ANVIL: Anomaly-based Vulnerability Identification without Labelled Training Data

Weizhou Wang Eric Liu Xiangyu Guo Xiao Hu Ilya Grishchenko David Lie University of Toronto

Abstract

Supervised-learning-based software vulnerability detectors often fall short due to the inadequate availability of labelled training data. In contrast, Large Language Models (LLMs) such as GPT-4, are not trained on labelled data, but when prompted to detect vulnerabilities, LLM prediction accuracy is only marginally better than random guessing. In this paper, we explore a different approach by reframing vulnerability detection with LLMs as anomaly detection. Since the vast majority of code does not contain vulnerabilities and LLMs are trained on massive amounts of such code, vulnerable code can be viewed as an anomaly when compared to LLM's predicted code, freeing the model from the need for labelled data to provide a learnable representation of vulnerable code. Leveraging this perspective, we used LLMs to perform a masked code reconstruction task, and compared the LLM-generated code to its original version to identify anomalies. Our experiments demonstrate that LLMs trained for code generation exhibit a significant gap in reconstruction accuracy between vulnerable and non-vulnerable code, which can be quantified using an anomaly score.

Using this insight, we implement ANVIL, a detector that identifies software vulnerabilities at line-level granularity. Our experiments explore the discriminating power of different anomaly scoring methods, as well as the sensitivity of ANVIL to context size. We also study the effectiveness of ANVIL on various LLM families, and conduct leakage experiments on vulnerabilities that were discovered after the knowledge cutoff of our evaluated LLMs. On a collection of vulnerabilities from the PrimeVul dataset, ANVIL outperforms state-of-theart line-level vulnerability detectors, LineVul, LineVD, and LLMAO, which have been trained with labelled data, despite ANVIL having never been trained with labelled vulnerabilities. Specifically, our approach achieves $1.25 \times$ to $1.62 \times$ better Top-5 accuracies, 27% to 69% better Normalized MFR, and 1.10× to 1.15× times better ROC scores on line-level vulnerability detection tasks.

1 Introduction

Probabilistic approaches to static vulnerability detection have garnered considerable research attention due to the promise of automatically detecting common bug patterns by training a model on historical data. Confidence in statistical modelling has only grown with the recent advances in large language models (LLMs). One of the significant advantages of LLMs is that they can be trained in a self-supervised fashion without labelled training data. This has enabled them to be trained on very large training sets. This approach has led to impressive and surprising results in code understanding and code generation [12, 38].

Unfortunately, the same cannot be said about their ability to detect vulnerabilities. As shown in a recent study [34], even with cutting edge LLMs like GPT-4, vulnerability prediction accuracy on real world code is little better than a coin-flip. One reason for this limitation is that the pretraining process of LLMs is designed primarily to generate correct code, without explicitly incorporating the concept of "vulnerability", leaving them with insufficient domain knowledge to reason about whether a given code is vulnerable. On the other hand, current learning and LLM-based vulnerability detectors, while attempting to embed the notion of code vulnerabilities, still suffer from the lack of a large and well-labelled set of representative vulnerability dataset [7, 10]. Efforts to compensate for the dearth of labelled data by selecting relevant code features (e.g., the granularity of the code context, commit history, control-flow, data-flow, etc.) and converting these features into a learnable representation (e.g., a vector or a graph) [8, 13, 16, 20, 21, 24, 32] have only yielded modest improvements. Similarly, recent attempts to generate new vulnerable training corpora [24, 27, 28] have not improved real-world performance significantly, as synthetically generated vulnerabilities still cannot capture the huge variety of ways that vulnerabilities can manifest themselves.

LLMs are effective because they are trained on massive datasets, enabling them to learn the general structure and patterns required to generate correct code. However, this broad understanding does not translate well to recognizing vulnerable code patterns due to fundamentally imbalanced corpora. Out of the huge amounts of unlabelled code that LLMs are trained on, only a minuscule portion will contain vulnerabilities. Instead of attempting to learn bug patterns from this sparse signal, we advocate for the inverse. Since bugs are inherently *anomalous*, we believe that LLMs should be leveraged as a model for correct code, and bug detection can be cast as an anomaly detection problem. By comparing the original code to the LLM-generated version in a masked code reconstruction task, we can determine whether the original code falls outside the LLM's generation distribution, thereby identifying anomalies.

There have been previous attempts at modelling software defects and vulnerabilities as anomalous code. For example, a study of code "naturalness" [30] leveraged statistical methods to model software bugs and their fixes, creating a purely statistical 10-gram model trained on 35 million LOC for next-token prediction. Then, given a sequence (i.e., line of code), the model returned a probability of that sequence based on its internal statistics. Using this probability, the approach calculated the cross-entropy and using this data, the work identified a statistically significant difference in entropy (a measure of prediction uncertainty) when evaluating buggy versus fixed code, which enabled their approach to achieve bug detection that was competitive with static-analysis-based tools.

We hypothesize that the 10-gram model result can be improved by the modern transformer-based LLMs since they have considerably greater representational power than *n*-gram models, being trained on data sets with 10's-100's of billions of LOC (a 4-5 order of magnitude increase). However, the application of LLMs to anomaly-based vulnerability detection poses new challenges. First, measuring the entropy of predictions might be too coarse when applied to LLM results because LLMs are so precise at generating code that even a one character difference can be significant. For example, an out-of-bound array access may result from a missing equal sign in an "if" statement. To address this limitation, this paper explores different methods for quantifying the anomaly level of predictions. Second, unlike the previous generation of 10-gram models, LLMs can use a context of hundreds to thousands of tokens to make predictions, raising the question of the appropriate context size to use. Third, the design space of LLMs is very large, with a diversity of model architectures, training objectives and datasets. Thus, we also explore whether anomaly-based bug detection with LLMs generalizes to different model architectures and how it performs as models scale to larger capacities and training sets. Finally, given that modern LLMs are trained on large training sets of code, there is a risk that a pre-trained LLM may have seen both the vulnerable and fixed version of some code - commonly referred to as "leakage". We evaluate our approach on a leakage-free dataset we created that only contains vulnerabilities found after the training date of the LLMs we use to demonstrate that

our results generalize to unseen vulnerabilities.

To summarize, we make the following contributions:

- We propose an anomaly-based vulnerability detection method, ANVIL, that leverages the learned knowledge of pre-trained LLMs. We demonstrate that masked code reconstruction accuracy is an effective way to differentiate between vulnerable and non-vulnerable code.
- We evaluate different hyperparameters for our masked code reconstruction technique, namely the anomaly scoring function, model size and prompt context. From our experiments, the hybrid anomaly function, larger models and compound statement contexts enable more accurate and efficient vulnerability detection.
- Our anomaly-based tool outperforms LineVul [13], LineVD [16], and LLMAO [41], three state-of-the-art LLM-based detectors trained to predict line-level vulnerabilities. Specifically, ANVIL achieves 1.25× to 1.62× better Top-5 accuracies and 27% to 69% improvements in Normalized MFR for vulnerability prioritization, as well as 1.10× to 1.15× higher ROC-AUC scores for linelevel vulnerability classification.

2 Methodology

2.1 Overview

Similar to previous research [30] in code "naturalness", we consider a piece of code anomalous (i.e., unnatural) if a model considers it out-of-distribution. Specifically, the feedback we utilize is the LLM reconstruction accuracy on a code generation task, when masking each line of code for inference. LLMs are trained to predict the most probable token given a chain of previous tokens, hence a low reconstruction accuracy compared to the ground truth indicates that the ground truth has deviated from the learned representation of the model. Figure 1 describes the workflow of our technique, ANVIL. For each line of code under analysis, we withhold the original line as the ground truth and instruct the LLM to reconstruct it based on the surrounding context. The LLM-generated code is then compared with the ground truth to compute an anomaly score, which quantifies the divergence between them. While in our study, we only implement ANVIL to support C/C++ languages as a proof of concept, we believe the underlying approach can be generalized to detect vulnerabilities in other programming languages.

