Capturing Failures of Large Language Models via Human Cognitive Biases

Abstract

Large language models generate complex, open-ended outputs: instead of out-putting a class label they write summaries, generate dialogue, or produce working code. In order to asses the reliability of these open-ended generation systems, we aim to identify qualitative categories of erroneous behavior, beyond identifying individual errors. To hypothesize and test for such qualitative errors, we draw inspiration from human cognitive biases—systematic patterns of deviation from rational judgement. Specifically, we use cognitive biases as motivation to (i) generate hypotheses for problems that models may have, and (ii) develop experiments that elicit these problems. Using code generation as a case study, we find that OpenAI's Codex errs predictably based on how the input prompt is framed, adjusts outputs towards anchors, and is biased towards outputs that mimic frequent training exam-ples. We then use our framework to elicit *high-impact errors* such as incorrectly deleting files. Our results indicate that experimental methodology from cognitive science can help characterize how machine learning systems behave.

1 Introduction

Recent large language models have achieved new, exciting capabilities. In contrast to traditional classifiers, these models can generate open-ended text, enabling use cases like summarization [Stiennon et al., 2020], dialog [Thoppilan et al., 2022], and code generation [Chen et al., 2021].

The open-ended power of these systems, however, poses new reliability challenges. We must understand not only when systems err, but also the *kinds* of errors they make, as some errors are much more costly than others. For example, erroneous code that does not compile is less dangerous than code that deletes all files in the home directory. Studying how frequently an error occurs is difficult, as the same error (e.g. delete all files) can appear in a wide range of syntactically diverse outputs. In order to better reason about how systems err, we need methods to test whether systems make the same qualitative error across different prompts, even when the generated outputs differ.

In this paper, we primarily study failures of code generation models. These models complete programs from comments, descriptions of code functionality, or initial lines of code. Code generation is particularly amenable to study since it is *objective*: generated solutions are unambiguously correct or incorrect. Yet it is also *open-ended*: the set of programs a model could output is arbitrarily large, so the rates at which different outputs appear (often at most once per dataset) are not very descriptive.

To identify qualitative failure modes of large language models, we draw from the literature on human cognitive biases. Cognitive biases are systematic ways in which humans deviate from rational judgment [Tversky and Kahneman, 1974]. For example, Tversky and Kahneman find that humans inadequately adjust estimates away from initial values, and disproportionately recall distinctive examples. To uncover cognitive biases, Tversky and Kahneman ask questions that are crafted to

Figure 1: Illustration of our experimental framework. We first use a cognitive bias (framing effect) to inspire a potential failure mode of Codex (relying on irrelevant information in the prompt). Next, we transform inputs in a way that we suspect will elicit the targeted failure mode (prepending irrelevant functions). We evaluate if transforming inputs changes accuracy, and if the model outputs have elements that are indicative of the targeted failure.

reveal some conjectured irrationality. They uncover qualitative insights into human behavior from the responses, without relying on complete mechanistic insight into the minds that they aim to analyze.

In this work, we extend Tversky and Kahneman's experimental methodology to elicit failure modes of large language models, without relying on complete mechanistic insight into their behavior (Figure 1). Given a potential failure mode (e.g. relying on irrelevant information in the input), we construct a transformation over inputs that largely preserves semantics, but that we suspect will elicit the failure (e.g. prepending an irrelevant function). We first measure if the model is sensitive to the transformation, by measuring if it decreases accuracy. Then, we check that the model outputs have elements that are indicative of the targeted failure (e.g. copies the irrelevant function).

We draw on four different cognitive biases to hypothesize potential failures of OpenAI's Codex [Chen et al., 2021], then apply our framework to each. Our results indicate that Codex often relies on irrelevant information when generating solutions, adjusts its solution towards related-but-incorrect solutions, is biased based on training-set frequencies, and reverts to computationally simpler problems when faced with a complex calculation. We also apply our framework to OpenAI's GPT-3 [Brown et al., 2020], and show that it updates its predictions towards anchors.

Finally, we show that our framework can uncover *high-impact errors*: errors that are harmful and difficult to undo. Specifically, we use our framework to systematically generate prompts where Codex erroneously deletes files. Our results indicate that experimental methodology from cognitive science can help uncover failure modes of complex machine learning systems.

