DISTDEBUG-BENCH: Can LLM Agents Diagnose Bugs in Distributed Systems?

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Abstract

LLM agents show strong potential in software engineering tasks such as competitive programming and code synthesis. Existing benchmarks, however, largely focus on small-scale settings such as self-contained Python modules, leaving distributed systems, an essential class of production software, under-examined. We introduce DISTDEBUG-BENCH, a benchmark for evaluating agents' ability to diagnose complex-system failures at the code level. It curates 120 high-quality real-world bugs from 11 widely used distributed systems, spanning 5 programming languages and 10 failure patterns. Through an extensive evaluation of state-of-the-art LLMs and agents, we identify substantial gaps between current agents and real-world debugging tasks. A recurring gap is tunnel vision due to a lack of explicit reasoning structure and nondeterminism from inconsistent tool-use scope. The benchmark and evaluator are available at https://github.com/OrderLab/distdebug-bench, with agent traces and a leaderboard at https://distdebug.com.

1 Introduction

Large language models (LLMs) and agents have made rapid progress on software engineering tasks, including code synthesis, repository navigation, and issue resolution. Early code benchmarks measure LLM capabilities on short-program synthesis [3, 4]; programming-contest benchmarks evaluate more challenging algorithmic problem solving [12, 36]; RepoBench measures cross-file repository-context use [17]; later benchmarks such as SWE-bench and SWE-bench Verified evaluate whether an agent can resolve GitHub issues by producing a patch that passes a test suite [6, 14]. These benchmarks help shape the designs of both models and agent harnesses.

However, much existing benchmarking focuses on small-scale or locally scoped settings, such as standalone functions, self-contained modules, and library-style repositories. These settings are useful, but leave a critical real-world target under-examined: *debugging large-scale, production systems*. Existing evaluations also often emphasize patch generation, which can conflate diagnosis with repair synthesis. A patch may suppress a symptom without addressing the root cause; even when the final fix changes only a few lines, finding that cause may require understanding a much larger causal chain. In many production workflows, engineers must therefore diagnose a failure before writing a proper fix. This debugging workflow is crucial yet not directly measured by existing benchmarks.

Distributed systems debugging is a particularly important and challenging setting in this gap. Production distributed systems, including distributed storage, coordination, and stream processing, underpin modern computing services. Their failures are costly and hard to debug for several reasons. First, these systems span millions of lines of code across many subsystems and layered abstractions, creating

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a large search space in which the true root cause occupies only a tiny subset. Second, failures involve interactions across modules, network calls, and service tiers. An observed symptom often exposes only downstream effects while the originating fault is far away. Third, the available evidence is noisy and incomplete: issue reports, logs, stack traces, and artifacts provide only *partial observations*. Large-scale distributed systems also experience gray failures with ambiguous symptoms [11, 18]. Debugging therefore requires reasoning from high-level symptoms to low-level code under weak evidence. Developers need to generate and test competing hypotheses, reconcile evidence with code context, trace causal relationships across modules and historical design decisions, and revise assumptions as new evidence appears. Distributed-system debugging thus stresses capabilities central to LLM and agent design: *long-context retrieval*, *large-repository navigation*, *multi-hop causal reasoning*, *decision-making under noisy and incomplete evidence*, and *belief revision*.

To address this gap, we introduce DISTDEBUG-BENCH, a benchmark for evaluating LLM agents on *code-level diagnosis of real-world distributed-system failures*. In each benchmark instance, the agent is given an incident report, accompanying evidence (*e.g.*, stack trace), and the pre-fix repository snapshot, and must identify one or more source-code blocks that best explain the observed failure. Outputs are judged against human-annotated labels that capture the root-cause regions. The benchmark contains 120 curated incidents from 11 distributed systems and 5 languages, with 218 gold root-cause blocks. It captures the properties that make distributed-system debugging difficult: 45% require multiple blocks to express the root cause, 23% span multiple files, 69% of reports do not mention the gold method, and 63% contain no usable internal stack trace. The benchmark repository is available at `https://github.com/OrderLab/distdebug-bench`, with project documentation and updates at `https://distdebug.com`.

We evaluate state-of-the-art LLMs and agents on the benchmark. The results reveal a substantial gap between broad repository navigation and precise diagnosis. The strongest configuration achieves high file-level coverage but much lower block-level coverage and precision. Agents often find a relevant component but fail to pinpoint the causal source lines. Performance degrades most for opaque reports and cross-component bugs. Through trace inspection, we find a recurring failure mode: agents commit early to a plausible local explanation and rarely recover when the initial search scope misses the causal region. We call this behavior *tunnel vision* and analyze how it arises from evidence structure and tool-call trajectories. Non-determinism further compounds the problem: even a leading solution produces flaky diagnoses on 17% of incidents, rising to 26% on no-stack opaque reports.

Our contributions are:

- A benchmark task for distributed-system diagnosis in which agents localize the pre-fix code blocks that best explain an observed failure.
- A curated dataset of real-world failures in widely used distributed systems, with a label design process focused on reproducibility and non-trivial causal structure.
- An empirical evaluation of current LLM agents and harnesses showing gaps in precise causal localization, especially for weak-evidence and cross-component failures.
- A failure-mode analysis identifying key obstacles for reliable debugging.

2 Related Work

Code-generation and repository-context benchmarks. Early code benchmarks such as HumanEval and MBPP evaluate functional correctness for short programming tasks [3, 4]. LiveCodeBench and LiveCodeBench Pro extend this line with newer competitive-programming tasks and broader code-execution signals [12, 36]. Repository-context benchmarks such as RepoBench move beyond single-file synthesis by testing retrieval and completion across files [17]. These benchmarks measure code synthesis abilities but not failure diagnosis.

Bug, repair, and debugging datasets. Long-running software-engineering datasets such as Defects4J, ManyBugs, Bugs.jar, Bears, BugSwarm, and BugsInPy provide reproducible real bugs and failing and passing versions for testing, fault localization, and automated program repair [15, 16, 19, 26, 30, 31]. More recent LLM debugging benchmarks such as DebugBench directly test whether models can repair or reason about *injected* buggy snippets [29]. In these datasets, the primary oracle is typically a test suite, a fixed version, or a snippet-level repair target; diagnosis is not directly evaluated.

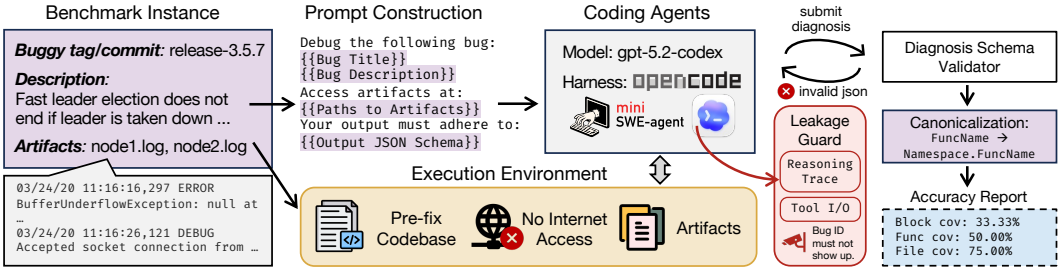


Figure 1: DISTDEBUG-BENCH task flow.

DISTDEBUG-BENCH explicitly measures root-cause localization. Agents must identify the precise causal block(s). It targets large-scale distributed systems. Its bugs are drawn from challenging, real-world failures. Its diagnosis tasks model the multi-source, noisy evidence of a real incident.

Issue-resolution agent benchmarks. SWE-bench changed the evaluation target from standalone synthesis to real GitHub issue resolution, and follow-on suites add human verification, visual software domains, live task collection, multilingual ecosystems, broader repository languages, and long-horizon enterprise-style tasks [6, 9, 14, 25, 33–35]. SWE-Gym further targets training environments and verifiers for software-engineering agents [23]. DISTDEBUG-BENCH shares the basic format (repository-level context and natural-language issue reports) but differs along several axes: (1) they score patches, while DISTDEBUG-BENCH scores root-cause localization directly; (2) SWE-bench-style instances are dominated by single-process projects with short causal chains; (3) they require a test suite for evaluation, a constraint that biases curation toward bugs whose fixes are exercised by existing tests. For many distributed systems failures, a regression test rarely exists at the time of diagnosis.

Operational benchmarks. Recent benchmarks evaluate LLM agents on IT automation and fault localization. AIOpsLab evaluates AIOps agents in microservices with fault injection, workload generation, and telemetry export [5]. ITBench evaluates AI agents across IT automation tasks [13]. SREGym evaluates SRE agents in a controlled platform [7]. Benchmarks such as OpenRCA [32], RCAEval [24], and PetShop [10] evaluate LLMs on fault localization: identifying the faulty services, machines, or resources from telemetry, metrics, traces, and injected performance anomalies. These benchmarks are valuable for AIOps [8] workflows. They typically operate over telemetry data and focus on coarse-grained localization. DISTDEBUG-BENCH addresses a complementary problem: *precise source-level debugging*. In incident response, engineers still need to perform the last-mile reasoning that connects symptoms to the root cause block(s) in code, which is often the most time-consuming step. DISTDEBUG-BENCH evaluates LLMs' abilities in this task. It preserves repository-level, source-code grounding while capturing the weak evidence, interactions, and ambiguity characteristic of distributed-system debugging.

3 Benchmark

DISTDEBUG-BENCH is a benchmark for *code-level diagnosis* of real-world distributed-system failures. DISTDEBUG-BENCH evaluates coding agents in the real-world systems maintenance workflow where developers have to start with a high-level symptom, explore possible root causes by forming and testing multiple causal hypotheses from partial evidence, and identify where in the code the root cause is encoded before any fix is applied. Each benchmark instance asks the agent to identify, from an incident report, a realistic evidence bundle, and the pre-fix snapshot, the code block or set of blocks that best explains the observed failure.

3.1 Task Workflow

Figure 1 shows the task flow. The agent-facing input consists of an incident report, issue-side artifacts such as logs or stack traces, the pre-fix repository snapshot, and a structured output schema. The fixing change and human root-cause labels are held out for annotation and scoring. The agent must return one or more repository-local source blocks that encode the pre-fix failure mechanism. The

evaluator validates the structured output, canonicalizes symbols, and scores whether the predicted blocks capture the minimal causal region(s) that explain the incident.

The benchmark release contains 120 incidents from 11 production-scale distributed systems across 5 languages. Each incident is selected to be diagnosable from issue-side context, stripped of answer-leaking discussion, and labeled with precise pre-fix root-cause blocks. Because DISTDEBUG-BENCH is built from public historical bugs, we also include leakage controls in the task workflow. The prepared workspaces anonymize directory names and remove commits after the task snapshot. The leakage guard in Figure 1 scans visible reasoning and tool inputs/outputs for the original issue identifiers withheld from the agent. Flagged attempts are discarded and rerun; Appendix A.6 describes the guard and follow-up audit process. For GPT-5.4, flagged attempts fell from 102/360 to 4/360 after future-commit removal. In closed-book probes on 66 reports without exact gold file/function names, revealing the issue ID gives no advantage: function coverage is 17.3% with the ID and 21.4% without it, versus 45.5% with repository access. Repository access substantially improves localization in these probes, although pretraining exposure cannot be ruled out (Appendix G.2).

3.2 Bug Selection

DISTDEBUG-BENCH is constructed from issue reports and fixing changes drawn from large-scale, production distributed systems. We select candidate bugs using three principles: the issue should describe a representative production failure, the available evidence should support diagnosis without revealing the answer, and the fixing change should support precise root-cause labeling.

Real production failures. We prioritize high-impact, user-facing bugs on stable releases. Eligible issues must be closed, have an associated fix, and describe a concrete failure rather than a feature request or internal cleanup.

Realistic and diagnosable evidence. We prefer reports that expose the kinds of signals engineers actually use in practice: stack traces, logs, reproduction steps, cross-linked prior issues, or explicit symptom descriptions. We also check that the issue is diagnosable from issue-side context alone. Later issue-thread comments that reveal the root cause or patch strategy are removed or ignored, since those no longer reflect realistic diagnosis.

Labelable root-cause fixes. We retain bugs whose fixes are local enough to support precise root-cause labeling. Large refactorings, test-only changes, and patches dominated by unrelated cleanup are excluded because they make the minimal causal span ambiguous. As a practical filter, we also exclude fixes with more than 100 changed source lines unless the causal span remains clearly separable from auxiliary edits.

3.3 Root-Cause Labeling

For each accepted issue, annotators derive the minimal pre-patch root-cause span from the fixing change and its surrounding context. Each label marks a buggy code block with a file path, function or method symbol, and a start/end line range in the pre-fix snapshot. Appendix A.1 shows the concrete label format.

In practice, annotators inspect the issue report, the fixing patch, and the affected code, and then mark only the code that embodied the bug before the fix. We exclude tests, helper refactors, defensive hardening, and incidental cleanup unless these are necessary parts of the minimal causal explanation, because such edits do not directly encode the original failure mechanism. Each issue is independently annotated by three annotators and then reconciled into a final gold label set; Appendix A.5 reports inter-annotator agreement.

In addition to root-cause labels, we record characterization metadata for each bug. These fields include textual code anchors, crash-to-fix distance, issue-side signals, and coarse root-cause type. They are not scoring targets or agent-facing hints, but they provide the foundation for the fine-grained breakdowns in Section 4.