2.2 FIM & Context Selection

Code LLMs are trained to perform specific generative tasks, such as code-completion, where the model is trained to predict the next *n* tokens given a code prefix, or fill-in-the-middle

```
Generated
                                                                                                        Output
double avg(int arr[], int size) {
                                                 double avg(int arr[], int size) {
                                                                                               LLM
   int sum = 0;
                                                     int sum = 0;
                                         Apply
   for(int i = 0; i < size; i++) {
                                                     for(int i = 0; i < size; i++) {
                                         Mask
                                                                                                                               Anomaly
       sum += arr[i]:
                                                                                                                   Function
                                                                                             Masked Line
   return (double)sum / size;
                                                     return (double)sum / size;
                                                                                           sum += arr[i];
```

Figure 1: Overview workflow

(FIM, also known as infilling) [3], where the model is trained to generate tokens that fall between a given prefix and suffix.

We focus on the utility of FIM as the mode of code generation for detecting anomalies. Although code completion is a more prevalent task, research by Bavarian et al. [3] highlights that FIM tasks generally result in lower reconstruction loss due to additional constraints provided by the suffix. This structure allows FIM to capture downstream control and data flows, which are crucial for generating correct code for a masked line.

However, adapting the FIM task for anomaly detection introduces several challenges due to constraints such as GPU memory and LLM token limits, which make it impractical to use entire source files as context. To address this, we empirically evaluate two methods for finding a context size: (1) a fixed size approach and (2) a semantics-aware approach.

For (1), we investigate the effect of fixing the context to a specific number of source-code lines, equally split between the prefix and suffix. For instance, a 500-line context means using 250 lines preceding and succeeding the line under analysis. Still, a fixed context length, such as a 150-line prefix and suffix, may not sufficiently encapsulate all relevant code features for vulnerability detection. In cases where the relevant context extends beyond this 300-line window, such as in functions longer than 300 lines, critical contextual information will not be conveyed to the LLM. Conversely, in smaller functions that span only 50 lines, a 300-line context window could introduce irrelevant information.

Alternatively, (2) uses an Adaptive Context (AC) approach, which adaptively selects the maximum compound statement surrounding the line under analysis. This method ensures that the model receives semantically relevant context to effectively learn the code's features without being burdened by irrelevant information that the model might inherently ignore. In C/C++, a compound statement refers to a sequence of statements enclosed by curly braces ("{" and "}"). Given a line of source code, we define the AC as the largest compound statement that encapsulates the line. For each line, ANVIL statically extracts the AC to use as the context. Often, the AC equates to the function body containing the line; however, if the AC is too large, exceeding token limits or GPU memory capacity, we resort to the next smaller compound statement that meets these constraints. We note that this technique is agnostic to the

hardware used, but empirically, we found that a context limit of 500 lines reached near-maximum peak usage of our GPU memory. Hence, our AC selection only considers compound statements that are 500 lines or fewer.

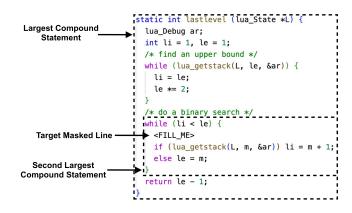


Figure 2: An example from *lua* (a scripting language developed in C) showing the largest and second-largest compound statement for a line of interest

If no suitable compound statement is identified, we default to providing a prefix and suffix of 150 lines each. This choice is based on empirical data from our experiments, where a context window of 300 lines, among all fixed context sizes, struck the best balance between GPU memory utilization and reconstruction accuracy, leading to the most efficient differentiation between vulnerable and non-vulnerable lines.

Figure 2 illustrates an example of how we extract the compound statements associated with a masked line. Since our analysis is performed at the source code level, we rely on matching brackets to identify compound statements. Specifically, we use a stack to store the line numbers of all opening braces, and then match each closing brace with the latest unmatched opening brace on the top of the stack.

2.3 Anomaly Score

To quantify the discrepancies between the generated line and ground truth, we assign an *anomaly score* to each generated line. We define the anomaly score as a function that takes the masked ground truth (p) and the generated line (q) as

arguments and calculates a value $\delta(p,q)$, which estimates how "anomalous" the ground truth is. A higher score suggests a greater likelihood of the current line being a vulnerability.

Previous work on code "naturalness" [30] used a similar metric to this anomaly score, which calculates a cross-entropy loss for each token based on 10-gram frequencies counted from their corpus. In our study, we compute the anomaly score as the average cross-entropy loss across the entire reconstructed line generated by the LLM compared to the masked ground truth. This yields an overall loss measure $\delta_{loss}(p,q) = loss(p,q)$ for the line. Although the loss theoretically ranges from 0 to infinity, in practice, we observed that it typically falls between 0 and 1 in our experiments.

Alternatively, we propose using an exact match indicator as another metric. This approach assigns a value of 1 if the LLM's generated line matches the ground truth exactly, and 0 otherwise. An exact match implies that, during model inference for each token, the most probable token in each token distribution is perfectly aligned with the ground truth, and hence the line under analysis should not be considered anomalous. Thus, we can use the negation of exact string match for anomaly scoring, $\delta_{EM}(p,q) = -\mathbb{1}_{exact_match}(p,q)$.

As LLMs are very accurate, using either metric alone may not fully capture the nuances of using LLMs for anomaly detection. The exact match indicator, being binary, produces an overly sparse distribution that may penalize slight deviations between the prediction and ground truth too harshly. The loss value, although continuous, might miss subtle yet critical discrepancies. On the one hand, it can under-estimate the level of anomaly: if a masked ground truth is " $if(a \ge 0)$ " and the model predicts that the line is "if(a > 0)", the off-by-onecharacter '=' might result in a low average loss but represents a significant logical change, such as becoming the root cause of an out-of-bound array access. On the other hand, it can also over-estimate the level of anomaly: if the model has low certainty about many of the tokens in an otherwise perfectly correct prediction, it can end up computing a larger loss than the off-by-one character prediction above.

Consequently, we propose a third scoring function as outlined in Equation 1:

$$\delta_{Hybrid}(p,q) = loss(p,q) - \mathbb{1}_{exact\ match}(p,q) \tag{1}$$

This hybrid approach combines both the loss and the exact match indicator, which allows us to heavily reward exact matches, signifying non-anomalous outputs; such matches typically result in a small anomaly score near -1. In comparison, non-matches, indicating anomalies, tend to result in higher scores larger than 0. However, if LLM generates code with an error, the exact match would say completely the opposite, since we entrust LLMs to generate bug-free code. Thus, we rely on the loss measure to capture finer discrepancies that could still indicate vulnerabilities. In Section 3.2, we have conducted experiments comparing all three functions, and

validated that the hybrid approach offers the most reliable performance in identifying vulnerabilities as anomalous.

3 Evaluation

This section explores the effectiveness of ANVIL's anomaly-based vulnerability detection through five research questions. These questions investigate the different aspects of ANVIL's design and how varying parameters affect the efficacy of vulnerability detection. Additionally, we compare ANVIL to previous vulnerability detectors and address LLM data leakage concerns.

