Beyond Synthetic Benchmarks: Evaluating LLM Performance on Real-World Class-Level Code Generation

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Large language models (LLMs) have demonstrated strong performance on function-level code generation benchmarks, yet real-world software development increasingly demands class-level implementations that integrate multiple methods, attributes, and dependencies within authentic project contexts. This gap between benchmark performance and practical utility raises critical questions about LLMs' readiness for production code assistance, particularly regarding their ability to generalize across familiar and novel codebases.

We introduce a benchmark derived from real-world open-source repositories, comprising classes divided into seen and unseen partitions to evaluate generalization under practical conditions. We systematically examine how input specification completeness and retrieval-augmented generation affect class-level correctness across multiple state-of-the-art LLMs.

Our evaluation reveals a substantial performance gap: while LLMs achieve 84–89% correctness on synthetic benchmarks, they attain only 25–34% on real-world class tasks, with minimal distinction between familiar and novel codebases. Comprehensive documentation provides marginal improvements (1–3%), whereas retrieval augmentation yields greater gains (4–7%) by supplying concrete implementation patterns. Error analysis identifies AttributeError, TypeError, and AssertionError as dominant failure modes, with distinct patterns between synthetic and real-world scenarios.

These findings provide actionable insights for enhancing context modelling, documentation strategies, and retrieval integration in production code assistance tools.

CCS Concepts: • **Software and its engineering** → **Software libraries and repositories**; *Object oriented languages*; • **Computing methodologies** → **Natural language processing**.

Additional Key Words and Phrases: Large Language Models, Code Generation, Software Repository Mining, Benchmark Dataset

1 INTRODUCTION

Large language models (LLMs) have demonstrated remarkable progress in code generation, enabling tools like GitHub Copilot [4] and Amazon CodeWhisperer [2] to assist developers in writing their code on various levels of granularity [16, 55]. However, existing benchmarks such as **CoderEval** [65] and **ClassEval** [21] primarily focus on either function-level generation or manually crafted

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class-level tasks. These benchmarks do not fully capture the complexities of **real-world software projects**, where developers frequently encounter interdependent classes, project-specific patterns, and the need to generalize to unseen codebases. The reliance on synthetic benchmarks creates a critical evaluation gap: if LLMs perform well on carefully constructed, self-contained tasks but fail on authentic production code with realistic dependencies and complexity, practitioners cannot reliably predict tool performance in actual development workflows. This disconnect between benchmark performance and real-world capability means developers may adopt LLM-based tools with inflated confidence, leading to productivity losses, increased debugging time, and potential integration of incorrect code into production systems. This gap raises a critical question: *How well do LLMs generate class-level code for real-world projects, particularly when faced with unseen contexts?* Addressing this question is essential for advancing LLM-assisted programming tools because it enables more accurate assessment of their practical capabilities, guides developers in setting appropriate expectations for tool reliability, and identifies specific technical limitations that must be overcome before these tools can be safely deployed in production environments where code correctness directly impacts software quality and development velocity.

Current research on LLM-based code generation has largely centred on function-level tasks, with benchmarks like **HumanEval** [16] and **MBPP** [11] evaluating standalone functions. While **ClassEval** [21] introduced class-level evaluation, its manually crafted tasks do not reflect the intricacies of real-world projects. Specifically, synthetic benchmarks present several fundamental limitations: they typically feature self-contained classes with minimal external dependencies, whereas real-world code heavily relies on framework integrations, third-party libraries, and project-specific utilities; they use simplified test suites with straightforward assertions, while production code requires handling complex type systems, inheritance hierarchies, and dynamic attribute resolution; and they lack the evolutionary complexity of real codebases where classes must integrate with existing architectural patterns and legacy code. These differences mean that high performance on synthetic benchmarks does not guarantee success on realistic tasks, creating a false sense of LLM capabilities. Recent work on **retrieval-augmented generation (RAG)** for code [55, 63] has shown promise in improving LLM performance by incorporating relevant context, but its application to class-level generation in real-world settings remains underexplored. Furthermore, studies on **repository-level code generation** [55] highlight the need for benchmarks that capture project-level dependencies, which are absent in existing evaluations.

We address the issues above by curating a **new benchmark of real-world class-level code** from open-source projects, categorized as **'seen'** (classes from repositories created before model training cutoffs, likely encountered during training) and **'unseen'** (classes from repositories created after training cutoffs, guaranteeing they were not in training data). Our evaluation framework assesses LLM performance across different docstring variants, generation strategies, and RAG-augmented setups. We systematically analyze the impact of docstrings, the effectiveness of RAG, and the types of errors that arise in generated code, providing insights into the strengths and limitations of current LLMs. More specifically, we investigate the following research questions:

- **RQ1 (Real-World Performance): How do LLMs perform on class-level code generation in real-world projects compared to synthetic benchmarks?** We find that LLMs achieve 84–89% correctness on synthetic benchmarks but only 24–35% on real-world code. Models show nearly identical performance on seen and unseen real-world data.
- **RQ2 (Docstring Impact): Does the presence of docstrings improve the functional correctness of LLM-generated classes?** Our finding shows that complete docstrings provide improvements of 1–3% in correctness, in some cases, although this improvement is not frequent enough to gain statistical significance.

- **RQ3 (RAG for Unseen Code): Can retrieval-augmented generation (RAG) using seen data improve LLM performance on unseen class-level tasks?** We find that RAG provides substantial benefits of 4–7% specifically when documentation is incomplete (partial docstrings), but minimal value with complete documentation. This shows that RAG helps by providing concrete implementation examples when specifications lack implementation details.
- **RQ4 (Error Analysis): What are the most common errors in LLM-generated class-level code?** We find that `AttributeError`, `TypeError`, and `AssertionError` account for 84% of all failures. Synthetic benchmarks show assertion-dominated failures while real-world code shows attribute-and-type-dominated failures, and RAG operates through error substitution, reducing logic failures while occasionally introducing dependency errors.

By answering the above RQs, this paper makes the following contributions:

(1) A curated dataset of **real-world class-level code** from open-source projects, categorized as seen and unseen.
(2) A systematic evaluation of LLM performance on class-level generation, comparing **real-world projects** to synthetic benchmarks.
(3) An analysis of the impact of **docstrings** and **RAG** on functional correctness and error types.
(4) Insights into **common errors** and practical implications for improving LLM-assisted programming tools.

Paper Structure

The remainder of this paper is organized as follows: Section 2 discusses related work and background. Section 3 describes the design of our benchmark and the overall view of the research methodology. Sections 4, 5, 6, and 7 address RQs 1, 2, 3, and 4, respectively. Section 8 discusses the implications of our findings. Section 9 describes some of the limitations of our study and outlines future directions. Section 10 discusses the threats to validity. Finally, Section 11 concludes the paper.

2 BACKGROUND AND RELATED WORK

2.1 Large Language Models for Code Generation

Large Language Models (LLMs) have achieved remarkable success in code generation, enabling tools like GitHub Copilot and Amazon CodeWhisperer to assist developers in writing functions, classes, and even entire modules [11, 16]. Recent works highlight the rapid evolution of LLMs for code-related tasks, including code completion, translation, and repair [14, 32]. Models such as GPT-4, CodeGen, and WizardCoder have demonstrated strong performance on benchmarks like HumanEval and MBPP, but their effectiveness in real-world scenarios—particularly for class-level code generation—remains underexplored [21].

To understand the limitations of current LLMs, **Wang et al.** [58] conducted an in-depth analysis of 557 incorrect code snippets generated by six prominent LLMs (CodeGen-16B, InCoder-1.3B, GPT-3.5, GPT-4, SantaCoder, and StarCoder) on the HumanEval dataset. They found that *syntactic errors*—such as missing or incorrectly structured code blocks—accounted for 40% of all errors, indicating that even advanced LLMs struggle with fundamental code structure. Furthermore, they identified common *semantic errors*, including incorrect logical flow and flawed conditional statements. Notably, GPT-4 exhibited only 7 of the 13 error categories, while smaller models displayed all or most error types, demonstrating a clear correlation between model size and error frequency. This analysis reveals that while LLMs have advanced significantly, they still face challenges in generating syntactically and semantically correct code, particularly for complex tasks. Complementing this, **Yeo et al.** [64] developed a systematic evaluation framework using LeetCode problems and found that GPT-4, when using optimal prompting strategies, outperforms

85% of human participants on coding contests. Their study revealed that prompt engineering significantly impacts LLM performance, with detailed prompts (including function signatures, input/output examples, and constraints) yielding substantially better results than basic descriptions. These findings underscore both the potential and limitations of LLMs: while they can match or exceed human performance on well-defined tasks with appropriate prompting, they struggle with structural correctness and semantic reasoning in more complex scenarios. Our study extends this understanding by evaluating LLMs on real-world class-level code generation, which presents additional challenges beyond the function-level tasks examined in prior work.

2.2   Benchmarks for Code Generation

Benchmarks serve as standardized evaluation datasets that measure LLM performance on code generation tasks, providing a crucial foundation for comparing model capabilities and tracking progress in the field. Existing benchmarks for code generation primarily focus on function-level tasks. For example, **HumanEval** [16] and **MBPP** [11] evaluate standalone functions, while **CoderEval** [65] introduces non-standalone functions but still operates at the function level. **ClassEval** [21] is the first benchmark to evaluate class-level code generation, but its tasks are manually crafted and do not reflect the complexities of real-world projects, such as cross-class dependencies and project-specific patterns.

Despite their widespread adoption, existing benchmarks face significant limitations in test coverage and real-world applicability. **Liu et al.** [38] introduced **EvalPlus**, which augments HumanEval with 80x more test cases using both LLM-based and mutation-based test generation strategies. Their extensive evaluation across 26 LLMs revealed that HumanEval's original test suites suffer from severe *test insufficiency*, allowing models to pass with fragile code that fails on edge cases. Specifically, EvalPlus reduced pass@k scores by 19.3–28.9%, exposing previously undetected errors and even altering model rankings—for instance, WizardCoder-CodeLlama and Phind-CodeLlama outperformed ChatGPT on HumanEval+ but not on the original HumanEval. This finding demonstrates that prior benchmarks significantly overestimate LLM capabilities due to weak test suites. Building on this, **Yu et al.** [66] proposed **HumanEval Pro** and **MBPP Pro**, which evaluate LLMs on *self-invoking code generation*—tasks requiring models to generate and subsequently utilize their own generated functions to solve more complex problems. Their evaluation of 20 LLMs revealed substantial performance degradation: for example, o1-mini achieved 96.2% pass@1 on HumanEval but only 76.2% on HumanEval Pro, indicating that while frontier LLMs excel at generating isolated code snippets, they struggle with reasoning about and reusing their own code. These studies collectively highlight critical gaps in current benchmarking practices: insufficient test coverage, limited evaluation of code reusability, and a focus on synthetic rather than real-world tasks. Our benchmark addresses these gaps by curating tasks from real-world repositories with comprehensive test suites, cross-class dependencies, and project-specific patterns that better reflect practical software development challenges.

2.3   Retrieval-Augmented Generation for Code

Retrieval-Augmented Generation (RAG) has emerged as a promising approach to enhance LLM performance by incorporating relevant context during generation [55, 63]. Recent studies have explored RAG for code generation, demonstrating its potential to improve accuracy and contextual relevance [35]. For instance, **CodeRAG** [35] integrates retrieval into the reasoning process, allowing LLMs to interact with source code snippets from repositories. However, most RAG studies focus on function-level or file-level tasks, and their application to class-level generation in real-world settings remains limited [55].

To systematically investigate when and how retrieval benefits code generation, **Wang et al.** [59] introduced **CodeRAG-Bench**, a comprehensive benchmark encompassing three categories of tasks: basic programming (e.g., HumanEval), open-domain programming (e.g., ODEX), and repository-level problems. They aggregated retrieval sources from five diverse datastores—competition solutions, online tutorials, library documentation, StackOverflow posts, and GitHub repositories—to evaluate 10 retrievers and 10 LLMs. Their key findings reveal that while high-quality retrieved contexts can significantly improve code generation (e.g., GPT-4 gains substantial improvements on DS-1000 and repository-level tasks), current retrieval models struggle to fetch useful contexts, especially when queries have limited lexical overlap with relevant documents. Moreover, generation models face challenges in effectively utilizing retrieved contexts due to limited context window sizes and insufficient RAG integration capabilities. This analysis demonstrates that RAG's potential for code generation remains underutilized, particularly for tasks requiring deep contextual understanding. Complementing this, **Li et al.** [35] proposed a repository-level RAG framework called **CodeRAG**, which constructs dual graphs—a requirement graph modeling functional dependencies and a DS-code graph capturing both dependency and semantic relationships—to comprehensively retrieve supportive code snippets. Their agentic approach enables LLMs to iteratively reason and retrieve relevant code, achieving substantial improvements (40.90 and 37.79 Pass@1 increases for GPT-4o and Gemini-Pro on DevEval) compared to non-RAG baselines, and even outperforming commercial tools like GitHub Copilot and Cursor. These studies collectively demonstrate that RAG significantly enhances LLM performance on complex, repository-level tasks by providing contextually relevant information. However, the application of RAG to *real-world class-level* code generation, particularly in distinguishing between seen (classes from training projects) and unseen (classes from new projects) contexts, remains an open research question. Our study addresses this gap by evaluating RAG's effectiveness on real-world class-level tasks and investigating whether retrieval strategies differ in their utility across seen versus unseen project contexts.

2.4 Docstrings and Prompt Engineering

Docstrings play a crucial role in code generation, as they provide natural language descriptions of code functionality. Studies have shown that the quality and presence of docstrings significantly impact LLM performance [48, 62]. For example, **ClassEval** [21] includes human-labelled docstrings to mitigate data leakage and improve prompt quality. Recent work on docstring compression [62] further highlights the importance of concise and informative docstrings for effective code generation. Prompt engineering techniques, such as chain-of-thought and few-shot prompting [67], have also been explored to enhance LLM performance in code-related tasks [49, 56].

