

# DyCodeEval: Dynamic Benchmarking of Reasoning Capabilities in Code Large Language Models Under Data Contamination

Simin Chen<sup>1</sup> Pranav Pusarla<sup>1</sup> Baishakhi Ray<sup>1</sup>

<sup>1</sup>Columbia University

## Abstract

The rapid evolution of code large language models (LLMs) underscores the need for effective and transparent benchmarking of their reasoning capabilities. However, the current benchmarking approach heavily depends on publicly available, human-created datasets. The widespread use of these fixed benchmark datasets makes the benchmarking process to be *static* and thus particularly susceptible to data contamination—an unavoidable consequence of the extensive data collection processes used to train Code LLMs. Existing approaches that address data contamination often suffer from human effort limitations and imbalanced problem complexity. To tackle these challenges, we propose DyCodeEval, a novel benchmarking suite for evaluating Code LLMs under potential data contamination. Given a seed programming problem, DyCodeEval employs multiple agents to extract and modify the context without altering the core logic, generating semantically equivalent variations. We introduce a dynamic data generation methods and conduct empirical studies on two seed datasets across 21 Code LLMs. Results show that DyCodeEval effectively benchmarks reasoning capabilities under contamination risks while generating diverse problem sets to ensure consistent and reliable evaluations. Our project webpage can be found at this link<sup>1</sup>.

## 1. Introduction

Large language models (LLMs) have demonstrated significant potential as assistant software developers, particularly in code generation (Chen et al., 2021; Guo et al., 2024; Jiang et al., 2024; Di et al., 2024). Consequently, numerous code-focused LLMs have been developed. These models

are trained on vast corpora of natural language and programming language data. Once well-trained, they can comprehend human instructions and generate corresponding code snippets.

As diverse model architectures and training algorithms for code LLMs continue to emerge (Vaswani et al., 2017; Shazeer et al., 2017), a key focus in code LLM research is the effective benchmarking of each model’s code reasoning capability. For instance, each model is released on HuggingFace every week. Without a standardized and transparent benchmarking suite, assessing these models’ performance and driving improvements becomes a significant challenge.

However, existing benchmarking suites for evaluating code LLMs are inadequate due to their static benchmarking schema, which can lead to potential data contamination from unintended data crawling. Research suggests that such contamination may already be present in current LLMs (Brown et al., 2020; Jain et al., 2024). Although some methods aim to provide contamination-free benchmarking for code LLMs, they still rely on manual efforts. For example, LiveCodeBench (Jain et al., 2024) proposes crawling new programming problems from online platforms and benchmarking LLMs based on timestamps, while PPM (Chen et al., 2024) attempts to systematize new programming problems by combining manually defined operators. However, these methods have several limitations: (1) *Significant Manual Effort*: These methods still require substantial manual input to create such datasets. For example, PPM necessitates manually defining the lambda operator, while LiveCodeBench shifts the burden of manual design to question authors on coding platforms. (2) *Imbalanced Semantic Complexity*: The newly generated benchmarking datasets often lack semantic equivalence with the original ones. As a result, when a model performs worse on these benchmarks, it is challenging to determine whether the lower score reflects diminished model capabilities or increased benchmark complexity. And thus, these new benchmark’s results fail to provide meaningful guidance for model developers to improve their models effectively.

<sup>1</sup><https://codekaleidoscope.github.io/dycodeeval.html>

To address this limitation, rather than manually creating benchmarking datasets with uncertain semantic complexity, we aim to develop an automated method for dynamically evaluating code LLMs. However, designing such a method presents two key challenges: (1) *Generating Semantically Diverse Yet Complexity-Controlled Problems*. The first challenge is that how to ensure the generated problems vary in semantics while maintaining controlled complexity; and (2) *Providing Comprehensive Benchmarking*. A proper benchmark programming problem must include fine-grained test cases and canonical solutions to rigorously assess correctness.

To address these challenges, we draw inspiration from metamorphic testing (Chen et al., 2018), a widely used approach in software testing to tackle the oracle problem. In our case, we leverage the principles of metamorphic testing to automate comprehensive benchmarking. Specifically, we define a metamorphic relationship for programming problems. A programming problem includes *complexity-related algorithmic abstraction* and *complexity-unrelated context description*. Modifying the *complexity-unrelated context description* alters the problem’s semantics without changing its inherent complexity. Building on this relationship, DyCodeEval employs LLM-based agents to generate diverse contexts for a seed problem, automatically transforming existing problems into semantically varied yet complexity-preserving versions. Additionally, DyCodeEval integrates a validation agent as a probabilistic oracle to verify the correctness and consistency of the newly generated problems, ensuring reliability.

We used DyCodeEval to generate new evaluation sets to assess Code LLM performance under both data contamination and real-world benchmarking scenarios. Our key findings are as follows:

1. Our method effectively reflects Code LLMs’ reasoning capabilities in a manually crafted contamination environment (§4.2).
2. The performance of Code LLMs on our dynamic benchmarks degraded significantly, suggesting potential data contamination (§4.3).
3. DyCodeEval generates semantically diverse programming problems, and its inherent randomness makes the likelihood of generating identical problems extremely low, thereby reducing the risk of data contamination (§4.4).
4. Despite its randomness, DyCodeEval consistently produces stable benchmarking results, ensuring reliable evaluation (§4.5).

