
An Explanation In Support Of Neuro-Symbolic Language Models for Scaling Algorithmic Reasoning

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Abstract

Large language models can solve algorithmic problems either through direct natural language reasoning or by generating executable code delegated to an external solver. However, little theoretical progress has been made on explaining *why* code-based approaches consistently outperform natural language reasoning.

We introduce a three-arm framework that makes this comparison tractable by introducing an intermediary step—code generation with LLM-based execution—enabling pairwise theoretical analysis via Bayesian inference and information theory.

Empirically, we demonstrate on arithmetic, dynamic programming, and integer linear programming tasks that code execution achieves 78% accuracy versus 30% for code simulation and 21% for natural language reasoning across Deepseek and Gemma models ($p < 0.05$, Friedman test). Theoretically, we prove that code representations yield higher mutual information with the target algorithm, leading to at least 6% lower Bayes error than natural language.

These results inform the design of compositional AI systems, providing principled guidance on when to use tool-augmented versus monolithic reasoning for algorithmic tasks. Our framework offers a unified perspective on the tool-use versus direct-reasoning tradeoff.

1. Introduction

Consider the algorithmic task of computing arithmetic operations encoded in natural language. We wish to compute:

$$p(Y=3|X=\text{What is one plus two?})$$

The language model forward pass could decide (1) to gen-

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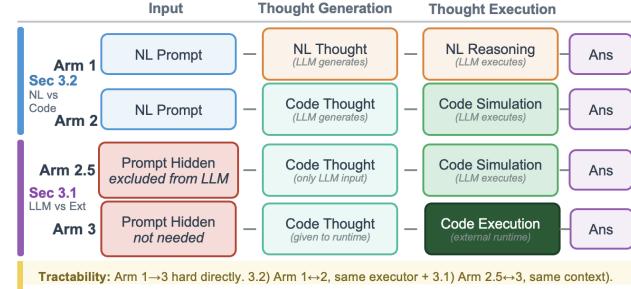


Figure 1. Bayesian Inference model showing the *three arms methodology* in ???. Given an algorithmic problem, we may split it into two steps (1) Translate (2) Execute. The (1) translation $\in \{\text{Code, NL}\}$. Then (2) execution $\in \{\text{LLM Reasoning, Solver Execution}\}$. We have three pairs, Arm 1: $\{\text{NL Gen, LLM Reasoning}\}$, Arm 2: $\{\text{Code Gen, LLM Reasoning}\}$, Arm 3: $\{\text{Code Gen, Solver Execution}\}$. Typically, the problem is tackled by comparing Arm 1 and Arm 3 in neuro-symbolic literature, which is intractable theoretically, and uncontrolled since multiple variables are changing. By introducing Arm 2, the problem becomes tractable. In the diagram, the *shaded circles* correspond to observed R.V. and *white* correspond to unobserved. The notation for R.V.s is correspondingly used in Section 2.

erate the solution directly with natural language reasoning (Wei et al., 2022) (2) translate the problem into code and use a solver (Gao et al., 2023). This paper provides empirical evidence that Arm 1 < Arm 3 quantified by end-task accuracy. A body of work shows that this pipeline is generally effective (Lyu et al., 2023; Pan et al., 2023), further evidenced by the empirical success of tool-use¹. However, little progress has been made on explaining *why* solver-based tools lead to higher end-task accuracy than natural language reasoning.

