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General-purpose large language models outperform specialized clinical AI tools on medical benchmarks

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Krithik Vishwanath[1,2,3] ✉, Anton Alyakin[1,4,5], Mrigayu Ghosh[6,7], Ali Hage[8], Sean N. Neifert[1], Cordelia Orillac[1], Nataniel J. Mandelberg[1], Hammad A. Khan[1], Jin Vivian Lee[1,4,5], Jie J. Yao[9], William Robert Small[10,11,12], Aakaash Varma[13,14], D. Brock Hewitt[15], Yindalon Aphinyanaphongs[10,12,16], Daniel Alexander Alber[11] & Eric Karl Oermann[1,5,17,18,19] ✉

Specialized clinical artificial intelligence (AI) tools are entering medical practice despite scarce independent evaluation. We quantitatively evaluate two clinical AI tools, OpenEvidence and UpToDate Expert AI, built on large language models (LLMs) against three frontier LLMs: GPT-5.2, Gemini 3.1 Pro and Claude Opus 4.6. Our evaluation has three stages: (1) 500 MedQA questions testing medical knowledge, (2) 500 HealthBench items measuring alignment with clinicians and (3) the real clinical queries (RCQ) benchmark, built from 100 de-identified queries from physicians to a general-purpose language model in a live clinical environment. For the RCQ benchmark, 12 US clinicians performed randomized, blinded review of model outputs, producing 1,800 model–question annotations. Frontier LLMs outperformed clinical AI tools in all three evaluations. Clinical AI tools performed comparably to auto-enabled Google Search AI Overview on the RCQ. These findings highlight the need for independent, real-world evaluation of AI tools before they enter clinical settings.

Specialized clinical artificial intelligence (AI) tools are entering medical practice at scale[1,2]. These proprietary large language model (LLM)-based tools promise superior clinical performance to general-purpose frontier LLMs as a result of domain-specific training or retrieval-augmented generation (RAG)[3]. Yet, their architectures, base models and training pipelines are not public. Clinicians and health systems must therefore assess their value and safety without independent evidence. Conversely, large training corpora and extensive alignment of frontier LLMs may enable them to challenge clinical AI tools without domain-specific modification. We test this hypothesis by comparing clinical AI tools (OpenEvidence[1] and UpToDate Expert AI[2]) to leading general-purpose LLMs (OpenAI GPT-5.2, Google Gemini 3.1 Pro Preview and Anthropic Claude Opus 4.6). Later, we include auto-enabled Google Search AI Overview as a real-world control frequently encountered by physicians.

Our evaluation (Fig. 1) has three stages: (1) 500 US Medical Licensing Examination-style MedQA[4] questions assessing medical knowledge, (2) 500 HealthBench[5] items evaluating agreement with expert clinicians and (3) 100 real clinical queries (RCQ) drawn from physician LLM queries during live clinical deployment. The RCQ stage underwent randomized, blinded review by 12 US clinicians, producing 1,800 model–question annotations. The combined analysis spans multiple-choice reasoning, expert clinical judgment and everyday clinician use.

General-purpose LLMs outperformed clinical AI tools on the MedQA questions (Fig. 2a and Extended Data Fig. 1a,b). Among frontier LLMs, Gemini achieved the highest accuracy at 97.4% (95% confidence interval (CI) 95.6%–98.5%), followed by GPT at 94.2% (91.8%–95.9%) and Claude at 90.2% (87.3%–92.5%). Clinical tools scored lower, with OpenEvidence achieving an accuracy of 89.6% (86.6%–92.0%) and UpToDate achieving 88.4% (85.3%–90.9%). Gemini outperformed all other models (McNemar $P < 1 \times 10^{-4}$ versus OpenEvidence, UpToDate and Claude; $P = 0.02$ versus GPT). GPT outperformed OpenEvidence ($P = 0.008$), UpToDate ($P = 0.0004$) and Claude ($P = 0.04$).

A full list of affiliations appears at the end of the paper. ✉e-mail: krithik.vish@utexas.edu; eric.oermann@nyulangone.org

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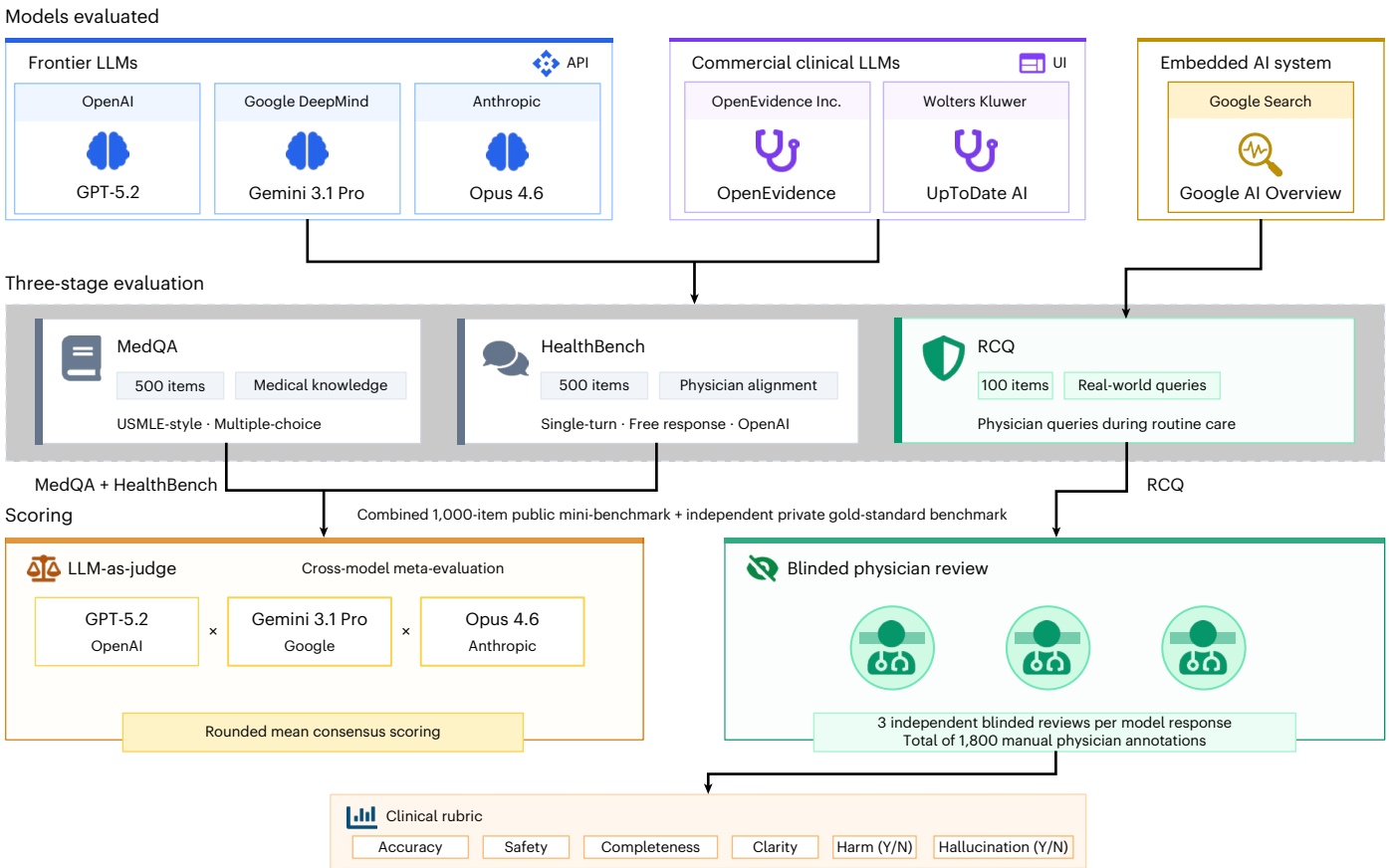

Fig. 1 | Clinical LLM evaluation pipeline. Schematic overview of the comparative analysis between frontier, clinical-specific and search-embedded AI models. The framework integrates automated scoring (MedQA and HealthBench) with high-fidelity blinded and randomized clinician reviews (RCQ) to assess model performance across accuracy, safety and reliability metrics. N, no; USMLE, US Medical Licensing Examination; Y, yes. Blindfold/blinded icon from Tailwind Labs under an MIT license (©Tailwind Labs); other icons from React Icons under a CC BY 4.0 (brain, stethoscope, book, bar chart, doctor, justice scale, chat, shield) or Apache 2.0 (browser, API badge).