We summarize our contribution as follows:

- **Novel Problem Characterization.** We identify a limitation in current static benchmarking schemas, as they are insufficient for effectively evaluating modern Code LLMs, especially when data contamination occurs and the model’s training process lacks transparency.
- **New Methodology Design.** We propose a novel approach that separates *context* and *algorithm* in programming problems. Building on this concept, we introduce a dynamic benchmarking method, DyCodeEval, which generates programming problems for benchmarking without introducing additional complexity to the dataset. This approach mitigates the impact of data contamination, ensuring transparent and reliable benchmarking.
- **Empirical Findings.** We conduct an empirical evaluation of DyCodeEval, and the results demonstrate that traditional static benchmarks can create a false sense of accuracy. In contrast, our dynamic benchmarking approach provides consistently reliable results, even under data contamination scenarios. Additionally, DyCodeEval generates semantically diverse programming problems while maintaining stable benchmarking results.

## 2. Background & Related Work

### 2.1. Benchmarking LLMs

Numerous benchmarks have appeared in order to test the effectiveness of LLMs (Hendrycks et al., 2021; Clark et al., 2018; Cobbe et al., 2021). Many emphasize an understanding of knowledge and language such as Glue (Wang et al., 2019) and SciQ (Welbl et al., 2017). Others focus on reasoning capabilities e.g. DROP (Dua et al., 2019) and BBH (Suzgun et al., 2022). And some focus on specific capabilities such as coding (Chen et al., 2021). Fig. 1 shows an example of a programming problem from a popular coding benchmark HumanEval, where a programming problem usually includes three parts: prompt, canonical solution, and test cases.

### 2.2. Data Contamination Free Benchmarking LLMs

Data contamination has become a prevalent issue in benchmarking LLMs (Brown et al., 2020; Jain et al., 2024). Many researchers have developed methods in hopes of mitigating its effect. Given the public nature of benchmarks, one set of approaches (Jacovi et al., 2023; Rajore et al., 2024) aims to implement ideas such as encryption and privatization in order to protect the data from training use. Other attempts to solve this problem include DyVal (Zhu et al., 2024a) which leverages the structure of DAGs to dynamically generate evaluation sets, TreeEval (Li et al., 2024) which uses a high performing LLM as an examiner utilizing a tree planning

Prompt	Canonical Solution	Test Cases
<pre> from typing import List def has_close_elements(numbers: List[float], threshold: float):     """     Check if in given list of numbers, are any two numbers     closer to each other than given threshold.     &gt;&gt;&gt; has_close_elements([1.0, 2.0, 3.0], 0.5)     False     &gt;&gt;&gt; has_close_elements([1.0, 2.8, 3.0, 4.0, 5.0, 2.0], 0.3)     True     """ </pre>	<pre> for idx, elem in enumerate(numbers):     for idx2, elem2 in enumerate(numbers):         if idx != idx2:             distance = abs(elem - elem2)             if distance &lt; threshold:                 return True     return False </pre>	<pre> def check(candidate):     assert candidate([1.0, 2.0, 3.9, 4.0, 5.0, 2.2], 0.3) == True     assert candidate([1.0, 2.0, 3.9, 4.0, 5.0, 2.2], 0.05) == False     assert candidate([1.0, 2.0, 5.9, 4.0, 5.0], 0.95) == True     assert candidate([1.0, 2.0, 5.9, 4.0, 5.0], 0.8) == False     assert candidate([1.0, 2.0, 3.0, 4.0, 5.0, 2.0], 0.1) == True     assert candidate([1.1, 2.2, 3.1, 4.1, 5.1], 1.0) == True     assert candidate([1.1, 2.2, 3.1, 4.1, 5.1], 0.5) == False </pre>

Figure 1. Benchmark programming problem example

strategy, and ITD (Inference Time Decontamination) (Zhu et al., 2024c) which detects and rewrites leaked samples of benchmarks without changing its complexity. Unfortunately, the limitation of these existing methods is the lack of complexity equivalence with the original problem set. Therefore, there is a need to devise a benchmark that maintains equivalence while providing contamination free data.

### 2.3. LLM as Judgement Agent

Recently, LLMs have become increasingly used as examiners given their capabilities of analyzing large amounts of data and providing unbiased assessments (Bai et al., 2023; Fernandes et al., 2023). This growing trend has gained interest for two reasons: (1) Enhanced generation of training/testing data (Li et al., 2024; Liu et al., 2024) (2) Accurate evaluation and comparison of LLM outputs such as in PandaLM (Wang et al., 2024) and DyVal (Zhu et al., 2024b). Additionally, as LLMs have been able to perform remarkably well on unseen tasks, they offer a faster, equally accurate alternative to human evaluation, (Chiang & yi Lee, 2023).

## 3. Methods: DyCodeEval

### 3.1. Design Overview

There are two key challenges in designing a dynamic evaluation schema for benchmarking code LLMs. (1) *Generating Semantically Diverse yet Complexity-Controlled Problems*: There is currently no systematic method for generating programming problems that maintain a consistent complexity level while ensuring semantic diversity. Existing approaches often rely on manual effort, either through predefined rules or domain experts, making them difficult to scale efficiently and incapable of precisely controlling problem complexity. (2) *Ensuring Comprehensive Benchmarking*: To effectively evaluate code LLMs, the generated programming problems must include fine-grained test cases and canonical solutions to rigorously assess correctness.

We draw inspiration from metamorphic testing to generate programming problems using LLMs as agents. Metamorphic testing, widely used in software engineering, defines relationships to address the automatic oracle problem. In our

approach, a programming problem prompt consists of two components: *complexity-related algorithmic abstraction* and *complexity-unrelated context description*. Our key metamorphic relationship states that modifying the *complexity-unrelated context description* preserves both the problem’s canonical solutions and complexity, enabling controlled problem generation. Additionally, since LLMs are trained on a vast diverse corpus, we can utilize them as agents to suggest relevant and meaningful *complexity-unrelated context descriptions*, further enhancing problem diversity.