Scaling collection. The released DISTDEBUG-BENCH labels are fully human-curated and human-reconciled; LLM outputs are not used in the gold annotation workflow. Because human review remains the main bottleneck for scaling, we also developed an LLM-assisted label extraction pipeline

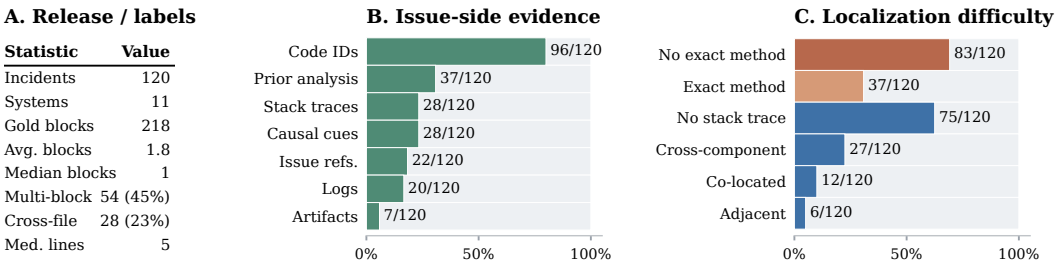


Figure 2: DISTDEBUG-BENCH workload characterization.

for future exploration. Appendix B describes it and reports a small-subset comparison between this pipeline, a non-LLM extraction pipeline, and the final human labels.

3.4 Benchmark Characteristics

Figure 2 summarizes the workload beyond headline dataset size. The characterization highlights three properties that make diagnosis difficult and later enable fine-grained evaluation breakdowns: the issue-side evidence is heterogeneous, the symptom is often far from the causal code, and many gold labels require non-local or multi-block explanations.

Issue-side evidence and anchors. DISTDEBUG-BENCH reports contain heterogeneous evidence rather than a uniform problem statement. Code identifiers appear in 96 / 120 reports, but direct debugging signals are much less common: 37 include prior analysis, 28 include stack traces, 20 include logs, and only 7 include attached artifacts. Even when a report contains concrete code names, it rarely names the final fix location: in 83 / 120 reports (69.2%), the exact gold method does not appear in the title or description.

Crash-to-fix distance. Stack traces are also an incomplete localization signal. In 75 / 120 incidents, the report does not contain a usable project-internal stack trace. Among the remaining incidents with an observable crash site, only 12 are co-located with the fix, while 27 require crossing component boundaries from the crash site to the gold block and 6 are adjacent within nearby package structure.

Ground-truth structure. The labels also show that diagnosis is not simply a single-location search problem. DISTDEBUG-BENCH contains 218 gold buggy blocks, and 45.0% of incidents require more than one block to express the root cause. Nearly a quarter span multiple files. At the same time, the median block is only five lines, so a useful agent must both follow non-local causal chains and report precise source spans. Appendix A gives the full artifact layout, raw instance examples, label schema, and classification rules used for the benchmark characterization. For a concrete illustration of how issue evidence can point to a plausible symptom-side investigation space while the actual root cause lies elsewhere, Appendix A.2 walks through ZooKeeper-2845.

4 Evaluation

We evaluate whether current software-engineering agents can identify source-level root causes for DISTDEBUG-BENCH incidents, and where their diagnosis quality breaks down. The central result is that contemporary agents often reach relevant code, but their diagnosis process breaks down when weak incident evidence requires sustained navigation and hypothesis revision.

4.1 Evaluation Setup

We run DISTDEBUG-BENCH along two axes: (1) the model axis and (2) the harness axis. We first conduct a thirteen-configuration sweep on Mini-SWE-Agent, including GPT-6 Astra, Claude Opus 5.5, GPT-5.5, GPT-5.4, GPT-5.3-Codex, GPT-5.2-Codex, Hunyuan3, o4-mini, DeepSeek V4 Flash, Kimi K2.6, Gemini 3.5 Flash, MiMo v2.5, and MiMo v2.5 Pro. We then run GPT-5.2-Codex and o4-mini on Codex CLI v0.128.0 and OpenCode v1.4.6 to provide harness comparison points. Mini-SWE-Agent is the common minimal harness used by existing coding benchmarks [27, 28]; Codex CLI is OpenAI's terminal coding agent [20]; and OpenCode is a highly starred open-source

terminal coding agent [1]. The model set mixes recent high-end systems, including GPT-6 Astra [22], Claude Opus 5.5 [2], and GPT-5.4 [21], with popular economical or coding-specialized options such as o4-mini, Kimi K2.6, DeepSeek V4 Flash, Gemini 3.5 Flash, and MiMo.

In addition to the 17 main configurations, we run exploratory experiments with DeepSeek V4 Pro, Qwen3.6 Max, and Qwen3-Coder-Next on non-uniform subsets of the benchmark. These runs are not included in the main 120-bug leaderboard, but Appendix F reports paired comparisons against Mini-SWE GPT-5.2-Codex on the exact subset completed by each model.

We set reasoning effort to medium when the model exposes that control. The MiMo endpoint did not expose a reasoning-effort parameter, so those runs use the provider default. Claude Opus 5.5 runs at its API default effort, which is medium, with a 16,000-token output limit and Anthropic prompt caching. For each bug/configuration pair, we average three attempts within the bug, then average equally across all 120 bugs (Appendix D.3). Appendix D.1 reports 95% bug-level bootstrap confidence intervals (CIs) for every configuration and paired comparisons between them.

Metrics. We compare generated diagnoses with labeled ground truth using coverage and precision at four granularities: file, function, line, and block. File and function metrics measure whether an agent reaches the right component. Line coverage is the stricter overlap metric, while block coverage is a coarser region-hit metric that asks whether each labeled causal block is reached. We use block coverage as the main localization metric because it is less sensitive to exact line-boundary choices and better captures whether an agent navigates to the causal region. We report line coverage as a stricter sensitivity metric (Appendix D). We also report *mention coverage*: whether the natural-language causal-chain explanation mentions gold functions even when the structured block prediction is wrong. To measure nondeterminism, we compute the within-bug standard deviation of block coverage across the recorded attempts and the number of *flaky* bugs whose block-coverage spread exceeds 0.2.

Metric validity. Block coverage measures recovery of the annotated causal code, not every plausible intervention or repair point. Most zero-coverage diagnoses reflect incorrect or incomplete causal reasoning: in a score-blinded audit of 20 zero-coverage GPT-5.4 diagnoses, 16 were incorrect or incomplete, and the other four reconstructed the failure mechanism but blamed a different intervention point. Benchmark-wide, most missed gold blocks are genuine misses: in a stratified random sample of 100 incompletely covered attempts from all 17 configurations, two independent AI reviewers, each given the historical fix, agree that 8.8% (95% CI 4.3–14.3%) of the missed blocks were correctly localized; counting them would raise block coverage by 5.0 points. Coverage also matters downstream. In a six-bug within-bug repair pilot, high-coverage diagnoses led the repair agent to edit the gold block in 50.0% of attempts, versus 11.1% without a diagnosis and 0% with low-coverage diagnoses (Appendix G.1).

4.2 Main Evaluation Results

Table 1 summarizes diagnosis performance across the main model and harness configurations. GPT-6 Astra has the highest aggregate block coverage in our complete sweep: with Mini-SWE, it reaches 83.9% file coverage, 53.9% line coverage, and 67.7% block coverage. Across configurations, however, file coverage remains substantially higher than line and block coverage, and higher-coverage settings often come with broader predictions: Mini-SWE GPT-5.5 has 34.7% block precision and names 3.6 blocks per diagnosis on average. These aggregate results point to a consistent theme: **Current SOTA agents can often reach relevant code, but their diagnosis process breaks down when weak incident evidence requires sustained navigation and hypothesis revision.** The category breakdown below makes this gap visible: the weakest cases are not only those without starting evidence, but also those where the report provides an initial symptom location without naming the causal code.

Finding 1. *Agents struggle when the report does not point to the exact code locations.* Performance drops sharply when the bug report does not mention the ground-truth function names or files, suggesting that the report's initial content heavily shapes the final diagnosis and that agents have limited ability to revise early hypotheses.

Appendix E gives a concrete trace example for this failure mode.

Table 1: Main evaluation results across the 120 common DISTDEBUG-BENCH incidents. The shaded column is the primary localization metric, with 95% bug-bootstrap CIs in brackets; bold values are column bests. Table 9 gives CIs for the other metrics.

Configuration		Localization coverage				Explanation		Instability	Effort		
Model	Harness	File	Func.	Line	Block	Block prec.	Mention	Flaky bugs	Blocks/diag.	Calls/run	Tokens/run
Mini-SWE-Agent model sweep											
GPT-6 Astra	Mini-SWE-Agent	83.9	58.4	**53.9**	**67.7** [60.4, 74.9]	42.7	**82.8**	**15 (12%)**	3.3	**9.0**	**132K**
Opus 5.5	Mini-SWE-Agent	82.0	**60.4**	53.5	66.4 [59.2, 73.6]	45.5	80.6	17 (14%)	2.8	9.2	193K
GPT-5.5	Mini-SWE-Agent	**84.0**	55.5	52.7	65.1 [57.8, 72.3]	34.7	74.7	20 (17%)	3.6	21.9	468K
GPT-5.4	Mini-SWE-Agent	79.8	53.1	50.4	63.1 [56.0, 70.2]	39.5	69.0	21 (18%)	2.9	25.0	690K
5.3-Codex	Mini-SWE-Agent	79.1	53.0	44.5	59.7 [52.2, 67.2]	40.0	68.9	22 (18%)	2.6	22.7	416K
MiMo v2.5 Pro	Mini-SWE-Agent	78.9	60.1	43.4	58.5 [50.8, 66.1]	38.1	72.6	22 (18%)	2.9	55.5	2643K
Kimi K2.6	Mini-SWE-Agent	77.5	51.2	44.8	58.3 [50.9, 65.6]	45.9	70.6	32 (27%)	2.4	66.1	2796K
MiMo v2.5	Mini-SWE-Agent	77.8	54.2	43.9	58.0 [50.5, 65.4]	36.5	69.5	26 (22%)	3.1	49.7	2332K
DS V4 Flash	Mini-SWE-Agent	76.0	49.4	45.9	56.4 [49.0, 63.7]	32.7	67.0	32 (27%)	3.2	61.6	2291K
5.2-Codex	Mini-SWE-Agent	75.9	48.8	45.9	56.4 [48.5, 64.2]	43.1	63.6	18 (15%)	2.3	32.1	666K
HY3	Mini-SWE-Agent	72.5	49.1	39.7	51.6 [44.1, 59.1]	38.7	64.5	35 (29%)	2.7	32.5	768K
Gemini 3.5	Mini-SWE-Agent	65.9	43.5	37.3	47.5 [39.7, 55.2]	**51.1**	58.5	22 (18%)	1.4	25.0	498K
o4-mini	Mini-SWE-Agent	63.1	35.4	28.2	39.3 [32.1, 46.8]	34.7	49.8	30 (25%)	1.8	29.9	498K
Harness comparison											
5.2-Codex	Codex CLI	64.6	46.8	35.5	46.5 [38.6, 54.5]	49.3	59.7	17 (14%)	1.6	N/A	500K
o4-mini	Codex CLI	55.5	36.7	23.6	34.4 [27.3, 41.9]	33.4	51.3	27 (22%)	1.7	N/A	331K
5.2-Codex	OpenCode	69.1	45.3	37.1	50.0 [42.4, 57.9]	47.7	58.8	23 (19%)	1.8	18.2	532K
o4-mini	OpenCode	54.2	30.5	23.5	33.5 [26.6, 40.7]	38.4	44.5	25 (21%)	**1.4**	13.5	280K

Note. Values are averaged over 120 common bugs, after averaging the first three valid scored attempts within each bug. Coverage, block precision, mention coverage, and flaky-bug rates are percentages. Appendix D.3 details attempt selection and the common cohort. Lower is better for flaky bugs, blocks/diagnosis, calls/run, and tokens/run. Flaky bugs have block-coverage spread above 0.2 across attempts. 5.3-Codex and 5.2-Codex abbreviate GPT-5.3-Codex and GPT-5.2-Codex.

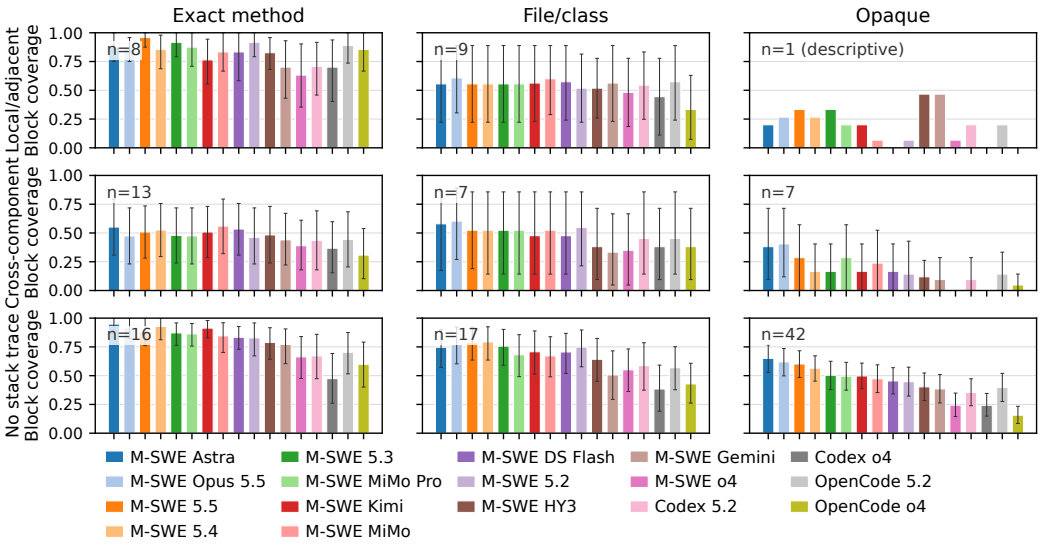


Figure 3: Block coverage by incident evidence. Columns group the issue text's strongest code anchor; rows group the first crash stack frame. Coverage drops most when reports lack a precise anchor and the crash site is absent or cross-component. Panel counts give bugs per cell; whiskers show 95% bug-bootstrap CIs (none for the single-bug cell).