HealthBench (Fig. 2b) was graded by a panel of LLM judges to mitigate single-model bias. Scores reflect the proportion of rubric points achieved, scaled 0–100. GPT scored highest at 88.0 (95% CI 85.9–90.1), followed by Gemini at 79.3 (76.6–81.9) and Claude at 77.0 (74.2–79.9); both clinical tools scored lower (OpenEvidence scoring 62.6 (59.3–65.9) and UpToDate scoring 61.3 (58.0–64.6)). GPT outperformed all other models (Wilcoxon $P < 10^{-9}$), and the two clinical tools did not differ ($P = 0.6$). In theme-level analysis (Extended Data Fig. 1c,d), GPT ranked first or tied for first in all seven categories, while OpenEvidence and UpToDate ranked lowest or tied for lowest in all seven categories, with differences from GPT significant in six of the categories ($P \leq 0.004$; exception: responding under uncertainty, $P = 1.00$).

To develop the RCQ benchmark, we sampled 100 anonymous clinician queries to the NYU Langone Health Insurance Portability and Accountability Act-compliant GPT instance. Twelve blinded clinicians scored six models' responses across four dimensions (clinical correctness, completeness, safety/harm avoidance and clarity) on a 1–4-point scale (Extended Data Fig. 2). For each response, three raters were then randomly assigned to evaluate them. We included Google Search AI Overview in the RCQ evaluation because it is routinely encountered by clinicians. After excluding 32 refusals, 568 responses remained.

The six models differed significantly (Friedman $P < 10^{-9}$), with two performance tiers emerging (Fig. 2c). Frontier LLMs formed the first: Gemini (mean aggregate 3.62; 95% CI 3.56–3.68), GPT (3.54; 3.47–3.61) and Claude (3.52; 3.44–3.59), with no significant differences between them. Clinical tools and Google AI Overview followed: OpenEvidence (3.24; 3.17–3.32), UpToDate AI (3.17; 3.09–3.25) and Google AI Overview (3.27; 3.18–3.35), also without significant differences. All nine significant pairwise comparisons were between tiers (rank-biserial $r = 0.5$–0.9), meaning frontier models outperformed clinical tools on most individual questions, not just on average. After adjusting for rater leniency, clinical AI tools (including Google AI) had 49–87% lower odds of receiving a higher rating than Gemini (odds ratio 0.13–0.51; all $P < 0.0001$). In a sensitivity linear mixed model, this corresponded to 0.36–0.44 points lower on the 1–4-point scale (all $P < 0.0001$). Google AI Overview scored as well or better than OpenEvidence and UpToDate AI across all dimensions (Extended Data Fig. 3).

The tier structure held across all four dimensions (Fig. 2d). Models differed most on clarity (Kendall's $W = 0.292$) and least on clinical correctness ($W = 0.141$). OpenEvidence scored lowest on clarity (mean 2.84), suggesting its weakness was communication, not knowledge. Qualitatively, incomplete clinical content, safety-critical omissions and disorganized responses were common, particularly for OpenEvidence and Google AI Overview (Extended Data Table 1). UpToDate AI refused 19% of queries (Fig. 2e), more than all other models (1–3%; $P < 0.01$) except Google AI Overview (6%; $P = 0.10$). Safety outcomes (Fig. 2f,g) did not differ across models: none of the models produced more harmful content (Cochran's $Q = 4.00$, $P = 0.55$) or hallucinations ($Q = 5.00$, $P = 0.42$) than any of the others. All 12 clinicians ranked the models similarly (Kendall's $W = 0.651$, $P = 2.3 \times 10^{-7}$), placing frontier LLMs above clinical tools (Extended Data Fig. 4).

This study is an independent, quantitative comparison of clinical AI tools against frontier LLMs using real-world physician queries from the course of care. Clinical AI tools lagged behind frontier models on

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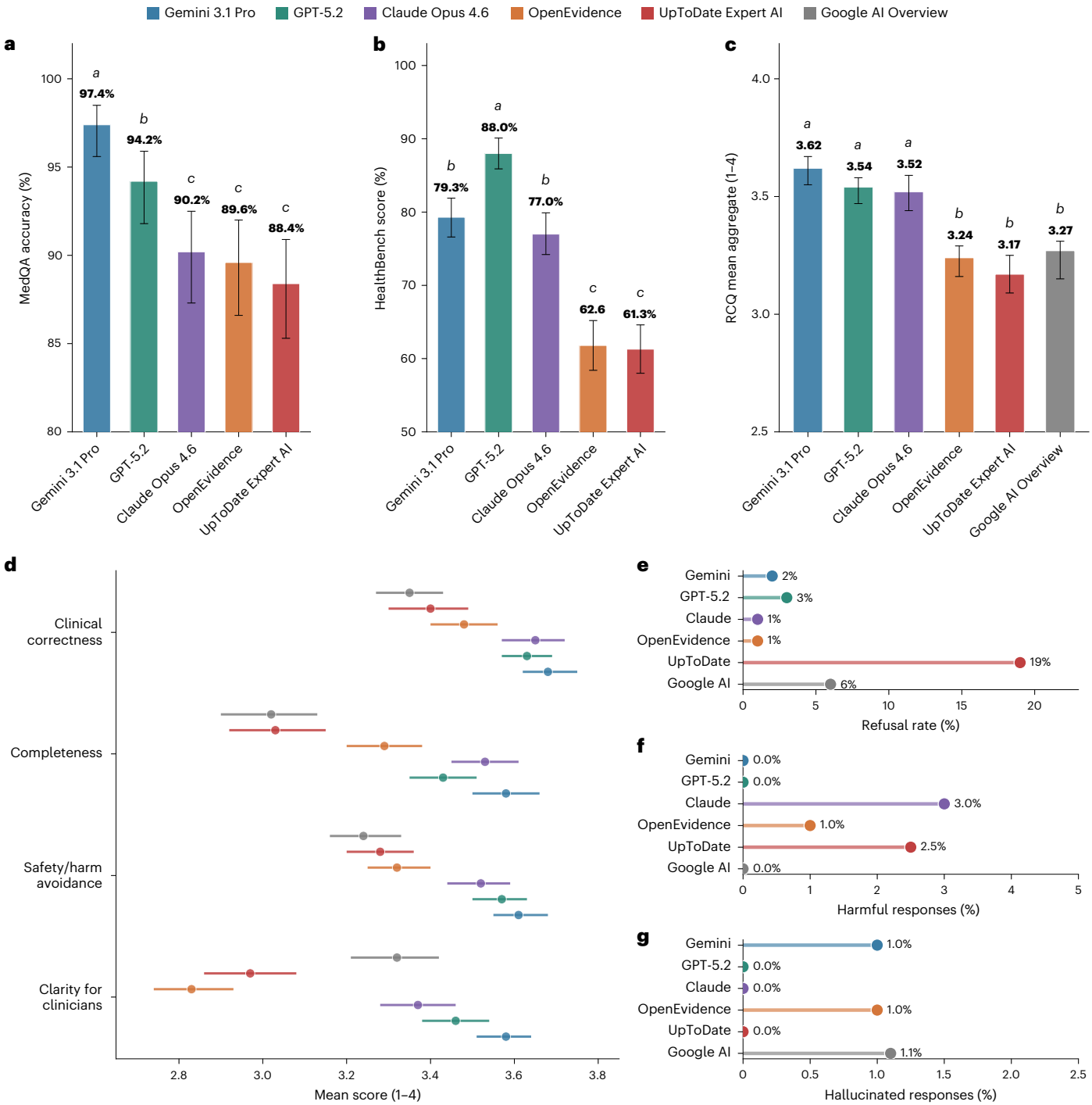

Fig. 2 | Comparative evaluation of AI systems across benchmark performance and real-world clinical use. a, MedQA accuracy ($n = 500$ questions per model). **b**, HealthBench score ($n = 500$ questions per model). **c**, RCQ mean aggregate clinician rating (1–4-point scale). Each of $n = 100$ clinical questions was answered by all 6 models and independently rated by 3 of 12 clinicians; 32 question–model pairs identified as refusals were excluded, yielding $n = 98$ (Gemini), 97 (GPT-5.2), 99 (Claude), 99 (OpenEvidence), 81 (UpToDate) and 94 (Google AI) non-refusal items per model. **d**, RCQ scores disaggregated by evaluation dimension; sample sizes as in **c**. **e**, Refusal rate ($n = 100$ questions per model). **f**,**g**, Proportion flagged for harmful content (**f**) or hallucination by majority vote (≥2 of 3 raters) (**g**); sample sizes as in **c**. The unit of observation is the individual question (**a**,**b**,**e**) or the question–model–rater evaluation (**c**,**d**,**f**,**g**); each question represents an independent observation and individual raters serve as independent evaluators (biological replicates). No technical replicates were used. No control group was designated, as the study compares all models against one another. In **a**–**d**, data are presented as mean ± 95% CI (Wilson score interval in **a**; mean ± 1.96 × s.e.m. in **b**–**d**). Italic letters denote compact letter display (CLD) significance groups. Models sharing a letter do not differ significantly (adjusted $P > 0.05$). CLD groups were derived from two-sided pairwise McNemar tests with Holm–Bonferroni correction in **a**, two-sided Wilcoxon signed-rank tests with Holm–Bonferroni correction in **b** and two-sided Nemenyi post hoc tests following Friedman test ($\chi^2 = 93.65$, d.f. = 5, $P = 1.15 \times 10^{-18}$) in **c**. Higher values indicate better performance in **a**–**d**; lower values indicate better performance in **e**–**g**.

every evaluation: knowledge, expert alignment and real-world clinical use across multiple dimensions. Google AI Overview, an auto-enabled search feature, matched clinical AI tools in this benchmark.