- **RQ1:** How do different anomaly scoring methods affect ANVIL's vulnerability detection capability?
- RQ2: How does the vulnerability detection performance of ANVIL vary across different model sizes and model architectures?
- RQ3: Does an adaptive context size enhance vulnerability detection performance compared to a fixed context size?
- **RQ4:** How does ANVIL perform relative to supervised-learning-based vulnerability detection approaches?
- RQ5: Can ANVIL's anomaly and vulnerability detection capabilities generalize to unseen data?

3.1 Experiment Setup

All experiments were conducted using LLMs and tokenizers from the Huggingface library [40], on a machine equipped with an Intel Xeon 4509Y CPU and an Nvidia H100 GPU with 80GB of HBM. For all LLMs, we used a temperature value of zero during code generation to prevent randomness.

Previous studies [7, 10] have revealed that popular vulnerability datasets, such as Devign [43] and CVEFixes [4], often suffer from high mislabelling rates. These datasets may mistakenly label commits unrelated to vulnerabilities as vulnerable, and may also include modifications which are irrelevant to vulnerability patching. To mitigate the influence of such mislabelling issues, we carefully selected real-world vulnerability datasets with high-accuracy labels.

Specifically, we employed three datasets to evaluate ANVIL across different dimensions. First, RQ1 through RQ3 serve as internal evaluations, exploring how ANVIL performs under varying configurations, such as different LLMs and context sizes. For these experiments, we selected the Magma [14] fuzzing benchmark due to its reliable vulnerability labelling. Specifically, Magma's labels are backed by a *Proof of Vulnerability* (PoV) for each bug, representing an input that triggers the vulnerability. These PoVs are extracted through manual reviews of bug reports and source files and

complemented by extensive fuzzing campaigns, ensuring that all vulnerable and patched lines are directly relevant to the vulnerabilities. Magma contains 138 real-world CVEs from nine widely-used open-source C/C++ projects (libpng, libtiff, libsndfile, libxml2, poppler, openssl, sqlite3, php, and lua), comprising approximately 240,000 LoC and 5,227 functions. The dataset features both CVE (vulnerable) and patched (non-vulnerable) versions of these projects, with 256 lines labelled as vulnerable.

After identifying the most effective configurations of ANVIL, we conduct a detailed comparison between ANVIL and various supervised-learning-based vulnerability detectors to address RQ4. To address potential overfitting from the previous configuration tuning process (RQ1–RQ3) and to demonstrate ANVIL's ability to generalize to a broader range of real-world projects, we selected the PrimeVul [10] dataset, which comprises over 700 open-source C/C++ projects. Despite being significantly larger than Magma, PrimeVul still maintains a low labelling error rate. It achieves more than 90% labelling accuracy through automated validation, which analyzes corresponding vulnerability descriptions in the NVD database. For RQ4, we use the PrimeVul validation and test datasets, which consist of 1,142 vulnerable functions and approximately 47,000 benign functions.

Additionally, to address concerns of LLM data leakage, in RQ5, we test our methodology with vulnerabilities unseen by the LLMs during training. Because each LLM we tested is trained at a different time, it is difficult to find a large set of vulnerabilities that is guaranteed to be unseen by all models. Therefore, we choose to test only the CodeLlama-13B model for leakage experiments because it generally provides the best performance, as per its ROC-AUC in Table 3. We leave the construction of leakage-free datasets and evaluation of other LLMs for future work. In total, we collected 100 CVEs from 53 different repositories using a script from the CVEFixes project [4]. All collected CVEs were published between December 31, 2023 to April 20, 2024, which is later than the knowledge-cutoff of the CodeLlama-13B model we used, ensuring that the CVEs are unseen by the model. Instead of only relying on lines changed by defect-fixing commits, which may contain irrelevant information, we manually inspect each commit to extract only the vulnerable lines of code. This resulted in a dataset of 147 vulnerable functions, containing 235 vulnerable LoC and 10,956 non-vulnerable LoC. We also included 3,589 benign functions consisting of 101,173 LoC to reflect the mixed and imbalanced nature of vulnerable and benign code in real-world projects. This dataset is publicly available in our repository to support future research.

3.2 RQ1: Anomaly Score

To evaluate which scoring function described in Section 2.3 can best distinguish vulnerable lines as anomalies, we conducted experiments using CodeLlama-13B on the Magma

dataset. Specifically, we examine the distributions generated by each anomaly score for each line type (i.e., vulnerable and non-vulnerable) and evaluate the ability of each anomaly score to discriminate between the vulnerable and non-vulnerable distributions. This evaluation was framed as a binary classification task, with the *Area Under the Receiver Operating Characteristic Curve* (ROC-AUC) score used as a metric to measure the significance of the distinction between vulnerable and non-vulnerable lines. An ROC-AUC score of 0.5 indicates random guessing, while higher scores reflect a greater discrimination capability.

We employed the FIM task, using 256 vulnerable LOC from the Magma dataset as vulnerable samples. Considering the impracticality of using over 240k non-vulnerable lines due to vast dataset size and time constraints, we balanced the dataset by randomly sampling 256 non-vulnerable lines for a comparative analysis. Additionally, we evaluated the vulnerability-anomaly detection capability across various fixed context sizes ranging from [500, 400, 300, 200, 100, 80, 60, 40, 20, 10, 2] lines, split evenly between the prefix and the suffix surrounding the masked line. We limited the context size to a maximum of 500 lines in our evaluation, as sizes larger than this often caused out-of-memory errors on the GPU during our experiments, potentially compromising evaluation accuracy.

For each reconstructed line of code, we calculated anomaly scores using the three scoring functions described in Section 2.3 (δ_{loss} , δ_{EM} , and δ_{Hybrid}). Figure 3 presents three graphs, each corresponding to one scoring function. For the Exact Match (EM) scores, we reported the averaged values due to their binary nature. In contrast, for the Loss and Hybrid scores, we plotted the median values instead of the mean. This decision was informed by our observation of several outliers in these two scoring functions, particularly in non-vulnerable lines where the reconstructed line differed significantly from the masked line, which could skew the mean and provide a less accurate representation of the data. To provide a comprehensive view of the Loss and Hybrid scores, we also included box plots [39], with the mean values represented by small triangles.

Across all scoring functions, the median scores reveal a discernible gap between vulnerable and non-vulnerable lines, and this gap stabilizes typically when the context size exceeds 100 lines. This observation reinforces the notion that the LLM processes these two categories differently. Conversely, as the context size decreases below 100 lines, the anomaly scores for both vulnerable and non-vulnerable lines gradually increase, and the gap between the two types shrinks, indicating that the LLM becomes less accurate in reconstructing the masked lines regardless of their categories. This performance decline occurs because smaller context sizes do not provide the model with sufficient information for accurate generation. Among the scoring functions, the Hybrid function exhibited a larger median gap between vulnerable and non-vulnerable lines

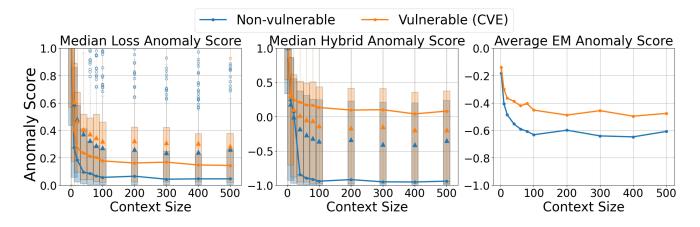


Figure 3: Anomaly scores on various functions

Table 1: ROC-AUC using different scoring functions

	Context Size											
Func	2										Wins	
δ_{EM}	52.2%	55.3%	56.1%	58.2%	58.5%	60.1%	59.0%	55.5%	59.2%	57.6%	56.6%	1
δ_{loss}	53.6%	56.3%	56.0%	59.1%	60.1%	61.4%	60.4%	61.1%	62.5%	61.6%	59.6%	3
δ_{hybrid}	54.1%	55.4%	56.1%	59.4%	60.5%	61.7%	60.6%	60.4%	62.9%	61.7%	59.5%	8

compared to the Loss function alone. Furthermore, the mean values of the two line types for the Loss score showed a smaller gap compared to those under the Hybrid score, which, despite also being affected by outliers, still displayed a more distinguishable separation between the two line types.

