

SWINGARENA: ADVERSARIAL PROGRAMMING ARENA FOR LONG-CONTEXT GITHUB ISSUE SOLVING

Anonymous authors

Paper under double-blind review

ABSTRACT

We present SWINGARENA, an adversarial evaluation framework for Large Language Models (LLMs) that approximates real-world software development workflows. Unlike traditional static benchmarks, SWINGARENA models the collaborative process of software iteration by pairing LLMs as *submitters*, who generate patches, and *reviewers*, who create test cases and verify the patches through continuous integration (CI) pipelines. To support these interactive evaluations, we introduce a retrieval-augmented code generation (RACG) module that handles long-context challenges by providing relevant code snippets from large codebases across multiple programming languages (C++, Python, Rust, and Go). Our adversarial evaluation can surface limitations that are often overlooked by traditional evaluation settings. Our experiments, using over 400 high-quality real-world GitHub issues selected from a pool of 2,300 issues, indicate differing behavioral tendencies across models in patch generation versus validation. SWINGARENA offers a scalable and extensible approach to evaluating LLMs in CI-driven software development settings.¹

1 INTRODUCTION

Large Language Models (LLMs) have become potent accelerators of software development (Chen et al., 2021; Li et al., 2023; Lozhkov et al., 2024), capable of synthesizing high-quality code snippets, automatically detecting and repairing defects, and providing interactive guidance throughout the development cycle. To assess these three capabilities—code-generation fidelity, automated debugging, and conversational assistance—a benchmark must evaluate each dimension individually and in concert. Existing suites including HumanEval (Chen et al., 2021) and MBPP (Austin et al., 2021) focus on the functional correctness of concise, self-contained snippets, offering a valuable first glance at model proficiency. Nevertheless, their narrow scope fails to capture the richer, iterative workflows that typify modern software engineering. Recent efforts including SWE-Bench (Jimenez et al., 2023) move toward greater realism by grounding tasks in genuine GitHub issues and repositories. Nevertheless, they typically rely on static or partially simulated contexts like single unit tests, omitting the critical role of the full Continuous Integration (CI) pipeline and its automated safeguards that define professional development. Moreover, these benchmarks tend to assume a one-shot coding paradigm, whereas industrial software engineering is inherently iterative: debugging, testing, and incremental refinement are the norm, not the exception.

In real-world software development, coding tasks involve collaborative, iterative workflows with complex project requirements and automated systems. For example, a common scenario involves a contributor submitting a pull request (PR) to a large open-source project, where a CI pipeline—often implemented with GitHub Actions—automatically builds up validation environments, runs unit tests, enforces style guides, executes linters², and validates compatibility with the existing codebase. Any failure triggers an iterative dialogue between contributor and reviewers, where comments are exchanged and resolved, patches are applied, and checks are rerun until all pass, allowing the PR to be approved and merged into the upstream codebase. This submitter–reviewer loop epitomizes modern

¹The complete codebase and benchmark are submitted in this link and will be open-sourced after the anonymity period.

²A linter is a tool that automatically analyzes code for grammar errors, style issues, and potential bugs to ensure consistency and quality.

collaborative workflows, yet a robust evaluation framework for such interactions in LLMs is still lacking, leaving a critical aspect of real-world practice largely unaddressed by current benchmarks.

To meet the demands of this real-world process, conventional benchmarks (Zan et al., 2024; 2025; Rashid et al., 2025; Aleithan et al., 2024) often fall short by focusing only on basic questions like "Does the code pass a unit test?"—a threshold that is far beneath the expectations of professional software development. A meaningful evaluation must instead ask, "Can the model submit code that is valid, compliant, and able to pass a full CI pipeline and peer review?"

Current evaluation practices suffer from three critical blind spots. First, static benchmarks use fixed, predictable challenges that fail to capture the dynamic, adversarial nature of real software development where patches face adaptive scrutiny. Second, they evaluate single-agent performance in isolation, missing collaborative interactions and role-switching dynamics essential to modern software engineering workflows. Third, they focus narrowly on functional correctness while ignoring comprehensive quality gates that determine real-world success.

LLMs must overcome one of the core challenges in software engineering: effectively analyzing and interpreting over long contexts in code repositories. Realistic CI tasks involve sprawling codebases where essential information is scattered across thousands of lines and multiple files. To enable fair evaluation across diverse model architectures and context window sizes, we implement a Retrieval-Augmented Code Generation (RACG) system that provides standardized context access using established retrieval components. This helps ensure models receive comparable context—useful for isolating the effects of our adversarial evaluation protocol from retrieval artifacts.

Thus, we introduce SWINGARENA, an adversarial evaluation framework that operationalizes full CI workflows as dynamic testing arenas for LLMs. Our contributions are:

- A standardized adversarial CI evaluation protocol that pairs a submitter and a reviewer, executed on repository-native workflows (PK-style dual-role evaluation with role switching and clear scoring).
- A multi-language long-context retrieval pipeline (RACG) combining syntax-aware chunking, dense reranking, and token-budget-aware packing across C++, Python, Rust, and Go, positioned as a strong baseline to support SwingArena rather than a standalone algorithmic contribution.
- A curated, CI-grounded dataset of 2,300 real GitHub issues with solutions (4 languages), including 400 evaluation instances (100 per language) and a 100-sample ablation split, with scripts to reproduce retrieval and CI execution.

We evaluate SWINGARENA across multiple state-of-the-art proprietary and open-source models, including GPT-4o, Claude-3.5, Gemini-2.0, DeepSeek-V3, and several open-source alternatives, demonstrating the framework’s broad applicability and revealing distinct behavioral patterns across different model architectures. Our experiments reveal that while some models excel at aggressive patch generation, others prioritize correctness and CI stability, highlighting the nuanced trade-offs that emerge in realistic software engineering scenarios.

We adopt an adversarial CI protocol rather than static tests to cover repository-native gates (style, security, coverage) and the interaction between patching and testing that fixed suites under-report. This design enables us to measure end-to-end success on real pipelines and to quantify stability, reviewer effects, and RACG’s contribution under context limits across models and languages. [We view Win Rate as a relative, adversarial outcome rather than an absolute capability, and we therefore interpret it only in conjunction with Reviewer CI Pass Rate \(RPR\) and Submitter CI Pass Rate \(SPR\), which summarize reviewer and submitter behavior against human-written CI checks. In ongoing analysis \(to be included in the camera-ready version\), we further examine stratified and fixed-effects models of conditional win rates to complement the raw metric, but we do not treat Win Rate alone as a standalone measure of absolute strength.](#)

2 RELATED WORK

2.1 BENCHMARKS FOR EVALUATING REAL-WORLD SOFTWARE ENGINEERING

Real-world software engineering is complex, requiring nuanced reasoning over large, evolving codebases. While LLM-based agents have made significant progress (Yang et al., 2024; Zhang et al., 2024; Xia et al., 2024), evaluating the quality of generated code remains challenging (Wang

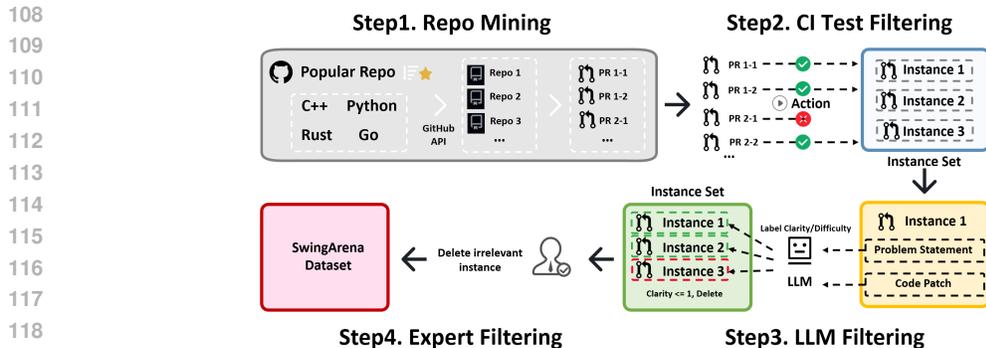


Figure 1: Overview of SWINGARENA data construction pipeline, including repository collection, pull request extraction, task instance creation, quality filtering, and multiple CI-based validation.

et al., 2025a). SWE-Bench (Jimenez et al., 2023) takes an important step toward realism by using GitHub issues, pull requests, and unit tests grounded in real repositories. However, it is limited to Python, focuses only on unit test success, and includes noisy or weakly aligned test cases. Recent multi-language extensions (Zan et al., 2024; 2025; Rashid et al., 2025; Aleithan et al., 2024) often require manual Docker setup, hindering automation and preventing the use of stricter CI-based arenas to evaluate LLMs under realistic development workflows. How to integrate real-world workflows of multi-language code development, submission, and review into evaluation remains an open problem.

2.2 CODE EVALUATION

Evaluating the effectiveness of code-generation agents remains a challenge (Wang et al., 2025c). Prior work explores diverse assessment strategies (Evtikhiev et al., 2023; Yetistiren et al., 2022; Siddiq et al., 2024), but most focus on function-level correctness (Mündler et al., 2024; Zhuge et al., 2024; Liu et al., 2024b; Dunsin, 2025) and overlook repo-level effects. We push evaluation to the software-wide scale by embedding generated patches into a CI pipeline, allowing us to observe their impact on build stability, regression risk, and interactions across the codebase. Current benchmarks (Jain et al., 2024; Wang et al., 2025b) still lack the ability to stress-test models in this way, as they do not incorporate an adversarial agent that detects corner cases and synthesizes new unit tests. [Complementary efforts such as BigCodeBench \(Zhuo et al., 2024\) and DebugBench \(Tian et al., 2024\) target diverse function-level generation and debugging capabilities, but likewise do not embed models into full CI pipelines or dual-role submitter-reviewer workflows.](#)

2.3 RETRIEVAL-AUGMENTED GENERATION

The context window limitations of LLMs present a major bottleneck for handling large-scale codebases. Retrieval-augmented generation (RAG) techniques (Jimenez et al., 2023; Xie et al., 2025; Yang et al., 2024) have emerged as a practical solution. While advanced code retrieval methods explore structured representations like code graphs or abstract syntax trees for more semantic understanding, many current approaches still rely on lexical methods like BM25 (Robertson et al., 2009) for document and function name retrieval, without incorporating static code analysis or fine-grained code structure understanding. This often hampers performance on tasks that require identifying relevant functional code blocks from complex repositories (Feng et al., 2020; Parvez et al., 2021; Zhang et al., 2023b; Xia et al., 2024). For SWINGARENA, we opted for a robust and language-agnostic RAG pipeline to serve as a strong, reproducible baseline, acknowledging that more sophisticated, language-specific retrieval strategies represent a promising direction for future work. [In parallel, CodeRAGBench \(Generation, 2024\) studies whether retrieval augments code generation in controlled settings, and RepoCoder \(Zhang et al., 2023a\) proposes repository-level iterative retrieval-generation; both are complementary to our CI-based, dual-role arena.](#)

3 SWINGARENA

3.1 DATA CONSTRUCTION

This section details the data construction pipeline for SWINGARENA. The pipeline comprises several stages: Repository Mining, CI Test Filtering, LLM Filtering, and Expert Filtering. An overview of this process is presented in Figure 1.

Repository Mining We identify high-quality repositories via the GitHub API, prioritizing those with high popularity including star count as a proxy for code quality and community validation. We assume that patches and unit tests merged into such repositories have typically undergone extensive expert review, increasing the likelihood of correctness. For each selected repository, we collect metadata including license, forks, and activity, and clone those meeting our predefined criteria. From these, we extract real-world PRs linked to their corresponding issues, along with code diffs, patch content, and metadata. We then convert raw PRs data into structured benchmark instances by extracting problem statements from PR descriptions and issues, gathering associated patches and available test cases. Each task is enriched with contextual metadata including repo ID, base commit SHA, patch, timestamp, and CI lists, enabling realistic and fine-grained evaluation of LLM performance. We release a curated, CI-grounded dataset of 2,300 (issue, PR) pairs and provide 400 evaluation instances (100 per language) plus a 100-sample ablation split. We include license-aware distribution and scripts for reproducible retrieval and CI execution. We focus on four programming languages—Rust, Go, C++, and Python—based on their prevalence in open-source repositories and the maturity of their associated CI ecosystems. These languages dominate most large-scale codebases and account for a substantial portion of real-world software development activity.

