

Structured Prompting Enables More Robust, Holistic Evaluation of Language Models

Asad Aali, Muhammad Ahmed Mohsin, Vasiliki Bikia, Arnav Singhvi, Richard Gaus, Suhana Bedi, Hejie Cui, Miguel Fuentes, Alyssa Unell, Yifan Mai, Jordan Cahoon, Michael Pfeffer, Roxana Daneshjou, Sanmi Koyejo, Emily Alsentzer, Percy Liang, Christopher Potts, Nigam H. Shah, Akshay S. Chaudhari

Stanford University

Abstract

As language models (LMs) are increasingly adopted across domains, high-quality benchmarking frameworks that accurately estimate performance are essential for guiding deployment decisions. While frameworks such as Holistic Evaluation of Language Models (HELM) enable broad evaluation across tasks, they often rely on fixed prompts that fail to generalize across LMs, yielding unrepresentative performance estimates. Unless we estimate each LM’s ceiling (maximum achievable via changes to the prompt), we risk underestimating performance. Declarative prompting frameworks, such as DSPy, offer a scalable alternative to manual prompt engineering by crafting structured prompts that can be optimized per task. However, such frameworks have not been systematically evaluated across established benchmarks. We present a reproducible *DSPy+HELM* framework that introduces structured prompting methods which elicit reasoning, enabling more accurate LM benchmarking. Using four prompting methods, we evaluate four frontier LMs across seven benchmarks (general/medical domain) against existing HELM baseline scores. We find that without structured prompting: (i) HELM underestimates LM performance (by 4% average), (ii) performance estimates vary more across benchmarks (+2% standard deviation), (iii) performance gaps are misrepresented (leaderboard rankings flip on 3/7 benchmarks), and (iv) introducing reasoning (*chain-of-thought*) reduces LM sensitivity to prompt design (smaller performance Δ across prompting methods). To our knowledge, this is the first large-scale benchmarking study to empirically characterize LM behavior across benchmarks and prompting methods, showing that scalable performance ceiling estimation enables more decision-useful benchmarks. We open-source (i) *DSPy+HELM* Integration¹ and (ii) Prompt Optimization Pipeline².

1 Introduction

Language models (LMs) have rapidly advanced in text generation, spurring deployment across diverse domains (Thirunavukarasu et al., 2023; Van Veen et al., 2024; Seo et al., 2024). Yet, integrating LMs into downstream workflows remains challenging as LMs frequently commit errors (Aali et al., 2025). Even state-of-the-art general-purpose frontier LMs exhibit non-trivial hallucination rates (Wang et al., 2024a; Sivarajkumar et al., 2024; Bang et al., 2025; Tamber et al., 2025). Such concerns are compounded by LMs’ sensitivity to prompt design (Razavi et al., 2025), introducing variability in leaderboard performance.

While benchmarking frameworks such as Holistic Evaluation of Language Models (HELM) (Liang et al., 2022; Bedi et al., 2025) enable holistic evaluation via a comprehensive suite covering diverse tasks, public leaderboards typically evaluate multiple LMs under a fixed prompt per benchmark. However, fixed prompts rarely generalize well across LMs, leading to unrepresentative performance estimates that obscure underlying strengths and weaknesses of LMs. Hence, broader LM adoption necessitates scalable estimation of performance ceilings (i.e., the maximum achievable via prompt-only changes), thereby allowing practitioners to weigh cost–benefit tradeoffs and choose the right model for each downstream task.

¹*DSPy+HELM* Integration: <https://github.com/stanford-crfm/helm/pull/3893>

²Prompt Optimization Pipeline: <https://github.com/StanfordMIMI/dspy-helm>

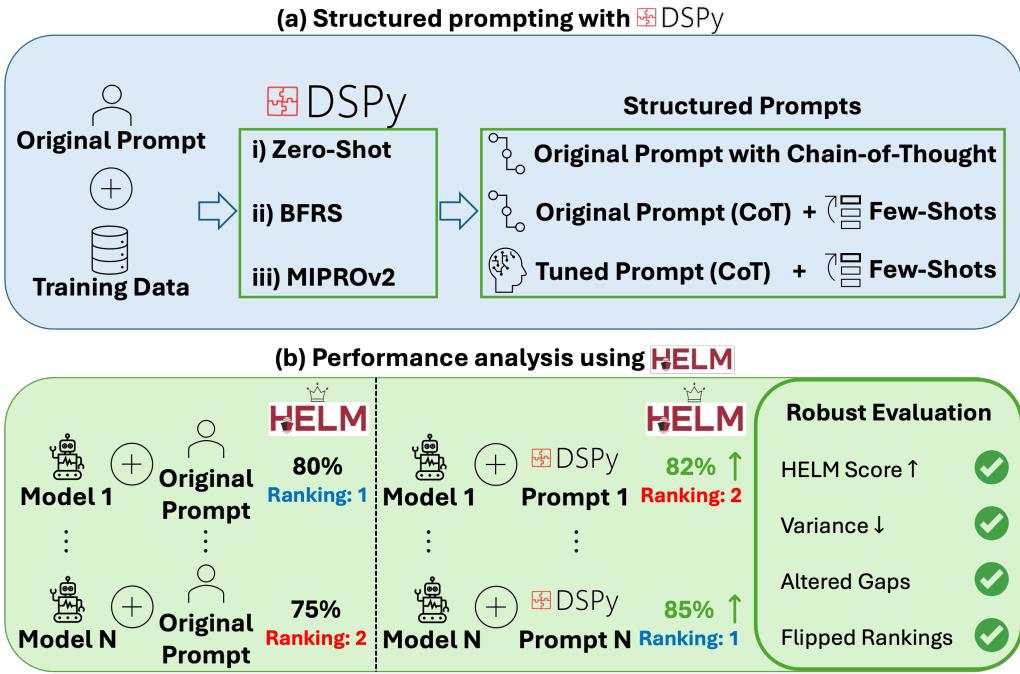


Figure 1: Pipeline overview. (a) DSPy takes HELM’s baseline prompt and produces structured prompt variants. (b) HELM evaluates models under each prompt variant. With structured prompting, we observe more robust evaluation: (i) improved performance, (ii) reduced variance, (iii) altered gaps (flipped rankings).

Prompt engineering has emerged as a practical alternative to fine-tuning. Well-designed prompts improve performance, as demonstrated by Nori et al. (2024); Maharjan et al. (2024), combining few-shot selection, chain-of-thought (CoT) (Wei et al., 2022), and ensembling. However, these methods rely on hand-engineered prompts, demanding domain expertise and iterative experimentation, making them labor-intensive and often non-robust to new model rollouts (Wang et al., 2025). Consequently, researchers have explored automatic prompt optimization (APO) (Li et al., 2025), which treats prompt design as an optimization problem.

DSPy (Khattab et al., 2023) is a widely used declarative framework that represents prompts as modular, parameterized components with an intuitive structure that allows moving from zero-shot prompts to more adaptive prompting styles all within a single unified system supporting reproducible, structured prompting. Moreover, DSPy supports automatic prompt optimizers (APOs) such as MIPROv2 (Opsahl-Ong et al., 2024), which can convert high-level task specifications into optimized instructions and few-shot examples.