To quantify these findings, we computed ROC-AUC scores for the distributions of vulnerable and non-vulnerable lines for each scoring function and context size pair, as shown in Table 1. For each context size, we highlighted in bold the scoring function that achieved the highest ROC-AUC score. The results indicate that the combined scoring function (δ_{Hybrid} , Equation 1) most frequently yields the highest ROC-AUC, achieving the largest number of "wins" across context sizes. In contrast, the Exact Match Accuracy consistently produced the lowest scores for most context sizes, earning only one tied win at a context size of 20 lines.

3.3 RQ2: Model Architecture and Model Size

While RQ1 uses CodeLlama-13B as a representative model, in this section, we investigate whether this capability of distinguishing vulnerabilities as anomalous generalizes to other LLMs. To this end, we conduct the FIM workload on various open-source code LLMs. Open-source models, unlike closed-source ones such as OpenAI's GPT-3.5/4 series [29], provide not only the final generated tokens but also allow access to intermediate results like loss values and GPU usage, offering greater transparency and adaptability for research needs. We select the top-ranked base models from Huggingface's Big

Code Models Leaderboard as of May 1st, 2024 [5], focusing on those that support FIM code generation. To manage GPU memory efficiently, we restricted model parameters to fewer than 15 billion. Furthermore, during our evaluation, we observed that the StarCoder2-15B model exhibited poor performance on the FIM task due to an implementation bug highlighted in its original paper [22]. We thus substituted it with the StarCoderBase-15B model [19].

Additionally, we are curious about how model sizes affect the performance of distinguishing vulnerabilities as anomalous. To maintain a controlled experiment, we focused on evaluating different sizes of the same LLM architecture. Unfortunately, for CodeQwen and StarCoderBase, only one model size is available. As a result, we concentrated on CodeLlama, the only LLM with multiple model sizes suitable for the FIM task—specifically, the 7B and 13B versions.

Table 2: Code LLMs used in the experiments

14010 2. 00	uc LLI	Table 2. Code ELIVIS used in the experiments										
Model Name	Size	Training data ¹	Publication									
CodeLlama	13B	29B LOC	Aug 2023									
CodeLlama	7B	29B LOC	Aug 2023									
CodeQwen	7B	171B LOC	Apr 2024									
StarCoderBase	15B	57B LOC	May 2023									

Therefore, the evaluation involves four models, which are detailed in Table 2 [19, 31, 37]. Our experiments applied the

¹Approximated using 4 characters per token and 70 characters per LOC.

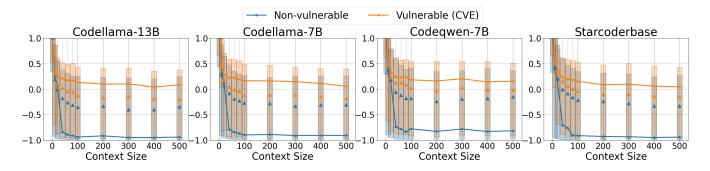


Figure 4: Median Anomaly Score on various LLMs under different contexts

Table 3:	ROC-AUC	on different l	LLMs
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		Context Size										
LLM	2	10	20	40	60	80	100	200	300	400	500	
CodeLlama-13B	54.1%	55.4%	56.1%	59.4%	60.5%	61.7%	60.6%	60.4%	62.9%	61.7%	59.5%	
CodeLlama-7B	54.0%	56.4%	56.7%	57.8%	59.7%	60.8%	60.0%	59.8%	61.7%	59.6%	58.7%	
CodeQwen	54.4%	55.7%	54.8%	58.8%	60.0%	61.9%	60.3%	60.3%	60.6%	59.1%	58.1%	
StarCoderBase	55.8%	55.5%	55.3%	58.1%	58.5%	59.8%	60.0%	59.3%	61.0%	61.7%	61.0%	

FIM task across these LLMs, assessing their median anomaly score under various fixed context sizes, as depicted in Figure 4. This figure reveals common patterns across the LLMs: 1) All models showed a performance drop in mask reconstruction accuracy with inadequate context sizes; 2) After LLMs acquire enough context information, their anomaly scores become constant; and 3) Significant gaps were observed between the scores for vulnerable and non-vulnerable lines across all models.

To precisely determine whether these LLMs can distinguish between vulnerable and non-vulnerable lines, we calculated ROC-AUC scores for contexts between 2 to 500 lines, as shown in Table 3. All scores consistently exceeded the 50% threshold of random guessing, and surpassed 60% once the models had sufficient context window sizes, demonstrating the effectiveness of all models in identifying vulnerable lines as anomalous. Notably, the best performance was achieved by CodeLlama-13B with a fixed context size of 300, reaching an ROC-AUC score of 62.9%.

When comparing different sizes of the same model, particularly CodeLlama-13B and CodeLlama-7B, the larger model achieved slightly higher ROC-AUC scores, particularly when performance stabilized at larger context sizes, indicating that it has greater discriminating ability. However, this improvement is not without trade-offs. The larger CodeLlama model requires significantly more GPU memory for inference. On our H100 GPU, we discovered three samples which triggered CUDA out-of-memory errors when using large fixed context sizes; specifically, for 400 and 500 line contexts, one sample and two samples respectively trigger this error due to long line lengths. In contrast, the 7B parameter model had a peak memory usage of 52GB, out of the 80GB available on the

H100 GPU with the same workload. This reduction in memory usage makes the smaller model a better choice when GPU memory is limited. While we have only conducted experiments on the CodeLlama models, these results are promising, and suggest that vulnerability detection accuracy should improve with neural model scaling.

3.4 RQ3: Adaptive Context Size

While fixed context sizes have demonstrated fairly good performance in anomaly detection, we propose that utilizing the Adaptive Context (AC) will further improve general anomaly and vulnerability discovery. To evaluate the effectiveness of AC, we conducted AC FIM experiments using the CodeLlama-13B model and compared the results to those obtained with fixed context sizes. As shown in Figure 5, the blue bars represent results using fixed context sizes, with values directly taken from the first row of Table 3, while the orange bar represents the result achieved with AC. Adopting AC significantly improves the ROC-AUC score between vulnerable and non-vulnerable lines to 63.7%, surpassing all ROC-AUC scores achieved with fixed context sizes. Additionally, the AC approach resolved the CUDA out-of-memory issues encountered with larger fixed contexts, reducing peak memory usage to 67GB and improving energy efficiency. These results highlight a key insight: while varying fixed context sizes can affect the average discrimination ability, certain lines may significantly confuse the model when an inappropriate context size is used. Allowing the context to not have to be centered around the line being analyzed also reduces the presence irrelevant lines that may confuse the model. A dynamic context size based on code structure, such as AC, proves more effective in distinguishing between vulnerable and non-vulnerable code.

We further measured the time required to conduct experiments under different context sizes, as shown in Figure 5. The graph reveals a clear trend: with fixed context sizes, the elapsed time increases as the context window enlarges. This outcome is expected, as larger context sizes demand more computational resources, leading to increased contention for hardware resources and, ultimately, slower inference times for the LLM. The longest time recorded in our experiment occurred with a 500-line context size, requiring approximately 70 minutes to complete, which is more than double that for a 100-line context size.

