TDD-Bench Verified: Can LLMs Generate Tests for Issues Before They Get Resolved?

Toufique Ahmed, Martin Hirzel, Rangeet Pan, Avraham Shinnar, and Saurabh Sinha
IBM Research

***Abstract*—Test-driven development (TDD) is the practice of writing tests first and coding later, and the proponents of TDD expound its numerous benefits. For instance, given an issue on a source code repository, tests can clarify the desired behavior among stake-holders before anyone writes code for the agreed-upon fix. Although there has been a lot of work on automated test generation for the practice "write code first, test later", there has been little such automation for TDD. Ideally, tests for TDD should be fail-to-pass (i.e., fail before the issue is resolved and pass after) and have good adequacy with respect to covering the code changed during issue resolution. This paper introduces TDD-Bench Verified, a high-quality benchmark suite of 449 issues mined from real-world GitHub code repositories. The benchmark's evaluation harness runs only relevant tests in isolation for simple yet accurate coverage measurements, and the benchmark's dataset is filtered both by human judges and by execution in the harness. This paper also presents Auto-TDD, an LLM-based solution that takes as input an issue description and a codebase (prior to issue resolution) and returns as output a test that can be used to validate the changes made for resolving the issue. Our evaluation shows that Auto-TDD yields a better fail-to-pass rate than the strongest prior work while also yielding high coverage adequacy. Overall, we hope that this work helps make developers more productive at resolving issues while simultaneously leading to more robust fixes.**

***Index Terms*—test-driven development, test generation, LLMs, benchmarks**

I. Introduction

Benchmarks can inspire technological progress, but benchmarks for automated software engineering tasks lag behind the increased adoption of stronger and stronger large language models (LLMs). To be meaningful, benchmarks need to be realistic and measurable while also being challenging. For instance, the HumanEval benchmark [1] is measurable and was, initially, challenging for LLMs, but more recently lost popularity when LLM performance on it became saturated.

One important software engineering task that can benefit greatly from an up-to-date benchmark is Test-driven development, or *TDD* [2]. TDD is the practice of "test first, write code later", where a software developer writes tests before writing corresponding code. This means the tests initially fail, and, if everything goes right, they pass after applying the code changes. Compared to the common practice of "write first, test later", TDD makes requirements clearer, enhances confidence in the code once written, and leads to tests that emphasize the interface over implementation details. For example, up-front tests can clarify the desired behavior between the interested parties for an issue on a source code repository, including the project maintainer, the user opening the issue, and the developer submitting a pull request to close it. Subsequently, the same tests can serve as acceptance criteria for the pull request once the code is written.

This paper contributes a new benchmark, TDD-Bench-Verified. This new benchmark is derived from SWE-bench [3], a dataset for issue resolution comprising 2,294 issues mined from 12 popular Python GitHub repositories. Deriving TDD-Bench-Verified involved two modifications: filtering for high-quality instances and evaluating test generation instead of issue resolution. For filtering, it reuses an extensive human annotation campaign done by OpenAI [4] to avoid underspecified issues, overly-specific tests, and flaky test environments. While the OpenAI annotation campaign filtered the dataset down to 500 issues, some of the remaining issues were still problematic. Therefore, we applied additional automated filters, resulting in an even higher-quality subset of 449 issues. Each issue yields one *instance* $x = \langle d_{\text{issue}}, c_{\text{old}} \rangle$ comprising a natural-language issue description d_{issue} together with the original version of a codebase c_{old} right before the issue was addressed.

A *solution* to TDD-Bench-Verified consists of a function $genTests$ that takes an instance x and returns a set of tests $y = genTests(x)$. TDD-Bench-Verified provides an *evaluation harness* that uses various testing tools in a containerized environment to implement an evaluation metric $tddScore$ that a solution $genTests$ tries to maximize. While the solution $genTests$ has access to c_{old} only, the evaluation metric also uses the hidden golden new code $\hat{c}_{\text{new}}$ right after the issue was fixed. The metric $tddScore$ combines two factors. First, $failToPass(x, y)$ is a binary correctness metric that checks whether the tests y fail on the old code c_{old} and pass on $\hat{c}_{\text{new}}$. Satisfying the $failToPass$ criterion is necessary but not sufficient for a good test suite. Therefore, the second factor, $adequacy(x, y)$, measures adequacy of tests y with respect to instance x via coverage on the old and new code.

Because writing tests up-front is tedious for humans, recent work has started automating that task using LLMs. Libro [5] prompts Codex [1] with Java issues from Defects4J [6] and achieves 33% success rate in creating fail-to-pass tests for 750 issues, prompting the LLM 50 times for each issue (pass@50); when considering only one generation per issue (pass@1), its success rate drops to 19.9%. Plein et al. [7] also prompt LLMs with Defects4J issues, but generate fail-to-pass tests in only 6% of cases using ChatGPT. Mündler et al. [8] introduce a benchmark, SWT-bench, that is similar to TDD-Bench-Verified in that it evaluates how well a solution can generate

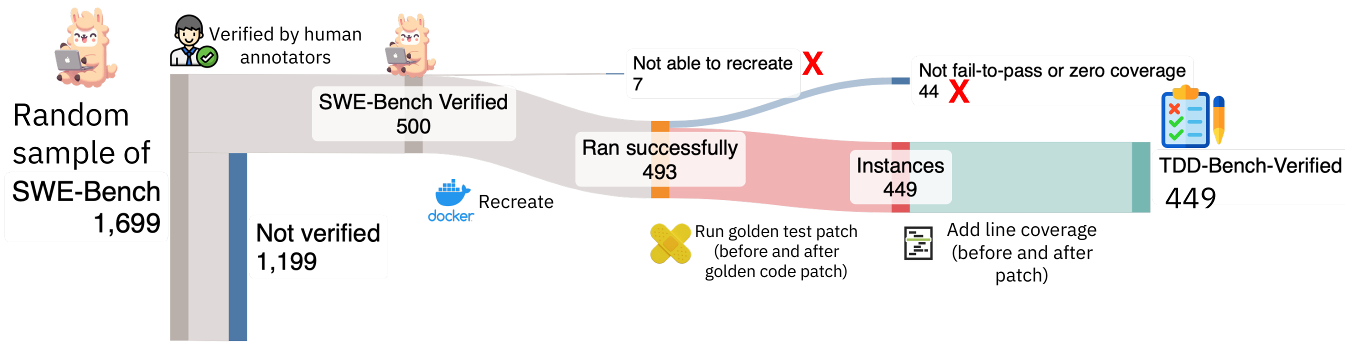

Fig. 1: Overall flow of TDD-bench dataset filtering starting from SWE-bench verified

fail-to-pass tests from issue descriptions. However, SWT-bench applies less rigorous quality filters than TDD-Bench-Verified, and it measures coverage in a more round-about way by first running more tests than just the submitted ones and then subtracting them back out. The paper that introduces SWT-bench [8] also experiments with various solutions, but only evaluates them on a subset of 276 single-file issues called SWT-bench Lite instead of the full 1,983 instances of SWT-Bench. Their best solution, SWE-Agent+, is derived from SWE-Agent [9], and using GPT-4 achieves a fail-to-pass rate of 19.2% on SWT-bench Lite. Similarly, LIBRO, while it performs better on Defects4J issues with multiple trials per issue, achieves a fail-to-pass rate of only 15.2% on SWT-Bench Lite. These numbers indicate that generating tests from issues is challenging even for the latest frontier models with the latest agents.

In addition to our new benchmark, this paper also introduces Auto-TDD, our new solution that achieves 21.7% fail-to-pass rate on SWT-bench Lite, outperforming the best prior solution SWE-Agent+ [8] while being simpler. Auto-TDD accomplishes this strong performance through a combination of prompting and symbolic techniques. It decomposes the task into a 3-step pipeline of LLM calls, making the problem solved by each step simpler while also being more predictable end-to-end than a fully dynamic agent. It is designed to work not only with frontier models but also with open-source models, by using few-shot prompts to help models better understand the task and format at hand. Auto-TDD uses symbolic techniques to gather and render the right code context to use as LLM input, and has a simple LLM output format designed to better match the pre-training distribution of the model. In addition, it uses symbolic techniques to repair mistakes in LLM-generated code before submitting a test.

Most evaluations in this paper are based on the 449 instances in TDD-Bench-Verified, except when a direct comparison to SWE-Agent+ necessitated the use of the 276 instances used by their paper [8]. The experiments used three LLMs (llama-3.1-70b, mistral-large, and GPT-4o). The best-performing LLM was GPT-4o with a fail-to-pass rate of 23.6% on TDD-Bench-Verified. We conducted various ablation experiments finding, among other things, that LLM-based test file selection was crucial. While our primary focus was TDD, we also experimented with a variant of the benchmark and solution for a "write code first, test later" scenario. In terms of adequacy, we found bimodal results. When model-generated tests were fail-to-pass, their coverage was above 90% (similar to human-written tests), but other model-generated tests had a coverage of less than 60%.