Figure 3 divides the bugs into 9 categories, based on two kinds of help that an incident report can give the agent: whether the text names an exact gold method, a gold file/class, or no causal code at all; and whether the first crash stack frame is local or adjacent to the fix, cross-component, or absent. The figure reports per-cell sample counts and 95% bug-bootstrap CIs; small-cell comparisons are exploratory.

When the report names an exact method, agents often do well: the best current configuration reaches 95.8% block coverage on local-or-adjacent method-named bugs, and 94.8% on no-stack method-named bugs. When the report is opaque, even the best configuration reaches only 65.0% on the 42

no-stack opaque bugs and 40.5% on the seven cross-component opaque bugs. For a fixed baseline, Mini-SWE GPT-5.2-Codex shows the same pattern: its no-stack method-named coverage is 82.8%, but it falls to 44.7% on no-stack opaque bugs and 14.3% on cross-component opaque bugs.

Adding newer models raises absolute coverage but does not remove this weak-evidence gap. GPT-6 Astra improves no-stack opaque block coverage to 65.0%, yet this remains 29.8 percentage points below its 94.8% coverage on no-stack method-named bugs. These cases are common: opaque reports account for 50 of the 120 benchmark bugs, including 42 without a stack trace. The central challenge is navigating from symptoms to causal code without direct code references.

Finding 2. *Run-to-run determinism remains a central challenge.* Repeated attempts on the same bug can produce different diagnoses, so a single successful run can hide brittle behavior. This instability is especially visible on weak-evidence bugs even for the strongest model.

Diagnosis quality varies across repeated attempts on the same bug. GPT-6 Astra has the best aggregate block coverage, but 15 of its 120 bugs are flaky under our definition. Claude Opus 5.5 has 17 flaky bugs, GPT-5.5 has 20, GPT-5.4 has 21, GPT-5.2-Codex has 18, Kimi K2.6 and DeepSeek V4 Flash each have 32, MiMo v2.5 Pro has 22, MiMo v2.5 has 26, and HY3 has 35. The instability is most concerning on bugs that require navigation from weak evidence. For Mini-SWE GPT-5.5, the no-stack opaque cell has 11/42 flaky bugs (26%) and a mean within-bug block-coverage standard deviation of 9.2 points, while the no-stack method-named cell has only 2/16 flaky bugs (13%) and a 3.7-point mean standard deviation. Mini-SWE GPT-5.2 shows the same contrast: 11/42 no-stack opaque bugs are flaky (26%) compared with 1/16 no-stack method-named bugs (6%). This variance matters for diagnosis because it suggests that reports with little direct code evidence create brittle search trajectories rather than only uniformly harder examples.

Finding 3. *Newer models expose trade-offs rather than uniform performance improvements.* Broader exploration can help weak-evidence cases, but agents must remain precise when the report already provides strong evidence. Token usage does not correlate with coverage across different models.

The full Mini-SWE-Agent model sweep shows that stronger models do not simply raise every category. GPT-5.5 improves aggregate block coverage from 56.4% to 65.1% relative to GPT-5.2-Codex, and it improves difficult weak-evidence cells: no-stack opaque bugs improve by 15.4 percentage points. At the same time, GPT-5.5 emits more predicted blocks per diagnosis than GPT-5.2-Codex, 3.6 versus 2.3, and its block precision falls from 43.1% to 34.7%. GPT-5.5 recovers more causal code on opaque reports, but its broader diagnoses also include more irrelevant code. Agents need to search beyond weak initial clues without losing precision when the report already provides a strong local code reference.

The additional full-suite and subset experiments show the same broad lesson. MiMo v2.5 Pro reaches 58.5% block coverage and 72.6% mention coverage, but uses 2.6M tokens per run on average. MiMo v2.5 is slightly lower at 58.0% block coverage and 69.5% mention coverage while using 2.3M tokens per run. Kimi K2.6 reaches 58.3% block coverage, but uses 2.8M tokens per run on average and has 32 flaky bugs; DeepSeek V4 Flash roughly matches GPT-5.2-Codex block coverage while using 3.4$\times$ the tokens and lower block precision. Gemini 3.5 Flash is more conservative: it has the highest block precision, 51.1%, but only 47.5% block coverage. In Appendix F, DeepSeek V4 Pro gains 5.0 points while using 2.5$\times$ the GPT-5.2 tokens. Model scaling can improve exploration, but it is not a substitute for reliable diagnosis control.

Finding 4. *Harnesses change how much code the model names, but the core difficulty remains.* Different harnesses encourage different diagnosis styles: Mini-SWE names more code, Codex CLI is narrower and more precise, and OpenCode sits between them. This shifts the coverage–precision balance, but all harnesses still struggle when reports lack direct causal code references.

Comparing GPT-5.2-Codex across harnesses shows that the harness affects how diagnoses are shaped. Mini-SWE-Agent reaches 56.4% block coverage but 43.1% block precision, while Codex CLI reaches

lower block coverage, 46.5%, but the highest block precision, 49.3%. OpenCode is between them at 50.0% block coverage and 47.7% block precision. The blocks/diagnosis column in Table 1 is consistent with this tradeoff: Mini-SWE GPT-5.2-Codex predicts 2.3 blocks per diagnosis on average, compared with 1.6 for Codex CLI and 1.8 for OpenCode. These differences matter, but they are secondary to the shared failure mode: every harness degrades sharply when the report lacks direct causal code references.

4.3 Failure Modes and Design Implications

The results above describe a tunnel-vision problem. Agents often reach relevant files, but their diagnoses are broad, unstable, and especially weak when the report does not name the causal code. We next summarize failure modes and their implications for future harness designs. This analysis also shows why DISTDEBUG-BENCH's fine-grained evidence breakdown is useful: it provides concrete signals for diagnosing where current agents fail and what future harnesses should improve.

Failure Mode 1: Tool-call nondeterminism drives diagnosis nondeterminism. *Subtle differences in command choice can move runs into different evidence scopes, so future harnesses need adaptive tools that augment context automatically when early evidence is incomplete.*

Tool calls are a core reason why weak-evidence diagnosis becomes unstable. The models are often good at high-level sensemaking once they are in a relevant region, but the task requires many precise execution decisions: which keywords to search, which directory to enter, which file to open, which symbol to follow, and when to abandon a branch. When the report names the relevant method, different command orders can still converge on the same local region. When the report is opaque, small differences in search terms, directory choices, or the first file opened can send attempts into different subsystems.

For example, in ZooKeeper-3769, node logs are available. The core enabler for a successful diagnosis would be to identify the `BufferUnderflowException` inside `node1.log`. We observe that both successful and failed runs read `node1.log` early on, but successful runs searched for `Exception` in the log, while failed runs looked at specific line numbers or searched for election-related logs. Those reads revealed other transient errors but missed the `BufferUnderflowException`. Similar behavior appears in ZooKeeper-2845. The root cause is in `ZooKeeperServer.java` under the broader `/server` path, but the issue report strongly emphasizes quorum behavior. Existing agents often narrow early to the `/server/quorum` subdirectory. Once there, keyword search no longer surfaces the correct class, and the run is steered toward a wrong but coherent diagnosis.

Design Implication 1. To reduce nondeterminism, harnesses should provide tooling layers that infer the model's intent and automatically augment the context with relevant information. For example, when an agent searches for a keyword, static analysis can return related class definitions, symbol uses, and upstream/downstream contexts. This makes tool execution more precise when two commands are semantically aligned but differ subtly. Future harnesses should not leave context construction entirely to ad hoc shell commands: they need adaptive retrieval and context-augmentation tools that can surface adjacent files, call paths, related symbols, and competing hypotheses when the current search scope is under-supported.

Failure Mode 2: Agents rarely revise initial hypotheses or discover causal code beyond the initial evidence. *When the first plausible code region is not the causal one, agents often keep deepening that local explanation instead of reopening the search. In our trace proxy, only 21.0% of parseable runs showed late recovery, and only 11.25% of successful runs did so.*

We define *late recovery* as a run whose final diagnosis files do not appear among the first two inspected source files. Across 124 parseable Mini-SWE-Agent and OpenCode runs with at least two inspected source files, late recovery occurs in only 26 cases (21.0%): 7/21 for Mini-SWE-Agent and 19/103 for OpenCode. Among successful runs, only 9/80 (11.25%) are late recoveries.

Bugs like ZooKeeper-2845 and Ray-8889 illustrate this lock-in. Even when an agent finds code that appears to match the reported symptom (such as the downstream data consumer that raises the exception), it often stops at that obvious location instead of asking what earlier lifecycle state made the symptom possible. Trace inspections show that agents can ask relevant follow-up questions, but whether they explore those questions is highly nondeterministic and shaped by early exploration. This

early lock-in reinforces Failure Mode 1: because agents struggle to adapt their scope and reasoning, small tool-execution inaccuracies can become diagnosis failures.

Design Implication 2. Distributed-systems debugging requires each hypothesis path to accumulate its own evidence. Under the current single, ever-growing context, alternatives become harder to compare. This calls for context-management strategies beyond context compaction or sub-agent delegation, such as switching context when pursuing different hypotheses or actively pruning invalidated hypotheses.

Failure Mode 3: Diagnosis is hard because agents lack reliable self-correction signals before finalizing. *Distributed-system failures often suffer from reproducibility challenges. Agents often cannot run tests and get quick feedback on whether the diagnosis is correct; instead, they must rely on rigorous reasoning over source code and observed artifacts.*

Diagnosis often lacks a cheap pass/fail check for whether a proposed source region is the true root cause. Agents often cannot tell, before finalizing, whether the selected code region accounts for the full causal chain or whether another hypothesis still needs to be explored. Even when a runnable environment is available, agents can give up on testing because they prioritize reproducing the entire incident rather than decomposing it into smaller, unit-test-style checks. Similarly, when agents converge early, they sometimes stop for engineering-practice reasons and lack explicit correctness specifications for deciding whether upstream or downstream code should be blamed.

Our core insight is that bug diagnosis does not inherently lack correctness signals. Rather, signals such as raised exceptions, crash sites, logs, and documentation require consistent evaluation and cross-checking before they become useful.

Design Implication 3. Future harnesses should actively inject correctness signals into the agent's context, or use an evaluator sub-agent to verify whether the proposed diagnosis fully explains the buggy behavior from start to end, and whether the decision to blame one component is backed by concrete evidence.

5 Conclusion and Future Work

We introduced DISTDEBUG-BENCH, a benchmark for evaluating LLM agents on code-level root-cause diagnosis of real-world distributed system failures. Our evaluation shows that current agents often reach relevant repository regions but struggle to pinpoint the precise causal locations, especially when issue reports lack direct code references or stack traces. Trace analysis highlights inconsistent tool use and limited revision of initial hypotheses. The evidence breakdown, run-to-run flakiness, and failure-mode analyses together suggest that distributed system debugging requires more than broader search or model scaling. These findings motivate future research and agent harness designs that support adaptive context construction, hypothesis-aware context management, and correctness checks, so that exploration, revision, and validation are explicit parts of agentic diagnosis workflows.

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Appendix Overview

The appendix provides supporting material for the benchmark construction, evaluation protocol, and trace analysis.

- Appendix A describes the released benchmark artifacts, example entries, curation process, classification labels, root-cause label format, annotation agreement, and threats to validity.
- Appendix B summarizes the automated collection-pipeline evaluation against human labels.
- Appendix C gives the prompts used for diagnosis generation.
- Appendix D reports metric-sensitivity checks, bootstrap confidence intervals, and leave-one-system-out results.
- Appendix E gives trace-level details behind the failure-mode discussion.
- Appendix F reports exploratory subset experiments for additional model families.
- Appendix G reports the evaluator-validity audit, diagnosis-assisted repair pilot, and contamination analyses.

A Benchmark Details

A.1 Artifact Layout

Each benchmark instance is stored as a small directory under `benchmark/bugs/<system>/<bug-id>/`. The release separates issue-side evidence from gold labels so that an evaluator can instantiate the task without looking at the fixing patch.

```
benchmark/bugs/<system>/<bug-id>/
  report.json        # title, description, metadata, links, artifact pointer
  blocks.json        # gold pre-fix root-cause blocks
  artifact_dir/      # optional logs, screenshots, reproducer files, traces
```

The `report.json` file contains the original issue title and description, project metadata, and flags such as whether the report contains logs, stack traces, prior analysis, or attached artifacts. The `blocks.json` file is the scoring target. Each block uses repository-local coordinates in the pre-fix snapshot:

```
{
  "file": "src/java/main/org/apache/zookeeper/server/ZooKeeperServer.java",
  "function": "ZooKeeperServer.shutdown",
  "start_line": 555,
  "end_line": 558
}
```

These coordinates are interpreted in the pre-fix checkout. Labels intentionally exclude tests, incidental refactors, defensive hardening, and cleanup unless those edits are necessary to explain the original failure mechanism.