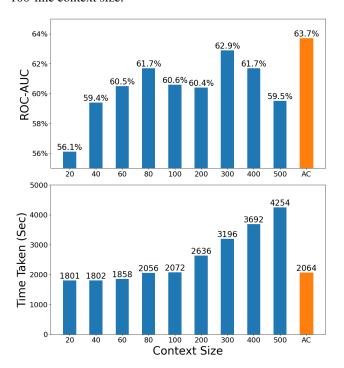


Figure 5: ROC-AUC and Time Taken on fixed context sizes vs. Adaptive Context

Interestingly, applying AC as the context size significantly reduced the time required to complete one round of experiments, making it nearly as fast as using a fixed context size of 80 lines. To investigate this phenomenon, we analyzed the distribution of AC sizes across our 512 data samples, as shown in Figure 6. The majority of AC sizes are relatively small, with a median value of 86.5 lines. Given our observation that elapsed time correlates positively with context size, this smaller median size explains the shorter experimental runtime when using AC. The graph also surges in AC frequency at a size of 300 lines. This anomaly arises because, as described in Section 2.2, a default context size of 300 lines was applied when no suitable compound statement could fit within the 500-line limit.

In summary, the AC approach not only provides the best

discriminative ability between vulnerable and non-vulnerable lines, but also serves as an economical solution. By reducing GPU memory usage and improving throughput, AC achieves greater computational efficiency while maintaining high detection accuracy, making it a practical choice for real-world applications.

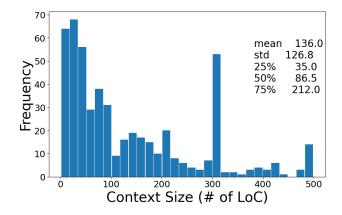


Figure 6: Frequency of Different AC Sizes

3.5 RQ4: Line-level Vulnerability Detection

In this section, we compare ANVIL, which does not require any labelled vulnerability data, against three state-of-the-art supervised-learning-based vulnerability detectors: LineVul, LineVD and LLMAO. LineVul [13] is a software vulnerability detector that offers both function-level and line-level detection capabilities. It fine-tunes CodeBert [12] using the BigVul [11] vulnerability dataset. The detection process begins at the function level, where LineVul assesses whether a function contains any vulnerabilities. If a function is deemed vulnerable, LineVul then evaluates each line within that function. It does this by aggregating the attention scores from CodeBert's transformer layers for each token in a line, resulting in a composite attention score for that line. A higher score means the line is more likely to be vulnerable. The intuition is that, for a vulnerability prediction task, lines with higher attention scores are deemed more critical to the model output, suggesting a greater likelihood of being linked to the root cause of the vulnerability.

LineVD [16], like LineVul, also supports both function-level and line-level detection. LineVD also uses CodeBert to generate code embeddings for each function and its constituent lines. However, it extends its analysis framework by utilizing Joern [17] to create a graph representation for each function. This graph, along with the code embeddings, are processed using a graph attention network and a multi-layer perceptron to simultaneously detect vulnerabilities at both the function and line levels. This system is trained on the BigVul dataset, similar to LineVul. A distinctive feature of LineVD is

its *consistency mechanism*: it employs element-wise multiplication within its model to ensure that if a function is classified as non-vulnerable, then all lines within that function are also automatically classified as non-vulnerable.

LLMAO [41], in contrast, is an LLM-based line-level vulnerability detector built on the CodeGen [25] series. Unlike LineVul and LineVD, which utilize CodeBERT with 350M parameters, LLMAO employs larger models with 6B and 16B parameters, similar in scale to ANVIL. LLMAO adopts a two-phase approach: first, it uses CodeGen with a left-to-right causal mask to collect the final hidden states for each line of code (LoC) within a context window of 128 lines. These hidden states are then concatenated and passed through a transformer-based bidirectional classification head to compute probabilities for each line, indicating their likelihood of being vulnerable.

While LLMAO was originally trained on the Devign [43] dataset, we retrained it using the BigVul dataset to ensure consistency with LineVul and LineVD. This decision was driven by Devign's limited coverage, as it includes only two opensource projects (FFmpeg and Qemu), which fail to provide sufficient generalizability to real-world scenarios involving diverse and massive codebases. In contrast, BigVul contains data from 4,296 open-source projects, offering a significantly broader and more comprehensive representation. To validate this reasoning, we also conducted the same set of experiments using LLMAO's original checkpoint from their GitHub repository, trained on Devign. These results, presented in Appendix A, demonstrate that adopting the more diverse BigVul training set improves almost all metrics evaluated in this section.

In this section, all experiments on ANVIL are conducted using the CodeLlama-13B model with the hybrid anomaly scoring function (Equation 1) and Adaptive Context (AC), because these configurations generally yield the best performance as demonstrated in answers to the previous RQs. To ensure fairness and comparability in our evaluation, we will use the same metrics as those employed by our baseline models. We evaluate ANVIL vs. LineVul, LineVD, and LLMAO using two types of metrics: 1) We approach vulnerability detection as a binary classification task and measure the linelevel classification performance using the ROC-AUC score. This metric treats each line of code as an independent data sample, assessing the overall classification accuracy without setting a specific threshold. 2) To gauge how effective ANVIL's anomaly score is as a vulnerability prioritization tool, we evaluate all detectors with a ranking task in which each line within a known vulnerable function is assigned a detector-specific score. We then use Top-N accuracy (a standard metric for measuring vulnerability prioritization ability) to assess whether any of the vulnerable lines are included in the detector's top-n most confident predictions. Additionally, we report the Mean First Ranking (MFR) to provide an overall measure of the models' vulnerability prioritization capabilities. For these metrics, each function with known vulnerabilities is treated as a separate data sample.

All evaluations in this section are performed on the Prime-Vul dataset, as described in Section 3.1. We selected this dataset over Magma and BigVul to mitigate potential over-fitting: the supervised-learning-based detectors were trained and tuned on the BigVul dataset, while ANVIL, despite not requiring supervised training, relied on the Magma dataset to select its configurations in previous experiments. These processes could introduce biases specific to their respective datasets. By using PrimeVul as a separate evaluation dataset, we ensure independence and promote a fairer comparison.

3.5.1 Line-level Vulnerability Classification

We first evaluate ANVIL against LineVul, LineVD, and LL-MAO in their ability to classify line-level vulnerabilities using ROC-AUC scores. This experiment uses the PrimeVul dataset with several accommodations to ensure fairness and compatibility. First, following the independent testing principle outlined in [7,27,28], we ensured that the testing and training sets include data from different sources to avoid bias caused by duplicated data distributions. Specifically, we included only projects not part of the training datasets (BigVul and Devign) used for the baselines. For the remaining data, we included only functions with corresponding source files available to support varying context window sizes used in ANVIL and LLMAO. Furthermore, in line with LineVD and LLMAO. all whitespace lines were removed from the source files. To identify vulnerable lines, we considered only vulnerable functions with paired patched versions, comparing the differences between the two to obtain a superset of potentially vulnerable modified lines. Comments were then filtered out to create the final set of vulnerable lines. This process resulted in a dataset containing 924 vulnerable LoC and 33,820 non-vulnerable LoC from 273 vulnerable functions, along with 325,353 LoC from 9,273 benign functions. These vulnerabilities span 40 different CWEs, with detailed breakdowns provided in Appendix B.

