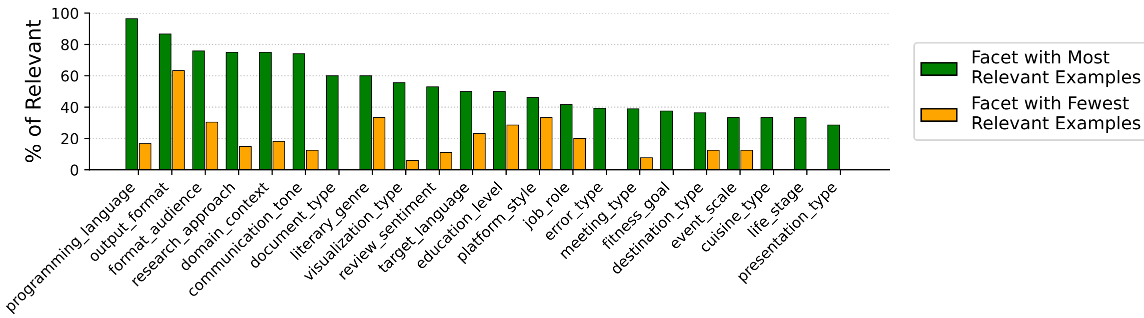


Figure 11. BM25 retrieval results for calibrating the variation in content validity as reported by BENCHBROWSER

We confirm that the lexical baseline also demonstrate the same trend in spread of relevance, reiterating that the variation in relevant count spread observed is an artifact of the properties of the use-case and not due to lossy semantic retrieval. Ultimately, this confirms that some facets are underserved compared to other dominant facets.

J.1. Example of Use Cases showing Spread in Relevant Count across Variations

This table includes some examples of the task descriptions and consequent yields of examples across variations along the same axes.

Table: Dispersion in Prec$_{sys}$@k across facets of the same use-case. Most use-cases show substantial spread: some variants (green) have many *Relevant* examples, while others (red) are barely represented.

math word problems *Domain Context* "solve finance calculation problem"	**Prec$_{sys}$@k**
finance calculation	1.000
travel distance	0.933
construction material	0.900
cooking measurement	0.679
inventory management	0.500

social media posts *Platform Style* "write Instagram post"	**Prec$_{sys}$@k**
Instagram	0.862
TikTok	0.759
Facebook	0.714
Twitter	0.700
YouTube	0.700
LinkedIn	0.621

instruction following *Output Format* "format response as Markdown"	**Prec$_{sys}$@k**
Markdown	0.967
JSON	0.867
table	0.633
YAML	0.448
bullet list	0.414

creative writing *Literary Genre* "write fantasy story"	**Prec$_{sys}$@k**
fantasy	0.759
science fiction	0.679
mystery	0.654
romance	0.625
horror	0.583
historical fiction	0.517

summarization style *Format Audience* "summarize as legal memo"	**Prec$_{sys}$@k**
legal memo	1.000
bullet points	0.667
executive summary	0.533
headline	0.483
tweet	0.483
academic abstract	0.440

email composition *Communication Tone* "write follow-up email"	**Prec$_{sys}$@k**
follow-up	0.880
formal business	0.808
urgent request	0.577
apologetic	0.476
casual friendly	0.462
thank you	0.391

debugging code *Error Type* "debug syntax error"	**Prec$_{sys}$@k**
syntax error	0.867
runtime error	0.714
logic error	0.690
security vulnerability	0.393
performance issue	0.357
memory leak	0.269

data analysis *Visualization Type* "create line graph analysis"	**Prec$_{sys}$@k**
line graph	0.593
bar chart	0.556
scatter plot	0.500
pie chart	0.500
histogram	0.481
heatmap	0.333

research methodology *Research Approach* "design experimental study"	**Prec$_{sys}$@k**
experimental study	0.759
quantitative research study	0.655
observational study	0.552
case study research	0.407
qualitative research study	0.357
mixed methods study	0.208

code generation *Programming Language* ”write function in Python”	**Prec**$_{sys}$@k
Python	0.867
C++	0.741
Rust	0.478
JavaScript	0.346
Java	0.286
Go	0.100

language translation *Target Language* ”translate text to Chinese”	**Prec**$_{sys}$@k
Chinese	0.600
German	0.586
French	0.467
Japanese	0.448
Spanish	0.379
Arabic	0.276

event planning *Event Scale* ”plan small dinner party”	**Prec**$_{sys}$@k
small dinner party	0.600
birthday celebration	0.524
large conference	0.414
fundraising gala	0.412
corporate retreat	0.333

trip planning *Destination Type* ”plan trip to multiple cities”	**Prec**$_{sys}$@k
trip to multiple cities	0.733
trip under budget cap	0.640
trip to Asia	0.467
trip to Europe	0.400
trip to rural village	0.267
weekend trip only	0.227

financial advice *Life Stage* ”provide financial advice for you...”	**Prec**$_{sys}$@k
young professional	0.593
mid-career adult	0.538
pre-retiree	0.500
retiree	0.500
college student	0.333
new parent	0.263