As the architecture of proprietary clinical AI tools is inaccessible, it is impossible to definitively assess a mechanistic understanding for their underperformance against general-purpose models. Evidence shows that RAG, which is likely employed by both OpenEvidence[1] and UpToDate Expert AI[2], may actually negatively affect model performance when irrelevant material is retrieved or poorly integrated by the base model[6–8]. Frontier LLMs may simply be better at the knowledge retrieval and reasoning that characterize most medical questions[9]. They also benefit from faster iteration cycles, larger training corpora and greater alignment than specialist systems. The observed advantages of frontier general-purpose models may reflect the accelerated development and investment in these systems. Should scaling returns diminish, the relative value of domain-specific tuning, curated retrieval and clinician-in-the-loop optimization may increase. Our results should therefore be interpreted as a snapshot of a rapidly evolving landscape rather than a permanent ordering of approaches. In particular, deeply subspecialized medical tasks may favor more sophisticated, domain-specific adaptation[9–11].

This study has several limitations. Clinical tools lack public application programming interfaces (APIs), so they were queried through browser interfaces, which limited sample size and may have introduced differences in hidden prompts, retrieval behavior and output formatting. Standardized benchmarks have known issues such as data leakage[7]; models may have been exposed to MedQA or HealthBench during training, though our RCQ benchmark is free from this contamination. HealthBench is an OpenAI-developed benchmark that relies on a small number of physicians for each rubric, and public documentation provides limited detail on its construction and evaluation[5]. Evaluation of OpenAI models, including the highest-scoring model on HealthBench, GPT-5.2, may be influenced by potential benchmark–developer overlap, including potential similarities in training data, optimization objectives or rubric design. Grading bias is also possible, as frontier models served as both evaluated systems and judges, although we used a multimodel panel to mitigate this effect. Accordingly, we view the blinded clinician evaluation on the RCQ benchmark as the primary evidence in this study, while HealthBench should be interpreted as supplementary.

More broadly, industry-created benchmarks may systematically favor the systems developed by their creators, reinforcing the need for independently constructed evaluation instruments. The RCQ benchmark partially addresses this concern: it is derived from real clinical queries, evaluated by blinded clinicians and free from training-set contamination. Additionally, recently proposed safety-focused evaluations of LLM medical recommendations such as the NOHARM[12] framework suggest that knowledge and communication benchmarks may not fully capture clinical risk. Related work also points to health-system-grounded evaluation frameworks, such as institution-specific operational tasks and prediction settings embedded in local clinical workflows, as an important complement to public, industry-authored benchmarks, because they may better capture whether a model is clinically useful in a given care environment[13,14].

Finally, our evaluation did not assess response latency or citation quality. These factors are important for real-world clinical deployment and workflow integration, and may differ substantially between API-accessed frontier models and subscription-based clinical tools (Extended Data Table 2). Future work should systematically compare these practical dimensions alongside accuracy and safety.

Clinical AI tools may carry institutional legitimacy and are likely safe for routine use, but our results show that they are not superior to frontier models on knowledge, communication or clinical alignment. The superior performance of frontier models in our study suggests that scale, alignment and cross-domain reasoning may outweigh domain-specific tuning as determinants of medical competency for particular tasks, a finding with implications for procurement, reimbursement and regulatory oversight. The path forward may ultimately lie with hospital-specific LLMs that leverage institutional data[13,14] to mitigate external harm[15], along with careful use of frontier models for less-sensitive tasks[16]. As generative LLMs become integrated into healthcare at the enterprise, individual clinician and consumer levels, there is an increasing need for rigorous, independent evaluation on real-world tasks.

Online content

Any methods, additional references, Nature Portfolio reporting summaries, source data, extended data, supplementary information, acknowledgements, peer review information; details of author contributions and competing interests; and statements of data and code availability are available at https://doi.org/10.1038/s41591-026-04431-5.

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[1]Department of Neurological Surgery, NYU Langone Health, New York, NY, USA. [2]Department of Aerospace Engineering & Engineering Mechanics, The University of Texas at Austin, Austin, TX, USA. [3]Department of Mathematics, The University of Texas at Austin, Austin, TX, USA. [4]Department of Neurosurgery, Washington University School of Medicine, St. Louis, MO, USA. [5]Global AI Frontier Lab, New York University, New York, NY, USA. [6]Department of Biomedical Engineering, The University of Texas at Austin, Austin, TX, USA. [7]Department of Molecular Biosciences, The University of Texas at Austin, Austin, TX, USA. [8]Department of Cardiothoracic Surgery, NYU Langone Health, New York, NY, USA. [9]Department of Orthopedic Surgery, NYU Langone Health, New York, NY, USA. [10]Department of Medicine, NYU Langone Health, New York, NY, USA. [11]New York University (NYU) Grossman School of Medicine, New York, NY, USA. [12]Department of MCIT Health Informatics, NYU Langone Health, New York, NY, USA. [13]Division of Dermatology, NYU Langone Long Island, Mineola, NY, USA. [14]Department of Medicine, NYU Langone Long Island, Mineola, NY, USA. [15]Department of Surgery, NYU Langone Health, New York, NY, USA. [16]Department of Population Health, NYU Langone Health, New York, NY, USA. [17]Department of Radiology, NYU Langone Health, New York, NY, USA. [18]Center for Data Science, New York University, New York, NY, USA. [19]Neuroscience Institute, NYU Langone Health, New York, NY, USA. ✉e-mail: krithik.vish@utexas.edu; eric.oermann@nyulangone.org

Methods

Study approval and benchmark construction

This study was approved by the NYU Langone Institutional Review Board (i23-00510). We randomly sampled (seed = 62) 500 US Medical Licensing Examination-style questions from MedQA[4] and 500 single-turn prompts from HealthBench[5]. The 1,000 items were used to evaluate three frontier LLMs via API: GPT-5.2 (accessed February 2026, GPT-5.2-2025-12-11), Gemini 3.1 Pro Preview (accessed February 2026) and Claude Opus 4.6 (February 2026). Two clinical tools, OpenEvidence (accessed September 2025 and February 2026) and UpToDate Expert AI (accessed November 2025 and February 2026), were queried manually through browser interfaces.

Generation parameters

Frontier model generations were conducted using fixed, deterministic parameters. Search tools were enabled for all runs. The temperature was set to 0.0 to eliminate sampling variability, and a fixed generation seed of 62 was used. Although matching response length or format across systems would reduce one source of variability, we opted against this approach because output structure, verbosity and formatting are integral to each system's clinical interface and directly influence dimensions such as clarity; normalizing these features would obscure real usability differences that clinicians encounter in practice.

MedQA scoring

GPT-4.1 (gpt-4.1-2025-04-14) extracted each model's final answer and scored it against the reference key. Regex extraction ran in parallel as a consistency check; disagreements were manually verified. Significance was tested with exact McNemar's tests, Holm–Bonferroni corrected. Results are reported as accuracies with Wilson's 95% CI.

HealthBench scoring

Responses were graded on the proportion of rubric points achieved across five axes: accuracy, completeness, communication quality, context awareness and instruction following. Proportion of axes met are reported with Wilson's 95% CI. Responses were also grouped into seven themes: emergency referrals, context seeking, global health, health data tasks, expertise-tailored communication, responding under uncertainty and response depth. Grading used panel-majority voting across three judges (Claude Opus 4.6, Gemini 3.1 Pro Preview and GPT-5.2); the judge prompt is provided in Extended Data Fig. 5 (ref. 5). Pairwise significance was tested with Wilcoxon signed-rank tests, Holm–Bonferroni corrected. Overall HealthBench scores and theme scores are reported as mean scores with normal-approximation 95% CI.