Investigating the role of docstring quality in code generation, **Yang et al.** [62] proposed **ShortenDoc**, a novel compression method specifically designed for docstrings in code generation tasks. Recognizing that docstrings often contain redundant information that increases token costs and processing time, they conducted extensive experiments on six datasets using five open-source LLMs (1B–10B parameters) and GPT-4o. Their findings demonstrate that state-of-the-art generic prompt compression methods achieve only 10% reduction before causing performance degradation, whereas ShortenDoc achieves 25–40% compression while *preserving* code generation quality. This result indicates that redundancy in docstrings can be strategically reduced without sacrificing LLM understanding, thereby improving efficiency and reducing API costs—particularly important for large-scale deployments. The study reveals that not all information in docstrings contributes equally to code generation quality, and careful compression can eliminate noise while retaining essential semantic content. Expanding on the broader role of prompt engineering, **Taherkhani et al.** [53] introduced **EPiC (Evolutionary Prompt engineering for Code)**, which leverages a lightweight evolutionary algorithm to automatically refine prompts for code generation. Unlike

iterative feedback methods that require numerous LLM interactions, EPiC achieves up to 6% improvement in pass@k while being 2–10 times more cost-effective than baselines. Their approach systematically optimizes prompt phrasing, structure, and specificity using minimal LLM queries, demonstrating that prompt engineering can substantially enhance code generation without extensive computational overhead. These findings collectively emphasize that both the *content* (what information docstrings contain) and *presentation* (how prompts are structured) critically influence LLM performance. While existing work has explored docstring quality and prompt optimization for function-level tasks, their impact on *real-world class-level* code generation—where docstrings must describe complex interdependencies and longer code structures—remains underexplored. Our study investigates how different docstring qualities (skeleton-only vs. full docstrings) affect LLM performance on class-level tasks, and whether this impact varies between seen and unseen project contexts.

While existing benchmarks and techniques have advanced the field, they primarily focus on function-level or manually crafted tasks. Real-world software development involves complex, interdependent classes and project-specific patterns, which are not fully captured by current evaluations. Additionally, the application of RAG and prompt engineering to class-level code generation in real-world settings remains an open research area. This paper aims to address these gaps by evaluating LLMs on real-world class-level code generation, with a focus on the impact of docstrings, RAG, and the seen vs. unseen context.

3 METHODOLOGY

3.1 Dataset Curation

To evaluate the performance of LLMs on real-world class-level code generation, we introduce *RealClassEval*, a dataset comprising two versions of Python classes: *pre-cutoff* and *post-cutoff*. Each version contains 200 classes mined from real-world GitHub open-source projects. The complete data curation process is illustrated in Figure 1, which shows our systematic approach from initial data collection through final dataset construction.

3.1.1   Data Collection: The *pre-cutoff* data is sourced from the **CodeSearchNet** dataset [31], which is one of the most widely used datasets for training code-related LLMs. **CodeSearchNet** has been used as a pre-training corpus for numerous prominent models including StarCoder [36], CodeT5+ [57], and UniXcoder [27], and has been used in numerous existing research papers [32, 44, 47, 51]. The dataset comprises 2 million (comment, code) pairs from open-source GitHub repositories across six programming languages. Even though we cannot guarantee that this dataset was indeed part of the training data of the LLMs we investigated in this research, we believe that it is a safe assumption to make, given the widespread adoption and popularity of this dataset in the LLM training community. The *post-cutoff* data consists of repositories created after March 31, 2025 [1], guaranteeing these classes are unseen by the evaluated LLMs.

For the *post-cutoff* data collection, we used the GitHub search API to systematically identify Python repositories created after March 31, 2025. We selected repositories that: (1) were created after our cutoff date to ensure temporal separation from LLM training data, (2) had active development with at least 10 commits to ensure maturity, (3) contained Python as the primary programming language, and (4) were not forks to eliminate duplicates. This systematic selection process yielded a pool of candidate repositories from which we subsequently applied filtering criteria to identify engineered projects.

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[1]All LLMs under investigation were either released before this date or their knowledge cut-off dates are before this date.

3.1.2   Data Filtering: To ensure our dataset comprises high-quality, production-level software, we retain only *engineered projects* [43] from both the CodeSearchNet dataset and the post-cutoff repositories, adhering to the filtering criteria outlined in [61]. Importantly, we did not include all projects from CodeSearchNet; rather, we applied the same rigorous filtering criteria to both data sources to ensure consistency and quality. Specifically, we applied three filtering criteria to both *pre-cutoff* (CodeSearchNet) and *post-cutoff* (newly collected) repositories:

(1) **License Declaration**: We excluded repositories not declaring a license or using non-standard licenses (marked as "Other" in the GitHub search tool). For the remaining repositories, we removed projects declaring licenses not commonly used for software projects, such as Creative Commons Attribution licenses and font licenses, as these typically indicate documentation or non-code projects.

(2) **Project Size and Activity**: We filtered out repositories with fewer than a minimum threshold of stars (10+) and commits (50+) to ensure the projects represent mature, community-recognized software rather than personal experiments or abandoned prototypes.

(3) **Code Quality Indicators**: We excluded projects with incomplete documentation, lack of structured code organization (e.g., no clear module structure), or repositories that primarily contain tutorials, educational materials, or code snippets rather than production-quality software.

These filtering criteria, applied uniformly to both CodeSearchNet and post-cutoff repositories, ensure that our dataset comprises classes from well-maintained, professionally developed open-source projects that reflect real-world software engineering practices.

3.1.3   Static Analysis and Class Extraction: From the filtered engineered projects in both data sources, we use **Understand™** [9] to perform static analysis and identify Python classes. Class extraction is carried out using Python's `ast` library, parsing all `.py` files to extract class skeletons while preserving class and method signatures and docstrings. An example of the class skeleton extraction process is shown in Table 1, which illustrates how we transform a complete human-written class into a skeleton that serves as input for LLM code generation.

3.1.4   Post-processing and Sampling: Post-processing involves removing problematic classes (e.g., those with Python 2 syntax), ensuring only parsable and valid skeletons remain. Following *ClassEval*'s exclusion criteria [21], we omit tasks with complex execution dependencies, identified through static metrics. However, unlike *ClassEval*, we do not filter by domain, retaining a diverse range of topics such as Game Development, File Handling, and Management Systems. This approach ensures our dataset reflects the heterogeneity of real-world projects, providing a robust foundation for evaluating LLM performance on class-level code generation. From the remaining classes, we randomly sample 200 classes per version (*pre-cutoff* and *post-cutoff*) to create a balanced dataset for evaluation.

3.2   Code Generation

3.2.1   Selected LLMs. To evaluate the performance of LLMs on class-level code generation, we selected seven state-of-the-art models: **Qwen 2.5 Coder** [30], **GPT-4.1** [5], **GPT-5** [6], **GPT-OSS** [10], **Codestral** [1], **DeepSeek-V3** [37], and **Llama-4 Maverick** [7]. These models were chosen to represent a diverse range of architectures, training paradigms, and capabilities, ensuring a comprehensive evaluation of both general-purpose and code-specific LLMs, as well as models with varying reasoning abilities. They also vary in their release dates, knowledge cutoffs, parameter sizes, context window lengths, and specialization. Table 2 summarizes these characteristics, highlighting the diversity of our evaluation set.

Table 1. An example Python class and corresponding class skeleton from `codenerix/django-codenerix` [8].

| **Human-written Class** | ```python
class SubdomainMiddleware(object):
    """
    A middleware class that adds a ``subdomain`` attribute to the current request.
    """
    def get_domain_for_request(self, request):
        """
        Returns the domain that will be used to identify the subdomain part
        for this request.
        """
        return get_domain()

    def process_request(self, request):
        """
        Adds a ``subdomain`` attribute to the ``request`` parameter.
        """
        domain, host = map(lower,
            (self.get_domain_for_request(request), request.get_host()))

        pattern = r'^(?:(?P<subdomain>.*?)\.)?%s(?::.*)?$' % re.escape(domain)
        matches = re.match(pattern, host)

        if matches:
            request.subdomain = matches.group('subdomain')
        else:
            request.subdomain = None
            logger.warning('The host %s does not belong to the domain %s, '
                'unable to identify the subdomain for this request',
                request.get_host(), domain)
``` |
| **Class Skeleton** | ```python
class SubdomainMiddleware(object):
    '''
    A middleware class that adds a ``subdomain`` attribute to the current request.
    '''

    def get_domain_for_request(self, request):
        '''
        Returns the domain that will be used to identify the subdomain part
        for this request.
        '''
        pass

    def process_request(self, request):
        '''
        Adds a ``subdomain`` attribute to the ``request`` parameter.
        '''
        pass
``` |

Table 2. Characteristics of the evaluated LLMs.

Model	Release Date	Knowledge Cutoff	Parameters	Context Window	Key Characteristics
Qwen 2.5 Coder	September 2024	June 2024	32B	128K	Code-specific, optimized for code generation and reasoning
GPT-4.1	April 2025	June 2024	1.8T (estimated)	1M	General-purpose, advanced reasoning and instruction-following
GPT-5	August 2025	October 2024	2.5T (estimated)	400K	General-purpose, state-of-the-art reasoning and context handling
GPT-OSS	August 2025	June 2024	20B	128K	General-purpose (open), strong code capabilities
Codestral	January 2025 (Latest)	Unknown	100B	256K	Code-specific, specialized for code, lightweight and efficient
DeepSeek-V3	August 2025	July 2024	126B	128K	General-purpose, focused on long-context code understanding
Llama-4 Maverick	April 2025	August 2024	400B	1M	General-purpose, balanced reasoning and code generation

3.2.2 Prompt Design. We use a standardized prompt to ensure consistency across all models. The prompt is designed to instruct the LLM to implement a complete Python class based on the provided class skeleton, without additional explanations. The prompt template is as follows:

> You are an expert Python programmer who can correctly implement complete Python classes based on the provided class skeletons. Implement the following class. Do not explain the code. The given class skeleton is as follows:
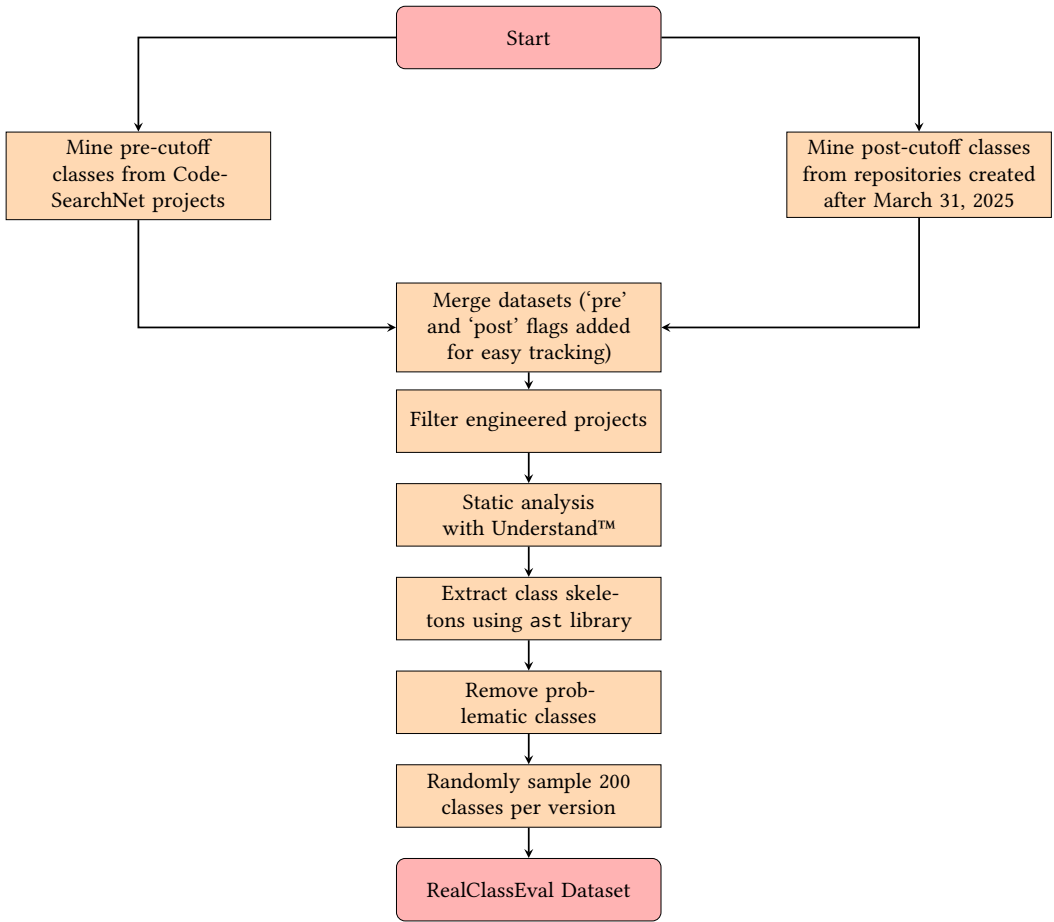> [CLASS SKELETON]

Fig. 1. Workflow for *RealClassEval* dataset curation.

Unlike *ClassEval* [21], which explores three generation strategies (holistic, incremental, and interactive), we focus exclusively on **holistic generation**. This decision is motivated by the findings of ClassEval, which indicate that models with longer context windows (such as GPT-3.5 and GPT-4 in their study) perform well in holistic generation. Given that all our selected models have substantial context windows (ranging from 128K to 1M tokens), we opt for holistic generation to leverage their ability to process and generate entire class implementations in a single pass.

3.3 Test Suite Generation

To assess functional correctness, we use **PYNGUIN** [40] to automatically generate test suites for the classes in our dataset. **PYNGUIN** (the PYthoN General UnIt test geNerator) is an automated unit test generation framework specifically designed for Python programs. Unlike test generation tools for statically typed languages like Java, **PYNGUIN** addresses the unique challenges posed by Python's dynamic typing and lack of explicit type information. The tool takes a Python module as input along with its dependencies and automatically generates unit tests that maximize code coverage using various test generation algorithms.

We specifically employ **PYNGUIN**'s **DynaMOSA** (Dynamic Many-Objective Sorting Algorithm) [45] algorithm for test generation. **DynaMOSA** is a many-objective evolutionary algorithm specifically designed for automated test case generation with dynamic selection of coverage targets. The algorithm treats test case generation as a many-objective optimization problem where each coverage target (e.g., branches, statements) is treated as a separate objective. **DynaMOSA** dynamically adjusts its focus based on the control dependency hierarchy, prioritizing uncovered targets and progressively exploring the search space. This approach has been shown to achieve higher branch coverage compared to traditional whole-suite or random test generation approaches, making it particularly suitable for evaluating the functional correctness of complex, real-world Python classes.