presentation slides *Presentation Type* ”create conference presentation s...”	**Prec**$_{sys}$@k
conference	0.619
business	0.483
academic	0.464
sales	0.458
investor	0.455
training	0.222

product reviews *Review Sentiment* ”write mixed product review”	**Prec**$_{sys}$@k
mixed	0.552
negative	0.500
detailed	0.370
brief	0.286

meeting notes *Meeting Type* ”take project meeting notes”	**Prec**$_{sys}$@k
project meeting	0.565
board meeting	0.526
team standup	0.478
client meeting	0.409
interview meeting	0.333
brainstorming meeting	0.240

lesson planning *Education Level* ”create preschool lesson plan”	**Prec**$_{sys}$@k
preschool	0.579
elementary school	0.458
college	0.407
high school	0.333
middle school	0.320
adult education	0.304

interview questions *Job Role* ”create sales representative inte...”	**Prec**$_{sys}$@k
sales representative	0.500
data scientist	0.455
marketing manager	0.316
customer service	0.300
software engineer	0.292
product manager	0.200

legal documents *Document Type* ”draft partnership agreement”	**Prec**$_{sys}$@k
partnership agreement	0.385
licensing agreement	0.370
employment contract	0.333
non-disclosure agreement	0.250
privacy policy	0.231
terms of service	0.182

workout design *Fitness Goal* ”design workout for rehabilitation”	**Prec**$_{sys}$@k
rehabilitation	0.500
weight loss	0.273
strength	0.261
muscle gain	0.208
endurance	0.185
flexibility	0.111

K. Additional Details for Convergent Validity

We use the following eight models for our evaluation: `Llama-3.1-8B-Instruct`, `Llama-3.1-Tulu-3-8B`, `Mistral-7B-Instruct-v0.2`, `Qwen1.5-14B-Chat`, `Qwen2.5-7B-Instruct`, `c4ai-command-r7b-12-2024`, `gemma-2-9b-it`, and `gemma-3-4b-it`.

Accuracy Aggregation for Model Performance Estimates Model performance is evaluated using the specific metric designated by each benchmark, as summarized in Table 14. The majority of benchmarks supported by the Eval Harness (Gao et al., 2024a) provide per-sample accuracy based on strict string matching or binary accuracy. We utilize these values directly, noting that they are bounded between 0 and 1 (representing a binary exact match or correct prediction).For benchmarks employing Win-Rate as a metric, we generate model inferences and compute the win-rate against a fixed set of standard responses generated by the `Ministral-8B-Instruct-2410` model. In this comparison, a win against the reference model is encoded as 1, while a tie or loss is encoded as 0 (serving as an accuracy equivalent).For the remaining benchmarks requiring custom evaluation protocols, we employ the specific metrics and libraries provided by the original authors. For instance, InfoBench (Qin et al., 2024) is evaluated using the provided code for its designated DRFR metric. These per-sample scores are subsequently aggregated to derive the final model performance estimates.

To control for any variance in retrieval results induced due to difference in prompting language - we write 5 task descriptions per use-case and retrieve examples for each. Then we pick the prompt that has the high automatic precision and have those annotated by human annotators. To account for the change the Kendall's τ due to the size of the retrieved set and measure the internal heterogeneity of the gold set, we compare the Kendall's τ across 3 configurations which is described in detail below.

Table 14. Summary of evaluation metrics used across benchmarks.

Category	Key Used	Processors / Benchmarks
Custom Metric	*Varied*	complexbench (point_judges), infobench (DRFR), ifeval (inst_level_strict_acc), llmbar (gold label)
Accuracy	acc	nutribench, truthfulQA, arc, mmlu, winogrande, hellaswag, headqa, logiqa, mathqa, openbookqa, sciq, pubmed, wmdp, moral_stories, big-bench tasks
	exact_match	triviaqa
	pass@1	humaneval
Exact Match	exact_match	bbh, gsm8k, webqs
F1 Score	f1/f1_abstractive	qasper
Win Rate	win	mtbench, koala, vicuna, lfqa, alpacaeval

K.1. Correcting Kendall's Tau

For each task, we first select a single prompt per (model, task): the prompt that maximizes the model's mean accuracy over that prompt's retrieved subset. Let a_m^{ret} denote model m's mean accuracy on its retrieved items (with n_m retrieved items), and let $\bar{a}_m^{\text{gold}}$ denote its mean accuracy on the full gold test set. We report three Kendall's τ correlations per task (averaged over S Monte Carlo trials):

(i) **Same-cardinality retrieved–gold: τ_{ret}.** We compare the retrieved ranking $\{a_m^{\text{ret}}\}_m$ to a gold ranking estimated at *matched evaluation budget*. In each trial, for every model m we subsample n_m examples from its gold pool, compute the subsampled gold accuracy $a_m^{\text{gold},(s)}$, and compute $\tau\Big(\{a_m^{\text{ret}}\}_m, \{a_m^{\text{gold},(s)}\}_m\Big)$. τ_{ret} is meant to ascertain if the retrieved set induces the same model ordering as the gold set, if both were evaluated with the same number of examples.