CI Test Filtering After mining repositories, we integrate corresponding CI configurations including GitHub Actions and Travis CI³ for each PR to fully replicate the real-world end-to-end software development process, including each repository’s testing and build requirements. We retain only instances that pass all CI checks, ensuring they meet project-specific quality standards. Instances with verified test coverage are prioritized. This step ensures that the benchmark consists of code changes that correspond with real-world, automated validation pipelines. By integrating CI, our pipeline enforces strict validation, creating our original instances pool that mirrors the constraints and practices of professional software development.

LLM Filtering To improve dataset quality and balance instances’ difficulty, we employ LLM-as-a-Judge (Zheng et al., 2023) to assess the clarity of each pull request’s problem statement and estimate its overall difficulty. The LLM (Grok-3-beta (xAI, 2024)) is required to provide reasons for its clarity and difficulty assessments, ensuring each evaluation is supported by a rationale for the sake of final expert verification. This process enables us to categorize tasks by complexity, creating a structured and balanced benchmark.

Expert Filtering Following LLM-as-a-Judge, human experts finally reviewed and calibrated LLM-generated assessments. Annotators examine the clarity and difficulty scores, along with the model’s rationales, and either confirm or correct them. If the model’s justification is unclear or misaligned with the problem, human experts intervene to ensure consistent and accurate labeling. This step mitigates LLM hallucinations and filters out low-quality problem statements, incoherent instances, or those with unreachable expired multi-modal attachment. After all the data construction process, the final data statistics of SWINGARENA can be found in Appendix B.

3.2 ARENA

In this section, we introduce SWINGARENA, an adversarial evaluation framework that facilitates direct competition against models on a set of programming tasks. Unlike traditional benchmarks focused on static code snippet completion or single unit tests, SWINGARENA creates a dynamic, interactive environment that simulates real-world software development workflows. Specifically, the framework emulates the process of submitting and reviewing code changes: one model assumes the role of a *submitter*, proposing a candidate solution, while another acts as a *reviewer*, critiquing the submission and—crucially—generating additional test cases to expose potential flaws or edge cases.

³Travis CI is a continuous integration service that automates testing and deployment of code changes in software projects.

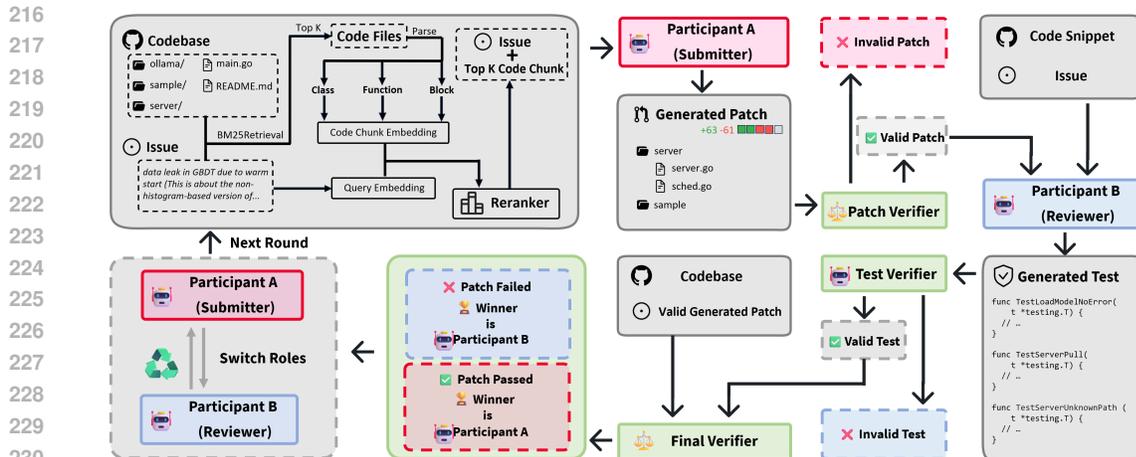


Figure 2: Illustration of the SWINGARENA adversarial evaluation framework. The framework simulates an adversarial software engineering workflow between two agents—a *submitter* and a *reviewer*—who alternate roles and iteratively refine their solutions based on CI feedback.

This interactive setup accurately mirrors human software collaboration and enables richer evaluation along multiple dimensions, including reasoning depth, code robustness, and collaborative capability.

Interactive Environment SWINGARENA employs a modular, multi-agent interactive environment in which language models act as software engineers. Each model receives the same task description and access to a shared codebase but may take different approaches to generate patches and unit test cases. Our end-to-end environment includes components for retrieving relevant code, extracting and ranking code chunks, and generating code patches. Code modifications are validated using real-world CI pipelines, ensuring that models produce solutions that compile, run, and pass automated tests. This design takes a significant step toward aligning model evaluation with real-world software engineering scenarios.

Battle Protocol We denote a single round of adversarial patch and test case generation, evaluation, and scoring between the submitter and reviewer as a battle. Given a problem statement with a buggy program and its description, the submitter generates a patch to fix the issue, while the reviewer creates a test case to challenge the patch’s correctness. A CI pipeline evaluates both: the patch is checked for compilation and correctness, and the test case is assessed for its ability to expose flaws. The submitter receives a score of +1 for patches passing all tests (including the reviewer’s), or -1 for any failure. The reviewer receives a score of +1 if their test fails the submitter’s patch, revealing a fault, and -1 if it fails the golden human patch. Models alternate roles across multiple rounds with CI feedback for iterative refinement, simulating dynamic software development.

Verification SWINGARENA integrates real-world CI workflows into the evaluation process as a verification mechanism. For each repository, existing CI pipelines—including GitHub Actions—are executed locally within container environments, preserving the exact logic, dependencies, and toolchains used by human developers. Language-specific execution logic is supported when needed including Rust’s `cargo` system⁴, ensuring accurate and faithful testing. All evaluations run inside isolated Docker containers to guarantee reproducibility and avoid cross-task contamination. Given that different repositories require different runtime environments, developers can mitigate environment-related inconsistencies by supplying configuration files including `Dockerfile`, `.yaml`, or `environment.yml` to automatically construct test environments in containers for consistent and automated verification.

Evaluation SWINGARENA evaluates models through adversarial interactions between two agents—the patch generator (the *submitter*) and the test case generator (the *reviewer*)—executed within real-world CI workflows. Each task begins by validating the baseline and golden human patch via CI to establish correctness references. In each round, the *submitter* proposes a patch, compared

⁴`cargo` is Rust’s build tool and package manager, used to compile code, manage dependencies, and run tests.

270 against the golden human fix; incorrect patches incur penalties. The *reviewer* then generates a unit
271 test intended to expose faults. The patch and test are jointly applied and verified through CI: passing
272 tests reward the *submitter*, and failing ones reward the *reviewer*. Both models take turns acting as
273 the submitter and reviewer across rounds, ensuring a balanced assessment of their patch and test
274 generation capabilities. Evaluation metrics include binary task success, win rates, and Best@k, with
275 CI logs and model outputs aggregated into structured reports.

276 **Reviewer Test Quality Gates** To control evaluation variance and prevent exploitative behavior,
277 reviewer-generated tests must: compile and pass when applied to the golden patch; refrain from
278 modifying production code or existing tests; limit edits in any new test file to a bounded number
279 of lines; avoid sources of nondeterminism; and conform to repository linting and style guidelines.
280 Any violation results in automatic test rejection and forfeiture of the reviewer’s reward. These gates
281 apply only to reviewer-generated tests; instability arising from repository-native tests or external
282 environment is analyzed separately in our failure typology (F2) in Appendix C. See Appendix D for
283 more details.

284 3.3 RETRIEVAL-AUGMENTED CODE GENERATION

285 Real-world programming tasks often involve reasoning over large, multi-file codebases where relevant
286 context is scattered across thousands of lines. While prior work (Xia et al., 2024; Xie et al., 2025)
287 has tried to address long-context challenges (mainly in Python) via AST-based parsing (Foundation,
288 2023), these approaches typically (i) emphasize a single language, (ii) lack a *context packing policy*
289 under strict token budgets, and (iii) do not integrate with adversarial CI evaluation. To address
290 these limitations, SWINGARENA introduces a multi-language Retrieval-Augmented Code Generation
291 (**RACG**) framework that combines static code analysis and dense retrieval to efficiently extract, rank,
292 and *pack* relevant code snippets to the model. [We position RACG as a standardized baseline \(not
293 a standalone algorithmic contribution\) that is nevertheless necessary to harmonize context access
294 across models/languages, control long-context confounds and variance, and avoid unfairly favoring
295 larger context windows; its utility is empirically supported by consistent gains in Table 3.](#)

296 **File Retriever** During the file retrieval stage, SWINGARENA employs a coarse-to-fine-grained
297 filtering step using a lightweight `FileRetriever`. This component utilizes a classical sparse
298 retrieval method, BM25, to rank source files based on lexical similarity to the problem description,
299 treating the problem as a query and each file as a document. This step prunes irrelevant files early,
300 narrowing the search space and boosting efficiency for subsequent dense retrieval. Only the top-*k*
301 most relevant files, as ranked by BM25, are forwarded to the `CodeChunker` for decomposition
302 into semantically meaningful code chunks. This hierarchical retrieval pipeline—from file-level
303 sparse matching to chunk-level dense reranking—enables SWINGARENA to scale effectively to large
304 codebases while preserving high precision in final context selection.

305 **CodeChunker** SWINGARENA uses a hierarchical, syntax-aware chunking strategy via
306 `CodeChunker` to decompose codebases into semantically meaningful units including functions,
307 classes, and blocks, preserving structural integrity and improving retrieval precision. It supports
308 multiple programming languages—including Rust, Python, C++ and Go—through language-specific
309 parsing rules. When parsing is unavailable or impractical, the system falls back to regex-based
310 heuristics tailored to each language, ensuring robustness across diverse codebases.

311 **CodeReranker** Given the large number of candidate chunks in real-world codebases, SWINGARENA
312 employs `CodeReranker` to prioritize content effectively within the model’s context window. It
313 uses CodeBERT (Feng et al., 2020) to encode both the problem statement and code chunks into dense
314 vectors, ranking them by cosine similarity to identify the most relevant segments. Beyond naive
315 top-*k* selection, we incorporate (i) language-aware tie-breaking (favoring definitions over usages), (ii)
316 proximity bias (neighboring chunks near an already selected target receive a small boost), and (iii)
317 de-duplication across files to avoid redundant inclusions.

318 **Token Budget-Aware Context Management** To optimize token usage under strict token limits,
319 SWINGARENA employs a dynamic token budgeting mechanism that incrementally selects and packs
320 code chunks until the token threshold is reached. Crucially, it adapts chunk granularity based on
321 available context window size—favoring coarser chunks when space permits and switching to finer-
322 grained chunks as the budget tightens. Each included chunk is enriched with metadata (file path,
323

symbol kind, function/class name, line range). This policy is *deterministic* given the retrieval scores and budget, improving reproducibility across runs.

Variance Control in the Adversarial Arena The adversarial setting introduces interaction-induced variance. We bound variance via: (i) fixed system and user prompts; (ii) a capped number of rounds and retries; (iii) temperature=0 decoding in all primary evaluations, using controlled higher-temperature sampling only in the scaling-law study; (iv) unified CI recipes executed via `act` with pinned images; and (v) fixed random seeds for retrieval and any sampling components. These choices make our outcomes reproducible while retaining the benefits of interactive stress.