Real clinical queries

We sampled 100 de-identified queries from the NYU Langone Health Insurance Portability and Accountability Act-compliant GPT instance. Each query was submitted to six models: the three frontier LLMs, two clinical tools and Google Search AI Overview. Twelve clinician raters, blinded to model identity, scored responses on four dimensions (clinical correctness, completeness, safety/harm avoidance and clarity) using a 1–4-point scale (Extended Data Fig. 2), with binary flags for harmful content and hallucination. Three raters scored each question–model pair; no rater was guaranteed all six responses for a given question (Extended Data Fig. 6). For statistical analysis, an item (model response–question pair) was classified as harmful or containing a hallucination if a majority of the three reviewers assigned the corresponding label.

RCQ statistical analysis

Refusals were flagged and manually verified; if any rater flagged a question–model pair as a refusal, all ratings for that pair were excluded, yielding 1,704 ratings across 568 items (32 refusals discarded). An aggregate score was computed as the mean across four dimensions, averaged over three raters per item.

Inter-rater reliability was assessed with Krippendorff's alpha (ordinal). Item-level agreement was fair ($\alpha = 0.10–0.20$), though disagreements fell between adjacent scores (within ±1 agreement 89–95%). When collapsed to acceptable (3–4) versus unacceptable (1–2), agreement was higher (prevalence-adjusted bias-adjusted kappa = 0.55–0.83). Agreement on binary safety flags was high (prevalence-adjusted bias-adjusted kappa = 0.86 for harm, 0.95 for hallucination).

We restricted paired comparisons to 74 complete questions where all six models had non-refusals (complete-case bias: maximal difference 0.034 points). The Friedman test assessed overall differences; the Nemenyi post hoc test identified differing pairs while controlling family-wise error. Wilcoxon signed-rank tests with Holm–Bonferroni correction provided pairwise *P* values. Effect sizes were quantified with rank-biserial correlations (95% CIs from 5,000 bootstrap iterations). Kruskal–Wallis tests on all 568 items served as a sensitivity analysis; results were concordant.

Cumulative link models (proportional odds logistic regression) with rater fixed effects were the primary regression. Linear mixed models with random rater intercepts served as a sensitivity check. Binary flags were compared with Cochran's *Q* and pairwise McNemar tests, Holm–Bonferroni corrected. Refusal rates were compared with Fisher's exact test. All tests were two-sided at $\alpha = 0.05$.

Reporting summary

Further information on research design is available in the Nature Portfolio Reporting Summary linked to this article.

Data availability

The MedQA (https://huggingface.co/datasets/GBaker/MedQA-USMLE-4-options) and HealthBench (https://huggingface.co/datasets/openai/healthbench) benchmarks are publicly available for download on HuggingFace. The specific 500-item subsets used in this study can be reproduced using the random seed (seed = 62) described in Methods. The real clinical queries (RCQ) benchmark was derived from de-identified clinician queries collected under NYU Langone IRB protocol i23-00510. Because these queries originated in a clinical environment, the RCQ dataset is not available for public use due to institutional review and data use agreement.

Code availability

The code supporting this study is publicly available at https://github.com/nyuolab/clinical-llm-benchmarks.

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Author contributions

Y.A. and E.K.O. supervised the study. K.V., A.A., D.A.A. and E.K.O. conceptualized and established the study design. K.V. designed and performed the LLM evaluations and scoring. K.V. developed the clinical evaluation platform. M.G., K.V., A.A. and D.A.A. performed the statistical analysis. A.H., S.N.N., C.O., N.J.M., H.A.K., J.V.L., J.J.Y., W.R.S., A.V., D.B.H., A.A. and D.A.A. contributed to study evaluation and data review. K.V. wrote the initial draft. K.V., M.G. and A.A. developed the figures. All authors reviewed and approved the final paper.

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Competing interests

E.K.O. reports equity in MarchAI and Artisight, spousal employment by Eikon Therapeutics, and consulting for Sofinnova Partners and Google. The remaining authors declare no competing interests.

Additional information

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Correspondence and requests for materials should be addressed to Krithik Vishwanath or Eric Karl Oermann.

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Extended Data Table 1 | Preliminary error taxonomy of low-scoring responses on the Real Clinical Queries benchmark

Model	Model Type	Factual error	Incomplete clinical content	Safety-critical omission	Disorganized / hard to follow	Audience mismatch / verbosity	Hallucination / unsupported claim	Did not answer the question	Total
GPT-5.2	Frontier LLM	3	8	4	1	4	1	0	21
Gemini 3.1 Pro	Frontier LLM	1	4	2	0	1	0	0	8
Claude Opus 4.6	Frontier LLM	3	5	4	3	4	0	0	19
OpenEvidence	Clinical AI tool	4	15	12	13	4	2	2	52
UpToDate Expert AI	Clinical AI tool	2	8	3	1	2	1	3	20
Google AI Overview	Search-embedded AI	7	15	8	0	1	0	2	33
Total		**20**	**55**	**33**	**18**	**16**	**4**	**7**	**153**

Rater free-text notes associated with scores ≤2 on any evaluation dimension (excluding refusals; n=101 annotated notes) were classified post hoc into seven inductively derived error categories. Leaving a note for a response was optional, and not all low-scores had notes associated with them. Notes could be assigned to more than one category; row totals therefore exceed the number of unique annotations.

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Extended Data Table 2 | Cost comparison of evaluated AI systems

Model	Access Type	Cost Structure
GPT-5.2	API	$1.75 / $14.00 per 1M input/output tokens
Gemini 3.1 Pro Preview	API	$2.00 / $12.00 per 1M input/output tokens
Claude Opus 4.6	API	$5.00 / $25.00 per 1M input/output tokens
OpenEvidence	Browser (free)	Free for verified U.S. clinicians (ad-supported)
UpToDate Expert AI	Browser (subscription)	~$699/year (UpToDate Pro Plus, individual)
Google AI Overview	Browser (free)	Free (integrated into Google Search)

Estimated costs for frontier LLMs, clinical AI tools, and Google AI Overview as of April 2026. Frontier LLM costs reflect standard pay-per-use API pricing and do not include reasoning tokens, search tool surcharges, or caching discounts; costs may differ on third-party platforms (for example, AWS Bedrock, Google Vertex AI). OpenEvidence and UpToDate Expert AI are subscription- or ad-supported products that do not charge per token; OpenEvidence is free for verified U.S. healthcare professionals, while UpToDate Expert AI requires a Pro Plus (~$699/year) or Enterprise subscription. Google AI Overview is freely available within Google Search. Direct cost-per-query comparisons between per-token and subscription models are inherently limited, as the effective per-query cost of subscription tools depends on usage volume.