The design overview of DyCodeEval is shown in Fig. 2. Given a seed programming problem from existing benchmarks, DyCodeEval generates a semantically different yet complexity-equivalent problem using a metamorphic relationship. DyCodeEval comprises four agents: (1) Scenario Proposer, (2) Context Generator, (3) Prompt Rewriter, and (4) Validator. The Scenario Proposer suggests real-world domains (e.g., banking, healthcare, education), from which DyCodeEval randomly selects one. The Context Generator then analyzes input types in the canonical solution and assigns a relevant context for each input variable based on the selected scenario. The Prompt Rewriter reformulates the problem to align with the input variable contexts and chosen scenario. Finally, the Validator ensures the new problem remains consistent with the original. If inconsistencies are detected, DyCodeEval will repeat the aforementioned process until a valid variant is produced.

### 3.2. Detailed Design

**Scenario Proposer Agent.** The Scenario Proposer enhances diversity and minimizes repetition in generated programming problems, reducing potential data contamination. It first selects scenarios from a predefined pool (e.g., banking, healthcare, education, transportation, social networking) and uses them as examples to prompt an LLM for new scenario suggestions. The newly generated scenarios are then added to the pool. By iteratively updating the pool and querying the LLM with varied examples, DyCodeEval continuously expands the scenario diversity until the scenario pool reaches a pre-defined size, ensuring the generated scenarios remain diverse and practical. The prompt used for querying the LLM and the suggested scenario examples are

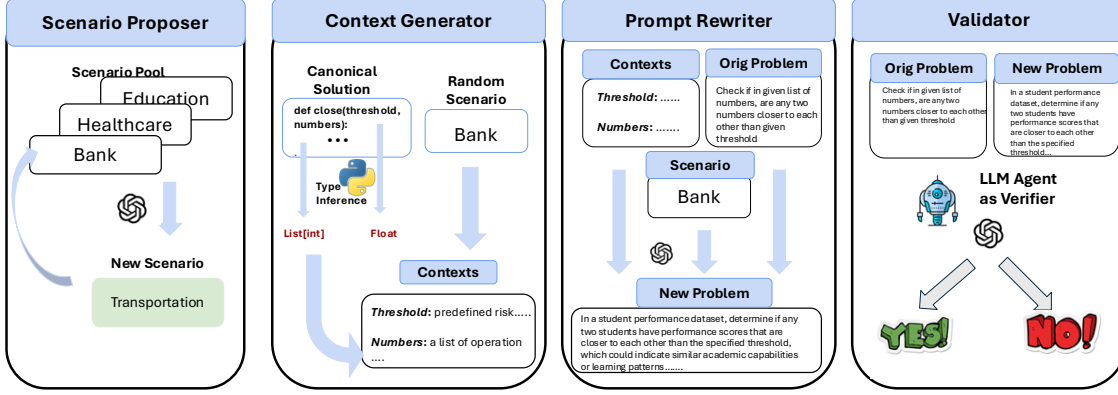


Figure 2. Design overview of DyCodeEval

**Algorithm 1** Type Inference Algorithm.     Abstract ( $\cdot$ )

**Input:** Value list  $\mathcal{V}$ .

**Output:** Set of data types  $\vec{\tau}$ .

```

1:  $\vec{\tau} = \{ \}$                                      // Initialization.
2: for each  $v$  in  $\mathcal{V}$  do
3:    $\tau = \text{Type}(v)$ 
4:   if  $\tau \in \text{Basic Types}$  then
5:      $\vec{\tau} = \vec{\tau}.add(\text{Type}(v))$ 
6:   else
7:      $\tau^* = \text{Abstract}(\text{ToList}(v))$ 
8:      $\vec{\tau}.add(\tau[\tau^*])$                            // Composite type.
9:   end if
10: end for
11: return  $\vec{\tau}$ 

```

listed in Appendix C.

**Context Generation Agent.** After proposing a set of scenarios, the context generation agent randomly selects one from the pool and assigns context information to each input variable of the programming problem based on the chosen scenario.

In languages like Python, input types are not explicitly defined. To address this, the agent uses abstraction for type inference. It analyzes ASSERT statements in test cases, collects concrete input values from the canonical solution, and abstracts the input type based on these values. Our type inference algorithm, shown in Alg. 1, works as follows: for each concrete value, it first checks if the type is a basic type (e.g., int, float). If so, it updates the type set. Otherwise the value is a composite type so it recursively iterates over all the elements and updates the type set with types like List[int] or Tuple[int | string]. Notice that while our abstract-based type inference may not capture all return value types, it is sound and guarantees that the collected types will always appear in the canonical solution.

After collecting the input data types, the agent prompts the LLM with the scenario and input type information, asking it to assign meaningful context to each input variable based on the given scenario. See Appendix C for prompt templates of our context generation.

**Prompt Rewriting Agent.** With the scenario and context information for each input variable, the prompt rewriting agent then rewrites the seed programming problem prompt to be tailored to the scenario with meaningful context. Note that we did not ask the LLM to generate the new prompt from scratch. Instead, we provided the detailed scenario and asked it to perform a rewriting task, which is simpler than a generation task. With this approach, leveraging detailed context and a more straightforward task, our agent can generate semantically diverse programming problem prompts. See Appendix C for prompt templates of our prompt rewriting.