However, declarative prompting frameworks such as DSPy have not been systematically evaluated across established benchmarks, and there is limited guidance on how structured prompting compares with existing baselines. To address this gap, we integrate DSPy with HELM (Figure 1) and present:

1. A reproducible *DSPy+HELM* framework that introduces structured prompting methods which elicit reasoning, enabling more robust evaluation of LMs across HELM benchmarks.
2. An evaluation of prompting methods (Zero-Shot, Bootstrap Few-Shot with Random Search, MIPROv2) against HELM’s baseline across four LMs and seven HELM benchmarks that span general and medical domains (reasoning, knowledge QA, problem-solving, error-classification), where each prompting method leverages a distinct mechanism for refining prompts to estimate LM performance ceilings.
3. Empirical evidence that without structured prompting: (i) HELM underestimates LM performance (by 4% average), (ii) performance estimates vary more across benchmarks (+2% standard deviation), (iii) performance gaps are misrepresented (leaderboard rankings flip on 3/7 benchmarks), and (iv) introducing reasoning (CoT) reduces LM sensitivity to prompt design (smaller Δ across prompts).

Prompt 1: HELM Baseline

Given a patient note and a clinical question, compute the requested medical value.
Patient Note and Question: -----

Prompt 2: Zero-Shot CoT

Your input fields are: “INPUTS”
Your output fields are: “REASONING” and “OUTPUT”
Your objective is: Given the fields “INPUTS”, produce the fields “OUTPUT”

INPUTS:

Given a patient note and a clinical question, compute the requested medical value.
Patient Note and Question: -----

Respond with the corresponding output fields, starting with “REASONING”, then “OUTPUT”.

Prompt 3: BFRS (Few-Shot Optimized)

Your input fields are: “INPUTS”
Your output fields are: “REASONING” and “OUTPUT”
Your objective is: Given the fields “INPUTS”, produce the fields “OUTPUT”

IN-CONTEXT EXAMPLES (K Demos):

INPUTS: <input text> → REASONING: <steps>, OUTPUT: <output text>

INPUTS:

Given a patient note and a clinical question, compute the requested medical value.
Patient Note and Question: -----

Respond with the corresponding output fields, starting with “REASONING”, then “OUTPUT”.

Prompt 4: MIPROv2 (Instruction + Few-Shot Optimized)

Your input fields are: “INPUTS”
Your output fields are: “REASONING” and “OUTPUT”
Your objective is: You are a highly skilled medical expert working in a busy emergency room. A patient presents with a complex medical history and concerning symptoms. The attending physician needs your immediate assistance in calculating a critical risk score to guide treatment decisions. The patient’s life may depend on your accuracy.

IN-CONTEXT EXAMPLES (K Demos):

INPUTS: <input text> → REASONING: <steps>, OUTPUT: <output text>

INPUTS:

Given a patient note and a clinical question, compute the requested medical value.
Patient Note and Question: -----

Respond with the corresponding output fields, starting with “REASONING”, then “OUTPUT”.

Figure 2: Structured prompting methods evaluated in our study (Zero-Shot CoT, BFRS, MIPROv2). Each box corresponds to one method, showing how instructions and context differ across methods. For BFRS and MIPROv2, K denotes the number of in-context demonstrations (Inputs → Reasoning, Output).

Model	API Identifier	Release	Context	Reasoning
Claude 3.7 Sonnet	anthropic/clause-3-7-sonnet-20250219	02/19/2025	200k	✗
Gemini 2.0 Flash	google/gemini-2.0-flash-001	02/01/2025	1000k	✗
GPT 4o	openai/gpt-4o-2024-05-13	05/13/2024	128k	✗
o3 Mini	openai/o3-mini-2025-01-31	01/31/2025	200k	✓

Table 1: Language models evaluated in our study. Columns show API identifiers, release dates, maximum context windows, and native reasoning modes (yes/no). We choose widely used models to evaluate whether prompting meaningfully affects even top-tier frontier models. All experiments were run in August 2025.

2 Methodology

DSPy (Khattab et al., 2023) is a framework for composing modular LM pipelines. Formally, let Φ denote a LM program with m modules. Each module i has a prompt template p_i containing a set of variables (open slots) for the instruction and K demonstration examples. Let V be the set of all such prompt variables across Φ , and let $V \rightarrow S$ denote an assignment of each variable $v \in V$ to a concrete string $s \in S$. We write $\Phi_{V \rightarrow S}$ to denote running program Φ under a particular prompt assignment. Given a dataset $D = (x, y)$ of inputs x with ground-truth y and an evaluation metric μ that compares the program’s output $\Phi(x)$ against y , the optimization maximizes μ over all instructions and demonstrations:

$$\Phi^* = \arg \max_{V \rightarrow S} \frac{1}{|D|} \sum_{(x, y) \in D} \mu(\Phi_{V \rightarrow S}(x), y). \quad (1)$$

2.1 Prompting Methods

Baseline Prompting

As a baseline, we evaluate LMs using the following prompting methods:

- 1. HELM Baseline.** HELM supports multiple prompting configurations; we adopt the commonly reported fixed, zero-shot (hand-crafted) prompt configuration without CoT as the baseline for comparison.
- 2. Zero-Shot Predict.** DSPy’s Zero-Shot Predict configuration is an unoptimized non-adaptive baseline, which we instantiate with the `dspy.Predict` module. Each module’s instruction prompt is initialized with the same HELM baseline instruction, without in-context demonstrations (i.e. $K = 0$).

Structured Prompting

In addition, we evaluate LMs using the following structured prompting methods (Figure 2):

- 1. Zero-Shot CoT.** DSPy’s Zero-Shot CoT configuration utilizes the same prompting structure as Zero-Shot Predict, but instead instantiates the `dspy.ChainOfThought` module, which elicits step-by-step rationales, instructing the LM to generate an explicit reasoning trace with the output.
- 2. BFRS.** Bootstrap Few-Shot with Random Search (BFRS) (Algorithm 1) leverages the idea of bootstrapping and random sampling to select the best few-shot demonstrations (fixed instructions) in two phases: (i) Bootstrapping demonstrations: the LM program Φ is run on a subset of training inputs to gather traces for each module. Whenever the output of $\Phi(x)$ for an example x achieves a sufficiently high score (on metric μ), the input-output pair is taken as a candidate demonstration. (ii) Random few-shot search: Given demonstration pools, BFRS randomly samples sets of K demonstrations per module, inserts them into the module, and evaluates the program on a validation split. After trying N combinations, the program with the highest score is returned (with hyperparameters K and N).