Battle Protocol We denote a single round of adversarial patch and test case generation, evaluation, and scoring between the submitter and reviewer as a battle. Given a problem statement with a buggy program and its description, the submitter generates a patch to fix the issue. Concurrently, the reviewer creates a test case designed not merely to validate, but to strategically challenge the patch’s correctness by probing for edge cases and potential weaknesses. To foster this adversarial nature, the reviewer is provided with contextual hints including which parts of the code were most changed by the patch, and is prompted to design tests that specifically target the logic of the fix. A CI pipeline evaluates both: the patch is checked for compilation and correctness, and the test case is assessed for its ability to expose flaws. The submitter receives a score of +1 for patches passing all tests (including the reviewer’s), or -1 for any failure. The reviewer receives a score of +1 if their test fails the submitter’s patch, revealing a fault, and -1 if it fails the golden human patch. Models alternate roles across multiple rounds with CI feedback for iterative refinement, simulating dynamic software development.

4 EXPERIMENTS

4.1 EXPERIMENTAL SETUP

Baselines We evaluate our method against several strong models: GPT-4o (Achiam et al., 2023), a general-purpose LLM optimized for multimodal reasoning; Claude-3.5 (Anthropic, 2024), an instruction-tuned model for code reasoning; Gemini-2.0 (Google, 2023), a multimodal model with robust programming capabilities; and DeepSeek-V3 (Liu et al., 2024a), a code-augmented model trained on large-scale software repositories. Besides, we use Qwen2.5-Coder-7B-Instruct (Hui et al., 2024) to do ablation studies.

Data Division We collect over 2,300 issues with corresponding solutions from GitHub, covering Rust, Python, Go, and C++. This dataset is divided into two parts: 400 high-quality samples filtered from the full set (100 per language) are used for evaluation, while the remaining data are reserved for future community-driven training. To comprehensively evaluate both the *submitter* and the *reviewer* across programming languages while ensuring experimental efficiency, we design two settings: (1) a battle scenario using the 400 selected samples, and (2) an ablation experiment using 100 samples (25 high-quality random samples from each language).

Metrics Let \mathcal{T} denote the set of tasks and k the number of independent attempts per task (when applicable). We formalize the following metrics.

Best@ k : a task is counted as solved if at least one of k independent generations succeeds. Formally, $\text{Best}@k = \frac{1}{|\mathcal{T}|} \sum_{t \in \mathcal{T}} \mathbf{1}\{\exists i \leq k : \text{success}(t, i)\}$.

CI pass rate: we report submitter-side and reviewer-side CI check pass rates averaged over tasks. Submitter CI Pass Rate (SPR) averages, for each task, the fraction of submitter-side checks passed by the generated patch (excluding reviewer tests), then averages across tasks. Reviewer CI Pass Rate (RPR) analogously averages the fraction of reviewer-generated tests that pass against the golden patch.

Formally, let $\mathcal{C}_{\text{sub}}(t)$ denote submitter-side checks for task t and $\mathcal{C}_{\text{rev}}(t)$ reviewer-side checks. Then

$$\text{SPR} = \frac{1}{|\mathcal{T}|} \sum_{t \in \mathcal{T}} \frac{1}{|\mathcal{C}_{\text{sub}}(t)|} \sum_{c \in \mathcal{C}_{\text{sub}}(t)} \mathbf{1}\{\text{pass}(t, c)\}, \quad \text{RPR} = \frac{1}{|\mathcal{T}|} \sum_{t \in \mathcal{T}} \frac{1}{|\mathcal{C}_{\text{rev}}(t)|} \sum_{c \in \mathcal{C}_{\text{rev}}(t)} \mathbf{1}\{\text{pass}(t, c)\}.$$

Win Rate: the fraction of battles whose final outcome is that the submitter’s patch passes all CI checks (including reviewer tests) and agrees with the golden fix. Note that Win Rate is *adversarial*:

Table 1: Evaluation of Code Submission vs. Test Submission Capabilities Among Proprietary LLMs.

Matchup	Submitter	Reviewer	RPR	SPR	Win Rate
GPT-4o vs GPT-4o	GPT-4o	GPT-4o	0.71	0.68	0.97
GPT-4o vs Claude	GPT-4o	Claude	0.65	0.55	0.90
GPT-4o vs Gemini	GPT-4o	Gemini	0.61	0.55	0.94
GPT-4o vs DeepSeek	GPT-4o	DeepSeek	0.61	0.55	0.94
Claude vs GPT-4o	Claude	GPT-4o	0.66	0.55	0.89
Claude vs Claude	Claude	Claude	0.62	0.62	1.00
Claude vs Gemini	Claude	Gemini	0.59	0.55	0.96
Claude vs DeepSeek	Claude	DeepSeek	0.64	0.54	0.90
Gemini vs GPT-4o	Gemini	GPT-4o	0.61	0.55	0.94
Gemini vs Claude	Gemini	Claude	0.60	0.56	0.96
Gemini vs Gemini	Gemini	Gemini	0.72	0.63	0.91
Gemini vs DeepSeek	Gemini	DeepSeek	0.64	0.64	1.00
DeepSeek vs GPT-4o	DeepSeek	GPT-4o	0.60	0.55	0.95
DeepSeek vs Claude	DeepSeek	Claude	0.60	0.55	0.95
DeepSeek vs Gemini	DeepSeek	Gemini	0.68	0.64	0.96
DeepSeek vs DeepSeek	DeepSeek	DeepSeek	0.70	0.66	0.96

higher values may also indicate weaker reviewer tests, so it should be interpreted together with SPR/RPR. In our analysis, SPR and RPR serve as the primary capability metrics for submitter and reviewer, while Win Rate is used as an adversarial outcome indicator that must be read jointly with SPR/RPR rather than as a standalone measure of absolute strength.

Implementation Details For both patch and test case generation in the arena evaluation, we employ carefully crafted system and user prompts. The system prompt instructs the model to behave as a senior software engineer or test automation expert, while the user prompt provides the issue description, relevant code snippets, and contextual metadata. All model outputs are required to follow a structured JSON schema to enable automated downstream evaluation.

We set the generation temperature to 0 to ensure deterministic outputs. The maximum number of generated tokens is configured based on the model’s context window and the specific requirements of each task. For RACG, we limit the number of retrieved files to 5 and allow a maximum of 16 code chunks per query, where each chunk corresponds to a syntactic code chunk. The retrieval agent supports up to 3 retry attempts to handle transient failures or timeouts.

Battle Protocol Configuration: We configure the evaluation rounds through system parameters. In our experiments, we set a total of **10 rounds** for each battle, where each agent executes **5 rounds** in each role (submitter and reviewer). This configuration aims to provide a balanced assessment of both agents’ capabilities while maintaining experimental efficiency. The battle terminates after completing all rounds, and the final win rate is computed from cumulative outcomes across rounds.

Fairness and Harmonization For fairness, we harmonize the maximum prompt-plus-generation token budget across proprietary models to a common value B and do not exceed B even if a model supports a larger context window. We log API versions and evaluation dates, apply the same rate limits, and use identical decoding parameters (temperature, top- p). We also record API failures and retries and confirm in Appendix that excluding failed calls does not change rankings. Open-source model results are reported in Table 4.

Reproducibility and Artifacts We provide anonymized artifacts (prompts, JSON schemas, scripts, pinned images). The exact evaluation workflow is summarized by Algorithm 1.

4.2 MAIN RESULT

Adversarial Programming Battle Outcomes Table 1 presents a comparative analysis of RPR, SPR across proprietary LLMs evaluated using the SWINGARENA adversarial framework. Each model is tested in both self-play scenarios and cross-play scenarios (Claude vs Gemini), alternating roles as *submitter* and *reviewer*. Several trends emerge: Strong Self-Consistency: All models show high win rates when reviewing their own submissions—Claude (1.00), GPT-4o (0.97), Gemini

(0.91), DeepSeek (0.96)—indicating strong internal alignment between patch generation and test case generation.

GPT-4o’s Aggressive Patching Advantage: GPT-4o achieves win rates ≥ 0.90 as a submitter regardless of the reviewer, highlighting its dominance in producing adversarially-strong patches. However, its relatively lower RPR/SPR scores (0.65/0.55 vs Claude) suggest variability in overall correctness. **DeepSeek and Gemini’s Reliability:** Although their win rates as submitters are slightly lower, DeepSeek and Gemini yield the highest CI pass rates (up to 0.66 and 0.64 respectively), reflecting their strength in generating reliably test-passing code. **Asymmetry in Matchups:** Pairwise comparisons reveal minor asymmetries (GPT-4o vs Claude at 0.90 vs Claude vs GPT-4o at 0.89), indicating the reviewer model subtly affects the outcome, likely due to differing review strictness.

Overall, GPT-4o excels in assertive patch generation, while DeepSeek and Gemini prioritize correctness and CI stability. The evaluation further underscores the critical yet nuanced role reviewers play in determining adversarial outcomes. **This is not contradictory: Win Rate measures adversarial end-point success, whereas SPR averages submitter-side CI checks across tasks/attempts. An “all-or-nothing” submitter can attain high Win Rate by producing fewer but complete, CI-clean patches that withstand reviewer tests, while still exhibiting lower average SPR due to unstable intermediate attempts or broader CI fragility. Conditional on passing non-reviewer CI, GPT-4o shows a high probability of winning against reviewer tests, consistent with an aggressive patching strategy.**

Table 2: Best@3 across Models and Languages.

Model	Average	C++	Go	Rust	Python
Gemini	0.57	0.64	0.58	0.51	0.57
DeepSeek	0.59	0.64	0.61	0.58	0.52
GPT-4o	0.57	0.63	0.53	0.56	0.54
Claude	0.55	0.63	0.55	0.52	0.50

Language-Specific Evaluation In addition to the overall performance comparison, we further analyze how each model performs across different programming languages. As shown in Table 2, DeepSeek achieves the highest average Best@3 score (0.59), followed closely by Gemini and GPT-4o (both at 0.57), and Claude (0.55). When broken down by language, all models perform best on C++ and relatively worse on Rust and Python, suggesting variation in model proficiency across language-specific problem formulations. Notably, DeepSeek shows generally strong results across languages, particularly in Rust (0.58) and Go (0.61), suggesting more robust generalization on our tasks. These results indicate that DeepSeek exhibits a relatively balanced multi-language code reasoning ability among the evaluated models.

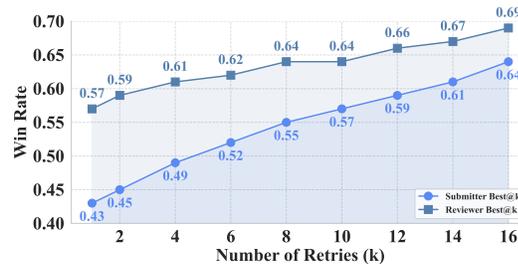


Figure 3: Best@k win rate.

Open-source matchups. Additional results on open-source models are summarized in Table 4.

Best@k Sampling for Win Rate We analyze the probability of success when the *submitter* (*Qwen2.5-Coder-7B-Instruct*) and *reviewer* (*Qwen2.5-Coder-7B-Instruct*) independently generate up to k attempts per role at temperature 0.25. A task counts as successful if at least one attempt yields a passing outcome. This Best@k curve characterizes test-time scaling behavior under our arena protocol.

4.3 ABLATION STUDY ON COMPONENTS OF SWINGARENA

Ablation Study on RACG Table 3 reports ablation results for the RACG module on *submitter*. The upper section shows results across four programming languages (C++, Python, Go, Rust), comparing model performance with and without RACG. The lower section includes retrieval-based baselines: BM25 and Top-k related retrievals (with $k = 2, 10, 20$) followed by reranking. Across languages, incorporating RACG generally improves both Best@3 and Win Rate. For instance, in the C++ setting, RACG raises Best@3 from 0.38 to 0.42 and Win Rate from 0.77 to 0.84; similar gains are observed in several cases for Python, Rust, and Go. Compared to retrieval-only methods, RACG-enhanced

approaches often outperform BM25. Top-20 retrieval achieves the strongest baseline result (Best@3 = 0.43, Win Rate = 0.73), representing a 0.11 improvement in win rate over using BM25 alone (0.62).