The contributions of this paper are:

- A new benchmark, TDD-Bench-Verified, for the practice of "test first, write code later", that evaluates the correctness and adequacy of tests generated from issue descriptions in real-world software projects, available at https://github.com/IBM/TDD-Bench-Verified.
- A new technique, Auto-TDD, that dramatically improves the state of the art for generating tests before the code-to-be-tested is written, by combining new effective LLM prompting techniques with symbolic techniques.
- An evaluation of Auto-TDD on TDD-Bench-Verified, along with a qualitative and quantitative exploration of related aspects around test adequacy, human-written tests, and the alternative practice of "code first, test later".

We hope that TDD-Bench-Verified and Auto-TDD will inspire improvements in automated test-driven development, improve developer productivity, and ultimately lead to more robust software.

II. TDD-Bench-Verified Benchmark

This section introduces TDD-Bench-Verified, our new benchmark that requires generating tests given only an issue description and an old version of the code, but without access to the new code to be tested.

A. SWE-Bench

TDD-Bench-Verified builds upon SWE-bench [3]. SWE-bench was mined from GitHub pull requests (PRs) that resolved issues. Each SWE-bench instance is a pair $x = \langle d_{\text{issue}}, c_{\text{old}} \rangle$ of an issue description d_{issue} alongside the old version c_{old} of the code just before the PR. Furthermore, the SWE-bench evaluation harness uses a set of golden tests $\hat{y}$ and golden new code $\hat{c}_{\text{new}}$ mined from the same PR.

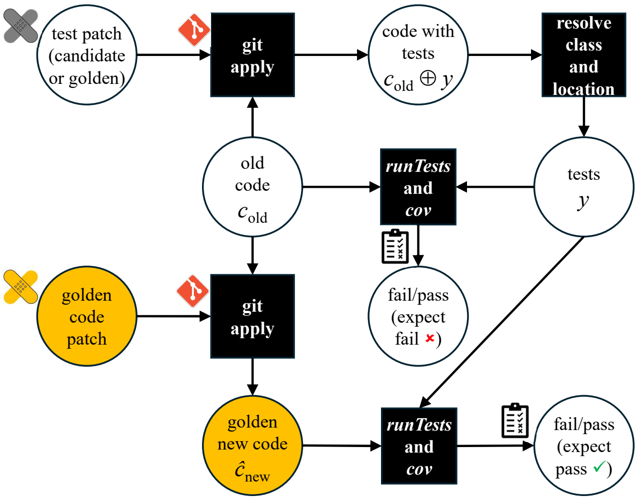

Fig. 2: Evaluation harness for TDD-Bench-Verified.

A solution to SWE-bench is a function *genCode* that takes an instance x and returns a new version $c_{new} = genCode(x)$ of the code. The golden new code $\hat{c}_{new}$ and golden tests $\hat{y}$ in the mined PR are hidden from the solution *genCode*, which only has access to x. SWE-bench evaluates a solution *genCode* by the sum, over all instances x, of the pass criterion $pass(x, c_{new}) = I\big(fail \notin runTests(\hat{y}, c_{new})\big)$. (Here, $I(p)$ is the indicator function that returns 1 if predicate p is true and 0 otherwise.) While TDD-Bench-Verified instances $x = \langle d_{issue}, c_{old} \rangle$ look the same as SWE-bench instances, TDD-Bench-Verified solutions are functions *genTests* instead of functions *genCode*, and it uses an evaluation function *tddScore* instead of *pass*. The evaluation function *tddScore* uses the hidden golden new code $\hat{c}_{new}$ but it does not use the hidden golden tests $\hat{y}$.

B. TDD-Bench-Verified Evaluation Harness

Fig. 2 shows the harness for evaluating tests, which typically come from a solution *genTests* but we can also apply the harness on golden tests from a PR. The evaluation harness runs in a containerized environment. Starting at the top left, tests come in the form of a patch, which git applies on top of the old code c_{old}. Next, the harness analyzes the resulting code $c_{old} \oplus y$ to resolve the exact list of contributed test functions y. Once this resolution step is done, the harness can execute the exact tests y without accidentally running any other tests that happen to be in the same file but were not part of the test patch. This yields test results including coverage of the contributed tests on the old code. At least one of those results should be a failure for the tests to be relevant to the issue at hand.

Moving on to the bottom half of Fig. 2, the code changes come from the golden code patch mined from the same PR, which git applies to obtain the new code $\hat{c}_{new}$. The harness executes the tests y again, this time on the new code, to obtain a second set of test results. This time, all tests should pass, to validate that the issue was indeed resolved. An example test patch is presented in Fig. 3.

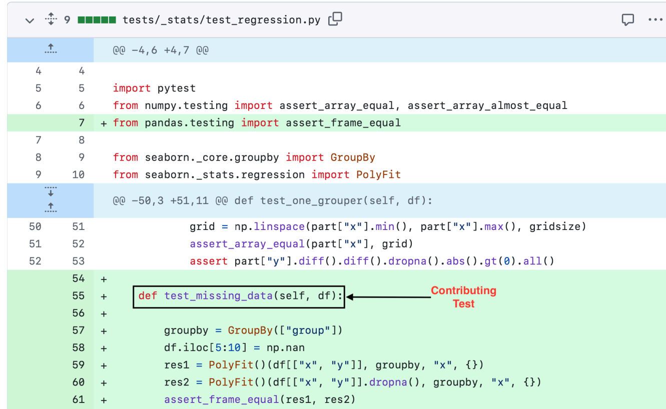

Fig. 3: Example test patch with one contributed test. Although the test file name `test_regression.py` and test name `test_missing_data` are available in this text, the class `TestPolyFit` enclosing `test_missing_data` is missing. By applying the test patch to the base commit and parsing the file, we retrieve `TestPolyFit`, which is required to run `test_missing_data`.

C. Dataset Filters

SWE-bench uses filters to only keep mined instances x for which $\hat{y}$ contains at least some tests that fail on c_{old} and pass on the golden new code $\hat{c}_{new}$ from the same mined PR. SWE-Bench Verified is a subset of the original instances from SWE-Bench, consisting of 500 instances vetted by human annotators [4]. The annotators filtered out instances where the issue description d_{issue} was underspecified or where the golden tests $\hat{y}$ were too specific, i.e., would reject some valid new code c_{new}. They also removed some instances where tests failed due to environment problems instead of the solution.

In the same spirit, TDD-Bench-Verified applies further filters to obtain an even higher-quality subset of instances. In a nutshell, the filtering process applies the TDD-Bench-Verified evaluation harness described above to the supposedly golden tests $\hat{y}$ from the original PR. Specifically, substituting $\hat{y}$ wherever y occurs in Fig. 2 checks whether the PR indeed contributed tests that went from failing to passing. We filter out any instance where the contributed tests do not satisfy that criterion. As it turns out, while the human annotators of SWE-bench Verified were diligent, a few instances slipped past their filters, and we drop those for TDD-Bench-Verified.

Fig. 1 visualizes this filtering process. Starting from the original 500 instances of SWE-bench verified, we first drop 7 instances whose environment we could not recreate. Next, we run the test harness on the golden tests $\hat{y}$. This filters out 44 additional instances because the tests do not have the expected fail-to-pass behavior or have zero line coverage on the golden code patch. In the end, 449 high-quality instances remain. We summarize key statistics of TDD-Bench-Verified in Table I.

D. Evaluation Metric

This section defines the evaluation metric *tddScore* of our benchmark. Passing a test does not necessarily mean it is adequate to address the issue. Aleithan *et al.* reported that

TABLE I: Different attributes of the TDD-Bench-Verified instances.

Project	# of Instances	Fraction of Dataset (in %)	# of Files	# of Test Files	Average # of Lines Deleted and Added		Average Word Count in Issue Description
					On Code	On Tests	
Astropy	18	4.0	1,990	351	11.9	28.7	304.5
Django	212	47.2	6,863	810	12.0	24.7	145.6
Flask	1	0.2	275	27	3.0	5.0	35.0
Matplotlib	32	7.1	4,656	102	9.3	20.0	260.5
Pylint	10	2.2	3,833	51	24.7	33.8	347.1
Pytest	16	3.6	639	114	24.6	53.5	250.1
Requests	5	1.1	155	9	3.6	6.6	85.2
Scikit-learn	25	5.6	1,772	242	11.8	17.1	297.6
Seaborn	2	0.5	353	34	13.5	18.5	182.5
Sphinx	41	9.1	1,917	137	17.5	26.1	186.2
Sympy	67	14.9	2,050	617	12.1	11.9	114.2
Xarray	20	4.5	394	67	17.1	24.3	301.0
Overall	449	100.0	24,897	2,561	13.2	23.3	182.0

*File counts are based on the main branches of the project (cloned on October 29, 2024).