Given the diverse nature of real-world projects in our dataset, we lack sufficient domain knowledge to manually create meaningful test cases for each class, limiting our ability to measure correctness beyond structural similarity. **PYNGUIN** with its **DynaMOSA** algorithm provides a suitable solution as it automatically generates test cases with high branch coverage, enabling us to evaluate the functional correctness of LLM-generated classes without manual intervention. By using automated test generation, we ensure a consistent and reproducible evaluation methodology across all 400 classes in our dataset.

3.4 Result Analysis

3.4.1 Performance Analysis. To evaluate the performance of LLMs on class-level code generation, we employ **pass rate** as our primary metric, defined as the ratio of passed tests to the total number of tests for each generated class. This metric is directly related to the widely used **Pass@1** [16], which measures the probability that a generated solution passes all tests in a single attempt. While **Pass@1** is typically used for function-level evaluation, our pass rate extends this concept to class-level correctness, providing a robust measure of functional accuracy across the entire class implementation. For a given class i, the pass rate metric is defined as:

$$\text{Pass Rate}_i = \frac{\text{Number of Passed Tests}_i}{\text{Total Number of Tests}_i} \tag{1}$$

We compare one pass rate distribution against another to see:

- if a statistically significant difference exists between distributions.
- when a statistically significant difference exists, how big the difference is in terms of effect size.

We detail the statistical approaches taken to answer the RQs in their respective sections.

3.4.2 Error Analysis: We analyze the distribution of error types (e.g., `AttributeError`, `TypeError`) across data splits, docstring conditions, and RAG settings. The distributions are used to show the overall error patterns. Then we go deeper into the analysis by comparing the distributions against one another to see if they differ with enough statistical significance.

In summary, by focusing on pass rates, paired comparisons, and error distributions, we provide a comprehensive and reproducible assessment of factors influencing functional correctness in real-world settings.

4 REAL-WORLD PERFORMANCE

This section addresses RQ1: *How do Large Language Models (LLMs) perform on class-level code generation in real-world projects compared to synthetic benchmarks?* We evaluate all seven LLMs (Codestral, Deepseek-V3, GPT-4.1, GPT-5, GPT-OSS, Llama 4 Maverick, Qwen Coder 2.5) across

three datasets: *ClassEval* (100 synthetic classes), *RealClassEval* Pre-Cutoff (200 classes from Code-SearchNet, likely seen during training), and *RealClassEval* Post-Cutoff (200 classes from post-cutoff projects, unseen). The performance metric is the pass rate, defined as the proportion of passed test cases per class.

4.1 Approach

To compare LLM performance across synthetic and real-world datasets, we employed a multi-step statistical approach combining per-class and pooled analyses.

4.1.1 Data Processing. We processed test reports for each LLM per dataset. Per-class pass rates were computed for analysis. For the pooled analysis, test cases were aggregated into total passed and failed counts per dataset and LLM.

4.1.2 Kruskal-Wallis Test. We used the Kruskal-Wallis test [33] to compare per-class pass rates across the three datasets (ClassEval, Pre-Cutoff, Post-Cutoff) for each LLM. In the current context, the null hypothesis states that the pass rate distributions are identical across datasets, while the alternative hypothesis states that at least one dataset's distribution differs.

$$H_0 : F_{ClassEval} = F_{Pre-cutoff} = F_{Post-cutoff} \tag{2}$$

$$H_A : F_j \neq F_k \text{ for at least one pair } (j, k),$$
$$\text{where } j, k \in \{\text{ClassEval, Pre-cutoff, Post-cutoff}\}, j \neq k \tag{3}$$

The Kruskal-Wallis test is a non-parametric alternative to one-way ANOVA, suitable for non-normal data and independent samples across three or more groups. The non-normality of pass rates was confirmed by the Shapiro-Wilk test [50], justifying the non-parametric approach.

4.1.3 Mann-Whitney U Tests. For significant Kruskal-Wallis results, we conducted pairwise Mann-Whitney U tests [42] (ClassEval vs. Pre-Cutoff, ClassEval vs. Post-Cutoff, Pre-Cutoff vs. Post-Cutoff) to identify specific differences. The Mann-Whitney U test is a non-parametric approach suitable for unpaired samples (distinct classes across datasets) and non-normal pass rates. We applied Benjamini-Hochberg [12] False Discovery Rate (FDR) correction globally across all 21 tests (3 comparisons per LLM, 7 LLMs) to control for multiple comparisons. The pairwise tests pinpoint which datasets differ, while FDR correction balances Type I error across all tests.

4.1.4 Effect Sizes and Confidence Intervals. We computed Cliff's Delta [17, 41] to quantify the magnitude of pairwise differences, with thresholds [18, 47]: negligible (<0.147), small (<0.33), medium (<0.474), large (≥0.474). Bootstrap 95% confidence intervals (CIs) were calculated for Cliff's Delta to quantify uncertainty, using 1,000 iterations. Cliff's Delta provides an effect size for ordinal data, and CIs ensure robust estimation of differences. Although Cliff's Delta help us get the effect size of differences, it does not quantify the actual differences. Hence, we also calculate the mean differences between the distributions with bootstrap 95% CIs for an easy interpretation.

4.1.5 Pooled Analysis. We performed a chi-square analysis [22, 46] on pooled test case counts (passed vs. failed) using 2×2 contingency tables for pairwise dataset comparisons. The chi-square test is appropriate for categorical outcomes. Cramér's V [20] quantified effect sizes [18]: negligible (<0.1), small (<0.3), medium (<0.5), large (≥0.5), with bootstrap CIs for proportion differences. FDR correction was applied globally across all 21 tests. The pooled analysis provides an aggregate view of performance, complementing per-class results and ensuring robustness.
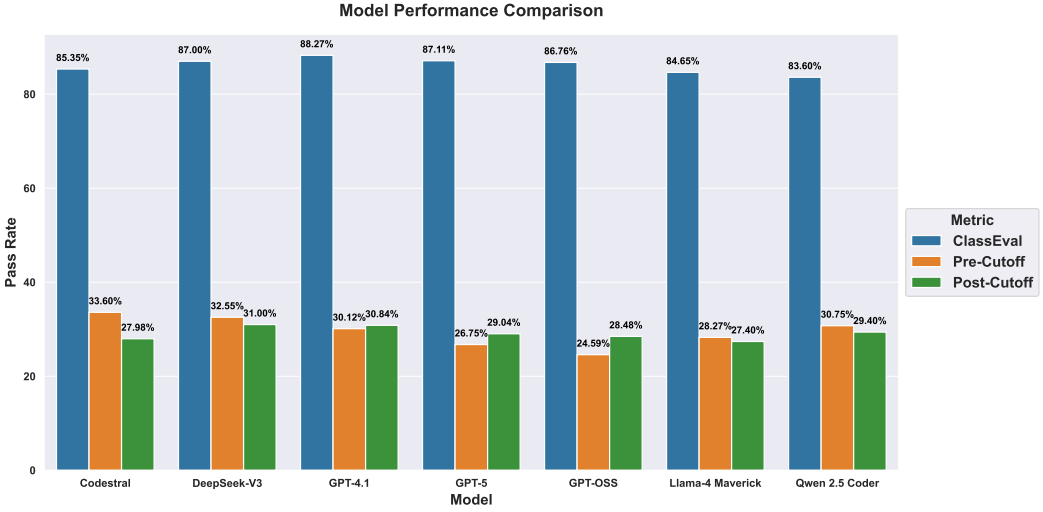
Fig. 2. Performance of different LLMs on ClassEval, Pre-cutoff, and Post-cutoff data.

4.2 Findings

The analysis revealed significant performance differences across datasets, with *ClassEval* consistently outperforming both versions of *RealClassEval*. Figure 2 shows the overall trend of performance of different LLMs across the three datasets under investigation, while Tables 3 and 4 summarize the per-class analysis result and pooled analysis result, respectively.

4.2.1 Per-Class Analysis. The Kruskal-Wallis test revealed significant differences in per-class pass rates across datasets for all LLMs (all $p < 1 \times 10^{-14}$). Pairwise Mann-Whitney U tests (Table 3) confirmed significant differences for ClassEval vs. Pre-Cutoff and ClassEval vs. Post-Cutoff, with mean pass rate differences of 46.93–61.24% and 43.96–57.05%, respectively, and medium to large effect sizes (Cliff's Delta between 0.46 and 0.64). Pre-Cutoff vs. Post-Cutoff comparisons showed no significant differences for six of seven models after FDR correction. GPT-OSS showed a statistically significant difference (FDR-adjusted $p = 0.019$), though the effect size remained negligible (Cliff's Delta = -0.125). All Pre-Cutoff vs. Post-Cutoff comparisons had negligible practical effect sizes. Power analysis across all seven models indicated high statistical power (0.974–0.983), ensuring reliable detection of differences.

4.2.2 Pooled Analysis. The chi-square analysis (Table 4) on pooled test case counts confirmed significant differences for ClassEval vs. Pre-Cutoff and ClassEval vs. Post-Cutoff, with pass rate differences of 51.75–62.17% and 54.20–58.28%, respectively, and medium to large effect sizes (Cramér's V ranging between 0.44 to 0.56). No significant differences were found for Pre-Cutoff vs. Post-Cutoff across all models (all FDR-adjusted $p > 0.07$, negligible effect sizes). Bootstrap 95% CIs confirmed that the observed gaps are robust.

4.3 Interpretation

The findings indicate that LLMs perform significantly better on synthetic benchmarks (ClassEval, 83.60–88.27%) than real-world projects (Pre-Cutoff and Post-Cutoff, 24.59–33.60%), answering RQ1. The lack of significant differences between Pre-Cutoff (seen) and Post-Cutoff (unseen) for six of seven models, combined with negligible effect sizes across all models, suggests that training

Table 3. Per-Class Pass Rate Analysis. **Bold** entries represent statistical significance.

Model	Comparison	Mean Diff (95% CI)	FDR-Adjusted p for Mann-Whitney U Tests	Effect Size
Codestral	ClassEval vs. Pre-Cutoff	46.93 [39.46, 54.93]	**1.0233e-10**	Medium
	ClassEval vs. Post-Cutoff	51.50 [44.36, 58.63]	**1.4623e-16**	Large
	Pre-Cutoff vs. Post-Cutoff	4.57 [-4.32, 13.74]	0.4623	Negligible
Deepseek-V3	ClassEval vs. Pre-Cutoff	50.50 [42.82, 57.75]	**1.9786e-11**	Medium
	ClassEval vs. Post-Cutoff	49.38 [42.82, 56.03]	**3.5676e-14**	Large
	Pre-Cutoff vs. Post-Cutoff	-1.12 [-9.49, 7.59]	0.6604	Negligible
GPT-4.1	ClassEval vs. Pre-Cutoff	59.45 [52.51, 65.89]	**6.5615e-19**	Large
	ClassEval vs. Post-Cutoff	53.17 [47.17, 59.09]	**1.7335e-19**	Large
	Pre-Cutoff vs. Post-Cutoff	-6.28 [-13.98, 2.07]	0.0613	Negligible
GPT-5	ClassEval vs. Pre-Cutoff	61.24 [54.63, 68.16]	**1.4079e-20**	Large
	ClassEval vs. Post-Cutoff	57.05 [49.81, 63.58]	**4.7559e-18**	Large
	Pre-Cutoff vs. Post-Cutoff	-4.18 [-12.33, 3.59]	0.3013	Negligible
GPT-OSS	ClassEval vs. Pre-Cutoff	58.71 [51.48, 65.82]	**4.7559e-18**	Large
	ClassEval vs. Post-Cutoff	49.66 [42.19, 56.93]	**7.1164e-16**	Large
	Pre-Cutoff vs. Post-Cutoff	-9.05 [-17.54, -0.46]	**0.0188**	Negligible
Llama 4 Maverick	ClassEval vs. Pre-Cutoff	54.25 [46.70, 61.57]	**4.4041e-16**	Large
	ClassEval vs. Post-Cutoff	49.68 [42.60, 56.61]	**7.1164e-16**	Large
	Pre-Cutoff vs. Post-Cutoff	-4.57 [-12.92, 3.78]	0.1355	Negligible
Qwen Coder 2.5	ClassEval vs. Pre-Cutoff	50.02 [42.22, 57.07]	**1.9965e-13**	Large
	ClassEval vs. Post-Cutoff	43.96 [36.70, 51.25]	**7.5609e-12**	Medium
	Pre-Cutoff vs. Post-Cutoff	-6.07 [-14.40, 2.82]	0.1200	Negligible

on **CodeSearchNet**-like data does not meaningfully improve performance on real-world class-level code generation, possibly because these datasets represent function-level code, rather than class-level code.

The high pass rate of *ClassEval* compared to *RealClassEval* was significant, but the reasons were initially unclear, as the large effect sizes (Cliff's Delta 0.46–0.64) and pass rate differences did not fully explain the gap. To investigate, we conducted a mixed-effects model [15, 34] analysis, including dataset, test count, and LLM-generated code metrics (lines of code, method count, cyclomatic complexity, and class coupling) as predictors, with LLM as a random effect. We chose these 4 static metrics because they represent both the length of the code as well as the complexity of the code. The model showed significant dataset effects (e.g., $\beta = -1.060$, $p < 0.001$) and a negative test count effect ($\beta = -0.014$, $p < 0.001$). While lines of code ($\beta = -0.002$, $p = 0.006$) and cyclomatic complexity ($\beta = 0.043$, $p < 0.001$) showed statistical significance, their effects were small relative to the dataset effect, indicating that code complexity metrics explain only a minor portion of the performance gap.

We then qualitatively analyzed test suites for randomly selected classes. The analysis revealed that *ClassEval* test suites used simple equality assertions (e.g., numerical, dictionary checks) with no external dependencies, while *RealClassEval* suites involved complex type metadata checks and system dependencies (e.g., numpy, MCPContext), leading to errors like AttributeError. This suggests that *ClassEval*'s simpler test suites, despite higher test counts, enable higher pass rates by requiring straightforward functionality, unlike *RealClassEval*'s complex, dependency-heavy tests.