(ii) **Internal gold stability (full vs. subset): τ_{gold}.** To quantify how much rankings can fluctuate due to *finite-sample effects alone*, we measure stability of gold rankings when reducing the gold set to n examples. Let $n = \lfloor \frac{1}{M} \sum_m n_m \rfloor$ be the mean retrieved cardinality for the task. In each trial we subsample n gold examples per model, compute $a_m^{\text{gold},(s)}$, and compute $\tau\Big(\{\bar{a}_m^{\text{gold}}\}_m, \{a_m^{\text{gold},(s)}\}_m\Big)$. Thus, τ_{gold} provides a task-specific ceiling on rank agreement attributable to reduced evaluation budget.

(iii) **Gold–gold repeatability (subset vs. subset): τ_{sanity}.** As an additional control, we measure ranking repeatability when *both* rankings are estimated from gold subsets of the same size. In each trial, we draw two independent subsamples of size n per model, compute the corresponding accuracies, and compute $\tau\Big(\{a_m^{\text{gold},(s_1)}\}_m, \{a_m^{\text{gold},(s_2)}\}_m\Big)$. This isolates the intrinsic uncertainty in rankings when the evaluation budget is n (without referencing the full gold ranking).

Across tasks (Fig.12), τ_{gold} and τ_{sanity} quantify how reliable model ordering is even within the original benchmark at the relevant sample size. Across most tasks, the τ_{sanity} are relatively close to the maximum achievable τ_{gold}, indicating that model rankings are *stable* at the retrieved-set cardinality *internally*; This helps us confirm that the low τ_{ret} we observe is not due to inherent heterogeneity in the gold set. However, τ_{ret} is often much lower (sometimes near-zero or negative) indicative of alternate explanations: we posit, divergent operationalizations and rubric mismatches which ultimately engender claims with low convergent validity for those use-cases.

L. Limitations

Our human evaluation is constrained in scale (number and diversity of use-cases and annotators) to keep annotation cost manageable: while we collect 4,200 relevance judgments, precision and coverage estimates may still not fully generalize beyond the evaluated use-cases.

The content-validity analysis in §5 does not elicit or verify facets from annotators, avoiding a second layer of costly human validation; Additionally, these facets are generated by a language model, which introduces risk of biased recall of facets. We expect that using the LLM as Judge can provide a reasonable preliminary signal of the any representational disparity for a use-case which can be significantly improved using human supervision. Consequently, analysis in this section is intended to be useful primarily for highlighting the presence of representation disparities that practitioners can inspect rather than as actionable claims about which facets must be served more equitably since that is heavily conditioned on a human-in-the-loop elicitation and relevance review assessment of the retrieved examples.

Finally, BENCHBROWSER can only surface misalignment within the benchmark items and model outputs it indexes; expanding coverage (more benchmark suites and models, or commercial-model inferences required for some metrics) is resource-intensive, so any diagnosis is bounded by the current index and the feasible set of evaluated models.

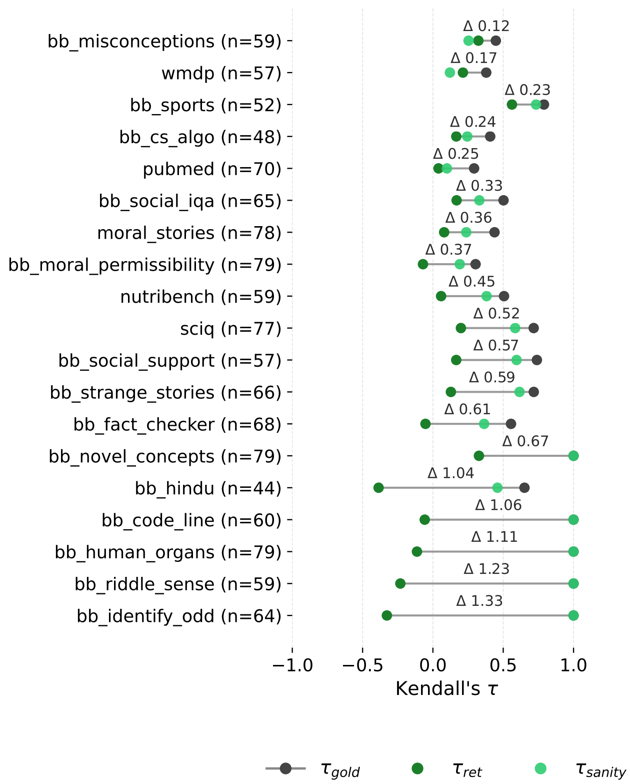


Figure 12. τ_{sanity} and τ_{gold} provide gold-only stability controls; τ_{ret} measures additional divergence introduced by retrieval.