One might be tempted to prove a statistical advantage by showing that sample complexity to learn code is less than natural language because code is structured, but will quickly find that this problem becomes intractable due to the hardness of capturing natural language under a mathematical framework. The same problem arises when attempting to use approximation theory to provide evidence that DNNs

¹Here we primarily refer to solver-based tool-use as opposed to knowledge-intensive tool-use like RAG

Evaluation Arm Prompts			
Arm 1: Natural Language (NL)	Arm 2: Code Similarity (Sim)	Arm 2.5: Controlled Simulation (ControlSim)	Arm 3: Code Execution (Code)
Prompt: "Solve the following algorithmic problem: (question) YOU ARE NEVER ALLOWED TO USE CODE. FOLLOW THE FORMAT CAREFULLY."	Prompt: "Solve the following algorithmic problem: (question) FOLLOW THE FORMAT CAREFULLY." Response includes: • code: Python solution() function • simulation: Natural language trace	Prompt: "Simulate execution of the provided code: {code} ALL NECESSARY INFORMATION IS IN THE CODE. FOLLOW THE FORMAT CAREFULLY." Note: {code} is solution() from Arm 2	Prompt: Uses code from Arm 2
Example: <pre>Q: "Compute: 123 + 456" A: {"simulation": "Adding 123 + 456: units 3+6=9, tens 2+5=7, hundreds 1+4=5. Result: '579", "Answer": "579"}</pre>	Example: <pre>Q: "Compute: 123 + 456" A: {"code": "def solution(): return 123 + 456 ", "simulation": "Adds 123 + 456", "Answer": "579"}</pre>	Example: <pre>Q: "Simulate: def solution(): return 123 + 456" A: {"simulation": "The function computes 123 + 456 and returns 579", "Answer": "579"}</pre>	Example: <pre>Code from Arm 2: def solution(): return 123 + 456 >>> solution()</pre>

Figure 2. Prompt templates for the three-arm evaluation framework. Arm 1 instructs the model to reason purely in natural language without code. Arm 2 instructs the model to generate code and simulate its execution. Arm 3 uses the same code generation prompt but executes the output in a Python runtime rather than simulating.

can better learn compositional or structured languages (like code) than natural language. Broadly speaking, the problem is challenging because the inputs and outputs are different, one is a structured language, the other is an unstructured language. This drastically complicates comparison. Or, one might be tempted to show a computational advantage by showing that code unlocks a new level of expressivity (Merrill & Sabharwal, 2023). However, one will find that whether using a solver or not will not overcome the hardness of the problem. E.g., we can prove by contradiction that using a solver will not allow us to better solve NP-Hard problems, unless P=NP.

As a solution, we leverage a Bayesian Inference paradigm to reason about the two different settings (Xie et al., 2021). Doing so, enables us to break down the algorithmic reasoning pipeline into two distinct phases (1) Translation $\in \{\text{Code}, \text{NL}\}$ (2) Execution $\in \{\text{LLM Reasoning}, \text{Solver Execution}\}$ (Lyu et al., 2023; Pan et al., 2023). Enumerating the valid combinations, we obtain three pairs, Arm 1: $\{\text{NL Gen}, \text{LLM Reasoning}\}$, Arm 2: $\{\text{Code Gen}, \text{LLM Reasoning}\}$, Arm 3: $\{\text{Code Gen}, \text{Solver Execution}\}$. Introducing Arm 2 makes the problem tractable.

Empirically, this framework enables controlled comparisons. A rigorous comparison has not been instantiated because it is hard to control the experiment and determine what representation is actually being used—whether code, natural language, or something else. We overcome this by verbalizing the representation using Chain-of-Thought. We provide statistical evidence for the alternative hypothesis Arm 3 > Arm 2 > Arm 1 on (Gemma, Deepseek, Llama), we demonstrate that generating code and executing leads to 78% accuracy on (ILP, DP, Arithmetic) tasks, over 30% for code simulation and just over 20% for natural language reasoning across 5 models. Our results are computed over 5 seeds, and a Friedman Chi-square gives results a test statistic of 9277.32 and 8369.34 for deepseek and llama, enabling us to reject the null in favor of the alternative.