Table 4. Pooled Chi-Square Analysis

Model	Comparison	Mean Diff. (95% CI)	FDR-Adjusted p	Effect Size
Codestral	ClassEval vs. Pre-Cutoff	51.75 [47.28, 56.83]	**1.9032e-106**	Medium
	ClassEval vs. Post-Cutoff	57.37 [53.22, 61.24]	**1.7756e-168**	Large
	Pre-Cutoff vs. Post-Cutoff	5.62 [-0.32, 11.33]	0.1034	Negligible
Deepseek-V3	ClassEval vs. Pre-Cutoff	54.44 [49.81, 59.06]	**4.7305e-126**	Medium
	ClassEval vs. Post-Cutoff	55.99 [52.14, 59.88]	**7.6134e-174**	Large
	Pre-Cutoff vs. Post-Cutoff	1.55 [-3.73, 7.40]	0.7654	Negligible
GPT-4.1	ClassEval vs. Pre-Cutoff	58.14 [52.86, 62.83]	**2.3701e-152**	Large
	ClassEval vs. Post-Cutoff	57.43 [53.83, 61.24]	**1.8167e-193**	Large
	Pre-Cutoff vs. Post-Cutoff	-0.71 [-6.43, 5.09]	0.8587	Negligible
GPT-5	ClassEval vs. Pre-Cutoff	60.36 [55.90, 65.02]	**7.4220e-155**	Large
	ClassEval vs. Post-Cutoff	58.06 [53.93, 61.48]	**1.0034e-191**	Large
	Pre-Cutoff vs. Post-Cutoff	-2.29 [-7.94, 3.10]	0.5707	Negligible
GPT-OSS	ClassEval vs. Pre-Cutoff	62.17 [57.34, 66.66]	**2.2409e-149**	Large
	ClassEval vs. Post-Cutoff	58.28 [54.47, 62.06]	**7.6322e-179**	Large
	Pre-Cutoff vs. Post-Cutoff	-3.89 [-9.75, 1.57]	0.2758	Negligible
Llama 4 Maverick	ClassEval vs. Pre-Cutoff	56.38 [51.37, 61.21]	**3.4048e-123**	Medium
	ClassEval vs. Post-Cutoff	57.25 [53.33, 60.92]	**2.3489e-174**	Large
	Pre-Cutoff vs. Post-Cutoff	0.87 [-4.96, 6.84]	0.8587	Negligible
Qwen Coder 2.5	ClassEval vs. Pre-Cutoff	52.85 [47.58, 57.67]	**7.2034e-106**	Medium
	ClassEval vs. Post-Cutoff	54.20 [50.31, 58.12]	**1.2561e-152**	Large
	Pre-Cutoff vs. Post-Cutoff	1.35 [-4.57, 7.58]	0.7779	Negligible

> **RQ1 Findings:** LLMs achieve significantly higher pass rates on synthetic benchmarks
> compared to real-world projects. Six of seven models show no significant performance
> differences between Pre-Cutoff and Post-Cutoff real-world data, with all models exhibiting
> negligible effect sizes, indicating that memorization does not explain real-world failures.

5 IMPACT OF DOCSTRINGS

To understand the impact of documentation on code generation quality, we conducted a comprehensive ablation study examining how varying levels of docstring completeness affect the functional correctness of LLM-generated classes. This section addresses RQ2: *Does the presence of docstrings improve the functional correctness of LLM-generated classes?* We evaluate seven LLMs (Codestral, Deepseek-V3, GPT-4.1, GPT-5, GPT-OSS, Llama 4 Maverick, Qwen Coder 2.5) across two versions of *RealClassEval*: Pre-Cutoff and Post-Cutoff. Like before, the performance metric is the pass rate, defined as the proportion of passed test cases per class.

5.1 Approach

To assess the effect of docstring completeness on LLM performance, we employed a within-subjects experimental design combining per-class and pooled analyses. The per-class analysis preserves granularity by comparing pass rates across docstring conditions, while the pooled analysis aggregates test case outcomes to assess overall trends. The following details the steps.

5.1.1 The Ablation Study. We evaluated all seven LLMs across three documentation conditions: (1) **full docstrings** containing complete documentation for all program units (class and methods), including function descriptions, parameter types, and return specifications; (2) **partial docstrings**

with documentation for some program units; and (3) **no docstrings** providing only function signatures. This design isolated the effect of documentation while controlling for snippet-specific characteristics. Each snippet was tested under all three conditions, yielding paired samples for repeated-measures analysis.

5.1.2 Friedman Test. Given the continuous and bounded nature of our outcome variable (pass rate $\in [0, 1]$) and the paired structure of our data, we employed non-parametric statistical methods. The Friedman test [24, 25] compared pass rates across three docstring conditions (full, partial, no) for each model-dataset combination, testing the null hypothesis that pass rate distributions are identical versus the alternative that at least one condition differs.

5.1.3 Wilcoxon Signed Rank Test. For significant Friedman results, post-hoc Wilcoxon signed-rank tests [60] assessed pairwise differences (full vs. partial, full vs. no). High skewness (|skewness| > 1 in 11 of 28 comparisons) prompted sign tests to validate Wilcoxon results.

Similar to RQ1, other steps of the analysis include a pooled chi-square analysis on aggregated pass-fail counts, using 2×2 contingency tables for pairwise comparisons. Benjamini-Hochberg False Discovery Rate (FDR) correction ($\alpha = 0.05$) was applied separately per dataset (7 Friedman, 14 Wilcoxon, 14 chi-square tests per dataset), treating Pre-Cutoff and Post-Cutoff as independent experiments on different code populations. Effect sizes included Cliff's Delta (negligible < 0.147, small < 0.33, medium < 0.474, large $\geq$ 0.474) and Cramér's V (negligible < 0.1, small < 0.3, medium < 0.5, large $\geq$ 0.5), with bootstrap 95% confidence intervals (CIs).

5.1.4 Temporal Analysis. A temporal analysis computed benefit ratios (# improved / # worsened) and net benefits (# improved / # unchanged × 100) for Pre-Cutoff and Post-Cutoff datasets, with a Wilcoxon test comparing model-wise ratios to assess whether docstring benefits differ between seen and unseen data.

5.2 Findings

The analysis revealed limited and model-specific effects of docstrings on code generation quality. Figure 3 shows pass rate distributions across docstring conditions, with only subtle differences between conditions. Tables 5, 6, and 7 summarize per-class, model-specific, and temporal results, respectively.

5.2.1 Per-Class Analysis. The Friedman test revealed significant differences in pass rates across docstring conditions for three model-dataset combinations after FDR correction: Codestral (Pre-Cutoff: p=1.02e-03, p_{FDR}=0.007), GPT-4.1 (Pre-Cutoff: p=2.93e-03, p_{FDR}=0.010), and Deepseek-V3 (Post-Cutoff: p=6.81e-03, p_{FDR}=0.048). However, pairwise Wilcoxon tests (Table 5) showed only two significant improvements after FDR correction: Codestral (Pre-Cutoff, full vs. partial: p_{FDR}=0.049) and Deepseek-V3 (Post-Cutoff, full vs. partial: p_{FDR}=0.048), with mean differences of -0.058 (-2.72%) and -0.047 (-1.51%) tests, respectively. The negative values indicate that full docstrings outperform partial docstrings. Notably, the full vs. no comparison yielded no significant results after FDR correction.

All Cliff's Delta values ranged from 0.003 to 0.077 (negligible effect sizes), reflecting minimal practical magnitudes. High skewness prompted sign tests for 11 comparisons, with two showing significance after FDR correction, corroborating the corresponding Wilcoxon results. The remaining 26 of 28 Wilcoxon comparisons showed no significant effects, indicating that for most models, docstrings do not significantly impact functional correctness.

5.2.2 Pooled Analysis. The chi-square analysis on pooled test case counts showed no significant differences after FDR correction for either dataset (all p_{FDR} > 0.05). Cramér's V values ranged from
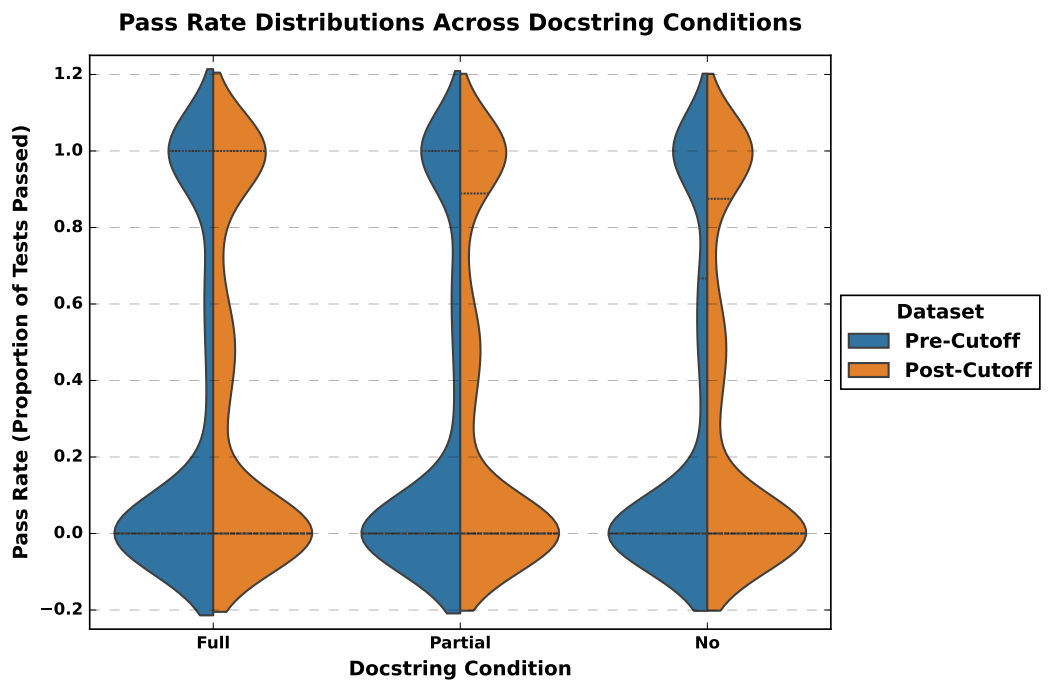
Fig. 3. Violin plot of per-class pass rate distributions across docstring conditions (full, partial, no) for Pre-Cutoff and Post-Cutoff datasets, aggregated across all models.

Table 5. Summary of docstring ablation results across all models and datasets with respect to the number of test cases. Negative differences indicate full docstrings cause more test cases to pass compared to the alternative condition.

Comparison	Mean % Diff Range	Improved	Worsened	Unchanged	Significant (after FDR Correction)	Effect Size
Full vs. Partial	-0.15% to -2.72%	97	147	2,371	2/28	Negligible
Full vs. No	-0.44% to -3.13%	113	235	2,267	0/28	Negligible
Combined	**-0.15% to -3.13%**	**210**	**382**	**4,638**	**2/56**	**Negligible**

0.002 to 0.066 (negligible effect sizes), confirming that aggregate test-level effects are minimal. This finding suggests that while individual snippets may show improvements, these effects do not accumulate to produce population-level differences in test case outcomes.

5.2.3   Model-Specific Analysis. Table 6 presents model-specific improvements with full docstrings. Only two of 14 model-dataset combinations showed statistically significant benefits: Codestral in Pre-Cutoff data (full vs. partial: 2.72%, p_{FDR}=0.049) and Deepseek-V3 in Post-Cutoff data (full vs. partial: 1.51%, p_{FDR}=0.048). The remaining 12 combinations showed no significant effects despite observed numerical differences ranging from 0.15% to 3.13%.

5.2.4   Temporal Analysis. Table 7 presents the comparative impact of docstrings across temporal boundaries. The benefit ratio (# improved / # worsened) was 1.58:1 for Pre-Cutoff data and 2.14:1 for Post-Cutoff data, representing a numerical difference of 35%. However, a Wilcoxon test comparing

Table 6. Model-specific performance improvements with full docstrings. Percentages represent pass rate increases when using full documentation relative to partial or no docstrings. **Bold** entries achieved statistical significance after FDR correction ($p_{FDR} < 0.05$).

Model	Dataset	Full vs. Partial (%)	Full vs. No (%)	Average Benefit (%)
Codestral	Pre-Cutoff	**2.72**	3.13	2.93
	Post-Cutoff	1.01	0.80	0.90
Deepseek-V3	Pre-Cutoff	0.93	1.59	1.26
	Post-Cutoff	**1.51**	1.32	1.42
GPT-4.1	Pre-Cutoff	0.51	2.11	1.31
	Post-Cutoff	1.02	0.90	0.96
GPT-5	Pre-Cutoff	0.71	1.59	1.15
	Post-Cutoff	0.15	0.93	0.54
GPT-OSS	Pre-Cutoff	1.33	1.58	1.45
	Post-Cutoff	0.32	0.63	0.48
Llama 4 Maverick	Pre-Cutoff	1.88	2.58	2.23
	Post-Cutoff	0.64	0.44	0.54
Qwen Coder 2.5	Pre-Cutoff	0.41	1.91	1.16
	Post-Cutoff	0.93	1.36	1.15

Table 7. Temporal analysis of docstring impact. Benefit ratio is the ratio of classes that improved to classes that worsened with full docstrings. The Wilcoxon test comparing Pre-Cutoff vs. Post-Cutoff benefit ratios across models was non-significant.

Dataset	Classes Helped	Classes Hurt	Benefit Ratio	Net Benefit (%)
Pre-Cutoff	210 (8.9%)	133 (5.6%)	1.58:1	3.3%
Post-Cutoff	172 (7.1%)	80 (3.3%)	2.14:1	3.8%

model-wise benefit ratios was non-significant (p=0.326), indicating no statistically reliable difference in docstring effectiveness between pre-cutoff and post-cutoff data. This null result reflects substantial model-specific variability: Codestral showed stronger Post-Cutoff benefits (benefit ratio 0.61 post vs. 0.30 pre for full vs. no), while Deepseek-V3 showed the opposite pattern (0.37 post vs. 0.60 pre). The lack of a consistent temporal effect suggests that docstring effectiveness depends primarily on model-specific factors rather than whether code was in the training data.