31.1% of the passed patches are suspicious due to weak test cases in SWE-Bench [10]. To ensure test adequacy or relevance, we also compute the coverage of the submitted test-patch. One key difference between SWE-Bench and TDD-Bench-Verified is that SWE-Bench runs an entire test file to evaluate the submitted patch, whereas we only run the contributing tests y retrieved from the test-patch. Not running other test cases enables us to precisely track the coverage of the submitted tests. If the tests are relevant, they should cover the deleted lines in the base commit c_{old} and the added lines in the commit $\hat{c}_{\text{new}}$ where the issue was addressed. We integrated the Python Coverage package into all 12 repositories and updated the scripts to allow us to run specific test cases and compute coverage from them.

The function $\mathit{tddScore}$ evaluates the quality of tests generated by a solution $\mathit{genTests}$ over a set $X = \{x_0, x_1, \dots\}$ of instances. It returns a number between 0 and 100, the higher the better. It is defined as 100 times the arithmetic mean of the per-instance scores:

$$\mathit{tddScore}(X, \mathit{genTests}) = \frac{100}{|X|} \sum_{x \in X} \mathit{tddScore}\big(x, \mathit{genTests}(x)\big)$$

Given a set of tests $y = \mathit{genTests}(x)$ submitted for an instance, the per-instance score is a product of two factors:

$$\mathit{tddScore}(x, y) = \mathit{failToPass}(x, y) \cdot \mathit{adequacy}(x, y)$$

The first factor is a binary correctness metric, using the indicator function for the tests y failing on the old code times the indicator function for the tests y passing on the new code. While the solution $\mathit{genTests}$ only has access to the old code c_{old}, the evaluation metric also uses the hidden golden new code $\hat{c}_{\text{new}}$ right after the issue was fixed.

$$\mathit{failToPass}(x, y) = \\ I\big(\mathit{fail} \in \mathit{runTests}(y, c_{\text{old}})\big) \cdot I\big(\mathit{fail} \notin \mathit{runTests}(y, \hat{c}_{\text{new}})\big)$$

The second factor is the adequacy of the tests, defined as a fraction between 0 and 1 based on test coverage on the old and new code:

$$\mathit{adequacy}(x, y) = \\ \frac{|\mathit{cov}(y, c_{\text{old}}) \cap (c_{\text{old}} \setminus \hat{c}_{\text{new}})| + |\mathit{cov}(y, \hat{c}_{\text{new}}) \cap (\hat{c}_{\text{new}} \setminus c_{\text{old}})|}{|c_{\text{old}} \setminus \hat{c}_{\text{new}})| \quad + \quad |\hat{c}_{\text{new}} \setminus c_{\text{old}}|}$$

Adequacy focuses on just the coverage of lines added and deleted when going from the old code to the new code, because those are the most relevant lines to be tested. In the above, $\mathit{cov}(y, c)$ is the set of lines covered by running tests y on code c; $(c_{\text{old}} \setminus \hat{c}_{\text{new}})$ is the set of lines deleted by the PR patch; and $(\hat{c}_{\text{new}} \setminus c_{\text{old}})$ is the set of lines added by the PR patch. We had initially considered defining adequacy with two separate fractions for the deleted vs. added lines. However, that was not only poorly weighted but brittle, because in some cases, the numerator or denominator of one of the fractions was zero. Later in the paper, we will give an example for how adequacy can vary between hand-written tests and LLM-generated tests.

III. Test Generation from Issue Descriptions

This section first presents a baseline test-generation technique, which is a zero-shot approach for generating *test files* from issue descriptions. Then, it presents Auto-TDD, which implements a few-shot approach for generating *test functions* from issue descriptions.

A. Baseline: Zero-shot Test File Generation

Recent instruction-tuned LLMs excel at following instructions [11, 12]. We start with a simple zero-shot approach to generate a fail-to-pass test given the repository name and the issue description (see detailed prompt in Fig. 4-❶). Given the prompt, the model generates a complete test file with all necessary imports to make it compilable. While in real scenarios, test files usually have multiple test cases, this baseline usually generates only a single test per instance. The

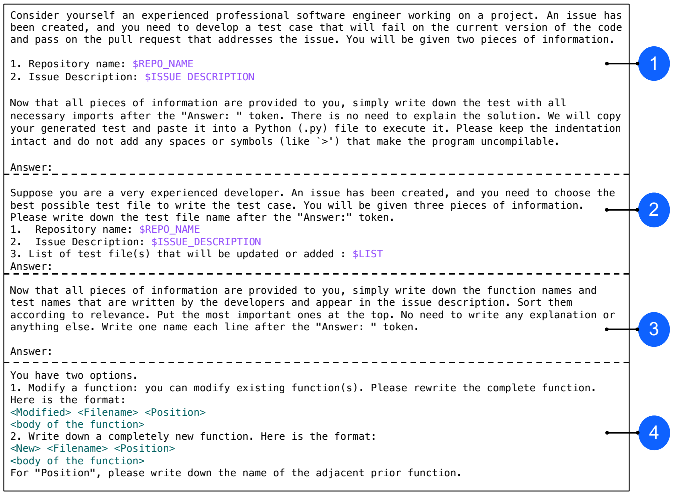

Fig. 4: Different prompts used in our approach: ❶ shows the prompt for the baseline zero-shot test file generation; ❷–❹ show the prompts used for the sequence of LLM calls (for selecting a test file, identifying issue-related test functions, and generating test functions) in Auto-TDD.

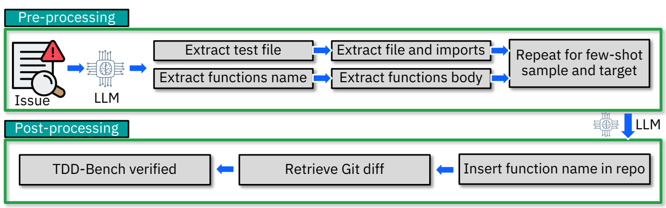

Fig. 5: Overall architecture of Auto-TDD.

generated test file (we call it `test_tdd.py`) needs to be placed in the right directory because otherwise, the imports in the test may not work. Fortunately, all Python projects in TDD-Bench-Verified have at least one directory called `tests`; some projects have multiple such directories. So we follow the simple approach of searching for the `tests` directory and placing `test_tdd.py` in that directory.

B. Auto-TDD: Few-shot Test Function Generation

One of the caveats of the baseline approach is that it does not support the typical practice of developers while addressing an issue—that they do not write a new test file but instead update an existing test file, by adding tests to it or modifying tests in it (the SWE-Bench test patches strongly support this hypothesis). Working on an existing file helps the developers with necessary imports and all the dependencies, thus simplifying their task. To support in-file test-function insertion, we need a better approach that localizes the most suitable test file and the position in the file at which to insert the generated tests. Auto-TDD uses a pipeline of three LLM calls. The first LLM call uses a zero-shot prompt to select a test file. The second LLM call uses a zero-shot prompt to guess issue-related functions (discussed below). The third LLM call uses a few-shot prompt to generate a new test function and pick a position for inserting it in the test file.

Few-shot learning is popular in natural language processing (NLP) [13]. Few-shot learning and its variants are also widely applied in developing techniques for various software-engineering tasks, such as unit test generation [14], code summarization [15, 16], program repair [17], and program synthesis [18]. In few-shot learning, we do not update the model's parameters; instead, we make the model perform a certain task by providing multiple examples in the context. The idea is to present the model with a certain number of input-output pairs and ask the model to provide the solution for the last instance. Research has shown that few-shot learning is generally superior to zero-shot learning for most tasks. Besides, in few-shot learning, it is easy to instruct the model to generate the output in a specific format, which is important for post-processing the results.

1) Selecting a test file: Selecting the correct test file can be crucial for improving test generation performance. The third LLM call of Auto-TDD is intended to generate only the test function, without knowing the necessary imports and dependencies, so we should not expect the model to handle those aspects. If the first LLM call can correctly select the test file, the third call can thus benefit from the imports. Additionally, as part of the approach, we provide the model with a file name, its classes, and methods to determine where to insert into or modify the test file. Because we do not know (or cannot infer from the benchmark) the correct test file, we should apply some methods to find it. To do so, we first navigate to the base commit c_{old} where the issue was created and collect all files that contain at least one test.