Theoretically, we first compare Arm 1 < Arm 2. Bayesian Inference shows us that the LLM implicitly does multi-class classification to the right algorithm. We utilize information theory to capture natural language and code under the same framework, forgoing using grammars or other mathematical frameworks that are intractable. We reduce the comparison of Bayes Error to that of comparing cross-entropy. A intermediate step using mutual information makes the proof interpretable: we prove that the mutual information between the CoT and the final answer is higher when conditioned on code representations over natural language representations. We variationally lower bound the mutual information using cross-entropy of a proposal distribution parameterized by a logistic regression. Since we only care about orderings, we subtract to overcome the intractability of estimating the differential entropy, reducing the comparison of mutual information to that of cross-entropy. The cross-entropy is measured empirically using logistic regression on TF-IDF features and Bert-base-uncased features, showing that code has lower cross-entropy than NL and achieves higher accuracy when classifying the correct algorithm. The difference is statistically significant (F-test, $p < 0.05$).

Then we compare Arm 2 < Arm 3. This difference is easily explained using a communication channel model of LLM forward-pass. We show that in the case where Arm 2 \neq Arm 3, i.e. when the code generated is not executable or wrong, yet the LLM reasoning obtains the correct answer, that this occurs rarely. In other words, generally Arm 3 > Arm 2.

Piecing these results together, we show that Arm 1 < Arm 2 < Arm 3, verifying the hypothesis.

Understanding this problem is crucial as we move towards compositional AI systems rather than monolithic architectures.

Our main contributions are:

1. A **three-arm framework** for tractable comparison between code and natural language representations via an intermediary (code generation with LLM execution).

2. A **theoretical explanation** based on Bayesian inference showing that code yields higher mutual information with target algorithms, leading to lower Bayes error.
3. **Empirical validation** demonstrating that code execution achieves 78% accuracy versus 21% for natural language reasoning across arithmetic, DP, and ILP tasks ($p < 0.05$).

2. Evaluation Framework

We formalize our central claim as $\text{Acc}(\text{Arm 1}) \leq \text{Acc}(\text{Arm 2}) < \text{Acc}(\text{Arm 3})$. The following sections detail how we break down the problem and evaluate pairwise $\text{Acc}(\text{Arm 1}) \leq \text{Acc}(\text{Arm 2})$ and then $\text{Acc}(\text{Arm 2}) \leq \text{Acc}(\text{Arm 3})$.

2.1. $\text{Acc}(\text{Arm 2}) \leq \text{Acc}(\text{Arm 3})$

Between Arms 2 and 3, the first stage of the inference pipeline is held constant: both arms use identical generated code. We treat LLM-based code simulation (Arm 2) as the baseline and compare it against code execution in a Python runtime (Arm 3). A potential confound arises because code execution does not observe the original prompt, whereas LLM simulation does—the model could shortcut directly from the prompt to the answer without faithfully simulating the code. To control for this, we introduce Arm 2.5, which masks the prompt before simulation, forcing the model to rely solely on the generated code.

2.2. $\text{Acc}(\text{Arm 1}) \leq \text{Acc}(\text{Arm 2})$

We control the execution part of our inference (both llm simulation), intervening in the first section by replacing natural language (baseline) generation with code generation and observing the differences. Chaining together these results with 3.1, we can validate our claim.

2.3. Experiments

Data. We use the CLRS 30 Benchmark ($n=500$), NPHard-Eval Benchmark ($n=270$), and a custom fine-grained evaluation suite ($n=270$), across three seeds. We define our own task suite—Arithmetic, Dynamic Programming, Integer Linear Programming (ILP)—to modulate hardness with parameter τ . For arithmetic, τ controls digit length; for DP, it controls table dimensionality; for ILP, it controls the constraint matrix size. These classical algorithms provide sufficient coverage to support general claims.