5.3 Interpretation

The findings provide limited evidence that docstrings improve LLM code generation quality, addressing RQ2 with a nuanced answer: **for most models, docstrings do not significantly improve functional correctness**. Only 2 of 7 models (Codestral and Deepseek-V3) showed statistically significant benefits in specific conditions, and even these effects were negligible in magnitude. This pattern, combined with adequate statistical power (0.78–0.87) obtained from power analysis, suggests that docstring effects are genuinely minimal rather than obscured by insufficient sample sizes.

5.3.1 Interpreting Limited Effects. The lack of widespread significance after FDR correction indicates that raw numerical improvements (averaging 1.25% across all comparisons) are largely

attributable to random variation and multiple testing rather than true effects. The fact that pooled chi-square tests found zero significant results reinforces this interpretation: individual snippet-level fluctuations do not aggregate into reliable population-level patterns.

The high skewness in 11 of 28 comparisons reveals that most classes show zero difference across docstring conditions, with occasional large outliers driving mean differences. This suggests docstring benefits may be highly context-dependent, helping only in specific scenarios rather than providing universal improvements. The negligible Cliff's Delta values (all < 0.15) confirm minimal practical effect sizes, even for the two statistically significant findings.

5.3.2 Model-Specific Heterogeneity. The restriction of significant effects to two models (Codestral and Deepseek-V3) suggests that docstring processing may depend on model architecture or training procedures. Possible explanations include:

(1) **Training data composition**: Models trained on repositories with comprehensive docstring conventions may learn stronger associations between documentation and correct implementations.
(2) **Attention mechanisms**: Architectural differences in how models weight docstring tokens versus signature tokens could affect documentation utilization.
(3) **Model capacity**: Smaller or more constrained models may rely more heavily on explicit documentation, while larger models can infer intended behavior from signatures alone.

The fact that five of seven models showed no significant effects despite testing on the same code snippets indicates that documentation alone is insufficient for most current LLMs. This contrasts with conventional software engineering wisdom emphasizing documentation importance [13, 52], suggesting that LLMs may process code differently than human developers.

Future research should investigate: (1) whether specific docstring formats (e.g., type hints vs. natural language descriptions) provide differential benefits; (2) whether docstrings interact with other contextual factors like code complexity; (3) why certain models (Codestral, Deepseek-V3) show sensitivity while others do not; and (4) whether docstring benefits emerge for more complex classes beyond our dataset's scope.

> **RQ2 Findings:** Comprehensive statistical analysis with appropriate multiple comparison corrections found significant improvements in only 2 of 7 models, and only for the full vs. partial comparison. Effect sizes were uniformly negligible.

6 IMPACT OF RETRIEVAL-AUGMENTED GENERATION (RAG)

This section investigates whether retrieval-augmented generation (RAG) can further enhance functional correctness for unseen code by providing relevant examples from seen data. RQ3: *Can retrieval-augmented generation (RAG) using seen data improve LLM performance on unseen class-level code generation tasks?* We evaluate the same seven LLMs (Codestral, Deepseek-V3, GPT-4.1, GPT-5, GPT-OSS, Llama 4 Maverick, Qwen Coder 2.5) on the *RealClassEval* Post-Cutoff dataset with and without RAG-retrieved examples. The examples were retrieved from *RealClassEval* Pre-cutoff data. The same performance metric of pass rate was used.

6.1 Approach

To assess RAG's impact on unseen code generation, we employed a controlled experimental design comparing LLM performance with and without retrieved documentation from seen data.

6.1.1   Data Preparation. The evaluation utilized *RealClassEval*'s Post-Cutoff dataset, ensuring all test cases were from projects created after model training cutoffs. For each target class skeleton, RAG retrieved the top $k = 2$ most similar class skeletons from Pre-Cutoff data using cosine similarity on embeddings (obtained from **GraphCodeBert** [28]), and then, appended their complete implementations (signature, docstrings, and body) to the prompt. The choice of $k = 2$ is motivated by the fact that the length of the prompt can get very long depending on the implementation of the example classes. Since the API service providers of the LLMs charge not only based on the output produced, but also based on the length of input, it was beyond our budget to use $k > 2$.

6.1.2   Study Design. Like RQ2, three docstring conditions were tested to investigate RAG's interaction with documentation completeness: (1) **full docstrings**; (2) **partial docstrings**; and (3) **no docstrings**. Each condition was evaluated both with RAG (retrieved examples included) and without RAG, yielding paired samples for within-subjects analysis.

6.1.3   Statistical Analysis. No new statistical methodology has been used in RQ3 that has not been used in the previous two RQs. All the statistical tests applied in this study have been discussed in the previous section(s). Here is the quick summary of what steps we have taken to answer RQ3.

Given the paired structure (each snippet tested with and without RAG) and non-normal pass rate distributions, we again applied the Wilcoxon signed-rank test to compare pass rate differences (RAG - non-RAG) for each model-docstring combination. To control Type I errors while preserving statistical power, we applied the Benjamini-Hochberg False Discovery Rate (FDR) correction ($\alpha = 0.05$) *separately for each docstring condition* (7 model comparisons per condition). The decision to apply FDR correction separately per docstring type (rather than globally across all 21 tests) warrants justification. We treat the three docstring conditions as substantively distinct experimental contexts: full docstrings provide complete specifications (testing RAG's value when information is abundant), partial docstrings create information gaps (testing RAG's gap-filling capability), and no docstrings represent minimal guidance (testing RAG's effectiveness without structural anchors). These conditions address fundamentally different research sub-questions about RAG's mechanism, making separate FDR families appropriate. This choice was made because we want to understand RAG's effect on different docstring types individually, and not collectively.

FDR-adjusted p-values are reported, with statistical significance defined as $p_{FDR} < 0.05$. Cliff's Delta measured effect size (negligible < 0.147, small < 0.33, medium < 0.474, large $\geq$ 0.474) with 95% bootstrap confidence intervals. For distributions with high skewness (|skewness| > 1), sign tests validated Wilcoxon results. Power analysis for paired designs estimated the study's ability to detect observed effect sizes.

6.2   Findings

RAG demonstrates significant benefits for code generation with partial docstrings, with 5 of 7 models showing statistically significant improvements after FDR correction. Benefits are negligible with full docstrings and marginal with no docstrings, revealing a critical interaction between RAG and documentation completeness. Table 8 summarizes results across docstring conditions, while Table 9 presents detailed model-specific findings organized by documentation level.

6.2.1   RAG with Full Docstrings: No Significant Benefits. When complete documentation is provided, RAG shows no significant improvements across all seven models (Table 9). Mean improvements range from -0.8% (GPT-5) to 3.1% (Deepseek-V3), with all $p_{FDR} > 0.21$. This pattern suggests that when models receive comprehensive docstrings, retrieved examples provide minimal additional value.

Table 8. Summary of RAG effects across docstring conditions on Post-Cutoff dataset. Positive mean differences indicate RAG outperforms non-RAG. Per-type FDR correction applied separately to each condition (7 tests per condition).

Docstring Condition	Mean Percentage Point Improvement	Improved	Worsened	Significant (p_{FDR}<0.05)
Full	1.55	81	51	0/7
Partial	4.25	125	55	5/7
No	3.58	141	77	0/7
Overall	**3.11**	**347**	**183**	**5/21**

6.2.2 RAG with Partial Docstrings: Strong Significant Benefits. RAG demonstrates its strongest and most consistent benefits when documentation is incomplete, with 5 of 7 models achieving statistical significance after FDR correction (Table 9). Significant improvements range from 4.2% (Llama 4 Maverick, p_{FDR}=0.024) to 6.9% (Deepseek-V3, p_{FDR}=0.023), with adequate statistical power (0.634−0.838) supporting these findings. Codestral (5.3%, p_{FDR}=0.030), GPT-4.1 (4.6%, p_{FDR}=0.023), and Qwen Coder 2.5 (5.2%, p_{FDR}=0.024) also show significant benefits. Only GPT-5 shows negative effects (-0.6%, p_{FDR}=0.739), suggesting model-specific architectural differences in utilizing retrieved context.

All Cliff's Delta values, however, remain negligible (0.049−0.078), indicating modest practical effect sizes despite statistical significance. However, power analysis confirms adequate detection capability for the observed effects (power 0.634−0.838 for significant results), validating that these are genuine improvements rather than false positives.

6.2.3 RAG with No Docstrings: Marginal Non-Significant Benefits. Without documentation, RAG shows positive trends for most models but fails to achieve statistical significance after FDR correction. Deepseek-V3 (5.8%, p_{raw}=0.016, p_{FDR}=0.057) and Qwen Coder 2.5 (5.9%, p_{raw}=0.014, p_{FDR}=0.057) approach significance, while Codestral shows a strong trend (5.7%, p_{raw}=0.061, p_{FDR}=0.142). Power analysis reveals moderate detection capability (0.550−0.725), suggesting these effects may be genuine but require larger samples for definitive confirmation.

6.3 Interpretation

The findings reveal a critical interaction between RAG and documentation completeness, answering RQ3: RAG significantly improves code generation quality for unseen tasks, but *only when documentation is incomplete*. This "information gap hypothesis" [26, 39] suggests RAG's primary value lies in compensating for missing context rather than enhancing already-comprehensive specifications.

6.3.1 The Information Gap Hypothesis. The striking pattern—5/7 models significant with partial docstrings, 0/7 with full docstrings, 0/7 with no docstrings—supports a specific mechanistic interpretation. When documentation is complete, models already possess sufficient behavioural specifications; retrieved examples offer redundant information. When documentation is absent, models lack structured guidance to effectively leverage retrieved examples—the context is too noisy without docstrings to anchor understanding. The "sweet spot" occurs with partial docstrings: enough structure to interpret retrieved examples, but insufficient detail to implement correctly without them.

6.3.2 Practical Significance Despite Negligible Effect Sizes. Although all Cliff's Delta values are negligible (<0.147), the improvements have practical importance. A 4−7% pass rate increase translates

Table 9. Model-specific RAG effects organized by docstring condition. A positive difference means RAG performed better, and vice versa. **Bold** entries indicate statistical significance.

Docstring Type	Model	Mean Pass Rate % Diff.	FDR-Adjusted p	Improved/ Worsened
Full	Codestral	1.79	0.536	11/6
	Deepseek-V3	3.07	0.536	16/9
	GPT-4.1	0.84	0.617	6/4
	GPT-5	-0.77	0.798	12/13
	GPT-OSS	2.37	0.536	17/8
	Llama 4 Maverick	2.33	0.536	13/6
	Qwen Coder 2.5	0.94	0.617	6/5
Partial	Codestral	5.27	**0.030**	18/6
	Deepseek-V3	6.94	**0.023**	22/7
	GPT-4.1	4.63	**0.023**	16/5
	GPT-5	-0.58	0.739	15/18
	GPT-OSS	3.26	0.212	21/11
	Llama 4 Maverick	4.25	**0.024**	18/4
	Qwen Coder 2.5	5.18	**0.024**	15/4
No	Codestral	5.74	0.142	22/8
	Deepseek-V3	5.84	0.057	23/11
	GPT-4.1	2.75	0.357	18/13
	GPT-5	1.07	0.667	20/18
	GPT-OSS	2.20	0.488	18/12
	Llama 4 Maverick	2.15	0.357	18/8
	Qwen Coder 2.5	5.93	0.057	22/7

to 0.12−0.28 additional test cases passed per class with a very restrictive assumption of mean test counts of 3−4 per class. For large codebases generating thousands of classes, this compounds to substantial quality gains. Moreover, combining RQ2's docstring benefits (mean 1.25%) with RQ3's RAG benefits (4.25% for partial docstrings) yields a potential 5.5% cumulative improvement—equivalent to approximately 0.19 tests per class, or roughly 190 additional correct implementations per 1,000 generated classes. Additionally, RQ2 showed benefits of up to 2.7% with full docstrings, while RQ3 shows 4.3−6.9% with partial docstrings plus RAG. This suggests RAG and documentation are complementary strategies, with RAG providing greater relative benefits when documentation is lacking.

6.3.3 Model-Specific Patterns and GPT-5's Anomaly. Five models (Codestral, Deepseek-V3, GPT-4.1, Llama 4 Maverick, Qwen Coder 2.5) show consistent positive responses to RAG with partial docstrings, while GPT-5 exhibits negative effects (-0.8% full, -0.6% partial). This suggests architectural differences in context utilization. GPT-5 may suffer from "context interference"—retrieved examples conflicting with its training-based priors—or may weight retrieved context differently than documentation. GPT-OSS shows positive trends (3.3% partial) but insufficient power for significance, suggesting moderate responsiveness.

The consistency of the pattern across five diverse architectures (transformer variants, different training datasets, varying parameter counts) strengthens the generalizability of the information

gap hypothesis. RAG's effectiveness is not model-specific but rather a property of the interaction between retrieval quality and documentation completeness. However, we do acknowledge that further investigation is required to conclusively state why these inter-model differences are being observed.

6.3.4 Interpret with Caution. It is worth mentioning that power analysis revealed that this study's power varies substantially across conditions (0.059–0.838), with full and no docstring conditions showing inadequate power (mostly <0.30) to detect true effects. This explains why full docstrings show no significant results despite positive trends—the study cannot reliably distinguish small RAG benefits from noise. However, the partial docstring condition achieves adequate power (0.634–0.838) for significant results, validating these findings. Future work should increase sample sizes to achieve higher power for detecting true effects across all conditions.

> **RQ3 Findings:** This study provides the first systematic evidence that RAG's effectiveness in code generation depends critically on documentation completeness, with the strongest benefits occurring when models receive partial documentation. This finding bridges RQ2's insights on documentation effects and suggests a path toward more efficient code generation.

7 ERROR ANALYSIS

To understand the nature of failures in LLM-generated code, this section addresses RQ4: *What are the most common errors in LLM-generated class-level code?* We analyze error distributions across all experimental conditions from RQ1–RQ3, examining *all failed test cases* across seven LLMs, three datasets, and multiple documentation and retrieval configurations. We characterize overall error patterns, compare synthetic versus real-world failure modes, and investigate how docstrings and RAG affect error types.

7.1 Approach

To comprehensively characterize error patterns in LLM-generated code, we employed a multi-level statistical analysis examining error type distributions across experimental conditions.