A.2 ZooKeeper-2845 Example

ZooKeeper-2845 is the running example used in the paper. The report describes a data-inconsistency scenario where a transaction can be present in the on-disk log but absent from the snapshot during leader election. The report repeatedly mentions quorum behavior and leader election, while the gold label is in the server lifecycle path:

```
Title: Data inconsistency issue due to retain database in leader election
Key symptom: txn is ahead of snapshot; a restarted server replays T1 and
diverges from the new quorum.
Gold file: src/java/main/org/apache/zookeeper/server/ZooKeeperServer.java
Gold function: ZooKeeperServer.shutdown
```

This example is useful because the issue-side narrative points strongly toward `/server/quorum`, but the minimal causal block is in `ZooKeeperServer.shutdown`. It therefore exercises the benchmark behavior we care about: the agent must move from a plausible symptom-level subsystem to the code that preserves inconsistent state across restart.

Figure 4 visualizes this distance. The observed symptom is a quorum divergence after restart: nodes A and B agree at state T0, while node C later replays transaction T1 from its local log. The issue report gives useful evidence, including retained `ZKDatabase` state, leader election, snapshot/log skew, empty DIFF sync, and txn-log replay. These clues naturally make leader election, quorum-peer coordination, learner sync, snapshot handling, and transaction-log replay plausible first inspection targets. The gold root cause is different: `ZooKeeperServer.shutdown` retains the in-memory database across restart, allowing stale state to survive into later quorum formation. The example therefore shows why DISTDEBUG-BENCH measures more than component discovery. A diagnosis must follow the non-local causal hop from a realistic symptom-side search space to the lifecycle code that preserves the bad state.

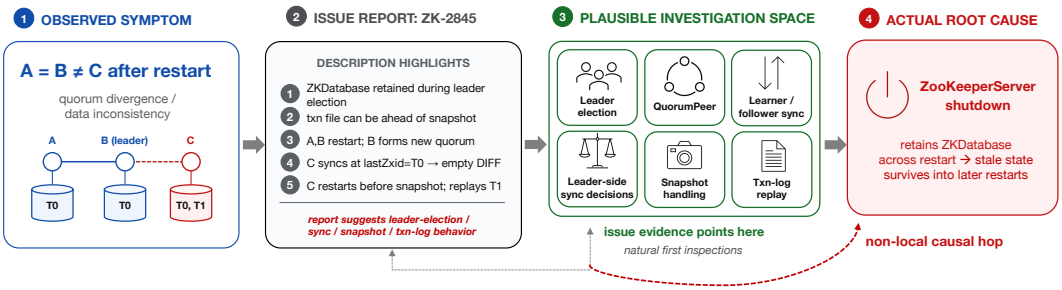


Figure 4: Causal distance in ZooKeeper-2845. The issue evidence points toward leader election, learner sync, snapshots, and transaction-log replay, but the labeled root cause is the lifecycle block in `ZooKeeperServer.shutdown` that retains stale `ZKDatabase` state across restart.

```
Issue: ZOOKEEPER-2845
Title: Data inconsistency issue due to
retain database in leader election

In ZOOKEEPER-2678, ZKDatabase is retained to
reduce the unavailable
time during leader election. ...

But if the txn log is ahead of the snapshot,
the ensemble can become
inconsistent:
1. a request is written to leader C as txn
T1
2. followers A and B restart before
receiving T1
3. A and B form a new quorum after restart
4. C later restarts and replays T1 from disk
5. C now contains Node N, but A and B do not
```

(a) Issue-side evidence

```
buggy_blocks:
  - file: src/java/main/org/apache/
  zookeeper/server/ZooKeeperServer.java
    function: ZooKeeperServer.shutdown
    lines: 528-562
  - file: src/java/main/org/apache/
  zookeeper/server/quorum/QuorumPeer.java
    function: QuorumPeer.getLastLoggedZxid
    lines: 926-931

metadata:
  severity: Major
  root_cause_type: Logic
  bug_complexity: Medium
  distributed: Yes
```

(b) Gold label

Figure 5: Raw benchmark excerpt for `ZooKeeper-2845`.

A.3 Curation Process

DISTDEBUG-BENCH is curated through a staged process. The goal is to retain bugs that are realistic debugging tasks, diagnosable from issue-side evidence, and precise enough to support source-level labels.

Candidate discovery. For each project, we collect historical public issues and apply automatic filters for concrete fixed bugs. The filters require closed or resolved issue status, an associated fixing change, a bug-like issue type or resolution, and an informative report. When project metadata is available, we prioritize high-impact user-facing issues, such as Major, Critical, or Blocker bugs on stable releases. We exclude feature requests, documentation-only issues, test-only issues, and reports whose description is too sparse to support diagnosis.

Fix-level filtering. The fixing change is used only for curation and labeling, not as agent input. We remove broad refactorings, formatting-only changes, dependency updates, and patches dominated by unrelated cleanup. As a practical filter, we exclude fixes with more than 100 changed source lines unless the causal span is clearly separable from auxiliary edits.

Manual audit. Annotators check that an issue is diagnosable from issue-side context alone. Issue reports and attached artifacts are used as the diagnosis context, while fixing commits are used only for human annotation. Later issue-thread comments that reveal the root cause or patch strategy are removed or ignored. This keeps the task focused on diagnosis from incident evidence rather than retrieval of the final answer from post-fix discussion.

Table 2: Example attrition in the ZooKeeper curation slice.

Stage	Issues	Selection step
Public issue pool	4,789	Historical ZooKeeper issues considered by the collection pipeline.
Automatic candidates	99	Closed fixed bugs that pass metadata, report, and fixing-change filters.
Manual inspection sample	50	Candidate issues sampled for human audit and root-cause labelability checks.
Released ZooKeeper incidents	18	Accepted incidents with diagnosable issue-side context and reconciled source-level labels.

Table 3: Textual code-reference categories used in the evaluation breakdown.

Category	Definition	Count
Method-named	A non-generic gold function name appears in the issue text	37
File/class-named	A gold file stem or class name appears, but no gold method	33
Opaque	No extracted gold method, file, or class identifier appears	50

Root-cause reconciliation. Each accepted issue is independently inspected by three annotators. The annotators derive candidate pre-fix root-cause blocks from the fixing change and surrounding code context, exclude tests and incidental cleanup, and reconcile disagreements into a final block set. The reconciled labels are the only labels used for benchmark scoring.

The benchmark also records characterization metadata for each accepted issue. These metadata fields are not scoring targets and are not provided as hints to agents. They support the analysis in the paper by letting us break down results by textual code anchors, crash-to-fix distance, issue-side evidence, and root-cause structure.

A.4 Classification Axes

The benchmark characterization uses two gold-aware axes. These classifications are used only for analysis and figure breakdowns; they are not provided to agents at evaluation time.

For the main category figure, we merge co-located and adjacent into a *local/adjacent* row because the adjacent cell is small. The resulting 3-by-3 table contains 42 no-stack opaque bugs, 16 no-stack method-named bugs, 17 no-stack file/class-named bugs, 7 cross-component opaque bugs, 13 cross-component method-named bugs, 7 cross-component file/class-named bugs, 1 local/adjacent opaque bug, 8 local/adjacent method-named bugs, and 9 local/adjacent file/class-named bugs.

We also assign a coarse root-cause type to each incident for benchmark characterization. These types are descriptive and are not used by the evaluator or exposed to agents. They give a high-level view rather than an exhaustive failure taxonomy; finer distinctions such as state management, protocol interaction, recovery, configuration, and multi-component coordination would require separate annotation.

A.5 Label Structure

DISTDEBUG-BENCH contains 218 gold buggy blocks across 120 incidents. Multi-block labels are common: 54 incidents (45.0%) require more than one block, and 28 incidents (23.3%) span multiple

Table 4: Crash-to-fix categories used in the evaluation breakdown.

Category	Definition	Count
Co-located	First project-internal stack frame is in a gold file	12
Adjacent	Crash frame and gold file are in nearby package structure	6
Cross-component	Stack trace exists, but the first internal frame is elsewhere	27
No stack trace	No usable project-internal stack frame is found	75

Table 5: Issue-side signal fields extracted for benchmark characterization.

Signal present in report or attached evidence	Incidents
At least one code identifier	96
Prior analysis or causal explanation	37
Stack trace	28
Logs	20
Cross-linked issue reference	22
Attached artifacts	7

Table 6: Coarse root-cause categories in the full 120-incident benchmark.

Root-cause type	Incidents
Logic	56
Concurrency	28
Error handling	26
Performance	4
Resource leak	3
Communication	2
Dependency	1

files. The median labeled block length is five lines. This is why the paper reports file/function/line metrics but uses block coverage as the primary diagnosis metric: useful diagnoses must reach the right region without being overly sensitive to exact line-boundary choices.

Annotation agreement. All 120 incidents followed the same three-annotator labeling and reconciliation workflow. For 51 incidents, we retained the separate annotation sheets from before adjudication, allowing us to measure agreement among the independent labels. For the other 69 incidents, disagreements were resolved through discussion without retaining separate pre-adjudication sheets. The agreement statistics below therefore describe the 51 retained cases.

We assess agreement at file, normalized-function, and block granularity. Table 7 reports Krippendorff's α and pairwise Jaccard similarity at the first two levels, and block coverage and F1 under the benchmark's five-line-tolerant block criterion. Annotators agree strongly on the causal files and functions. Differences in exact line-span boundaries and in how annotators split a causal region into blocks reduce exact-span agreement, while agreement under the block criterion remains high.

Table 7: Pre-adjudication annotation agreement on the 51 incidents with retained individual annotation sheets.

Granularity	Measure	Agreement
File	Krippendorff's α	0.919
	Pairwise Jaccard	0.902
Normalized function	Krippendorff's α	0.866
	Pairwise Jaccard	0.837
Block (five-line tolerance)	Coverage	0.861
	F1	0.848

A.6 Threats to Validity

Internal validity. The main internal risks are that the benchmark labels or evaluator may not faithfully measure the diagnosis behavior we intend to study.

Label validity. Distributed-system fixes can include upstream state transitions, downstream guards, invariant checks, tests, and auxiliary cleanup. We mitigate this by deriving labels from the historical fixing change and surrounding code context, having three annotators independently inspect each issue, reconciling disagreements into a final block set, and labeling only the minimal pre-fix code that embodied the failure mechanism. Tests, helper refactors, defensive hardening, and incidental cleanup

are excluded unless they are necessary to the causal explanation. On the 51 incidents with retained independent annotation sheets, agreement is high at the file and function levels (Appendix A.5).

Evaluation sensitivity. A distributed-system failure may be mitigated or repaired at several locations, but the causal chain that produces it is specific. In ZooKeeper-2845, for example, one could add defensive handling later in the synchronization or recovery path. Such a change might reduce the observed inconsistency, but it would not identify the underlying bug: the retained database and shutdown/restart behavior let the state used during synchronization diverge from the transaction log. DISTDEBUG-BENCH therefore labels the causal code identified through issue resolution and expert annotation, rather than every location where a symptom could be detected, masked, or repaired; it does not claim that the historical patch is the only valid fix. A diagnosis that blames a different plausible intervention point can still receive low block coverage. The paper therefore reports file, function, line, and block metrics, together with mention coverage, which records whether the causal explanation refers to the labeled functions even when the selected blocks differ. Appendix G.1 shows that such alternatives explain few zero-coverage diagnoses and that higher-coverage diagnoses guide repair more effectively. Future evaluators could combine multi-reference annotations with semantic and program-analysis-based validation to better capture equivalent causal explanations.

Benchmark leakage. DISTDEBUG-BENCH is built from public user-reported failures. This makes the benchmark realistic and reproducible, but it also means that model pretraining exposure cannot be ruled out completely. Public benchmark identifiers and future fix information can contaminate an evaluation if agents retrieve them rather than diagnose from allowed evidence. We mitigate direct answer leakage during benchmark construction by removing issue-thread discussion that reveals the root cause or patch strategy, anonymizing workspace names, and removing commits after the task snapshot. The leakage guard Figure 1 scans visible reasoning and tool inputs/outputs for the original issue identifiers withheld from the agent. This check targets direct retrieval of historical answers, including searches for future fixing commits. Flagged attempts are discarded and rerun, and the team investigates residual identifiers or accessible fix information in the prepared environment. Because detection depends on explicit identifier matches, implicit recall requires separate evaluation. Appendix G.2 reports guard-hit counts, closed-book probes, and correlations with public-exposure proxies.

External validity. The main external risk is whether conclusions from DISTDEBUG-BENCH generalize beyond this benchmark sample.

Selection bias. DISTDEBUG-BENCH intentionally focuses on closed, high-impact failures in production distributed systems whose fixes support precise source-level labeling. This selection makes the task realistic and scoreable, but it excludes failures whose fixes are dominated by broad refactorings, configuration changes, test-only changes, or ambiguous cleanup. The benchmark should therefore be interpreted as covering code-level diagnosis for labelable distributed-system bugs rather than all incident-response work.