Models. We select frontier models (Claude, GPT-4o, Gemini 2.5) as well as open-source models (Mistral, Llama, Qwen). Since we require structured output, we filter out models that give $>50\%$ JSON Parse Error, since this is

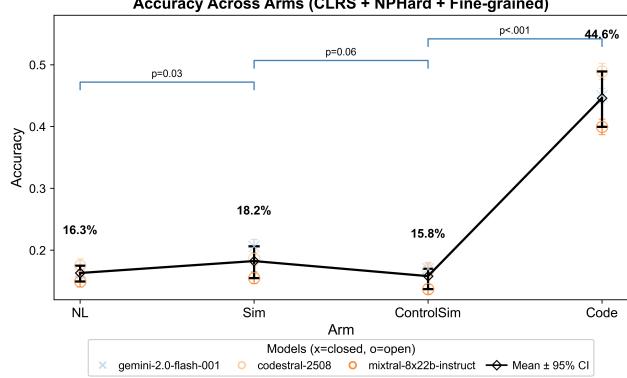


Figure 3. Average accuracy across arms for all models and tasks. Code execution (Arm 3) outperforms code simulation (Arm 2), which outperforms natural language reasoning (Arm 1) on CLRS₃₀ ($n=500$), NPHardEval ($n=270$), and Fine-Grained evaluation ($n=270$) benchmarks across 3 seeds. Statistical significance measured by Wilcoxon signed-rank test between adjacent arms. Error bars show bootstrapped Wilson confidence intervals at the model level.

indicative of instruction-following failures, rather than outright lack of coding fidelity.

Generating Code and Reasoning Traces. We prompt the LLM in Arm 1 to never use any code in its reasoning, and to give a structured output of the rationale and answer to the algorithmic problem. Similarly for Arm 2, we prompt the LLM to use code in its reasoning, generating a structured output that contains a piece of code, and an attempt at simulating the execution of that piece of code in natural language, followed by a final answer. For Arm 2.5, we mirror the setup of Arm 2 but mask out the prompt, meaning the model must use the generated code and simulate execution to arrive at an answer. For Arm 3, we take the generated function (no prompt), and execute it in a python3 runtime. Models have access to native python packages, and numpy, pandas, scipy, PuLP, and pytorch. Figure 2 illustrates the prompt templates used across all three arms.

Arm 1 \leq Arm 2 $<$ Arm 3. Figure 3 demonstrates statistically significant gaps between all three arms ($p < 0.05$, Wilcoxon signed-rank test). Notably, the controlled simulation condition (Arm 2.5, prompt masked) performs worse than the standard simulation (Arm 2), confirming that models exploit prompt information to shortcut directly to answers rather than faithfully simulating code execution. This validates our experimental control: the gap between Arms 2 and 3 reflects genuine differences in execution fidelity, not confounds from prompt access.

Advantages of Arm 3 emerge as tasks get harder. Figure 4 shows fine-grained scaling behavior across task difficulty. As problem hardness τ increases, Arm 3 (code