7.1.1 Error Classification and Data Processing. We extracted error types from test execution reports across all conditions, parsing exception types from failure messages. Errors were classified into Python's standard exception taxonomy: `AttributeError`, `TypeError`, `AssertionError`, `ValueError`, `NameError`, `KeyError`, `IndexError`, `ZeroDivisionError`, `FileNotFoundError`, `ImportError`, `RuntimeError`, and `SyntaxError`. Non-standard errors, such as API specific exceptions, were aggregated into an "Other" category. For each failed class, we tabulated error counts by type, aggregating across test cases to create error frequency distributions.

7.1.2 Statistical Analysis. We applied chi-square tests of independence [23, 46] to compare error type distributions across conditions, appropriate for categorical error frequency data. The analysis comprised four complementary comparisons:

Overall Distribution Analysis: Chi-square goodness-of-fit test [19] against uniform distribution was applied to confirm non-uniform error patterns in order to establish that error types are non-randomly distributed across failures. The larger the chi-squared value, the greater the deviation from the uniform distribution, implying that there is a meaningful pattern worth investigating.

In our context, this test can be realized as follows: Let p_i denote the probability of observing error type i where $i \in \{\text{AttributeError}, \text{TypeError}, \ldots, \text{Other}\}$. We tested:

$$H_0 : p_i = \frac{1}{13} \quad \forall i \in \{1, 2, \ldots, 13\}$$

$$H_A : \exists i \text{ such that } p_i \neq \frac{1}{13}$$

(4)

with test statistic $\chi^2 = \sum_{i=1}^{13} \frac{(O_i - E_i)^2}{E_i}$ where O_i is the observed count and $E_i = \frac{\sum_{i=1}^{13} O_i}{13}$ is the expected count under uniform distribution. The test has $df = 12$ degrees of freedom.

Like previous RQs, Cramér's V for effect size and bootstrap 95% confidence intervals (CIs) to quantify heterogeneity were also applied.

Dataset-Wise Comparison: Pairwise chi-square tests were applied to compare error distributions across ClassEval, Pre-Cutoff, and Post-Cutoff datasets for each model (3 comparisons × 7 models = 21 tests). Global Benjamini-Hochberg FDR correction ($\alpha = 0.05$) controlled Type I error across all tests, consistent with RQ1 methodology. Cramér's V quantified effect sizes with thresholds: negligible (<0.1), small (<0.3), medium (<0.5), large (≥0.5).

Docstring Impact: Chi-square tests were again applied to compare error distributions for full vs. partial and full vs. no docstrings within Pre-Cutoff and Post-Cutoff datasets separately (2 comparisons per model-dataset pair). Per-dataset FDR correction (14 tests per dataset) followed RQ2's approach of treating temporal contexts as distinct experimental families.

RAG Impact: Another set of Chi-square tests was applied to compare RAG vs. non-RAG error distributions within each docstring condition for Post-Cutoff data (7 models per condition). Per-docstring-condition FDR correction (7 tests per condition) mirrored RQ3's methodology, treating documentation levels as substantively different information environments.

For all comparisons, we computed statistical power to detect observed effect sizes, ensuring reliable inference. Bootstrap CIs (1,000 iterations) provided robust uncertainty estimates for effect sizes. To contextualize statistical findings, we computed percentage point differences in error type frequencies, identifying which errors increase or decrease across conditions.

7.2 Findings

The analysis revealed three major patterns: (1) a consistent error signature dominated by `AttributeError`, `TypeError`, and `AssertionError`; (2) fundamental differences in error types between synthetic benchmarks and real-world code; and (3) RAG-induced error substitution where retrieval prevents certain failures while introducing others. Table 10 summarizes the overall distribution, Tables 11 and 12 detail dataset comparisons, and Table 13 characterizes RAG's error substitution mechanism.

7.2.1 Overall Error Distribution. Across all failed test cases, three error types dominate LLM-generated code failures (Table 10). `AttributeError` accounts for 43.84% of all errors, `TypeError` for 21.65%, and `AssertionError` for 18.51%. These top three error types collectively represent 84% of all failures, indicating a concentrated failure signature. Chi-square goodness-of-fit testing against a uniform distribution confirmed highly non-uniform patterns ($\chi^2 = 37847.3$, $p < 10^{-300}$), with "Other" errors contributing 10.42%. Notably, `SyntaxError` accounted for 0% of failures, demonstrating that modern LLMs generate syntactically valid Python code consistently across all experimental conditions.

7.2.2 Dataset-Wise Comparison: Synthetic vs. Real-World Error Patterns. Error distributions differ fundamentally between synthetic benchmarks and real-world projects (Figure 4), with large effect sizes across all model-dataset comparisons (Table 11). All 21 pairwise comparisons showed statistically significant differences after FDR correction. ClassEval vs. Pre-Cutoff and ClassEval vs. Post-Cutoff comparisons yielded large effect sizes (Cramér's V: 0.59–0.85, all $p_{\text{FDR}} < 10^{-116}$), while

Table 10. Overall error type distribution across all conditions.

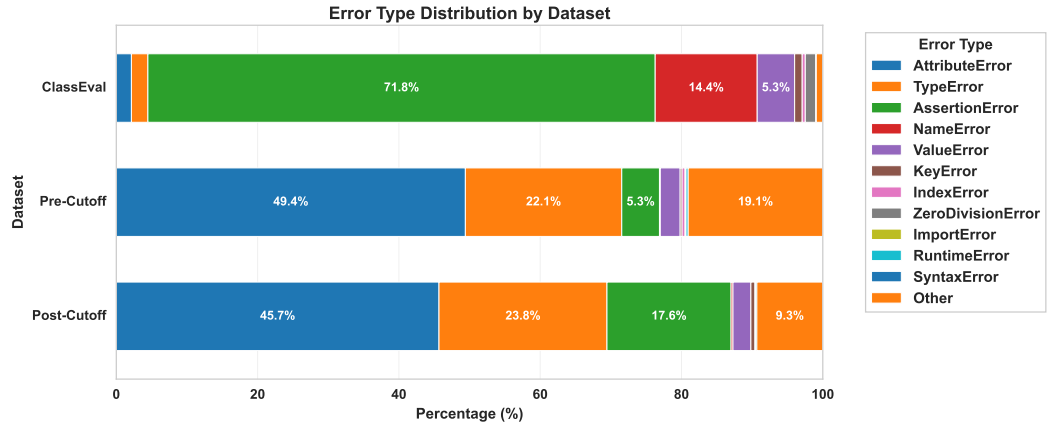| Error Type	Count	Percentage (%)
AttributeError	11,270	43.84
TypeError	5,566	21.65
AssertionError	4,759	18.51
Other	2,679	10.42
ValueError	702	2.73
NameError	339	1.32
KeyError	156	0.61
FileNotFoundError	89	0.35
RuntimeError	46	0.18
ZeroDivisionError	39	0.15
IndexError	38	0.15
ImportError	22	0.09
SyntaxError	0	0.00

Fig. 4. Distribution of Error Types Across Datasets.

Pre-Cutoff vs. Post-Cutoff comparisons showed smaller but still significant differences (Cramér's V: 0.22–0.28, all $p_{\text{FDR}} < 10^{-18}$). Statistical power exceeded 0.99 for all tests, ensuring reliable detection of effects.

Detailed analysis of error type frequencies by dataset (Table 12) reveals the source of these differences. ClassEval's error profile is dominated by AssertionError (71.80%), reflecting the logic-focused nature of synthetic test suites, with NameError (14.44%) as the second most common failure. On the other hand, real-world datasets show AttributeError dominance (49.41% Pre-Cutoff, 45.67% Post-Cutoff) and substantial TypeError frequencies (22.14% Pre-Cutoff, 23.78% Post-Cutoff), while AssertionError drops to 5.34% (Pre-Cutoff) and 17.57% (Post-Cutoff).

Quantifying these differences reveals striking patterns. ClassEval produces 66.47 percentage points (pp) more AssertionError than Pre-Cutoff and 54.24 pp more than Post-Cutoff, while showing 47.27 pp fewer AttributeError than Pre-Cutoff and 43.53 pp fewer than Post-Cutoff.

Table 11. Dataset-wise error distribution comparison with global FDR correction (21 tests). All comparisons are significant after correction.

Model	Comparison	p_{raw}	p_{FDR}	Effect Size
Codestral	ClassEval vs Pre-Cutoff	5.46e-144	**1.04e-143**	Large
	ClassEval vs Post-Cutoff	3.10e-116	**4.65e-116**	Large
	Pre-Cutoff vs Post-Cutoff	1.01e-28	**1.32e-28**	Small
Deepseek-V3	ClassEval vs Pre-Cutoff	5.91e-164	**2.07e-163**	Large
	ClassEval vs Post-Cutoff	1.75e-142	**3.07e-142**	Large
	Pre-Cutoff vs Post-Cutoff	3.23e-30	**4.52e-30**	Small
GPT-4.1	ClassEval vs Pre-Cutoff	1.01e-164	**4.23e-164**	Large
	ClassEval vs Post-Cutoff	1.43e-136	**2.32e-136**	Large
	Pre-Cutoff vs Post-Cutoff	3.89e-24	**4.80e-24**	Small
GPT-5	ClassEval vs Pre-Cutoff	2.17e-176	**2.72e-175**	Large
	ClassEval vs Post-Cutoff	2.52e-157	**7.56e-157**	Large
	Pre-Cutoff vs Post-Cutoff	5.45e-24	**6.35e-24**	Small
GPT-OSS	ClassEval vs Pre-Cutoff	1.76e-168	**9.25e-168**	Large
	ClassEval vs Post-Cutoff	4.94e-145	**1.04e-144**	Large
	Pre-Cutoff vs Post-Cutoff	3.77e-18	**3.77e-18**	Small
Llama 4 Maverick	ClassEval vs Pre-Cutoff	2.59e-176	**2.72e-175**	Large
	ClassEval vs Post-Cutoff	4.71e-148	**1.10e-147**	Large
	Pre-Cutoff vs Post-Cutoff	4.33e-21	**4.79e-21**	Small
Qwen Coder 2.5	ClassEval vs Pre-Cutoff	6.69e-176	**4.68e-175**	Large
	ClassEval vs Post-Cutoff	3.04e-150	**7.97e-150**	Large
	Pre-Cutoff vs Post-Cutoff	1.36e-19	**1.42e-19**	Small

NameError is 14.32 pp more common in ClassEval than Pre-Cutoff and 14.17 pp more than Post-Cutoff. Conversely, real-world code exhibits 19.80–21.45 pp more TypeError than ClassEval. These differences directly explain RQ1's performance gap: synthetic benchmarks test logical correctness through assertions, while real-world projects require correct object attribute access and type consistency—capabilities LLMs struggle to master.

The small but significant differences between Pre-Cutoff and Post-Cutoff (Cramér's V: 0.22–0.28) contrast with the negligible pass rate differences found in RQ1. While seen and unseen real-world code exhibit similar overall correctness, they show subtle error pattern variations. Pre-Cutoff code produces more "Other" errors (18.29% vs. 9.16%), suggesting idiosyncratic project-specific failures in older codebases, while Post-Cutoff shows slightly more AssertionError (17.57% vs. 5.34%), potentially reflecting evolution in testing practices toward more explicit assertions in recent projects.

7.2.3 Docstring Impact on Error Patterns. Similar to the docstrings' effect on overall pass rate (RQ2), they do not significantly affect error type distributions either. Of 28 pairwise comparisons (full vs. partial, full vs. no, across 7 models and 2 datasets), none achieved statistical significance after FDR correction (all $p_{\text{FDR}} > 0.05$). Effect sizes ranged from negligible to small (Cramér's V: 0.04–0.18), with most comparisons showing negligible effects. Power analysis revealed, on average, moderate detection capability (power: 0.19–0.92), suggesting that observed non-significance reflects a genuine absence of large effects.

Table 12. Error type distributions by dataset (%). ClassEval shows AssertionError dominance, while real-world datasets show AttributeError and TypeError prominence.

Error Type	ClassEval	Pre-Cutoff	Post-Cutoff
AssertionError	71.80	5.34	17.57
AttributeError	2.14	49.41	45.67
TypeError	2.33	22.14	23.78
NameError	14.44	0.12	0.27
Other	0.63	18.29	9.16
ValueError	5.30	2.81	2.53
ZeroDivisionError	1.51	0.14	0.00
KeyError	1.07	0.27	0.61
IndexError	0.44	0.42	0.01
FileNotFoundError	0.29	0.76	0.18
RuntimeError	0.05	0.30	0.20
ImportError	0.00	0.00	0.03
SyntaxError	0.00	0.00	0.00

This finding indicates that docstrings do not change error types even if, in isolated cases, they improve the pass rate. Models make the same categories of mistakes—primarily AttributeError, TypeError, and AssertionError—regardless of documentation completeness. Documentations affect all error types proportionally, rather than preventing or increasing specific failure modes selectively.

7.2.4 RAG Error Substitution Mechanism. An overall pattern of RAG's effect on error distributions are shown in Figure 5. RAG not only improves pass rates with partial docstrings (RQ3) but also significantly alters error type distributions for three models: Deepseek-V3 (χ^2 = 20.68, p_{FDR} = 0.030), GPT-5 (χ^2 = 16.62, p_{FDR} = 0.047), and Llama 4 Maverick (χ^2 = 18.78, p_{FDR} = 0.047), all with partial docstrings. Effect sizes are small (Cramér's V: 0.14–0.16) but statistically significant with adequate power (0.86–0.94). No significant differences emerged for full or no docstring conditions, consistent with RQ3's "information gap hypothesis."

Detailed analysis of error substitution patterns (Table 13) reveals RAG's dual mechanism: substantial reductions in logic and object access errors, coupled with occasional increases in dependency-related failures. Aggregating across the three significant models, RAG with partial docstrings reduces 13.31% of all errors, but the reduction is not uniform across error types.

AttributeError shows the largest absolute reduction (639 → 579, −60 instances), followed by "Other" errors (129 → 76, −53), AssertionError (233 → 190, −43), ValueError (46 → 28, −18), and TypeError (320 → 304, −16). These reductions suggest that retrieved examples help models understand correct object attribute access patterns, type specifications, and logical requirements, preventing the most common failure modes.