In an initial attempt, we took the list of test files and tried to find any name present in the issue description d_{issue}. This conclusively found a test file for only 3 instances (<1%). However, even when the test file name does not appear directly, there are enough hints in the issue description that could be leveraged to guess the correct test file. For example, instance astropy__astropy-12907[1] does not contain the test file name `astropy/modeling/tests/test_compound.py` directly, but it contains multiple mentions of terms like "model" and "compound" that an LLM can leverage to guess the correct test file. We tried a simple zero-shot approach, where we presented the model with the issue description d_{issue} and names of test files in c_{old} and asked the model to choose a suitable test file (see detailed prompt in Fig. 4-❷). Table II shows that LLMs are good at identifying test files from the issue description (56%–62% accuracy, depending on the model) and perform much better than a traditional frequency-based retrieval algorithm like BM25 (only 15% accuracy).

2) Guessing issue-related functions: Extracting issue-related functions could be helpful for the LLM in generating the test function. The issue description d_{issue} often contains names of functions. However, there are two challenges: (1) some functions are library functions that do not have any project-specific information, and (2) it is hard to parse the function names from the issue description because it is written

[1]https://github.com/astropy/astropy/issues/12906

TABLE II: BM25-based and model-based test file retrieval.

Approach	# of Correct Retrieval (Out of 449)	Accuracy (in %)
BM25	69	15.4
Llama-3.1	253	56.3
Mistral-Large	278	61.9
GPT-4o	276	61.5

in natural language. Hence, we perform a second LLM call to parse these function names. This step uses a zero-shot prompt (see detailed prompt in Fig. 4-❸), and the LLM returns a list of functions. Then, we look into the repository for those functions and, if we find them, we simply add them into the context as relevant functions.

3) Generating and inserting a test function: This is the third and most important LLM call in Auto-TDD. The prompt starts with a guideline that instructs the model about context information provided and the expected output format. After the guideline, the prompt contains three context-solution pairs as few-shot samples. Each context provides four pieces of information: (1) repository name, (2) issue description d_{issue}, (3) issue-related functions from Section III-B2, and (4) a skeleton of the test file from Section III-B1 containing the test class names, test method declarations, and imports. It is important to know the name of the test file and be aware of the adjacent function to locate where to insert the test.

We instruct the model to follow a specific output format in the guideline and also demonstrate some samples in the context. In terms of tests, we can have two distinct scenarios: (1) the LLM modifies an existing test, and (2) the LLM writes a completely new test. To handle these cases, the prompt instructs the LLM to start its response with "Modified" or "New" to indicate the type of test case (see detailed prompt in Fig. 4-❹). The LLM also generates the test file name and position of the generated test. For modification, we try to find the function and replace the existing one with the new one. If the function cannot be found, we add the new function at the end of the file. We also adjust the indentation by looking at the original function or the prior function if we fail to find the function. For a newly written function, we expect the model to generate the name of the adjacent existing function. We find that function in the file and insert the newly written function right after that. For a newly written function, the model is allowed to write "first" to indicate that the function can be written at the beginning of the file, right before the existing first functions. Like modification, in this case, we also repair the indentation and have a fallback plan with inserting the function at the end if anything goes wrong or the model hallucinates any name. Note that we could generate a patch from the model. However, from our initial experiment, we found that the model hallucinates the line number of the patch and could not be applied to the original code for evaluation. Therefore, instead of asking the LLM to generate a patch, we ask it to generate a function, which we insert into the repository after the LLM call. Then, we just use the `git diff` tool to obtain a patch that can be used for evaluation with TDD-Bench-Verified. Fig. 5 presents the overall architecture of Auto-TDD.

IV. Evaluation Methodology

We designed the evaluation to answer the following research questions:

- **RQ1:** How does Auto-TDD perform in generating fail-to-pass tests on TDD-Bench-Verified instances?
- **RQ2:** How do different components of Auto-TDD contribute to its performance?
- **RQ3:** How does Auto-TDD perform in the "write first, test later" setup?
- **RQ4:** What is the coverage adequacy of developer-written tests and Auto-TDD-generated tests?

The rest of this section briefly discusses the dataset, models, and methodology used to answer the research questions. Section V presents the results of this evaluation.

A. Dataset and Models

The evaluation dataset consists of the 449 instances of TDD-Bench-Verified discussed in Section II. These instances belong to 12 popular Python repositories. We also collected three samples from the SWE-Bench Dev [3] split to use as few-shot samples, which belong to 3 different repositories disjoint from the 12 test-split repositories.

We selected three models for our experiments: Llama-3.1, Mistral-large, and GPT-4o.

Llama-3.1: Llama 3.1 is a multilingual instruction-tuned LLM. It is an auto-regressive language model that uses an optimized transformer architecture. The models are tuned by supervised fine-tuning (SFT) and reinforcement learning with human feedback (RLHF) to align with human preferences. It outperforms many of the available open-source and closed chat models on several benchmarks. We used the 70 billion parameter model with 128K context window for our experiments.

Mistral-large: Mistral-large is an instruction-tuned LLM with 123 billion parameters. It has state-of-the-art reasoning, knowledge, and coding capabilities. By design, the model is multilingual, proficient in coding, and possesses agentic capabilities. Additionally, it features a large context window of 128K.

GPT-4o: GPT-4o is a recent frontier model by OpenAI. Unlike Llama-3.1 and Mistral-Large, which are open-source models, GPT-4o is a closed-source model; the model size is not known publicly. GPT-4o is multimodal and can accept text or image inputs and output text. It is also very good at coding with a context window of 128K tokens. We use the temperature value 0 and maximum output of 4096 tokens to keep the configuration similar to the other models.

B. Methodology for RQ1 (effectiveness of Auto-TDD)

In the first research question, we investigate the effectiveness of the baseline approach and Auto-TDD described in Section III. Note that LLMs (especially the smaller models) sometimes generate unparseable code. These models also have an inclination to generate natural language descriptions with the test, which is sometimes unavoidable even with explicit instructions. Also, if the models generate solutions with wrong format, we cannot process those samples. We report the number of samples we lost for various reasons. After that, we compute the number of instances for which each model generates failing test(s). Some tests may pass even on the prior version c_{old} of the code; such tests are irrelevant for us. Eventually, we report the number of failing and passing instances in PR commits $\hat{c}_{\text{new}}$ along with the fail-to-pass rate $\frac{1}{|X|}\sum_{x\in X} \mathit{failToPass}(x, \mathit{genTests}(x))$ and the final score $\mathit{tddScore}(X, \mathit{genTests})$, which also factors in test adequacy.

We also compare our approach with the approaches proposed by Mündler *et al.* [8]. Their strongest approach, SWE-Agent+, is the prior state of the art for our task. However, their paper evaluates on a different set of 276 instances, which is a subset of SWE-Bench Lite, and not verified. Therefore, for this comparison, we use their 276 instances instead of our 449 instances. Note that Auto-TDD runs only the contributed tests, whereas Mündler *et al.* run the complete test file. However, we believe the fail-to-pass metric is still comparable. The sole purpose of this experiment is to compare with the state-of-the-art existing technique.

C. Methdology for RQ2 (ablation study for Auto-TDD)

As described in Section III-B, Auto-TDD augments the context with test file structure and imports, as well as relevant functions. In this research question, we perform an ablation by removing each component individually to show how the performance degrades. We start with the relevant functions, followed by the test file imports. It is essential to have at least the test file structure to make Auto-TDD work. To show the importance of LLM-based file selection, we replace the LLM-selected file with the file selected by BM25 retrieval (Table II).