We mitigate this risk with a principled, staged selection process. For each project, we first collect historical public issues and apply automatic filters for concrete fixed bugs: closed or resolved status, an associated fixing change, high-priority or user-facing failures when priority is available, sufficiently informative reports, and non-test/non-documentation components. We then filter by the fixing change itself, excluding broad refactorings and patches dominated by unrelated cleanup, and using a practical 100 changed-source-line cutoff unless the causal span is clearly separable from auxiliary edits. The ZooKeeper attrition example in Table 2 illustrates the scale of this filtering process. Finally, annotators manually audit the survivors to confirm that the issue-side context is diagnosable without post-report discussion, that the report does not reveal the root cause or patch strategy, and that the fixing change maps to precise pre-fix root-cause blocks. Each accepted issue is then independently annotated by three annotators and reconciled into the final label set. This process does not eliminate selection bias, but it makes the benchmark population the result of consistent filters and explicit audits rather than convenience sampling.

B LLM-Assisted Collection Pipeline

Benchmark curation requires human review for two reasons: identifying true bug reports while excluding feature requests, and identifying the source blocks that encode each root cause. We

Table 8: Collection-pipeline hit-rate comparison on the 12-bug shared subset. Scores compare generated labels with human labels.

Construction method	GT blocks	Pred. blocks	Recall	Precision	F1
LLM-assisted label generation	22	15	54.5%	73.3%	62.6%
Non-LLM extraction pipeline	22	52	77.3%	34.6%	47.8%

developed an LLM-assisted pipeline for the second bottleneck. The pipeline starts from a bug-fixing commit, fetches the diff automatically, and prompts an LLM to output only the relevant root-cause blocks. Preliminary checks show a non-trivial gap between LLM-produced labels and human labels, but we still see potential in LLM-assisted automation pipelines. Below, we report a small-subset comparison between this LLM-assisted pipeline, a non-LLM extraction pipeline, and the final human labels. The first bottleneck is also important, but determining whether a bug is diagnosable from the available information is essentially asking LLM agents to diagnose the bug itself, which is the task we are trying to test in the first place. We will explore fully automating DISTDEBUG-BENCH's curation process while maintaining benchmark quality, with more fine-grained bug classification based on information availability.

We evaluate two benchmark-construction variants against the human labels used in DISTDEBUG-BENCH. The LLM-assisted variant starts from the bug-fixing diff and prompts an LLM to select the source blocks that correspond to the root cause, excluding tests, helper refactors, and other auxiliary edits. The non-LLM variant is a deterministic extraction pipeline that derives candidate labels from the fixing change using source-level parsing and filtering, without asking an LLM to judge semantic relevance. Both variants address the second curation bottleneck from Section 3: separating root-cause code blocks from other edits in a known fixing change.

The LLM-assisted variant is available only on a small subset, so the table below reports both variants on the same 12 bugs completed by the LLM-assisted pipeline. The non-LLM pipeline covers additional bugs, but those are omitted from this comparison to keep the subset fixed.

Table 8 shows a coverage–precision trade-off. On the shared subset, the non-LLM pipeline recovers more human-labeled blocks, but it does so by emitting many more candidate labels. The LLM-assisted pipeline is more conservative: it predicts 15 blocks compared with 52 for the non-LLM pipeline, yielding higher precision but lower recall. This suggests that LLM-assisted label extraction is a promising direction for scaling benchmark construction, but the remaining recall gap shows that it is not yet a substitute for human label review and motivates future work on more reliable automated curation.

C Evaluation Prompts and Output Contract

All evaluated harnesses receive the same diagnosis task: inspect the pre-fix repository and issue-side evidence, identify the pre-fix code blocks that encode the root cause, and emit a structured diagnosis. The exact interaction syntax differs by harness, but the task-level constraints are shared:

- the agent should diagnose rather than patch the bug;
- the agent should not modify repository source files;
- lightweight static analysis is preferred over long-running integration tests or networked services;
- the final answer must be valid JSON in the required schema; and
- each completed bug/configuration pair contributes three valid diagnoses.

C.1 Mini-SWE-Agent Prompt Excerpt

The Mini-SWE-Agent experiments use a diagnosis-only system prompt and an instance prompt that instructs the model to write `diagnosis.json`. The following excerpt shows the task-level content, omitting harness-specific formatting examples for shell actions.

```
You are a helpful assistant that can interact with a computer through bash
tool calls.

Your job is diagnosis only. Do not modify the codebase and do not attempt to
produce a fix.

Please debug the root cause of this distributed-systems issue:

{{ issue_title_and_description }}

Produce a code-level diagnosis of the bug. Your final output is a JSON file
named diagnosis.json. Do not include Markdown in the file.

Requirements:
- Do not modify repository source files.
- Use the repository, issue text, stack traces, and attached artifacts to reason
  about the root cause.
- You may inspect code and run lightweight local commands.
- Avoid long-running integration tests or networked services.
- Keep diagnosis.json as pure JSON with exactly the schema below.
- Before finishing, validate diagnosis.json with the provided validator.
```

C.2 Codex CLI and OpenCode Prompt Excerpt

For Codex CLI and OpenCode, the prompt is expressed in the harness' native task format, but preserves the same diagnosis-only contract. The following excerpt is representative of the task message:

```
You are auditing a historical bug report for a distributed system.
Your task is to inspect the repository, understand the failure scenario, and
identify the exact code blocks responsible.

Instructions:
- Work only inside the current Git repository, already checked out at the target
  pre-fix commit.
- Use lightweight static analysis such as search, grep, and reading code.
- Avoid running integration tests unless necessary.
- When you reach a conclusion, provide a JSON object that follows the supplied
  schema. Do not embed Markdown or extra commentary.
- Each buggy block must have an accurate file, function, start_line, and
  end_line.
- Include a concise bug_cause_description tying everything together.
```

C.3 Diagnosis Schema

The required output schema is intentionally small. The evaluator ignores optional narrative fields for scoring and compares the predicted block coordinates against the gold labels at file, function, line, and block granularity.

```
{
  "buggy_blocks": [
    {
      "file": "relative/path/to/file",
      "function": "fully.qualified.FunctionName",
      "start_line": 123,
      "end_line": 130,
      "bug_contribution": "How this block contributes to the bug.",
      "patch": "Optional minimal patch sketch or explanation."
    }
  ],
  "bug_cause_description": "Overall root-cause explanation."
}
```

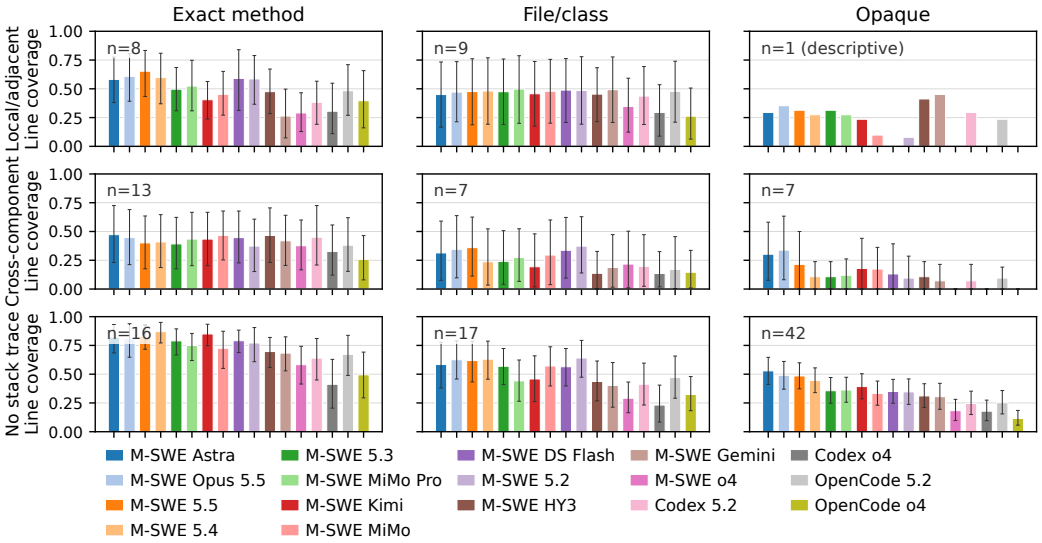


Figure 6: Line coverage by incident evidence. Columns split bugs by whether the issue text contains an exact gold method, only a gold file/class anchor, or no direct textual anchor. Rows split bugs by whether the first crash stack frame is local or adjacent to the fix, in another component, or absent. Line coverage is stricter than block coverage and measures overlap with labeled causal lines. Panel counts give bugs per cell; whiskers show 95% bug-bootstrap CIs (none for the single-bug cell).

The `patch` field is present for compatibility with some harness outputs, but patch quality is not scored in this release. The scoring pipeline uses only the predicted source coordinates and, for mention coverage, the natural-language function references in `bug_cause_description`.

D Coverage Metric Sensitivity

The main paper uses block coverage as the primary localization metric because it asks whether the agent reached each labeled causal region while being less sensitive to exact line-boundary choices. Line coverage is stricter: a diagnosis can reach the correct block but receive lower line coverage if the predicted span is shifted, shorter, or longer than the human label. We therefore treat line coverage as a sensitivity check rather than the main story.

Figure 6 shows that the qualitative pattern from the main block-coverage figure remains visible under the stricter metric: opaque and no-stack bugs remain much harder than method-named bugs. The absolute values are lower because line overlap penalizes span-boundary mismatch.

Figure 7 provides the paired GPT-5.4 versus GPT-5.2 sensitivity view. The line metric shows the same caution as the block metric: newer models improve several weak-evidence cells, but the changes are not uniform across categories.

D.1 Aggregation and Statistical Uncertainty

For metric scores s_{ir} on bug i and attempt r, we first compute $\bar{s}_i = R_i^{-1} \sum_{r=1}^{R_i} s_{ir}$ and then report $\hat{\mu} = n^{-1} \sum_{i=1}^{n} \bar{s}_i$. Each included bug has $R_i = 3$ selected attempts, and bugs have equal weight. Each bootstrap replicate samples n bug IDs with replacement and averages their $\bar{s}_i$ values. The 2.5th and 97.5th percentiles of 10,000 replicate means form the reported 95% interval. We use sorted bug IDs, NumPy's PCG64 generator with seed 2026, and linear interpolation for quantiles. Category intervals use the same procedure within each category. A cell with fewer than two bugs has no CI.

These intervals treat bugs as independent draws from the incidents the benchmark represents. They capture sensitivity to which bugs are sampled, not rerun variability on a fixed bug, which we report separately as within-bug standard deviations and flaky-bug counts. Because bugs from the same system may be correlated, Appendix D.2 also reports per-system and leave-one-system-out results.

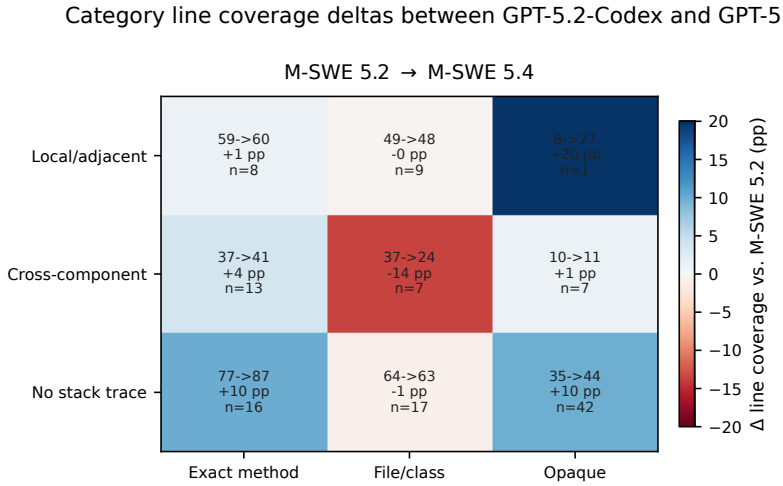


Figure 7: Paired line-coverage deltas between Mini-SWE GPT-5.2-Codex and Mini-SWE GPT-5.4 by evidence category. Each cell reports absolute GPT-5.2→GPT-5.4 line coverage, the paired delta in percentage points, and the number of bugs.

Table 9: Mean scores and 95% bug-level bootstrap confidence intervals across 120 common bugs, in percent.

