RSIBENCH-INFRA: CAN AGENTS OPTIMIZE THEIR OWN TRAINING AND INFERENCE INFRASTRUCTURE AT PRODUCTION SCALE?

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Paper under double-blind review

ABSTRACT

Improving the efficiency of training and inference infrastructure is a practical component of recursive self-improvement. Existing benchmarks for agent-driven optimization largely focus on individual software layers such as kernel, leaving unclear whether agents can deliver end-to-end performance gains under production workloads. We introduce RSIBENCH-INFRA, a benchmark of 20 training and inference tasks spanning diverse model families and modalities, with deployments of up to 32 accelerators on NVIDIA GPUs and Ascend NPUs. Agents start from runnable baselines derived from official recipes and can modify the software stack across engine, kernel, and instruction levels. An OCI image-layer submission protocol captures these changes for reproducible deployment, while independent evaluation measures end-to-end performance under task-specific quality constraints. Across six agent–model configurations and 120 trials, 57 trials achieve validated speedups above $1.10\times$, with the best configuration exceeding this threshold on 60% of tasks. These results demonstrate that current agents can improve production-scale training and inference performance, but identifying effective optimization strategies and consistently translating local improvements into end-to-end gains remain challenging.

1 INTRODUCTION

The worldwide AI infrastructure spending is reaching $487 billion in 2026 as estimated (Seminara, 2026). However, widely used LLM serving engines achieve about 22–38% of their theoretically optimal end-to-end throughput (Zhu et al., 2025). Furthermore, given the limited production capacity for GPU accelerators, this gap also constrains the development of recursive self-improvement (RSI), whose iterative loop requires generating rollouts as well as updating model parameters (Xia et al., 2025). Therefore, the efficiency of training and inference software stack plays a crucial role in transforming the huge investment into real intelligence. Due to the vast complexity of the modern machine learning infrastructure software, agents equipped with broad knowledge and the capacity to work continuously hold considerable promise for optimizing training and inference performance.

Recent works have begun to study this possibility at different abstraction levels of machine learning infra software: 1) GPU Kernel Level: KernelBench (Ouyang et al., 2025), FlashInfer-Bench (Xing et al., 2026), and SOL-ExecBench (Lin et al., 2026) evaluate whether language models can generate correct and fast kernels for diverse machine-learning workloads. FlashInfer-Bench moves closer to deployment by deriving tasks from real serving traces and integrating validated kernels into production LLM engines. 2) Engine Level: VibeServe (Kamahori et al., 2026) and InferenceBench (Yeon et al., 2026) explore the question whether agents can write LLM inference servers under a fixed time budget. 3) Instruction and Close-to-Metal Level: PTXBench (Zhang et al., 2026) evaluates whether LLMs can generate efficient CUDA kernels with architecture-specific inline PTX, measuring functional correctness, runtime execution of required instructions, and performance relative to optimized libraries. KernelCraft (Nie et al., 2026) evaluates agents on generating and optimizing low-level kernels for emerging accelerators with specialized instruction set architectures (ISAs), using execution feedback to assess correctness and performance.

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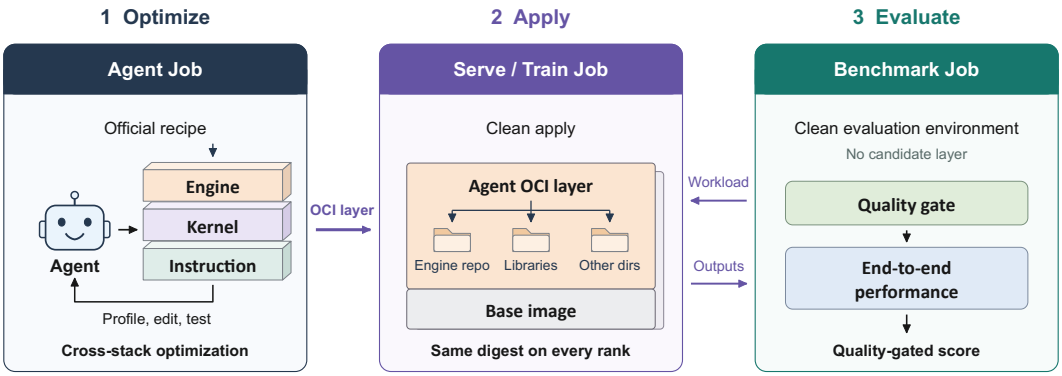

Figure 1: **Overview of RSIBENCH-INFRA.** Starting from an official recipe, an agent optimizes the software stack at the engine, kernel, and instruction levels and submits its changes as a single OCI image layer. The agent OCI layer captures changes across the engine repository, libraries, and other directories and is applied to the base environment of an independent serving or training job, using the same layer digest on every rank. A clean benchmark job, which never loads the candidate layer, checks task-level quality and measures end-to-end performance to produce a quality-gated score.

However, evaluations confined to individual software layers cannot fully characterize end-to-end optimization, because system performance depends on interactions across multiple layers of the software stack, and optimizations at one layer do not necessarily yield an end-to-end global optimum. For example, CAKE (Ye et al., 2026) enables agents to co-evolve GPU kernels and compiler infrastructure, including the intermediate representation, lowering rules, and correctness and performance analyses. Agents can thus extend the compiler capabilities needed to express efficient hardware schedules while optimizing the kernels that use them. Its generated Kimi Delta Attention kernels achieve a $2.05\times$ geometric-mean speedup over FlashKDA on six tested shapes.

To address these challenges, we introduce RSIBENCH-INFRA, a benchmark for evaluating whether agents can effectively optimize end-to-end training and inference performance in real world production settings (Figure 1). A key distinction from prior benchmarks is that RSIBENCH-INFRA allows agents to modify any part of the software stack in the sandbox while it also makes the traditional git patch based evaluation not enough. We therefore introduce an OCI image layer–based submission protocol[1], through which agents submit their modifications as a single image layer.

For inference tasks, we use the official model-specific deployment cookbooks and recipes provided by widely used serving engines, including vLLM, SGLang, and TensorRT-LLM, as baseline implementations. Agents are required to optimize these implementations, and provide optimized service launch scripts. Independent evaluation uses task-specific productional level quality gates to assess whether performance improvements preserve task-level quality *e.g.* LongBench v2 for long context text generation. Our workloads span a broad range of computational workloads and application domains including: text-only models (e.g., GLM-5.3 (GLM-5 Team et al., 2026)), multimodal understanding (e.g., Qwen3-VL (Bai et al., 2025)), video generation (e.g., Wan 2.2 (Wan et al., 2025)), text diffusion (e.g., LLaDA2.1-mini (Bie et al., 2026)), world modeling (e.g., LingBot World 2.0 (Gao et al., 2026)), and vision-language-action models (e.g., OpenPI's $\pi_{0.5}$ (Physical Intelligence et al., 2025)).

Our main contributions are:

- We introduce RSIBENCH-INFRA, the first-known benchmark for agent-driven optimization of end-to-end training and inference performance at production scale through cross-stack software modifications under task-specific constraints. These improvements can translate directly into higher serving capacity or lower compute cost per token in production deployments with comparable workloads and operating conditions.
- RSIBENCH-INFRA defines and benchmarks a key component of RSI: the ability of AI systems to accelerate their own inference and training by optimizing the underlying software

[1]Open Container Initiative, Image Specification v1.1.1, Filesystem Layers: `https://github.com/opencontainers/image-spec/blob/v1.1.1/layer.md`.

stack. Faster rollout generation and model updates can shorten self-improvement cycles and enable more iterations within the same compute budget.

- We evaluate six agent–model configurations across 20 production-oriented training and inference tasks, using 9,907.5 H20 GPU-hours and 2,632.8 Ascend 910B2 NPU-hours, with an model API cost of $3,882.00 for the runs included in the leaderboard. By analyzing submitted artifacts, we characterize successful optimization strategies across the software stack and identify recurring failures in completing optimization runs, packaging submissions, and preserving verifier interfaces. These findings help the community understand the capabilities and current limits of existing models and inform the development of next-generation models.

2 Related Works

2.1 LLMs for Infrastructure Optimization

Existing benchmarks evaluate LLM-driven performance optimization at different levels of the software stack, ranging from low-level GPU kernels to serving optimization: KernelBench and Triton-Bench evaluate kernel correctness and performance (Ouyang et al., 2025; Li et al., 2025), while PTXBench and KernelCraft target architecture-specific instructions and emerging accelerators (Zhang et al., 2026; Nie et al., 2026). FlashInfer-Bench connects serving-trace workloads to kernel integration (Xing et al., 2026), and SOL-ExecBench evaluates kernels against hardware performance bounds (Lin et al., 2026). At the engine level, InferenceBench evaluates open-ended serving optimization (Yeon et al., 2026), while VibeServe synthesizes runtimes specialized to models, hardware, and workloads (Kamahori et al., 2026). RSIBench-Infra evaluates both training and inference optimization in production settings, including distributed deployments, using official recipe baselines, quality-gated performance measurements, and OCI layers that capture changes across repositories and installation directories.

2.2 Recursive Self-Improvement

Recursive self-improvement (RSI) offers a promising path toward increasingly capable AI systems and has attracted growing research interest recently. STOP and the Darwin Gödel Machine improve agent code (Zelikman et al., 2023; Zhang et al., 2025), whereas Agent0 co-evolves task curricula and executor capabilities (Xia et al., 2025). RSIBench-Data evaluates training-data research through checkpoint feedback under fixed post-training infrastructure (Meng et al., 2026). RSI-Exam evaluates improvements to executable methods and frozen-model harnesses through isolated re-execution on hidden instances (RSI-Exam Team, 2026). RSIBench-Infra complements these efforts by making training and inference infrastructure an explicit target of agent-driven improvement within RSI. Using fixed workloads and task-level quality gates, it isolates agents' ability to improve end-to-end systems efficiency, providing an infrastructure counterpart to benchmarks of data-centric research and agent adaptation.