D. Methodology for RQ3 (“write first, test later”)

While TDD has been well-known to the community for several years now [2], it has both pros and cons. Adopting TDD can be challenging for developers with less experience in using unit testing or writing modular code. Thus, the traditional approach of “write first, test later” is still popular. Although our benchmark is primarily aimed at TDD, it can be easily adapted for this approach. Writing tests is a challenging task either way, and it would be beneficial to write tests for both approaches. To accommodate the “write first, test later” approach, instead of providing the solution with only the base commit c_{old}, we can also provide it with the commit $\hat{c}_{new}$ that addresses the issue. To ensure the validity of the submitted golden patch, the test still needs to fail on the base commit and pass on the later commit. For our baseline, we made a small change by adding the code patch (computed via `git diff`) that addresses the issue to the three pieces of information used in the prompt. This way, we can convey the changes made to address the issue. Alternative approaches could be explored to convey this information, but this is beyond the scope of this paper. In short, we repeat the same methodology we followed in RQ1, but we include the code patch in the prompt.

```python
def test_abstract_model_field_equality(self):
        class A(models.Model):
            class Meta:
                abstract = True
            myfield = models.IntegerField()
        class B(A):
            pass
        class C(A):
            pass
        field_b = B._meta.get_field('myfield')
        field_c = C._meta.get_field('myfield')
        self.assertNotEqual(field_b, field_c)
        self.assertNotEqual(hash(field_b), hash(field_c))
```

(a) Test generated by GPT-4o

```python
def test_abstract_inherited_fields(self):
        """Field instances from abstract models are not equal."""
        class AbstractModel(models.Model):
            field = models.IntegerField()
            class Meta:
                abstract = True
        class InheritAbstractModel1(AbstractModel):
            pass
        class InheritAbstractModel2(AbstractModel):
            pass
        abstract_model_field = AbstractModel._meta.get_field('field')
        inherit1_model_field = InheritAbstractModel1._meta.get_field('field')
        inherit2_model_field = InheritAbstractModel2._meta.get_field('field')
        self.assertNotEqual(abstract_model_field, inherit1_model_field)
        self.assertNotEqual(abstract_model_field, inherit2_model_field)
        self.assertNotEqual(inherit1_model_field, inherit2_model_field)
        self.assertLess(abstract_model_field, inherit1_model_field)
        self.assertLess(abstract_model_field, inherit2_model_field)
        self.assertLess(inherit1_model_field, inherit2_model_field)
        self.assertNotEqual(hash(abstract_model_field), hash(inherit1_model_field))
        self.assertNotEqual(hash(abstract_model_field), hash(inherit2_model_field))
        self.assertNotEqual(hash(inherit1_model_field), hash(inherit2_model_field))
```

(b) Developer-written test

Fig. 6: Model-generated and developer-written fail-to-pass test for addressing the same issue (django__django-13401).

E. Methodology for RQ4 (adequacy and hand-written tests)

Although tests are useful for finding bugs, they can be inadequate. To measure adequacy, we use code coverage, a widely used metric. Section II-D explains how our new benchmark, TDD-Bench-Verified, handles coverage in its evaluation metric and evaluation harness. Fig. 6 shows two test samples: one written by a developer and the other generated by GPT-4o. Both tests transition from fail to pass after applying the golden patch. But, are they equally adequate or good? The model-generated test is not necessarily as good as the human-written one. The original issue (django__django-13401[2]) was that fields from abstract models are considered equal across different models, which can lead to unexpected behavior when using sets or other data structures that rely on equality comparisons. The GPT-4o-generated test only asserts that the fields are not equal, whereas the developer-written test also asserts that the fields are less than each other and that their hash values are not equal. The developer-written test is thus more comprehensive as it also checks the comparison of fields and their hash values.

The coverage metric is a good indicator of such quality distinctions. For example, the coverage for these two tests is

[2]https://github.com/django/django/pull/13401

0.71 and 0.96, respectively. This research question investigates the adequacy of human-written tests $\hat{y}$ and model-generated tests y. We are interested in knowing whether they have similar and sufficient adequacy. To do so, we collect the coverage information of model-generated fail-to-pass tests and compare them with the human-written tests. To further break down the results, we follow two steps. First, we measure the coverage of model-generated fail-to-pass tests only and compare them with developer-written tests. Second, we compare coverage for other tests separately to see the coverage difference between fail-to-pass tests and other tests (i.e., fail-to-fail, pass-to-fail, or pass-to-pass).

V. Evaluation Results

This section discusses the results and our findings on the four research questions.

A. RQ1: Performance of Our Baseline and Auto-TDD

For zero-shot test file generation, we have three models, and we used the exact same prompts for all of them. We were able to generate fail-to-pass tests for 51, 57, and 84 instances for the Llama-3.1, Mistral-large, and GPT-4o models, respectively (see Table III). The values for $tddScore$ are quite close to the percentages of fail-to-pass instances (e.g., 18.7% vs. 17.2% for GPT-4o). As discussed earlier, we consider coverage in our final score. Because a few fail-to-pass tests do not have perfect coverage, it is slightly lower than the percentage of instances. We discuss more about coverage in Section V-D.

In Auto-TDD, we also used the structure of a test file to determine the location of the test case. For file localization, we use the model under consideration to select a file to write the test, and the models are quite good at this task, achieving 56%–62% Top-1 accuracy (see Table II). We also ask the model to extract relevant function names from the issue description and search for those functions in the repository. If we find the functions, we simply add them to the context. Three samples are taken from the SWE-Bench Dev split to be used as few-shot examples. These are from three completely different repositories, and there is little chance that the model will be biased by these three samples. For two models, GPT-4o and Mistral-large, Auto-TDD achieved 20–22 more fail-to-pass tests compared to our zero-shot approach. However, for Llama-3.1, Auto-TDD generates 14 fewer fail-to-pass tests than the baseline approach. There is a possibility that Llama-3.1 is being misled by the context provided for the few-shot samples. Note that the goal of few-shot samples is not to add related information here, but rather to force the model to follow a specific format for post-processing.

Distribution of generated tests: We achieved 7.8%–22.6% final scores using different models (Column 8 of Table III). We analyzed the distribution of passing and failing tests to see how many samples actually go through our whole pipeline without getting dropped because of syntactic errors. Since our full process depends on parsing and indentation of the program (it's Python!), we can lose a few tests in intermediate steps for not fully aligning with our expected format. Column 3 of Table III shows that we lost 52–69 samples for the zero-shot file generation baseline that way. Weaker models sometimes generate code that is not parseable or do not produce solutions in the expected format. However, GPT-4o and Mistral-large are relatively good at generating parseable and well-formatted tests, and we lost fewer samples compared to Llama-3.1. We also observe how the model-generated tests perform before and after the insertion of the golden code patch $\hat{c}_{\text{new}}$ (that addresses the issue). The results show that a large number of tests fail on c_{old} (243–395), but the number of tests passing after insertion is low (37–106). Note that failure on c_{old} is necessary for a test to be relevant for the corresponding issue, and GPT-4o and Mistral generate a lot of failing tests, while generating the maximum number of tests that pass on $\hat{c}_{\text{new}}$.

Comparing with Approaches Proposed by Mündler et al.*:* Mündler *et al.* [8] proposed a set of approaches for generating fail-to-pass tests. We ran Auto-TDD on their dataset to study how the approaches compare. They also have zero-shot approaches, which differ from our zero-shot baseline. Instead of generating a complete function, all of their approaches (including zero-shot ones) instruct the model to generate a specific form of "diff". Two of their approaches use a golden patch in the prompt, which resembles our "write first, test later" setting (see Section V-C). Mündler *et al.*'s SWE-agent and SWE-agent+ approaches are derived from SWE-Agent, which was originally designed for generating golden code patches [9]. Table IV shows the results. Auto-TDD performs better than their best-performing approach, generating 60 (21.7%) fail-to-pass tests compared to 53 (19.2%) fail-to-pass tests generated by SWE-agent+. Some of this gain comes from our simpler output format and some from our carefully crafted neuro-symbolic pipeline.

Finding 1. Auto-TDD is able to generate fail-to-pass tests with $tddScore$ between 7.8% and 22.6%, depending on the model. Auto-TDD improved performance for GPT-4o and Mistral-large over the baseline, but the performance decreases for Llama-3.1.

B. RQ2: Ablation of Auto-TDD

Table V shows that each component contributes to the overall performance of Auto-TDD. If we remove the relevant functions, both Mistral-Large and GPT-4o performance go down by 1.78% and 1.34% respectively. The import statements are also important and removing them results in 2.23%–3.79% performance degradation. However, the most significant component is the right file selection. When we replace the model-selected file with the BM25-retrieved file, the performance drops by 8.47%–9.13%. Because the performance of Auto-TDD is worse than the zero-shot baseline with Llama-3.1, we exclude Llama-3.1 from this experiment.

Finding 2. Each component of Auto-TDD contributes to its performance. However, LLM-based test file selection plays the most significant role.

TABLE III: Performance of the baseline technique and Auto-TDD on TDD-Bench-Verified.

Model	Technique	Syntactic Errors or Formatting Issues	On c_{old} Fail	On $\hat{c}_{\text{new}}$ Fail	On $\hat{c}_{\text{new}}$ Pass	# of Fail-to-Pass Tests in (%)	*tddScore*
Llama-3.1	Zero-shot	69	243	192	51	11.4	10.3
	Auto-TDD	75	331	294	37	8.2	7.8
Mistral-Large	Zero-shot	52	292	235	57	12.7	11.8
	Auto-TDD	11	395	318	85	18.9	18.3
GPT-4o	Zero-shot	52	273	189	84	18.7	17.2
	Auto-TDD	15	392	286	106	23.6	22.6

TABLE IV: Comparing with approaches proposed by Mündler *et al.* [8] on their 276 instances (not TDD-Bench-Verified).