Additionally, as ANVIL operates differently from the three other tools, we made the following adjustments. LineVul depends on CodeBERT for generating embeddings, which is limited to processing a maximum of 512 input tokens. For functions longer than 512 tokens, LineVul truncates them to include only the first 512 tokens, discarding the rest of the function. Consequently, we evaluate LineVul only on lines of code that it keeps, which is 455 vulnerable LoC and 192,382 non-vulnerable LoC in the dataset, whereas the other three tools are evaluated on the full dataset of approximately 360k LoC. Notably, LineVD, another CodeBERT-based detector, does not have this limitation because it generates embeddings for each line individually, allowing it to process longer functions without truncation.

Another limitation of LineVul is that it supports line-level

Table 4: Vulnerability Detection & Localization on PrimeVul Dataset

Method	Trained On	Top-1↑	Top-3↑	Top-5↑	MFR↓	N-MFR↓	ROC-AUC↑
ANVIL (7B) ANVIL (13B)	N/A N/A	10.3% 9.9%	26.7% 27.8%	37.0% 37.4 %	28.9 26.8	0.23 0.22	55.7% 56.8%
LLMAO (6B) LLMAO (16B)	BigVul BigVul	7.4% 9.2%	19.9% 17.3%	26.6% 26.2%	50.8 49.3	0.40 0.39	51.5% 51.8%
LineVD	BigVul	9.2%	15.4%	23.1%	38.0	0.30	49.6%
LineVul	BigVul	4.0%	17.0%	30.0%	17.6	0.71	49.6%

detection only within functions it classifies as vulnerable, whereas line-level detection is not performed for functions deemed non-vulnerable, as per its original implementation. In contrast, all other tools support line-level detection across all functions. To maintain consistency in evaluation, we adopted the method used in LineVD: for functions classified as non-vulnerable by LineVul, all lines within those functions are automatically considered non-vulnerable.

Furthermore, LineVD employs a different method for identifying vulnerable lines when comparing a CVE with its patched version. Specifically, in addition to lines directly modified in the patch, LineVD considers lines that are control- or data-dependent on the patched lines as vulnerable. In contrast, ANVIL and the other two baselines label only the patched lines as vulnerable. To resolve this discrepancy and ensure a fair comparison, we disabled LineVD's dependency mechanism during our experiments.

As shown in Table 4, ANVIL achieves ROC-AUC scores of 56.8% and 55.7% with the CodeLlama-7B and 13B models, respectively, outperforming all three baselines. Specifically, ANVIL (13B) achieves 1.10 times higher ROC-AUC than the best configuration of LLMAO (16B) and 1.15 times higher than both LineVD and LineVul. Notably, LineVul and LineVD achieve only 49.6% ROC-AUC, which is essentially the same as random guessing (50%), while LLMAO performs marginally better than random. Moreover, when comparing different LLM sizes within the same model (e.g. 7B vs. 13B), the improvement in performance is modest, with gains of less than 1.02 times, as observed in both ANVIL and LLMAO.

3.5.2 Vulnerability Prioritization

This experiment evaluates the ability of vulnerability detectors to prioritize vulnerable lines within a given vulnerable function by comparing their Top-N accuracies and Mean First Ranking (MFR). Specifically, the Top-N accuracy metric measures whether at least one vulnerable line is included among the top N lines ranked by each detector. For ANVIL, lines are ranked based on their anomaly scores, while LineVul ranks lines using attention scores, and LineVD and LLMAO rely on logits from their respective neural network heads for line ranking. We report Top-1, Top-3, and Top-5 accuracies, as

these metrics are originally used by the baselines. Additionally, we calculate MFR (smaller is better) by averaging the rank of the first vulnerable line in each function, providing a comprehensive measure of the tools' overall vulnerability prioritization performance. This experiment uses 273 vulnerable functions from the PrimeVul dataset, consisting of 924 vulnerable LoC and 33,820 non-vulnerable LoC.

As shown in Table 4, ANVIL achieves significantly higher Top-N accuracies compared to its baselines. While ANVIL achieves a moderate 1.12 times better Top-1 accuracy than the best-performing version of LLMAO (16B) and LineVD, its performance becomes more pronounced when the criterion is relaxed to Top-3 and Top-5, with ANVIL achieving 1.40 and 1.41 times higher accuracies, respectively. Notably, the 512-token limit of LineVul results in an average of only 25 available LoC per function, compared to an average function length of approximately 141 LoC for the other detectors. This shortened context theoretically gives LineVul an advantage in achieving higher Top-N accuracy due to the smaller candidate pool. However, its performance falls short, achieving only 4.0% Top-1 accuracy (the lowest among all detectors), and its Top-3 and Top-5 accuracies remain significantly below those of ANVIL.

This shortened function length also explains why LineVul is biased in achieving the highest MFR. To provide a more objective and unbiased comparison, we introduce the Normalized Mean First Ranking (N-MFR), which adjusts the MFR by normalizing it against the average function length. For Line-Vul, we apply a normalization factor of 25 LoC, while for all other detectors, we use an average function length of 141 LoC. The N-MFR results reveal that ANVIL with CodeLlama-13B achieves the best score of 0.22, which is 44% lower than LL-MAO (16B) and 27% lower than LineVD. Although LineVul originally records the best MFR, its normalized version drops to 0.71, making it the worst performer among all detectors.

The impact of LLM size on detection performance appears to be limited. For ANVIL, using the larger CodeLlama-13B model does not result in significant improvements over the 7B version. Interestingly, for Top-1 accuracy, the 7B model slightly outperforms the 13B model by 0.4%. This trend is even more pronounced with LLMAO, where the 6B version

Table 5: ANVIL's ger	eralizability on 2024	CVEFixes Dataset

Method	Dataset	Top-1↑	Top-3↑	Top-5↑	Top-10↑	MFR↓	N-MFR↓	ROC-AUC↑
ANVIL (13B)	PrimeVul	9.9%	27.8%	37.4%	53.1%	26.8	0.22	56.8%
ANVIL (13B)	2024 CVEFixes	9.5%	26.5%	40.1%	67.4%	16.6	0.22	67.9%

generally outperforms the 16B version, except at Top-1 accuracy. These observations align with our earlier findings on ROC-AUC, suggesting that smaller models can sometimes offer better performance and may be a more practical choice than their larger counterparts.

3.6 RQ5: Data Leakage

In this section, we address concerns about potential data leakage that may arise if our patches for the vulnerabilities in our evaluation dataset were present in the LLM training data, as this may enable the LLM to correctly generate the patched code instead of the vulnerable code, as discussed in 3.1. Due to time constraints and the extensive scope of validations required across multiple models, this evaluation is primarily conducted on the CodeLlama-13B model. For this experiment, we continue to use the hybrid scoring function and AC configuration.

We assess the generalizability of ANVIL in detecting linelevel vulnerabilities by comparing its detection performance between the aforementioned PrimeVul dataset and our newly collected 2024 CVEFixes dataset, as described in 3.1. Similar to RQ4, this evaluation examines both vulnerability classification and prioritization performance.

Similar to previous experiments, we evaluate vulnerability prioritization using Top-N accuracies, MFR, and N-MFR. For N-MFR, we apply a normalization factor of 76 LoC, representing the average vulnerable function length in the 2024 CVE-Fixes dataset. The evaluation is conducted on the 147 vulnerable functions extracted from 100 CVEs. As shown in Table 5, ANVIL demonstrates consistent Top-1 to Top-5 accuracies across both datasets. However, while these accuracies remain at similar levels, we initially expected slightly higher metrics for CVEFixes compared to PrimeVul due to the shorter average function length in CVEFixes. This discrepancy possibly arises from the manual inspection process described in Section 3.1, where our limited familiarity with the codebases may have resulted in some vulnerabilities being overlooked and mislabelled as non-vulnerable. These overlooked vulnerabilities could appear among ANVIL's top predictions, leading to nominally lower Top-1 and Top-3 accuracies.