Benchmark	Input → Output	Task	Samples
MMLU-Pro	Reasoning Question → Answer	Multi-Task Reasoning	1,000
GPQA	Graduate Question → Answer	Graduate-Level QA	446
GSM8K	Math Problem → Solution	Numeric Problem-Solving	1,000
MedCalc-Bench	Patient Note → Computed Value	Computational Reasoning	1,000
Medec	Medical Narrative → Errors	Error Classification	597
HeadQA	Medical Question → Answer	USMLE-Style QA	1,000
MedBullets	Medical Question → Answer	USMLE-Style QA	308

Table 2: HELM benchmarks (publicly available) evaluated in our study. Columns summarize each benchmark’s input → output mapping, underlying task type, and number of test samples. The benchmarks span reasoning, knowledge QA, problem-solving, and error-classification tasks across both general and medical domains.

3. MIPROv2. MIPROv2 (Algorithm 2) is an optimizer that jointly selects instructions and K few-shot demonstrations via: (i) bootstrapping demos, (ii) grounded instruction proposals from a proposer LM conditioned on dataset summaries, program structure, exemplar demos, and trial history, and (iii) Bayesian search over instruction-demo pairs. It treats each configuration \mathbf{v} as hyperparameters, learns $p(y | \mathbf{v})$ from trial outcomes, and steers toward high-scoring regions. For efficiency, candidates are scored on mini-batches of size B , with periodic full-dataset D evaluations of top contenders; the best full-data configuration is returned (with hyperparameters instruction text, demo-set, and K).

2.2 Benchmarks

We choose seven benchmarks (Table 2) based on (i) public availability, (ii) task diversity (reasoning, knowledge QA, problem-solving, error classification), and (iii) domain coverage (general/medical).

MMLU-Pro. MMLU-Pro (Wang et al., 2024b) is an enhanced version of MMLU that focuses on more challenging, reasoning-intensive questions. It expands answer choices from four to ten and removes trivial items, providing a more discriminative measure of higher-order reasoning. The metric μ is exact match.

GPQA. GPQA (Rein et al., 2024) is a graduate-level multiple-choice benchmark covering biology, physics, and chemistry to test advanced reasoning. The metric μ is the fraction of correct answers (exact match).

GSM8K. GSM8K (Cobbe et al., 2021) consists of grade school math word problems designed to evaluate reasoning. The task requires computing a final numeric answer, and the metric μ is exact match.

MedCalc-Bench. MedCalc-Bench (Khandekar et al., 2024) is a medical calculation benchmark, where the input is a patient note and a question asking for a numerical/categorical value. The evaluation metric μ is exact match for the *risk*, *severity*, and *diagnosis* categories, and a within-range correctness check for others.

Medec. Medec (Abacha et al., 2024) is an error detection and correction benchmark, where each input contains a narrative that may contain factual errors, and the task is to identify/correct these errors. The evaluation metric μ involves checking how accurately LMs identify whether a note contains an error (binary).

HeadQA. HeadQA (Vilares & Gómez-Rodríguez, 2019) is a collection of biomedical multiple-choice questions for testing medical knowledge, where questions cover medical knowledge and often resemble medical board exams. The performance metric μ is exact match between the prediction and the correct option.

MedBullets. MedBullets (Medbullets, 2025) is a benchmark of USMLE-style medical questions with multiple-choice answers. MedBullets covers broad topics and is designed to reflect the difficulty of medical licensing exams. Like HeadQA, the primary metric μ is exact match accuracy on the correct answer.

Algorithm 1 BFRS: Bootstrap Few-Shot with Random Search

Require: Seed program Φ_{seed} ; train/val sets $D_{\text{tr}}, D_{\text{val}}$; threshold τ ; demos per module K_i ; trials R ; minibatch size B .

- 1: **Bootstrap:** For each $(x, y) \in D_{\text{tr}}$: run Φ_{seed} ; if $\mu(\Phi_{\text{seed}}(x), y) \geq \tau$, then for each module i add $(u_i(x), \Phi_{\text{seed}}^{(i)}(u_i(x)))$ to \mathcal{B}_i .
- 2: **Search:** For $r=1:R$:
- 3: **for** $i = 1:m$ **do**
- 4: Sample $S_i^{(r)} \leftarrow \text{SampleK}(\mathcal{B}_i, K_i)$
- 5: Let $\mathbf{v}^{(r)} \leftarrow (I_1^{\text{seed}}, S_1^{(r)}, \dots, I_m^{\text{seed}}, S_m^{(r)})$.
- 6: Draw minibatch $\mathcal{B} \subset D_{\text{val}}$ with $|\mathcal{B}| = B$; compute $\hat{J}_B(\mathbf{v}^{(r)})$ by equation 5.
- 7: **Select:** $\mathbf{v}^* \in \arg \max_r \hat{J}_B(\mathbf{v}^{(r)})$; optionally re-evaluate $J(\mathbf{v}^*)$ on full D_{val} .
- 8: **Return** \mathbf{v}^* and the resulting $\Phi_{\mathbf{v}^*}$.

Algorithm 2 MIPROv2: Joint Optimization of Instructions & Demos

Require: Train/val sets $D_{\text{tr}}, D_{\text{val}}$; candidate sizes T_i (instructions), K_i (demos per module); minibatch size B ; escalation period E ; TPE quantile γ ; trials T .

- 1: **Bootstrap demos:** Build $\{\mathcal{B}_i\}_{i=1}^m$ as in equation 3.
- 2: **Propose instructions:** For each i , sample $\mathcal{I}_i = \{I_i^{(t)}\}_{t=1}^{T_i}$ from proposer LM using task/program-aware context.
- 3: Initialize history $\mathcal{H}_0 \leftarrow \emptyset$; best full-eval $(\mathbf{v}^\dagger, J^\dagger) \leftarrow (\text{seed}, 0)$.
- 4: **for** $t = 1:T$ **do**
- 5: Fit/update TPE from \mathcal{H}_{t-1} to obtain ℓ, g in equation 6.
- 6: **Acquire candidate:**
- 7:
$$\mathbf{v}^{(t)} \in \arg \max_{\mathbf{v} \in \prod_i (\mathcal{I}_i \times \mathcal{B}_i^{K_i})} \frac{\ell(\mathbf{v})}{g(\mathbf{v})}.$$
- 8: Draw minibatch $\mathcal{B} \subset D_{\text{val}}$, $|\mathcal{B}| = B$, score $y^{(t)} = \hat{J}_B(\mathbf{v}^{(t)})$.
- 9: Append to history: $\mathcal{H}_t \leftarrow \mathcal{H}_{t-1} \cup \{(\mathbf{v}^{(t)}, y^{(t)})\}$.
- 10: **if** $t \bmod E = 0$ **then**
- 11: Select top- K by running mean; evaluate each on full D_{val} to get $J(\cdot)$.
- 12: If any $J(\mathbf{v}) > J^\dagger$ then update $(\mathbf{v}^\dagger, J^\dagger) \leftarrow (\mathbf{v}, J(\mathbf{v}))$.
- 13: **Return** \mathbf{v}^\dagger and $\Phi_{\mathbf{v}^\dagger}$.