**Validation Agent.** Although we provide the LLM with detailed scenario and context information for rewriting, there are cases where the rewriting agent unintentionally alters the consistency. To address this, we design a validation agent to assess whether the generated question maintains the integrity of the original intent and informational content. The validation prompt is designed from two angles: (1) it directs the LLM to compare the seed programming problem prompt with the rephrased prompt, ensuring the preservation of the core concept and factual accuracy, and (2) it asks the LLM to check whether the seed canonical solutions align with the generated programming problem prompt. Specifically, we design two comparison prompts to query the LLM and retain only those rewritten prompts for which both comparison responses are "YES".

To ensure the consistency of the generated programming problems, we also include a human verification step. The details of our validation prompt and the human verification process are presented in Appendix C and Appendix D.

Fig. 3 illustrates an example of programming problems that

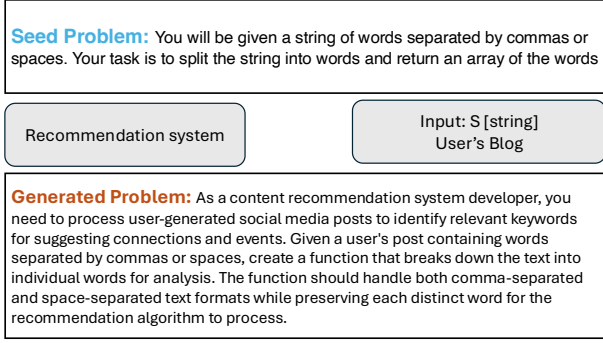


Figure 3. A generated example from DyCodeEval

are semantically diverse yet complexity-equivalent, generated under the scenario of a recommendation system with the context of a user’s blog. From this example, we observe that our step-by-step guided approach significantly enhances the semantic diversity of the generated problems, while also reducing the risk of data contamination. This is achieved by leveraging the vast combination space of scenarios and contexts.

### 3.3. Theoretical Collision Analysis

DyCodeEval generates programming problems dynamically with randomness, reducing the risk of potential data contamination. To analyze this, we conduct a collision analysis. The randomness in DyCodeEval arises from both the scenario proposal and context generation phases. We assume the scenario proposer generates  $||S||$  scenarios, and for each scenario, the context generation produces  $||C||$  contexts, while ignoring randomness in the rewriting phase. Based on this, we present the following theorem.

**Theorem 3.1.** *After running DyCodeEval  $M + 1$  times on the same seed problem, Then the probability that the  $M$  samples after the first one are all different from the first sampled item satisfies:  $P \geq 1 - \exp\left(-\frac{M}{||S|| \times ||C|| - 1}\right)$ .*

**Theorem 3.2.** *After running DyCodeEval  $M$  times on the same seed problem, If  $M \ll ||S|| \times ||C||$ , the probability of at least one collision (i.e., two or more generated problems being the same) after  $M$  generations satisfies the following bound:  $P \leq 1 - \exp\left(-\frac{M^2 - M}{2||S|| \times ||C||}\right)$ .*

**Theorem 3.3.** *Consider the seed dataset of size  $D$ , After running DyCodeEval  $M + 1$  times on this dataset, If  $M \ll ||S|| \times ||C||$ , Then the probability that the  $M$  generated dataset after the first one are all not the same as the first generated dataset satisfies:  $1 - e^{-\frac{M}{(||S|| \times ||C||)D - 1}} \leq P$*

The proof could be found in Appendix A

## 4. Evaluation

### 4.1. Experimental Setup

**Seed Dataset.** We conduct our evaluation using two datasets: *HumanEval* (Chen et al., 2021) and *MBPP-Sanitized* (Austin et al., 2021). Both datasets are widely utilized in existing research and serve as standard benchmarks for evaluating code generation models. More details about the dataset could be found in Appendix B.

**Implementation Details.** We use CLAUDE-3.5-SONNET as our foundation model to generate the benchmarking dataset. Specifically, we create 50 scenarios, and for each scenario, we randomly generate 50 contexts. During dataset generation, we set the LLM temperature to 0.8, while in our validation agent, we use a temperature of 0. For each code LLM under benchmarking, we employ  $v_{LLM}$  to launch the model. For closed-source code LLMs, we query the commercial API for evaluation.

### 4.2. Benchmarking Contaminated Model

**Models.** We conduct our study with three public-available Code LLMs: LLAMA-3.2-1B, LLAMA-3.2-3B, and DEEPSEEK-CODER-1.3B. The selected code generation models are diverse in terms of model architecture, model size, and training methods.

**Model Contamination Process.** For each model, we simulate data contamination by intentionally leaking a portion of the benchmarking dataset during fine-tuning. (1) For the static benchmarking method, we directly include part of the benchmarking dataset in fine-tuning. (2) For our method, we run it twice on each seed dataset to generate two versions of new programming problems, one for training and the other for benchmarking. We experiment with leaked data percentages of 0%, 25%, 50%, 75%, and 100%, producing four distinct contaminated models. Each polluted model is then evaluated on the benchmarking dataset using the  $PASS@1$  metric. The formal definition of  $PASS@1$  is shown in (1), where  $n$  is the number of the generated solution candidate, and  $c$  is the number of the correct solutions that can pass all test cases.

$$PASS@K = \mathbb{E}_{\text{Problems}} \left[ 1 - \frac{\binom{n-c}{k}}{\binom{n}{k}} \right] \quad (1)$$

**Main Results.** The study results are presented in Fig. 4, where there are two rows and three columns. Each column represents evaluation on a different LLM while the rows show static (first) vs dynamic (second) benchmarking. In each column, the left section displays the results for the model fine-tuned on the *HumanEval* dataset, while the right section shows the results for the model fine-tuned on the *MBPP* dataset. The red bars represent the performance