We implement Top-k Related as: BM25 file-level retrieval to obtain the top-k related files, coarse block-level chunking of those files, a cosine-similarity dense reranker between the problem statement and each chunk, and greedy packing under the token budget; unlike RACG, it does not use syntax-aware granularity switching, proximity bias across neighboring chunks, or cross-file de-duplication. We acknowledge that our RACG design, particularly the fixed Top-5 file retrieval limit, may act as a bottleneck for complex issues requiring broader context. As detailed in our failure analysis in Appendix C, a notable portion of failures can be attributed to retrieval limitations. This suggests that while our RACG serves as a strong baseline, exploring more dynamic retrieval strategies is a key avenue for future improvement.

Patch Localization Accuracy Table 6 shows the fraction of queries whose golden-patch file is retrieved within the Top-2, Top-10, or Top-20 results under four strategies: lexical file-level BM25 and chunk-level retrieval over Block, Function, and Class units (with hits mapped back to their parent files). Finer granularity boosts accuracy—switching from BM25 to class-level retrieval more than doubles the Top-10 hit rate (20.7% (\rightarrow) 48.7%). Most of the improvement arises early in the ranking; curves flatten beyond Top-10, implying that the correct file is usually exposed near the top of the list. BM25 can lag as it relies primarily on term overlap, lacking the structural and semantic cues exploited by chunk-based methods. While class-level retrieval is generally effective due to its rich contextual information and noise suppression, it often exceeds the LLM’s context window, limiting its practical utility. To address this, we adopt a block-level reranker, which offers a finer granularity that fits within context limits while still guiding locating the correct patch position.

Table 3: RACG Ablation Comparison.

Method	Best@3	Win Rate
C++ w/ RACG	0.42	0.84
C++ w/o RACG	0.38	0.77
Python w/ RACG	0.46	0.84
Python w/o RACG	0.44	0.71
Rust w/ RACG	0.58	0.75
Rust w/o RACG	0.49	0.72
Go w/ RACG	0.45	0.80
Go w/o RACG	0.37	0.71
BM25	0.38	0.62
Top-2 Related	0.42	0.69
Top-10 Related	0.43	0.72
Top-20 Related	0.43	0.73

4.4 DATA ANALYSIS AND FAILURE PATTERNS

We discuss the data analysis and failure patterns in Appendix C. In brief, prompts are much shorter than repository context (code dominates the token budget); token usage across model pairs remains manageable; and finer-grained retrieval substantially improves Top-10 file hit rates over BM25. See Figures 6, 4 and Table 6 for details.

5 CONCLUSION

This paper introduces SWINGARENA, a unified framework for evaluating and enhancing LLM-based program repair and test generation under real-world constraints. By modeling interactions between submitter and reviewer agents across multiple languages, it offers holistic benchmarking aligned with practical software workflows. To handle large, diverse code contexts, we propose a Retrieval-Augmented Code Generation (RACG) module combining static analysis, dense retrieval, and token-aware context packing. Experiments across four languages show SWINGARENA provides nuanced insights into model capabilities, revealing trade-offs between patch assertiveness, correctness, and review strictness, thus bringing evaluation closer to real-world scenarios.

REFERENCES

- 540
541
542 Josh Achiam, Steven Adler, Sandhini Agarwal, Lama Ahmad, Ilge Akkaya, Florencia Leoni Aleman,
543 Diogo Almeida, Janko Altenschmidt, Sam Altman, Shyamal Anadkat, et al. Gpt-4 technical report.
544 *arXiv preprint arXiv:2303.08774*, 2023.
- 545 Reem Aleithan, Haoran Xue, Mohammad Mahdi Mohajer, Elijah Nnorom, Gias Uddin, and Song
546 Wang. Swe-bench+: Enhanced coding benchmark for llms. *arXiv preprint arXiv:2410.06992*,
547 2024.
- 548 Anthropic. Introducing claude 3.5 sonnet, 2024. URL <https://www.anthropic.com/news/claude-3-5-sonnet>. Accessed: 2025-05-12.
549
550
- 551 Jacob Austin, Augustus Odena, Maxwell Nye, Maarten Bosma, Henryk Michalewski, David Dohan,
552 Ellen Jiang, Carrie Cai, Michael Terry, Quoc Le, et al. Program synthesis with large language
553 models. *arXiv preprint arXiv:2108.07732*, 2021.
- 554 ByteDance_Seed. Seed-coder : Let the code model curate data for itself. *arXiv preprint*
555 *arXiv:upcoming*, 2025.
556
- 557 Mark Chen, Jerry Tworek, Heewoo Jun, Qiming Yuan, Henrique Ponde De Oliveira Pinto, Jared
558 Kaplan, Harri Edwards, Yuri Burda, Nicholas Joseph, Greg Brockman, et al. Evaluating large
559 language models trained on code. *arXiv preprint arXiv:2107.03374*, 2021.
- 560 Daniel Dunsin. Enhancing software quality and efficiency: The role of generative ai in automated
561 code generation and testing. *arxiv*, 2025.
562
- 563 Mikhail Evtikhiev, Egor Bogomolov, Yaroslav Sokolov, and Timofey Bryksin. Out of the bleu: how
564 should we assess quality of the code generation models? *Journal of Systems and Software*, 203:
565 111741, 2023.
- 566 Zhangyin Feng, Daya Guo, Duyu Tang, Nan Duan, Xiaocheng Feng, Ming Gong, Linjun Shou, Bing
567 Qin, Ting Liu, Daxin Jiang, et al. Codebert: A pre-trained model for programming and natural
568 languages. *arXiv preprint arXiv:2002.08155*, 2020.
- 569 Python Software Foundation. ast — abstract syntax trees. <https://docs.python.org/3/library/ast.html>, 2023.
570
571
- 572 CRAC Generation. Coderag-bench: Can retrieval augment code generation, 2024.
- 573 Google. Gemini: Our new large language model, December 2023. URL <https://blog.google/technology/ai/google-gemini-ai/>. Accessed: 2025-05-12.
574
575
- 576 Binyuan Hui, Jian Yang, Zeyu Cui, Jiayi Yang, Dayiheng Liu, Lei Zhang, Tianyu Liu, Jiajun Zhang,
577 Bowen Yu, Keming Lu, et al. Qwen2. 5-coder technical report. *arXiv preprint arXiv:2409.12186*,
578 2024.
- 579 Kush Jain, Gabriel Synnaeve, and Baptiste Rozière. Testgeneval: A real world unit test generation
580 and test completion benchmark. *arXiv preprint arXiv:2410.00752*, 2024.
581
- 582 Carlos E Jimenez, John Yang, Alexander Wettig, Shunyu Yao, Kexin Pei, Ofir Press, and Karthik
583 Narasimhan. Swe-bench: Can language models resolve real-world github issues? *arXiv preprint*
584 *arXiv:2310.06770*, 2023.
- 585 Raymond Li, Loubna Ben Allal, Yangtian Zi, Niklas Muennighoff, Denis Kocetkov, Chenghao Mou,
586 Marc Marone, Christopher Akiki, Jia Li, Jenny Chim, et al. Starcoder: may the source be with
587 you! *arXiv preprint arXiv:2305.06161*, 2023.
588
- 589 Aixin Liu, Bei Feng, Bing Xue, Bingxuan Wang, Bochao Wu, Chengda Lu, Chenggang Zhao,
590 Chengqi Deng, Chenyu Zhang, Chong Ruan, et al. Deepseek-v3 technical report. *arXiv preprint*
591 *arXiv:2412.19437*, 2024a.
- 592 Zhijie Liu, Yutian Tang, Xiapu Luo, Yuming Zhou, and Liang Feng Zhang. No need to lift a finger
593 anymore? assessing the quality of code generation by chatgpt. *IEEE Transactions on Software Engineering*, 2024b.

- 594 Anton Lozhkov, Raymond Li, Loubna Ben Allal, Federico Cassano, Joel Lamy-Poirier, Nouamane
595 Tazi, Ao Tang, Dmytro Pykhtar, Jiawei Liu, Yuxiang Wei, et al. Starcoder 2 and the stack v2: The
596 next generation. *arXiv preprint arXiv:2402.19173*, 2024.
- 597
598 Niels Mündler, Mark Niklas Müller, Jingxuan He, and Martin Vechev. Code agents are state of the
599 art software testers. In *ICML 2024 Workshop on LLMs and Cognition*, 2024.
- 600
601 Md Rizwan Parvez, Wasi Uddin Ahmad, Saikat Chakraborty, Baishakhi Ray, and Kai-Wei Chang.
602 Retrieval augmented code generation and summarization. *arXiv preprint arXiv:2108.11601*, 2021.
- 603
604 Muhammad Shihab Rashid, Christian Bock, Yuan Zhuang, Alexander Buccholz, Tim Esler, Simon
605 Valentin, Luca Franceschi, Martin Wistuba, Prabhu Teja Sivaprasad, Woo Jung Kim, et al. Swe-
606 polybench: A multi-language benchmark for repository level evaluation of coding agents. *arXiv
preprint arXiv:2504.08703*, 2025.
- 607
608 Stephen Robertson, Hugo Zaragoza, et al. The probabilistic relevance framework: Bm25 and beyond.
609 *Foundations and Trends® in Information Retrieval*, 3(4):333–389, 2009.
- 610
611 Mohammed Latif Siddiq, Simantika Dristi, Joy Saha, and Joanna CS Santos. The fault in our stars:
612 Quality assessment of code generation benchmarks. In *2024 IEEE International Conference on
Source Code Analysis and Manipulation (SCAM)*, pp. 201–212. IEEE, 2024.
- 613
614 Runchu Tian, Yining Ye, Yujia Qin, Xin Cong, Yankai Lin, Yinxu Pan, Yesai Wu, Hui Haotian, Liu
615 Weichuan, Zhiyuan Liu, et al. Debugbench: Evaluating debugging capability of large language
616 models. In *Findings of the Association for Computational Linguistics: ACL 2024*, pp. 4173–4198,
617 2024.
- 618
619 Xinchun Wang, Pengfei Gao, Chao Peng, Ruida Hu, and Cuiyun Gao. Codevisionary: An agent-
620 based framework for evaluating large language models in code generation. *arXiv preprint
arXiv:2504.13472*, 2025a.
- 621
622 Yibo Wang, Congying Xia, Wenting Zhao, Jiangshu Du, Chunyu Miao, Zhongfen Deng, Philip S Yu,
623 and Chen Xing. Projecttest: A project-level unit test generation benchmark and impact of error
624 fixing mechanisms. *arXiv preprint arXiv:2502.06556*, 2025b.
- 625
626 You Wang, Michael Pradel, and Zhongxin Liu. Are "solved issues" in swe-bench really solved
627 correctly? an empirical study. *arXiv preprint arXiv:2503.15223*, 2025c.
- 628
629 xAI. Grok-3. <https://x.ai>, 2024. Accessed: 2025-05-15.
- 630
631 Chunqiu Steven Xia, Yinlin Deng, Soren Dunn, and Lingming Zhang. Agentless: Demystifying
632 llm-based software engineering agents. *arXiv preprint arXiv:2407.01489*, 2024.
- 633
634 Chengxing Xie, Bowen Li, Chang Gao, He Du, Wai Lam, Difan Zou, and Kai Chen. Swe-fixer:
635 Training open-source llms for effective and efficient github issue resolution. *arXiv preprint
arXiv:2501.05040*, 2025.
- 636
637 John Yang, Carlos Jimenez, Alexander Wettig, Kilian Lieret, Shunyu Yao, Karthik Narasimhan,
638 and Ofir Press. Swe-agent: Agent-computer interfaces enable automated software engineering.
639 *Advances in Neural Information Processing Systems*, 37:50528–50652, 2024.
- 640
641 Burak Yetistiren, Isik Ozsoy, and Eray Tuzun. Assessing the quality of github copilot’s code
642 generation. In *Proceedings of the 18th international conference on predictive models and data
analytics in software engineering*, pp. 62–71, 2022.
- 643
644 Daoguang Zan, Zhirong Huang, Ailun Yu, Shaoxin Lin, Yifan Shi, Wei Liu, Dong Chen, Zongshuai
645 Qi, Hao Yu, Lei Yu, et al. Swe-bench-java: A github issue resolving benchmark for java. *arXiv
preprint arXiv:2408.14354*, 2024.
- 646
647 Daoguang Zan, Zhirong Huang, Wei Liu, Hanwu Chen, Linhao Zhang, Shulin Xin, Lu Chen, Qi Liu,
Xiaojian Zhong, Aoyan Li, et al. Multi-swe-bench: A multilingual benchmark for issue resolving.
arXiv preprint arXiv:2504.02605, 2025.