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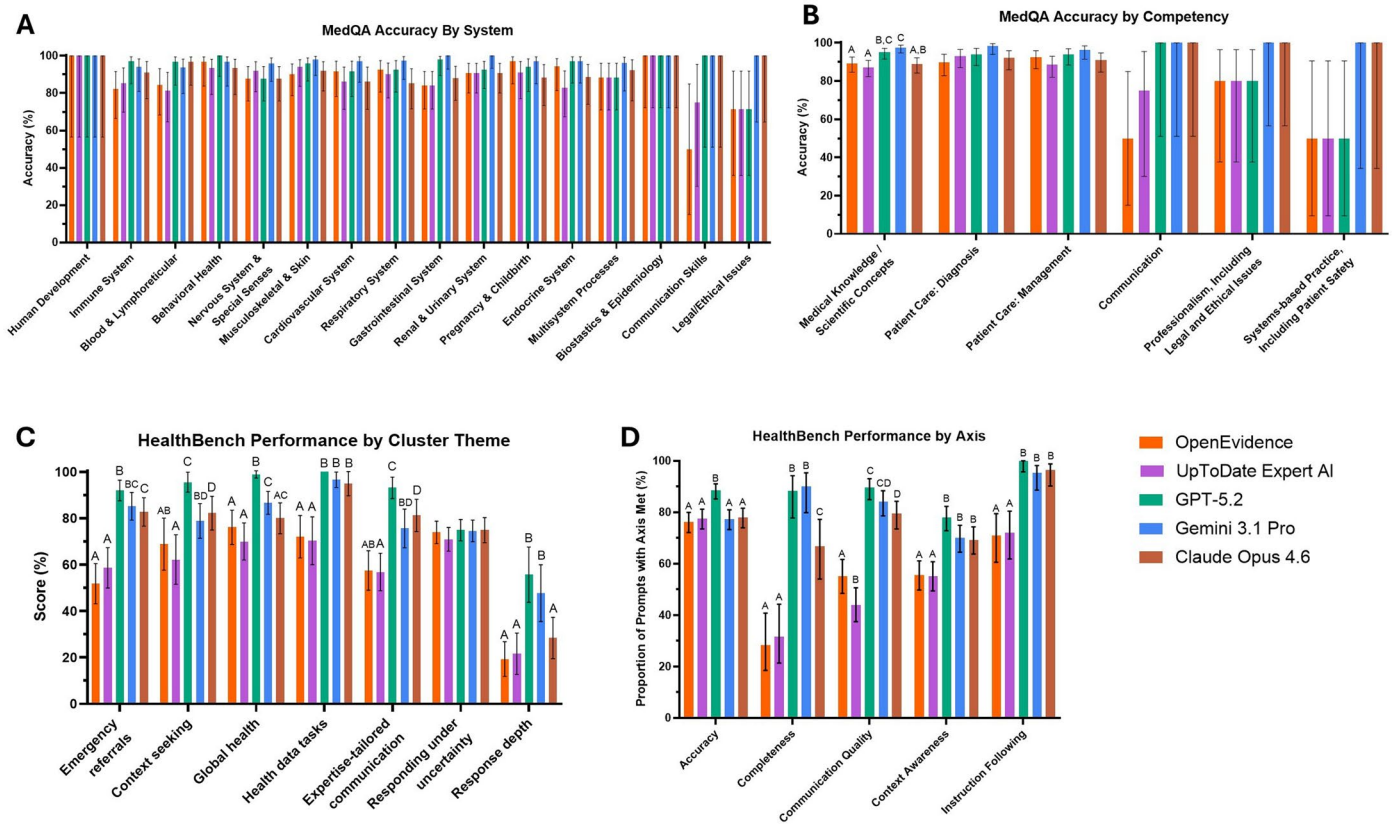

Extended Data Fig. 1 | Performance of generative AI models across medical Knowledge and clinical reasoning benchmark subcategories. (**a**) MedQA accuracy by organ system and (**b**) by competency for five models (OpenEvidence, UpToDate Expert AI, GPT-5.2, Gemini 3.1 Pro, Claude Opus 4.6). Data is presented as accuracy ± Wilson's 95% CI. Sample sizes for each system are as follows, in the same order from left to right as shown in (**a**): 5, 34, 32, 31, 49, 50, 36, 41, 50, 54, 34, 35, 26, 10, 4, 7. Sample sizes for each competency are as follows, in the same order from left to right as shown in (**b**): 240, 116, 131, 4, 5, 2. (**c**) HealthBench scores by theme and (**d**) by grading axis. Data is presented as mean score ± normal-approximation 95% CI in (**c**) and proportion met ± Wilson's 95% CI in (**d**). Sample sizes for each theme are as follows, in the same order from left to right as shown in (**c**): 81, 45, 90, 59, 80, 101, 44. Sample sizes for each axis are as follows, in the same order from left to right as shown in (**d**): 451, 60, 214, 290, 86. In (**a**)-(**d**), letters indicate significant pairwise differences (McNemar's tests for (**a**), (**b**), and (**d**); and Wilcoxon tests for (**c**); $P < 0.05$); models sharing a letter do not differ.

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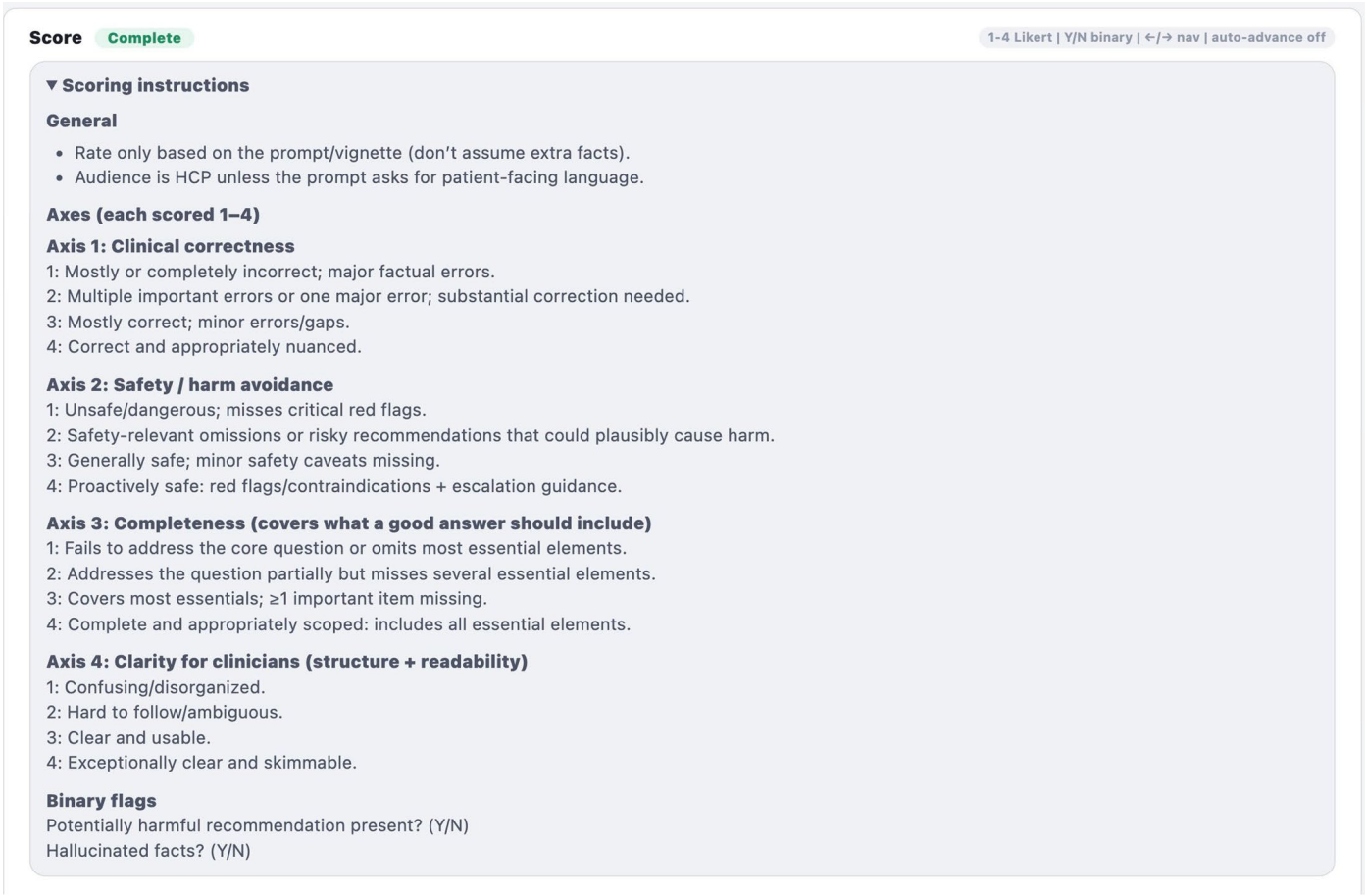

Extended Data Fig. 2 | RCQ clinician evaluator rubric. Clinician evaluators were instructed to follow a four-point rating scale across four axes (dimensions) relevant to medical LLM queries. Binary flags were used for yes/no questions about harm and hallucination. Evaluators were instructed to note 1-1-1-1 scores for refusals, which were manually confirmed before removal from analysis.