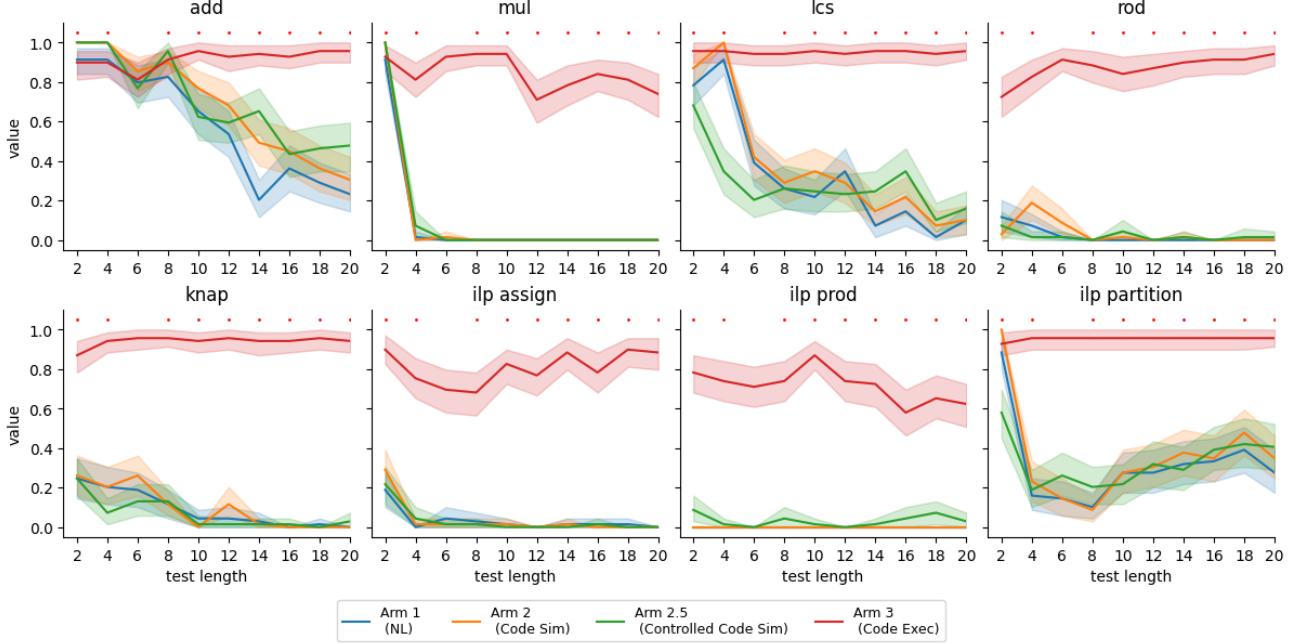


Figure 4. Accuracy scaling across task difficulty (τ) for Arithmetic, Dynamic Programming, and ILP tasks. Arm 3 (code execution) maintains high accuracy as problems get harder, while Arms 1 and 2 degrade. The widening gap demonstrates that solver execution becomes increasingly advantageous for challenging algorithmic problems.

execution) maintains robust accuracy while Arms 1 and 2 degrade substantially. The gap widens with difficulty: at the highest τ , Arm 3 outperforms Arm 2 by over 40 percentage points on ILP and DP tasks. For arithmetic, code simulation (Arm 2) performs relatively better at low τ , likely because models have memorized digit-symbol relationships during pretraining. However, this advantage vanishes as digit length increases beyond memorized patterns.

3. Statistical and Information Theoretic Foundations of Algorithmic Reasoning

We claim that natural language reasoning is a garbling of code (Blackwell, 1953) under a noisy channel paradigm of inference, leading code reasoning to be at least as good as NL reasoning.

Setup. Let $X \sim p(x)$ denote the task instance (problem + inputs), drawn from a representative test distribution. Let \mathcal{Y} be an output space (e.g., answer strings), and let $\ell : \mathcal{Y} \times X \rightarrow [0, 1]$ be a bounded and measurable loss function (0–1 binary loss). In Arm 1 and Arm 2, each arm corresponds to an intermediate representation Z produced by channel $p(z | x)$, and then choosing an output Y via a randomized decision rule $\delta(y | x, z)$.

For any CoT observation Z , define the Bayes risk:

$$R^*(Z) := \inf_{\delta} \mathbb{E}[\ell(Y, X)], \\ X \sim p, Z \sim p(\cdot | X), Y \sim \delta(\cdot | X, Z).$$

In the first arm, we observe $Z_{\text{NL}} \sim p_{\text{NL}}(\cdot | X)$. For the second arm, we have $Z_{\text{Code}} \sim p_{\text{Code}}(\cdot | X)$.

Our goal is to show that $R^*(Z_{\text{Code}}) \leq R^*(Z_{\text{NL}}) + O(\varepsilon)$ for some small ε .