To address the potential effects of this mislabelling, we extended our analysis to include Top-10 accuracy, providing a more lenient criterion to better reflect ANVIL's performance. Under this expanded evaluation, ANVIL achieved Top-5 and Top-10 accuracies of 40.1% and 67.4%, respectively, on the

2024 CVEFixes dataset, outperforming the corresponding 37.4% and 53.1% accuracies observed with PrimeVul. This aligns with our earlier claim that ANVIL should theoretically perform better on the CVEFixes dataset due to its shorter function lengths. Notably, as the criterion is relaxed from Top-5 to Top-10, the performance gap between the two datasets widens further, highlighting ANVIL's ability to prioritize vulnerabilities effectively within the top-ranked lines in a function. Additionally, after normalization, the N-MFR for both datasets converges to a score of 0.22. This consistency underscores the robustness of ANVIL's vulnerability prioritization capabilities, even when applied to unseen data.

For the evaluation of vulnerability classification, we assess all 3,736 functions in the 2024 CVEFixes dataset, which consists of 235 vulnerable LoC and over 110k non-vulnerable LoC, using ROC-AUC scores. As shown in Table 5, ANVIL achieves a surprisingly higher ROC-AUC score of 67.9% on the 2024 CVEFixes dataset compared to 56.8% on the Prime-Vul dataset. This result confirms that our anomaly-based approach is robust and remains unaffected by potential data leakage, accurately detecting vulnerabilities even in unseen datasets. The observed difference in ROC-AUC scores may be influenced by various factors, such as differences in average function length, dataset difficulty, the degree of sparsity of vulnerabilities, or the ratios between vulnerable and benign functions. Future research could systematically explore how these dataset characteristics impact the performance of vulnerability detectors.

4 Discussion

4.1 Vulnerable vs Patched lines

In addition to collecting 512 vulnerable and non-vulnerable lines, we also gathered 353 patched lines from the Magma dataset for the 138 known CVEs. These lines include the additions and modifications made to address the CVEs. Additionally, when a defect-fixing commit deletes a line, we also include the subsequent line in the same location as part of the patched lines.

While our primary focus remains on vulnerability detection, an intriguing pattern emerged regarding these patched lines. To elucidate, we examined the exact match accuracy (the proportion of lines where the generated output exactly matches the ground truth) across all line types under various contexts, depicted on the left side of Figure 7. Expectedly,

Table 6: Patch vs.	Viil vs Non-viil	Lines in ROC-AUC

		Context Size										
P-value	2	10	20	40	60	80	100	200	300	400	500	AC
Patch vs. Vul	53.4%	53.9%	54.0%	55.5%	55.3%	56.1%	55.4%	57.1%	57.1%	57.1%	57.2%	56.8%
Patch vs. Non-vul	50.8%	51.1%	50.7%	52.3%	53.7%	53.9%	54.0%	52.7%	54.8%	53.7%	51.7%	55.2%

non-vulnerable lines exhibited higher exact match accuracies compared to vulnerable lines, but notably, patched lines consistently showed intermediate accuracy levels.

Further analysis using the median anomaly scores under fixed context sizes revealed that patched lines generally align closely with non-vulnerable lines, albeit slightly higher. This trend shifts markedly under the Adaptive Context (AC) settings, where the exact match accuracy for patched lines drops below 50%, causing their median anomaly scores to approach those of vulnerable lines (see Figure 7 right).

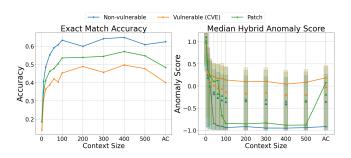


Figure 7: Patch vs. Vulnerable vs. Non-vulnerable Lines

To explore this further, we evaluated our approach's ability to distinguish patched lines from other line types using ROC-AUC scores, as shown in Table 6. All scores comparing patched lines to other line types exceeded the 50% threshold of random guessing, indicating that ANVIL differentiates among the three line types. However, the extent to which the scores surpass the threshold varies. Specifically, the ROC-AUC scores for distinguishing patched lines from vulnerable lines exceed the threshold by more than 1.14 times, whereas the scores for differentiating patched lines from non-vulnerable lines show a narrower margin, ranging from 50.7% to 55.2%.

The conclusion we draw from these experiments is that even after a vulnerability has been patched, the code that fixes the vulnerability still exhibits some statistical differences with regular code. One potential explanation is that some percentage of patched code is still vulnerable which is consistent with insights from the Magma study [14], so the patched lines are actually a mix of vulnerable and non-vulnerable code. Another potential explanation is that the code locations where vulnerabilities tend to exist may simply have higher entropy, or depend on global factors, which makes it difficult for current LLMs (and possibly even for humans) to generate correctly.

5 Threats to Validity

Internal Validity. One threat to internal validity lies in data labelling. Correctly labelling which lines are vulnerable continues to be a challenging problem, as vulnerabilities are often difficult to isolate and determining what code is considered vulnerable can take a wide array of definitions. To mitigate this, we use the Magma dataset for our vulnerability detection experiments (RQ1-3). As mentioned in Section 3, all labelled vulnerabilities in Magma are provided with a PoV through extensive manual review and fuzzing campaigns, which ensures that lines labelled as vulnerable are indeed related to the vulnerabilities. Further, manual labelling by a small set of developers, results in better consistency that is less impacted by different definitions of "vulnerable" that may arise in datasets scraped from CVE reports.

Maintaining a diverse range of code samples while ensuring high labeling accuracy is always a trade-off in generating vulnerability datasets. To the best of our knowledge, we choose the PrimeVul dataset in RQ4 due to its extensive diversity of open-source projects and the large number of vulnerabilities it contains. Moreover, as mentioned earlier, PrimeVul validates the authenticity of vulnerabilities by automatically analyzing corresponding vulnerability descriptions from the NVD database. This process enables PrimeVul to achieve a labeling accuracy of over 90%, outperforming other vulnerability datasets such as Devign [43], DiverseVul [7], and CrossVul [26].

For the leakage experiment (RQ5), we manually curate the 2024 CVE-Fixes datasets to ensure the correctness of the labelling. However, while manual curation on its own is likely to have fewer mislabels than simply scraping data from CVE reports, it can still be imperfect due to the complex structure and the large code base of the included projects.

External Validity. While we demonstrated in Section 3.6 that ANVIL generalizes to non-leakage data by evaluating its consistency across the PrimeVul and 2024 CVEFixes datasets, we concede that numerically comparing the accuracy across two data sets may yield an imprecise conclusion since the two datasets are composed of two different samples of vulnerabilities, and it is nearly impossible to control this. While we could increase the number of samples in both datasets to decrease the chances of sample error, manually finding and reproducing real vulnerabilities is intensive and difficult. Future research could aim to collect a more extensive non-leakage dataset to conduct a larger-scale evaluation, providing a more robust statistical comparison of ANVIL's generalizability to

non-leakage data.

Construct Validity. To comment on our selection of measurements, we use common metrics that are also employed in other works to evaluate the vulnerability detection capability of all examined tools. Additionally, we set the temperature of the LLMs used in our experiments to 0 to remove any potential randomness in the generation of output.