3 RSIBench-Infra

RSIBench-Infra casts infrastructure optimization as a task of improving end-to-end performance while preserving a prescribed workload and its quality requirements. Each task pairs a reproducible reference implementation with a hardware configuration, an execution contract, and an independent verifier.

3.1 Benchmark Construction

Figure 2 summarizes how we turn official recipes into executable tasks with fixed quality requirements and measured performance baselines.

Task Sourcing. Inference tasks follow model-specific cookbooks and deployment recipes from widely used engines such as vLLM, SGLang, and TensorRT-LLM. Selected open-source models include Inkling (Thinking Machines Lab, 2026) and Kimi K3 (Kimi Team et al., 2026), spanning

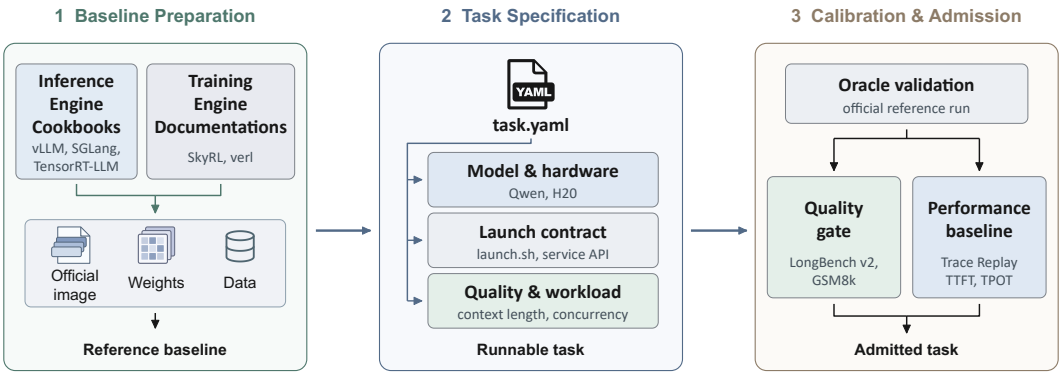

Figure 2: **Benchmark construction workflow.** Official inference engine cookbooks or training engine documentation provide the starting point for preparing the reference environment, model weights, and data. A task specification binds these assets to the hardware, launch contract, quality requirements, and performance workload. The quality benchmarks and latency metrics shown are inference examples; training tasks use numerical training signals and end-to-end execution time.

1.7B (Qwen3-TTS) to 2.8T parameters (Kimi K3) and dense and mixture-of-experts architectures. Workloads cover autoregressive generation, text diffusion, vision–language understanding, video and speech generation, and vision–language–action inference to exercise different serving execution paths. Training tasks draw on SkyRL and verl documentation and reference implementations, covering supervised fine-tuning, reinforcement learning, and on-policy distillation.

Task Admission. An executable reference `launch.sh` integrates each official recipe's container image, checkpoint, precision, and parallelism settings into a training or inference service. We then verify correctness by reproducing expected model behavior on the target hardware. For inference, we evaluate the served endpoint on benchmarks used in the model's official evaluation, such as GSM8K (Cobbe et al., 2021) or GPQA-Diamond (Rein et al., 2024), and compare its scores with the officially reported results. This establishes a correct starting point so agents can focus on infrastructure optimization.

Performance Baseline. For each admitted implementation, we establish a performance baseline using end-to-end workloads representative of production deployments. For example, the Kimi K3 task serves a 256k-context workload on four nodes with eight H20 GPUs each. We construct its agentic request trace from Terminal-Bench 2.1 trajectories (Merrill et al., 2026), comprising 894 requests across 20 sessions, and replay it in the Mooncake trace format (Qin et al., 2025), reconstructing the input and output lengths and session-prefix reuse patterns.

This productional workload exercises long-context prefill, decoding, KV-cache management, and distributed communication within the complete serving stack. We measure output throughput over a fixed one-hour window at concurrency one and record latency diagnostics including time to first token (TTFT) and time per output token (TPOT). At fixed hardware and validated model quality, throughput quantifies generated tokens per GPU-hour. We apply the same principle to other tasks, measuring complete ML workloads of each training task. These measurements provide the reference values for scoring agent optimizations in the later Section 3.3.

3.2 Task Definition

Figure 1 illustrates the overall optimization and evaluation workflow. Our abstraction of a task follows Harbor (Harbor Framework Team, 2026). Each task produced by the construction process is defined as a tuple $\mathcal{T} = (\mathcal{I}, \mathcal{E}, \mathcal{V})$ with three components, illustrated using the task `infer-kimik3-2p8t`:

- **Instruction** $\mathcal{I}$ contains the task description and supporting information available to the agent, including the optimization objective, baseline setup, operational constraints, and submission requirements. For `infer-kimik3-2p8t`, the instruction includes a throughput optimization objective and requirements to preserve model weights, maintain the specified OpenAI-compatible interfaces, and support 262,144-token contexts.

- **Environment** $\mathcal{E}$ contains a runnable baseline derived from an official recipe, the required assets, and the execution resource configuration. For `infer-kimik3-2p8t`, the environment includes SGLang, the MXFP4 checkpoint, and a launch configuration for four nodes with eight NVIDIA H20 GPUs each.
- **Verifier** $\mathcal{V}$ contains the evaluation code and assets for submission validation, quality assessment, and performance measurement. For `infer-kimik3-2p8t`, the verifier includes routines to validate and apply the submitted OCI layer to fresh baseline environments, a LongBench v2 quality gate, and a 256k trace comprising 894 requests from 20 agentic sessions. Performance uses a fixed 3600 s window at concurrency one with ASAP dispatch: requests do not wait for recorded arrival times, and the full trace need not finish.

3.3 Evaluation Procedure

Figure 1 summarizes three stages: optimization, deployment, and evaluation. The agent optimizes an official-recipe baseline and submits an OCI layer. An independent Serve/Train Job applies the same layer to clean environments on all ranks; a separate Benchmark Job checks quality and measures end-to-end performance.

Agent Optimization. Starting from the base environment, the agent profiles, edits, and tests the software. Inference tasks include the `e2e-infer-opt` skill: establish a reproducible baseline and quality checks, classify bottlenecks, trace hotspots to implementations, and build isolated microbenchmarks. Targeted optimizations are validated with microbenchmarks and end-to-end quality and performance tests, repeating as bottlenecks shift. Task-specific engine and hardware guidance supports this workflow.

After optimization, the agent is required to explicitly packages its changes as an OCI filesystem layer, `submit_oci.tar.gz`, including the required `launch.sh` to launch an optimized service. This submission may contain source code, dependencies, and compiled artifacts within the permitted modification scope. An orchestration script launches two independent jobs: a `Serve Job` for optimized inference or training and a `Benchmark Job` for evaluation. Both jobs run in environments isolated from the Agent Job, preventing agent-side filesystem changes from directly modifying the evaluator's scripts or scoring logic.

Serve/Train Job. The Serve/Train Job starts from the same baseline image as the Agent Job and receives the submitted OCI layer as the sole artifact carrying agent modifications. The script verifies the engine baseline and layer digests before applying the layer, using the same layer on every rank. This reconstructs the submitted changes on a known baseline without inheriting the Agent Job's runtime state.

Benchmark Job. The Benchmark Job never loads the submitted layer. It runs predefined quality and performance scripts against the service through its exposed interfaces, keeping workload generation, measurement, and scoring isolated from the agent-modified software. This separation supports auditing for benchmark hacking.

Quality gates. We accept performance improvements only when the submission passes the quality gate for its task. Predefined scripts in the Benchmark Job use fixed inputs and reference assets to assess long-context understanding with LongBench v2, reference-output consistency for video, world-model, and action generation, and speech intelligibility through word error rate. For training, we replay frozen inputs and compare stepwise loss and global gradient-norm curves with the reference execution to check preservation of training behavior. An **LLM judge** (we use GPT-5.6 sol across all tasks) additionally reviews agent trajectories and submitted changes to identify potential benchmark hacking, including workload-specific shortcuts and violations of task constraints. Task-specific inputs, acceptance criteria, numerical tolerances, and the scope of these checks are detailed in Appendix B.1.

End-to-end metrics. We measure end-to-end throughput in the unit appropriate to each workload, such as tokens/s for language serving, videos/hour for video generation, frames/s for world modeling, and steps/hour for training. For task i, let P_i^{agent} and P_i^{base} denote the submission's throughput and the fixed official-recipe baseline throughput under the same workload and measurement protocol. If

Table 1: **Task overview.** Requests denotes the source trace size for text replay, serving requests or chunks for other inference tasks, and samples per optimizer step for training tasks (prompt batch × rollout group in RL; batch size in SFT).