648 Fengji Zhang, Bei Chen, Yue Zhang, Jacky Keung, Jin Liu, Daoguang Zan, Yi Mao, Jian-Guang Lou,
649 and Weizhu Chen. Repocoder: Repository-level code completion through iterative retrieval and
650 generation. *arXiv preprint arXiv:2303.12570*, 2023a.

651 Kechi Zhang, Jia Li, Ge Li, Xianjie Shi, and Zhi Jin. Codeagent: Enhancing code generation
652 with tool-integrated agent systems for real-world repo-level coding challenges. *arXiv preprint*
653 *arXiv:2401.07339*, 2024.

654 Xiangyu Zhang, Yu Zhou, Guang Yang, and Taolue Chen. Syntax-aware retrieval augmented code
655 generation. In *Findings of the Association for Computational Linguistics: EMNLP 2023*, pp.
656 1291–1302, 2023b.

657 Lianmin Zheng, Wei-Lin Chiang, Ying Sheng, Siyuan Zhuang, Zhanghao Wu, Yonghao Zhuang,
658 Zi Lin, Zhuohan Li, Dacheng Li, Eric P. Xing, Hao Zhang, Joseph E. Gonzalez, and Ion Stoica.
659 Judging llm-as-a-judge with mt-bench and chatbot arena, 2023. URL <https://arxiv.org/abs/2306.05685>.

660 Qihao Zhu, Daya Guo, Zhihong Shao, Dejian Yang, Peiyi Wang, Runxin Xu, Y Wu, Yukun Li,
661 Huazuo Gao, Shirong Ma, et al. Deepseek-coder-v2: Breaking the barrier of closed-source models
662 in code intelligence. *arXiv preprint arXiv:2406.11931*, 2024.

663 Mingchen Zhuge, Changsheng Zhao, Dylan Ashley, Wenyi Wang, Dmitrii Khizbullin, Yunyang Xiong,
664 Zechun Liu, Ernie Chang, Raghuraman Krishnamoorthi, Yuandong Tian, et al. Agent-as-a-judge:
665 Evaluate agents with agents. *arXiv preprint arXiv:2410.10934*, 2024.

666 Terry Yue Zhuo, Minh Chien Vu, Jenny Chim, Han Hu, Wenhao Yu, Ratnadira Widayarsi, Imam
667 Nur Bani Yusuf, Haolan Zhan, Junda He, Indraneil Paul, et al. Bigcodebench: Benchmarking code
668 generation with diverse function calls and complex instructions. *arXiv preprint arXiv:2406.15877*,
669 2024.

670
671
672
673
674
675
676
677
678
679
680
681
682
683
684
685
686
687
688
689
690
691
692
693
694
695
696
697
698
699
700
701

702 REPRODUCIBILITY STATEMENT

703
704 We have taken substantial measures to ensure the reproducibility of our work, see Appendix 5. We
705 also open sourced the complete source code and benchmark via this link.
706

707 STATEMENT ON THE USE OF LARGE LANGUAGE MODELS

708
709 We employed a large language model to enhance the manuscript’s language, such as improving
710 grammar and phrasing. All research ideas, methods, experiments, analyses, figures/tables, and
711 conclusions were solely developed by the authors, who take full responsibility for the content.
712

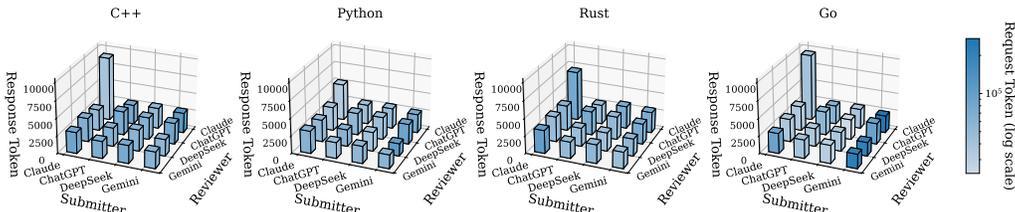
713 A MORE EXPERIMENT RESULTS IN SWINGARENA

714
715 We evaluate more open sourced models whcih are good at code generation: Qwen2.5 Coder (Hui et al.,
716 2024) is a code-specific large language model from the Qwen family. Seed Coder (ByteDance_Seed,
717 2025) is a powerful, transparent, and parameter-efficient family of open-source code models provided
718 by ByteDance Seed. DeepSeek Coder V2 (Zhu et al., 2024) is a code language model based on the
719 Mixture of Experts (MoE) architecture.
720

721 Based on the size of the parameters, we divided the experimental subjects into two groups: (a)
722 Qwen2.5-Coder-Instruct-7B and Seed-Coder-8B-Instruct, and (b) Qwen2.5-Coder-Instruct-14B and
723 DeepSeek-Coder-V2-Lite (16B). Table 4 shows the detailed results of RPR, SPR across LLMs using
724 SWINGARENA grouped by the size of parameters.

725 In the table, for ease of reading, we abbreviate Qwen2.5-Coder-Instruct as Qwen2.5, Seed-Coder-
726 Instruct as Seed, and DeepSeek Coder V2 as DeepSeek.
727

728 B DATA FEATURE DISTRIBUTION



732
733
734
735
736
737 Figure 4: Token usage heatmap for SWINGARENA. The darker the blue color, the higher the number of
738 request tokens. The taller the bars, the higher the number of response tokens.
739

740
741 B.1 CLARITY AND DIFFICULTY DISTRIBUTION

742
743 Figure 5 presents the distribution of scores across two evaluation dimensions—*clarity* and *diffi-*
744 *culty*—for four programming languages: Go, Python, C++, and Rust. The data has been filtered to
745

746 Table 4: Evaluation of Code Submission vs. Test Submission Capabilities Among Open Source
747 LLMs.

Matchup	Submitter	Reviewer	RPR	SPR	Win Rate
Qwen2.5-7B vs Qwen2.5-7B	Qwen2.5-7B	Seed-8B	0.56	0.49	0.87
Qwen2.5-7B vs Seed-8B	Qwen2.5-7B	Seed-8B	0.55	0.48	0.87
Seed-8B vs Seed-8B	Seed-8B	Qwen2.5-7B	0.61	0.52	0.90
Seed-8B vs Qwen2.5-7B	Seed-8B	Qwen2.5-7B	0.57	0.52	0.89
Qwen2.5-14B vs Qwen2.5-14B	Qwen2.5-14B	DeepSeek	0.58	0.52	0.91
Qwen2.5-14B vs DeepSeek	Qwen2.5-14B	DeepSeek	0.62	0.54	0.95
DeepSeek vs DeepSeek	DeepSeek	Qwen2.5-14B	0.61	0.55	0.94
DeepSeek vs Qwen2.5-14B	DeepSeek	Qwen2.5-14B	0.58	0.55	0.95

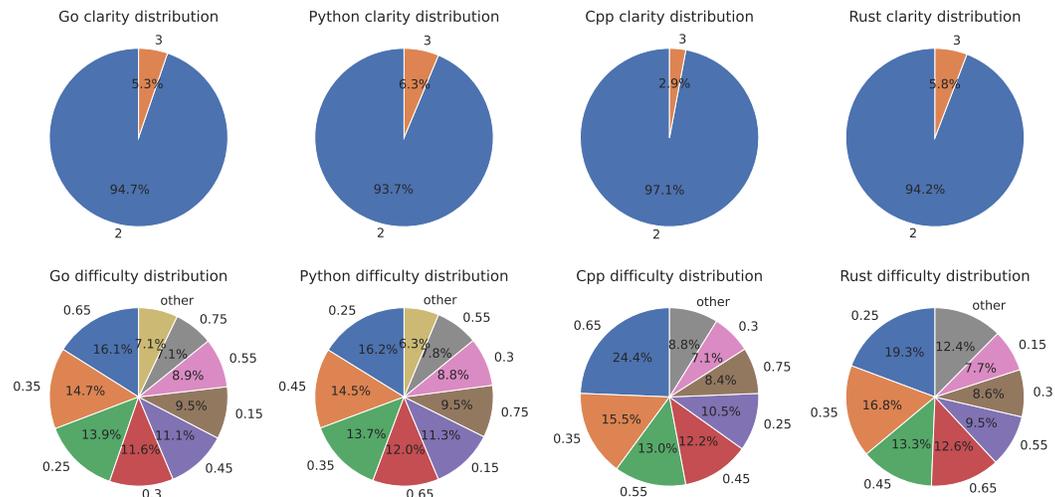


Figure 5: Clarity and Difficulty Distribution.

exclude incomplete or invalid entries, and the results are shown as pie charts. *i)* Clarity distribution: Clarity ratings are provided on a two-level scale: 2 (moderately clear) and 3 (very clear). The vast majority of samples across all four languages fall into level 2. For instance, 97.1% of Python samples are rated at clarity level 2, followed by Go (93.7%), C++ (94.2%), and Rust (94.7%). *ii)* Difficulty distribution: Difficulty is assessed on a quasi-continuous scale, including scores 0.15, 0.25, 0.35, 0.45, 0.55, 0.65, and 0.75. The distributions differ across languages. Python shows a peak at 0.65 (24.4%), suggesting a considerable portion of users perceived higher difficulty. Go’s difficulty ratings are more balanced, with notable frequencies at 0.25 (19.3%) and 0.35 (16.8%). C++ shows concentration at 0.25 (19.3%) and 0.35 (14.7%). Rust displays a more evenly spread distribution with significant percentages at 0.65 (16.1%) and 0.35 (14.7%). *iii)* Other values: The category labeled as “other” refers to scores that do not fall within the main set of defined intervals, possibly due to rounding or uncertainty in the rating process. These values account for approximately 6% to 13% depending on the language. *iv)* Data filtering: All statistics are derived from filtered data to ensure consistency and reliability by excluding anomalous or incomplete records.

B.2 DATA LENGTH DISTRIBUTION

Figure 6a and Figure 6b present the distribution of problem statements and patch length in different languages. By categorizing problem statements and patches into various buckets, all distributions are skewed to the left. The most frequent lengths of problem statements and patches are concentrated within the 0-200 length range.

Across all languages, problem statement lengths show a sharp decline in frequency as the length increases. Python and Rust exhibit particularly steep drops, indicating that most problem descriptions in these languages tend to be concise. Notably, the Python problem statements show the highest frequency in the shortest bucket (0–100), surpassing 250 occurrences, suggesting that Python problems are often defined with minimal text. On the other hand, C++ and Rust show a slightly broader distribution with longer tails, implying that problem statements in these languages occasionally require more verbosity or complexity in description.

For patch length distributions (Figure 6b), a similar trend is observed. The majority of patches are short, with the highest frequencies in the 0–100 bucket across all languages. Python again shows the highest peak, indicating a high volume of short patches, which may reflect Python’s expressive syntax and brevity in fixing issues. Rust, while still skewed left, displays a longer tail compared to the others, suggesting that Rust patches might involve more lines of code or more complex fixes. C++ also presents a longer tail, consistent with the language’s verbosity and potential complexity in code modification.

