

Rethinking the Value of Agent-Generated Tests for LLM-Based Software Engineering Agents

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Large Language Model (LLM) code agents increasingly resolve repository-level issues by iteratively editing code, invoking tools, and validating candidate patches. In these workflows, agents often write tests on the fly, a paradigm adopted by many high-ranking agents on the SWE-bench leaderboard. However, we observe that GPT-5.2, which writes almost no new tests, can even achieve performance comparable to top-ranking agents. This raises the critical question: whether such tests meaningfully improve issue resolution or merely mimic human testing practices while consuming a substantial interaction budget.

To reveal the impact of agent-written tests, we present an empirical study that analyzes agent trajectories across six state-of-the-art LLMs on SWE-bench Verified. Our results show that while test writing is commonly adopted, but resolved and unresolved tasks within the same model exhibit similar test-writing frequencies. Furthermore, these tests typically serve as observational feedback channels, where agents prefer value-revealing print statements significantly more than formal assertion-based checks. Based on these insights, we perform a controlled experiment by revising the prompts of four agents to either increase or reduce test writing. The results suggest that changes in the volume of agent-written tests do not significantly change final outcomes. Taken together, our study reveals that current test-writing practices may provide marginal utility in autonomous software engineering tasks.

Additional Key Words and Phrases: Large Language Model, Agent-Written Tests, Agent Trajectory Analysis, Software Development Agent

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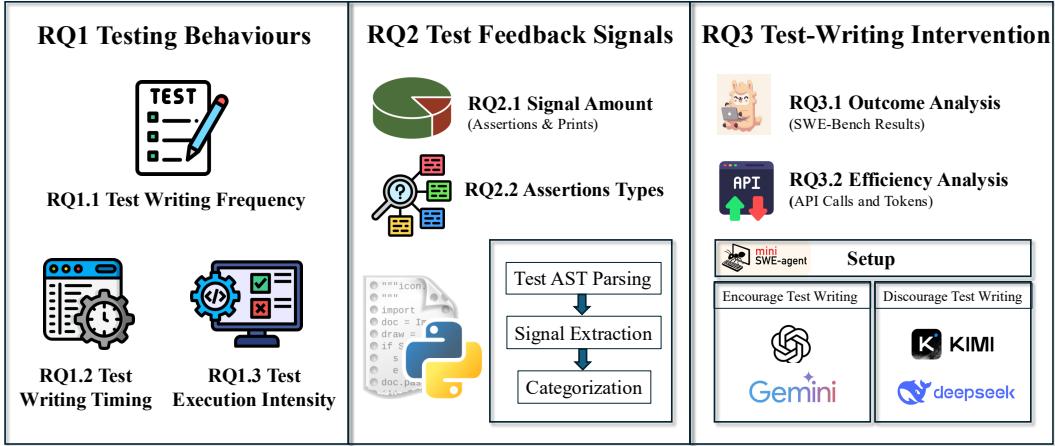


Fig. 1. Overview of our study design. RQ1 profiles emergent testing behaviours (test writing frequency, timing, and execution analysis). RQ2 characterizes the feedback signals encoded in agent-written tests (assertions vs. value-revealing prints) and the types of assertions. RQ3 applies prompt interventions to encourage or discourage writing tests, and measures both outcome impact and efficiency impact.

1 Introduction

Code agents are increasingly used as effective paradigm for resolving software issues, where Large Language Models (LLMs) [5, 10, 32] are integrated into scaffold of tools and interaction protocols to edit real repositories, invoke tools, and attempt to resolve issues end-to-end [11, 13, 16, 33, 34, 44, 45, 47]. In this paper, a *code agent* denotes an LLM coupled with external tools and an iterative action–observation loop, and the *scaffold* refers to the surrounding tool interface and interaction protocol that specifies the agent’s allowed actions and feedback. Among the diverse skills required by code agents, testing plays a critical role, which expose regressions, validate hypotheses, and provide a feedback loop during patch development [23, 29, 45, 54].

When operating on repository-level tasks, agents typically use tests as a primary validation interface, which come from two main sources. The first is the repository’s existing, human-written test suite, which reflects developer intent and established project conventions [6, 18]. The second is *agent-written tests*—new test artifacts written by the agent during problem solving that were not present in the original codebase. In contrast to curated human-written tests, agent-written tests are written *on the fly* during issue resolution, and their reliability depends on the model’s understanding of the specification, domain knowledge, and the semantics of the target codebase. Agent-written tests can be beneficial by surfacing edge cases and providing actionable feedback for fault localization and patch refinement. However, they can also be harmful if they embed incorrect assumptions or oracles, diverting effort toward satisfying the test rather than resolving the target issue. Moreover, test generation and execution introduce non-trivial overhead—consuming API calls and tokens and increasing context footprint—which can reduce the remaining budget available for core debugging and patching [20]. When the resulting signals are low-value, this overhead may dilute the agent’s focus and become net detrimental.

To better understand agent-written tests, we conduct a preliminary quantitative analysis of agent trajectories on SWE-bench Verified [31] using MINI-SWE-AGENT [40], where testing is optional and not enforced by any hard-coded procedure. We find that agent-written testing is prevalent for several strong models. For example, *claude-opus-4.5* (ranked #1 in this setting, 74.4% resolution)

generates at least one new test artifact in about 83% of tasks. Surprisingly, we observe a pronounced contrast for *gpt-5.2*: it achieves a comparable resolution rate (71.8%), only 2.6 percentage points below *claude-opus-4.5*, while generating near-zero new tests (only in 0.6% of tasks). This interesting observation inspires us to a core question: *Do agent-written tests truly facilitate task resolution, or do models merely mimic a learned software development practice while the resulting tests contribute little to the final patch?* If the latter holds, then the widespread creation and execution of agent-written tests may represent a significant resource waste, consuming significant interaction budget without meaningful gains in task success. Therefore, we argue that a systematic empirical study is needed to understand the role of agent-written tests in resolving software issues.

Prior work mostly evaluates and benchmarks LLM-generated tests under predefined testing objectives and fixed quality metrics (e.g., unit tests, assertions, or issue-reproducing tests), typically with respect to a fixed target program or snapshot of code under test [24, 30, 38, 42, 43, 51, 53]. However, in complex real-world GitHub issue resolution [18, 19], the codebase and candidate patches evolve over time, and test writing and usage arise dynamically as self-directed behaviors rather than pre-specified evaluation objectives. Yet the intrinsic tendency of high-autonomy agents to write and use tests during such issue resolution—and the impact of such agent-written tests on resolution outcomes—has not been systematically studied. This motivates a closer empirical investigation of agent-written tests, guided by the following three research questions.

Research questions and overview. Guided by the above gap, we conduct a systematic empirical study of agent-written tests in GitHub issue resolution. Figure 1 summarizes our study design, which decomposes the problem into three complementary research questions. **RQ1** characterizes the agents’ testing behaviors when test writing is required by the prompt: whether agents write tests, when they introduce them, and how intensively they execute them. **RQ2** shifts from behaviors to *test content*, investigating what feedback signals agent-written tests actually emit at execution time (assertions vs. value-revealing prints) and what types of assertions agents use. **RQ3** evaluates *impact*: by revising prompts to encourage or discourage writing new tests, we measure whether changing test-writing behavior meaningfully alters task resolution outcomes and what efficiency costs (API calls and tokens) these changes incur.