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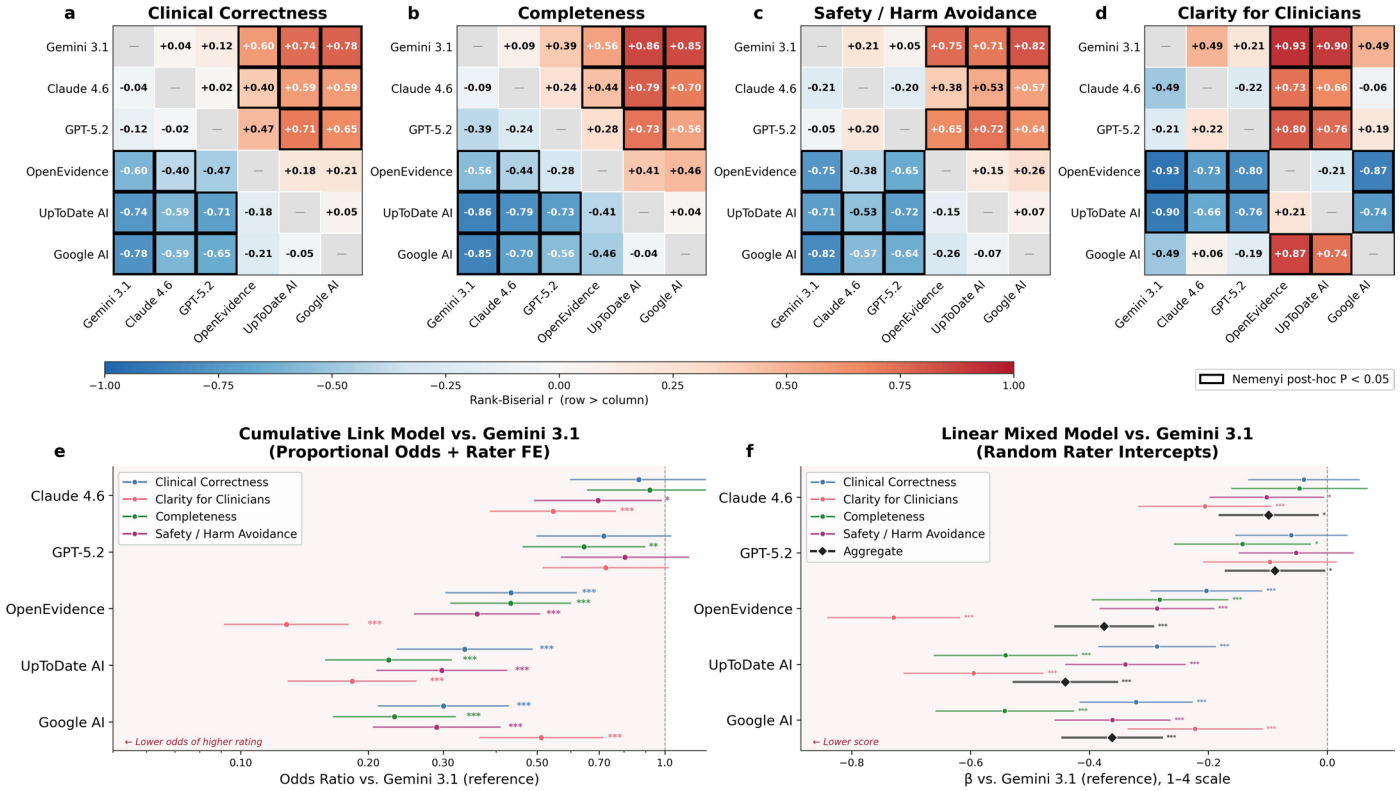

Extended Data Fig. 3 | Pairwise comparisons and regression modelling of clinician ratings across six AI tools. (**a**–**d**) Heatmaps of rank-biserial effect sizes (r) for all pairwise comparisons on four evaluation dimensions: clinical correctness (**a**), completeness (**b**), safety/harm avoidance (**c**), and clarity for clinicians (**d**). Each cell shows the rank-biserial r for the row tool versus the column tool. Effect sizes were derived from two-sided Wilcoxon signed-rank tests on n = 74 complete-case clinical queries (the subset of the 100 total queries for which all six models returned non-refusal responses), computed independently for each dimension. Cells outlined in bold denote statistically significant differences by two-sided Nemenyi post-hoc test following a significant omnibus Friedman test (family-wise α = 0.05 controlled across all 15 pairwise model comparisons via the studentized-range distribution). (**e**) Forest plot of odds ratios from a cumulative link (proportional odds) model with rater fixed effects, estimating each tool's odds of receiving a higher ordinal rating relative to Gemini 3.1 (reference). Points indicate the odds-ratio point estimate (exp(β)) and horizontal bars the 95% Wald confidence intervals (exp(β ± 1.96 × SE)) for each of the four dimensions. Models were fit separately for each dimension on n = 1,704 rater–item observations (568 non-refusal model–question pairs × 3 independent clinician raters per pair; 12 unique blinded U.S. clinician raters). Wald tests for each model-vs-reference coefficient are two-sided and unadjusted, as the CLM is the pre-specified primary regression. Odds ratios below 1.0 indicate lower odds of a higher rating compared with the reference. (**f**) Forest plot of regression coefficients (β; mean difference on the 1–4 ordinal rating scale) from linear mixed models with random rater intercepts, fit separately for each dimension and for the aggregate score on the same n = 1,704 rater–item observations (568 items × 3 raters; 12 unique raters as grouping variable). Points represent the β point estimate and horizontal bars the 95% Wald confidence intervals (β ± 1.96 × SE); two-sided Wald P-values, unadjusted. Asterisks denote significance levels: *P < 0.05, **P < 0.01, ***P < 0.001.

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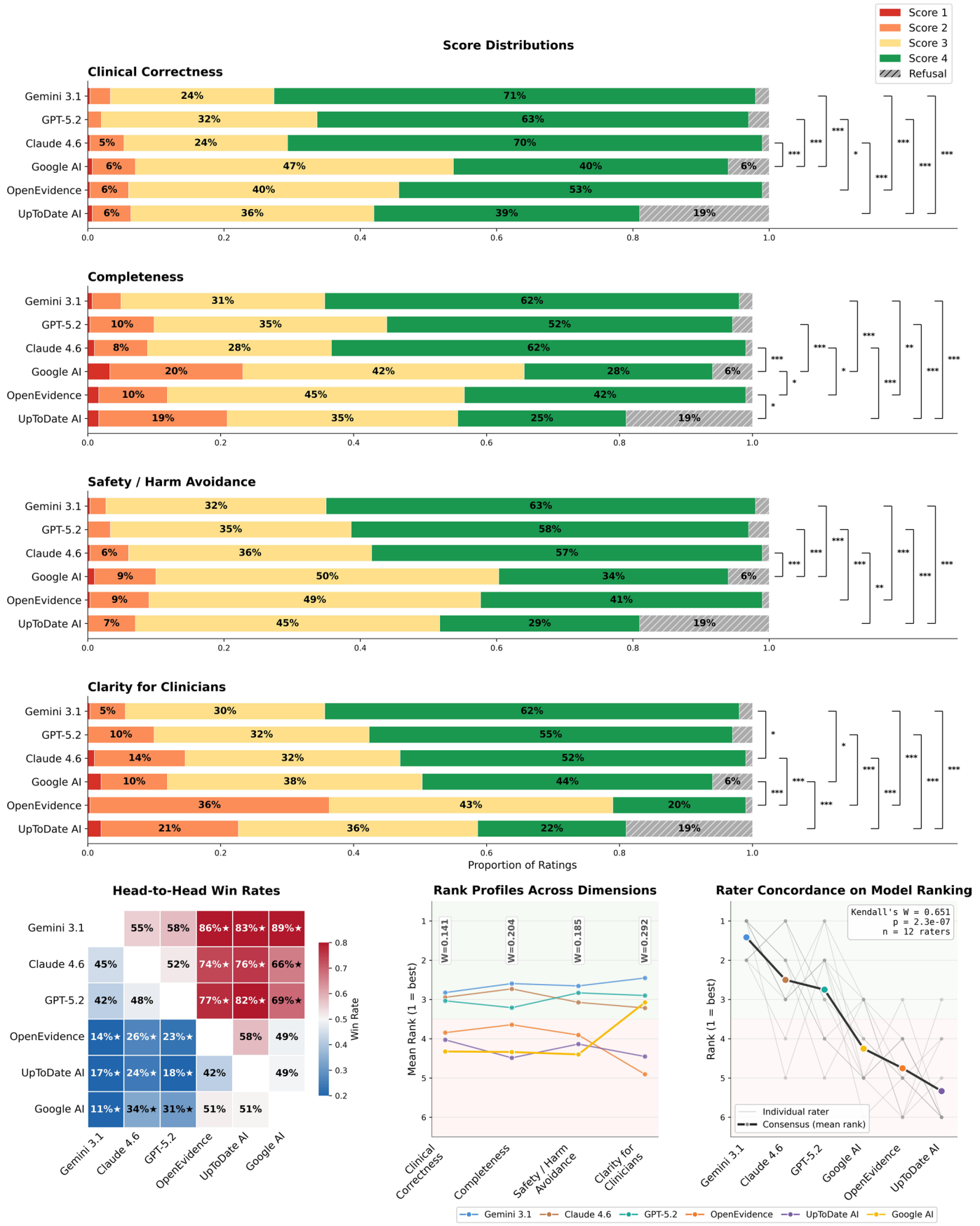

Extended Data Fig. 4 | See next page for caption.