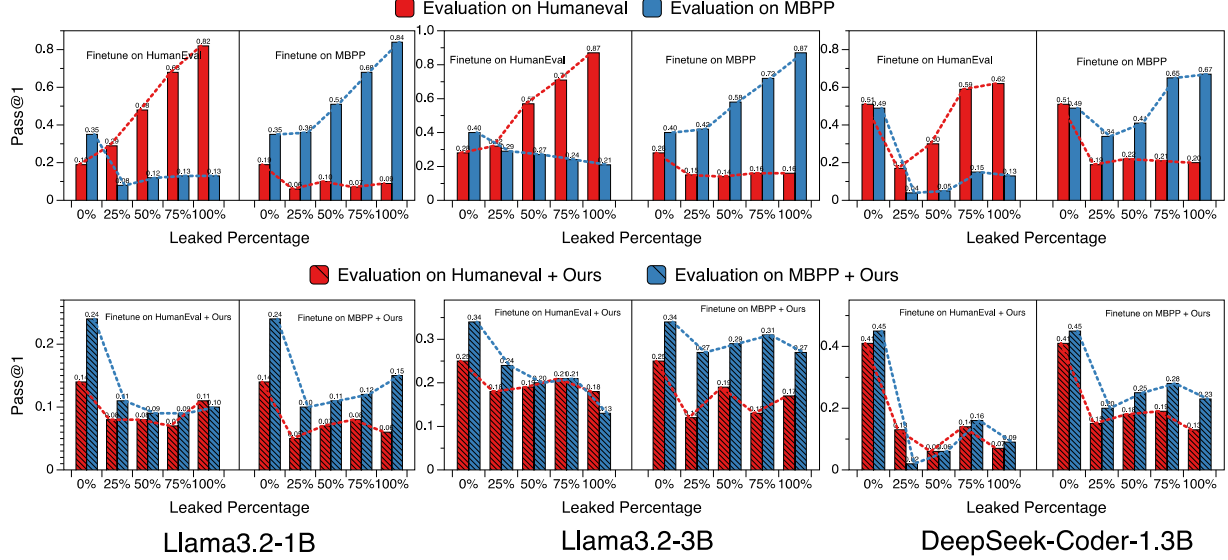


Figure 4. Results of benchmarking on contaminated models

of the fine-tuned model benchmarked on the *HumanEval* dataset, and the blue bars represent its performance benchmarked on the *MBPP* dataset.

From the results, we make the following observations: (1). Data contamination creates a false sense of code reasoning capability under static benchmarks. When the benchmarking dataset is leaked and used for fine-tuning, the model achieves a higher *Pass@1* score on the corresponding benchmark. However, this improvement does not accurately reflect the model’s true reasoning ability, as its performance declines on other benchmarks that were not included in fine-tuning. (2). Our dynamic benchmarking mitigates the impact of data contamination. Different from static benchmarks, our approach prevents contaminated models from achieving artificially high *Pass@1* scores after fine-tuning. This is due to the randomness in our method, which ensures minimal or no overlap between different runs, reducing the risk of direct data leakage. (3). Our dynamic benchmarking dataset provides results comparable to manually curated, non-contaminated datasets. In static benchmarking, as the percentage of leaked data increases, the model’s *Pass@1* score on the contaminated benchmark steadily improves. However, its performance on other benchmarks remains relatively stable, showing little variation across different contamination levels. Interestingly, this stability also applies to our method. If the base model is not contaminated on the selected seed dataset, this suggests that our approach provides competitive benchmarking results similar to those of human-curated datasets. (4). A notable anomaly is observed in DEEPSEEK-CODER. When only 25% of the benchmarking dataset is used for fine-tuning, the model’s *Pass@1* score drops below that of the original, unmodified model.

We hypothesize that the model may already be overfitted to the contaminated dataset, and further fine-tuning with limited data could destabilize this overfitting without providing enough new information to help the model adapt.

### 4.3. Benchmarking On-the-wild Model

We then apply DyCodeEval to benchmark more on-the-wild code LLMs, besides the model used in §4.2. We consider the following code LLMs: LLAMA-3.1-8B, CODELLAMA-7B, CODELLAMA-13B, DEEPSEEK-V2-LITE, DEEPSEEK-CODER-V2-LITE-BASE, LLAMA-3.1-8B-INSTRUCT, QWEN2.5-CODER-7B, QWEN2.5-7B-INSTRUCT, QWEN2.5-7B, CLAUDE-3.5-HAIKU, CLAUDE-3.5-SONNET, QWEN2.5-CODER-7B-INSTRUCT.

The results are presented in Fig. 5, with the left figure showing the results on HumanEval and the right showing the results on MBPP. In each figure, the x-axis represents the *Pass@1* scores on our generated dataset, and the y-axis represents the *Pass@1* scores on the seed dataset. The blue region corresponds to the regression area of the on-the-wild model, the red region represents the regression area of the overfitted model on this dataset, and the orange area indicates the overfitted model on the other dataset.

From these results, we observe that for both seed datasets, the on-the-wild model’s *Pass@1* scores maintain a linear relationship, while the overfitted model appears as an outlier. A notable finding from our on-the-wild evaluation is that the model QWEN2.5-CODER-7B consistently falls outside the 95% confidence interval of the regression area, suggesting it may be contaminated on both datasets.