Summary of findings. Across models, agent-written testing is best understood as a *model-dependent process style* rather than a dependable driver of success. First, **RQ1** shows that test writing is a widespread behavior among the studied models, while resolved and unresolved trajectories within a model exhibit broadly similar test-writing rates. When agent-written tests exist, unsuccessful trajectories tend to spread test writing slightly more over the run and execute tests more frequently. Second, **RQ2** reveals that agent-written tests primarily function as an *observational* feedback channel: value-revealing prints consistently dominate assert-based checks, and assertion forms concentrate on local-property and exact-value checks, with relational/range-style constraints remaining rare. Third, **RQ3** provides controlled evidence that large shifts in whether agents write tests translate into only small shifts in task outcomes for most tasks, while efficiency effects can be substantial—inducing tests can increase interaction overhead without improving resolution, whereas suppressing tests can materially reduce API calls and token usage with only modest success losses.

Contributions. The contribution of this work can be summarized as:

- **A behavioral analysis of agent-written tests from code agents.** We characterize the agent-written testing behaviors of base LLM agents, including *whether* they create new test artifacts, *when* such test creation occurs within a trajectory, and *how* these tests are executed. Our results show that test writing and execution intensity are largely *model-dependent process styles* and only

weakly align with task success (e.g., some high-performing models resolve many tasks while writing almost no tests).

- **A feedback-signal analysis of agent-written tests with a four-category assertion categorization.** We separate verification-oriented assertions from observational outputs and introduce a rule-based AST classifier that maps assertions into four assertion categories. We find that tests largely serve an *observational* role: value-revealing prints consistently outnumber assertions; assertion usage is dominated by local-property and exact-value checks, whereas relational/range-style constraints are uncommon.
- **A causal evaluation of agent-written tests on task resolution.** Through controlled prompt interventions that either encourage or suppress writing new test files, we quantify the causal effects of agent-written tests on task success and interaction efficiency. We demonstrate that large flips in test-writing status translate into only small changes in resolution outcomes for most tasks, whereas efficiency effects can be substantial—inducing tests can increase token and interaction overhead without improving success, while suppressing tests yields large cost reductions with only modest success drops.

Paper organization. Section 2 describes our study setting, data collection, and measurement procedures. Section 3 characterizes emergent agent-written testing behaviors as process events—whether agents write tests, when they appear, and how intensively they are executed (RQ1). Section 4 shifts to test content by analyzing what feedback signals agent-written tests produce at execution time (assertions vs. value-revealing prints) and what types of assertions they use (RQ2). Section 5 evaluates the outcome and efficiency impacts of agent-written tests via prompt interventions that induce or suppress test writing (RQ3). Section 6 discusses implications, limitations, and future directions. Section 7 reviews related work, and Section 8 concludes the paper.

2 Methodology

In this section, we introduce the methodology for this study, including the benchmark, studied agent and LLMs, the extraction of agent-written tests, and implementation details. Our study is guided by three research questions:

- **RQ1:** What Testing Behaviors Emerge Under a Light Agent Scaffold?
- **RQ2:** What Feedback Signals Do Agent-Written Tests Provide, and What Types of Assertions Do They Use?
- **RQ3:** Do Agent-Written Tests Truly Affect Task Resolution?

2.1 Benchmark

We use SWE-bench Verified as a standardized source of real-world software-engineering trajectories in which agents autonomously write and use tests when solving a GitHub issue [18]. SWE-bench Verified is a filtered subset of SWE-bench that contains 500 instances. Each benchmark instance provides a GitHub issue, a fixed repository snapshot, and the official evaluation harness. We analyze agent-written test artifacts within the observed trajectories [31].

2.2 Agent and its LLMs

While many recent LLM-based agents incorporate *curated testing components*, such as specialized validation modules, dedicated test-planning stages, or multi-agent coordination [8, 23, 36, 54], these frameworks can confound a model’s *intrinsic* tendencies with scaffold-induced constraints. To better isolate base-model behavior, we adopt `mini-SWE-agent` [40, 41]. It provides a lightweight agent work loop restricted to a standard bash interface: the agent interacts with the repository solely through the bash tool, executing commands in a bash shell (e.g., running `python`) and using

standard command-line utilities to inspect and modify files. The model can create executable Python test files on the fly and run them via bash as part of its workflow. Crucially, `mini-SWE-agent` does *not* provide additional testing-specific functions or dedicated testing tools (e.g., test planners or structured testing modules), so the testing decisions (whether, when, and how) are left to the model. Accordingly, any observed behaviors (e.g., creating or running test artifacts) can be interpreted as model-native ones.

We select a diverse set of strong LLMs to capture heterogeneous agent-written testing behaviors under `mini-SWE-agent`. Specifically, we reference the SWE-bench *Bash Only* leaderboard¹ as of the time of writing (2025-12-11) and identify the top-six *model families* (rather than top entries, since a family may appear with multiple variants on the leaderboard). For each family, we use its highest-ranked model as the representative: *claude-opus-4.5* [1] (74.4%), *gpt-3.5-turbo-0613* [14] (74.2%), *gpt-5.2* [32] (71.8%), *kimi-k2-thinking* [28] (63.4%), *minimax-m2* [26] (61.0%), and *deepseek-v3.2-reasoner* [9] (60.0%).

2.3 Data Extraction of Agent-Written Tests

In our study, agent-written tests are test files that an agent writes using the bash tool during task resolution. We extract agent-written tests from task trajectories, which are time-ordered interaction logs recorded during issue resolution that include the agent's intermediate reasoning, concrete actions (e.g., bash commands), and the resulting observations. To find test files written during a trajectory, we scan the logged bash actions for file-writing operations, most commonly here-doc writes such as `cat <'EOF' > path/to/file.py ...EOF`. We then keep only files whose paths match common Python test naming patterns, including filenames that start with `test_` or end with `_test.py` or `tests.py`.

2.4 Implementation Details

We run all experiments using the official `MINI-SWE-AGENT` codebase. All tasks are executed on a Linux server (Ubuntu 22.04.5) with an AMD Ryzen Threadripper PRO 7975WX CPU (32 cores / 64 threads), 251 GiB RAM. For model inference, we access the four LLMs through a combination of official provider APIs and the OpenRouter API. For evaluation, we use the official SWE-bench `sb-cli` tool to score each submitted patch under the benchmark harness. Across all experiments reported in this paper, the total LLM API cost is approximately USD 1,600.

3 RQ1: What Testing Behaviors Emerge Under a Light Agent Scaffold?

Motivation. In a high-autonomy setting where testing is optional, agents may or may not write tests during issue resolution. RQ1 establishes a descriptive baseline of these emergent testing behaviors—what tests agents write, when they introduce them, and how intensively they run them. This baseline (i) clarifies what "testing" looks like in this setting and (ii) provides grounded behavioral variables for later research questions.

Experiment Design. RQ1 uses **resolved vs. unresolved trajectories** as a comparative lens to characterize systematic differences in test-related behaviors. We emphasize that these outcome-stratified comparisons are *not intended to establish causality* regarding task success; rather, they serve as a diagnostic tool to surface consistent differences in testing practices between successful and unsuccessful problem-solving processes. RQ1 reports descriptive summaries of three complementary aspects of test-oriented behavior:

- **Frequency** (RQ1.1): whether the agent writes tests, and how many.

¹<https://www.swebench.com/>

- **Timing** (RQ1.2): when test writing happens during issue resolution.
- **Execution** (RQ1.3): how intensively tests are run, and their outcomes.

3.1 RQ1.1 Frequency: Do Agents Write Test Artifacts?

Goal and measurements. We examine whether base LLMs write test artifacts under a light scaffold. For each task, we record (i) whether the agent writes at least one test artifact, and (ii) if so, how many distinct test artifacts it writes. We report results separately for resolved and unresolved tasks.