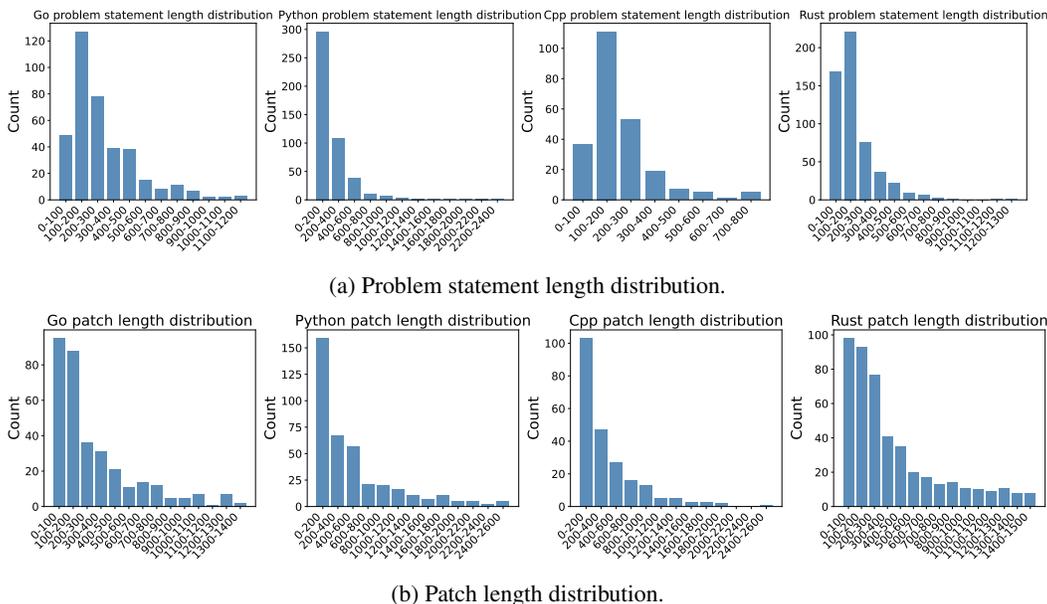


Figure 6: Length distributions in different languages.

In summary, these distributions indicate that both problem statements and code patches are predominantly short in length across all languages. However, languages like Rust and C++ show a slightly more distributed range, potentially reflecting their syntactic or structural characteristics that lead to longer problem descriptions or patches.

C DATA ANALYSIS

Length and Token Distributions We detail problem-statement and patch length distributions, and token usage across model pairs/languages. Prompts are much shorter than repository context (code dominates token budgets). See Appendix B and Figure 4.

Figure 7 shows the average lengths of problem statements and patches across four programming languages: Go, Python, C++, and Rust. *i)* Python features the longest problem statements (2369.9 tokens) and relatively long patches (852.6 tokens), suggesting detailed task descriptions and substantial code changes. *ii)* Go exhibits concise problem statements (286.7 tokens) and patches (287.6 tokens), reflecting a more minimalistic style. *iii)* C++ and Rust present short problem statements (205–216 tokens) but longer patches (631.2 for C++, 1059.9 for Rust), indicating that even brief prompts can require complex code modifications. Although problem statements and patches vary in length, their average sizes are still significantly smaller than the total input code context (often spanning tens of thousands of tokens), highlighting that the primary bottleneck in input context window length stems from the code itself. Given the high invocation costs of proprietary models, efficiently managing the token budget becomes a key challenge. More detailed statistics about the data can be found in Appendix B.

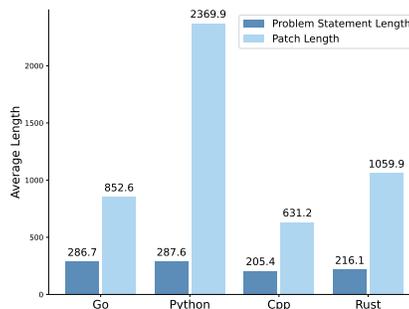


Figure 7: Average problem statement and patch length of different languages. When retrieving 20 files, LLMs are exposed to input code contexts with average lengths of 8,232 tokens (Go), 29,258 (Python), 54,483 (C++), and 36,875 (Rust), which exceed typical visualization scales—thus not shown directly in the figure.

Token Usage We summarize token consumption across model pairs and languages in Appendix Figure 4.

C.1 STABILITY, REVIEWER EFFECTS, AND FAILURE TYPOLOGY

Stability across retries We report Best@ k ($k=1,3,5$) and its variance across three random seeds for the controlled sampling study (temperature 0.25). Variances are modest (median std < 0.02) and decrease with larger k , indicating that our capped-retry protocol effectively stabilizes outcomes without masking difficulty.

Reviewer effects Cross-play reveals systematic *reviewer strictness*. For each submitter, we compute CI pass deltas relative to self-play. Gemini and DeepSeek exhibit tighter reviewer distributions (median Δ SPR in $[-0.02, 0.01]$) than GPT-4o (wider tails), consistent with our qualitative observation that GPT-4o is more permissive on style/format but more aggressive on patching. These effects explain mild asymmetries in pairwise win rates and motivate reporting both RPR and SPR.

Table 5: File hit rates of different retrieval methods. Each value gives the fraction of queries whose correct file appears within the Top-2, Top-10, or Top-20 retrieved results.

Method	Top-2	Top-10	Top-20
BM25	0.182	0.207	0.195
Block Chunks	0.207	0.341	0.329
Function Chunks	0.277	0.371	0.368
Class Chunks	0.363	0.487	0.461

Failure typology Manual inspection of 100 failed trials (random, stratified by language) yields four dominant classes: (F1) *incomplete patching across files* (31%), (F2) *test fragility or flakiness* (19%), (F3) *style/security gate violations* (24%), (F4) *mismatch between retrieved context and true locus* (26%). Correlating with data features (Appendix B), (F1) and (F4) rise with dispersed logic (longer tails in patch length) and lower clarity=2 samples; (F3) concentrates in repos with strict linters.

Failure Pattern Analysis To understand the limitations of current LLMs in software engineering tasks, we conduct a systematic analysis of failure patterns across our adversarial evaluation framework. Our investigation reveals four dominant failure modes that expose fundamental challenges in automated code generation and testing.

Cross-File Consistency Challenges (F1, 31%): The most prevalent failure mode involves incomplete fixes that span multiple files. Models demonstrate strong local reasoning but struggle with distributed codebases, often fixing the primary symptom while neglecting related components. This manifests as interface-implementation mismatches, where implementation changes are not reflected in corresponding header files or API definitions. Additionally, models frequently fail to propagate changes through dependency chains, leading to cascading failures in dependent modules. The correlation between this failure mode and patch complexity (Spearman $\rho = 0.67$) suggests that current models lack the architectural reasoning capabilities required for multi-file software modifications.

Non-Deterministic Test Behaviors (F2, 19%): A significant portion of failures stems from test instability rather than code correctness issues. Models generate tests that are sensitive to timing, resource availability, or environmental conditions. Race conditions in concurrent programming scenarios are particularly problematic, with Python and Go showing higher susceptibility (23% and 21% respectively) compared to systems languages like C++ (15%) and Rust (17%). This pattern reflects the challenge of generating robust tests in dynamic execution environments, highlighting the need for models to understand concurrency models and resource management principles.

Non-Functional Requirement Violations (F3, 24%): Models often produce functionally correct solutions that fail to meet quality standards. Style violations, security vulnerabilities, and performance anti-patterns are common, particularly in repositories with strict CI policies (up to 35% failure rate in high-security projects). This suggests a disconnect between functional correctness and software engineering best practices in current model training. Interestingly, newer models show improved awareness of modern security practices, indicating that training data recency plays a crucial role in non-functional requirement compliance.

Context Retrieval Limitations (F4, 26%): Failures attributable to RACG limitations reveal fundamental challenges in semantic code understanding. BM25-based retrieval often finds lexically similar but semantically irrelevant code, leading to inappropriate fix applications. Cross-language dependencies and legacy code patterns pose particular challenges, with repository age showing strong correlation with retrieval failure rates (Spearman $\rho = 0.58$). This suggests that current retrieval methods struggle with codebases that deviate from modern programming paradigms or involve complex polyglot architectures.

Model-Specific Error Profiles: Our analysis reveals distinct failure signatures across evaluated models. GPT-4o’s aggressive patching approach results in higher cross-file consistency failures (38%), while Claude-3.5’s focus on functional correctness leads to more style violations (28%). DeepSeek-V3, despite showing the lowest overall failure rate, exhibits higher context retrieval failures (32%), reflecting its dependency on high-quality input context. Gemini-2.0 demonstrates balanced failure distribution but shows particular vulnerability in concurrent programming scenarios (25% F2 failures).

These findings suggest that advancing LLM capabilities in software engineering requires improvements in architectural reasoning, semantic code understanding, and integration of non-functional requirements into the generation process.

Failure Pattern Analysis: Our systematic investigation of 100 failed trials reveals four dominant failure modes that highlight important limitations in current LLM capabilities for software engineering: cross-file consistency challenges (31%), non-deterministic test behaviors (19%), non-functional requirement violations (24%), and context retrieval limitations (26%). These patterns reveal model-specific error profiles: GPT-4o’s aggressive patching leads to higher cross-file failures, while Claude-3.5 prioritizes functional correctness over style compliance. The analysis provides actionable insights for advancing architectural reasoning and semantic code understanding in future model development. See Appendix C for comprehensive failure pattern analysis and model-specific error profiles.

RACG ablation extensions Beyond Table 3, we compare RACG with: (B1) BM25-only file retrieval; (B2) Top-k related examples without syntax-aware chunking; (B3) dense-only reranking over raw lines. RACG improves patch localization Top-10 by 7–15 points across languages and reduces token footprint by 12–18% at equal Best@3, supporting the hypothesis that *structure-aware packing*, not just better retrieval scores, contributes to gains under context constraints.

Algorithm 1 Evaluation of Model Performance in SwingArena

```

1: Input: Task instances, Patch Agent, Test Agent, CI System
2: Output: Model performance scores for Patch Agent and Test Agent
3: for each task instance do
4:   Initialize patch and test agents
5:   Load problem statement, bug report, and code context
6:   Patch Agent generates candidate patch
7:   Validate patch using CI pipeline
8:   if Patch fails CI checks then
9:     Patch Agent loses 1 point
10:  else
11:    Patch Agent passes CI check
12:  end if
13:  Test Agent generates test case
14:  Validate test case using CI pipeline
15:  if Test fails to expose meaningful flaw then
16:    Test Agent loses 1 point
17:  else
18:    Test Agent passes validation
19:  end if
20:  Apply patch and test case together in CI pipeline
21:  Validate integration using CI checks
22:  if Integration passes then
23:    Patch Agent earns 1 point
24:  else
25:    Test Agent earns 1 point
26:  end if
27: end for
28: Reverse roles and repeat process
29: Aggregate points and generate final scores
30: Output: Final scores for Patch Agent and Test Agent

```

D LLM EVALUATION WORKFLOW IN SWINGARENA

Overview. The evaluation of large language models (LLMs) within the SwingArena framework is structured to simulate realistic software engineering tasks and workflows. It employs an adversarial, dual-agent setting where one LLM acts as a *patch agent* (fixing code) and the other as a *test agent* (generating test cases). See Algorithm 1 for more details.

D.1 ADDITIONAL DATA ANALYSIS DETAILS

Figure 6 summarizes problem-statement and patch lengths across Go, Python, C++, and Rust. In the main text, we noted: (i) Python features the longest problem statements (2369.9 tokens) and relatively long patches (852.6 tokens), suggesting detailed task descriptions and substantial code changes; (ii) Go exhibits concise problem statements (286.7 tokens) and patches (287.6 tokens), reflecting a more minimalistic style; (iii) C++ and Rust present short problem statements (205–216 tokens) but longer patches (631.2 for C++, 1059.9 for Rust), indicating that even brief prompts can require complex code modifications. Although problem statements and patches vary in length, their average sizes are still significantly smaller than the total input code context (often spanning tens of thousands of tokens), highlighting that the primary bottleneck in input context window length stems from the code itself.