2   Related Work

Large language models. Recent work has developed large, capable, autoregressive language models, which predict future tokens from past tokens [Radford et al., 2019, Wang and Komatsuzaki, 2021, Brown et al., 2020, Chen et al., 2021, Rae et al., 2021]. These models can be used for open-ended generation tasks such as summarization [Stiennon et al., 2020, Ziegler et al., 2019, Rothe et al., 2020], dialogue [Ram et al., 2018, Thoppilan et al., 2022], and long form question answering [Fan et al., 2019], among others. Model-generated code has been used to solve both programming and statistics questions [Chen et al., 2021, Tang et al., 2021].

There is some existing work studying failures of large language models. Benchmarks that measure model performance on multiple choice questions [Wang et al., 2019b,a, Hendrycks et al., 2021b], mathematics [Hendrycks et al., 2021c, Cobbe et al., 2021], long-form question answering [Lin et al., 2021, Gabriel et al., 2021, Shuster et al., 2021, Krishna et al., 2021], and coding problems [Hendrycks et al., 2021a, Chen et al., 2021] reveal inputs that the model errs on, but not the kind of error it makes. Another line of work shows that test-based language models can internalize bias and stereotypes [Sheng et al., 2019, Nadeem et al., 2020, Groenwold et al., 2020, Blodgett et al., 2021, Gehman et al., 2020], and proposes applying fairness measurements from cognitive social sciences to machine learning systems [Jacobs and Wallach, 2021]. Some work adversarially prompts models to leak training data [Carlini et al., 2020], or output specific content [Wallace et al., 2019, Carlini et al., 2020]. And a final line of work identifies additional potential failures of current and future machine learning systems [Bender et al., 2021, Bommasani et al., 2021, Weidinger et al., 2021].

```
Prompt              ┌ def largest_divisor(n: int) -> int:          def sum(x, y):                  ┐ Irrelevant
                    │     """ For a given number n, find the largest     """                          │ Preceding
                    │     number smaller than n that divides n evenly    Returns the sum of x and y    │ Function
                    │     >>> largest_divisor(15)                        """                          │
                    │     5                                              print('Hello world')         ┘
                    └     """
Canonical  ┌ for i in reversed(range(n)):                    def concatenate(s):
Solution   │     if n % i == 0:                                   """
           └         return i                                     Returns the concatenation of all
Test       ┌ def check(candidate):                               strings in s
Cases      │     assert candidate(3) == 1                        """
           └     assert candidate(49)== 7                        print('Hello world')
```

Figure 2: **Left.** Example of a HumanEval problem from Chen et al. [2021]. The problem contains a prompt (blue), a canonical solution to the prompt (green), and a few test-cases (black). The prompt contains two components: a function signature (first line), and a docstring (remaining lines). **Right.** Illustration of our framing experiment. The transformed prompt (everything above the black line) contains an irrelevant preceding function (IPF) prepended to a prompt from HumanEval (blue). The IPF contains a randomly chosen prompt from HumanEval (purple) and a framing line (red). The output Codex generates (below the black line) matches the framing line. When we omit the random HumanEval prompt and the framing line (leaving only blue), Codex produces the correct output.

Cognitive biases. Tversky and Kahneman [1974] define *human cognitive biases*: systematic patterns of deviation from rational judgment. They observe that humans employ heuristics when computing probabilities or assessing values, and that these heuristics lead to predictable errors. An extensive body of follow-up work has added to, refined, and validated the pool of known cognitive biases [Tversky and Kahneman, 1973, 1981, Strack et al., 1988, Kahneman and Frederick, 2002, Windhager et al., 2010, Meyer, 2014].

Some known failure modes of large language models resemble cognitive biases. Zhao et al. [2021] and Liu et al. [2021] show that the specific random samples used for few-shot learning can change GPT-3's prediction on binary and multiple choice tasks. Similarly, Wallace et al. [2019] show that innocuous prompts can routinely generate toxic model output. Our framework builds on this work by (i) identifying the link to cognitive biases, (ii) focusing on open-ended generation, and (iii) leveraging Tversky and Kahneman's experimental methodology to elicit qualitative failure modes.

3 Codex Experiments

3.1 Model and benchmarks

We primarily study OpenAI's Codex, a large language model trained to generate code from docstrings [Chen et al., 2021]. For our experiments, we use the OpenAI API to query the "davinci-001" version of Codex, and use greedy decoding ot generate solutions. Details of this model architecture are not public, but it is likely similar to the largest model from Chen et al. [2021]: a 12B parameter version of GPT-3 [Brown et al., 2020] that is fine-tuned on GitHub instead of the CommonCrawl. Like GPT-3, Codex is an autoregressive model—given a sequence of previous tokens, Codex predicts the next token. Practitioners query Codex with partial programs, docstrings, or function signatures, and receive completions as output.