Configuration	File	Function	Line	Block	Block precision	Mention
MiniSWE GPT-6 Astra	83.9 [78.1, 89.1]	58.4 [50.6, 66.3]	53.9 [46.9, 61.2]	67.7 [60.4, 74.9]	42.7 [36.6, 48.9]	82.8 [76.9, 88.3]
MiniSWE Claude Opus 5.5	82.0 [76.0, 87.5]	60.4 [52.9, 68.0]	53.5 [46.4, 60.7]	66.4 [59.2, 73.6]	45.5 [39.7, 51.5]	80.6 [74.5, 86.1]
MiniSWE GPT-5.5	84.0 [78.1, 89.3]	55.5 [47.5, 63.4]	52.7 [45.7, 59.7]	65.1 [57.8, 72.3]	34.7 [29.6, 39.7]	74.7 [67.9, 81.2]
MiniSWE GPT-5.4	79.8 [73.8, 85.3]	53.1 [45.3, 61.0]	50.4 [43.4, 57.4]	63.1 [56.0, 70.2]	39.5 [34.3, 44.8]	69.0 [62.0, 75.8]
MiniSWE GPT-5.3-Codex	79.1 [72.7, 85.0]	53.0 [44.9, 60.8]	44.5 [37.7, 51.4]	59.7 [52.2, 67.2]	40.0 [34.4, 45.7]	68.9 [61.8, 75.7]
MiniSWE MiMo v2.5 Pro	78.9 [72.3, 84.9]	60.1 [52.6, 67.6]	43.4 [36.6, 50.3]	58.5 [50.8, 66.1]	38.1 [32.3, 44.0]	72.6 [65.6, 79.1]
MiniSWE Kimi K2.6	77.5 [71.3, 83.2]	51.2 [43.8, 58.7]	44.8 [37.9, 51.9]	58.3 [50.9, 65.6]	45.9 [39.3, 52.6]	70.6 [63.7, 77.1]
MiniSWE MiMo v2.5	77.8 [71.4, 83.9]	54.2 [46.6, 61.9]	43.9 [37.1, 50.9]	58.0 [50.5, 65.4]	36.5 [31.0, 42.1]	69.5 [62.4, 76.3]
MiniSWE DeepSeek V4 Flash	76.0 [69.7, 81.9]	49.4 [41.5, 57.2]	45.9 [39.0, 53.0]	56.4 [49.0, 63.7]	32.7 [27.7, 37.9]	67.0 [59.9, 74.0]
MiniSWE GPT-5.2-Codex	75.9 [69.0, 82.3]	48.8 [40.6, 56.8]	45.9 [38.8, 53.1]	56.4 [48.5, 64.2]	43.1 [36.6, 49.5]	63.6 [56.0, 71.0]
MiniSWE HY3	72.5 [65.4, 79.3]	49.1 [41.5, 56.7]	39.7 [33.1, 46.4]	51.6 [44.1, 59.1]	38.7 [32.2, 45.4]	64.5 [57.3, 71.5]
MiniSWE Gemini 3.5 Flash	65.9 [58.3, 73.0]	43.5 [35.7, 51.3]	37.3 [30.2, 44.6]	47.5 [39.7, 55.2]	51.1 [42.9, 59.1]	58.5 [51.0, 65.9]
MiniSWE o4-mini	63.1 [55.4, 70.3]	35.4 [28.5, 42.8]	28.2 [22.2, 34.5]	39.3 [32.1, 46.8]	34.7 [28.1, 41.2]	49.8 [42.2, 57.2]
Codex CLI GPT-5.2-Codex	64.6 [57.0, 72.0]	46.8 [38.9, 54.8]	35.5 [28.5, 42.5]	46.5 [38.6, 54.5]	49.3 [41.2, 57.4]	59.7 [52.3, 67.0]
Codex CLI o4-mini	55.5 [47.9, 62.9]	36.7 [29.4, 44.1]	23.6 [17.5, 29.9]	34.4 [27.3, 41.9]	33.4 [26.2, 40.7]	51.3 [43.8, 58.8]
OpenCode GPT-5.2-Codex	69.1 [61.9, 76.0]	45.3 [37.5, 53.2]	37.1 [30.2, 44.3]	50.0 [42.4, 57.9]	47.7 [40.2, 55.4]	58.8 [51.3, 66.3]
OpenCode o4-mini	54.2 [46.5, 61.7]	30.5 [23.5, 37.6]	23.5 [17.7, 29.4]	33.5 [26.6, 40.7]	38.4 [30.7, 46.2]	44.5 [36.9, 52.1]

Paired differences. For configurations A and B, we compute one difference per bug, $d_i = \bar{s}_{i,A} - \bar{s}_{i,B}$, and estimate $\hat{\Delta} = n^{-1} \sum_i d_i$. Each replicate samples the same n bug IDs for both configurations, equivalently resampling the d_i values. We use the same 10,000-replicate percentile procedure, seed, and quantile convention as above. Pairing is by bug ID, not by attempt index; the three selected attempts are averaged within each bug before resampling.

Table 10 reports block-coverage differences for the twelve other Mini-SWE configurations against GPT-5.2-Codex, GPT-5.5 against GPT-5.4, and the four matched-model Mini-SWE versus Codex CLI/OpenCode comparisons. These intervals are unadjusted for multiple comparisons. Pairing makes model comparisons more sensitive than comparing separate intervals: the individual CIs of Mini-SWE GPT-5.5 and GPT-5.2-Codex overlap, yet GPT-5.5's 8.71-point gain has a paired CI of $[4.96, 12.91]$.

Table 10: Paired block-coverage differences on the 120 common bugs, in percentage points (candidate minus reference). Intervals are unadjusted pairwise 95% bug-bootstrap CIs; they are not simultaneous leaderboard bounds.

Candidate	Reference	Difference	95% CI
MiniSWE GPT-6 Astra	MiniSWE GPT-5.2-Codex	$+11.32$	$[+6.02, +17.07]$
MiniSWE Claude Opus 5.5	MiniSWE GPT-5.2-Codex	$+10.06$	$[+5.72, +14.69]$
MiniSWE GPT-5.5	MiniSWE GPT-5.2-Codex	$+8.71$	$[+4.96, +12.91]$
MiniSWE GPT-5.4	MiniSWE GPT-5.2-Codex	$+6.76$	$[+3.00, +10.83]$
MiniSWE GPT-5.3-Codex	MiniSWE GPT-5.2-Codex	$+3.34$	$[+0.13, +6.88]$
MiniSWE MiMo v2.5 Pro	MiniSWE GPT-5.2-Codex	$+2.13$	$[-2.67, +6.85]$
MiniSWE Kimi K2.6	MiniSWE GPT-5.2-Codex	$+1.96$	$[-2.53, +6.73]$
MiniSWE MiMo v2.5	MiniSWE GPT-5.2-Codex	$+1.58$	$[-2.73, +6.07]$
MiniSWE DeepSeek V4 Flash	MiniSWE GPT-5.2-Codex	$+0.01$	$[-4.52, +4.54]$
MiniSWE HY3	MiniSWE GPT-5.2-Codex	-4.76	$[-9.51, -0.16]$
MiniSWE Gemini 3.5 Flash	MiniSWE GPT-5.2-Codex	-8.92	$[-14.88, -3.07]$
MiniSWE o4-mini	MiniSWE GPT-5.2-Codex	-17.10	$[-22.32, -12.20]$
MiniSWE GPT-5.5	MiniSWE GPT-5.4	$+1.95$	$[-1.26, +5.36]$
MiniSWE GPT-5.2-Codex	Codex CLI GPT-5.2-Codex	$+9.86$	$[+5.62, +14.46]$
MiniSWE GPT-5.2-Codex	OpenCode GPT-5.2-Codex	$+6.35$	$[+2.51, +10.40]$
MiniSWE o4-mini	Codex CLI o4-mini	$+4.85$	$[+0.24, +9.59]$
MiniSWE o4-mini	OpenCode o4-mini	$+5.75$	$[+0.88, +10.47]$

D.2 System Composition and Leave-One-System-Out Sensitivity

We examine whether the aggregate result is dominated by one system using Mini-SWE-Agent with GPT-5.4. Table 11 reports block coverage and 95% CIs for all 11 systems. Each bug contributes the mean of its three selected attempts; system-specific intervals resample bugs within that system using the procedure in Appendix D.1.

For each leave-one-system-out comparison, we remove every bug from one system and recompute the file- and block-coverage means over the remaining bugs. We retain equal weight per bug and bootstrap the remaining bugs for each interval; we do not average the system-level means. Table 12 gives all 11 exclusions and the full-cohort reference. Removing a system changes block coverage by at most 2.7 percentage points, from a full-cohort mean of 63.1%. The file-to-block coverage gap remains between 15.1 and 17.8 points under every exclusion. The difficulty of precise causal-code localization therefore persists without any single system.

Table 11: MiniSWE GPT-5.4 block coverage by system. Scores and 95% bug-bootstrap CIs are percentages; each bug has three selected attempts.

System	Bugs	Block coverage [95% CI]
Overall	120	63.1 [56.0, 70.2]
Elasticsearch	19	70.1 [53.3, 85.4]
etcd	8	45.8 [14.6, 75.0]
HBase	5	36.7 [6.7, 70.0]
HDFS	10	52.7 [26.0, 79.3]
Ignite	3	61.1 [0.0, 100.0]
Ignite 3	7	64.3 [35.7, 88.1]
Kafka	8	79.2 [58.3, 93.8]
Kubernetes	10	81.7 [60.0, 100.0]
Ray	21	56.7 [41.1, 72.2]
TiKV	11	43.4 [19.2, 67.7]
ZooKeeper	18	78.7 [58.3, 95.4]

Table 12: Leave-one-system-out results for MiniSWE GPT-5.4. Coverage and CIs are percentages. Δ is block coverage minus the full-cohort mean; the file–block gap and Δ are in percentage points. Each remaining bug retains equal weight.

Omitted system	Bugs left	File	Block [95% CI]	Δ	File–block gap
None	120	79.8	63.1 [56.0, 70.2]	+0.0	16.6
Elasticsearch	101	78.4	61.8 [53.8, 69.7]	−1.3	16.5
etcd	112	80.5	64.4 [57.0, 71.7]	+1.2	16.1
HBase	115	80.8	64.3 [56.7, 71.5]	+1.2	16.5
HDFS	110	79.5	64.1 [56.9, 71.6]	+1.0	15.4
Ignite	117	79.7	63.2 [55.9, 70.4]	+0.1	16.5
Ignite 3	113	80.2	63.1 [55.6, 70.3]	−0.1	17.1
Kafka	112	79.2	62.0 [54.6, 69.4]	−1.1	17.2
Kubernetes	110	78.9	61.5 [54.1, 69.1]	−1.7	17.4
Ray	99	82.1	64.5 [56.3, 72.5]	+1.4	17.6
TiKV	109	80.2	65.1 [57.7, 72.3]	+2.0	15.1
ZooKeeper	102	78.2	60.4 [52.5, 67.9]	−2.7	17.8

D.3 Attempt Selection and Common Evaluation Cohort

For each bug/configuration pair, we sort attempts by numeric run ID and retain the first three with a schema-valid `diagnosis.json` and completed, matching scores. Attempts missing these artifacts do not occupy a slot; later completed attempts can replace them. Selection does not depend on the score: completed incorrect diagnoses, including schema-valid empty predictions, remain in the evaluation. Additional valid attempts after the first three are unused.

The primary comparison uses all 120 benchmark incidents across 17 configurations: 13 Mini-SWE model configurations, plus two models on each of Codex CLI and OpenCode. The three-attempt protocol gives $120 \times 17 \times 3 = 6{,}120$ selected attempts. The same bug cohort and attempt selection apply to accuracy, cost, variability, stratified results, and both kinds of CI.

All primary metrics are recomputed from these selected attempts before aggregation. The released selection manifest records selected run IDs, skipped attempts, extra unused valid attempts, the fixed bug cohort, and source hashes. The portable score snapshot retains the selected per-attempt scores and the common bug IDs, allowing both kinds of CI to be recomputed without access to full trajectories. Exploratory subset and separate trace analyses state their own cohorts.

E Failure-Mode Trace Details

This appendix gives the operational details behind the failure-mode discussion in Section 4. The main category breakdown measures late discovery at the outcome level: in cross-component opaque bugs, the report gives an initial stack-trace anchor but does not name the causal code, and agents still rarely reach the right block. The diagnostics below quantify the same failure modes using post hoc measurements over execution traces; they are not part of benchmark scoring.

E.1 Trace Artifacts

Each completed run stores the final structured diagnosis, run statistics, and a chronological trajectory:

```
run-XX/
  diagnosis.json         # final structured diagnosis emitted by the harness
  diagnosis_scores.json  # overlap scores against human labels
  session_stats.json     # cost, token, timing, and call-count summary
  trajectory.json        # model messages plus tool calls and tool outputs
```

The run directories of all 6,120 scored attempts are released at https://huggingface.co/datasets/OrderLab/distdebug-bench-traces. `paper/reproduce.py` in https://github.com/OrderLab/distdebug-bench rescores them with the released evaluator and rebuilds Table 1, Figure 2, and Figure 3.

The trace analyses below read `trajectory.json` after evaluation has finished. They do not change scoring. We use the traces to inspect which files, logs, and symbols each run searched before producing its final diagnosis.

E.2 Late-Discovery Proxy

For runs with parseable execution traces, we define *late recovery* as a run where none of the final diagnosis files appears among the first two inspected source files. This is a conservative proxy for recovering from an early search miss. It does not prove that the agent reasoned by backtracking, but it identifies cases where the final answer was not already present in the first local source-code scope.

Among the 80 successful runs in this trace subset, only 9 are late recoveries (11.25%). Current harnesses are therefore strongly shaped by early search scope: they can follow evidence inside an initial region, but successful diagnoses rarely arise after the correct file was absent from the first two inspected source files.

Table 13: Late-recovery proxy over parseable Mini-SWE-Agent and OpenCode traces.

Harness	Runs with early scope	Late recoveries	Rate
Mini-SWE-Agent	21	7	33.3%
OpenCode	103	19	18.4%
Combined	124	26	21.0%

Table 14: Early tool-use divergence across three Mini-SWE-Agent attempts per bug.