6 Related Work

Anomaly Detection. Previous work has used anomalous behaviour as a signal for buggy code. Yun et al. [42] detects API misuse by automatically modelling correct usage patterns from existing code. They use this model to detect anomalous uses, and hence bugs. Other work from Ahmadi et al. [2] operates on singular codebases and clusters functionally similar yet inconsistent code. Their insight is that small inconsistencies between code that should have the same functionality can lead to bugs (e.g., missing checks). To our knowledge, the only previous attempt to leverage LLMs for anomalyguided vulnerability detection used a simple string distance score between the LLM prediction and ground truth to determine if a line of source code is anomalous [1]. A string distance calculation lacks any contextual understanding of the strings being compared, and ANVIL improves upon this with a cross-entropy-based anomaly measure. Further, their method of detecting anomalies relies heavily on pre-existing source code comments; our approach makes no assumptions on the contents of the software under examination. Finally, LLM-based anomaly detection in other non-code domains has also been explored, especially in log analysis and network intrusion detection [35].

LLMs. Following the success of natural language generation with LLMs, work such as CodeBERT [12] and CodeT5 [38] have applied transformers toward code generation and understanding. CodeBERT is an encoder-only transformer that supports bidirectional natural language-programming language generation. However, it requires an additional decoder layer that cannot benefit from pre-training. To address this, Wang et al. proposed CodeT5, an encoder-decoder transformer. CodeT5 is pre-trained on tasks in which source code identifiers, and entire spans of code are masked for prediction. VulGen [28], extends previous work by training an LLM to identify and inject vulnerabilities into the codebase of programs for the purposes of synthetic data generation.

ML-Assisted Dynamic Vulnerability Detection. Many existing fuzzers have leveraged machine learning to boost fuzzer performance. He et al. [15] demonstrated a neural network's ability to imitate the behaviour of a symbolic execution engine. They use a symbolic engine to generate input transactions to a corpus of smart contracts. They then train a neural network to predict these transactions. The network is then integrated into a fuzzer which they use to test unseen

smart contracts. Recent work from Shi et al. [33] uses a convolutional neural network to classify the behaviour of basic blocks. They then correlate these basic blocks with bytes in the program input. They use this mapping from input bytes to classification to guide an input-format-aware fuzzer. As for LLMs, a recent paper utilized ChatGPT to parse protocol specifications into a machine understandable grammar, which they then integrate into a protocol aware fuzzer [23].

ML-Assisted Static Vulnerability Detection. To detect use-before-initialization bugs, Li et al. [18] augments a state-of-the-art static analysis tool with an LLM prompting technique. They utilize the LLM to extract variable initialization information when the static analysis tool is unable to handle certain code features (e.g., unsupported library calls). Conversely, Sun et al. [36] first utilizes an LLM to match candidate source code with potential vulnerabilities, then uses static analysis to verify its legitimacy. They apply their method to find vulnerabilities in smart contracts.

ML-Based Vulnerability Detection. ML-based vulnerability detection largely focuses on the architecture and code features that will better lead a model to automatically detect vulnerabilities. An early approach from Russell et al. [32] attempts to find C/C++ vulnerabilities with a convolutional neural network. Further advances have experimented with graph neural networks [9] and long short-term memory models [21]. Unfortunately, on unseen bugs, these architecture level differences have not resulted in generalization to unseen, real-world vulnerabilities [6], and another work shows the same trend for prompt-guided LLM-based detectors [34]. Other work has focused on using different code features for training vulnerability detectors, such as code slices [24] and bug-triggering paths [8].

7 Conclusion

To address the limitations of existing supervised-learningbased vulnerability detectors, we propose ANVIL, which leverages model mispredictions as guidance for software vulnerability detection. Our method capitalizes on the inherent capabilities of LLMs to generate normatively correct code, utilizing discrepancies between the model's code predictions and the actual code, to flag potential vulnerabilities. By employing code generation tasks across different LLMs, we have shown that this discrepancy is generalizable to different model sizes and model architectures, and we also reveal that using Adaptive Context (AC) outperforms simpler fix-sized contexts, providing a more relevant and precise framework for evaluating vulnerabilities. Furthermore, comparative evaluations against state-of-the-art supervised models LineVul, LineVD and LL-MAO highlight ANVIL's superior performance, achieving significantly better vulnerability identification without the need for a labelled training dataset.

Open Science

We make all code for our tool ANVIL available at https://anonymous.4open.science/r/anvil. All datasets and scripts used for running our evaluation are also made public for reproducibility.

Ethics Considerations

All experiments in this study were conducted using publicly available vulnerability datasets, where all vulnerabilities are published with unique Common Vulnerabilities and Exposures (CVE) IDs in the NVD database, and have been properly patched. All projects involved in the experiments are open-source and publicly accessible on the Internet, ensuring that no privacy concerns arise from our experiments. Furthermore, all vulnerability detectors used in this study are based on open-source machine learning models and are locally deployed on our machines. This eliminates the need for live systems or third-party API services and no private information is used in this study. Additionally, all detectors operate as static ML-based tools, analyzing only the static source code without interacting with any running systems.

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A Vulnerability Classification & Prioritization on PrimeVul using LLMAO Trained on Devign

Method	Trained On	Top-1↑	Top-3↑	Top-5↑	MFR↓	N-MFR↓	ROC-AUC↑
LLMAO (6B) LLMAO (16B)	Devign Devign	5.9% 4.4%		25.8% 22.5%		0.34 0.40	51.3% 50.7%

B Distribution of CWEs in PrimeVul Validation and Test sets

	CWE	Name	Line Count
1	CWE-787	Out-of-bounds Write	174
2	CWE-703	Improper Check or Handling of Exceptional Conditions	168
3	CWE-125	Out-of-bounds Read	101
4	CWE-416	Use After Free	88
5	CWE-476	NULL Pointer Dereference	77
6	CWE-369	Divide By Zero	56
7	CWE-400	Uncontrolled Resource Consumption	45
8	CWE-190	Integer Overflow or Wraparound	30
9	CWE-20	Improper Input Validation	29
10	CWE-119	Improper Restriction of Operations within the Bounds of a Memory Buffer	24
11	CWE-120	Buffer Copy without Checking Size of Input	23
12	CWE-354	Improper Validation of Integrity Check Value	18
13	CWE-617	Reachable Assertion	16
14	CWE-200	Exposure of Sensitive Information to an Unauthorized Actor	14
15	CWE-189	Numeric Errors	14
16	CWE-444	Inconsistent Interpretation of HTTP Requests	13
17	CWE-754	Improper Check for Unusual or Exceptional Conditions	13
18	CWE-362	Concurrent Execution using Shared Resource with Improper Synchronization	12
19	CWE-310	Cryptographic Issues	6
20	CWE-415	Double Free	6
21	CWE-59	Improper Link Resolution Before File Access	6
22	CWE-287	Improper Authentication	4
23	CWE-401	Missing Release of Memory after Effective Lifetime	3
24	CWE-843	Access of Resource Using Incompatible Type	3
25	CWE-191	Integer Underflow (Wrap or Wraparound)	2
26	CWE-276	Incorrect Default Permissions	2
27	CWE-399	Resource Management Errors	2
28	CWE-79	Improper Neutralization of Input During Web Page Generation	2
29	CWE-295	Improper Certificate Validation	2
30	CWE-668	Exposure of Resource to Wrong Sphere	1
31	CWE-704	Incorrect Type Conversion or Cast	1
32	CWE-665	Improper Initialization	1
33	CWE-269	Improper Privilege Management	1
34	CWE-345	Insufficient Verification of Data Authenticity	1
35	CWE-824	Access of Uninitialized Pointer	1
36	CWE-122	Heap-based Buffer Overflow	1
37	CWE-924	Improper Enforcement of Message Integrity During Transmission in	1
38	CWE-908	Use of Uninitialized Resource	1
39	CWE-134	Use of Externally-Controlled Format String	1
40	CWE-284	Improper Access Control	1
	Total		964