In order to identify whether Codex has some failure mode, we need to systematically generate prompts that elicit that failure. To do so, we apply transformations to standard prompts. We use two benchmarks as sources of prompts to transform: HumanEval, and MathEquations.

HumanEval. We use the HumanEval benchmark as a diverse source of "normal" prompts [Chen et al., 2021]. HumanEval contains 164 programming problems, each of which includes a function signature and a docstring. The docstring contains an English description of the desired functionality and a few example input-output pairs. HumanEval also contains a canonical solution for each program, which we use in Section 3.2.2. We give an example problem from HumanEval in Figure 2.

MathEquations. We also curate a set of prompts of basic arithmetic functions. For example, we prompt Codex to "Write a function that sums the squares of its inputs", or "Write a function that sums its inputs called product_plus_five". Further details are given in Sections 3.2.3 and 3.2.4.

	Functional accuracy		Outputs framing line	
	ORIGINAL	FRAMED	ORIGINAL	FRAMED
`raise NotImplemented error`	32.9	6.1	1.4	85.0
`pass`	32.9	5.5	9.7	90.2
`assert False`	32.9	8.8	0.0	81.4
`return False`	32.9	7.3	11.5	54.5
`print("Hello world!")`	32.9	20.1	0.0	32.6

Table 1: Results of the framing experiments. We compare functional accuracy and the rate at which framing line is outputted over HumanEval with (framed) and without (original) irrelevant preceding functions. We find that the irrelevant preceding functions lower functional accuracy across all framing lines. Moreover, we find that the outputted function often appears verbatim in the generated output, suggesting that Codex relies on irrelevant information in the prompt.

3.2 Codex results

In this section, we show how cognitive biases can (i) inspire hypotheses for potential failure modes, and (ii) help us design experiments to test these hypotheses. Our approach has three steps. First, we construct a transformation over prompts that largely preserves semantics, but that we suspect will elicit a specific cognitive-bias-inspired failure mode. Next, we measure if Codex is sensitive to the transformation, by measuring the decrease in accuracy. And finally, we check that the generated output has elements that are indicative of the targeted failure mode. Our approach mirrors the high-level methodology from Tversky and Kahneman [1974]; we empirically elicit specific failure modes using targeted prompts, without complete mechanistic insight into the system that we study.

We draw inspiration from four cognitive biases: the framing effect (Tversky and Kahneman [1981]; Section 3.2.1), anchoring (Tversky and Kahneman [1974]; Section 3.2.2), the availability heuristic (Tversky and Kahneman [1973]; Section 3.2.3), and attribute substitution (Kahneman and Frederick [2002]; Section 3.2.4).

3.2.1 Inspiration: Framing effect

We first draw inspiration from *the framing effect*: predictable shifts in human responses when the same problem is framed in different ways [Tversky and Kahneman, 1981]. In their study identifying the effect, Tversky and Kahneman [1981] find that subjects favor certainly saving 200 people over saving 600 with probability 1/3, yet prefer losing 600 with probability 2/3 over certainly losing 400 (even though these are equivalent). At its core, the framing effect shows how humans can rely on semantically irrelevant information when they make decisions.

Using the framing effect as inspiration, we hypothesize that Codex may generate solutions exclusively from irrelevant information in the prompt. To elicit this failure, we transform HumanEval prompts by prepending *irrelevant preceding functions*. Specifically, to generate irrelevant preceding functions, we combine a random prompt from HumanEval with a *framing line*. We test five framing lines: `raise NotImplementedError`, `pass`, `assert False`, `return False`, and `print("Hello world!")`. We first check that prepending these irrelevant preceding functions decreases Codex's functional accuracy.[1] Next, to test if Codex relied on irrelevant information in the prompt, we measure how much more frequently the framing line appears verbatim in the generated output.

We report the results of our framing experiments in Table 1. We find that adding irrelevant preceding functions consistently lowers functional accuracy, by between 12.8 and 27.4 points, across the different framing lines we tested. Moreover, Codex generates the framing line 67.4% of the time, compared to only 4.5% of the time over untransformed prompts. These results suggest that Codex erroneously relies on irrelevant information in predictable ways in the prompt to generate outputs, even in the extreme case when outputting the framing line contradicts the type specification in the function signature (`return False`).