Table 1. Per-model test writing rate by execution outcome

Model	#Tasks	Resolved		Unresolved		#Tasks	All	
		Tasks w/ tests (count, %)	Mean #tests	#Tasks	Tasks w/ tests (count, %)	Mean #tests	Tasks w/ tests (count, %)	Mean #tests
<i>claude-opus-4-5</i>	372	314 (84.4%)	3.33	128	101 (78.9%)	4.12	500	415 (83.0%)
<i>gemini-3-pro-preview</i>	371	235 (63.3%)	2.02	129	73 (56.6%)	2.16	500	308 (61.6%)
<i>gpt-5.2</i>	359	3 (0.8%)	1.00	141	0 (0.0%)	—	500	3 (0.6%)
<i>kimi-k2-thinking</i>	317	309 (97.5%)	3.48	183	178 (97.3%)	3.83	500	487 (97.4%)
<i>minimax-m2</i>	305	302 (99.0%)	4.82	195	191 (97.9%)	5.76	500	493 (98.6%)
<i>deepseek-v3.2-reasoner</i>	300	277 (92.3%)	3.55	200	169 (84.5%)	4.08	500	446 (89.2%)

Notes. "Tasks w/ tests (count, %)" reports the number (and percentage) of tasks that write at least one test artifact within each outcome split.

"Mean #tests" reports the mean number of *distinct* test artifacts, computed only over tasks that write at least one test artifact.

Results. Table 1 shows that writing tests is common for most models, but not for *gpt-5.2*. Some models write tests in almost every task (e.g., *minimax-m2* and *kimi-k2-thinking*). In contrast, *gpt-5.2* almost never writes tests (3/500 tasks). Within the same model, resolved and unresolved tasks usually have similar test-writing rates. When tests are written, unresolved tasks often write as many or more distinct test artifacts than resolved tasks. This may reflect that harder tasks trigger more trial-and-error.

RQ1.1 Test Writing: Key Pattern

Test writing is widespread across models in our high-autonomy issue-resolution setting. Most models write tests in a majority of tasks (61.6–98.6%), and four of six do so in at least 83% of tasks; *gpt-5.2* is a clear outlier with near-zero test writing (0.6%).

3.2 RQ1.2 Timing: When Are Tests Written During the Run?

Goal and measurements. Beyond whether tests are written (RQ1.1), we examine *when* test writing happens during a task execution. Writing tests in a tight window may look like a short "checking phase", while writing tests throughout the task may look like iterative debugging. This subsection is descriptive and does not claim effectiveness. We analyze only tasks that write at least one test artifact, so the timing metrics are defined. Because *gpt-5.2* writes tests in only 3 tasks (RQ1.1), we omit its per-model timing summaries in RQ1.2 to avoid unstable estimates. We also exclude it from later analyses that require tasks with test writing. We use three normalized **positions** within the task: the **first** test-writing position, the **last** test-writing position, and their **span**:

$$t_{\text{first}} = \frac{\min(S_{\text{write}})}{N_{\text{steps}}}, \quad t_{\text{last}} = \frac{\max(S_{\text{write}})}{N_{\text{steps}}}$$

$$s_{\text{write}} = t_{\text{last}} - t_{\text{first}} = \frac{\max(S_{\text{write}}) - \min(S_{\text{write}})}{N_{\text{steps}}}$$

Here, S_{write} is the set of step indices where the agent writes test artifacts, and N_{steps} is the total number of interaction steps in the task. t_{first} and t_{last} are normalized positions in $[0, 1]$. Smaller values mean the agent writes tests earlier in the task; larger values mean later. The span $s_{\text{write}} \in [0, 1]$ measures how spread out test writing is across the task. Larger values mean more dispersed test writing; smaller values mean a more concentrated window.

Table 2. Per-model timing of test writing events

Model	Resolved				Unresolved			
	#Tasks w/ tests	First test-writing position	Last test-writing position	Test-writing span	#Tasks w/ tests	First test-writing position	Last test-writing position	Test-writing span
<i>claude-opus-4-5</i>	314	0.34	0.75	0.41	101	0.30	0.78	0.48
<i>gemini-3-pro-preview</i>	235	0.53	0.67	0.14	73	0.55	0.70	0.15
<i>kimi-k2-thinking</i>	309	0.40	0.82	0.42	178	0.40	0.82	0.42
<i>minimax-m2</i>	302	0.35	0.86	0.51	191	0.29	0.85	0.56
<i>deepseek-v3.2-reasoner</i>	277	0.43	0.80	0.37	169	0.40	0.80	0.40
All models	1440	0.40	0.78	0.38	712	0.37	0.80	0.43

Notes. Values are macro-over-tasks means. “First test-writing position” and “Last test-writing position” are the normalized positions of the first and last test-writing events relative to `total_steps`. “Test-writing span” is the normalized distance between the first and last test-writing positions.

Results. Table 2 summarizes test-writing *positions* for tasks that write tests. Across all models, the average first test-writing position is 0.40 for resolved tasks and 0.37 for unresolved tasks. The average last test-writing position is 0.78 (resolved) and 0.80 (unresolved). Models differ in *when* they start writing tests. For example, *gemini-3-pro-preview* starts later (0.53–0.55), while *minimax-m2* and *claude-opus-4-5* start earlier (0.29–0.35). Most models finish test writing late in the task (last position around 0.75–0.86). Models also differ in how spread out test writing is. *gemini-3-pro-preview* has a short span (0.14–0.15). *minimax-m2* has a wider span (0.51–0.56), and *claude-opus-4-5* is also relatively wide (0.41–0.48). *kimi-k2-thinking* is almost identical between resolved and unresolved tasks (0.40–0.82; span 0.42). Overall, unresolved tasks have a slightly larger average span than resolved tasks (0.43 vs. 0.38).

RQ1.2 Test-Writing Timing: Key Pattern

Test writing typically finishes late, but its start time and span are mainly model-dependent; unresolved tasks are only slightly more spread out (0.43 vs. 0.38).

3.3 RQ1.3 Execution: How Intensively Are Agent-Written Tests Executed, and With What Process Outcomes?

Goal and measurements. RQ1.3 describes how agents execute tests after they have written them. We measure (i) how often tests are executed, (ii) how often they are rerun relative to the number of written test artifacts, and (iii) how often executions fail at the process level. We treat an execution as failed if it ends with a non-zero return code (and successful otherwise). This captures execution friction during interaction with the environment, not patch correctness.

For each task t , let E_t be the number of test executions, A_t the number of agent-written test artifacts, and F_t the number of executions with non-zero return codes. We report three task-level metrics: **ExecCount** (E_t), test executions per task; **ExecPerTest** (E_t/A_t), executions per written test artifact (rerun intensity); and **FailRate** (F_t/E_t), the fraction of executions that fail. We report macro-over-tasks means for each metric.

Table 3. Task-level execution effort and process-level outcomes of agent-written tests

Model	Resolved				Unresolved				Mean FailRate (%)
	#Tasks w/ tests	Mean ExecCount	Mean ExecPerTest	Mean FailRate (%)	#Tasks w/ tests	Mean ExecCount	Mean ExecPerTest	Mean FailRate (%)	
<i>claude-opus-4-5</i>	314	4.87	1.50	11.97	101	6.27	1.68	11.14	
<i>gemini-3-pro-preview</i>	235	2.71	1.51	8.53	73	2.79	1.40	7.08	
<i>kimi-k2-thinking</i>	309	5.39	1.62	24.95	178	6.54	1.76	21.05	
<i>minimax-m2</i>	302	7.19	1.55	24.11	191	9.70	2.09	24.10	
<i>deepseek-v3.2-reasoner</i>	277	3.74	1.11	27.37	169	4.66	1.32	29.55	
All models	1440	4.89	1.46	19.68	712	6.52	1.70	21.05	

Notes. #Tasks: tasks with test writing. Mean ExecCount: test executions per task. Mean ExecPerTest: executions per agent-written test artifact. Mean FailRate: % executions with non-zero return codes (macro-over-tasks).