Prompting Method	Claude 3.7 Sonnet	Gemini 2.0 Flash	GPT 4o	o3 Mini
HELM Baseline	$64.81\% \pm 22.6$	$61.41\% \pm 23.8$	$61.04\% \pm 23.9$	$70.93\% \pm 19.7$
Zero-Shot Predict	$65.10\% \pm 22.6$	$61.69\% \pm 22.7$	$59.69\% \pm 25.0$	$73.24\% \pm 20.3$
Zero-Shot CoT	$69.36\% \pm 18.8$	$66.21\% \pm 20.9$	$65.67\% \pm 22.5$	$72.73\% \pm 19.7$
BFRS	$69.34\% \pm 19.0$	$66.19\% \pm 21.2$	$65.87\% \pm 22.9$	$73.07\% \pm 19.7$
MIPROv2	$69.80\% \pm 19.0$	$66.19\% \pm 21.1$	$65.34\% \pm 23.0$	$73.07\% \pm 19.6$
Ceiling – Baseline (Δ)	+4.99%	+4.80%	+4.83%	+2.31%

Table 3: HELM leaderboard (macro-averaged over seven benchmarks) across four language models and five prompting methods. **Green** marks the "ceiling" performance for a model (best value across prompting methods). Entries are reported as the macro-average \pm standard deviation σ over seven benchmarks. At each model's ceiling, structured prompting on average leads to +4% in accuracy and -2% in σ across benchmarks.

2.3 Experimental Setup

Implementation details. We evaluate four frontier LMs (Table 1) to study whether prompting meaningfully impacts the performance of even top-tier LMs. We seed each DSPy program with the baseline instruction for the corresponding benchmark from HELM's prompt. For BFRS and MIPROv2 optimizers, we follow DSPy's data separation: the demonstration pool is bootstrapped *exclusively* from the training split, while candidate prompts are evaluated on a *disjoint* held-out validation split from the original training partition; neither optimizer ever sees the HELM leaderboard test set. Each benchmark's loader creates a fixed train/val partition (default 90/10 with the same seed), and we cap both bootstrapped and labeled demonstrations at $K \leq 3$ per module. All final scoring is performed via HELM, so outputs are judged identically regardless of how they were produced. All results reflect single, deterministic runs (temperature = 0), matching HELM's experimental setup. For HELM baselines, we report HELM's public leaderboard scores when the setup matches ours: (i) identical LM API version, (ii) zero-shot prompting, and (iii) no CoT reasoning. For benchmarks where the leaderboard setup does not match, we reproduce them with single, deterministic runs.

Metric calculation. To summarize overall gains, we take the mean of the three structured prompting methods (Zero-Shot CoT, BFRS, MIPROv2); for each LM, we first macro-average across benchmarks, and then average the Δ (absolute % change over baseline) across LMs. The change in variability is reported analogously using the per-model across-benchmark standard deviation, σ . For each model and prompting method, σ is computed over per-benchmark scores (equal weight per benchmark).

3 Results and Discussion

3.1 Impact of Structured Prompting on HELM Leaderboard

Improved performance over HELM baseline. Across LMs and benchmarks, structured prompting methods (Zero-Shot CoT, BFRS, MIPROv2) consistently improve over the HELM baseline (Table 3). On average, LMs gain +4% in absolute accuracy. Non-reasoning models benefit most (+5%), while the reasoning-optimized *o3 Mini* sees smaller but consistent gains (+2%).

Flipped leaderboard rankings. Taking ceiling performance into account alters three leaderboard rankings. On MMLU-Pro (Table 4), baseline *o3 Mini* > *Claude 3.7 Sonnet* (77.1% vs. 76.3%) reverses to *Claude 3.7 Sonnet* > *o3 Mini* (80.6% vs. 78.4%). On GSM8K, *GPT 4o* overtakes *Gemini 2.0 Flash*, shifting from (81.1% vs. 84.0%) to (90.7% vs. 84.2%). Finally, on MedCalc-Bench (Table 5), baseline *o3 Mini* > *Claude 3.7 Sonnet* (34.0% vs. 21.0%) becomes *Claude 3.7 Sonnet* > *o3 Mini* (35.3% vs. 34.7%), highlighting how prompting can act as a confounder in leaderboard rankings.

Benchmark	Prompting Method	Claude 3.7 Sonnet	Gemini 2.0 Flash	GPT 4o	<i>o3</i> Mini
MMLU-Pro	HELM Baseline	76.3% \pm 2.7	66.1% \pm 3.0	62.2% \pm 3.0	77.1% \pm 3.1
	Zero-Shot Predict	77.7% \pm 2.6	70.3% \pm 2.8	60.7% \pm 3.1	78.4% \pm 3.1 ↓
	Zero-Shot CoT	79.7% \pm 2.5	75.3% \pm 2.7	67.6% \pm 3.0	76.2% \pm 3.1
	BFRS	80.1% \pm 2.5	75.4% \pm 2.7	71.1% \pm 2.8	76.5% \pm 3.1
	MIPROv2	80.6% \pm 2.5 ↑	75.3% \pm 2.7	68.7% \pm 2.9	76.1% \pm 3.1
GPQA	HELM Baseline	57.0% \pm 4.7	53.4% \pm 4.7	45.5% \pm 4.7	57.6% \pm 4.5
	Zero-Shot Predict	62.1% \pm 4.5	54.5% \pm 4.7	41.7% \pm 4.5	66.6% \pm 4.3
	Zero-Shot CoT	61.4% \pm 4.7	59.2% \pm 4.5	52.5% \pm 4.7	66.4% \pm 4.5
	BFRS	64.1% \pm 4.5	61.0% \pm 4.5	49.3% \pm 4.7	65.5% \pm 4.5
GSM8K	MIPROv2	61.9% \pm 4.5	59.0% \pm 4.5	47.8% \pm 4.7	68.4% \pm 4.3
	HELM Baseline	80.5% \pm 2.5	84.0% \pm 2.3	81.1% \pm 2.5	88.6% \pm 2.0
	Zero-Shot Predict	83.0% \pm 2.3	77.3% \pm 2.6	84.6% \pm 2.2	93.6% \pm 1.6
	Zero-Shot CoT	83.3% \pm 2.3	83.1% \pm 2.4	90.7% \pm 1.8 ↑	92.6% \pm 1.7
	BFRS	83.2% \pm 2.3	84.2% \pm 2.3 ↓	90.4% \pm 1.9	93.0% \pm 1.6
	MIPROv2	84.0% \pm 2.3	83.5% \pm 2.3	89.8% \pm 2.0	93.4% \pm 1.6

Table 4: HELM leaderboard (general domain) across four language models and five prompting methods. **Green** marks the "ceiling" performance for a model (best value across prompting methods). ↑ and ↓ indicate a one-step increase or decrease in leaderboard rank, respectively. Entries are reported as mean \pm 95% bootstrap confidence interval. Overall, structured prompting consistently improves the robustness of benchmarks.