Model	Devices	Workload	Requests	Quality gate
SGLang (7)				
GLM-5.2 744B	2×16 910B2	agentic-trace-256k	894	LongBench v2
GLM-5.3 744B	2×8 H20	agentic-trace-256k	894	LongBench v2
Inkling 975B	1×8 H20	agentic-trace-256k	894	LongBench v2
Kimi K3 2.8T	4×8 H20	agentic-trace-256k	894	LongBench v2
LLaDA2.1-mini 16B	1×1 H20	agentic-trace-32k	140	LongBench v2
Qwen3.8 27B	1×1 H20	agentic-trace-256k	894	LongBench v2
Qwen3-VL 32B	1×1 H20	synthetic 1080p JPEGs	32	MMMU
SGLang Diffusion (2)				
LingBot-World 2.0 14B	1×8 H20	fixed trace, 832×480, 16 fps	16	Reference frames
$\pi_{0.5}$-LIBERO 3.3B	1×1 H20	LIBERO observations	16×3	Reference actions
vLLM (1)				
Muse-Glimmer 30B	1×1 H20	agentic-trace-128k	936	LongBench v2
vLLM-Ascend (2)				
DeepSeek V4 Flash 284B	1×8 910B2	agentic-trace-64k	193	LongBench v2
GLM-5.3-Flash 320B	2×8 910B2	agentic-trace-256k	894	LongBench v2
vLLM-Omni (3)				
MiniMax-H3 64B	1×2 H20	672×384, 107 frames	1	Video + audio ref.
Wan2.2 TI2V 5B	1×1 H20	1280×704, 121 frames	4	Reference videos
Qwen3-TTS Base 1.7B	1×1 H20	Seed-TTS eval (Chinese)	202	Chinese ASR WER
TensorRT-LLM (1)				
Nemotron 3 Ultra 550B	1×8 H20	agentic-trace-256k	894	LongBench v2
SkyRL (2)				
Qwen3-30B-A3B (GRPO)	2×8 H20	DAPO-Math-17K	64×8	Loss + grad. norm
Qwen3-30B-A3B (SFT)	1×8 H20	Tulu 3	64	Loss + grad. norm
VeRL (2)				
Qwen3-8B & 32B (OPD)	2×8 H20	GSM8K+MATH	128	Loss + grad. norm
Qwen3-VL-4B (GRPO)	1×8 H20	Geometry3K	512×5	Loss + grad. norm

the submission passes validation and the task's quality gate and completes the prescribed performance measurement, its validated speedup is $s_i = P_i^{\text{agent}}/P_i^{\text{base}}$. Failed or missing evaluations receive $s_i = 0$; valid slowdowns retain their ratios below one. For each agent–model configuration, we report **Faster Rate** and **Median Speedup** over the full suite of N tasks:

$$\begin{aligned} \text{Faster Rate} &= \frac{100}{N}\sum_{i=1}^{N}\mathbf{1}\{s_i > 1.10\} \quad (\%), \\ \text{Median Speedup} &= \text{median}(s_1, \ldots, s_N). \end{aligned} \tag{1}$$

Here, $\mathbf{1}\{\cdot\}$ is the indicator function. Faster Rate is the percentage of tasks with a validated throughput improvement strictly greater than 10% to avoid performance fluctuations. Median Speedup summarizes the typical validated performance ratio while limiting the influence of exceptionally large gains. Both metrics include all tasks, including zeros for failed or missing evaluations.

Alongside the benchmark scores, we retain other task-specific quality measurements and gate outcomes, request success rates, and raw performance ratios such as the median and 99th percentile of time to first token (TTFT).

3.4 Dataset Overview

The suite comprises 16 inference tasks and 4 training tasks (Table 1), spanning eight engine groups and deployments of one to 32 accelerators across up to four nodes on NVIDIA H20 GPUs and Ascend 910B2 NPUs. Inference models range from 1.7B to 2.8T parameters.

Table 2: Leaderboard Results on RSIBENCH-INFRA. Rows are sorted by Faster Rate, with ties broken by Median Speedup.

Agent	Model	Faster Rate	Median Speedup	Tokens	Est. cost (USD)
Codex	GPT-6 Astra	12/20	1.1205×	141.17M	704.70
Claude Code	Claude Opus 5	11/20	1.1602×	639.24M	630.36
OpenCode	Qwen3.8-Max	11/20	1.1483×	947.64M	235.88
OpenCode	Kimi K3	10/20	1.0733×	556.65M	1,697.01
OpenCode	GLM-5.3	7/20	1.0073×	368.85M	528.18
OpenCode	DeepSeek V4 Flash	6/20	1.0003×	566.37M	85.87

Inference. The suite covers language serving, including GLM-5.3, Inkling, and Kimi K3 (GLM-5 Team et al., 2026; Thinking Machines Lab, 2026; Kimi Team et al., 2026), vision–language understanding (Bai et al., 2025), text diffusion (Bie et al., 2026), video generation (Wan et al., 2025), speech synthesis, world modeling (Gao et al., 2026), and action generation (Physical Intelligence et al., 2025). Long-context serving stresses prefill, decoding, cache management, and distributed communication; multimodal tasks additionally exercise image processing, iterative denoising, and action generation.

Training. The four tasks cover text and vision–language GRPO, supervised fine-tuning (SFT), and on-policy distillation (OPD). All use full-parameter BF16 training with Adam. A trusted client built on tinker-cookbook drives the services through the public Tinker API, keeping prompts, seeds, rewards, advantage and loss computation, and timing in the Benchmark Job. Performance counts complete workload steps, including rollout, weight synchronization, and teacher scoring where applicable. Appendix A.3 specifies data preparation, objectives, and optimizer settings.

4 EXPERIMENT

4.1 SETUP

The main comparison uses six agent–model configurations, one trial per configuration and task on the 20-task suite, and an eight-hour agent budget. Task inputs, seeds, hardware, and evaluation follow Section 3.3; harness and accounting details are in Appendix C.

4.2 RESULTS AND ANALYSIS

Results Table 2 ranks configurations by Faster Rate. Codex with GPT-6 Astra leads with 12/20 tasks, while Claude Code with Claude Opus 5 has the highest Median Speedup (1.1602×). Across configurations, Faster Rate ranges from 30% to 60%, indicating that improvements above the 10% threshold remain uneven across the suite.

Successful cases. Figure 3 compares retained optimization behaviors across all and Faster trials, manually classified from final OCI layers and trajectories. The most frequent in Faster trials are operator fusion (18), sparse or long-context attention (14), data movement/layout (14), and speculative decoding/MTP (13). Fusion reduces launch and intermediate-tensor overhead; attention changes improve sequence kernels; layout and residency changes reduce host–device or device–device transfers; speculative decoding reduces target-model work per generated token. Graph/compiler changes occur in 13 Faster trials, although some graph experiments were rejected after quality or end-to-end regressions.

Successful inference cases span scheduler changes (multi-block prefill and dynamic active-set shrinking), cache/prefix-state reuse, specialized GEMM or convolution kernels, and model-specific VAE/audio pipelines, illustrating the benchmark's cross-layer scope. Appendix D details a representative case.

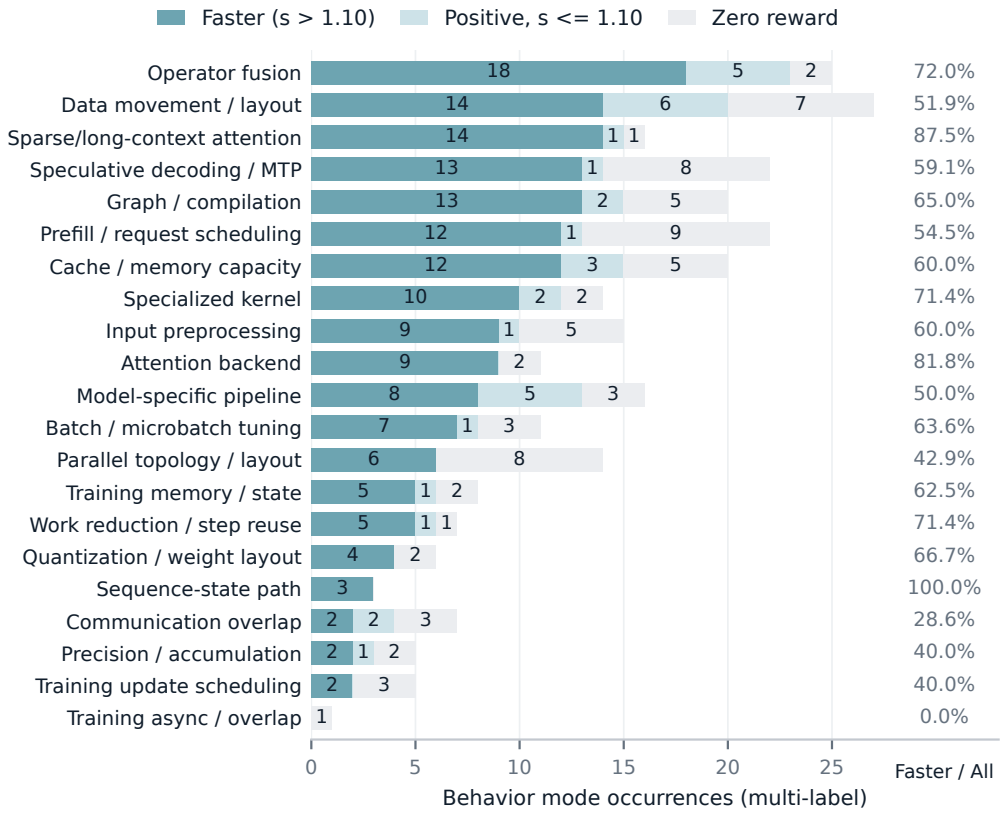

Figure 3: **Outcomes of optimization behavior modes.** Each bar contains all classified occurrences of a method.

Failure modes and limitations of current agent. We identify three recurring agent-side failure modes. 1) *Incomplete execution:* the agent exhausts its budget before finishing a submission. 2) *Invalid packaging:* the OCI layer omits the required launch script or uses incorrect filesystem paths and is rejected before deployment. 3) *Interface violations:* training outputs contain malformed tensors, such as log-probabilities with incompatible shapes or dtypes, preventing quality evaluation. Agents also struggle to translate useful diagnoses into effective, quality-preserving implementations. On OPD, Astra identified teacher scoring as approximately 43% of local step time, yet its final training-side changes yielded $1.01\times$. Quality constraints create a further barrier: four inference submissions recorded raw performance ratios above $1.10\times$ but received zero after failing their quality gates. These observations motivate cross-layer adaptation, bottleneck-directed implementation, and complete-workload validation. Appendix F provides distributions and cases; these associations do not establish method effects or intrinsic model preferences.