Model	Bugs with different first-five commands	Bugs with different first source file
GPT-5.4	120/120	29/120
GPT-5.3-Codex	120/120	27/120
GPT-5.2-Codex	120/120	32/120

E.3 Tool-Call Nondeterminism Scan

Tool-call nondeterminism is a concrete driver of diagnosis nondeterminism. We ran a lightweight scan over Mini-SWE-Agent trajectories on the 120-bug main evaluation set. For each bug and model, we compared the first five shell commands across the three completed runs. We also extracted the first source file path mentioned in a file-reading or code-search command. This measures early search divergence, not correctness.

The first-five-command measure is intentionally coarse and should not be read as an error rate: harmless differences such as command flags or ordering are enough to count as divergence. The first-source-file measure is more diagnosis-relevant. It shows that a non-trivial fraction of repeated attempts begin reading different code regions. On weak-evidence bugs, those early differences can determine which local explanation the run later tries to justify. This motivates adaptive tooling that augments the agent's context automatically when the early search scope is incomplete, rather than relying on a single sequence of manual shell commands to discover the right evidence.

E.4 Representative Trace Excerpts

Table 15 shows compact excerpts from raw trajectories. The table paraphrases long shell commands and tool outputs. Each row compares two attempts from the same harness and model on the same bug; it is not a cross-model comparison. The point of the excerpts is to make the branch point visible: small changes in search terms, code windows, and follow-up files can lock the same model into different local explanations.

These examples show how tool-call nondeterminism becomes diagnosis nondeterminism. In ZooKeeper-3769, changing the log query from election-state terms to exception and receiver-thread terms changes the next inspected file and the final causal story. In ZooKeeper-2845, both attempts

Table 15: Representative same-model trajectory contrasts behind the failure-mode analysis.

Case	Lower-scoring tool path	Higher-scoring tool path
ZooKeeper-3769, Mini-SWE HY3	Greps the node logs for election states and notifications, then follows the connection-management branch into `QuorumCnxManager` and `haveDelivered`. The final diagnosis blames connection delivery and has 0 block coverage.	Greps the same logs for `ERROR`, `Exception`, and `WorkerReceiver`, then opens the receive loop in `FastLeaderElection.java`. The final diagnosis covers `WorkerReceiver.run`.
ZooKeeper-2845, Mini-SWE GPT-5.3-Codex	Searches `retain`, `ZKDatabase`, `QuorumPeer`, `Learner`, and `DIFF/TRUNC`. The run inspects database loading and sync code, then diagnoses quorum synchronization; it misses `ZooKeeperServer.shutdown`.	Starts from similar retain-database searches, but adds `clear/shutdown` queries and opens the `ZooKeeperServer` shutdown window. The final diagnosis covers the lifecycle block that retains stale database state.

Table 16: Best-attempt headroom across the three selected Mini-SWE-Agent attempts per bug on the 120-bug common cohort.

Model	Mean block	Best-attempt block	Gain	Bugs with better attempt
GPT-6 Astra	67.7%	72.1%	4.4	17/120
Claude Opus 5.5	66.4%	72.8%	6.3	19/120
GPT-5.5	65.1%	71.7%	6.6	22/120
GPT-5.4	63.1%	69.5%	6.3	23/120
GPT-5.3-Codex	59.7%	65.8%	6.1	23/120
GPT-5.2-Codex	56.4%	61.0%	4.6	20/120

start with the same retain-database vocabulary, but one keeps deepening the sync hypothesis while the other asks which shutdown path preserves the stale state.

E.5 Attempt-Level Headroom

The repeated-run setup also gives a simple upper-bound check on whether agents could do better with more reliable execution and self-correction. For each bug, we compare the reported mean block coverage across three attempts with an oracle that takes the best attempt for that same bug. This is not a deployable metric, but it shows whether failures are entirely fixed by model capability or whether execution consistency leaves measurable headroom.

For GPT-5.5, 14 bugs are fully covered in at least one attempt but not across all three attempts. This means some failures are not caused by an absolute inability to identify the right source region. The same model and harness can sometimes find the right blocks, but the execution path is not reliable enough to do so consistently.

E.6 ZooKeeper-2845 Trace Pattern

ZooKeeper-2845 is an example of early scope lock-in. The report describes retained database state during leader election, a transaction log ahead of a snapshot, and an inconsistent quorum after restart. This language makes quorum and sync code a plausible first target. Several traces therefore begin by searching or reading under `src/java/main/org/apache/zookeeper/server/quorum`.

The gold block, however, is in:

```
src/java/main/org/apache/zookeeper/server/ZooKeeperServer.java
function: ZooKeeperServer.shutdown
lines: 555--558
```

Runs that stay inside quorum code can build a coherent explanation around leader election or synchronization while missing the lifecycle block that controls database retention. The agent is not failing to find any relevant code; it is stopping at the most obvious symptom-side location before looking for the earlier state transition that made the symptom possible. When the report does not directly name the exact causal code, the initial issue narrative can steer the agent toward a nearby but incomplete diagnosis.

E.7 Implication for Future Harnesses

These observations suggest three concrete harness requirements. First, the harness should provide adaptive context construction: when an early scope is under-supported, tooling should surface adjacent files, call paths, dataflow edges, related symbols, and alternative subsystems. Second, the harness should preserve hypotheses as separate state, so evidence collected for one branch does not become the default context for all later reasoning. Third, the harness should add correctness checks that ask whether the proposed source region explains the whole causal chain, rather than merely matching a symptom, stack frame, or local code path.

Table 17: Exploratory subset experiments. Each model is compared against Mini-SWE GPT-5.2-Codex on exactly the subset of bugs completed by that model. Cov., prec., and std. abbreviate coverage, precision, and standard deviation; all are percentages. Mini-SWE denotes Mini-SWE-Agent, and subset reports completed bugs / represented systems. Parenthesized values are paired deltas from GPT-5.2-Codex on the same subset. Flaky bugs are counts, and flaky delta is the model count minus the GPT-5.2-Codex count. Token usage is average tokens per run, with multiplier relative to GPT-5.2 on the same bugs.

Model	Harness	Subset	Block cov.	Block prec.	Mention cov.	Cov. std.	Flaky bugs	Flaky delta	Tokens/run
DeepSeek V4 Pro	Mini-SWE	67/10	62.5 (+5.0)	39.6 (-3.8)	72.3 (+11.6)	8.3 (+4.3)	16	+7	1.4M (2.5×)
Qwen3-Coder-Next	Mini-SWE	55/8	58.7 (-0.1)	45.9 (+1.9)	65.9 (+4.1)	16.7 (+12.4)	24	+17	4.4M (7.7×)
Qwen3.6 Max	Mini-SWE	35/7	61.1 (+5.9)	47.7 (+9.9)	71.9 (+13.5)	14.5 (+9.9)	15	+10	1.1M (1.9×)
Qwen3.6 Max	OpenCode	63/10	53.2 (-4.3)	46.0 (+3.7)	61.8 (+2.6)	8.2 (+3.5)	15	+6	0.6M (0.9×)

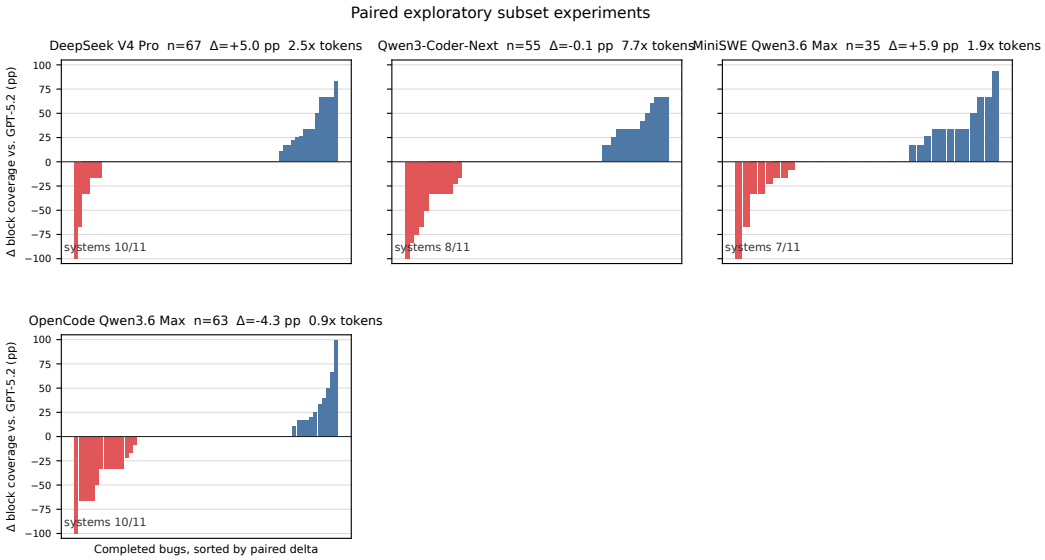


Figure 8: Paired per-bug block-coverage deltas for exploratory subset experiments. Each bar is one bug completed by the model, compared against Mini-SWE GPT-5.2-Codex on the same bug. Positive bars mean the exploratory model outperformed the baseline on that bug. These runs are not in the main leaderboard because the completed subsets are non-uniform.

F Exploratory Subset Experiments

We also ran exploratory experiments with DeepSeek and Qwen models under Mini-SWE-Agent and OpenCode. These runs are useful for model-selection context but are not leaderboard-comparable: each model completed a different subset of bugs. To make the comparison fairer, we compare each model only against Mini-SWE GPT-5.2-Codex on the exact same completed bug subset.

Table 17 shows that several exploratory models match or exceed the GPT-5.2-Codex baseline on their own completed subsets, but often with much higher token usage. Figure 8 shows the paired per-bug deltas, while Figure 9 shows the same paired comparison broken down by evidence category. Finally, Figure 10 compares models on common shared subsets: the 35 bugs completed by all partial exploratory rows, and a 55-bug set after removing the most constraining Mini-SWE Qwen3.6 Max run. These results support the main-paper decision to use a common 120-bug cohort with three valid scored attempts per configuration in the primary leaderboard while treating non-uniform subset experiments as exploratory evidence rather than primary benchmark results.

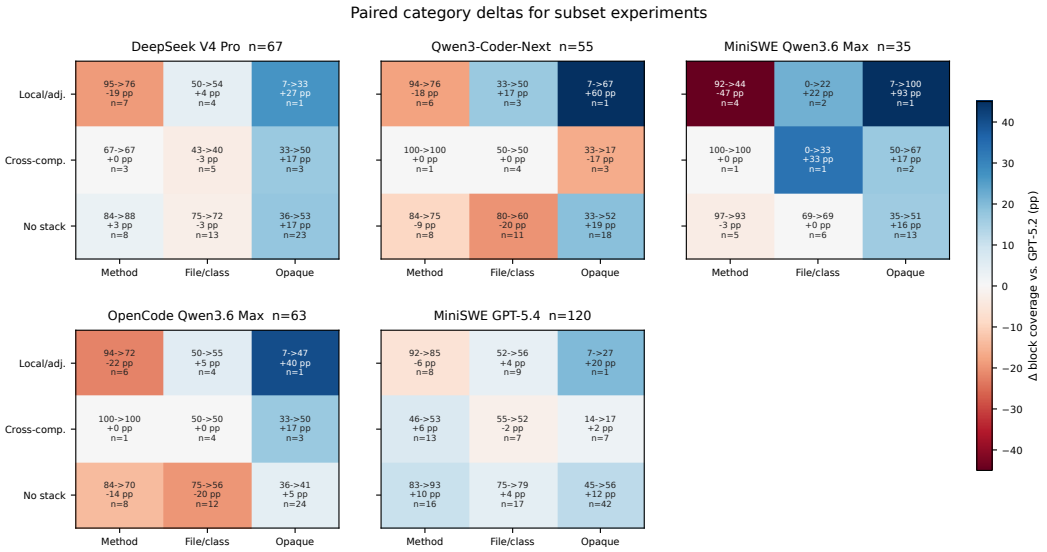


Figure 9: Paired block-coverage deltas by evidence category for subset experiments. Each cell compares a model with Mini-SWE GPT-5.2-Codex on the same completed bug subset and category. The GPT-5.4 panel uses the 120-bug shared evaluation set; the other panels are exploratory subset experiments. Cell text shows absolute GPT-5.2→model block coverage, the paired delta in percentage points, and the number of bugs. Counts are small in some cells, so the plot is descriptive rather than a leaderboard comparison.

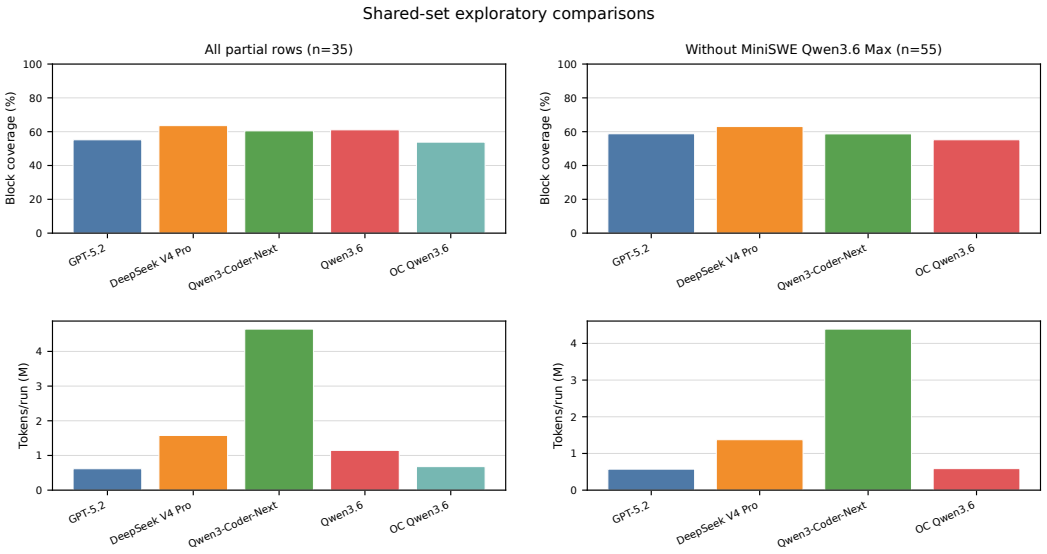


Figure 10: Shared-set exploratory comparisons. The left column uses the 35 bugs completed by all partial exploratory rows and Mini-SWE GPT-5.2-Codex. The right column removes the most constraining Mini-SWE Qwen3.6 Max run, yielding 55 shared bugs. The bottom row shows that several newer models require substantially more tokens even when evaluated on the same bug set.