Altered inter-model performance gaps. When evaluated at ceiling performance (best score across prompting methods), models can either narrow or widen their relative performance gaps, providing a more accurate view of true capability differences. Averaging across benchmarks, the gap between the top two models (*o3 Mini* and *Claude 3.7 Sonnet*) shrinks from 6% at baseline (70.9 vs. 64.8) to 3% (73.2 vs. 69.8). However, this trend is not uniform: on GPQA, the gap widens substantially, from 0.6% at baseline (57.6 vs. 57.0) to 4.3% at ceiling (68.4 vs. 64.1).

Reduced across-benchmark variance. Structured prompting methods reduce dispersion. Across-benchmark σ drops for *Claude 3.7 Sonnet* (22.6% \rightarrow 18.8%), *Gemini 2.0 Flash* (23.8% \rightarrow 20.9%), and *GPT 4o* (23.9% \rightarrow 22.5%), while *o3 Mini* is unchanged (19.7%), indicating lower sensitivity.

Benchmark-dependent sensitivity. Performance gains vary across benchmarks (Figure 3). Tasks requiring reasoning, such as MMLU-Pro, GPQA, GSM8K, MedCalc-Bench, and MedBullets, show the largest gains (average +5.5% absolute across models). In contrast, HeadQA and Medec exhibit smaller improvements (average +0.4% absolute across models). We hypothesize that HeadQA is bottlenecked by high baseline scores (~90%), while Medec likely reflects fundamental limits in the LM’s knowledge base.

Ranking stability analysis. To assess how structured prompting shifts relative model rankings, we compute mean ranks (1 = best, 4 = worst) across all seven benchmarks. Under structured prompting, the ranking spread compresses: *o3 Mini* remains the top model but becomes less dominant (1.29 \rightarrow 1.57), *Claude 3.7 Sonnet* improves and moves closer to the top (2.29 \rightarrow 2.00), *GPT 4o* also improves modestly (3.14 \rightarrow 3.00), while *Gemini 2.0 Flash* slightly declines (3.29 \rightarrow 3.43). Rank standard deviation (σ) shows a similar but more moderate pattern: *Claude 3.7 Sonnet* (0.95 \rightarrow 1.15) and *GPT 4o* (0.90 \rightarrow 1.00) become slightly more volatile, while *Gemini 2.0 Flash* stabilizes (0.76 \rightarrow 0.53), and *o3 Mini* remains roughly unchanged (0.76 \rightarrow 0.79). While effects are modest (only 3 instances of ranking flips across leaderboards), the results demonstrate that benchmark rankings are not invariant to prompt design.

Benchmark	Prompting Method	Claude 3.7 Sonnet	Gemini 2.0 Flash	GPT 4o	<i>o3</i> Mini
MedCalc-Bench	HELM Baseline	21.0% \pm 2.5	15.8% \pm 2.3	18.8% \pm 2.5	34.0% \pm 3.0
	Zero-Shot Predict	20.6% \pm 2.6	17.0% \pm 2.4	15.7% \pm 2.3	33.4% \pm 2.9
	Zero-Shot CoT	35.3% \pm 3.0 \uparrow	26.3% \pm 2.7	26.6% \pm 2.8	34.2% \pm 3.0
	BFRS	34.1% \pm 3.0	25.2% \pm 2.7	27.0% \pm 2.8	34.7% \pm 3.0 \downarrow
	MIPROv2	34.7% \pm 3.0	25.4% \pm 2.7	26.8% \pm 2.8	34.3% \pm 3.0
Medec	HELM Baseline	62.8% \pm 3.9	59.6% \pm 4.0	58.0% \pm 3.9	68.7% \pm 3.9
	Zero-Shot Predict	58.3% \pm 3.9	59.3% \pm 4.0	57.3% \pm 4.0	68.3% \pm 3.9
	Zero-Shot CoT	61.8% \pm 4.0	59.5% \pm 4.0	59.5% \pm 4.0	68.2% \pm 3.9
	BFRS	60.5% \pm 3.9	59.1% \pm 4.0	59.5% \pm 4.0	69.2% \pm 3.7
	MIPROv2	62.5% \pm 3.9	60.8% \pm 4.0	59.8% \pm 4.0	68.3% \pm 3.7
HeadQA	HELM Baseline	91.2% \pm 1.8	88.0% \pm 2.1	90.6% \pm 1.8	89.3% \pm 1.9
	Zero-Shot Predict	88.7% \pm 2.0	88.5% \pm 2.1	86.4% \pm 2.1	90.9% \pm 1.8
	Zero-Shot CoT	92.2% \pm 1.7	89.3% \pm 1.9	90.7% \pm 1.8	90.0% \pm 1.9
	BFRS	92.0% \pm 1.8	88.9% \pm 1.9	91.1% \pm 1.8	90.1% \pm 1.9
	MIPROv2	92.2% \pm 1.7	89.5% \pm 2.0	91.1% \pm 1.8	89.5% \pm 2.0
MedBullets	HELM Baseline	64.9% \pm 5.5	63.0% \pm 5.5	71.1% \pm 5.2	81.2% \pm 4.6
	Zero-Shot Predict	65.3% \pm 5.5	64.9% \pm 5.2	71.4% \pm 5.2	81.5% \pm 4.2
	Zero-Shot CoT	71.8% \pm 5.2	70.8% \pm 5.2	72.1% \pm 5.2	81.5% \pm 4.6
	BFRS	71.4% \pm 5.2	69.5% \pm 5.2	72.7% \pm 5.2	82.5% \pm 4.6
	MIPROv2	72.7% \pm 5.2	69.8% \pm 5.2	73.4% \pm 4.9	81.5% \pm 4.6

Table 5: MedHELM leaderboard (medical domain) across four language models and five prompting methods. **Green** marks the "ceiling" performance for a model (best value across prompting methods). \uparrow and \downarrow indicate a one-step increase or decrease in leaderboard rank, respectively. Entries are reported as mean \pm 95% bootstrap confidence interval. Overall, structured prompting consistently improves the robustness of benchmarks.

3.2 Chain-of-Thought vs Automatic Prompt Optimization

Performance gains primarily attributed to CoT. We study the impact of each prompting method on the leaderboard by averaging results across LMs and benchmarks. Moving from HELM’s baseline to Zero-Shot Predict yields minimal improvement (64.6% \rightarrow 64.9%). In contrast, introducing CoT reasoning and moving from Zero-Shot Predict to Zero-Shot CoT results in substantial gains (64.9% \rightarrow 68.5%). Interestingly, moving from Zero-Shot CoT to more sophisticated optimizers, such as BFrs and MIPROv2, does not lead to a meaningful additional improvement (68.5% \rightarrow 68.6%), indicating that once CoT is introduced, LMs become relatively insensitive to further optimization.