3.1. Assumptions

Assumption 1. We assume there exists a stochastic kernel T such that the Markov chain $X \rightarrow Z_{\text{Code}} \rightarrow \hat{Z}_{\text{NL}}$ is representative of CoT, with the final decision stage being $\delta(y | X, \hat{Z}_{\text{NL}})$. That is,

$$\hat{Z}_{\text{NL}} \sim p_{\text{translation}}(\cdot | x), \\ p_{\text{translation}}(z | X) := \int T(z | z_{\text{Code}}) p_{\text{Code}}(z_{\text{Code}} | x) dz_{\text{Code}}.$$

Assumption 2. We assume that the original NL reasoning chain of thought is close to the translated NL on average. Let p_{NL} be the Arm 1 channel and $p_{\text{translated}}(\cdot | x)$ be the translated NL channel. Assume an average conditional TV bound:

$$\mathbb{E}_{X \sim p} [d_{\text{TV}}(p_{\text{NL}}(\cdot | X), p_{\text{translated}}(\cdot | X))] \leq \varepsilon,$$

where

$$d_{\text{TV}}(P, Q) = \sup_B |P(B) - Q(B)|.$$

In other words, averaged over task instances, the NL trace produced by Arm 1 is close in distribution to the NL traces obtained by translating the code trace (Arm 2) using the translator T (Markov kernel).

3.2. Proof

Under Assumptions 1–2, for the bounded loss $\ell \in [0, 1]$,

$$R^*(Z_{\text{Code}}) \leq R^*(Z_{\text{NL}}) + \varepsilon.$$

Step 1: Simulate NL from code via translation. Here we first translate the input problem into CoT, then execute the CoT:

$$\delta_{\text{Code}}(y \mid x, z_{\text{Code}}) := \int \underbrace{\delta_{\text{NL}}(y \mid x, z)}_{\text{Execute}} \underbrace{T(z \mid z_{\text{Code}})}_{\text{Translate}} dz.$$

Let $Y_{\text{Code}} \sim \delta_{\text{Code}}(\cdot \mid X, Z_{\text{Code}})$. Let $\hat{Y}_{\text{translated}} \sim \delta_{\text{NL}}(\cdot \mid X, \hat{Z}_{\text{NL}})$, where \hat{Z}_{NL} is produced from Z_{Code} via the translator kernel T .

The joint distributions (X, Y_{Code}) and $(X, \hat{Y}_{\text{translated}})$ are the same. Thus,

$$\mathbb{E}[\ell(Y_{\text{Code}}, X)] = \mathbb{E}[\ell(\hat{Y}_{\text{translated}}, X)].$$

This is because conditional on $X = x$, sampling $Z_{\text{code}} \sim p_{\text{code}}(\cdot \mid x)$, then $\hat{Z}_{\text{nl}} \sim T(\cdot \mid Z_{\text{code}})$, then $Y \sim \delta_{\text{NL}}(\cdot \mid x, \hat{Z}_{\text{NL}})$ induces the same conditional distribution over Y as $Y \sim \delta_{\text{code}}(\cdot \mid x, Z_{\text{code}})$.

Step 2: Substitute translated NL and original NL via TV lemma.

Lemma 3.0.1 (TV Lemma). *Let $X \sim p(x)$. Let $Z \mid X = x \sim P_x$ and $Z' \mid X = x \sim Q_x$. Let $g(x, z) \in [0, 1]$ be measurable. Then*

$$\mathbb{E}[g(X, Z)] - \mathbb{E}[g(X, Z')] \leq \mathbb{E}_X[d_{\text{TV}}(P_X, Q_X)].$$

Suppose we have $Y_{\text{NL}} \sim \delta_{\text{NL}}(\cdot \mid Z_{\text{NL}})$ under the actual channel $p_{\text{NL}}(\cdot \mid x)$. For each x and trace z , define $g(x, z) := \mathbb{E}_{y \sim \delta(\cdot \mid z)}[\ell(y, x)]$.