However, RAG introduces new failures in specific categories. ImportError increases from 2 to 7 instances (+5, +250%), and KeyError from 11 to 16 (+5, +45%). These increases reflect a substitution mechanism: retrieved examples contain dependencies or data structures absent in the target class, leading models to generate code that references unavailable imports or dictionary keys. The pattern is particularly pronounced in GPT-5, which shows a substantial increase in ImportError (0 → 7), and Deepseek-V3, which also exhibits a significant increase in KeyError (1 → 12).
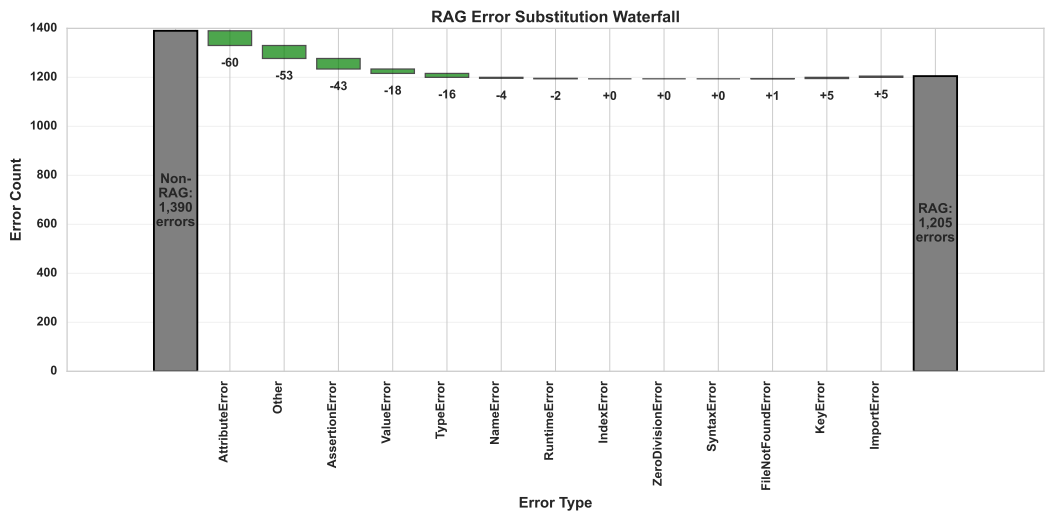
Fig. 5. Error Type Changes with RAG Implementation.

Table 13. RAG error substitution patterns for significant models with partial docstrings (aggregate across Deepseek-V3, GPT-5, Llama 4 Maverick). Negative differences indicate reductions, positive differences indicate increases.

Error Type	Non-RAG Count	RAG Count	Absolute Difference	Non-RAG %	RAG %	Percentage Point Difference
Reductions						
AttributeError	639	579	−60	45.97	48.05	+2.08
Other	129	76	−53	9.28	6.31	−2.97
AssertionError	233	190	−43	16.76	15.77	−0.99
ValueError	46	28	−18	3.31	2.32	−0.99
TypeError	320	304	−16	23.02	25.23	+2.21
NameError	6	2	−4	0.43	0.17	−0.26
Increases						
ImportError	2	7	**+5**	0.14	0.58	**+0.44**
KeyError	11	16	**+5**	0.79	1.33	**+0.54**
FileNotFoundError	2	3	+1	0.14	0.25	+0.11

Interestingly, while `AttributeError` and `TypeError` decrease in absolute count, their relative proportions increase (45.97% → 48.05% and 23.02% → 25.23%, respectively). This occurs because other error types decrease more substantially, making these harder-to-eliminate errors more prominent in the residual failure set. This suggests that `AttributeError` and `TypeError` represent fundamental challenges in class-level code generation that retrieval cannot fully address, as they require deep semantic understanding of object hierarchies and type systems that transcend pattern matching.

7.3 Interpretation

The error analysis reveals three critical insights into LLM class-level code generation failures: a concentrated error signature dominated by object access and type consistency issues, fundamental differences between synthetic and real-world failure modes, and a RAG-induced error substitution mechanism. These findings provide actionable guidance for both practitioners and researchers seeking to improve code generation quality.

7.3.1 The AttributeError-TypeError-AssertionError Triad. The dominance of three error types—`AttributeError` (43.84%), `TypeError` (21.65%), and `AssertionError` (18.51%)—accounting for 84% of all failures reveals a consistent failure signature across models and conditions. This concentration indicates that LLM code generation struggles stem from specific, identifiable challenges rather than diffuse implementation errors.

`AttributeError` predominance directly links to RQ1's qualitative finding that real-world test suites involve complex type metadata checks and object hierarchies. LLMs generate plausible class structures but fail to correctly implement attribute access patterns, particularly in inheritance hierarchies and dynamic attribute resolution. This suggests that current training paradigms, which excel at surface-level syntax and common patterns, do not sufficiently capture deep object-oriented semantics.

`TypeError` frequency reflects challenges in maintaining type consistency across method calls, particularly in dynamically typed Python where type annotations may be incomplete or absent. RQ2 showed that docstrings with explicit type information improve correctness by 1–3%, but the persistence of `TypeError` even with full documentation suggests that models struggle to propagate type constraints through code generation, often mixing incompatible types or calling methods with incorrect argument types.

`AssertionError` prevalence indicates logical implementation failures where generated code produces incorrect values despite syntactic validity. The stark difference between ClassEval (71.80%) and real-world datasets (5.34–17.57%) demonstrates that synthetic benchmarks primarily test logical correctness through explicit assertions, while real-world failures manifest through object access and type issues before reaching assertion points.

The absence of `SyntaxError` (0.00%) confirms that modern LLMs have mastered Python syntax completely. Even though this is opposite to the bug patterns in function-level LLM-generated code reported in a prior study [54], our finding validates LLMs' use for code generation tasks and shifts attention to semantic correctness—understanding object models, type systems, and logical specifications—as the remaining frontier.

7.3.2 Synthetic vs. Real-World Error Type Divergence. The large effect sizes (Cramér's V: 0.59–0.85) comparing ClassEval to real-world datasets definitively answer RQ1's open question about *why* performance gaps exist. Synthetic benchmarks do not merely present easier versions of the same challenges; they represent fundamentally different problem spaces with distinct failure modes.

ClassEval's assertion-dominated error profile (71.80% `AssertionError`) reflects its design philosophy: test logical correctness through input-output matching with simple equality checks. This aligns with the qualitative analysis in RQ1, which found ClassEval test suites use straightforward assertions like numerical comparisons and dictionary checks, requiring no external dependencies. LLMs perform well (83.67–88.51% pass rate) because they can generate logically correct implementations for well-specified pure functions.

Real-world code's attribute-and-type-dominated profile (45–49% `AttributeError`, 22–24% `TypeError`) reflects genuine software engineering challenges: navigating complex object hierarchies, managing

dependencies, and maintaining type consistency across diverse contexts. RQ1's qualitative analysis revealed that real-world test suites involve system dependencies (e.g., numpy, MCPContext) and metadata checks, leading to AttributeError when models misunderstand object structures. The 47 percentage point increase in AttributeError from ClassEval to Pre-Cutoff quantifies this fundamental shift.

The NameError pattern (14.44% ClassEval vs. 0.12–0.27% real-world) reveals another dimension of divergence. In ClassEval, models often fail to correctly reference variables or function names in self-contained implementations, while real-world code's lower NameError rate suggests that context from surrounding project code helps models resolve names correctly. This paradoxically indicates that isolated synthetic tasks may be harder for name resolution than contextualized real-world scenarios.

The small but significant Pre-Cutoff versus Post-Cutoff differences (Cramér's V: 0.22–0.28) warrant brief discussion despite their minor magnitude. While RQ1 found negligible pass rate differences between seen and unseen real-world code, error patterns show subtle temporal evolution. Pre-Cutoff code produces more "Other" errors (18.29% vs. 9.16%), suggesting older projects contain more idiosyncratic failure modes or deprecated patterns unfamiliar to models. Post-Cutoff's higher AssertionError (17.57% vs. 5.34%) may reflect modern testing practices favouring explicit assertions, though this requires further investigation.

7.3.3 RAG Error Substitution: Trading Logic Errors for Dependency Failures.
RQ3 demonstrated that RAG improves pass rates by 4–7% with partial docstrings, but the error distribution analysis reveals the *mechanism*: RAG prevents logic and object access failures while introducing dependency-related errors. This substitution pattern is not merely error reduction but error transformation.

The substantial reductions in AttributeError, AssertionError, and ValueError indicate that retrieved examples provide concrete implementations that models can adapt, particularly for object attribute access patterns and logical requirements. When partial docstrings specify *what* methods should do but not *how*, retrieved similar implementations fill the gap, showing models correct attribute usage and logic structure.

However, the increases in ImportError and KeyError reveal RAG's limitation: retrieved examples may contain dependencies or data structures absent in the target context. Models blindly copy import statements or dictionary key accesses from examples without verifying availability in the target class. GPT-5's dramatic ImportError increase ($0 \rightarrow 7$) suggests that this model particularly struggles to filter context-specific dependencies from retrieved examples, while Deepseek-V3's KeyError explosion ($1 \rightarrow 12$) indicates difficulty distinguishing data structure patterns from concrete key names.

The relative proportion increases for AttributeError (+2.08 pp) and TypeError (+2.21 pp) despite absolute reductions, revealing these as "hardcore" errors resistant to retrieval-based mitigation. Even with concrete examples, models struggle to fully master object attribute navigation and type consistency, suggesting these require deeper semantic understanding beyond pattern matching.

It is worth mentioning that Llama 4 Maverick shows the most balanced substitution profile, reducing errors across the board with minimal dependency increases, suggesting architectural differences in how models integrate retrieved context. Future work should investigate these model-specific retrieval utilization strategies to optimize RAG effectiveness.

7.3.4 Reflection on Previous RQs.
RQ4 cohesively integrates with prior research questions. RQ1 identified a performance gap between synthetic and real-world code (83.60–88.27% vs. 24.59–33.60% pass rates); RQ4 explains this through fundamentally different error types—ClassEval tests logical correctness through assertions, while real-world code requires object attribute mastery. RQ2 showed docstrings improve correctness by 1–3% even though mostly statistically non-significant; RQ4

reveals they reduce all error types proportionally without changing failure modes, clarifying the documentation's mechanism. RQ3 demonstrated RAG's 4–7% improvement with partial docs; RQ4 exposes the error substitution mechanism, trading logic failures for dependency errors, explaining both benefits and limitations.

> **RQ4 Findings:** LLM class-level code generation failures follow a consistent, concentrated signature dominated by object access and type errors. Synthetic benchmarks fundamentally mischaracterize these challenges by focusing on logical assertions. RAG offers promising but partial mitigation through error substitution.

8 DISCUSSION

This study provides the first comprehensive evaluation of LLM performance on class-level code generation using real-world projects, revealing fundamental gaps between synthetic benchmark performance and practical capabilities. In this section, we provide a combined summary of all RQs and share actionable insights for practitioners and researcher.

8.1 Key Findings

8.1.1 Performance Gap Between Synthetic and Real-World Code. Our investigation of LLM performance across synthetic and real-world datasets revealed a sharp divide that questions current evaluation practices. All seven models achieved high pass rates on ClassEval, matching previous research findings. However, their performance dropped dramatically on real-world projects. The very small differences between Pre-Cutoff and Post-Cutoff show that models handle both seen and unseen real-world code equally well—but in both cases, the performance is poor for class-level code generation.

Our detailed analysis showed that this gap comes from different task types rather than harder problems. ClassEval test suites use simple equality checks with few external requirements, allowing models to achieve high pass rates through basic logic. Real-world test suites involve complex type checks, external system requirements, and integration between components—all of which reveal core weaknesses in how models understand object-oriented programming concepts.

8.1.2 Docstrings as Incremental Improvement. Our systematic test across three documentation levels (full, partial, no docstrings) revealed that complete docstrings may provide improvements in isolated cases. The pattern of benefits differed between time periods, with stronger effects on Pre-Cutoff data compared to Post-Cutoff. This suggests models rely more on documentation for familiar code patterns, where specifications help clarify how to implement things. However, comparing across time periods showed large differences between individual models. Some models performed better with Post-Cutoff data, while others performed better with Pre-Cutoff data. This indicates that docstring effectiveness depends on how models are built and what data they were trained on, rather than simply whether the code was seen during training.

8.1.3 RAG's Information Gap Hypothesis. Retrieval-augmented generation showed a clear pattern with documentation completeness, supporting an "information gap hypothesis": RAG helps most when documentation is incomplete. This pattern shows how RAG works: when documentation explains what the code should do but doesn't show how to implement it (partial docstrings), retrieved examples fill this gap by providing actual code implementations.

Notably, GPT-5 performed worse with RAG, suggesting differences in how this model processes context. However, five other models with different designs (Codestral, Deepseek-V3, GPT-4.1, Llama

4 Maverick, Qwen Coder 2.5) all showed consistent positive results. This strengthens the general applicability of the information gap hypothesis beyond individual model behaviors.

8.1.4 Error Signatures and Substitution Mechanisms. Error analysis revealed a focused pattern of failures: `AttributeError`, `TypeError`, and `AssertionError` make up 84% of all failures. Notably, `SyntaxError` contributed 0%, confirming that modern LLMs have fully learned Python syntax, making semantic correctness (understanding meaning) the main remaining challenge.

Dataset comparison revealed very different types of errors between synthetic and real-world code. ClassEval's error profile is mostly `AssertionError`, reflecting tests that check logic, while real-world datasets show mostly `AttributeError` and `TypeError`, reflecting real complexity in object-oriented programming.

RAG's impact on error distributions revealed a pattern of replacing one error with another rather than reducing all errors equally. Across the three models with strong RAG effects (Deepseek-V3, GPT-5, Llama 4 Maverick), RAG reduced `AttributeError`, `AssertionError`, and `ValueError`, showing that retrieved examples help prevent logic mistakes and problems accessing objects. However, RAG increased `ImportError` and `KeyError`, suggesting that retrieved examples contain dependencies or data structures not present in the target code, which models copy without checking if they fit.

8.2 Why Class-level Code Generation?

Class-level generation represents the optimal granularity for practical code generation in object-oriented programming. Function-level generation is too granular, requiring numerous individual API calls that increase both processing time and cost while generating code in isolation without a broader context. At the other extreme, file-level or project-level generation requires excessive context that often exceeds model context windows and makes the generation task unwieldy. Class-level generation strikes the right balance: in object-oriented design, class diagrams explicitly define which classes need to be generated, providing clear boundaries for each generation task. More importantly, class diagrams encode hierarchical dependencies and relationships between classes—such as inheritance, composition, and associations—that can be leveraged to inform generation rather than treating each unit in isolation. This structural information provides just enough context to generate meaningful, interconnected code without overwhelming the model or requiring an impractical number of API calls.