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[1]Following Chen et al. [2021], we measure performance on HumanEval with *functional accuracy*: the fraction of programs that pass all of the test cases provided at the url: `https://github.com/openai/human-eval`.

```
def common(l1, l2):
  ret = set()
  for el in l1:
    for var in [l1, l2]:
    print(var)
```

| Anchor Function |
| HumanEval Prompt (def common...) |
| First solution lines |

| Full Prompt |
```
for var in [l1, l2]:
  if el1 in var:
    ret.add(e1)
return sorted(ret)
```

Figure 3: Illustration of our anchoring experiment using a real example (expanded in Figure 8). We construct the anchor function (left) by taking the function signature from the HumanEval prompt (blue), appending n lines of the canonical solution (green), then adding anchoring lines (red). We construct the full prompt (center) by combining the anchor function, the original HumanEval prompt, and the first n lines of the canonical solution. The solution Codex generates (right) combines elements of a canonical solution (checks condition and adds to ret.), with the anchor function (for var loop).

3.2.2 Inspiration: Anchoring

We next draw inspiration from *anchoring*: humans' tendency to insufficiently adjust their estimates away from initial values. For example, Tversky and Kahneman [1974] find that subjects' median estimate for the fraction of African countries in the UN shifts from 25% to 45%, based on whether they were first asked if the fraction was greater or less than 10% and 65%, respectively. Anchoring captures how humans adjust to partial information, versus irrelevant information (framing effect).

Using anchoring as inspiration, we hypothesize that Codex may adjust its output towards related solutions, when these solutions are included in the prompt. To elicit this failure, we prepend *anchor functions* to prompts: functions that are similar to a valid solution for a HumanEval prompt, but contain some error. We first check that prepending these anchor functions decreases Codex's functional accuracy, as in Section 3.2.1. Next, to test if Codex adjusts its output towards related solutions, we check that the generated solution contains elements of the anchor function.

We aim to construct anchor functions that are similar to functions in HumanEval prompt and that compile, but are incorrect. To do so, we take a prefix of the canonical solution, then add additional *anchor lines* that produce an incorrect output. See Figure 3 for an example. We describe two types of anchor lines, and how we test their influence on the generated solutions, in the following paragraphs.

Print-var anchor lines. We first study *print-var* anchor lines, which iterate over all variables in the function signature and print their values. For a function with inputs var1 and var2 , the associated print-var anchor lines are:

```
for var in [var1, var2]:
    print(var)
```

To study the influence of the print-var anchor lines on the solution, we measure how often (i) just the first line (for loop), and (ii) just the second line (print statement) appear in the generated solution.

Add-var anchor lines. We also study *add-var* anchor lines, which return the sum of all variables in the function signature (converted to strings). For a function with inputs var1 and var2 , the add-var anchor lines are:

```
tmp = str(var1) + str(var2)
return tmp
```

To study the influence of the add-var anchor lines on the solution, we measure how often `return tmp` appears in the generated solution.

Print-var results. In Figure 4, we show that prepending print-var anchor functions consistently lowers functional accuracy across different number of prompted canonical solution lines. We vary the number of canonical solution lines in order to study prompts of different difficulties; as the number of solution lines increases, the number remaining lines Codex must produce decreases.[2]

We additionally find that elements of anchor function often appear in Codex's output, suggesting that Codex adjusts its solution towards related solutions. In Figure 4, we see that Codex generates `for var` in 32%–61% of solutions when at least one line of the canonical solution is included, and

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[2]We filter out programs whose entire canonical solution would appear in the prompt; see Appendix A.1.

Figure 4: Results of the print-var anchoring experiment. **Left.** We measure the functional accuracy of Codex with no anchor function prepended (baseline acc) and with a print-var anchor function prepended (anchor acc), and find that prepending the anchor function consistently lowers accuracy. **Right.** We measure the influence of the anchor function on the generated solution by plotting the fraction of generated solutions that contain "`for var in`" from the print-var anchor prompt (for var loop), the fraction of generated solutions that include "`print(var)`" (prints var), and the fraction of generated solutions that output the anchor function verbatim without additional content (exact copy), as a function of the number of canonical solution lines added to the prompt.

generates `print(var)` in 26%–44% of solutions. Codex sometimes even incorporates the anchor lines into correct solutions; the `for var` loop is used in a correct solution for 3%–11% of all outputs, while `print(var)` is used in a correct solution for 1%–9% of outputs.