Approach	# of Fail-to-pass Tests	in (%)
ZeroShot	16	5.8
ZeroShotPlus*	28	10.1
LIBRO* [5]	42	15.2
AutoCodeRover [19]	25	9.1
SWE-Agent	46	16.7
SWE-Agent+	53	19.2
Auto-TDD	**60**	**21.7**

* follows "write first, test later" approach

TABLE V: Contribution of each component of Auto-TDD.

Model	Technique	# of Fail-to-Pass Tests	# of Fail-to-Pass Tests in (%)	Change in (%)
Mistral	Auto-TDD	85	18.9	NA
	Auto-TDD - related functions	77	17.1	-1.8
	Auto-TDD - import	68	15.1	-3.8
	Auto-TDD - file detection*	44	9.8	-9.1
GPT-4o	Auto-TDD	106	23.6	NA
	Auto-TDD - related functions	100	22.3	-1.3
	Auto-TDD - import	96	21.4	-2.2
	Auto-TDD - file detection*	68	15.1	-8.5

* We use BM25 to select the file instead of relying on the model's choice.

C. RQ3: TDD-Bench for "Write First, Test Later"

As mentioned in Section IV-D, in the "write first, test later" approach, we include the code patch computed using `git diff` in the prompt, presenting the actual code change (not tests) to our techniques and observe how they performs on TDD-Bench-Verified. The key takeaway here is that, although TDD-Bench-Verified is primarily developed for test-driven development, it can also be applied in other settings. We have slightly better performance in this setup as it provides more context to the model; e.g., for GPT-4o, Auto-TDD generated 109 fail-to-pass tests in this setting compared with 106 without the code patch. One of the interesting observations is that, even in this setup, Llama-3.1's performance goes down with Auto-TDD, whereas we have seen improved performance with both Mistral-Large and GPT-4o, exactly as we have seen with the TDD setup (Table III). As expected, the final *tddScore* is also slightly on the higher side: 23.6% with GPT-4o with Auto-TDD compared to 22.6% in the TDD setting. Although we have seen improved performance in the "write first, test later" approach, the difference is not very surprising. Note that, in the current evaluation, we used the `git diff` patch format to present the changed code to the model. Alternatively, this information could be presented in a more natural way, such as including the complete function or incorporating a natural language description of the change. However, this paper is primarily focused on the TDD approach, so we leave using different presentation schemes for future research.

TABLE VI: Performance of the zero-shot baseline and of Auto-TDD in the "write first, test later" setting.

Model	Technique	# of Fail-to-Pass Tests	# of Fail-to-Pass Tests in (%)	Final Score
Llama-3.1	Zero-shot	64	14.3	13.3
	AutoTDD	37	8.2	7.8
Mistral-Large	Zero-shot	79	17.6	16.3
	AutoTDD	97	21.6	20.8
GPT-4o	Zero-shot	99	22.1	20.4
	AutoTDD	109	24.3	23.6

TABLE VII: Comparing the adequacy of model-generated and developer-written tests.

Method	Category	Model	# of Tests	Adequacy Model Generated	Adequacy Developer Written	p-value
Zero-shot	Fail-to-Pass by model	Llama-3.1	51	0.91	0.95	0.03
		Mistral-large	57	0.93	0.96	0.04
		GPT-4o	84	0.92	0.95	0.01
	Others by model	Llama-3.1	329	0.55	0.94	<0.001
		Mistral-large	340	0.59	0.94	<0.001
		GPT-4o	313	0.53	0.93	<0.001
Auto-TDD	Fail-to-Pass by model	Llama-3.1	37	0.95	0.95	0.59
		Mistral-large	85	0.97	0.99	0.04
		GPT-4o	106	0.96	0.98	0.02
	Others by model	Llama-3.1	337	0.49	0.94	<0.001
		Mistral-large	349	0.53	0.93	<0.001
		GPT-4o	332	0.52	0.93	<0.001

*p-value is computed using pairwise Wilcoxon signed-rank test.

Finding 3. TDD-Bench is also applicable to the "write first, test later" approach, and our baselines do slightly better as expected, achieving 7.8%–23.6% scores with our set of models.

D. RQ4: Test Adequacy

Table III incorporates coverage as part of the *tddScore* column. Table VII drills down deeper on coverage, and we have seen very similar statistics for both of the baseline and Auto-TDD approaches. First, even human-written tests are not perfect and, in our benchmark, *the final score achieved by* *golden tests $\hat{y}$ is* 0.94. Next, we evaluate how the model-generated tests stack up with respect to coverage. Note that, all of the golden tests are fail-to-pass, but all model-generated ones are not. So, we discuss them in two separate groups: fail-to-pass tests and other tests. For all models and approaches, we found that model-generated fail-to-pass tests achieve 0.91–0.95 coverage, which is very close to the coverage achieved by developer-written tests. To make a fair comparison, we do not consider all the samples from human-generated tests. If model M has 100 fail-to-pass tests, we collected coverage for exactly those 100 instances from the developer-written set and made the comparison. We also performed a non-parametric pairwise Wilcoxon signed rank test and failed to reject the null hypothesis at 99% confidence interval.

To summarize, we observe that the coverage achieved by model-generated tests is slightly lower than that of human-written ones, but the difference is not statistically significant. On the other hand, if the tests are not fail-to-pass, they have much lower coverage compared to human-written ones. This indicates that it is quite difficult for tests to go from fail to pass without covering the deleted and added lines between between the old code and the patched code.

Finding 4. Model-generated fail-to-pass tests achieve similar coverage as the developer-written test (above 0.9). However, for other tests (e.g., fail-to-fail), the coverage is considerably low (below 0.6). This finding is independent of the models and approaches we used.

VI. Discussion

Relation between test adequacy and correctness: Although we have seen higher coverage for fail-to-pass tests compared to other tests, coverage does not indicate correctness. We have seen 35% of non-fail-to-pass tests with perfect coverage using GPT-4o based Auto-TDD. This indicates that tests can achieve perfect coverage even without being fail-to-pass. However, we have successfully localized several issue-related functions in Auto-TDD. If we can localize the position of the lines of code that would be deleted or updated to address the issue, we can use low coverage to discard candidates that are very unlikely to go from fail to pass because, for almost all fail-to-pass tests, the coverage is above 0.90.

Uniqueness of fail-to-pass tests generated by different models: Fig. 7 shows the number of instances with fail-to-pass tests generated by different models. GPT-4o, Mistral, and Llama-3.1 can uniquely generate fail-to-pass tests for 41, 22, and 4 instances, respectively. This means there are 67 instances uniquely solved by a single model. Additionally, fail-to-pass tests for 68 instances (37+21+7+5) can be generated by multiple models. Note that the fail-to-pass tests for a single instance generated by multiple models are not the same. Each model can generate completely different fail-to-pass tests. The union of all three models can generate fail-to-pass tests for a total of 135 out of 449 (30%) instances altogether. This indicates that an ensemble approach could potentially increase the performance of Auto-TDD, though with an increase in inferencing cost. We leave this for future research.

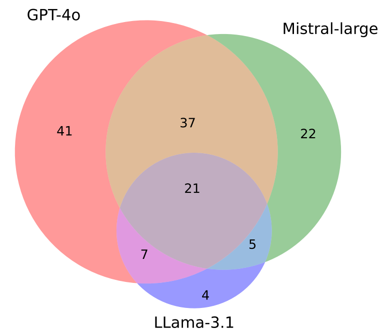

Fig. 7: Number of instances with fail-to-pass tests generated by different models.

VII. Threats to Validity

One major limitation of TDD-Bench-Verified is that it is mined from 12 popular Python repositories, so findings may not apply to other programming languages and repositories. We note that SWE-bench, despite having the same limitations, has been impactful, and one of the findings in the SWE-bench paper was that "difficulty does not correlate with issue resolution date", indicating that contamination problems (if any) are minor [3]. A limitation of Auto-TDD is that it only considers one test file or generate one block of code. In real life, test code can be spread across multiple files or blocks of code. Auto-TDD cannot generate tests that must be written across multiple files. Despite that, Auto-TDD exceeded the state-of-the-art performance, so we leave further improvements to future work. We use the Python `coverage` package for computing test coverage, but this package can fail for various reasons, such as permission issues, version incompatibility, or configuration problems. In TDD-Bench-Verified, we computed coverage for all projects, including SymPy. However, upon manual validation, we found the coverage information for SymPy to be unreliable. Therefore, we removed coverage from the final metric for SymPy instances. However, given that coverage for fail-to-pass tests was consistently above 0.9 and fewer than 15% of instances came from SymPy, this likely makes $< 1.5\%$ difference for the results.