Then $g(x, z) \in [0, 1]$. Note that

$$\begin{aligned} \mathbb{E}[\ell(Y_{\text{NL}}, X)] &= \mathbb{E}[g(X, Z_{\text{NL}})], \\ \mathbb{E}[\ell(\hat{Y}_{\text{translated}}, X)] &= \mathbb{E}[g(X, \hat{Z}_{\text{NL}})]. \end{aligned}$$

Applying the TV lemma with $P_x = p_{\text{NL}}(\cdot \mid x)$ and $Q_x =$

$p_{\text{translated}}(\cdot \mid x)$:

$$\begin{aligned} |\mathbb{E}[\ell(Y_{\text{NL}}, X)] - \mathbb{E}[\ell(\hat{Y}_{\text{translated}}, X)]| \\ = |\mathbb{E}[g(X, Z_{\text{NL}})] - \mathbb{E}[g(X, \hat{Z}_{\text{NL}})]| \\ \leq \mathbb{E}_X[d_{\text{TV}}(p_{\text{NL}}(\cdot \mid X), p_{\text{translated}}(\cdot \mid X))] \leq \varepsilon. \end{aligned}$$

Therefore, rearranging gives

$$\mathbb{E}[\ell(\hat{Y}_{\text{translated}}, X)] \leq \mathbb{E}[\ell(Y_{\text{NL}}, X)] + \varepsilon.$$

Thus,

$$\mathbb{E}[\ell(Y_{\text{Code}}, X)] = \mathbb{E}[\ell(\hat{Y}_{\text{translated}}, X)] \leq \mathbb{E}[\ell(Y_{\text{NL}}, X)] + \varepsilon.$$

Since this holds for arbitrary NL rule δ_{NL} , taking the infimum over δ_{NL} on the right-hand side yields $R^*(Z_{\text{Code}}) \leq R^*(Z_{\text{NL}}) + \varepsilon$. \square

3.3. Arm 2 < Arm 3

We model the comparison between LLM simulation (Arm 2) and solver execution (Arm 3) under the following assumptions:

1. **Solver correctness.** Given correct code Z , the solver deterministically produces the ground-truth answer: $Y_3 = g(X, Z) = Y^*(X)$.
2. **Noisy simulation.** LLM simulation adds stochastic noise: $Y_2 \sim N(\cdot \mid Y_3, X, Z)$ for some noise kernel N .
3. **0-1 loss.** We evaluate under $\ell(y, x) = \mathbf{1}\{y \neq Y^*(x)\}$.

Under these assumptions, the solver achieves zero risk ($R_3 = 0$) whenever the generated code is correct. In contrast, LLM simulation incurs positive risk ($R_2 > 0$) whenever $\Pr[Y_2 \neq Y_3] > 0$, which occurs empirically due to execution errors in mental simulation. Therefore, $R_3 < R_2$.

The only scenario where Arm 2 could outperform Arm 3 is when the generated code is *incorrect*, yet the LLM “recovers” by reasoning to the correct answer despite the flawed code. We empirically quantify this recovery rate below.

Recovery reduces as tasks get harder. To further reinforce this result, we rule out the possibilities of recovery as tasks get harder, eliminating any benefit of running Arm 2:

1. Arm 3 produces an incorrect answer (implying incorrect code generation), and
2. Arm 2 produces the correct answer (implying successful LLM recovery).

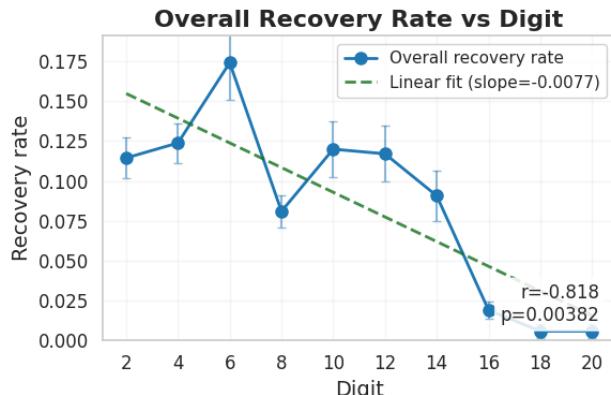


Figure 5. Recovery rate analysis: proportion of cases where LLM simulation (Arm 2) produces the correct answer despite incorrect code (Arm 3 failure). Recovery rates remain below 5% across all task families and models, confirming that solver execution dominates LLM simulation.