One concern might be that Codex just outputs the anchor function verbatim, as in Section 3.2.1, but we find that this does not explain the full results—when at least two solution lines are prompted, the output is an exact copy of the anchor function less than 20% of the time.

Add-var results. We next consider results for add-var anchor lines. Full results for the add-var anchor prompts are presented in Appendix A.1 and are qualitatively similar to the print-var results.

One again, we find that prepending the anchor function consistently lowers functional accuracy. Moreover, the outputted solutions often include an anchor line. For example, `return tmp` appears in 26.2%–46.7% of generated solutions, depending on how many canonical solution lines we prompt with. These results are not caused by Codex outputting the anchoring function verbatim: this only occurs between 7% and 12% of the time. Overall, our findings suggest that Codex can err by adjusting its output towards related solutions, when the solutions are included in the prompt.

3.2.3 Inspiration: Availability heuristic

We next draw inspiration from the *availability heuristic*: the tendency of humans to evaluate how frequently an example occurs based on how easy it is to recall. For example, Tversky and Kahneman [1973] find that humans tend to incorrectly report that there are more first words that start with "r" and "k" than have third letter "r" and "k", because the former quickly come to mind.

Using the availability heuristic as motivation, we hypothesize that Codex may err by outputting solutions to related prompts that appear more frequently in the training set. To elicit this failure, we start with prompts that apply a unary operation before a binary operation (unary-first), then flip the order (binary-first). Programmers tend to apply unary operations first (e.g. when computing Euclidean distances or variances), so we conjecture that they appear more frequently on GitHub. We first check that flipping the order of operations decreases accuracy. Next, to test if Codex instead outputs related prompts that occur more frequently in the training set, we measure whether Codex instead outputs the unary-first solution.

We consider all 12 combinations of the binary operations sum, difference, and product, with unary operations square, cube, quadruple, and square root. Across these combinations, we find that the Codex's accuracy drops from 50% to 17% when flipping the order from unary-first to binary-first. Moreover, among combinations where flipping the order leads to error, we find that 75% of the binary-first outputs are the unary-first solution. We exhibit a typical error in Figure 5: when prompted to square the sum of its inputs, Codex correctly generates the function name (`square_sum`), but

```
"""                                          """
Write a function that squares            Write a function that sums its
the sum of its inputs                    inputs called product_plus_2
"""                                          """
def square_sum(x, y):                    def product_plus_2(x, y):
    return x ** 2 + y ** 2                   return x * y + 2
```

Figure 5: **Left.** Availability heuristic example where Codex mixes up the order of operations. The correct function signature (blue), `square_sum` matches the prompt. However, the incorrect function call (red) instead squares its inputs before summing them. The prompt is above the horizontal line, while the generated code is below. **Right.** Attribute substitution example where Codex relies on the function name to generate output. Codex correctly generates the desired function name (blue), but errs by using the function name instead of the prompt to generate the return statement (red).

Name location	Correct	Matches function name	Other error
No name	100.0	-	0.0
Docstring	4.4	80.0	15.6
Function signature	4.4	70.0	25.6

Table 2: Results of the attribute substitution experiments. We report accuracy when we do not request a contradictory function name (no name), we request a function name in the docstring (docstring), and when we request a function name in the function signature (function signature). Overall, we find that Codex frequently generates solutions based on the function name.

instead sums the squares of its inputs. Our results suggest that Codex can err by outputting solutions to related, frequent prompts in the training set.

3.2.4 Inspiration: Attribute substitution

Finally, we draw inspiration from *attribute substitution*: the human tendency to respond to a complicated question using a simpler, related question [Kahneman and Frederick, 2002]. For example, a professor when asked how likely a candidate is to be tenured, may instead respond with how impressive they found their job talk.

Using attribute substitution as inspiration, we hypothesize that Codex may use simple-but-incorrect heuristics to generate solutions. To elicit this failure, we add requests for conflicting function names to MathEquation prompts. For example, in Figure 5 we prompt Codex to write a program that sums its inputs called `product_plus_2` . We first check that adding conflicting function names decreases Codex's functional accuracy. Next, to test if Codex uses simple-but-incorrect heuristics to generate solutions, we check whether the generate solution matches the function name.