Results. Table 3 summarizes execution effort and process-level outcomes for tasks that write tests. Across all models, unresolved tasks execute tests more often than resolved tasks (Mean ExecCount: 6.52 vs. 4.89). They also rerun tests more per written test artifact (Mean ExecPerTest: 1.70 vs. 1.46). FailRate is slightly higher for unresolved tasks (21.05% vs. 19.68%). Models differ strongly in execution intensity. *gemini-3-pro-preview* runs tests the least (ExecCount \approx 2.7–2.8). *minimax-m2* runs tests the most, especially for unresolved tasks (ExecCount 9.70; ExecPerTest 2.09). FailRate also varies by model. *claude-opus-4-5* and *gemini-3-pro-preview* have lower FailRate (about 7–12%), while *deepseek-v3.2-reasoner*, *kimi-k2-thinking*, and *minimax-m2* are higher (about 21–30%).

RQ1.3 Test Execution: Key Pattern

For tasks that write tests, unresolved tasks run tests more often and rerun them more per written test artifact (ExecCount 6.52 vs. 4.89; ExecPerTest 1.70 vs. 1.46), while process-level execution failures vary mainly by model (FailRate ~7%–30%).

3.4 Summary of RQ1.

RQ1 provides a descriptive baseline of agent-written testing behaviors in a high-autonomy setting. Test writing is common for most models, but extremely rare for *gpt-5.2* (Table 1). For tasks that write tests, test writing usually ends late in the task, while the start position and the writing span vary mainly by model (Table 2). Unresolved tasks are slightly more spread out in test writing on average (span 0.43 vs. 0.38). Finally, among tasks that write tests, unresolved tasks execute tests more often and rerun them more per written test artifact (Table 3). Process-level execution failures also vary by model (about 7–30% FailRate). RQ1, however, only describes *when* and *how often* tests appear and run, not what feedback they provide. RQ2 therefore analyzes the feedback encoded in agent-written tests.

4 RQ2: What Feedback Signals Do Agent-Written Tests Provide, and What Types of Assertions Do They Use?

Motivation. In our high-autonomy setting where testing is optional, tests may play different roles depending on the feedback they emit when executed. RQ1 treats tests as *events* in the trajectory—whether agents write them, when they appear, and how often they are run. RQ2 shifts to the *content* of those tests: the feedback they produce during execution. We capture this feedback

through two common signals in agent-written tests: **assertions** (which fail when conditions are violated) and **value-revealing prints** (which expose runtime values). This view clarifies what agents use tests for when resolving GitHub issues.

Experiment Design. RQ2 conditions on tasks that write at least one test artifact and reports descriptive summaries of two aspects of test feedback:

- **Signal counts** (RQ2.1): how many feedback statements appear in agent-written tests, split into assertions vs. value-revealing prints.
- **Assertion types** (RQ2.2): what types of assertions appear in agent-written tests, using a four-type categorization.

4.1 RQ2.1 Task-level feedback signal amount: How much feedback do agent-written tests encode?

Goal and measurements. Conditioning on tasks that contain *agent-written* test artifact, we quantify how many feedback statements are encoded in those artifacts. We distinguish two signal types: (i) **verification signals** (A), i.e., assert statements that specify explicit checks, and (ii) **observational signals** (P), i.e., *value-revealing* print statements that expose runtime values or computed expressions. To ensure P (prints) reflects observational feedback, we exclude pure-literal prints that emit only fixed strings (e.g., `print("here")`) and count only prints that expose runtime values, expressions, or execution results (e.g., `print(obj.attr)`). For each task t with test artifacts \mathcal{A}_t , and for signal type $S \in \{A, P\}$, let $n_{t,a}^S$ be the number of signal statements of type S in artifact $a \in \mathcal{A}_t$. We define the **task-level signal totals**:

$$N_t^S = \sum_{a \in \mathcal{A}_t} n_{t,a}^S, \quad N_t^{\text{total}} = N_t^A + N_t^P.$$

We report macro-over-tasks means of N_t^A (assertion count), N_t^P (value-revealing print count), and N_t^{total} (overall signal count) for each model, separately for resolved and unresolved tasks.

Table 4. Task-level feedback signal amount encoded in agent-written tests

Model	Resolved				Unresolved			
	#Tasks w/ tests	Assertions per task (\bar{N}^A)	Prints per task (\bar{N}^P)	Total signals per task (\bar{N}^{total})	#Tasks w/ tests	Assertions per task (\bar{N}^A)	Prints per task (\bar{N}^P)	Total signals per task (\bar{N}^{total})
<i>claude-opus-4-5</i>	314	5.16	25.00	30.16	101	5.36	25.61	30.97
<i>gemini-3-pro-preview</i>	235	1.45	4.34	5.79	73	1.62	5.04	6.66
<i>kimi-k2-thinking</i>	309	2.86	20.72	23.57	178	3.51	24.03	27.54
<i>minimax-m2</i>	302	7.37	34.06	41.43	191	4.66	43.09	47.76
<i>deepseek-v3.2-reasoner</i>	277	3.51	16.43	19.94	169	3.31	20.95	24.27

Note. Macro-over-tasks means computed over tasks with tests. Assertions count assert statements; prints count *value-revealing* prints. $\bar{N}^{\text{total}} = \bar{N}^A + \bar{N}^P$.

Results. Table 4 shows that, when agent-written tests are present, they can contain a substantial number of feedback statements per task (e.g., 19.94–47.76 total signals for several models). As shown in Figure 2, feedback is predominantly *observational*: for every model, value-revealing prints exceed assertions in the macro-average counts per task. Models differ markedly in overall signal volume. For example, *minimax-m2* encodes the largest total signal counts (41.43–47.76), whereas *gemini-3-pro-preview* encodes far fewer (5.79–6.66), with other models in between. Across models, unresolved tasks tend to show slightly higher total signal counts, driven mainly by more value-revealing prints (e.g., *deepseek-v3.2-reasoner*: +4.33 total; *minimax-m2*: +6.33 total). Assertion

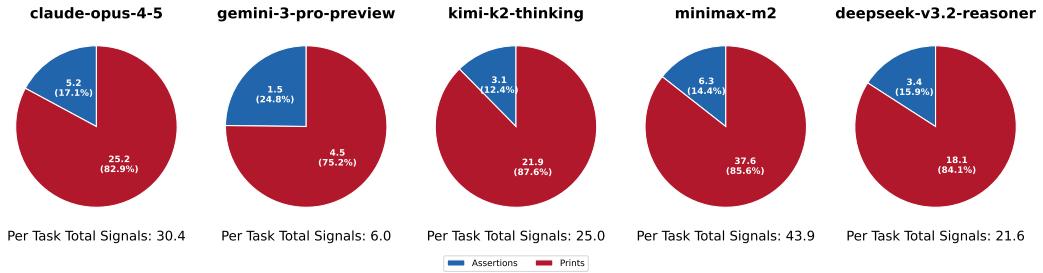


Fig. 2. Composition of feedback signals in agent-written tests across models. Value-revealing prints dominate over assertions for all models.

counts are comparatively stable; notably, *minimax-m2* has fewer assertions but more prints on unresolved tasks.