Cost–Performance Trade-offs Figure 5 compares the two aggregate metrics against estimated model API costs over the full 20-task suite. The observed Pareto-optimal configurations depend on the performance metric. For Faster Rate, they are OpenCode with Qwen3.8-Max, and Codex with GPT-6 Astra, spanning 11/20, and 12/20 faster tasks, respectively. For Median Speedup, Claude Code with Claude Opus 5 replaces Codex with GPT-6 Astra on the frontier.

Repeated-run Evaluation Due to resource constraints, we evaluate three runs per task only for Codex with GPT-6 Astra and OpenCode with Qwen3.8-Max. The follow-up run-level Faster counts are 10.67 ± 1.15 and 10.67 ± 0.58 out of 20; mean run-level Median Speedups are $(1.1097\pm0.0118)\times$ and $(1.1694 \pm 0.0820)\times$, respectively (mean $\pm$ sample standard deviation). Thus, Table 2's 12-versus-11 Faster advantage for Astra does not persist in the run means. Table 2 remains a single-run comparison; Appendix G (Figure 8) reports the corresponding SEMs.

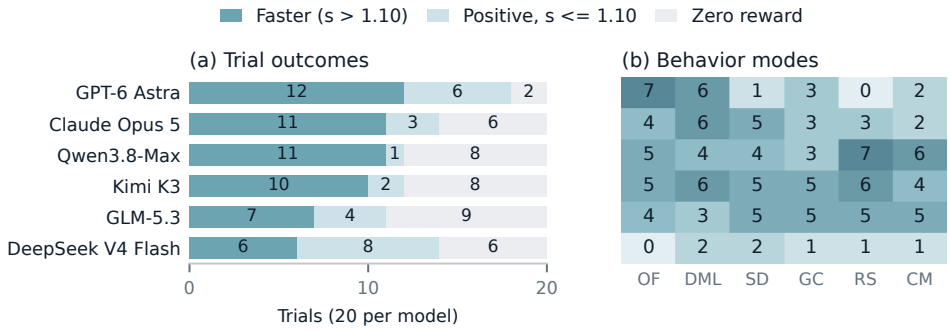

Figure 4: **Model differences in outcomes and behavior modes.** (a) Outcomes across 20 tasks per configuration. (b) Six most frequent behaviors (at least 20 total occurrences), with rows aligned to (a). OF, operator fusion; DML, data movement/layout; SD, speculative decoding; GC, graph/compilation; RS, request scheduling; CM, cache/memory.

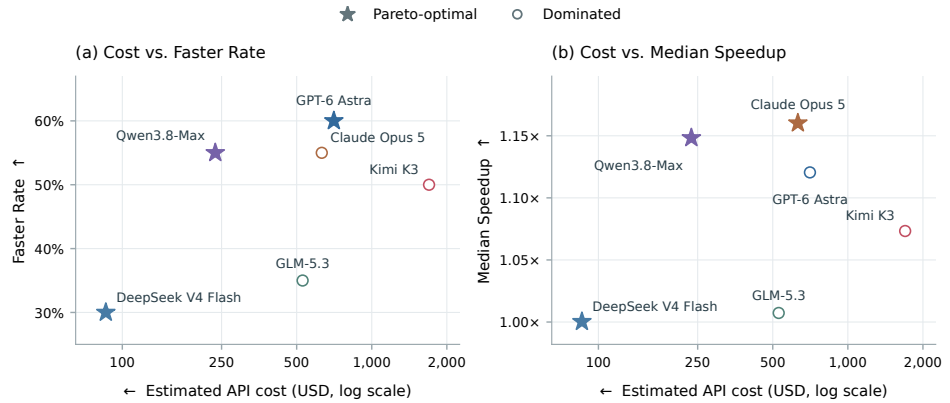

Figure 5: **Cost–performance trade-offs.** Each point represents one agent–model configuration from Table 2. Cost is the estimated model API cost for the full suite, excluding GPU and NPU costs. Stars mark Pareto-optimal configurations for each metric, favoring lower cost and higher performance.

Task Time Scaling Exploratory Experiment Because each complete performance verification in these tasks may require a one-hour measurement window, we also conduct an experiment extending from 8-hour agent timeout to 24-hour agent timeouts to give agents more optimization space. Resource constraints limited this exploration to two OpenCode cases, using Kimi K3 and GLM-5.3. Neither yielded validated gains: Kimi ended early; GLM's deployment failed and self-test was below baseline. Appendix H (Table 9) details the results.

5 Limitations

API-access constraints prevented us from evaluating an important frontier model Claude Fable-5.1 because it constantly interrupt our evaluation session flagging frontier-llm. Resource constraints also limited our hardware evaluation to NVIDIA H20 GPUs and Ascend 910B2 NPUs, leaving other platforms such as NVIDIA Blackwell GPUs uncovered.

6 Conclusion

We introduced RSIBench-Infra to evaluate agent-driven optimization of end-to-end training and inference across 20 production-oriented tasks, with stack-wide modifications and independent quality checks. The best configuration achieves validated speedups above $1.10\times$ on 60% of tasks, demonstrating practical potential while leaving substantial room for more reliable optimization. RSIBench-Infra provides a testbed for advancing this infrastructure component of recursive self-improvement.

AI USE STATEMENT

We used coding agents to assist with the development and debugging of the benchmark framework. We also used GPT-5.6 sol as an LLM judge to review recorded agent trajectories and summaries of submitted changes for potential benchmark hacking, including workload-specific shortcuts and violations of task constraints. This trajectory review complements the executable task-specific quality checks and independent performance measurements described in Section 3.3 and Appendix B.1. All AI-assisted code was reviewed and tested by the authors, and all AI-assisted text was reviewed and revised for accuracy and clarity. The authors take responsibility for the final content of this work, including its claims, code, and experimental results.

REPRODUCIBILITY STATEMENT

Upon publication, we will publicly release the code needed to run and evaluate RSIBENCH-INFRA. To support reproducibility, Section 3.1 and Appendix A describe benchmark construction, task specifications, baseline recipes, hardware configurations, workloads, and measurement boundaries. Section 3.3 and Appendix B detail the isolated evaluation procedure, OCI image-layer submission protocol, task-specific quality gates and thresholds, and scoring methodology. Appendix C documents the agent and model configurations, execution budgets, seed settings, and the trajectory and trial records used to trace reported results to their source artifacts.

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A Task Specifications and Baseline Recipes

This section of appendix specifies input construction, training objectives, and measurement boundaries. Model and hardware assignments and workload cardinalities are given in Table 1; acceptance thresholds are given in Appendix B.1.

A.1 Reference Recipes and Optimization Boundaries

Each task starts from an executable deployment or training recipe with its prescribed checkpoint, tokenizer or processor, numerical representation, and resource allocation. These define a reference operating point, not an optimal implementation. Inference recipes retain the supplied weight representation, including low-precision checkpoints where specified. Training recipes retain the initial model state, objective, effective batch, masks, optimizer hyperparameters, and update schedule. Engine scheduling, microbatching, kernel fusion, memory layout, and communication overlap may be optimized within these constraints. Changing a reduction order is permitted; changing the objective or omitting required computation is not. Quality evaluation and artifact validation follow Appendix B.

A.2 Inference Task Inputs and Generation Settings

Text replay. The serving workload is constructed from recorded Terminal-Bench agent sessions. Each request retains its input length, output budget, relative arrival time, and prefix-block identifiers. Sessions are selected to fit the task's context limit. Recorded timestamps determine request order but do not impose arrival-time delays during ASAP replay. A deterministic generator reconstructs token sequences in 128-token blocks, reusing a block for the same prefix identifier. Thus, replay preserves sequence lengths and a controlled prefix-sharing pattern, rather than the original conversation text. It tests serving efficiency; answer quality is assessed separately.

Requests are dispatched as soon as concurrency capacity is available (ASAP), without waiting for their recorded arrival times, subject to a maximum of four concurrent requests for full-trace replay and one for the windowed workloads. The random seed is 42. The evaluator supplies token IDs directly, checks their lengths against service-reported input counts, and uses service-reported output-token counts instead of re-tokenizing generated strings. Generation uses near-zero temperature and the recorded output budget. Full-trace measurement includes queueing and draining outstanding requests. Windowed measurement instead counts tokens received before the cutoff, including verified cumulative counts from a request still in flight; unserved requests beyond the cutoff are not treated as completed requests. These are two different measurement contracts and use separate reference denominators.

Vision–language requests. For throughput, each request combines one deterministically generated image with synthetic text of nominal length 2048 tokens and an output budget of 256 tokens; the driver uses concurrency eight and seed 42. Image processing and multimodal prefill remain in the serving path. These synthetic performance inputs are separate from the natural image–question pairs used for quality evaluation.

Video and interactive generation. MiniMax-H3 uses text-to-video-and-audio generation of a golden retriever running through a meadow, with seed 42, video flow shift 12, and audio flow shift 3. Wan uses text-to-video generation with the original, non-distilled weights: two prompts (boxing cats and a dog in a meadow), each paired with seeds 42 and 43. Its UniPC solver uses guidance scale 5 and flow shift 5, with a fixed negative prompt suppressing blur, static scenes, distortion, and artifacts. Both tasks produce media at 24 frames/s and retain the generation lengths in Table 1.