Why CoT reduces sensitivity to prompt design? We provide empirical evidence that reasoning LMs such as *o3 Mini*, or non-reasoning LMs with prompt-induced reasoning (CoT), are relatively insensitive to prompt design (Meincke et al., 2025). Now, we provide deeper insight into this phenomenon. With CoT, the model marginalizes over many reasoning paths; prompt optimization mostly reweights sampled paths. A model with parameters θ reads an input x (a benchmark item) under a prompt p , samples a reasoning path τ (chain-of-thought), and produces a final answer y ; we compare this to an alternative prompt p' (a structured prompt variant). CoT factorizes the joint as: (1) $P_\theta(\tau, y | x, p) = P_\theta(\tau | x, p) P_\theta(y | x, \tau, p)$ and predicts by marginalizing over paths (self-consistency): (2) $P_\theta(y | x, p) = \sum_{\tau} P_\theta(\tau | x, p) P_\theta(y | x, \tau, p)$. Because (2) is a marginalization of (1), the data-processing inequality implies that the change in the

Increase in Accuracy over HELM Baseline

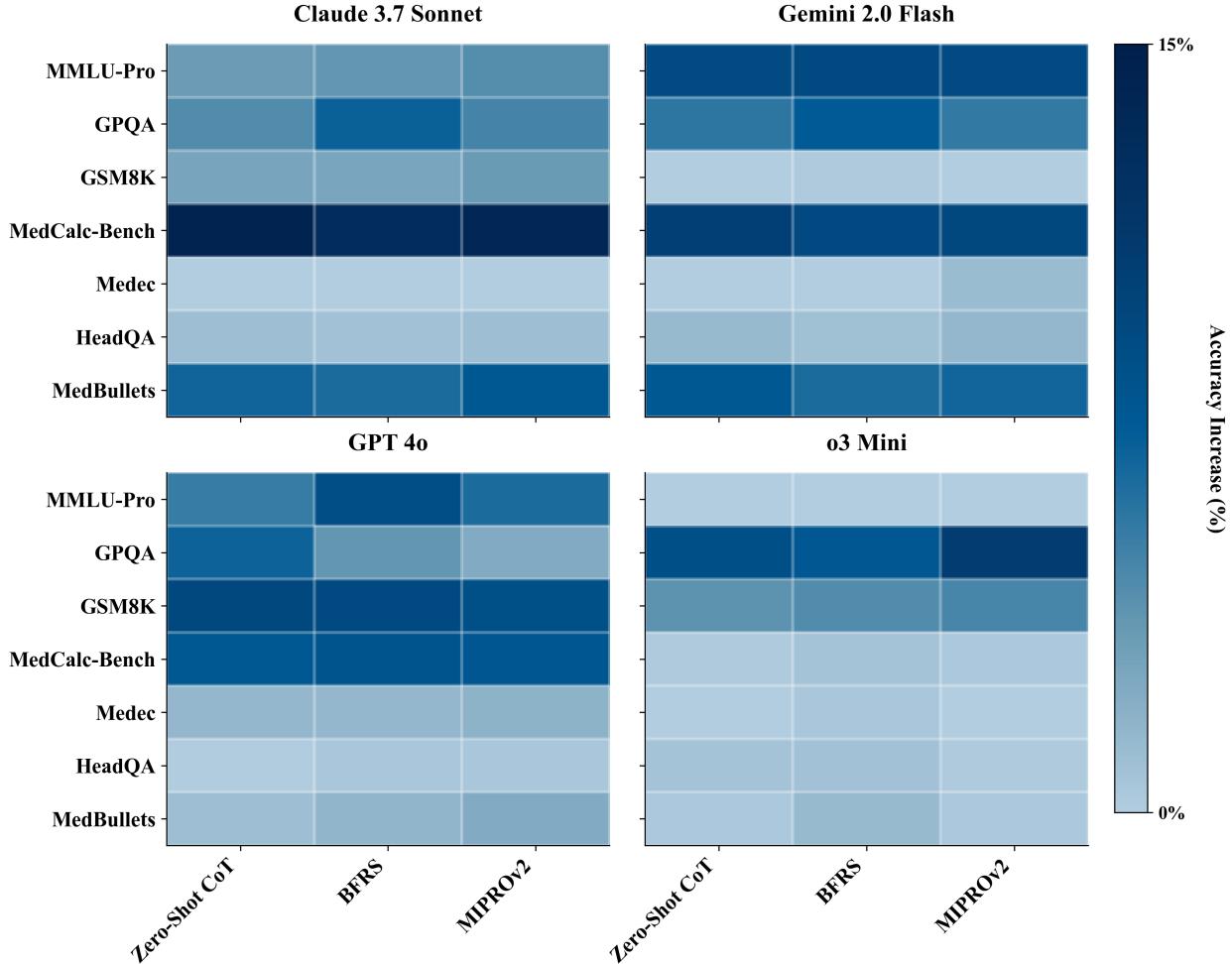


Figure 3: Heat map showing Δ (increase in accuracy) of each prompting method over HELM’s baseline (light=small, dark=large). Across four models, x-axis lists prompting methods, y-axis lists benchmarks. All structured prompting methods exhibit similar improvements, while *o3 Mini* remains relatively insensitive.

answer distribution between prompts p and p' is upper-bounded by the change in the path distribution: $\|P_\theta(y | x, p) - P_\theta(y | x, p')\|_{TV} \leq \|P_\theta(\tau | x, p) - P_\theta(\tau | x, p')\|_{TV} \leq \sqrt{\frac{1}{2} D_{KL}(P_\theta(\tau | x, p) \| P_\theta(\tau | x, p'))}$ where $\|\cdot\|_{TV}$ is total-variation distance and $KL(\cdot \| \cdot)$ is Kullback–Leibler divergence (applying the data-processing inequality followed by Pinsker’s inequality). We define the decision margin at item x under prompt p as $m(x; p) = P_\theta(y^* | x, p) - \max_{y \neq y^*} P_\theta(y | x, p)$, $y^* = \arg \max_y P_\theta(y | x, p)$. If the left-hand side of the previous bound is $< \frac{1}{2} m(x; p)$, then the prediction is invariant: $\arg \max_y P_\theta(y | x, p') = \arg \max_y P_\theta(y | x, p)$. Since CoT enlarges $m(x; p)$ by averaging diverse valid paths, typical prompt optimization, which mainly reweights $P_\theta(\tau | x, p)$, cannot flip decisions except on near-tied items, making reasoning models largely prompt-insensitive. If $KL(P_\theta(\tau | x, p) \| P_\theta(\tau | x, p')) \leq \kappa$ and $m(x; p) \geq 2\varepsilon$, then predictions are invariant under $p \rightarrow p'$ whenever $\sqrt{\kappa/2} < \varepsilon$.