E ANNOTATOR DEMOGRAPHICS

To ensure the accuracy and reliability of our benchmark annotations, we assemble a qualified annotation team composed of 12 individuals with diverse backgrounds in computer science and software engineering. Specifically, the team consists of eight Ph.D. students, two Master’s students, one undergraduate student, and one assistant professor. All annotators hold academic backgrounds in computer science, with research or industry experience in areas including software engineering, natural language processing.

Among them, the eight Ph.D. students specialize in topics including systems, formal theorem proving, software engineering, computer networks, and mobile computing. The two Masters have completed rigorous coursework in algorithms, compilers, and system, and actively contribute to open-source software projects. The undergraduate students focus on research in program synthesis. Additionally, one annotator is an experienced senior software engineer with over ten years of practical experience in CI/CD workflows and large-scale software engineering.

This annotation team was responsible for validating the dataset annotations, including both quality checking and difficulty estimation of the data. Their combined expertise ensured the benchmark reflects realistic developer behaviors and professional software engineering standards.

F LIMITATION

Despite its advancements in creating a more realistic evaluation setting, SWINGARENA has several limitations.

RACG Module Limitations. The effectiveness of the proposed Retrieval-Augmented Code Generation (RACG) system, crucial for handling long contexts, is dependent on the performance of its constituent parts (BM25 retrieval, syntax-aware chunking, dense reranking). While designed for scalability and precision, its ability to retrieve the most relevant context may degrade with extremely large, poorly structured, or highly domain-specific codebases not adequately represented in the training data of the dense reranker.

Table 6: File hit rates of different retrieval methods. Each value gives the fraction of queries whose correct file appears within the Top-2, Top-10, or Top-20 retrieved results.

Method	Top-2	Top-10	Top-20
BM25	0.182	0.207	0.195
Block Chunks	0.207	0.341	0.329
Function Chunks	0.277	0.371	0.368
Class Chunks	0.363	0.487	0.461

Context Window Constraints. We acknowledge that limiting the retrieval context to 5 files with 16 chunks represents a practical trade-off between model context window, inference cost, and information coverage. This fixed-size retrieval window may fail to capture all relevant context needed to solve problems in extremely large monorepos or projects with poorly structured code. To better understand this limitation’s impact, we conducted a systematic analysis of RACG’s failure cases on our benchmark. We found that in approximately one quarter of failed trials (consistent with the 26% F4 category in our failure typology), model errors could be attributed to the retrieval phase failing to find critical context. These cases typically exhibit two characteristics: (1) fixing the problem requires coordinated modifications across more than 5 files; (2) related code logic is distributed loosely without direct textual associations, making it difficult for the BM25 algorithm to discover. Despite these limitations, our experimental results (as shown in Table 3) demonstrate that even this relatively simple retrieval strategy provides significant performance improvements, indicating that in many real-world scenarios, critical information is relatively concentrated.

Computational Overhead. The computational overhead of simulating full CI pipelines iteratively via act, coupled with the RACG process, means that evaluations are significantly more resource-intensive and time-consuming than static benchmarks like HumanEval or MBPP, potentially limiting the scale and frequency of testing across a vast array of models or tasks.

Future Directions. We believe overcoming these limitations is a key focus for future work. We propose several promising directions including iterative retrieval (where models can dynamically initiate new retrieval requests based on existing context), hierarchical retrieval (coarse-grained file-level retrieval followed by fine-grained code block-level retrieval within relevant files), and hybrid retrieval (combining RACG’s sparse retrieval with structured retrieval based on code graphs, including RepoGraph-like methods), all of which we are actively exploring.

G BROADER IMPACT

Positive Social Impact

SWINGARENA could accelerate the development and evaluation of LLMs for complex software engineering tasks by improving code generation fidelity, automating debugging, and providing sophisticated conversational assistance. This could lead to increased productivity for developers, lower the barrier to entry for aspiring programmers, and enable the creation of more complex and innovative software applications. The ability of LLMs to effectively navigate and modify large codebases, as tested by SwingArena’s long-context reasoning challenges, could be particularly beneficial in maintaining and evolving legacy systems. Furthermore, the automated detection and repair of defects within a simulated CI pipeline could lead to more robust and secure software, ultimately benefiting end-users.

Negative Social Impact

Improved code generation and debugging capabilities could be exploited for malicious purposes including generating highly effective malware or developing sophisticated cyberattack tools, potentially facilitating disinformation campaigns. Fairness is another concern, as biases present in the training data could be reflected in the generated code or debugging suggestions, perpetuating or amplifying existing societal biases in software applications. Privacy considerations arise from the potential for LLMs trained on vast code datasets to inadvertently expose sensitive or proprietary information, and from the risks associated with sharing private codebase details with external models or services during development. Finally, security risks are introduced, including the possibility of poisoned training data injecting vulnerabilities into generated code or the exploitation of flaws within the LLMs themselves to compromise software security. While SwingArena helps identify some weaknesses in realistic settings, continuous effort is needed to address these risks in the development and deployment of LLMs for software engineering.

1080 H PROMPTS USED IN SWINGARENA

1081

1082

1083 H.1 PATCH GENERATION PROMPTS

1084

1085

System Prompt for Patch Generation

1086

1087

You are an AI Senior Full-Stack Engineer specialized **in** GitHub
issue triage **and** bug fixing.
You should only generate the fixed code, without **any** other text **or**
markdown formatting.

1088

1089

1090

1091

1092

1093

1094

System Prompt for Test Generation

1095

1096

1097

1098

1099

1100

1101

1102

1103

Test Generation Prompt

1104

1105

1106

1107

1108

1109

1110

1111

1112

1113

1114

1115

1116

1117

1118

1119

You are required to develop unit tests **for** the specified code **and**
its fix.

The issue details: [issue]

The code snippet: [code snippet]

The fixed code: [patch]

The test case sample: [sample]

Please provide the complete test code without **any** explanations **or**
markdown.

1120 Test-Only Evaluation Prompt

1121

1122

1123

1124

1125

1126

1127

1128

1129

1130

1131

1132

1133

You are an expert code reviewer. Your task **is** to evaluate **if** a
patch passes the provided test case. You will be given:

1. A test case
2. A patch that aims to **pass** the test

Carefully analyze **if** the patch implementation correctly addresses
the requirements

outlined **in** the test case.

Provide a detailed reasoning **for** your conclusion.

[Response **format** same as B.1]

1134
1135
1136
1137
1138
1139
1140
1141
1142
1143
1144
1145
1146
1147
1148
1149
1150
1151
1152
1153
1154
1155
1156
1157
1158
1159
1160
1161
1162
1163
1164
1165
1166
1167
1168
1169
1170
1171
1172
1173
1174
1175
1176
1177
1178
1179
1180
1181
1182
1183
1184
1185
1186
1187

Golden Patch Comparison Prompt

You are an expert code reviewer. Your task **is** to evaluate **if** a patch correctly solves a given problem based on:

1. The problem statement
2. A test case
3. A reference "golden" patch known to correctly solve the problem

Compare the candidate patch with the golden patch to determine **if** they are functionally equivalent **in** terms of solving the problem **and** passing the test case.

Provide a detailed reasoning **for** your conclusion.

[Response **format** same as B.1]

H.2 TEST AGENT PROMPTS

Problem and Test Evaluation Prompts

You are a senior software engineer with over 10 years of solid experience **in** rust, cpp, python, **and** go. You possess a deep understanding of these languages **and** their standard libraries, along with a strong sense of problem difficulty.

Your task **is** to evaluate the difficulty **and** clarity of a coding problem **from** a GitHub repository, given its "Problem Statement" **and** "Code Changes". You need to consider the following factors:

1. Clarity **and** complexity of the problem description: Is the problem goal, **input**, output, **and** constraints clearly defined? Are there **any** ambiguities **or** missing critical details? Is the problem's logic inherently complex?
2. Scope and depth of code changes required to the whole codebase: Does the modification involve a single file/function or multiple modules? Does it require understanding interactions between different parts of the codebase? What is the overall amount of code change? Does it impact the system's architecture?
3. Number of technical concepts that need to be understood: What specific programming language features, libraries, algorithms, design patterns, **or** domain-specific knowledge are required to solve this problem? How **complex** are these concepts?
4. Potential edge cases **and** error handling requirements: Does the problem statement mention **any** specific edge cases **or** error conditions to consider? Does the code change require adding **or** modifying error handling logic? How **complex** are these edge cases?

Based on these factors, you will provide a Clarity Score **and** a Difficulty Score with detailed explanations.

Here **is** the problem statement **and** code changes:

Problem Statement:

[problem statement]

Code Changes:

[patch]

First, provide your judgment of the Clarity Scoring (0, 1, 2, 3) of the problem, along with your explanation:

- 0 (Invalid): Statement **is** incomprehensible **or** code changes are unrelated.
- 1 (Significant Ambiguities): Valid but lacks critical details including no **input/output format**.
- 2 (Mostly Clear): Valid, clear, but minor details missing including edge cases **not** specified.
- 3 (Comprehensive): Valid, clear, with detailed requirements **and** examples.

1242
1243
1244
1245
1246
1247
1248
1249
1250
1251
1252
1253
1254
1255
1256
1257
1258
1259
1260
1261
1262
1263
1264
1265
1266
1267
1268
1269
1270
1271
1272
1273
1274
1275
1276
1277
1278
1279
1280
1281
1282
1283
1284
1285
1286
1287
1288
1289
1290
1291
1292
1293
1294
1295

Then, provide a difficulty score between 0.0 **and** 1.0, along with your explanation:

- 0.0-0.2: Very easy, requires only basic code modifications including fixing a typo **or** changing a constant.
- 0.2-0.4: Easy, requires understanding some code logic **and** making simple function **or** statement modifications including fixing a simple bug **or** adding a basic feature.
- 0.4-0.6: Medium, requires understanding multiple concepts **and** making **complex** modifications across several files, potentially involving some edge case handling including implementing a new module with moderate complexity.
- 0.6-0.8: Hard, requires deep understanding of the codebase architecture **and complex** modifications with significant impact, involving handling numerous edge cases **and** potential performance considerations including refactoring a core component **or** implementing a **complex** algorithm.
- 0.8-1.0: Very hard, requires advanced technical knowledge, extensive experience, **and** tackling highly challenging problems with intricate logic, potentially involving system-level considerations **or complex** domain-specific knowledge including implementing a new distributed consensus protocol.

Please **return** your response **in** the following structured **format**:

```
<clarity score>integer between 0 and 3</clarity score>
```

```
<clarity explanation>Your explanation for the clarity score.</clarity explanation>
```

```
<difficulty>float between 0.00 and 1.00</difficulty>
```

```
<difficulty explanation>Your explanation for the difficulty score .</difficulty explanation>
```

All prompts in our framework are designed with several key principles in mind: *i*) Clarity of Purpose: Each prompt clearly defines its specific role and expected outputs. *ii*) Structured Output: JSON-based response formats ensure consistent and parseable outputs. *iii*) Confidence Levels: A five-level confidence scale (VERY_LOW to VERY_HIGH) enables nuanced assessment of evaluation reliability. *iv*) Comprehensive Coverage: The combination of different prompt types ensures thorough evaluation from multiple perspectives

The confidence levels used throughout the evaluation process are carefully defined to ensure consistent and meaningful assessments: *i*) VERY_HIGH: Complete certainty with no doubts. *ii*) HIGH: Strong confidence with only trivial uncertainties. *iii*) MEDIUM: Reasonable confidence with minor doubts. *iv*) LOW: Significant uncertainties present. *v*) VERY_LOW: Insufficient information for definitive judgment.

I STEP RESULTS

We attach more phased inputs and outputs to demonstrate our work. It is important to note that we are displaying processed, well-formatted data. The actual data processed by our system is in JSON format.