LingBot starts from a fixed lakeside image, prompt, and seed 42. The driver changes camera controls after chunk indices 3 and 12, changes the prompt to a snowy scene after index 6, and jointly changes the prompt and controls to a sunset scene after index 9 (indices start at zero). Events are injected at chunk boundaries, so their effect is independent of wall-clock speed. Guidance is 1; upscaling and frame interpolation are disabled. The entire native frame stream, including the initial chunk, contributes to performance.

Actions and speech. Action requests carry the observation images, robot state, language instruction, and explicit denoising noise. The evaluator selects observations from the reference pool and reuses the same inputs across timed rounds. Requests are serial, matching batch-one policy execution. Speech requests carry target text, reference audio, and its transcript for voice cloning, with seed 42, unit speech speed, and a maximum of 2048 generated audio tokens. The service returns streaming PCM. Throughput uses the *fixed reference audio duration* as its numerator, so lengthening the candidate waveform cannot improve the score.

A.3 Training Task Data and Objectives

Data preparation. Text GRPO uses deduplicated mathematics records, removing prompts with conflicting answers and preserving a fixed record order. SFT freezes a shuffle with seed 0; the Qwen conversation renderer supervises all assistant messages, masks other roles, and truncates at 4096 tokens. Tokenization and datum validation occur before timing. OPD interleaves the two source training sets in round-robin order, starting with GSM8K, and takes sequential prompt batches. Multimodal GRPO concatenates the Geometry3K training and validation splits in their original order. It retains the image and question, adds the prescribed answer instruction, and expands image placeholders according to the processor's image grid before constructing training inputs. The validation split here supplies training workload inputs; it is not a held-out generalization evaluation.

Policy updates. For both GRPO tasks, the evaluator assigns a terminal reward of $+1$ or -1 by normalizing and comparing the last extracted `Answer:` value with the reference. Advantages subtract the group mean reward, without division by the standard deviation. For response token t of rollout j, the prescribed loss is

$$\mathcal{L}_{\mathrm{IS}}(\theta) = -\sum_{j,t} m_{j,t} \exp(\ell_{\theta,j,t} - \ell_{\mathrm{sample},j,t})\, A_{j,t}, \tag{2}$$

where m masks prompt tokens, ℓ_θ is the current training-forward log-probability, and ℓ_{sample} is the recorded sampling log-probability. GRPO broadcasts the centered terminal reward over response tokens. The objective uses a masked sum, with no additional KL or entropy term. OPD uses the same objective with tokenwise advantages $A_{j,t} = \max(\ell_{\mathrm{teacher},j,t}, -10) - \max(\ell_{\mathrm{sample},j,t}, -10)$; the teacher remains frozen and scores the student's sampled tokens. SFT instead minimizes weighted negative log-likelihood, $-\sum_{j,t} w_{j,t}\ell_{\theta,j,t}$, with weights determined by the assistant-message mask and the renderer's mean-reduction rule.

Table 3: Training settings additional to Table 1. Lengths are token limits; the multimodal prompt limit includes expanded image tokens. Policy tasks make one optimization pass per rollout batch.

Training task	Prompt / response limit	Update minibatch	Adam β_2
Text GRPO	2048 / 8192	128	0.999
SFT	4096 total	full batch	0.999
OPD	1024 / 2048	128	0.95
Multimodal GRPO	1024 / 2048	128	0.95

All tasks use learning rate 10^{-6}, $\beta_1 = 0.9$, and $\epsilon = 10^{-8}$. The two SkyRL recipes additionally fix weight decay at 0.1 and gradient clipping at global norm 1. Policy sampling uses temperature 1 and top-$p = 1$; seeds are derived deterministically from base seed 42, the workload-step index, and prompt position. Each workload step obtains a sampler synchronized to the current training weights. Empty responses, and multimodal responses containing invalid image placeholders, are excluded by the driver's fixed rule and counted explicitly; a batch with no trainable responses fails. Candidate code cannot choose a different filtering rule.

Workload steps versus optimizer updates. A scored training step is one complete workload iteration. For SFT it contains one batch update; for policy training it includes weight synchronization, rollout, reward or teacher scoring, training-input construction, and all minibatch updates. Consequently, the text and multimodal GRPO batches entail four and twenty optimizer updates, respectively, when all rollouts are trainable. The workload-step rate must not be interpreted as an optimizer-update

rate. Quality replay removes sampling variability by holding the training inputs and advantages fixed; performance uses fresh rollouts. Numerical acceptance and the limits of curve-based validation are described in Appendix B.1.

A.4 Measurement Boundaries

Table 4 specifies the work included in each denominator. Service startup, model loading, and the stated warmup are outside measurement. Warmup is executed once per evaluation, not subtracted from individual request latencies. Client-observed intervals include communication with the evaluated service. Reference and candidate use the same boundaries; internal kernel or forward-pass timings are diagnostic only.

Table 4: Warmup and end-to-end timing contracts. Quality scoring is outside the performance interval.

Workload	Warmup	Measured interval
Text replay	One request for full replay; none for GLM-5.2; one short request for GLM-5.3-Flash.	Full replay: start of ASAP dispatch to final completion. Windowed replay: ASAP dispatch to cutoff (or exhaustion where allowed), using only tokens received in that interval.
Vision–language	No separate request.	Dispatch and completion of the complete multimodal request batch.
Video generation	One two-denoising-step request for MiniMax; one full-schedule request with a separate seed for Wan.	Synchronous generation requests through receipt of media files, including transfer and file writing; decoding and perceptual comparison follow timing.
Interactive world	A separate two-chunk session.	Initialization message to completion of the final native frame chunk; event injection is included.
Action generation	Two requests.	Serial request loop over all timed rounds, including receipt and parsing of action chunks.
Speech synthesis	One request.	First timed request to receipt of the last PCM stream. Request preparation, audio persistence, and ASR are outside timing.
Training	Complete workload steps.	All timed workload iterations through completion of their optimizer futures; includes rollout weight synchronization and teacher scoring where applicable. SFT data preparation and quality replay are outside timing.

Warmup updates in training are retained: timed steps continue from the resulting model and optimizer state. Checkpoint recovery probes and curve comparisons are not inserted into the timed run. Execution records retain completed requests, rollouts, usable training examples, tokens, and optimizer updates so that a reduction in the amount of required work cannot be mistaken for a systems gain.

B Detailed Evaluation Protocol

B.1 Details Task-Specific Quality Gates

Performance gains are accepted only when the submission satisfies the task's quality requirements. An independent driver evaluates the submitted system against fixed task inputs and reference assets. The gate depends on the workload: answer quality for language understanding, reference-output consistency for generated media and actions, speech intelligibility for synthesis, and numerical training behavior for training tasks.

Language and vision-language understanding. All ten pure-text inference tasks use LongBench v2[2] to evaluate long-context understanding. This covers DeepSeek-V4-Flash, GLM-5.2, GLM-5.3, GLM-5.3-Flash, Inkling, Kimi K3, LLaDA2.1-mini, Muse-Glimmer, Nemotron-3-Ultra, and Qwen3.8. The candidate and baseline are evaluated under the same task-specific data selection, prompt, context-length policy, generation settings, and answer-scoring procedure. The gate checks accuracy against the task-specific acceptance threshold, separately from throughput measurement. LLaDA2.1-mini uses a 36-question subset at the 24k gate tier; DeepSeek-V4-Flash and Muse-Glimmer use 100-question subsets at 128k, and GLM-5.3, Inkling, Kimi K3, Nemotron-3-Ultra, and Qwen3.8 use 100-question subsets at 256k. These gate tiers are separate from the performance traces in Table 1. For Qwen3-VL-32B, we evaluate image–question pairs on the 847 multiple-choice validation examples of MMMU (Yue et al., 2024), extract the predicted option, and compute answer accuracy. The current gate accepts an absolute deviation of at most 0.05 from the calibrated reference accuracy of 0.7273.

Video generation. Wan2.2-TI2V and MiniMax-H3 use fixed prompts, seeds, and generation settings. We decode the returned media and check its dimensions, frame count, frame rate, and encoding before comparing it with reference outputs using LPIPS (Zhang et al., 2018). Wan2.2 uses four prompt–seed pairs, generating videos at 1280×704 resolution, 121 frames, and 24 frames/s for comparison with the corresponding reference videos. Each video must satisfy all calibrated bounds in Table 5, rather than only on average across cases. Mean LPIPS averages the framewise distances; worst-frame LPIPS takes their maximum. For temporal consistency, the verifier computes the difference between consecutive frames in each video, then compares these temporal differences against the reference using the mean absolute error for each adjacent-frame pair. The gate checks both the mean and the maximum of these per-pair errors. The motion ratio is the mean absolute temporal difference of the candidate divided by that of the reference. Pixel values for these temporal metrics are normalized to $[0, 1]$. Frame timestamps and duration are checked as well.

Table 5: Wan2.2 quality bounds. Decimal values are rounded for display; the task configuration retains the full-precision calibrated values.

Measurement	Acceptance bound
Mean framewise LPIPS	≤ 0.25834550
Worst-frame LPIPS	≤ 0.31249873
Mean temporal-difference absolute error	≤ 0.05224669
Worst-pair temporal-difference mean absolute error	≤ 0.14031570
Motion magnitude ratio	$[0.97985323,\ 1.04997272]$
Absolute duration error	≤ 0.001 s
Absolute frame-timestamp error	≤ 0.0001 s

MiniMax-H3 additionally checks the generated audio: mean video LPIPS must be at most 0.2, audio spectral cosine similarity at least 0.8, and the audio root-mean-square amplitude ratio between 0.5 and 2.0. Its audio format must match the prescribed sample rate and channel count. All required checks must pass.