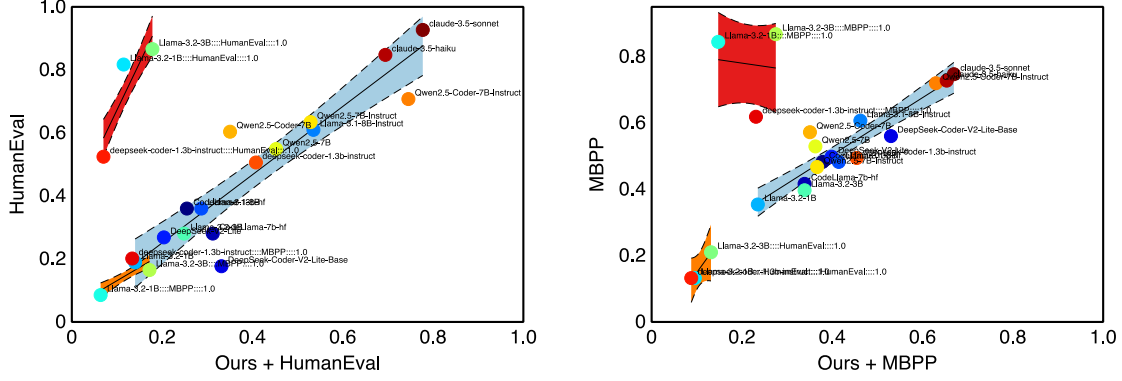


Figure 5. The on-the-wild benchmarking results

#### 4.4. Programming Problem Diversity

To evaluate the diversity of the generated programming problems, we conduct two experiments: one for external diversity and one for internal diversity. External diversity quantifies the dissimilarity between the generated and seed problems, while internal diversity measures the diversity within each problem-generation method across trials. We use two metrics: *BLEU-4* to measure syntactical diversity and *cosine similarity* of the prompt’s semantic embedding to measure semantic diversity. For semantic embedding, we use the GPT-2 model to obtain the embedding of each natural language prompt. Moreover, we also consider PPM (Chen et al., 2024) and a series of robustness-based mutation (Wang et al., 2023), such as token replacement, insert blank lines, as our comparison baseline.

The diversity results are shown in Table 1, where the first four columns represent internal diversity and the last four columns represent external diversity. From the results, we observe that DyCodeEval generates diverse programming problems both syntactically and semantically. Additionally, we find that all baseline methods exhibit high BLEU-4 and semantic similarity scores, as they rely on rule-based approaches to mutate the programming problems, which do not introduce significant diversity. In contrast, DyCodeEval leverages an LLM agent to suggest different scenarios and contexts, significantly increasing diversity.

#### 4.5. Benchmarking Stability

Note that DyCodeEval generates a unique benchmarking dataset each time. To assess its stability, we evaluate whether DyCodeEval can produce consistent benchmarking results despite this randomness. Specifically, we run DyCodeEval 10 times and measure the Pass@1 scores across these 10 generated benchmark datasets.

The mean and standard deviation of the Pass@1 scores are presented in Fig. 6. The results show that the variance

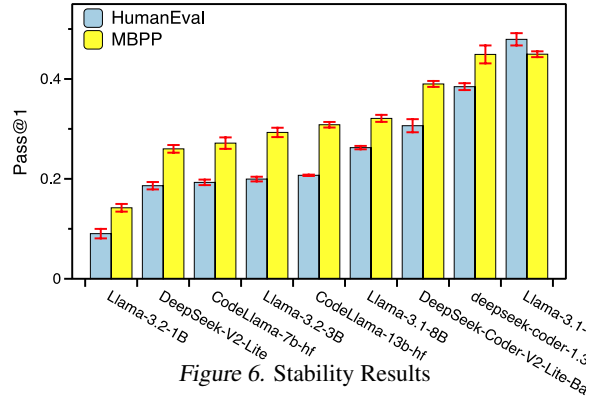


Figure 6. Stability Results

in benchmarking scores is minimal compared to the mean values, indicating that DyCodeEval provides stable benchmarking results across different random trials.

#### 4.6. Impact of Foundation LLM

In this section, we evaluate the feasibility of using less advanced LLMs to reduce dataset generation costs. Specifically, we replace our foundation model, CLAUDE-3.5-SONNET, with CLAUDE-3.5-HAIKU. We manually sample and assess generated problems from each model, checking their consistency rate. Our observations show that the consistency rate drops from 95% to 83%, highlighting the need for robust and capable LLMs to serve effectively as foundation models.

### 5. Application

Leveraging the dynamic nature of our method, we propose a new metric, DivPass, to address the limitations of the current gold standard, Pass@K. Unlike Pass@K, which generates  $n$  candidate solutions for a fixed problem and evaluates correctness, our approach creates  $n$  semantic variants of a seed problem. These variants preserve the original problem’s complexity by modifying only the description while

Table 1. The Diversity Results

Methods	Internal Diversity				External Diversity			
	HumanEval		MBPP		HumanEval		MBPP	
	BLEU-4 ↓	SemSim ↓	BLEU-4 ↓	SemSim ↓	BLEU-4 ↓	SemSim ↓	BLEU-4 ↓	SemSim ↓
Base	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Token Mutation	0.72	0.95	0.66	0.92	0.82	0.96	0.76	0.95
Char Mutation	0.81	0.97	0.78	0.94	0.84	0.97	0.78	0.92
Func Mutation	1.00	1.00	1.00	1.00	0.98	1.00	0.98	1.00
Insert Line	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
CommSyntax	1.00	1.00	1.00	1.00	0.81	0.98	0.73	0.99
PPM	0.97	0.96	0.96	0.94	0.69	0.89	0.57	0.84
Ours	<b>0.27</b>	<b>0.74</b>	<b>0.18</b>	<b>0.73</b>	<b>0.17</b>	<b>0.59</b>	<b>0.02</b>	<b>0.59</b>

maintaining the same underlying algorithmic abstraction. Additionally, the  $n$  variants expand the input space beyond that of  $\text{Pass@K}$ , making it more challenging to achieve full coverage. As a result,  $\text{DivPass}$  provides a more rigorous assessment of code LLMs’ reasoning abilities, particularly under potential data contamination. Compared to  $\text{Pass@K}$ , which evaluates solutions within a fixed problem context,  $\text{DivPass}$  introduces contextual variations during benchmarking. This allows it to better distinguish whether a model is merely memorizing problem context or genuinely reasoning to solve it.