RQ2.1 Test Signal Amount: Key Takeaway

When agents write tests, those tests often emit many runtime feedback statements per task, but the feedback is mostly observational: value-revealing prints consistently outnumber assertions across all models. Signal volume varies widely by model (e.g., *minimax-m2* high vs. *gemini-3-pro-preview* low), and unresolved tasks show slightly more total signals mainly due to additional prints, while assertion counts remain relatively stable.

4.2 RQ2.2 Assertion categorization: What kinds of verification do assertions encode?

Goal and measurements. RQ2.1 counts how many assert statements appear in agent-written tests, but counts alone do not tell us *what* those assertions check. Assertions can enforce different kinds of checks—for example, basic preconditions (e.g., non-None or type checks) versus checks against expected values or structures. Models may therefore differ not only in how often they assert, but also in what forms of checks they write. RQ2.2 provides a descriptive breakdown of assert statements into four assertion categories:

- **C1 Sanity checks.** The assertion only checks existence or type, without constraining the expected behavior. *Example:* `assert x is not None`.
- **C2 Property checks.** The assertion checks a property of a value or object (e.g., membership or validity) without fixing an exact output. *Example:* `assert hasattr(obj, "attr")`.
- **C3 Relational checks.** The assertion enforces a constraint such as a range, bound, or relationship between values. *Example:* `assert 0 <= score <= 1`. This category also includes checks that expect a specific exception, because they constrain the allowed behavior to “must fail with an exception of type *E*” rather than matching a single concrete output.
- **C4 Exact checks.** The assertion checks an exact value or deep structural equality. *Example:* `assert output == expected_output`.

To identify and categorize assertions, we implement a rule-based classifier over Python ASTs and map each extracted assertion to exactly one category. The classifier covers both native assert statements (e.g., `assert a == b`) and framework-provided assertion calls (e.g., `self.assertEqual(a, b)` in `unittest`). Concretely, for each test artifact, we parse the code into an AST and extract assertion events from: (i) native assert `<expr>` statements, and (ii) calls to framework assertion APIs. Some assert statements contain multiple checks in one line, combined with boolean operators (e.g., `assert a > 0 and b == 1`). In this example, `a > 0` is a constraint check (C3) and `b ==`

1 is an exact check (C4). For such compound assert statements, we decompose the expression into atomic checks and assign a single category by taking the highest category present under the ordering from C1 to C4, because the most specific check in the statement best reflects what the assertion is trying to enforce. Thus, assert $a > 0$ and $b == 1$ is labeled as C4.

Table 5. Assertion category distribution by model. Counts and percentages are computed over all assertion statements written by each model.

Model	#Assertions	C1 Sanity		C2 Property		C3 Relational		C4 Exact	
		#	%	#	%	#	%	#	%
<i>claude-opus-4-5</i>	2160	351	16.25%	807	37.36%	93	4.31%	909	42.08%
<i>gemini-3-pro-preview</i>	458	76	16.59%	154	33.62%	36	7.86%	192	41.92%
<i>kimi-k2-thinking</i>	1508	225	14.92%	622	41.25%	45	2.98%	616	40.85%
<i>minimax-m2</i>	3117	618	19.83%	1291	41.42%	132	4.23%	1076	34.52%
<i>deepseek-v3.2-reasoner</i>	1531	285	18.62%	537	35.08%	52	3.40%	657	42.91%

Note. C1–C4 denote four assertion categories defined by the form of the check (sanity, property, relational/approximate, and exact-output). Percentages are computed within each model, relative to the model's total assertion count.

Results. Table 5 shows that models have broadly similar assertion-category distributions. Across all five models, most assertions fall into **C2 Property** and **C4 Exact**, while **C3 Relational** remains consistently uncommon. For four models (*claude-opus-4-5*, *gemini-3-pro-preview*, *kimi-k2-thinking*, and *deepseek-v3.2-reasoner*), **C4 Exact** accounts for roughly 41–43% of assertions (40.85–42.91%), and **C2 Property** accounts for roughly 34–41% (33.62–41.25%). *minimax-m2* follows the same overall shape but allocates a smaller share to **C4 Exact** (34.52%) and larger shares to **C1 Sanity** (19.83%) and **C2 Property** (41.42%). Across all models, **C3 Relational** is rare (2.98–7.86%), with the highest proportion in *gemini-3-pro-preview* (7.86%). Overall, the consistent scarcity of **C3** may have several practical explanations. First, relational or approximate checks can be more delicate to specify and maintain than local property checks or direct equality checks. Second, such checks may be less common in everyday unit-test patterns that models imitate during code generation. Third, for many SWE-bench issues, agents may find it more straightforward to write either local property checks (**C2**) or exact expected outputs (**C4**) once they have a candidate fix. We treat these distributions as descriptive of which assertion forms appear in agent-written tests, not as evidence of correctness or impact on task resolution.

RQ2.2 Assertion Categorization: Key Takeaway

Across models, assertions in agent-written tests are dominated by two forms: checks of local properties (e.g., checking that a required attribute is present) and checks against exact expected values. Assertions that express relational or range-style constraints are consistently uncommon (only a small single-digit fraction of assertions across models). *minimax-m2* has a lower share of exact-value checks and a higher share of sanity-guard and local-property checks than the other models, but the overall pattern is similar across models.

4.3 Summary of RQ2

RQ2 shifts from *when* agents test (RQ1) to *what feedback* their agent-written tests produce at execution time. First, when tests are present, they often emit many feedback statements per task, but the feedback is dominated by *value-revealing prints* rather than assertions: across all models,

print signals consistently outnumber `assert` checks, and total signal volume varies widely by model (RQ2.1). Second, when agents do write assertions, models show broadly similar mixes of assertion forms: most assertions either check local properties (e.g., the presence or validity of an attribute or field) or check exact expected values, while relational or range-style constraints are rare (RQ2.2). Taken together, RQ2 characterizes agent-written tests primarily as an *observational* feedback channel: most test feedback comes from value-revealing prints. This naturally raises the next question: *do these agent-written tests meaningfully affect task resolution?* We address this in RQ3.

5 RQ3: Do Agent-Written Tests Truly Affect Task Resolution?

Motivation. In RQ1, we find a weak alignment between agent-written tests and the final task success in this high-autonomy setting. For example, *gpt-5.2* almost never writes new test artifacts (3/500 tasks, 0.6%), yet it still resolves 71.8% of tasks. In contrast, *claude-opus-4.5* writes at least one new test artifact in about 83% of tasks, but its resolution rate is only 2.6 percentage points higher (74.4%). RQ2 further shows that, when tests are written, most feedback comes from value-revealing prints rather than `assert`-based checks. These findings raise a direct question: *Do writing tests truly affect task resolution outcomes, and at what cost?*

Experiment Design. RQ3 answers two questions:

- **RQ3.1 (Outcome impact):** If we encourage or discourage agents to write tests, how do task resolution outcomes change?
- **RQ3.2 (Efficiency impact):** If we encourage or discourage agents to write tests, how do API calls and token usage change?

Model selection. To isolate the effect of agent-written tests, we design two complementary intervention experiments: (i) *encouraging* agents to write tests, and (ii) *discouraging* agents from writing new test files. We choose models for each setup based on their *baseline test-writing rate* observed in RQ1 (Table 1), defined as the fraction of tasks where the agent writes test artifacts.