VIII. Related Work

The introduction already discussed the most closely related work by Kang *et al.* [5], Plein *et al.* [7], and Mündler *et al.* [8]. Other related works can broadly be categorized into (a) benchmarks to evaluate the quality of code generated by various automated approaches, including LLMs, and (b) evaluations of automated test generation capabilities.

a) Code-related benchmarks: There has been a large body of works on creating benchmarks for code-related tasks. For instance, there are works on code translation [20, 21, 22], code generation [23, 24], code repair [25, 26], code summarization [16, 15], code review [27], and issue fixing [3, 28, 29, 30, 31, 32]. Among these works, the closest works are related to fixing GitHub issues. Recently, with the emergence of benchmark like SWE-bench [3], there has been a significant contribution in this direction. This includes works that enhance the SWE-Bench dataset by adding support for more programming languages [33] (arxiv), enabling dataset for multi-modal model by introducing the visual aspect of the issues [34] (arxiv), and performing more rigorous evaluation by applying various approaches and models [35] (arxiv); building agentic workflows to resolve the issues from SWE-Bench ([36, 37, 9, 38, 39, 40, 41] (arxiv) and [42]); and evaluating techniques such as chain of thoughts [43] (arxiv), understanding resolving issues [44] (arxiv), etc. Compared to these works, we focus on extending the capability of SWE-Bench to evaluate the correctness and adequacy of tests.

Also, there are works that create datasets to evaluate the quality of tests generated by LLMs [45, 46, 47]. However, when it comes to resolving specific use cases such as resolving GitHub issues, two of the closest works are done by Jain et al. [48] (arxiv) and Mündler et al. [8]. Jain et al. [48] focus on creating a parallel dataset similar to SWE-Bench but for test generation. The objective is to evaluate the capability of LLMs in test generation and test completion given a body of code. Compared to that, our objective is slightly different. Our starting point is an issue description and we focus on evaluating the quality of the tests generated by LLMs on hidden code patches. Mündler et al. [8] built SWT-Bench, a similar dataset to ours, but with less rigorous filters, leading to more but lower-quality instances than TDD-Bench-Verified. Due to the prohibitive cost of running the full SWT-Bench, Mündler et al. only experiment with a subset SWT-Bench Lite filtered to be less demanding. Also, the SWT-bench evaluation harness measures coverage in a more round-about way than TDD-Bench-Verified.

b) Evaluating test generation capability: There have been several works that attempted to use LLMs for test generations [49, 50, 51, 14, 52, 53, 54]. These works are mostly tied to the capability of LLMs to generate unit tests for Java, Python, and other programming languages. Also, there are works that create both benchmarks and evaluate the capability of LLMs in generating tests. Compared to that, our focus is on generating tests for GitHub patches and the setting of test-driven development.

IX. Conclusion

This paper contributes TDD-Bench-Verified, a challenging new benchmark for test-generation directly from issue descriptions before anyone writes the code to be tested. TDD-Bench-Verified is mined from real-world GitHub issues with strict filters and evaluation metrics. This paper also contributes Auto-TDD, an LLM-based solution for TDD-Bench-Verified that outperforms the previous state-of-the-art for this problem.

References

[1] M. Chen, J. Tworek, H. Jun, Q. Yuan, H. P. D. O. Pinto, J. Kaplan, Edwards *et al.*, "Evaluating large language models trained on code," *arXiv preprint arXiv:2107.03374*, 2021.

[2] K. Beck, *Test driven development: By example*. Addison-Wesley Professional, 2002.

[3] C. E. Jimenez, J. Yang, A. Wettig, S. Yao, K. Pei, O. Press, and K. R. Narasimhan, "Swe-bench: Can language models resolve real-world github issues?" in *The Twelfth International Conference on Learning Representations*, 2024.

[4] N. Chowdhury, J. Aung, C. J. Shern, O. Jaffe, D. Sherburn, G. Starace, E. Mays, R. Dias, M. Aljubeh, M. Glaese, C. E. Jimenez, J. Yang, K. Liu, and A. Madry, "Introducing SWE-bench Verified," Aug. 2024. [Online]. Available: https://openai.com/index/introducing-swe-bench-verified/

[5] S. Kang, J. Yoon, and S. Yoo, "Large language models are few-shot testers: Exploring LLM-based general bug reproduction," in *2023 IEEE/ACM 45th International Conference on Software Engineering (ICSE)*. IEEE, 2023, pp. 2312–2323.

[6] R. Just, D. Jalali, and M. D. Ernst, "Defects4j: A database of existing faults to enable controlled testing studies for java programs," in *Proceedings of the 2014 international symposium on software testing and analysis*, 2014, pp. 437–440.

[7] L. Plein, W. C. Ouédraogo, J. Klein, and T. F. Bissyandé, "Automatic generation of test cases based on bug reports: a feasibility study with large language models," in *International Conference on Software Engineering: Companion Proceedings (ICSE-Companion)*, May 2024, pp. 360–361. [Online]. Available: https://doi.org/10.1145/3639478.3643119

[8] N. Mündler, M. N. Müller, J. He, and M. Vechev, "SWT-bench: Testing and validating real-world bug-fixes with code agents," in *Advances in Neural Information Processing Systems (NeurIPS)*, Dec. 2024.

[9] J. Yang, C. E. Jimenez, A. Wettig, K. Lieret, S. Yao, K. Narasimhan, and O. Press, "Swe-agent: Agent-computer interfaces enable automated software engineering," *arXiv preprint arXiv:2405.15793*, 2024.

[10] R. Aleithan, H. Xue, M. M. Mohajer, E. Nnorom, G. Uddin, and S. Wang, "SWE-Bench+: Enhanced coding benchmark for LLMs," *arXiv preprint arXiv:2410.06992*, Oct. 2024.

[11] B. Peng, C. Li, P. He, M. Galley, and J. Gao, "Instruction tuning with gpt-4," *arXiv preprint arXiv:2304.03277*, 2023.

[12] S. Zhang, L. Dong, X. Li, S. Zhang, X. Sun, S. Wang, J. Li, R. Hu, T. Zhang, F. Wu *et al.*, "Instruction tuning for large language models: A survey," *arXiv preprint arXiv:2308.10792*, 2023.

[13] T. Brown, B. Mann, N. Ryder, M. Subbiah, J. D. Kaplan, P. Dhariwal, A. Neelakantan, P. Shyam, G. Sastry, A. Askell *et al.*, "Language models are few-shot learners," *Advances in neural information processing systems*, vol. 33, pp. 1877–1901, 2020.

[14] P. Bareiß, B. Souza, M. d'Amorim, and M. Pradel, "Code generation tools (almost) for free? a study of few-shot, pre-trained language models on code," *arXiv preprint arXiv:2206.01335*, 2022.

[15] T. Ahmed and P. Devanbu, "Few-shot training LLMs for project-specific code-summarization," in *Proceedings of the 37th IEEE/ACM International Conference on Automated Software Engineering*, 2022, pp. 1–5.

[16] T. Ahmed, K. S. Pai, P. Devanbu, and E. Barr, "Automatic semantic augmentation of language model prompts (for code summarization)," in *Proceedings of the IEEE/ACM 46th International Conference on Software Engineering*, 2024, pp. 1–13.

[17] N. Nashid, M. Sintaha, and A. Mesbah, "Retrieval-based prompt selection for code-related few-shot learning," in *2023 IEEE/ACM 45th International Conference on Software Engineering (ICSE)*. IEEE, 2023, pp. 2450–2462.

[18] M. R. H. Misu, C. V. Lopes, I. Ma, and J. Noble, "Towards ai-assisted synthesis of verified dafny methods," *Proceedings of the ACM on Software Engineering*, vol. 1, no. FSE, pp. 812–835, 2024.

[19] Y. Zhang, H. Ruan, Z. Fan, and A. Roychoudhury, "Autocoderover: Autonomous program improvement," in *Proceedings of the 33rd ACM SIGSOFT International Symposium on Software Testing and Analysis*, 2024, pp. 1592–1604.

[20] W. U. Ahmad, M. G. R. Tushar, S. Chakraborty, and K.-W. Chang, "Avatar: A parallel corpus for java-python program translation," *arXiv preprint arXiv:2108.11590*, 2021.