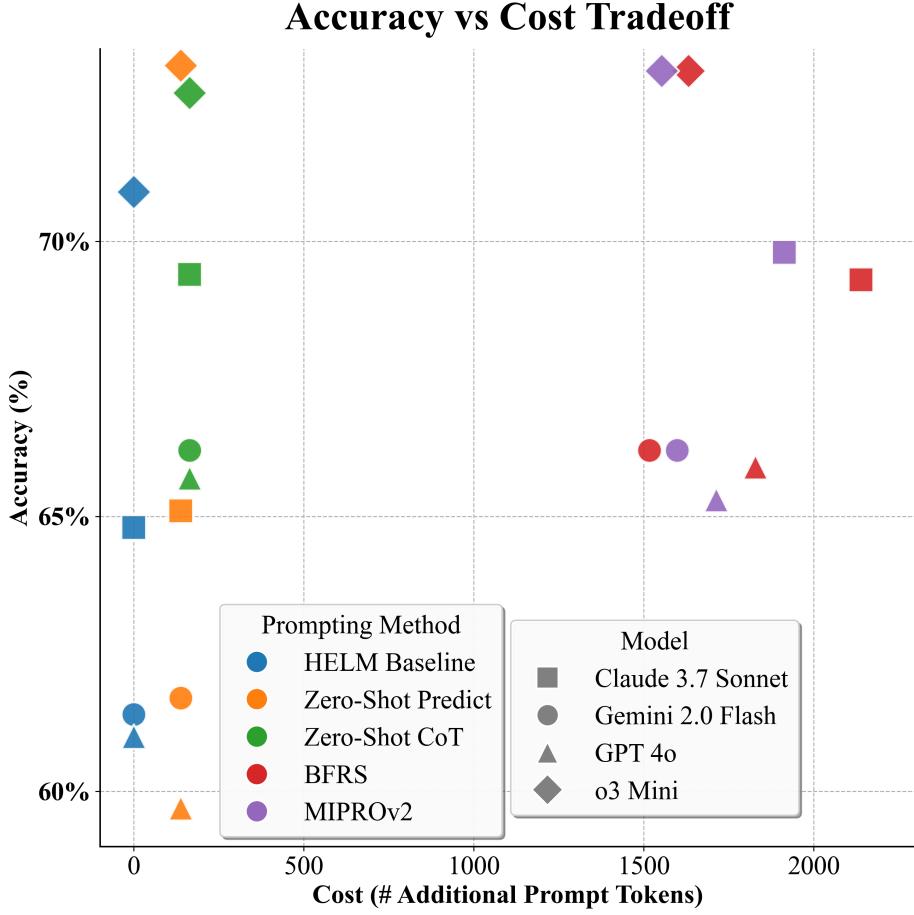


Figure 4: Accuracy vs cost tradeoff across prompting methods. Each point represents a model-prompt pair, with x-axis showing additional prompt tokens (relative to HELM baseline) and y-axis showing macro-averaged accuracy across benchmarks. Overall, Zero-Shot CoT is the most cost-effective structured prompting method.

3.3 Computational Cost Analysis

We evaluate the inference-time computational cost across prompting methods by examining token usage. BFRS and MIPROv2 optimizations are one-time expenses amortized over future runs: DSPy’s documentation reports that optimization runs range from a few cents to tens of dollars depending on the configuration,³. Our optimization costs match DSPy’s reported range. We therefore focus our analysis on inference-time tokens.

In our setup, the input (e.g., question, patient note) is identical across prompting methods, and outputs are capped at <200 tokens. As a result, differences in inference cost arise almost entirely from the *prompt prefix* (the instructions and demonstrations prepended to the input). Hence, we quantify the number of *additional prompt tokens* relative to the HELM baseline instruction, capturing each prompting method’s overhead.

DSPy introduces a lightweight structured prompt template across all methods, resulting in 138 additional tokens for Zero-Shot Predict and 164 tokens for Zero-Shot CoT, which further includes a brief reasoning header. In contrast, BFRS and MIPROv2 insert task-specific demonstrations, producing much larger prompts: averaged across LMs and benchmarks, BFRS adds 1,779 tokens per query and MIPROv2 adds 1,694.

Figure 4 shows the resulting tradeoff. Few-shot optimizers reach high ceilings but require the largest token budgets. Zero-Shot CoT captures most of these gains while using minimal additional prompt tokens, making Zero-Shot CoT the most cost-effective structured prompting method in our study.

³DSPy Optimizer Costs: <https://dspy.ai/learn/optimization/optimizers/>.

4 Related Work

Holistic benchmarking. The General Language Understanding Evaluation (GLUE) (Wang et al., 2018) benchmark was one of the first multi-task evaluation frameworks, aggregating nine distinct language understanding tasks. Benchmarks of increasing scale followed: (i) Measuring Massive Multitask Language Understanding (MMLU) (Hendrycks et al., 2020), including 57 tasks spanning STEM, humanities, social sciences, and (ii) Beyond the Imitation Game (BIG-Bench) (Srivastava et al., 2023), with 204 diverse tasks. The HELM framework is an established standard, designed for transparent, reproducible, and multi-metric evaluation of model capabilities (Liang et al., 2022). However, these benchmarks are typically evaluated using static prompts. Liang et al. (2022) note they opt for simple, generic prompts to orient development "towards generic language interfaces" that do not require "model-specific incantations". This reliance on fixed prompts, however, risks the underestimation of the true capabilities of LMs. Srivastava et al. (2023) conclude that standard few-shot prompting "substantially underestimates the best performance and capabilities of language models" when compared to methods that elicit reasoning (Suzgun et al., 2023).

Prompting methods. The discovery of in-context learning (Brown et al., 2020), where models learn from n-shot demonstrations, and the breakthrough of chain-of-thought (CoT) prompting (Wei et al., 2022) established the important role of prompt design in model performance. Complex, manually-composed strategies like Medprompt (Nori et al., 2023), which combine few-shot selection, CoT, and ensembling, demonstrate that an LM's performance ceiling often lies higher than with the use of static prompts. Because manual prompt engineering is impractical for systematically finding this ceiling, researchers often frame prompt design as a formal "optimization problem", leading to the field of APO. Early APO methods include generation-and-selection, such as Automatic Prompt Engineer (APE) (Zhou et al., 2022), which uses an LM to propose candidate instructions and a separate scoring function to select the best one. Subsequent systems expanded this search paradigm (Wang et al., 2023; Yang et al., 2023; Singla et al., 2024). These methods often outperform zero-shot or manually engineered prompts on a variety of general tasks. In the LM-as-Optimizer paradigm, an LM is instructed to iteratively refine prompts by showing it a trajectory of previously evaluated candidates and their scores. Other approaches have employed evolutionary search, like Promptbreeder (Fernando et al., 2024), which treats prompts as "genes" and evolves a population of instructions over generations using a LM to perform mutation. The DSPy framework (Khattab et al., 2023) generalizes these methods, providing a programming model that compiles declarative, multi-stage pipelines.

5 Limitations

Our study has several limitations. First, we focus on widely used frontier LMs rather than open-source models; while this choice highlights that even strong LMs remain sensitive to prompt design, it limits the generality of our findings. Second, our benchmark suite, though diverse, is not exhaustive, and future work should evaluate structured prompting across additional domains. Third, we evaluate a subset of structured prompting methods from the DSPy-family; alternative frameworks could yield higher ceilings. However, our goal is not to identify the optimal prompting method, but to demonstrate that fixed-prompt (without CoT) leaderboard evaluations systematically underestimate LM performance and often distort model comparisons.