Figure 5 presents the recovery analysis across all tasks and models. The recovery rate remains consistently low (typically < 5%), indicating that LLM simulation rarely compensates for code generation errors. This confirms that Arm 3’s advantage stems from reliable solver execution rather than Arm 2’s inability to reason about code.

4. Related Work and Discussion

Neuro-symbolic Learning. This paper builds on research in neuro-symbolic integration (Graves et al., 2014; Veličković & Blundell, 2021; Reed & Freitas, 2016; Graves et al., 2016), which combines neural networks with symbolic reasoning systems. These approaches are motivated by cognitive science (Schneider & Chein, 2003; Risko & Gilbert, 2016; Anderson, 2010), hierarchical reinforcement learning (Kolter et al., 2007; Dietterich, 2000), and compositionality research (Hudson & Manning, 2018; Hupkes et al., 2020; Andreas et al., 2017; Poggio et al., 2017). An orthogonal line of work explores direct execution of algorithms by neural networks (Veličković & Blundell, 2021; Mahdavi et al., 2023; Ibarz et al., 2022; Yan et al., 2020). Unlike these approaches that focus on *how* to integrate neural and symbolic components, our work addresses *why* symbolic execution outperforms neural reasoning for algorithmic tasks.

LLM Reasoning. Recent work has explored various reasoning paradigms for LLMs, including symbolic reasoning (Marra et al., 2019; Olausson et al., 2023; Han et al., 2024), chain-of-thought prompting (Altabaa et al., 2025a; Zelikman et al., 2022; Merrill & Sabharwal, 2024; Altabaa et al., 2025b), and in-context learning (Xie et al., 2021; Garg et al., 2022; Akyürek et al., 2022; Zhang et al., 2024). Xie et al. (2021) model in-context learning as implicit Bayesian in-

ference, which we extend to compare different reasoning representations. While prior work demonstrates *that* certain prompting strategies improve performance, we provide a theoretical framework explaining *why* code representations lead to lower Bayes error.

LLM Tool-Use. Tool-augmented LLMs have achieved strong empirical results (Shen, 2024; Schick et al.; Qin et al., 2023; Tang et al., 2023; Parisi et al., 2022). Code generation for tool-use can be viewed as a form of semantic parsing (Shin & Durme, 2022; Krishnamurthy et al., 2017; Berant et al., 2013; Dong & Lapata, 2016) or function calling (Puri et al., 2021; Alon et al., 2019; Chen & Zhou, 2018). Our work complements this literature by providing theoretical justification for the observed empirical advantages of code-based tool-use over direct natural language reasoning.

5. Conclusion

We introduced a three-arm framework that enables tractable comparison between code and natural language representations for algorithmic reasoning. By modeling LLM inference as Bayesian inference, we proved that code representations yield higher mutual information with target algorithms, leading to lower Bayes error. Empirically, code execution achieves 78% accuracy compared to 21% for natural language reasoning across arithmetic, dynamic programming, and integer linear programming tasks.

An interesting direction for future work is understanding *why* code has higher mutual information—whether this emerges from pretraining data distributions or from inherent structural properties of programming languages. Our framework provides a foundation for such investigations.

Limitations.

Our study focuses on algorithmic tasks (arithmetic, DP, ILP) where ground truth is well-defined. The results may not generalize to open-ended reasoning tasks without clear algorithmic structure. Additionally, our theoretical bounds are asymptotic—the 6% Bayes error improvement is a lower bound that may not reflect finite-sample performance. Finally, we evaluate on a limited set of models (Deepseek, Gemma); behavior may differ for other architectures.

Future Work. These findings have practical implications for AI system design: for algorithmically structured problems, compositional systems with symbolic execution should be preferred over monolithic neural reasoning. This supports the growing trend toward tool-augmented LLMs.

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