Table 2. Comparison of  $\text{Pass@K}$  and  $\text{DivPass@K}$  under contamination

Model	Pass@K			DivPass@K		
	k=3	k=5	k=10	k=3	k=5	k=10
Llama-3.2-1B	0.22	0.27	0.34	0.17	0.21	0.26
Llama-3.2-1B (C)	0.82	0.83	0.85	0.13	0.15	0.17
Llama-3.2-3B	0.35	0.40	0.48	0.31	0.36	0.43
Llama-3.2-3B (C)	0.88	0.88	0.89	0.24	0.27	0.29

Table 3. Comparison of  $\text{Pass@K}$  and  $\text{textttDivPass@K}$  on Wild Models

Model	Pass@K			DivPass@K		
	k=3	k=5	k=10	k=3	k=5	k=10
CodeLlama-7b-hf	0.39	0.46	0.56	0.34	0.40	0.49
CodeLlama-13b-hf	0.48	0.57	0.68	0.37	0.45	0.53
Llama-3.2-1B	0.22	0.27	0.34	0.17	0.21	0.26
Llama-3.2-3B	0.35	0.40	0.48	0.31	0.36	0.43
Llama-3.1-8B	0.48	0.56	0.65	0.39	0.45	0.53
Llama-3.1-8B-Instruct	0.72	0.77	0.83	0.64	0.69	0.75

To demonstrate the advantages of  $\text{DivPass}$ , we compare it against  $\text{Pass@K}$  on both contaminated and wild models, with  $K = 3, 5, 10$  for evaluation. The results are presented in Table 3 and Table 2. From the results in Table 2, we observe that when the model is trained on leaked data, the static metric  $\text{Pass@K}$  fails to accurately reflect the model’s

reasoning capabilities, with all  $\text{Pass@K}$  scores rising to very high levels (e.g., from 0.82 to 0.89). In contrast, our dynamic metric  $\text{DivPass@K}$  shows a slight decrease rather than a significant increase, highlighting the sensitivity of  $\text{DivPass}$  to data contamination. When comparing  $\text{Pass@K}$  and  $\text{DivPass@K}$  on models that were not specifically trained on the leaked dataset, both metrics show consistency in benchmarking code LLMs. Based on these observations, we conclude that our dynamic metric,  $\text{DivPass}$ , effectively reflects the reasoning capabilities of code LLMs, even under data contamination. Moreover,  $\text{DivPass@K}$  aligns with static benchmarking metrics when there is no data contamination.

## 6. Conclusion

In this paper, we introduce  $\text{DyCodeEval}$ , a new benchmarking suite that dynamically generates semantically equivalent diverse problems as a way to combat data contamination. We break this generation up into four distinct steps to systematically develop a new programming problem with the same algorithmic complexity but different context. Our experimental results show that while  $\text{Pass@k}$  with current benchmarks have caused inflated model scores,  $\text{DyCodeEval}$ -generated questions with  $\text{DivPass}$  has proven to perform as a reliable evaluation tool. We believe that these results show a promising path forward.

Our proposed work has several limitations: (1) Although LLMs provide a fully automated way to generate diverse programming problems for benchmarking, their computational cost is a significant concern. We found that a very large LLM is required to generate programming problems with a high consistency rate. Therefore, a future improvement could focus on enhancing the efficiency of the problem generation phase. (2) While generating questions using  $\text{DyCodeEval}$ , we observed instances where excessive information was provided, potentially confusing the reader. This highlights the opportunity for improving prompt generation through further experimentation.

## Impact Statement

Assessing the overall capabilities of LLMs is crucial in order to maintain the reliability and safety of model usage in society. Data contamination, however, raises an issue by causing inflated accuracy in model evaluation. Our work proposes a new benchmark DyCodeEval, designed to accurately measure a model’s true capabilities allowing us to deepen our understanding of them.

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## A. Proof of Theorem

### A.1. Proof of Theorem 3.1

The total number of possible distinct outcomes is  $||\mathcal{S}|| \times ||\mathcal{C}||$ , the size of the random space, let  $N = ||\mathcal{S}|| \times ||\mathcal{C}||$ . Since each of the  $M$  samples must **\*\*not\*\*** match  $X_1$ , and they are drawn independently, the exact probability is:

$$P(X_2 \neq X_1, \dots, X_{M+1} \neq X_1) = \left(\frac{N-1}{N}\right)^M.$$

We use the standard inequality for the logarithm:

$$\ln(1-x) \geq -\frac{x}{1-x}, \quad \text{for } 0 < x < 1.$$

Applying this to  $\frac{1}{N}$ , we get:

$$\ln\left(\frac{N-1}{N}\right) = \ln\left(1 - \frac{1}{N}\right) \geq -\frac{1/N}{1-1/N} = -\frac{1}{N-1}.$$

Exponentiating both sides:

$$\frac{N-1}{N} \geq e^{-\frac{1}{N-1}}.$$

Raising both sides to the power  $M$ :

$$\left(\frac{N-1}{N}\right)^M \geq e^{-\frac{M}{N-1}}.$$

### A.2. Proof of Theorem 3.2

Each sampled item is drawn independently and uniformly from the space of size  $N$ . We analyze the probability that all  $M$  sampled items are distinct.