6 Conclusion

By integrating DSPy with HELM, we empirically estimate LM performance ceilings, obtaining more representative estimates. Across seven benchmarks and four LMs, we find that without structured prompting: (i) HELM underestimates LM performance (by 4% average), (ii) performance estimates vary more across benchmarks (+2% standard deviation), (iii) performance gaps are misrepresented (leaderboard rankings flip on 3/7 benchmarks), and (iv) introducing reasoning (*chain-of-thought*) reduces LM sensitivity to prompt design (smaller performance Δ across prompting methods). Sensitivity is heterogeneous: reasoning LMs show marginal gains, whereas some benchmarks for non-reasoning LMs benefit more. Gains are largely agnostic to the structured prompting method. The key driver of improvement is the transition from the baseline prompt to *any* CoT variant, with Zero-Shot CoT providing the most cost-efficient instantiation. Together, we show that scalable and automated performance ceiling estimation enables more robust, decision-useful benchmarks.

Data Availability

The datasets used in this study are fully open-source, and include: MMLU-Pro (Wang et al., 2024b), GPQA (Rein et al., 2024), GSM8K (Cobbe et al., 2021), MedCalc-Bench (Khandekar et al., 2024), Medec (Abacha et al., 2024), HeadQA (Vilares & Gómez-Rodríguez, 2019), and MedBullets (Medbullets, 2025). Further distribution is subject to the data sharing agreements stipulated by the original creators.

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Competing Interest

No competing interests to declare.

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A Bootstrap Few-Shot with Random Search (BFRS)

Problem setup. Let Φ be a LM program with m modules $\{\Phi_i\}_{i=1}^m$, each parameterized by (i) an instruction string $I_i \in \mathcal{I}_i$ and (ii) an ordered list of K_i few-shot demonstrations $S_i = [(u_i^{(1)}, v_i^{(1)}), \dots, (u_i^{(K_i)}, v_i^{(K_i)})]$ drawn from a module-specific pool \mathcal{B}_i . Write the prompt configuration as $\mathbf{v} = (I_1, S_1, \dots, I_m, S_m) \in \mathcal{V}$, and define the end-to-end score on (x, y) as $\mu(\Phi_{\mathbf{v}}(x), y) \in [0, 1]$. Given train/validation splits $D_{\text{tr}}, D_{\text{val}}$, the objective is

$$J(\mathbf{v}) = \frac{1}{|D_{\text{val}}|} \sum_{(x,y) \in D_{\text{val}}} \mu(\Phi_{\mathbf{v}}(x), y). \quad (2)$$

Bootstrapping demonstration pools. BFRS constructs candidate demo pools $\{\mathcal{B}_i\}_{i=1}^m$ via rejection sampling with a seed program Φ_{seed} (typically zero-shot):

$$\mathcal{B}_i = \left\{ (u_i(x), \hat{v}_i(x)) \mid (x, y) \in D_{\text{tr}}, \mu(\Phi_{\text{seed}}(x), y) \geq \tau, \hat{v}_i(x) = \Phi_{\text{seed}}^{(i)}(u_i(x)) \right\}, \quad (3)$$

where $u_i(x)$ denotes input to module i induced by running Φ_{seed} on x , and $\tau \in [0, 1]$ is an acceptance threshold.

Random search over few-shot sets. With module instructions fixed at $I_i = I_i^{\text{seed}}$, BFRS draws R independent candidates

$$S_i^{(r)} \sim \text{SampleK}(\mathcal{B}_i, K_i) \quad \text{and} \quad \mathbf{v}^{(r)} = (I_1^{\text{seed}}, S_1^{(r)}, \dots, I_m^{\text{seed}}, S_m^{(r)}), \quad (4)$$

evaluates $\hat{J}_B(\mathbf{v}^{(r)})$ on a size- B minibatch of D_{val} ,

$$\hat{J}_B(\mathbf{v}) = \frac{1}{B} \sum_{(x,y) \in \mathcal{B} \subset D_{\text{val}}} \mu(\Phi_{\mathbf{v}}(x), y), \quad (5)$$

and returns the best $\mathbf{v}^* \in \arg \max_{r \in [R]} \hat{J}_B(\mathbf{v}^{(r)})$; optionally, \mathbf{v}^* is re-scored on the full D_{val} . Since $\mu \in [0, 1]$, Hoeffding implies $|\hat{J}_B(\mathbf{v}) - J(\mathbf{v})| \leq \sqrt{\frac{\ln(2/\delta)}{2B}}$ with prob. $\geq 1 - \delta$.

B MIPROv2

Search space and objective. As in §A, $\mathbf{v} = (I_1, S_1, \dots, I_m, S_m)$ parameterizes Φ and $J(\mathbf{v})$ is defined in equation 2. MIPROv2 *jointly* searches over instructions and few-shot demos by (a) bootstrapping demo candidates $\{\mathcal{B}_i\}$, (b) proposing instruction candidates $\{\mathcal{I}_i\}$ via a *proposer LM*, and (c) using Bayesian Optimization (BO) with a Tree-structured Parzen Estimator (TPE) surrogate to choose \mathbf{v} .

Initialization (proposal sets). For each module i , construct

$$\underbrace{\mathcal{B}_i}_{\text{demo sets}} \quad \text{by bootstrapping as in equation 3,} \quad \underbrace{\mathcal{I}_i}_{\text{instruction set}} = \{I_i^{(1)}, \dots, I_i^{(T_i)}\},$$

where $I_i^{(t)} \sim q_i(\cdot \mid \text{ctx}_i)$ are sampled by the proposer LM given context ctx_i (dataset summary, module signature/code, exemplar demos, and tip prompts).

Bayesian surrogate via TPE. Maintain a history $\mathcal{H}_t = \{(\mathbf{v}^{(s)}, y^{(s)})\}_{s=1}^t$ of tried configurations and noisy scores $y^{(s)} = \hat{J}_{B_s}(\mathbf{v}^{(s)})$ from minibatches. Let y^* be the γ -quantile of $\{y^{(s)}\}_{s=1}^t$ (e.g., $\gamma=0.2$). TPE models

$$\ell(\mathbf{v}) = p(\mathbf{v} \mid y < y^*), \quad g(\mathbf{v}) = p(\mathbf{v} \mid y \geq y^*), \quad (6)$$

and proposes \mathbf{v} to maximize the ratio $\ell(\mathbf{v})/g(\mathbf{v})$ (improvement proxy). With categorical choices per module, TPE factorizes \mathbf{v} across modules/slots and estimates equation 6 from \mathcal{H}_t by smoothed frequency models.

Noisy validation and escalation. Each candidate \mathbf{v} is scored on a minibatch \mathcal{B} of size B by equation 5. Every E trials, the current top- K candidates (by posterior mean or running average) are *escalated* to full- D_{val} evaluation; the best full-eval configuration \mathbf{v}^\dagger to date is tracked and returned at the end. Concentration of \hat{J}_B to J is controlled by B (Hoeffding bound as above).