8.3 Broader Implications

8.3.1 Rethinking Benchmark Design and Evaluation. The large performance gap between synthetic and real-world code and the different types of errors raise concerns about current code generation evaluation practices. If synthetic benchmarks mainly test correct logic using simple checks while real-world code requires an understanding of object-oriented concepts, then benchmark leaderboards don't accurately show real-world abilities. The 57–62 percentage point gap between *ClassEval* and *RealClassEval* is not just harder problems but a shift to different types of problems.

This finding has clear consequences for benchmark development: future benchmarks must keep realistic object structures, dependencies, and testing practices rather than simplifying to basic function tasks. The RealClassEval dataset introduced here provides a starting point, but the field needs varied, large-scale real-world benchmarks covering multiple programming languages, domains, and complexity levels.

The complete absence of `SyntaxError` across all conditions shows that generating correct syntax is solved; research focus should shift completely to understanding meaning and correctness. This suggests that we need evaluation metrics that measure understanding of meaning —- correct use of

object attributes, type consistency, and dependency management —- rather than relying solely on superficial code similarity or test pass rates. Checking if code has the same meaning, validating types, and analyzing how code executes represent promising approaches for future evaluation methods.

8.3.2 Practical Deployment Strategies. For practitioners deploying LLM-based code generation tools, our findings provide concrete guidance:

Set Realistic Expectations Based on Task Type: Organizations should not expect the high success rates seen on synthetic benchmarks when generating real-world class-level code. The pass rates found here more accurately reflect real capabilities for complex object-oriented tasks. This means that LLM-generated code requires mandatory human review and testing rather than being ready for production, which shapes appropriate use cases and deployment workflows.

Invest in Complete Documentation: While full docstrings improve correctness by only a small amount, this adds up across large codebases. Organizations should invest in complete documentation in code generation prompts, including class descriptions, method signatures, parameter types, and return specifications. The cost is minimal compared to the benefits, and IDE integrations should automate docstring extraction to maximize generation quality without manual effort.

Use RAG Selectively: RAG provides clear benefits specifically with partial documentation, but minimal value with full docstrings. Organizations should implement adaptive RAG strategies: enable retrieval when detecting incomplete documentation but disable it when documentation is complete, optimizing both performance and computational cost. Importantly, RAG introduces more dependency-related errors, requiring post-processing to validate that imports and key accesses in generated code exist in the target context.

8.3.3 Research Directions for Enhanced Code Generation. Our findings point to specific research directions that could substantially improve code generation capabilities:

Object-Oriented Semantic Learning: The dominance of attribute errors indicates that current training inadequately captures object-oriented concepts. Future work should investigate training objectives specifically targeting attribute resolution, inheritance relationships, and dynamic attribute handling. Graph neural network architectures that explicitly model class hierarchies may better capture these relationships than transformer architectures alone, and training data should emphasize class-level examples with complex inheritance rather than isolated function examples.

Type-Aware Generation Architectures: The type error challenge motivates research into generation architectures that maintain explicit type information during generation, enforcing consistency requirements. Integration with gradual typing systems could provide training signals for type-correct generation. Constraint-based generation that incorporates type checking as a hard requirement rather than a soft preference may prevent type errors during generation rather than filtering afterward.

Context-Aware Retrieval Filtering: RAG's increase in dependency errors indicates that current retrieval systems lack context-awareness. Research should develop retrieval mechanisms that filter retrieved examples based on target context, removing imports and data structure accesses incompatible with the target class. Semantic parsing of both retrieved examples and target specifications could enable intelligent filtering, or constraint satisfaction approaches could ensure retrieved examples satisfy target context requirements. This would prevent blindly copying dependencies that don't exist in the target environment.

Hybrid Documentation-Retrieval Approaches: Docstrings and RAG provide complementary benefits, suggesting that hybrid approaches combining optimal documentation detail with targeted

retrieval could maximize quality. Research should investigate what level of documentation, combined with what retrieval strategies yields optimal generation quality while minimizing human annotation effort.

8.3.4 Methodological Contributions. Beyond specific findings, this work makes methodological contributions to code generation research:

Real-World Dataset Introduction: RealClassEval provides the first carefully collected real-world class-level benchmark, enabling reproducible evaluation on real code generation tasks. While relatively small, it is 2 times larger than ClassEval and represents real industrial software engineering challenges, providing a foundation for future real-world benchmarks.

Error-Focused Analysis: Moving beyond overall pass rates to detailed error type analysis reveals why models fail, not just that they fail. The error replacement pattern identified in RAG analysis—replacing logic failures with dependency errors—would be invisible in pass-rate-only evaluation, yet provides critical insights into retrieval's benefits and limitations. Future code generation evaluations should adopt multi-level analysis, including pass rates, error distributions, and qualitative failure modes for complete understanding.

9 LIMITATIONS AND FUTURE WORK

While this study provides valuable insights into LLM class-level code generation, several limitations warrant discussion and motivate future research.

9.1 Limitaions

9.1.1 Sample Size. The most significant limitation concerns dataset size and the resulting small to negligible effect sizes in the majority of cases. Our study evaluated 200 classes per RealClassEval version, totalling 400 real-world classes compared to ClassEval's 100 synthetic tasks. While each split (pre-cutoff and post-cutoff) of RealClassEval is 2 times larger than the established ClassEval benchmark and represents the first carefully collected real-world class-level dataset, it remains relatively small by machine learning evaluation standards. This size limitation appears in consistently small effect sizes across research questions despite statistical significance. Docstring effects and RAG effects showed negligible practical differences, and error distribution comparisons between Pre-Cutoff and Post-Cutoff showed small effects compared to the synthetic-real divide. These small effect sizes reflect three factors: genuine small practical effects, high variability between classes, and limited statistical power to detect small-to-medium effects with 200 samples.

The limited size stems from practical constraints. Generating and testing over 16,000 classes across seven LLMs, two dataset versions, three docstring conditions, and two retrieval settings required substantial computational resources. At current API rates of different LLMs' API service providers, the total generation cost, including trial and error, was approximately $1000 for API calls alone, plus over 500 CPU hours for test execution. Each additional class requires LLM generation, test suite generation, and test execution, increasing costs directly with size. Expanding to 1,000+ classes per version would require proportional resource scaling, exceeding typical academic research budgets.

However, our sample size is sufficient for establishing fundamental patterns. The large performance gap between synthetic and real-world code and the error type differences are robust with clear practical significance. The small effect sizes for docstrings and RAG reflect genuine small benefits rather than merely insufficient power, as evidenced by consistency across multiple models, statistically significant findings in key comparisons, and coherent patterns. Critically, this study provides the first evidence from real-world data on class-level code generation, establishing baselines and patterns that synthetic benchmarks alone cannot reveal. Future work with larger-scale

real-world datasets is essential for more conclusive findings, particularly for detecting smaller effects and enabling more detailed analyses.

9.1.2 Model Selection and Recency. Our evaluation included seven state-of-the-art models as of October 2025, representing diverse architectures and capabilities. However, the rapid pace of LLM development means newer models may show different patterns. The consistency of findings across all seven models—particularly the performance gap between synthetic and real-world code and error type dominance—suggests robust patterns likely to persist in future models. Additionally, all evaluated models are transformer-based architectures, and alternative architectures like graph neural networks explicitly modeling code structure may show different strengths, particularly for understanding object-oriented concepts.

9.1.3 Task Scope: Class-Level vs. Other Granularities. This study focuses exclusively on class-level code generation, where models must implement complete classes with multiple methods given only skeletons. Findings may not generalize to other granularities: single-function generation may show higher success rates due to a simpler context, while generating entire projects likely shows even larger challenges due to cross-file dependencies. The choice of class-level granularity reflects realistic software engineering tasks and enables controlled evaluation, but complete understanding requires evaluation across multiple granularities. The error pattern insights may not generalize to other object-oriented granularities [54] and require validation.

9.1.4 Test Suite Generation Limitations. We relied on Pynguin for automatic test suite generation, using its DynaMOSA algorithm to achieve high branch coverage. While this approach enables objective correctness assessment without manual test writing, automatically generated test suites have limitations: they primarily assess that code executes without exceptions, but may not validate whether methods compute correct results, some classes may have low-coverage test suites missing errors in uncovered paths. Future work should incorporate test suite quality metrics or combine automatic test generation with manual validation for a subset of classes to ensure tests accurately assess correctness.

9.1.5 Prompt Engineering and Generation Settings. We used a standardized prompt across all models with consistent generation settings to ensure fair comparison, but this does not reflect optimal prompting strategies for each model. Some models may benefit from model-specific prompt formats, few-shot examples [67], or interactive refinement strategies. However, the finding that all seven models show similar error patterns suggests that fundamental capabilities rather than prompt optimization drive performance. The goal was to assess base capabilities under standardized conditions rather than optimizing each model individually, reflecting realistic deployment scenarios.

Despite these limitations, this study establishes fundamental patterns in LLM class-level code generation, provides the first real-world evaluation baseline, and illuminates specific research directions for advancing code synthesis capabilities. The limitations identified here directly motivate ongoing work developing large-scale, diverse, quality-controlled real-world benchmarks to enable more conclusive future studies.

9.2 Future Work

The field urgently requires large-scale, diverse, real-world code generation benchmarks to enable definitive evaluation and advance beyond synthetic task limitations. An ideal benchmark would include:

- **Scale**: 1,000+ classes per temporal category (Pre-Cutoff, Post-Cutoff), enabling detection of small-to-medium effect sizes with high power (> 0.80 for Cohen's $d = 0.2$) and supporting subgroup analyses by domain, complexity, or project characteristics.
- **Diversity**: Classes spanning multiple domains (web development, data science, systems programming, scientific computing), programming paradigms (object-oriented, functional, procedural mixtures), and complexity levels (simple data structures to complex business logic), ensuring generalization beyond specific niches.
- **Quality Control**: Systematic filtering for engineered projects with mature testing practices, automated test suite validation (ensuring tests actually execute and provide coverage), and manual spot-checking to verify task quality and remove problematic edge cases.
- **Metadata Richness**: Annotations for class complexity (cyclomatic complexity, coupling metrics), domain categories, required dependencies, and testing characteristics (unit vs. integration, coverage levels), enabling fine-grained analysis of where models succeed or fail.
- **Multi-Language Support**: Extension beyond Python to include Java, JavaScript, C++, and other languages, assessing whether findings generalize across language ecosystems or reflect Python-specific patterns.

Developing such a benchmark represents a substantial research contribution in itself, requiring substantial curation effort and computational costs. The insights from this study—particularly the importance of preserving realistic object hierarchies, dependencies, and testing practices—provide design principles for next-generation benchmarks.

10   THREATS TO VALIDITY

We discuss threats to validity following established guidelines for empirical software engineering research, addressing internal validity (confounding factors), external validity (generalizability), and construct validity (measurement accuracy).

10.1   Internal Validity

Internal validity concerns whether observed effects genuinely result from our experimental design rather than confounding variables. We employed several controls to ensure valid findings. We used identical prompts across all models and consistent generation settings to control for prompt-induced variance. All test suites were generated using Pynguin with identical configuration, ensuring consistent evaluation across classes. We used random sampling for class selection, reducing selection bias toward specific project types or domains. For RQ2 and RQ3, we employed within-subjects designs where each class was evaluated under all docstring conditions and both retrieval settings, controlling for snippet-specific difficulty.

10.2   External Validity

External validity concerns generalizability beyond our specific experimental setting. Several factors limit generalizability. All evaluated code is Python-based, and findings may not generalize to statically typed languages like Java or C++, which would likely show fewer type errors due to compile-time checking but potentially more complex type hierarchy challenges. All data derives from open-source GitHub repositories, potentially limiting generalizability to proprietary enterprise codebases, which may follow different conventions, have more comprehensive documentation, or employ domain-specific patterns unfamiliar to models trained predominantly on open-source data.

10.3 Construct Validity

Construct validity concerns whether measurements accurately capture intended constructs. We use pass rate as the primary correctness metric, which has limitations: it depends entirely on test suite quality, measures behavioural rather than structural correctness, and doesn't distinguish between semantically equivalent implementations. However, pass rate is standard in code generation research [29, 64, 67], provides an objective functional correctness measurement through execution, and is validated across multiple test cases per class. Our qualitative analysis examined test suites and failure modes, confirming that pass rates meaningfully distinguish correct implementations from failures.

11 CONCLUSION

This work provides the first comprehensive evaluation of large language model performance on class-level code generation using real-world projects, revealing fundamental limitations masked by synthetic benchmark evaluation. Through systematic analysis classes generated by seven state-of-the-art models, we demonstrate that current LLMs achieve 84–89% correctness on synthetic benchmarks but only 25–34% on real-world code—a 53–62 percentage point gap. This performance drop stems from fundamentally different error patterns rather than just harder problems: synthetic benchmarks test logical correctness through simple checks, while real-world code requires an understanding of object-oriented concepts. Real-world failures are dominated by `AttributeErrors` and `TypeErrors`, accounting for two-thirds of all failures, while synthetic benchmarks show mostly logic-induced errors. Modern LLMs have completely mastered Python syntax but struggle with object-oriented semantics. Importantly, models achieve nearly identical pass rates on seen and unseen real-world code, suggesting that fundamental understanding limitations rather than memorization-driven failures. This challenges current evaluation practices: synthetic benchmark performance does not accurately represent real-world capabilities, and organizations should expect much lower success rates when deploying LLM-based code generation tools.

Our studies reveal that documentation and retrieval provide complementary but modest improvements. This implies that the deployment of RAG should be done adaptively based on documentation completeness. The concentrated error patterns point to critical research directions: developing training objectives targeting attribute resolution and type consistency, creating context-aware retrieval systems that filter incompatible dependencies, and building large-scale real-world benchmarks spanning multiple languages and domains. As LLM-based development tools become increasingly common in software engineering, rigorous evaluation on real-world tasks becomes essential. This work establishes that synthetic benchmarks provide false confidence, and the path forward requires both better models with enhanced object-oriented understanding and better evaluation practices through diverse real-world benchmarks. We make our data and scripts available to facilitate replication and future research [3].

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