The first sample can be any of the  $N$  items, the second sample must avoid the first one, so there are  $N-1$  choices. Continuing this way, the probability that all  $M$  items are distinct is:

$$P(\text{no collisions}) = \frac{N}{N} \times \frac{N-1}{N} \times \frac{N-2}{N} \times \dots \times \frac{N-(M-1)}{N}.$$

Rewriting in factorial form,

$$P(\text{no collisions}) = \frac{N!}{N^M (N-M)!}.$$

According to our assumption  $M \ll \mathcal{S}|| \times ||\mathcal{C}||$ , Using the Stirling's approximation, then we have

$$\frac{N!}{(N-M)!} \geq N^M \exp\left(-\frac{M(M-1)}{2N}\right),$$

we get

$$P(\text{no collisions}) \geq \exp\left(-\frac{M(M-1)}{2N}\right).$$

---

The probability of at least one collision is the complement:

$$P(\text{at least one collision}) = 1 - P(\text{no collisions}).$$

Using the bound we derived,

$$P(\text{at least one collision}) \leq 1 - \exp\left(-\frac{M^2 - M}{2N}\right) = 1 - \exp\left(-\frac{M^2 - M}{2||\mathcal{S}|| \times ||\mathcal{C}||}\right)$$

### A.3. Proof of Theorem 3.3

Each sample can be represented as a  $D$ -tuple of balls  $(b_1, b_2, \dots, b_D)$ , where each  $b_i$  is one of the  $N$  balls from bag  $i$ . The total number of possible sample sets is:

$$T = N^D$$

Since each draw is independent, each sample set is chosen uniformly from  $T$ , meaning the probability of selecting any specific tuple is:

$$\frac{1}{N^D}$$

Let  $X_1$  be the initial sample (first draw). For each subsequent draw  $X_i$  (where  $i = 2, \dots, M + 1$ ), the probability that  $X_i = X_1$  (i.e., an exact match) is:

$$P(X_i = X_1) = \frac{1}{N^D}$$

Then Theorem 3.3 could be proved through Theorem 3.1.

## B. Dataset Description.

The *HumanEval* dataset, developed by OpenAI, is an open-source benchmark for evaluating the code generation capabilities of pre-trained code language models (LLMs). It comprises 164 Python programming problems, each consisting of a prompt, a canonical solution, and corresponding test inputs. Each prompt includes a natural language problem description, a function definition, and input/output examples.

The *MBPP-Sanitized* dataset, proposed by Google, features 427 Python programming problems collected through crowd-sourcing. Unlike *HumanEval*, it is a zero-shot dataset, meaning its prompts do not include input/output demonstrations. To enhance its utility in experiments, we refined the prompt format by adding function headers and converting natural language instructions into function docstrings.

## C. Prompt Templates & Scenario Examples

In the following, we show the scenario examples and prompt templates used during the four steps of DyCodeEval process.

### Scenario Examples

Real World Domain	Scenario
Banking	Predictive and Personal Detection in Financial Transactions
AI	Automated Customer Service Chatbots
Transportation	Predictive Maintenance for Fleet Vehicles
Banking	Personalized Financial Risk Assessment
Healthcare	Early Disease Prediction and Prevention
Education	Adaptive Learning Path Recommendation
Social Networking	Content Relevance and Personalization
Education	Adaptive Learning Path Personalization
Social Networking	Content Recommendation and Engagement Optimization

### Scenario Proposer

Suggest a real-world scenarios that could provide meaningful context. The scenarios should be general yet practical, covering areas such as {s1}, {s2}, {s3}, {s4}, {s5}, and other practical areas. Return each scenario on a separate line for clarity and just return the scenario without another reasoning steps.

Scenario example:

```
<example>
{exp}
< /example>
```

Suggested Real-world Scenarios:

```
<scenario>
```

### Content Generator

I have a natural language problem description, a description of the input types, and a real-world scenario. For each variable in the problem, suggest a meaningful context based on the provided scenario.

# Problem Description:

```
<problem_description>
{pb}
< /problem_description>
```

# Input Types:

```
<input_types>
{var}
< /input_types>
```

# Real-world Scenario:

```
<scenario>
{scenario}
< /scenario>
```

# Context for each Variable:

```
<context>
```

### Prompt Rewriter

---

Given an seed programming problem description, a selected real-world scenario, and contextualized input variables, rewrite the original problem to fit the new context. The rewritten problem description should preserve the original problem’s complexity and constraints while making it relevant to the given scenario. Ensure the new prompt is clear, concise, and maintains solution applicability. Just return the new rewritten problem description without any other texts.

# Problem Description:

{pb}

# Real World Scenario:

{scenario}

# Contextualized Input Variables:

{input\_variables}

# Rewritten Problem Description

### Validator 1

Assess whether the two given natural language instructions have the same meaning. Provide your answer as either 'Yes' or 'No' only.

# Instruction A:

{inst\_a}

# Instruction B:

{inst\_b}

# Your Answer:

### Validator 2

Does the following code solve the problem described in the Instruction. Provide your answer as either 'Yes' or 'No' only.

# Instruction:

{inst\_a}

# Code Solution:

{code}

# Your Answer:

## D. Human Verification

In order to add an extra level of validation between the original and generated prompts, we employ manual human verification. Given a benchmarked dataset and DyCodeEval-generated questions for each problem, we randomly pick N pairs of questions (each pair consists of a benchmarked problem and a newly generated question). Each pair of questions is then examined by two graduated student level student to determine if the core algorithm and complexity of the question is preserved, if there are inconsistency between these students, they discuss the case until the agreement is reached. Based on the radomly sampled N pairs of questions, the consistent rate is around 95%.