For the *encourage test writing* setup, we focus on low test-writing models and medium test-writing models, so there is meaningful headroom for increasing test creation. Specifically, we include ***gpt-5.2 (0.6%)***, an extreme **low test-writing model** in RQ1 with near-zero test creation. We also include ***gemini-3-pro-preview (61.1%)***, a **medium test-writing model** whose baseline test creation is already substantial but still leaves room for further increase.

For the *discourage test writing* setup, we start from **high test-writing models** that write tests in the vast majority of tasks in RQ1: four models show consistently high test-writing rates (83.0%–98.6%; Table 1). Due to budget constraints, we select two representatives from this group: ***kimi-k2-thinking (97.4%)*** and ***deepseek-v3.2-reasoner (89.2%)***.

Concretely, we use two prompt variants:

- **Encourage writing tests:** for *gpt-5.2* and *gemini-3-pro-preview*, we append an instruction to write at least *one* runnable new test file (a file whose name starts with `test_` or ends with `_test.py`), separate from the repository’s existing tests.
- **Discourage writing tests:** for *kimi-k2-thinking* and *deepseek-v3.2-reasoner*, we (i) remove the sentence “Test edge cases to ensure your fix is robust” and (ii) append an instruction to not write any new test files or scripts; robustness and edge cases should be handled using reasoning and code inspection only.

By comparing the performance of each agent under these revised prompts against their original performance, we isolate the specific impact of test writing. Consequently, the observed changes in behaviors or outcomes can be directly attributed to the agent-written tests.

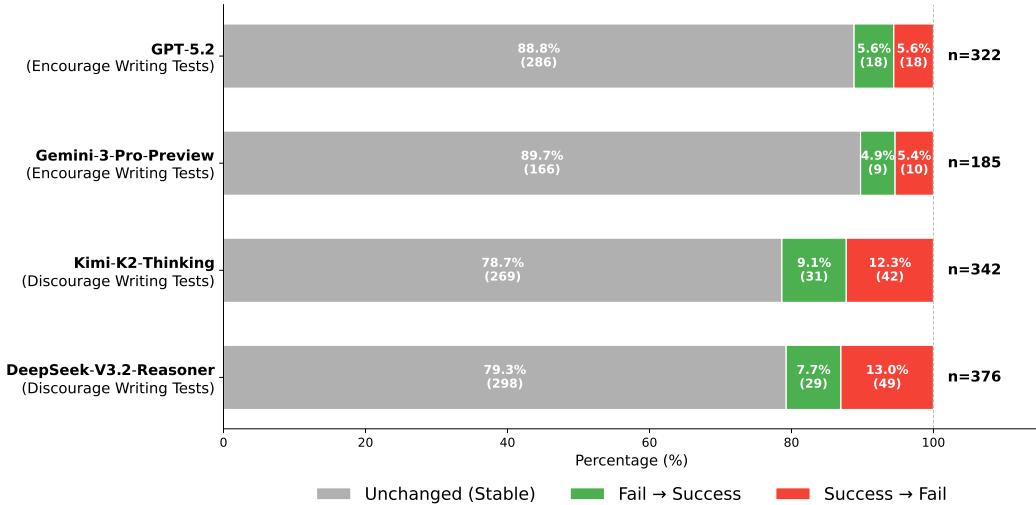


Fig. 3. Outcome-transition distribution on tasks with an intended test-status change

5.1 RQ3.1 Does encouraging or discouraging test writing change task resolution?

Goal and measurements. To answer whether encouraging or discouraging test writing changes task resolution, for each model, we compare each task under two conditions: the baseline run (under standard mini-SWE-agent prompt) and the intervention run (under our revised prompt). Specifically, we record two features: whether the run creates at least one new test artifact (*No test* vs. *Has test*) and whether the patch successfully resolve the issue (*Fail* vs. *Success*). Then, we analyze how these two features change from the baseline run to the intervention run. This respectively results in four possible transition groups for both test writing (*No test*→*No test*, *No test*→*Has test*, *Has test*→*No test*, *Has test*→*Has test*) and task outcome (*Fail*→*Success*, *Success*→*Fail*, *Stable Success*, and *Stable Fail*). To visualize the relationship between these shifts, we represent the results using a transition matrix. In this matrix, the rows represent the change in test-writing behavior, while the columns represent the change in task outcomes. This structure allows us to pinpoint the exact impact of our intervention. For instance, the intersection of *No test*→*Has test* and *Fail*→*Success* represents the instances where encouraging test-writing directly led to a breakthrough in resolving the task.

Results. Our prompt interventions substantially change whether models write test artifacts, but these shifts rarely translate into outcome changes. As shown in Table 6, the *encourage test writing* prompt flips test status for the **low test-writing model** *gpt-5.2* and the **medium test-writing model** *gemini-3-pro-preview*: 64.4% and 37.0% of tasks transition from *No test* to *Has test*, respectively. Conversely, the *discourage test writing* prompt removes tests at scale for the **high test-writing models** *kimi-k2-thinking* and *deepseek-v3.2-reasoner*, moving 68.4% and 75.2% of tasks from *Has test* to *No test*.

Despite these large test-status shifts, resolution outcomes are largely stable. Figure 3 shows that, across models, an average of 83.2% of tasks keep the same final resolution result after the intervention. Table 6 further indicates that even when test status flips, success rates change only slightly. For example, for *deepseek-v3.2-reasoner*, discouraging test writing removes tests in 376 tasks but yields a net decrease of only 20 resolved tasks, a small change relative to the behavioral

Table 6. Test-writing status flips and outcome transitions

Encourage writing tests					
Outcome transition	No test → No test	No test → Has test	Has test → No test	Has test → Has test	Total
Model: gpt-5.2					
Fail → Success	9	18	0	0	27
Success → Fail	9	18	0	0	27
Stable Success	111	218	1	2	332
Stable Fail	46	68	0	0	114
Net change in #Success	0	0	0	0	0
Model: gemini-3-pro-preview					
Fail → Success	0	9	0	8	17
Success → Fail	1	10	1	10	22
Stable Success	2	123	5	219	349
Stable Fail	4	43	0	65	112
Net change in #Success	-1	-1	-1	-2	-5
Discourage writing tests					
Outcome transition	No test → No test	No test → Has test	Has test → No test	Has test → Has test	Total
Model: kimi-k2-thinking					
Fail → Success	1	0	31	11	43
Success → Fail	1	0	42	13	56
Stable Success	5	2	189	65	261
Stable Fail	3	1	80	56	140
Net change in #Success	0	0	-11	-2	-13
Model: deepseek-v3.2-reasoner					
Fail → Success	10	2	29	7	48
Success → Fail	3	0	49	5	57
Stable Success	19	1	187	36	243
Stable Fail	18	1	111	22	152
Net change in #Success	7	2	-20	2	-9

Note. *Test status* is defined by whether the run writes at least one test artifact (“Has test”) or writes none (“No test”). Columns show the baseline→intervention *test-status transition*; rows show the baseline→intervention *outcome transition* (Fail/Success). The highlighted column indicates the *intended* test-status change (green: No test→Has test under **Encourage**; red: Has test→No test under **Discourage**); Δ reports the number (and percentage) of tasks in that intended-change column. **Net change(#Success)** is computed per column as (#Fail→Success) – (#Success→Fail).

shift. Overall, changing how often a model writes test artifacts appears to be a weak lever for shifting task outcomes in this setting.