G Evaluator Validity and Contamination Analyses

These studies test whether the localization metrics capture useful diagnosis information and whether public-issue exposure can explain the results.

G.1 Evaluator Validation

Score-blinded audit. We audit 20 GPT-5.4 diagnoses selected for zero block coverage, with the scores hidden during qualitative assessment. The assessment distinguishes whether a diagnosis reconstructs the failure mechanism from whether it places responsibility at the annotated code location. Table 18 gives the complete outcome breakdown. Four diagnoses reconstruct the causal chain but identify a different plausible intervention point. For example, one places responsibility for a nil-pointer failure in a specific `Op.check` implementation, while the accepted localization is in the broader `processRequest` path. The remaining 16 diagnoses have an incorrect hypothesis or an incomplete causal explanation.

Table 18: Score-blinded audit of 20 zero-block-coverage GPT-5.4 diagnoses.

Outcome	Diagnoses
Complete causal chain, different plausible intervention point	4
Incorrect hypothesis	9
Missing the decisive implementation link	2
Superficial or incomplete causal account	5
Total	20

Plausible alternative localizations therefore do not explain most zero-score outputs: 16/20 are incorrect or causally incomplete. Block coverage measures recovery of the annotated causal implementation rather than every plausible intervention or repair point, and the four alternative diagnoses show where this distinction matters. Because the audit samples only zero-scoring outputs, these proportions describe the sample rather than a benchmark-wide false-negative rate.

Benchmark-wide false-negative estimate. We estimate that rate from a stratified random sample of 100 of the 3,293 attempts with incomplete block coverage across all 17 configurations: 69 with zero and 31 with partial coverage, in proportion to the two strata. The sample contains 193 missed gold blocks. Two AI reviewers judge each missed block independently: Claude Opus 5.5 and GPT-6 Astra (run through Claude Code and Codex CLI), both at high reasoning effort, with read-only access to the pre-fix source and no network access. Each receives the issue report, the gold blocks, the historical fix as ground truth, and the diagnosis, without its score or the model that produced it. A missed block is a false negative when the diagnosis names the same code at shifted coordinates, or a different location that is itself the defect: changing it alone would prevent the reported failure, and it is not a caller, a symptom site, or a defensive check. We count a false negative only when both reviewers agree.

The reviewers agree on 85% of missed blocks (Cohen's $\kappa = 0.47$). Weighting each stratum by its number of missed blocks, 8.8% of missed gold blocks are false negatives, and counting them would raise block coverage by 5.0 points (Table 19). Of the 17 false-negative blocks, ten name the gold code at different coordinates, mostly because the gold block was annotated against a different revision than the task snapshot; we will correct these labels in the next release. The other seven are alternative locations. Either reviewer alone yields a higher rate, so requiring agreement gives the conservative estimate.

Diagnosis-assisted repair: design. We select 20 bugs from the GPT-5.4 diagnosis evaluation: a ten-bug high-coverage set with an available 100%-coverage diagnosis and a ten-bug low-coverage set with an available 0%-coverage diagnosis. Six sampled bugs have both high- and low-coverage diagnoses from the same model and harness. These mixed cases support a within-bug comparison with three conditions: no supplied diagnosis, a low-coverage diagnosis, and a high-coverage diagnosis. For the other cases, we compare no diagnosis with the available diagnosis condition. Each condition has three repair attempts per bug. In the six-bug comparison, the fixing model, repository, harness, tools, and budget are held fixed; the supplied diagnosis changes.

Table 19: Benchmark-wide evaluator false negatives, estimated from 100 sampled attempts with incomplete block coverage (193 missed gold blocks). Brackets are 95% stratified bootstrap intervals; the first row is the reported estimate.

Counting rule	False negatives among missed blocks	Block coverage gain (points)
Both reviewers agree	8.8% [4.3, 14.3]	5.0 [2.4, 8.0]
Claude Opus 5.5 alone	12.4% [7.1, 18.7]	7.2 [4.1, 10.5]
GPT-6 Astra alone	19.7% [12.7, 27.7]	9.8 [6.2, 13.7]

One author inspects each generated patch against the original issue resolution and PR. *Gold-file touch* indicates that the patch modifies a file where the reference repair should land; *gold-block touch* indicates that it modifies the annotated causal region. *Patch produced* reports whether a patch was generated.

Table 20: Diagnosis-assisted repair on the two ten-bug sets (30 attempts per row). Patch production and touch rates are percentages; cost is mean API cost per repair attempt in USD.

Set	Diagnosis	Patch	File touch	Block touch	Calls/run	Cost
High	None	100.0	86.7	56.7	34.6	$1.44
High	High coverage	100.0	90.0	60.0	30.1	$1.02
Low	None	93.3	26.7	6.7	40.4	$2.00
Low	Low coverage	100.0	20.0	0.0	40.6	$1.74

Table 21: Within-bug repair comparison on six mixed cases, with three attempts per condition per bug. Touch rates are percentages with raw counts.

Diagnosis	Attempts	File touch	Block touch	Calls/run	Cost
None	18	44.4 (8/18)	11.1 (2/18)	40.9	$2.02
Low coverage	18	33.3 (6/18)	0.0 (0/18)	37.9	$1.54
High coverage	18	61.1 (11/18)	50.0 (9/18)	31.6	$1.34

Repair results. On the six mixed cases, replacing a low-coverage diagnosis with a high-coverage one raises gold-file touch by 27.8 percentage points and gold-block touch by 50.0 points (Table 21). Relative to no diagnosis, the high-coverage diagnosis raises gold-block touch by 38.9 points while reducing mean model calls by 22.7% and API cost by 33.7%. The ten-bug sets show a similar pattern (Table 20). On the high-coverage set, the unaided agent already touches a gold block in 56.7% of attempts, so the diagnosis mainly reduces search effort: gold-block touch rises to 60.0%, while cost falls from $1.44 to $1.02. On the low-coverage set, no diagnosis is better than a bad one: gold-block touch falls from 6.7% to 0%.

This is a small pilot rather than a precise estimate of end-to-end patch correctness. Nevertheless, the controlled within-bug comparison provides evidence that diagnoses with higher block coverage guide downstream repair toward the causal code more reliably and efficiently.

G.2 Contamination Analyses

Direct answer retrieval and the leakage guard. Our reward-hacking check targets a concrete shortcut: retrieving the historical fix instead of diagnosing the failure from the supplied incident evidence and pre-fix code. Early experiments ran without the leakage guard. Subsequent trace inspection revealed agents searching for and retrieving future fixing commits. Workspace paths also exposed the original issue ID, giving agents a lookup key. We therefore removed commits after the task snapshot and anonymized workspace directory names.

The guard scans visible reasoning, tool inputs, and tool outputs for the original issue ID that should have been hidden from the agent. Matches include the full issue key or its numeric component, such as `git log | grep ZooKeeper-2845` and `git log | grep 2845`. We apply this check to recorded trajectories, including the early experiments, and discard flagged attempts, rerunning until

every bug has three clean attempts. Residual hits also prompt an audit of the prepared workspace to remove exposed identifiers or fix information. Table 22 reports affected trajectories separately from matching events in each channel; a single trajectory may contain many matches.

Table 22: Leakage-guard results before and after removing future commits. The reporting denominator is 360 attempts (120 bugs $\times$ 3 attempts). Parentheses give the percentage flagged; the last three columns count matching events.

Phase	Model	Flagged (%)	Reasoning	Tool output	Tool input
Before	GPT-5.4	102 (28.3)	0	1,007	16
After	GPT-5.4	4 (1.1)	0	22	0
After	GPT-5.5	4 (1.1)	0	22	2
After	GPT-6 Astra	3 (0.8)	0	10	2
After	Claude Opus 5.5	0 (0.0)	0	0	0
Before	GPT-5.3-Codex	8 (2.2)	0	88	0

Removing future commits substantially reduces detected leakage: GPT-5.4's flagged attempts fall from 102/360 (28.3%) to 4/360 (1.1%). The remaining hits show why trace checks are still needed after workspace cleanup.

Implicit memorization. Identifier matching detects explicit traces of answer retrieval, but a model could recall a bug without mentioning its ID. Long-public bugs can still be difficult: in ZooKeeper-2845, failed runs focus on the quorum subsystem suggested by the report and miss the causal code in `ZooKeeperServer.java` (Appendix E). We examine this question more broadly through issue-age correlations and closed-book diagnosis probes.

Issue age and public discussion. Across all 120 issues, GPT-5.4 block coverage has Pearson $r = -0.002$ and Spearman $\rho = -0.032$ with issue age, and $r = -0.052$ and $\rho = -0.048$ with the current issue-comment count. Across the 13 Mini-SWE configurations, issue-age correlations range from -0.081 to 0.087 (Pearson) and from -0.125 to 0.058 (Spearman; Table 23). Older or more discussed issues are not systematically easier to diagnose. GPT-5.4 coverage is negatively associated with time to resolution ($r = -0.269$ after removing per-system means and controlling for issue age), which is more consistent with difficult bugs taking longer to resolve than with greater public exposure making them easier.

Table 23: Correlations between issue age and bug-level block coverage for the 13 Mini-SWE configurations. r^2 values are percentages.

Model	Pearson r	Spearman ρ	Pearson r^2 (%)
GPT-6 Astra	0.051	0.026	0.26
Claude Opus 5.5	0.034	0.006	0.12
GPT-5.5	−0.001	−0.019	0.00
GPT-5.4	−0.002	−0.032	0.00
GPT-5.3-Codex	−0.006	−0.045	0.00
MiMo v2.5 Pro	0.087	0.058	0.75
Kimi K2.6	0.000	−0.065	0.00
MiMo v2.5	−0.010	−0.045	0.01
DeepSeek V4 Flash	−0.057	−0.111	0.32
GPT-5.2-Codex	−0.027	−0.079	0.08
HY3	−0.060	−0.103	0.36
Gemini 3.5 Flash	0.057	0.020	0.33
o4-mini	−0.081	−0.125	0.65

Closed-book probes: design. We evaluate GPT-5.4 without repository access, tools, Git history, or internet. Of the 120 benchmark reports, we exclude 54 that directly contain an exact gold filename or function name, leaving 66 bugs. Three attempts are averaged within each bug before averaging across bugs. We compare three prompt variants: the original report with its issue ID (identifiable), the report with its issue ID redacted (anonymous), and only the project and issue ID without the report (ID only). The repository-assisted reference in Table 24 uses the same 66 bugs. Without source access,

the model does not emit source line ranges, so closed-book line and block coverage are zero; we compare variants on file and function coverage.

Table 24: GPT-5.4 contamination probes on 66 bugs (198 attempts per closed-book variant). All entries are mean coverage percentages.

Condition	File	Function	Line	Block
Repository-assisted	75.2	45.5	45.9	56.9
Closed book: identifiable report	43.9	17.3	0.0	0.0
Closed book: anonymous report	43.2	21.4	0.0	0.0
Closed book: issue ID only	0.0	0.0	0.0	0.0

Closed-book results and interpretation. Repository-assisted coverage exceeds every closed-book variant. Providing the issue ID does not improve function coverage in these probes: identifiable reports yield 17.3%, compared with 21.4% for anonymous reports. ID-only prompts yield zero coverage at all four granularities. A separate report-to-ID probe recovers 0/360 exact target identifiers. In contrast, repository-assisted function coverage reaches 45.5% and block coverage reaches 56.9%. The gap supports codebase exploration as an important part of diagnosis: report text and issue identity alone do not recover the same causal-code detail.

Nonzero file/function coverage without repository access can reflect knowledge of project structure or clues in reports, including class and subsystem names that remain after exact gold filenames and function names are removed. For example, references to `LocalHealthMonitor`, `InitProducerId`, or `QuorumCnxManager.Listener` constrain plausible code regions.

Together, the trace audit identifies and removes direct answer-retrieval shortcuts, while the age analysis and closed-book probes test whether public issue exposure readily translates into accurate diagnosis. The results support the importance of exploring the repository to identify causal code. They do not rule out all pretraining exposure or implicit memorization.

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   Justification: The benchmark, evaluator, and agent runner are released at https://github.com/OrderLab/distdebug-bench, and the traces of all 6,120 scored attempts at https://huggingface.co/datasets/OrderLab/distdebug-bench-traces, with a script that rebuilds Table 1 and Figures 2 and 3 from them; Section 3 describes the released data format and Section 4 describes the evaluation protocol.

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