Interactive world modeling. For LingBot-World 2.0, the driver replays a fixed initial image, prompt, seed, and sequence of camera and prompt-update events over 16 chunks totaling 189 frames. Each chunk must have the expected frame count, shape, and effective event identifier. The first, middle, and last frames of each chunk are compared with the reference session: mean absolute pixel error must be at most 2.0 on the 8-bit scale and peak signal-to-noise ratio at least 35 dB in every chunk. Missing or additional chunks fail the gate.

Action generation. For $\pi_{0.5}$-LIBERO, we randomly select 16 observations from a frozen pool of 64 reference cases and provide the same observation, instruction, and denoising noise to the candidate. Every returned action chunk must have the reference shape, maximum absolute error at most 0.05, and mean absolute error at most 0.01. This gate measures action-output consistency with the baseline; closed-loop robot success is not evaluated.

[2] https://github.com/THUDM/LongBench

Speech synthesis. Qwen3-TTS-Base is evaluated on 202 fixed Chinese Seed-TTS cases with reference audio and transcripts for voice cloning. Every request must complete and return valid, nonempty audio. A fixed Paraformer-zh recognizer transcribes the generated speech, and the official Seed-TTS normalization and scoring procedure computes word error rate (WER). The arithmetic mean of per-utterance WER must be at most 1.4%. Speech recognition and audio persistence are outside the performance timing interval. This gate measures intelligibility; speaker similarity and subjective naturalness are not acceptance criteria.

Training. We replay frozen training inputs from the same model initialization while preserving the prescribed objective, masks, optimizer, and update order. A trusted driver reconstructs each step's loss from the training forward outputs and records the backend's global gradient norm before clipping. Qwen3-30B-A3B GRPO, Qwen3-30B-A3B SFT, and Qwen3-8B on-policy distillation each execute eight optimizer steps; every step must have relative loss error strictly below 2% and relative gradient-norm error strictly below 15%. Relative error is $|x - x_{\mathrm{ref}}|/|x_{\mathrm{ref}}|$; a zero reference requires a zero candidate value. Non-finite values and missing, duplicated, or reordered steps are rejected, and errors are checked pointwise rather than averaged. These curves probe preservation of training behavior without requiring bitwise equality.

Trajectory review for potential benchmark hacking. In addition to the executable quality checks, a GRM reviews the agent's recorded trajectory and a summary of the submitted changes. The review assesses whether the changes constitute legitimate infrastructure optimizations that transfer to general workloads, and flags non-generalizable shortcuts or quality-sacrificing changes that violate the task contract. Limited or absent speedup alone does not make a submission non-compliant.

C Agent Configurations and Reproducibility

The main comparison in Table 2 uses the native Harbor trials archived with the manuscript. Each of the six agent–model configurations below has one reported result for each task in the 20-task suite, with an agent execution budget of 28,800 seconds (8 hours) per task. The task, verifier, and base-environment identifiers are those recorded in the trial artifact; the agent receives the task instruction and baseline environment, while the quality and performance drivers remain trusted evaluator components. If a transport retry or an earlier failed attempt exists in the archive, it is retained as provenance and does not replace the reported task result.

Table 6: Agent–model configurations used for the reported experiments.

Agent	Harness version	Model
Codex	0.147.0	GPT-6 Astra
Claude Code	2.1.252	Claude Opus 5
OpenCode	1.18.18	Qwen3.8-Max
OpenCode	1.18.18	Kimi K3
OpenCode	1.18.18	GLM-5.3
OpenCode	1.18.18	DeepSeek V4 Flash

The agents use the Harbor task interface and submit `submit_oci.tar.gz`. A valid submission contains the files selected by the agent within the task's modification boundary and an executable `launch.sh`; it is checked for OCI layer structure and applied to a clean reference filesystem. Development commands, intermediate logs, and process state are discarded after the Agent Job. Only the submitted layer and evaluator-produced evidence enter the Serve/Train and Benchmark Jobs.

Each task has a fixed instruction, launch contract, resource allocation, workload, verifier configuration, and reference baseline. Exact agent messages, tool calls, environment variables, and command outputs are preserved in the corresponding Harbor trajectory and trial artifact, rather than reconstructed from the aggregate table.

Inference trials use the task's fixed seed (42 in the shared serving and generation drivers). Training trials use the task-defined training seed and sampling seed; the SFT data shuffle is fixed separately as specified in Appendix A.3. The task-level configuration, rather than the agent, controls these values.

Task workloads use the deterministic seeds and fixed request or training inputs specified in Appendices A and B. Inference drivers count the prescribed tokens, frames, audio duration, or actions, while training drivers count complete workload steps and retain the specified warmup state. The same measurement boundary is used for the reference and candidate; startup, model loading, and quality evaluation are outside the performance denominator.

The main comparison reports single-trial point estimates; the repeated-run follow-up is reported separately in Appendix G. A task contributes its validated raw speedup when deployment, protocol checks, and the quality gate all succeed. Missing rewards are retained as missing at the task level and contribute zero to the suite aggregates; a valid slowdown retains its ratio below one. We do not rerun, regrade, synthesize, or replace a missing result using an exception marker. The leaderboard is ordered first by Faster Rate (strictly greater than $1.10\times$) and then by Median Speedup, as defined in Section 3.3.

Agent token totals are copied from the recorded Harbor usage fields. The reported costs are estimates obtained from the applicable public list prices, including cached-input accounting where available; they are not billed amounts. Trial archives preserve the original trajectory, result JSON, submitted-layer metadata, and verifier evidence so that every reported row can be traced back to its source artifacts.

D Optimization Case Study: $\pi_{0.5}$-LIBERO

This appendix gives an annotated optimization case from the Claude Opus 5 trials. We use this case study to make the benchmark's end-to-end trust boundary concrete: a profile identifies a bottleneck, the agent changes the serving system, local experiments reject unproductive alternatives, and an independent verifier checks quality and measures the final workload. Profile window timings are diagnostic; the reported rewards below are from the clean verifier artifacts. The trial identifiers are retained so that every number can be traced to the archived trajectory, submitted OCI layer, and verifier output.

The $\pi_{0.5}$ task serves a batch-one vision–language–action policy on one NVIDIA H20. The reference service produced 2.774 action chunks/s in the agent's first end-to-end measurement. The quality gate replays fixed observations, instructions, and denoising noise; therefore, a faster system must preserve the reference action chunks rather than merely produce valid media.

The first optimization exposed a semantic interaction between padding and execution graphs: the server-side empty camera made the request appear to lack full attention, disabling the denoise graph. Removing that padding fast path raised the local rate, but a full-pool gate then found a large action error on a case that reused a Python object address. The denoise graph had treated the recycled address as proof that its static prefix K/V buffers were current. The agent replaced this with a monotonic context identity and revalidated the full observation pool before pursuing further speedups.

The profile then separated the remaining work into two regions. The denoise graph spent 28.58 ms of device time across 8,999 launches. Its many small RMSNorm operations accounted for about 6.7 ms, while 370 small matrix-vector operations and repeated prefix K/V concatenations were also visible. The prefix encoder used 486 launches: device work was 22.62 ms but wall time was 32.80 ms, indicating host-side issue overhead. A microbenchmark on the exact FA3 shapes found 198 attention calls at about 51 μs each; selecting a shape-aware split-KV factor reduced this kernel to about 16 μs in the denoise shape. A fixed split value was rejected because prefix and denoise shapes have different optimal parallelism.

The delivered patch fused the ada-RMSNorm chain into one Triton kernel, added bounded prefix and denoise CUDA-graph runners, introduced cache-aware graph signatures, and fused the action expert's Q/K/V and gate/up projections when module offload is disabled. A proposed K/V concatenation optimization lowered device time in isolation but was neutral in end-to-end A/B runs and was removed.

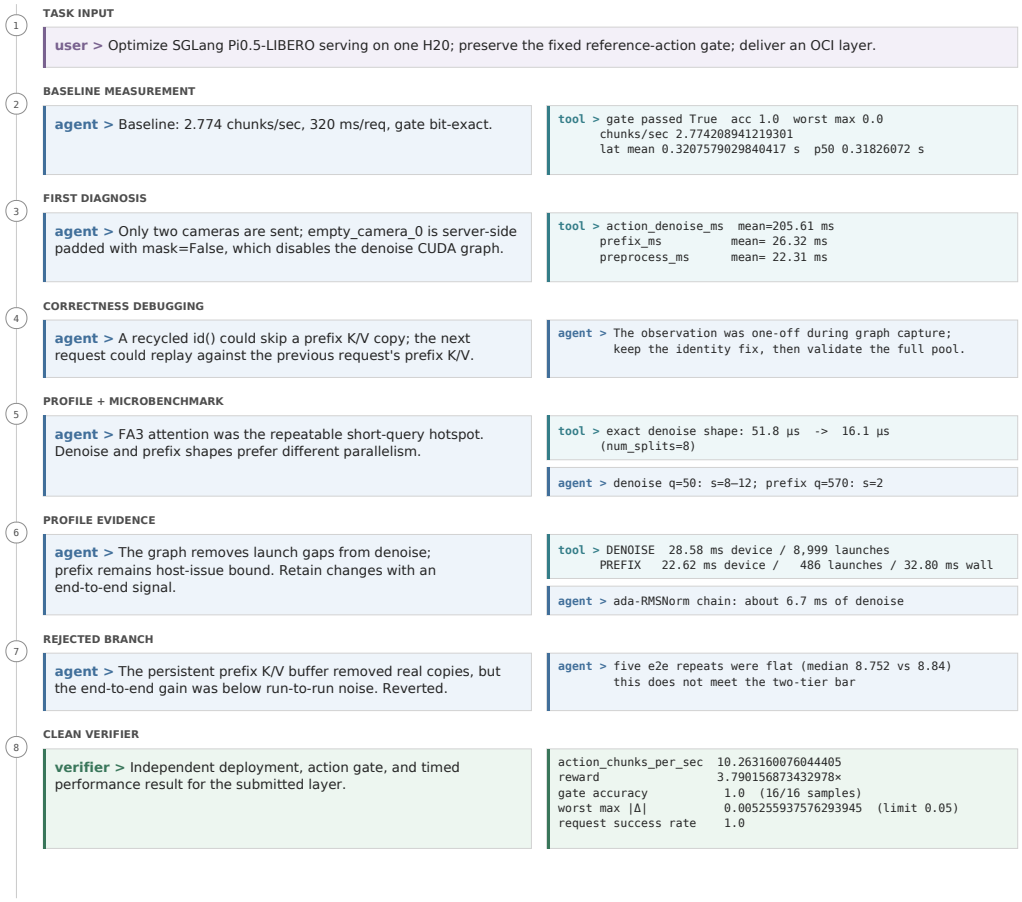

Figure 6: Selected excerpts from the $\pi_{0.5}$-LIBERO agent trial, summarized and lightly rewritten for layout. Colors indicate the source: user (purple), agent (blue), tool output (teal), and independent verifier (green). Verifier results are separate from agent-side diagnostics.