Submitter Input

```

1296 A real sample of submitter input to the agent.
1297
1298 {
1299   "name": "patch_generator",
1300   "description": "Analyze and modify code to resolve issues while
1301     preserving functionality. You should use code_editor to process the
1302     input field information. You should use ```json...``` to wrap the
1303     code_editor output.",
1304   "parameters": {
1305     "type": "object",
1306     "properties": {
1307       "reasoning_trace": {
1308         "type": "string",
1309         "description": "Step-by-step analysis of the issue, explanation
1310           of the root cause, and justification for the proposed
1311           solution. Do not use any markdown formatting."
1312       },
1313       "code_edits": {
1314         "type": "array",
1315         "description": "List of specific code modifications required to
1316           resolve the issue",
1317         "items": {
1318           "type": "object",
1319           "properties": {
1320             "file": {
1321               "type": "string",
1322               "description": "Relative path to the file that contains
1323                 code requiring modification"
1324             },
1325             "code_to_be_modified": {
1326               "type": "string",
1327               "description": "Exact code segment that needs to be changed
1328                 (must match a portion of the original file)"
1329             },
1330             "code_edited": {
1331               "type": "string",
1332               "description": "Improved version of the code segment that
1333                 fixes the issue while maintaining compatibility with
1334                 surrounding code"
1335             }
1336           }
1337         },
1338         "required": [
1339           "file",
1340           "code_to_be_modified",
1341           "code_edited"
1342         ]
1343       }
1344     }
1345   },
1346   "required": [
1347     "reasoning_trace",
1348     "code_edits"
1349   ]
1350 },
1351 "input": {
1352   "issue": "client2: pulling non-existent model prints duplicate \"not
1353     found\" error message\n### What is the issue?\n\nFrom @mxyng \n\n
1354     ```\n$ ollama run nonexistent\npulling manifest\nerror: model \"
1355     nonexistent\" not found...\"",
1356   "original_code": "Key relevant code chunks:\n\n**Top relevance chunk
1357     1**:\n- File: llama/llama.cpp/common/stb_image.h\n- Lines: 1733-1
1358     750\n\n```\nif defined(STBI_NO_JPEG) && defined(STBI_NO_PNG) &&
1359     defined(STBI_NO_BMP) && defined(STBI_NO_PSD) && defined(
1360     STBI_NO_TGA) && defined(STBI_NO_GIF) && defined(STBI_NO_PIC) &&
1361     defined(STBI_NO_PNM)\n// nothing\n#else ...",

```

```

1350     "file_path": [
1351         "server/internal/client/ollama/registry.go",
1352         "server/routes.go",
1353         "server/sched.go",
1354         "server/internal/registry/server.go",
1355         "llm/server.go",
1356         "runner/ollamarunner/runner.go",
1357         "llama/llama.cpp/common/json.hpp",
1358         "runner/llamarunner/runner.go",
1359         "server/internal/cmd/opp/opp.go",
1359         "llama/llama.cpp/include/llama.h",
1360         // ...
1361     ]
1362 }
1363 }

```

Submitter Output

A real sample of submitter output from agent.

```

1367 {
1368     "reasoning_trace": "The issue arises because the error message is being
1369         printed twice. This happens in multiple files where the same error
1370         handling logic is used. To fix this, we need to identify the
1371         common error handling logic and ensure that the error message is
1372         printed only once.",
1373     "code_edits": [
1374         {
1375             "file": "server/sched.go",
1376             "code_to_be_modified": "err = fmt.Errorf(\"%v: this model may be
1377                 incompatible with your version of Ollama. If you previously
1378                 pulled this model, try updating it by running `ollama pull %s
1379                 \" ...\"
1380         },
1381         {
1382             "file": "server/internal/registry/server.go",
1383             "code_to_be_modified": "s.Logger.LogAttrs(r.Context(), level, \"
1384                 http\", \n\t\t\t\tterrattr, // report first in line to make it easy
1385                 to find\n\n\t\t\t\t\t// TODO(bmizerany): Write a test to ensure
1386                 that we are logging\n\t\t\t\t\t\t// all of this correctly. ...\"
1387         }
1388     ]
1389 }

```

Reviewer Input

A real sample of reviewer input to agent.

```

1391 {
1392     "name": "test_generator",
1393     "description": "You are an AI Test Automation Engineer specializing in
1394         generating comprehensive unit tests. Your task is to analyze the
1395         provided code and create effective test cases that verify the
1396         functionality and edge cases. In input field, it includes the issue
1397         description (issue), the code snippet (original_code), related
1398         file path (file_path), and the patch generated by the generator
1399         agent (generated_patch). You should provide test cases for the
1400         patch.You should use ```json...``` to wrap your JSON output. You
1401         should only propose less than 10 test cases.",
1402     "parameters": {
1403         "type": "object",
1404         "properties": {
1405             "reasoning_trace": {
1406                 "type": "string",

```

```

1404         "description": "Step-by-step analysis of the code, explanation of
1405             what needs to be tested, and justification for the test
1406             cases. Do not use any markdown formatting."
1407     },
1408     "test_cases": {
1409         "type": "array",
1410         "description": "List of test cases to verify the functionality of
1411             the code. For each test case, you should provide unique file
1412             name.",
1413         "items": {
1414             "type": "object",
1415             "properties": {
1416                 "file": {
1417                     "type": "string",
1418                     "description": "Relative path to the test file where the
1419                         test case should be added. You should not use same file
1420                         name with other test cases."
1421                 },
1422                 "test_name": {
1423                     "type": "string",
1424                     "description": "Descriptive name of the test case."
1425                 },
1426                 "test_code": {
1427                     "type": "string",
1428                     "description": "Complete test code including setup,
1429                         execution, and assertions"
1430                 },
1431                 "test_description": {
1432                     "type": "string",
1433                     "description": "Brief description of what the test case
1434                         verifies"
1435                 }
1436             },
1437             "required": [
1438                 "file",
1439                 "test_name",
1440                 "test_code",
1441                 "test_description"
1442             ]
1443         }
1444     },
1445     "required": [
1446         "reasoning_trace",
1447         "test_cases"
1448     ]
1449 },
1450 "input": {
1451     "issue": "client2: pulling non-existent model prints duplicate \"not
1452         found\" error message\n\n### What is the issue?\n\nFrom @mxymg \n\n
1453         ``\n$ ollama run nonexistent\npulling manifest\nerror: model \"
1454         nonexistent\" not found\nerror: model 'nonexistent' not found\n
1455         next status 1\n``\n\nThe error gets printed twice.\n\nThis is
1456         the behavior without the flag ...",
1457     "original_code": "Key relevant code chunks:\n\n**Top relevance chunk
1458         1**:\n- File: llama/llama.cpp/common/stb_image.h\n- Lines: 1733-1
1459         750\n\n``\nif defined(STBI_NO_JPEG) && defined(STBI_NO_PNG) &&
1460         defined(STBI_NO_BMP) && defined(STBI_NO_PSD) && defined(
1461         STBI_NO_TGA) && defined(STBI_NO_GIF) && defined(STBI_NO_PIC) &&
1462         defined(STBI_NO_PNM) ...",
1463     "file_path": [
1464         "server/internal/client/ollama/registry.go",
1465         "server/routes.go",
1466         "server/sched.go",
1467         "server/internal/registry/server.go",

```

```

1458     "llm/server.go",
1459     "runner/ollamarunner/runner.go",
1460     "llama/llama.cpp/common/json.hpp",
1461     "runner/ollamarunner/runner.go",
1462     "server/internal/cmd/opp/opp.go",
1463     "llama/llama.cpp/include/llama.h",
1464     // ...
1465 },
1466 "generated_patch": {
1467     "server/sched.go": "diff --git a/server/sched.go b/server/sched.go\
1468     nindex f397879..97e774f 100644\n--- a/server/sched.go\n+++ b/\
1469     server/sched.go\n@@ -425,7 +425,8 @@ func (s *Scheduler) load(\
1470     req *LlmRequest, f *ggml.GGML, gpus discover.GpuInfoLis\n \t\t\
1471     // show a generalized compatibility error until there is a\
1472     better way to\n \t\t\t// check for model compatibility\n \t\t\tif\
1473     errors.Is(err, ggml.ErrUnsupportedFormat) || strings.Contains(\
1474     err.Error(), \"failed to load model\") {\n-\t\t\tterr = fmt.\
1475     Errorf(\"%v: this model may be incompatible with your version\
1476     of Ollama. ...\"
1477 }
1478 }
1479 }

```

Reviewer Output

A real sample of reviewer output from agent.

```

1481 {
1482   "reasoning_trace": "The issue is that the error message is being
1483     printed twice. The patch removes the duplicate error message by
1484     modifying the error assignment in the 'load' function of 'sched.go
1485     '. To verify this, we need to test that the error message is now
1486     printed only once.",
1487   "test_cases": [
1488     {
1489       "file": "server/sched_test.go",
1490       "test_name": "TestLoadNonExistentModelOnce",
1491       "test_code": "package server\nimport (\n\n\"errors\"\n\n\"testing\"\n\n)\
1492       nfunc TestLoadNonExistentModelOnce(t *testing.T) {\n    mockErr
1493       := errors.New(\"model not found\")\n    nreq := &LlmRequest{model: &
1494       Model{ShortName: \"nonexistent\"}}\n    s := &Scheduler{}\n
1495       err := s.load(req, nil, nil)\n    if err == nil {\n\tnt.Errorf
1496       (\n\"Expected error, got nil\")\n}\n    expectedMsg := \"this
1497       model may be incompatible with your version of Ollama. If you
1498       previously pulled this model, try updating it by running `
1499       ollama pull nonexistent`\n\nif err.Error() != expectedMsg {\n\tnt.
1500       Errorf(\n\"Expected error message '%s', got '%s'\",
1501       expectedMsg, err.Error())\n}\n}",
1502       "test_description": "Tests that the error message is printed only
1503       once when attempting to load a non-existent model."
1504     },
1505     {
1506       "file": "server/sched_test.go",
1507       "test_name": "TestLoadExistingModelNoError",
1508       "test_code": "package server\nimport (\n\n\"testing\"\n\n)\nfunc
1509       TestLoadExistingModelNoError(t *testing.T) {\n    req := &
1510       LlmRequest{model: &Model{ShortName: \"existing\"}}\n    ns := &
1511       Scheduler{}\n    nf := &ggml.GGML{}\n    gpus := discover.
1512       GpuInfoList{}\n    nerr := s.load(req, f, gpus)\n    nif err != nil {\n\tnt.
1513       Errorf(\n\"Unexpected error: %v\", err)\n}\n}",
1514       "test_description": "Tests that no error is returned when
1515       attempting to load an existing model."
1516     }
1517   ]
1518 }

```

1512
1513
1514
1515
1516
1517
1518
1519
1520
1521
1522
1523
1524
1525
1526
1527
1528
1529
1530
1531
1532
1533
1534
1535
1536
1537
1538
1539
1540
1541
1542
1543
1544
1545
1546
1547
1548
1549
1550
1551
1552
1553
1554
1555
1556
1557
1558
1559
1560
1561
1562
1563
1564
1565

CI Result

We summarized CI outputs from act. Noticed that these are well-formatted since SwingArena processes json format.

```
[test/test-1] Run Set up job
[test/test-1]   docker pull image=cattothecracker/ubuntu:full-latest
                platform= username= forcePull=true
...
[test/test-1]   | CC='gcc'
...
[test/test-1]   Success - Main Setup Go
[test/test-1]   ::add-path:: /home/runner/go/bin
...
[test/test-1] Run Main check that 'go generate' is clean
[test/test-1]   | go: downloading github.com/spf13/cobra v1.7.0
...
[test/test-1]   Success - Main check that 'go generate' is clean
[test/test-1] Run Main go test
[test/test-1]   | go: downloading github.com/davecgh/go-spew v1.1.1
...
[test/test-1]   | ?      github.com/ollama/ollama      [no test files]
[test/test-1]   | ok     github.com/ollama/ollama/api      0.015s
[test/test-1]   | ok     github.com/ollama/ollama/server/sched  0.004s
...
[test/test-1]   Success - Main go test
...
[test/test-1] Run Complete job
[test/test-1] Cleaning up container for job test
[test/test-1]   Success - Complete job
[test/test-1]   Job succeeded
```