RQ3.1: Outcomes vs. Test-Status Changes

Discouraging test writing removes agent-written tests at scale (at least 68% of tasks for *kimi-k2-thinking* and *deepseek-v3.2-reasoner*). Yet these large behavioral shifts rarely translate into outcome shifts: across models, 83.2% of tasks keep the same final resolution result after the intervention. Even when tests are induced, success does not increase correspondingly, suggesting that simply writing more test artifacts provides little leverage for fixing the underlying issue. Conversely, suppressing tests for hundreds of tasks does not cause outcomes to collapse; for *deepseek-v3.2-reasoner*, removing tests in 376 tasks yields only 20 fewer successes.

5.2 RQ3.2 How do API calls and token usage change?

Goal and measurements. We further analyze the following three metrics based on the trajectories generated in RQ3.1: (i) average *API calls* per task, (ii) average *input tokens* per task, and (iii) average *output tokens* per task.

Table 7. API calls and token usage under baseline vs. encourage-tests / discourage-tests conditions.

Model	Condition	Tasks resolved	Avg API Calls	Avg Input Tokens	Avg Output Tokens
<i>gpt-5.2</i>	Baseline	359 (71.8%)	19.76	242,855	24,550
	Encourage writing tests	359 (71.8%)	20.84	264,762	29,415
	Change	+0 (+0.0%)	+1.08 (+5.5%)	+21,907 (+9.0%)	+4,866 (+19.8%)
<i>gemini-3-pro-preview</i>	Baseline	371 (74.2%)	40.33	666,096	11,114
	Encourage writing tests	366 (73.2%)	39.21	641,307	10,943
	Change	-5 (-1.0%)	-1.11 (-2.8%)	-24,789 (-3.7%)	-171 (-1.5%)
<i>kimi-k2-thinking</i>	Baseline	317 (63.4%)	46.82	668,449	14,895
	Discourage writing tests	304 (60.8%)	30.25	340,689	8,468
	Change	-13 (-2.6%)	-16.57 (-35.4%)	-327,760 (-49.0%)	-6,427 (-43.1%)
<i>deepseek-v3.2-reasoner</i>	Baseline	300 (60.0%)	46.40	637,297	52,120
	Discourage writing tests	291 (58.2%)	35.06	427,780	44,823
	Change	-9 (-1.8%)	-11.35 (-24.5%)	-209,518 (-32.9%)	-7,297 (-14.0%)

Note. Changes are computed as (Condition – Baseline). **Encourage writing tests** is applied to *gpt-5.2* and *gemini-3-pro-preview*; **Discourage writing tests** is applied to *kimi-k2-thinking* and *deepseek-v3.2-reasoner*.

Results. Table 7 reports task outcomes (*Tasks resolved*) and efficiency metrics (*API calls and tokens*) under the baseline and the two intervention conditions. Overall, the interventions have only marginal impact on resolution rates, but they can noticeably reshape efficiency—in ways that depend on each model’s baseline test-writing propensity.

For the *encourage test writing* setup, the effects differ between the **low test-writing model** *gpt-5.2* and the **medium test-writing model** *gemini-3-pro-preview*. For *gpt-5.2*, encouraging test writing increases overhead: API calls rise by 5.5% and output tokens by 19.8%, while the resolution rate remains unchanged from baseline. In the **medium test-writing model** *gemini-3-pro-preview*, we observe *very small* shifts in overall usage (e.g., -1.5% completion tokens). A likely reason is that this model already writes tests in about 61.6% of baseline tasks, leaving limited headroom for the prompt to flip tasks from *No test* to *Has test*; accordingly, the net change in testing-related interaction is small on average. Moreover, because the model is nondeterministic, even when a task contains tests in both baseline and encouraged runs, the *amount* of testing (and related interaction) can differ slightly between the paired runs; these small within-task fluctuations average out, keeping aggregate cost differences modest and making a small decrease reasonable.

The most striking efficiency shifts appear in the *discourage test writing* setup for the **high test-writing models** *kimi-k2-thinking* and *deepseek-v3.2-reasoner*. Discouraging test writing yields large reductions in resource consumption, with input tokens dropping by 49.0% and 32.9%, respectively. For *kimi-k2-thinking*, average API calls fall by 35.4%, cutting interaction workload by over a third. Notably, these efficiency gains come with only small decreases in success rates (2.6% for *kimi-k2-thinking* and 1.8% for *deepseek-v3.2-reasoner*), suggesting that a substantial portion of the baseline interaction budget spent on test writing for these models has limited marginal benefit for task resolution in this setting.

RQ3.2: Cost changes are larger than outcome changes

Changing whether agents write tests has little effect on task resolution, but it can noticeably change efficiency. More tests increases overhead for **low test-writing model** models without improving outcomes: for *gpt-5.2*, API calls rise by 5.5% and output tokens by 19.8% while the resolution rate stays unchanged. In contrast, suppressing tests yields large efficiency gains for **high test-writing models**: input tokens drop by 49.0% (*kimi-k2-thinking*) and 32.9% (*deepseek-v3.2-reasoner*), and *kimi-k2-thinking* reduces API calls by 35.4%. These savings come with only small success drops (2.6% and 1.8%), implying that most test-writing overhead in these models delivers low marginal utility for producing a successful final patch.

Summary of RQ3. Overall, *more agent-written tests do not mean more solves* in this high-autonomy setting. When we push models to write more tests, test-writing status flips at scale (e.g., 64.4% for *gpt-5.2*), yet task outcomes remain largely unchanged: on average, 83.2% of tasks keep the same success/fail result after the intervention. At the same time, *more tests can be expensive*: for the *low test-writing model gpt-5.2*, inducing test writing increases overhead (+5.5% API calls; +19.8% output tokens) without any gain in resolution. Conversely, *fewer tests can be much cheaper* with only small outcome losses: for the *high test-writing models kimi-k2-thinking* and *deepseek-v3.2-reasoner*, suppressing test writing cuts input tokens by 49.0% and 32.9%, respectively, while success drops remain modest (2.6% and 1.8%). Taken together, varying the amount of agent-written tests strongly reshapes resource usage but has limited leverage on whether the final patch resolves the issue, suggesting that much of the test-writing effort provides low marginal utility for task resolution.

6 Discussion and Future Work

6.1 Implications

Our study indicates that the testing behaviors conducted by current LLMs, without clear guidance, do not show significant improvement in agent-based software issue resolution. It suggests two main implications for the research and practice in designing and utilizing LLM software agents.

On the one hand, our results reveal inherent limitations in how existing LLMs leverage testing for issue resolution when explicit guidance is absent. This highlights the importance of instructing agents not only on whether tests should be written, but also on how they should be designed and, more critically, how to effectively interpret and utilize the feedback generated by test execution.

On the other hand, given that current LLMs often fail to extract meaningful value from test writing, practitioners may consider adopting a more conservative approach to agent-generated tests. Such an approach could involve more sophisticated prompting strategies or guardrail mechanisms that help agents determine when test writing is truly necessary. Additionally, code agents could benefit from incorporating a cost–benefit monitoring framework that tracks the overhead associated with test-related activities, such as test writing, execution, and failure analysis, to assess their actual contribution to patch refinement.

6.2 Future work

Our findings motivate two future-work topics on agent-written testing in lightly scaffolded, high-autonomy setups.

Evaluating on-the-fly test quality in a non-stationary code state. Traditional test-quality metrics (e.g., coverage, mutation score, fault revelation) assume a *fixed snapshot* of the system under test and reproducible executions [2, 15, 27, 37, 39, 48, 50]. In agentic development, the code state is non-stationary: tests are written and run against intermediate repository versions that may later be overwritten, and the final patch may not preserve the exact state a test originally validated.