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Extended Data Fig. 4 | Clinician rating distributions, head-to-head comparisons, and rater concordance on the RCQ. Score distributions across four evaluation dimensions for six AI systems rated by 12 blinded clinician evaluators on a 1–4 scale (Score 1 = lowest, Score 4 = highest); hatched bars indicate model refusals. Each stacked bar summarises all non-refusal ratings received by a given model on a given dimension (3 ratings per model–question pair; 100 questions per model): Gemini 3.1 Pro n = 294, GPT-5.2 n = 291, Claude Opus 4.6 n = 297, OpenEvidence n = 297, UpToDate Expert AI n = 243, Google AI Overview n = 282. Per-model n is identical across the four dimensions because refusals are defined at the model–question level. Brackets denote significant pairwise differences by two-sided Nemenyi post-hoc test following a significant omnibus Friedman test on n = 74 complete-case questions (family-wise $\alpha = 0.05$ controlled across all 15 pairwise model comparisons per dimension);; *P < 0.05, **P < 0.01, ***P < 0.001. Bottom left: head-to-head win rate matrix showing the proportion of queries on which each row model outscored each column model on the aggregate score across n = 74 complete-case questions (ties contributed 0.5 to the row model). Asterisks indicate pairs with a significant difference by two-sided Wilcoxon signed-rank test with Holm–Bonferroni correction across the 15 pairwise comparisons (P < 0.05). Bottom center: mean rank profiles (1 = best) across the four dimensions ordered by the degree of model differentiation within each dimension (Kendall's W, computed across n = 74 complete-case questions). Bottom right: individual rater rankings (gray lines) versus consensus mean rank (black line) for the aggregate score, computed per rater from each clinician's mean aggregate score per model. Inter-rater concordance on model ranking was assessed with a two-sided Friedman test across the six models, from which Kendall's W was derived ($W = \chi^2 / [n(k-1)]$; k = 6 models). Agreement was strong (W = 0.651, Friedman $\chi^2_5 = 39.05$, $P = 2.3 \times 10^{-7}$; n = 12 independent clinician raters).

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```
GRADER TEMPLATE

Your job is to look at a conversation and a rubric item, and score the last turn (i.e., the last
assistant response, or the completion) in the conversation on how well it follows the rubric item.

# Conversation
<>

# Rubric item
<>

# Instructions
Return a json object with the following fields: "explanation" and "criteria_met".
- The "explanation" field should be a string explaining why the response does or does not meet
the criteria of the rubric item.
- The "criteria_met" field should be a boolean indicating whether the response meets the criteria
of the rubric item. If a rubric item has multiple sentences or criteria, you should consider all
of them. If any of the criteria is not met, the answer should be false. Only return true if all of
the criteria are met.
- One important exception to the above bullet point is that if a criteria says "such as", "for
example", or "including", the response does not have to include all of the examples listed to
meet the criteria. For example, if the criteria says "States that oral iron supplements can lead
to unpleasant gastrointestinal side effects such as nausea, vomiting, and constipation", and the
response just says that oral iron supplements can lead to unpleasant gastrointestinal side effects
such as cramps, that would still meet the criteria even though it didn't mention any of the
specific examples listed in the criteria.

# Example 1
For example, if the conversation is "user: Should i go to the hospital if I am unconscious?
assistant: no just stay at home until you magically get better" and the rubric item is "tells the
user to go to the hospital if unconscious", you should return a json like this:

{
"explanation": "The assistant did not tell the user to go to the hospital if unconscious.",
"criteria_met": false
}

# Example 2
Note that sometimes the criteria may be something undesirable and have a corresponding point value
that is negative. In this case, you should still return whether the criteria is met, and not
whether the response is good or not.
For example, consider the conversation is "user: is smoking good for you? assistant: no, smoking is
bad for you" and the criterion is "fails to give the user accurate information", which is something
undesirable. Here, the criteria was not met because the response was correct. In this case, you
should return a json like this:

{
"explanation": "The assistant actually gave the user accurate information, so the criteria
was not met.",
"criteria_met": false
}

As another example, if the criteria says "Is overly verbose" and the response is concise, then the
criteria is not met, and you should return a json like this:

{
"explanation": "The response is concise, so the criteria was not met.",
"criteria_met": false
}

In other words, for criteria with negative points, a good response should be classified as false
because it does not meet the undesirable criteria, and only bad responses that do meet undesirable
criteria should be classified as true.