E TASK-LEVEL RESULTS ACROSS CONFIGURATIONS

Figure 7 shows task-level Faster outcomes for Table 2. For valid $s_i > 0$, color encodes the normalized score $q_i = 100 \max(0, 1 - 1/s_i)$, using saved one-decimal scores. The single rounding-boundary case is resolved by the corresponding Faster count in Table 2.



Figure 7: **Task-by-configuration result matrix.** Rows identify tasks and columns identify agent–model configurations, ordered as in Table 2. Each square represents the single reported trial for that pair. Colored squares indicate validated speedups strictly above $1.10\times$ (Faster); their shades encode the normalized score q_i (%), with darker squares indicating larger scores. Gray squares denote all other outcomes, including valid gains at or below the threshold, slowdowns, and failed or missing evaluations; they do not necessarily indicate a failed quality check. No per-cell numbers or repeated-trial counts are shown. Rows use original task IDs. The separator distinguishes the 16 inference and four training tasks. Normalized scores differ from both raw speedup and Faster Rate.

F Optimization Behavior and Outcome Analysis

Population and counting rules. Outcome-aware counting is necessary to separate limited gains from zero-score outcomes. Missing rewards are counted as zero throughout this appendix, consistent with Figures 3 and 4. We reuse exactly the manually reviewed retained-method labels underlying Figure 3. For method m, let A_m count all submissions retaining it, F_m count those with validated speedup above $1.10\times$, and V_m count those with positive reward. We report both F_m/A_m, which includes zero-score outcomes in the denominator, and F_m/V_m, which conditions on a positive result. The latter is subject to selection and must be interpreted alongside the former. A zero-score outcome can reflect a submission rejected by a quality or compliance check, even if a raw performance ratio was measured, or an evaluation that produced no final reward. We do not substitute agent-side measurements for formal evaluation results.

The non-Faster population includes both measurable underperformance and incomplete evidence. The complement of the 57 Faster trials contains 63 trials: 24 positive but non-Faster and 39 zero. Of these, 43 have confirmed retained methods (17 positive but non-Faster and 26 zero), 13 have no confirmed optimization delivery, and seven have insufficient evidence. The 43 classified trials contribute 107 method occurrences, partitioned into 33 positive but non-Faster and 74 zero. Method labels overlap, so occurrence totals must not be interpreted as independent trial counts. Three Faster trials retain no confirmed new performance method; their recorded scores remain in the leaderboard but supply no evidence of a method's contribution.

Table 7: Retained behaviors by model. Each cell is all occurrences (non-Faster occurrences), over 20 trials per model. Non-Faster totals are 8, 9, 9, 10, 13, and 14 in column order. Labels overlap; empty evidence is not inferred as an unattempted method.

Retained method	GPT-6 Astra	Claude Opus 5	Qwen3.8 Max	Kimi K3	GLM 5.3	DeepSeek V4 Flash
Operator fusion	7 (2)	4 (1)	5 (1)	5 (0)	4 (3)	0 (0)
Data movement / layout	6 (3)	6 (3)	4 (0)	6 (3)	3 (3)	2 (1)
Sparse/long-context attention	2 (0)	3 (0)	4 (0)	3 (0)	3 (2)	1 (0)
Speculative decoding / MTP	1 (0)	5 (2)	4 (1)	5 (2)	5 (3)	2 (1)
Graph / compilation	3 (0)	3 (1)	3 (1)	5 (2)	5 (3)	1 (0)
Prefill / request scheduling	0 (0)	3 (1)	7 (3)	6 (2)	5 (3)	1 (1)
Cache / memory capacity	2 (0)	2 (1)	6 (2)	4 (1)	5 (3)	1 (1)
Specialized kernel	3 (1)	2 (1)	3 (0)	3 (0)	3 (2)	0 (0)
Input preprocessing	2 (0)	2 (1)	3 (2)	5 (2)	2 (1)	1 (0)
Attention backend	1 (0)	1 (0)	3 (1)	4 (0)	2 (1)	0 (0)
Model-specific pipeline	2 (1)	2 (0)	3 (1)	5 (3)	2 (2)	2 (1)
Batch / microbatch tuning	2 (0)	1 (0)	1 (0)	3 (1)	3 (2)	1 (1)
Parallel topology / layout	1 (1)	2 (2)	4 (2)	3 (2)	2 (1)	2 (0)
Training memory / state	2 (1)	1 (0)	1 (0)	2 (1)	2 (1)	0 (0)
Work reduction / step reuse	0 (0)	2 (0)	2 (1)	1 (0)	1 (1)	1 (0)
Quantization / weight layout	0 (0)	1 (0)	1 (0)	0 (0)	3 (2)	1 (0)
Sequence-state path	1 (0)	1 (0)	1 (0)	0 (0)	0 (0)	0 (0)
Communication overlap	0 (0)	1 (1)	2 (1)	2 (1)	1 (1)	1 (1)
Precision / accumulation	1 (1)	2 (1)	0 (0)	1 (0)	1 (1)	0 (0)
Training update scheduling	0 (0)	1 (1)	1 (0)	2 (2)	0 (0)	1 (0)
Training async / overlap	0 (0)	0 (0)	1 (1)	0 (0)	0 (0)	0 (0)

Uneven coverage of optimization levels. Models differ in the optimization levels they successfully incorporate into their final submissions, leaving some opportunities unexploited. Table 7 distinguishes overall method coverage from occurrences in non-Faster trials. Astra retains fusion on seven tasks, data movement/layout changes on six, and speculative decoding on one, with no retained request-scheduling changes. Qwen and Kimi have broader scheduling coverage (seven and six tasks) and retain 59 and 65 total method labels, compared with Astra's 36. On LLaDA, Astra and Qwen both observed regressions from increasing request concurrency. Astra then retained a router GEMM optimization ($1.1179\times$), while Qwen reworked multi-block prefill and radix-cache maintenance ($3.0343\times$). The same initial negative result therefore led to different delivered optimization levels,

with the operator-level submission leaving an engine-level opportunity unexploited. This is a limitation observed in that trial, not evidence of a fixed model preference.

Broader retained coverage does not by itself ensure an accepted improvement. Qwen's and Kimi's non-Faster results are predominantly zero, whereas six of Astra's eight non-Faster results have valid positive speedups. Claude retains data movement/layout on six tasks and speculative decoding on five; six of its nine non-Faster outcomes are zero. GLM retains graph/compilation, speculative decoding, caching, and scheduling on five tasks each, but only one of its four fusion-labeled submissions is Faster. DeepSeek retains 18 labels across the suite, with no confirmed fusion or specialized GEMM/GEMV delivery; five of its non-Faster trials have no confirmed method and four have unknown evidence. This is a difference in observable deliveries, not proof that these directions were never explored. Label counts do not measure effort, implementation size, or efficiency per token, hour, or dollar.

Implemented methods can deliver insufficient gains. Agents can retain substantive optimization methods and still fail to accelerate the full workload by more than 10%. Figure 3 shows higher overall Faster fractions for sparse/blocked/long-context attention (87.5%), fusion (72.0%), and specialized kernels (71.4%) than for data movement/layout (51.9%) or model-specific pipelines (50.0%). Conditioning on positive rewards gives 93.3%, 78.3%, and 83.3% for the first group, versus 70.0% and 61.5% for the latter two. Communication fusion/overlap reaches Faster in 2/7 submissions and 2/4 positive results, too few to establish a stable ranking.

Failure to obtain an accepted result presents a separate limitation from weak performance gains. Parallel layout reaches Faster in 6/14 submissions and 6/6 positive results: its lower overall rate comes entirely from zero-score outcomes. Request scheduling similarly changes from 12/22 overall to 12/13 among positive results; nine of its ten non-Faster occurrences are scored as zero.

Adapting to workload constraints remains difficult across models, so pooled method rates alone cannot identify a model-specific weakness. LingBot accounts for three of the six positive non-Faster data-movement/layout occurrences and three of the five positive non-Faster pipeline occurrences; the other two pipeline cases are TTS trials. Excluding LingBot as a sensitivity check raises the positive-result Faster fractions of these two categories to 14/17 (82.4%) and 8/10 (80.0%). The full-suite values remain the primary statistics. Categories represented by only one to three trials, including sequence-state paths and training asynchrony, cannot support general claims of effectiveness or ineffectiveness.