[21] J. Liu, C. S. Xia, Y. Wang, and L. Zhang, "Is your code generated by chatgpt really correct? rigorous evaluation of large language models for code generation," *Advances in Neural Information Processing Systems*, vol. 36, 2024.

[22] R. Pan, A. R. Ibrahimzada, R. Krishna, D. Sankar, L. P. Wassi, M. Merler, B. Sobolev, R. Pavuluri, S. Sinha, and R. Jabbarvand, "Lost in translation: A study of bugs introduced by large language models while translating code," in *Proceedings of the IEEE/ACM 46th International Conference on Software Engineering*, 2024, pp. 1–13.

[23] M. Chen, J. Tworek, H. Jun, Q. Yuan, H. P. D. O. Pinto, J. Kaplan, H. Edwards, Y. Burda, N. Joseph, G. Brockman *et al.*, "Evaluating large language models trained on code," *arXiv preprint arXiv:2107.03374*, 2021.

[24] J. Cao, Z. Chen, J. Wu, S.-C. Cheung, and C. Xu, "Javabench: A benchmark of object-oriented code generation for evaluating large language models," in *Proceedings of the 39th IEEE/ACM International Conference on Automated Software Engineering*, 2024, pp. 870–882.

[25] M. Martinez, T. Durieux, R. Sommerard, J. Xuan, and M. Monperrus, "Automatic repair of real bugs in java: A large-scale experiment on the defects4j dataset," *Empirical Software Engineering*, vol. 22, pp. 1936–1964, 2017.

[26] Q.-C. Bui, R. Scandariato, and N. E. D. Ferreyra, "Vul4j: A dataset of reproducible java vulnerabilities geared towards the study of program repair techniques," in *Proceedings of the 19th International Conference on Mining Software Repositories*, 2022, pp. 464–468.

[27] V. J. Hellendoorn, J. Tsay, M. Mukherjee, and M. Hirzel, "Towards automating code review at scale," in *Proceedings of the 29th ACM Joint Meeting on European Software Engineering Conference and Symposium on the Foundations of Software Engineering*, ser. ESEC/FSE 2021. New York, NY, USA: Association for Computing Machinery, 2021, p. 1479–1482. [Online]. Available: https://doi.org/10.1145/3468264.3473134

[28] T. F. Bissyandé, D. Lo, L. Jiang, L. Réveillere, J. Klein, and Y. Le Traon, "Got issues? who cares about it? a large scale investigation of issue trackers from github," in *2013 IEEE 24th international symposium on software reliability engineering (ISSRE)*. IEEE, 2013, pp. 188–197.

[29] X. Tan, M. Zhou, and Z. Sun, "A first look at good first issues on github," in *ACM Joint Meeting on European Software Engineering Conference and Symposium on the Foundations of Software Engineering*, 2020, pp. 398–409.

[30] M. Golzadeh, A. Decan, D. Legay, and T. Mens, "A ground-truth dataset and classification model for detecting bots in github issue and pr comments," *Journal of Systems and Software*, vol. 175, p. 110911, 2021.

[31] J. Zhou, S. Wang, C.-P. Bezemer, Y. Zou, and A. E. Hassan, "Studying the association between bountysource bounties and the issue-addressing likelihood of github issue reports," *IEEE Transactions on Software Engineering*, vol. 47, no. 12, pp. 2919–2933, 2020.

[32] J. Wu, H. He, W. Xiao, K. Gao, and M. Zhou, "Demystifying software release note issues on github," in *Proceedings of the 30th IEEE/ACM International Conference on Program Comprehension*, 2022, pp. 602–613.

[33] D. Zan, Z. Huang, A. Yu, S. Lin, Y. Shi, W. Liu, D. Chen, Z. Qi, H. Yu, L. Yu *et al.*, "Swe-bench-java: A github issue resolving benchmark for java," *arXiv preprint arXiv:2408.14354*, 2024.

[34] J. Yang, C. E. Jimenez, A. L. Zhang, K. Lieret, J. Yang, X. Wu, O. Press, N. Muennighoff, G. Synnaeve, K. R. Narasimhan *et al.*, "Swe-bench multimodal: Do ai systems generalize to visual software domains?" *arXiv preprint arXiv:2410.03859*, 2024.

[35] R. Aleithan, H. Xue, M. M. Mohajer, E. Nnorom, G. Uddin, and S. Wang, "Swe-bench+: Enhanced coding benchmark for LLMs," *arXiv preprint arXiv:2410.06992*, 2024.

[36] D. Chen, S. Lin, M. Zeng, D. Zan, J.-G. Wang, A. Cheshkov, J. Sun, H. Yu, G. Dong, A. Aliev *et al.*, "Coder: Issue resolving with multi-agent and task graphs," *arXiv preprint arXiv:2406.01304*, 2024.

[37] W. Tao, Y. Zhou, W. Zhang, and Y. Cheng, "Magis: LLM-based multi-agent framework for GitHub issue resolution," *arXiv preprint arXiv:2403.17927*, 2024.

[38] C. S. Xia, Y. Deng, S. Dunn, and L. Zhang, "Agentless: Demystifying LLM-based software engineering agents," *arXiv preprint arXiv:2407.01489*, 2024.

[39] N. Wadhwa, A. Sonwane, D. Arora, A. Mehrotra, S. Ut-

pala, R. B. Bairi, A. Kanade, and N. Natarajan, "Masai: Modular architecture for software-engineering ai agents," in *NeurIPS 2024 Workshop on Open-World Agents*.

[40] Y. Zhang, Y. Pan, Y. Wang, J. Cai, Z. Zheng, G. Zeng, and Z. Liu, "Pybench: Evaluating LLM agent on various real-world coding tasks," *arXiv preprint arXiv:2407.16732*, 2024.

[41] H. Ruan, Y. Zhang, and A. Roychoudhury, "Specrover: Code intent extraction via LLMs," *arXiv preprint arXiv:2408.02232*, 2024.

[42] Y. Zhang, H. Ruan, Z. Fan, and A. Roychoudhury, "AutoCodeRover: Autonomous program improvement," in *International Symposium on Software Testing and Analysis (ISSTA)*, 2024, pp. 1592–1604. [Online]. Available: https://doi.org/10.1145/3650212.3680384

[43] R. La Rosa, C. Hulse, and B. Liu, "Can github issues be solved with tree of thoughts?" *arXiv preprint arXiv:2405.13057*, 2024.

[44] Y. Ma, Q. Yang, R. Cao, B. Li, F. Huang, and Y. Li, "How to understand whole software repository?" *arXiv preprint arXiv:2406.01422*, 2024.

[45] N. Mündler, M. N. Müller, J. He, and M. Vechev, "Code agents are state of the art software testers," *arXiv preprint arXiv:2406.12952*, 2024.

[46] W. Wang, C. Yang, Z. Wang, Y. Huang, Z. Chu, D. Song, L. Zhang, A. R. Chen, and L. Ma, "Testeval: Benchmarking large language models for test case generation," *arXiv preprint arXiv:2406.04531*, 2024.

[47] M. Smytzek, M. Eberlein, B. Serce, L. Grunske, and A. Zeller, "Tests4py: A benchmark for system testing," in *Companion Proceedings of the 32nd ACM International Conference on the Foundations of Software Engineering*, 2024, pp. 557–561.

[48] K. Jain, G. Synnaeve, and B. Rozière, "Testgeneval: A real world unit test generation and test completion benchmark," *arXiv preprint arXiv:2410.00752*, 2024.

[49] J. Wang, Y. Huang, C. Chen, Z. Liu, S. Wang, and Q. Wang, "Software testing with large language models: Survey, landscape, and vision," *IEEE Transactions on Software Engineering*, 2024.

[50] M. Tufano, D. Drain, A. Svyatkovskiy, S. K. Deng, and N. Sundaresan, "Unit test case generation with transformers and focal context," *preprint arXiv:2009.05617*, 2020.

[51] V. Vikram, C. Lemieux, and R. Padhye, "Can large language models write good property-based tests?" *arXiv preprint arXiv:2307.04346*, 2023.

[52] G. Ryan, S. Jain, M. Shang, S. Wang, X. Ma, M. K. Ramanathan, and B. Ray, "Code-aware prompting: A study of coverage guided test generation in regression setting using LLM," *FSE*, 2024.

[53] M. Schäfer, S. Nadi, A. Eghbali, and F. Tip, "An empirical evaluation of using large language models for automated unit test generation," *IEEE Transactions on Software Engineering*, 2023.

[54] R. Pan, M. Kim, R. Krishna, R. Pavuluri, and S. Sinha, "Multi-language unit test generation using LLMs," *arXiv preprint arXiv:2409.03093*, 2024.