This complicates attribution (which version/function a test exercised), reproducibility, and the use of conventional quality pipelines. Future work should develop instrumentation for *execution-time* evaluation—e.g., capturing runnable snapshots (code, environment, commands) at test time—and define metrics that remain meaningful for transient intermediate artifacts.

Self-evolving test-generation strategies without over-constraining exploration. Our results highlight a tension between structure and autonomy: fully self-directed testing can increase cost with limited outcome gains, while a heavy, fixed workflow can narrow the search space and preclude better strategies. The goal is to produce *higher-value* tests by deciding *when* to test, *what* to verify (oracle design), and *how* to allocate budget across reasoning, editing, and validation, while preserving task-specific flexibility. A promising direction is *self-evolving* [17, 35, 46, 52] testing policies: instead of a static hand-written prompt, allow the agent to revise its own testing prompt/policy from environment feedback and failure modes, adapting to task context and model capabilities [12]. Future work can formalize this as closed-loop optimization under cost and safety constraints, and compare human-specified versus self-adapted strategies under matched budgets and controlled scaffolds.

6.3 Threats to Validity

Internal validity. Agent runs can vary due to stochastic decoding and tool/environment nondeterminism, which may affect both testing behavior and resolution outcomes. Moreover, success–failure comparisons can reflect differences in task difficulty or interaction length (e.g., debugging duration), not only testing. We mitigate these concerns by treating observational results as descriptive, by using prompt-only interventions that change test creation while keeping the agent setup fixed, and by reporting task-level outcome transitions (not only aggregate resolution deltas) to show where outcomes change versus remain stable.

External validity. Our findings are based on SWE-bench under a light scaffold and a specific set of models and providers; absolute magnitudes may differ under other benchmarks, programming languages, toolchains (e.g., enforced CI), or future model versions. To support transfer, we focus on patterns that may recur in similar agent settings (e.g., large cross-model differences in testing style, observation-dominant feedback, and limited outcome sensitivity under sizable shifts in test creation), and we provide precise measurement definitions and intervention prompts to facilitate replication under alternative setups.

Data construction validity. Measurements rely on explicit operational definitions and automated extraction. Test adoption is detected through newly created test-like files, and feedback signals are extracted via deterministic AST-based rules for assertions and value-bearing prints, including common helper-style assertion APIs and a tiered taxonomy for assertion forms. These procedures can miss unconventional test artifacts, project-specific helpers, or edge-case syntax patterns. We reduce construction error by using AST parsing (rather than surface regex heuristics), conservative rules for counting value-bearing prints, and fully deterministic, reproducible extraction and categorization; nevertheless, results should be interpreted with respect to these explicit definitions.

7 Related Work

Evaluation for LLM-Generated Tests. Prior work evaluates LLM-generated testing artifacts under *predefined* objectives, most commonly unit tests and assertions, via systems and empirical studies on test-suite quality and model/prompt improvements [21, 24, 38, 49, 51], including targeted oracle generation such as assertions [53]. Recent surveys further systematize this space, summarizing how requirements artifacts are translated into tests and the quality criteria used to judge generated tests [49]. Complementing academic evaluations, industrial studies report closed-loop pipelines that combine LLM-based test generation with mutation-guided feedback to steer or refine generated

tests toward stronger fault-revealing capability [15]. These studies typically score outputs with *fixed* quality metrics (e.g., coverage, mutation-based adequacy proxies, fault revelation) on a *fixed* target program or code snapshot. Benchmarks similarly cast testing as a standalone objective with fixed tasks and protocols (e.g., TestEval [43], SWT-bench [30]). In contrast, our study focuses on *agent-written tests* that emerge *dynamically* during high-autonomy, multi-step resolution of real-world GitHub issues, where the codebase and candidate patches evolve over time. We treat test writing and execution as an emergent process behavior, and characterize (i) *whether/when/how intensively* agents test, (ii) *what signals* tests encode (assertions vs. observational prints), and (iii) how these behaviors relate to *resolution outcomes*.

Trajectory Analysis of Software Agents. Recent work moves beyond final patch and binary success/failure by analyzing the intermediate reasoning and execution traces of LLM-based agents. Studies have examined action–observation patterns that distinguish successful from failed runs [3], compared trajectory length and fault-localization accuracy across agents [25], and proposed workflow taxonomies that decompose agent behavior into stages such as localization, patching, and testing-related steps [4]. Others conduct systematic failure analyses that identify root causes such as diagnostic errors and unproductive loops [22], while process-oriented studies further show that agents often hit recurrent execution errors during issue resolution, motivating lightweight checks and recovery components for robustness [7]. Overall, existing trajectory analyses emphasize action sequences, outcome separation, and error categorization, but rarely examine whether and how agents autonomously decide to test. Our work addresses this gap by characterizing the emergent testing behaviors observed in execution trajectories and analyzing what feedback agent-written tests actually provide during issue resolution.

8 Conclusion

This paper revisited the common intuition that "testing helps" for LLM-based software agents in a *high-autonomy* setting where writing and running tests is not specified in the prompt. Using SWE-bench Verified trajectories produced under a light agent scaffold, we separated (i) *whether/when/how* agents write and execute tests, (ii) *what feedback* those tests encode, and (iii) whether changing test-writing instructions *actually shifts* task outcomes and efficiency. RQ1 established a descriptive baseline of emergent testing behaviors. Across models, test writing propensity differs dramatically (from near-universal test writing to almost none), while within the same model, resolved and unresolved tasks show broadly similar test-writing rates. When tests are present, unresolved tasks tend to spread test writing across a slightly larger portion of the trajectory and execute tests more often, with higher re-execution intensity; process-level execution failures, however, vary mainly by model rather than outcome. These results highlight that in a high-autonomy setting, "testing" is a model-specific process behavior and is not tightly coupled to eventual success. RQ2 then showed that the *content* of agent-written tests is largely observational. Across all models, value-revealing prints consistently outnumber assert-based checks, and overall signal volume varies widely by model. When assertions are used, their forms are broadly similar across models: most are local property checks or exact-value checks, while relational or range-style constraints are consistently rare. Taken together, agent-written tests function primarily as a runtime probing interface rather than a systematic verification mechanism. Finally, RQ3 provided controlled evidence about impact. Prompt-only interventions flip test-writing status at scale—inducing tests for the *low test writing model* and suppressing test writing for *high test writing models*—yet task outcomes are mostly unchanged (the majority of tasks preserve the same success/fail result). In contrast, efficiency effects can be substantial: for *gpt-5.2*—the near-zero test-writing model—encouraging test writing increases overhead without improving resolution, whereas for test-heavy models (e.g., *kimi-k2-thinking* and *deepseek-v3.2-reasoner*) discouraging test writing can sharply reduce API calls and token usage

with only modest decreases in success. Overall, *more agent-written tests do not mean more solves*: varying test-writing effort primarily reshapes resource usage and interaction patterns, with limited leverage on whether the final patch resolves the issue.

Overall, our findings provide an empirical answer to the question: *Help or Habit?* In high-autonomy software issue resolution, agent-written tests often behave more like a reproduced *software development lifecycle routine* than a dependable source of help: models that write many tests are not consistently more successful, and prompt interventions that induce or suppress test writing rarely change whether the issue is resolved. What tests *do* change is the process footprint—API calls, token usage, and interaction steps—indicating that test writing frequently reflects how an agent chooses to work, not whether it can reliably validate the patch.

9 Data Availability

To support transparency and reproducibility, we publicly release a replication package containing the datasets, raw trajectories, and analysis scripts used in this study. The package is available on Figshare: <https://figshare.com/s/8fa6412b86bf54fc27e?file=61474897>.

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