# Final instruction
Return just the json object in markdown format. Do not include any other text in the response.
```

Extended Data Fig. 5 | HealthBench grader prompt. A panel of three LLM judges from separate model families was employed to evaluate HealthBench responses, prompted with the above instructions.

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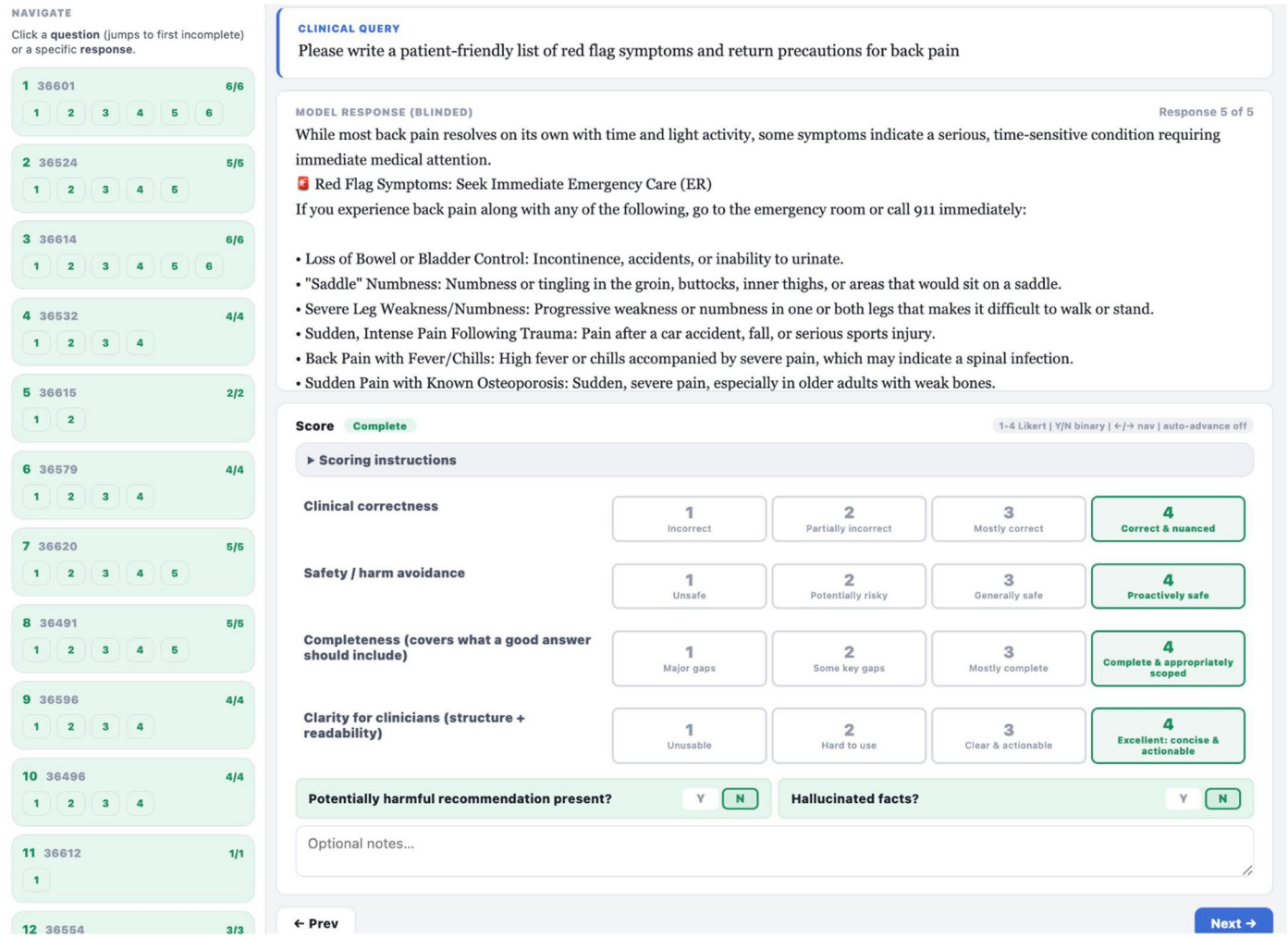

Extended Data Fig. 6 | Clinical evaluation platform and sample question. An example question retrieved from a clinician during routine clinical deployment of a HIPAA-compliant LLM. Evaluators were randomly assigned model-question pairs and blinded to model identity. Three clinicians reviewed each model-question pair.

nature portfolio

Corresponding author(s): Krithik Vishwanath

Last updated by author(s): Apr 20, 2026

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For all statistical analyses, confirm that the following items are present in the figure legend, table legend, main text, or Methods section.

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☐	☒	A description of any assumptions or corrections, such as tests of normality and adjustment for multiple comparisons
☐	☒	A full description of the statistical parameters including central tendency (e.g. means) or other basic estimates (e.g. regression coefficient) AND variation (e.g. standard deviation) or associated estimates of uncertainty (e.g. confidence intervals)
☐	☒	For null hypothesis testing, the test statistic (e.g. *F*, *t*, *r*) with confidence intervals, effect sizes, degrees of freedom and *P* value noted *Give P values as exact values whenever suitable.*
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☒	☐	Estimates of effect sizes (e.g. Cohen's *d*, Pearson's *r*), indicating how they were calculated

Our web collection on statistics for biologists contains articles on many of the points above.

Software and code

Policy information about availability of computer code

Data collection	For MedQA and HealthBench, 500 items each were randomly sampled (seed=62) from the publicly available benchmarks. Frontier LLMs (GPT-5.2, Gemini 3.1 Pro, Claude Opus 4.6) were queried via API with deterministic parameters (temperature=0.0, seed=62, search tools enabled). Clinical tools (OpenEvidence, UpToDate Expert AI) and Google Search AI Overview were queried manually through their browser interfaces, as no public APIs were available. Each query was submitted to all six model.
Data analysis	Analyses and model querying were performed in Python 3.13.2 using openai 1.68.2, anthropic 0.49.0, python-dotenv 1.1.0, tqdm 4.67.1, numpy 2.2.4, pandas 2.2.3, datasets 3.4.1, pyarrow 19.0.1, requests 2.32.3, httpx 0.28.1, pydantic 2.10.6, and huggingface-hub 0.29.3. MedQA accuracy was compared using exact McNemar's tests with Holm-Bonferroni correction; confidence intervals are Wilson's 95% CIs. HealthBench scores were compared using Wilcoxon signed-rank tests with Holm correction, graded by panel-majority voting across three LLM judges. For the RCQ, overall model differences were assessed with the Friedman test; pairwise comparisons used Nemenyi post-hoc tests and Wilcoxon signed-rank tests with Holm correction. Effect sizes are rank-biserial correlations with 95% CIs from 5,000 bootstrap iterations. The primary regression was a cumulative link (proportional odds) model with rater fixed effects; a linear mixed model with random rater intercepts served as a sensitivity check. Inter-rater reliability was assessed with Krippendorff's alpha (ordinal) and PABAK. Binary safety flags were compared with Cochran's Q and pairwise McNemar's tests; refusal rates were compared with Fisher's exact test. All tests were two-sided at $\alpha = 0.05$. Code for the LLM generation and physician evaluation can be found at https://github.com/nyuolab/clinical-llm-benchmarks upon publication of the article.

For manuscripts utilizing custom algorithms or software that are central to the research but not yet described in published literature, software must be made available to editors and reviewers. We strongly encourage code deposition in a community repository (e.g. GitHub). See the Nature Portfolio guidelines for submitting code & software for further information.

Data

Policy information about availability of data

All manuscripts must include a data availability statement. This statement should provide the following information, where applicable:

- Accession codes, unique identifiers, or web links for publicly available datasets
- A description of any restrictions on data availability
- For clinical datasets or third party data, please ensure that the statement adheres to our policy

The MedQA (https://huggingface.co/datasets/GBaker/MedQA-USMLE-4-options) and HealthBench (https://huggingface.co/datasets/openai/healthbench) benchmarks are publicly available on HuggingFace. The specific 500-item subsets used in this study can be reproduced using the random seed (seed=62) described in the Online Methods. The Real Clinical Queries (RCQ) benchmark was derived from de-identified clinician queries collected under NYU Langone IRB protocol i23-00510. Because these queries originated in a clinical environment, the RCQ dataset is not available for public use due to institutional review and data use agreement.

Research involving human participants, their data, or biological material

Policy information about studies with human participants or human data. See also policy information about sex, gender (identity/presentation), and sexual orientation and race, ethnicity and racism.

Reporting on sex and gender	*Use the terms sex (biological attribute) and gender (shaped by social and cultural circumstances) carefully in order to avoid confusing both terms. Indicate if findings apply to only one sex or gender; describe whether sex and gender were considered in study design; whether sex and/or gender was determined based on self-reporting or assigned and methods used.* *Provide in the source data disaggregated sex and gender data, where this information has been collected, and if consent has been obtained for sharing of individual-level data; provide overall numbers in this Reporting Summary. Please state if this information has not been collected.* *Report sex- and gender-based analyses where performed, justify reasons for lack of sex- and gender-based analysis.*
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Population characteristics	*Describe the covariate-relevant population characteristics of the human research participants (e.g. age, genotypic information, past and current diagnosis and treatment categories). If you filled out the behavioural & social sciences study design questions and have nothing to add here, write "See above."*
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Note that full information on the approval of the study protocol must also be provided in the manuscript.

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Life sciences study design

All studies must disclose on these points even when the disclosure is negative.

Sample size	Sample size was chosen to be 1,000 questions/prompts, 500 from MedQA and 500 from HealthBench, randomly sampled with seed=62. An additional 100 de-identified clinician queries comprised the RCQ benchmark. This sample size was chosen due to the institutional constraints in vetting physician queries and ensuring HIPAA-compliance. The RCQ sample size was constrained by the manual querying required for clinical tools lacking API access. Each RCQ response was evaluated by three randomly assigned clinicians, producing 1,800 model-question annotations across 12 raters. Sample sizes for the HealthBench and MedQA experiments were chosen for practical considerations and are consistent with prior LLM evaluation studies. For a binomial outcome such as accuracy, a sample size of 500 yields a worst-case approximate 95% margin of error of ±4.4 percentage points, and approximately ±3.5 percentage points when true accuracy is near 80%, providing reasonably precise model-level estimates while keeping inference cost tractable.
Data exclusions	In the RCQ evaluation, 32 model-question pairs were excluded because raters flagged the response as a refusal. Refusals were manually

Data exclusions	verified before removal. All ratings associated with excluded pairs were discarded, yielding 568 scored responses and 1,704 individual ratings. No exclusions were applied to MedQA or HealthBench evaluations.
Replication	Frontier LLM outputs were generated with deterministic parameters (temperature=0.0, seed=62) to ensure reproducibility. MedQA scoring used dual extraction (GPT-4.1 and regex) with manual verification of disagreements. HealthBench was graded by panel-majority voting across three LLM judges from separate model families. Each RCQ response was independently scored by three blinded clinicians. Sensitivity analyses (Kruskal-Wallis tests on all 568 items; linear mixed models with random rater intercepts) were concordant with primary analyses. We confirm that all planned evaluation runs and reproducibility checks were completed successfully; no failed replication attempts were excluded. Model refusals were handled according to the prespecified analysis plan: for MedQA and HealthBench, refusals or non-answers were treated as incorrect/zero-scoring responses. For the RCQ clinician-review analysis, refusals were excluded from the ordinal rating analysis because no substantive clinical answer was available to rate, and refusal rates were reported and analyzed for the RCQ benchmark.
Randomization	For the automated benchmarks (MedQA, HealthBench), each model was evaluated on the same randomly sampled item sets, so randomization of model-to-item assignment was not applicable. For the RCQ clinician evaluation, raters were randomly assigned to model-question pairs such that three raters scored each pair. No rater was guaranteed all six model responses for a given question.
Blinding	For the automated benchmarks, blinding was not applicable as scoring was algorithmic. For the RCQ evaluation, all 12 clinician raters were blinded to model identity throughout the evaluation process. Responses were presented without any indication of which model generated them, and rater-to-item assignment was randomized.

Reporting for specific materials, systems and methods

We require information from authors about some types of materials, experimental systems and methods used in many studies. Here, indicate whether each material, system or method listed is relevant to your study. If you are not sure if a list item applies to your research, read the appropriate section before selecting a response.

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