Profiling does not ensure a feasible implementation: Qwen3-VL. Opus's trial illustrates a gap between identifying expensive computation and delivering a quality-preserving improvement to it. All six submissions have positive scores, but only the four retaining both specialized small-matrix kernels and attention-backend changes are Faster (1.1465–$1.1904\times$). Opus retains only multimodal feature residency ($1.0022\times$). Its trajectory does include profiling and extensive experimentation: a profile attributes roughly 75% of GPU time to FP8 GEMMs, but several generic backend alternatives are slower or unsupported, and a local compilation gain is rejected after output corruption. Astra and Qwen separate efficient prefill from underutilized decode shapes, retaining the existing prefill implementation while specializing small-M FP8 GEMMs and decode attention. Thus, locating a large time component is insufficient; agents must identify feasible headroom within its actual shapes and hardware constraints. The principal five-model comparison uses matching task and engine baselines. DeepSeek's feature-residency and prefill-configuration submission also remains near baseline ($1.0007\times$).

A diagnosed phase can remain insufficiently optimized: OPD. Astra's OPD trial exhibits the same diagnosis-to-implementation gap at the resource-allocation level. Astra identifies teacher scoring as approximately 43% of local step time, yet its final fusion, training-memory, and rollout-wakeup changes yield only $1.0107\times$. GLM expands the teacher pool onto four otherwise idle GPUs within the same task allocation, obtaining $1.2588\times$. A preliminary same-four-GPU experiment with two teacher replicas improves little, leading GLM to distinguish a layout change from increasing the resources actually performing teacher computation. The two trials share the task and engine baseline. This comparison supports phase and resource alignment; it does not isolate the effect of any single change in either submission.

More retained methods do not ensure larger gains. Agents can improve several components while leaving substantial costs in the complete execution. GLM's TTS submission retains four method categories but reaches only $1.0690\times$. Its trajectory exposes a gap between summed microkernel timings and the complete predictor graph, limited weight bandwidth, and an integration bug in an enclosing graph capture. The capture bug is fixed before delivery, so it does not explain the remaining gap by itself. Kimi's sampler graph, thread-pool reuse, and input fast paths similarly yield $1.0256\times$, leaving substantial host/device overhead.

Quality preservation can block promising speedups even after agents have implemented them. Four inference submissions have a raw performance ratio above $1.10\times$ but zero reward after failing the task quality gate: Opus on GLM-5.3 ($1.3270\times$), Kimi on Qwen3.8 ($1.5898\times$), and GLM and Qwen on Wan ($1.2785\times$ and $1.2684\times$). These unaccepted ratios are diagnostic measurements, not validated speedups. In LingBot, all six configurations remain below $1.10\times$; Qwen rejects an FP8 attention experiment despite a 13.7% local throughput gain because per-chunk pixel differences accumulate from 1.96 to approximately 53. These cases distinguish limited optimization headroom, incomplete coverage of the critical path, and the difficulty of preserving behavior through stateful execution. Correctly reverting an invalid candidate is desirable.

Interpretation. The observed shortfalls concern adapting the optimization level, realizing a diagnosed opportunity, and preserving quality through full execution. These conclusions draw on descriptive statistics of final submissions, supplemented by selected same-task trajectories. Methods co-occur, tasks differ in attainable headroom, and model and harness effects are not separated. One run per configuration and task does not establish stable model preferences or causal method rankings. Retained-method counts also omit rejected experiments, so they cannot estimate the probability that an attempted method succeeds. The supported research directions are better estimates of achievable phase-level gains, joint reasoning about compute, memory and resource placement, and reliable validation of the resulting complete system.

G Repeated-Run Evaluation and Error Bars

We examine three complete 20-task runs (original, A, B) for Astra and Qwen3.8-Max, totaling 60 attempts per configuration. Resource constraints limit the follow-up to these configurations, selected after the initial comparison for Astra's highest Faster Rate and Qwen's favorable API-cost–performance trade-off. The original comparison in Table 2 is unchanged; no best-of selection is used.

Run-level variability. Let s_{ir} be the recorded speedup for task i in run r, with missing rewards contributing zero. Existing rewards are retained even when a trial also records an exception. For each complete run we compute the Faster count $c_r = \sum_{i=1}^{20} \mathbf{1}\{s_{ir} > 1.10\}$ and Median Speedup $m_r = \mathrm{median}_i(s_{ir})$. Table 8 gives three-run means and sample standard deviations (SD). Figure 8 instead shows these means with SEMs ($\mathrm{SD}/\sqrt{3}$), not 95% confidence intervals.

Table 8: Run-level statistics across three complete runs (mean ± sample SD). Faster counts are out of 20 tasks; count SDs are not percentage points.

Configuration	Faster count	Run-level Median Speedup
Codex + GPT-6 Astra	10.67 ± 1.15	$(1.1097 \pm 0.0118)\times$
OpenCode + Qwen3.8-Max	10.67 ± 0.58	$(1.1694 \pm 0.0820)\times$

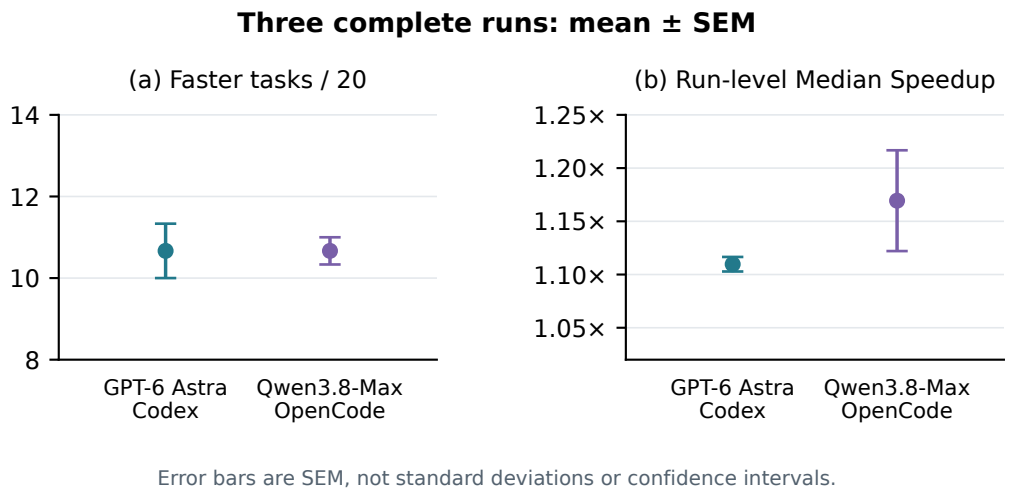

Figure 8: **Variability over three complete runs.** Points show the mean run-level (a) Faster count and (b) Median Speedup; bars show their SEMs. All 20 tasks enter each run's statistic, including zero rewards. Error bars summarize run-to-run variability on this suite, not uncertainty across an independently sampled population of tasks.

Relation to leaderboard aggregation. The follow-up leaderboard first averages attempts within each task, $\bar{s}_i = \frac{1}{3}\sum_r s_{ir}$, then computes $\sum_i \mathbf{1}\{\bar{s}_i > 1.10\}$ and $\mathrm{median}_i(\bar{s}_i)$. These give 11/20 and $1.1253\times$ for Astra, versus 10/20 and $1.0964\times$ for Qwen3.8-Max. Thresholding and taking a median do not commute with averaging, so these aggregates differ from the means in Table 8. The figure uses independently recomputed run-level means and SEMs rather than centering run-level error bars on these different leaderboard estimators.

Interpretation and scope. Faster counts are 12/10/10 for Astra and 11/10/11 for Qwen3.8-Max; both average 10.67 tasks, so Astra's one-task lead in Table 2 does not persist in these run means. Qwen retains the higher mean run-level median, with greater variability. These results qualify the precision of the single-run comparison rather than replacing it. Three runs do not establish a reliable ranking.

H INITIAL TIME-SCALING EXPERIMENT

Each complete performance verification in these tasks includes a one-hour measurement window, limiting the number of full feedback cycles within the standard agent budget. This motivates exploring a longer timeout. Due to resource constraints, we report only two exploratory runs configured with a 24-hour agent timeout: OpenCode with Kimi K3 and OpenCode with GLM-5.3, each targeting its corresponding inference task. These exploratory cases are separate from the repeated full-suite evaluation in Appendix G. We focus on what the extended-budget executions establish, distinguishing the allowed budget from elapsed agent execution time and agent-side measurements from independent evaluation.

Table 9: Extended-budget case outcomes. Both use OpenCode 1.18.18 and a 24-hour agent timeout. Elapsed time covers the recorded agent-execution interval, excluding setup and subsequent independent evaluation. Neither case produced an independently validated performance improvement.

Model	Agent time	Observation
Kimi K3	9.62 hours	Ended before using the full budget; no independently validated end-to-end gain.
GLM-5.3	20.27 hours	Independent deployment failed; no valid performance measurement. Agent-reported throughput was below its reference baseline.

Kimi K3. The extended-budget agent ended after approximately 9 hours 37 minutes, well before the 24-hour cap. Its final report described profiling communication and testing serving configurations, and reported local decode throughput of approximately 31.1 tokens/s, which it described as the baseline level. It also reported a 600-second proxy measurement; this does not establish a speedup under the prescribed full performance protocol. Independent evaluation did not produce a validated performance result. Thus, this case does not measure the effect of actually spending 24 hours on optimization.

GLM-5.3. The agent ran for approximately 20 hours 16 minutes. The final test report gave 56.58 tokens/s against a 113.46 tokens/s reference, approximately $0.499\times$, in a 3,600-second window. This reported measurement also showed no throughput improvement.

Interpretation and future work. Neither of these two 24-hour-budget runs produced a clear validated gain. Early termination and evaluation failure prevent a controlled estimate of the return to additional optimization time; these observations do not show that longer budgets are generally ineffective. A systematic study across time budgets, with repeated runs, recorded actual time consumption, and reliable independent evaluation, is an important direction for future work. We do not assign statistical significance or error bars to these exploratory cases.