We evaluate Codex using 90 MathEquation prompts where the desired solution and requested function name differ. To construct prompts, we begin with a prompt that Codex originally solves (sum, difference, or product), then append a request for a specific, contradictory function name (see Appendix A.2 for full implementation details).

We report our experimental results in Table 2. When we request a conflicting function name, Codex's accuracy drops from 100% to only 4.4%. This finding holds whether we request the function name in the docstring, or write it in the function signature. Moreover, for between 70% and 80% of prompts, Codex responds with the function specified in the function name. Our results indicate that Codex can err by using simple-but-incorrect heuristics to generate solutions.

4 GPT-3 Results

In this section, we extend our study from Codex to GPT-3. As in Section 3.2.2, we study anchoring: humans' tendency to insufficiently adjust their estimates away from an initial value [Tversky and Kahneman, 1974]. We largely replicate the anchoring study presented in Jacowitz and Kahneman [1995], but test the "davinci-001" version of OpenAI's GPT-3 instead of humans.

		Anchor change to model output		
p	No change	Towards anchor	Away from anchor	Gibberish
20%	10.7	28.6	10.7	50.0
50%	14.3	42.9	10.7	32.1

Table 3: Results of the adaptation of the anchoring study from Jacowitz and Kahneman [1995] on GPT-3. We consider anchors that are 20% and 50% increases and decreases from the ground truth answer, and measure how often GPT-3's revised prediction does not change, shifts towards / away from the anchor, or is gibberish, aggregated across lower and upper anchors.

In their original experiment, Jacowitz and Kahneman asked students to estimate quantities such as the length of the Mississippi river in miles. They then asked new students to estimate the same quantities, but first gave them a upper or lower bound on the true answer (e.g. the Mississippi river is longer than 700 miles), which they call *anchors*. They find that students tend to underestimate the true quantity when prompted with the lower anchor, and overestimate it when prompted with the upper anchor.

We adapt the anchoring study from Jacowitz and Kahneman [1995] by finding the true answer for 14 of their 15 original questions[3], then computing upper and lower anchors by increasing and decreasing the true answer by a fixed percentage p. See Appendix A.3 for a full list of questions and true answers. As an example, if the actual answer is 2000 and p is 50%, the upper anchor is 3000 and the lower anchor is 1000. We use this bound as an anchor, so that a typical prompt might be:

$$\underbrace{\text{The length of the Mississippi river (in miles) is greater than } 1000.}_{\text{anchor}}$$

What is the length of the Mississippi River (in miles)? Answer:

To study anchoring in GPT-3, we measure how prepending the anchor changes GPT-3's estimate. We categorize four potential changes: the estimate does not change, the estimate shifts towards the anchor, the estimate shifts away from the anchor, and the estimate is gibberish. We report the results in Table 3 for $p \in \{20\%, 50\%\}$. We find that GPT-3 routinely updates its estimate when an anchor is prepended, and tends to shift the estimate towards the anchor. We also find that while GPT-3's updated estimate sometimes matches the anchor exactly (67% of the time), it also often lands between the anchor and the original prediction, mirroring the behavior of humans.

Our replication has a few limitations. Like the original study our sample size is small, we construct prompts with templates, and many of the outputs—on average 41%—are gibberish. Nevertheless, our results suggest that GPT-3 incorporates the anchor during estimation.

5 High-Impact Errors

We have shown how our framework helps us elicit failures of large language models. In this section, we use our framework to construct cases where Codex makes *high-impact errors*: harmful errors that are hard to undo. Specifically, we construct prompts where Codex incorrectly deletes files.

As in Section 3.2.4 we draw inspiration from attribute substitution: the tendency of humans to respond to a complex question with a simpler, related question. Using attribute substitution as motivation, we hypothesize that Codex may simplify complex expressions such as conjunctions. Instead of checking all components of a conjunction at once, it might "give up" and consider subsets of the components individually (e.g. checking for A or $A \vee B$ instead of $A \wedge B$). To elicit this failure, we prompt Codex to delete files containing specific sets of package imports; see Figure 6 for an example. We measure how often Codex generates a simpler output that erroneously deletes files, as well as how often it produces the correct output. See Appendix A.4 for additional details.

We test for two types of simpler outputs: code deleting all files containing first package in the set (i.e. A instead of $A \wedge B$), and code deleting all files containing any package in the set (i.e. $A \vee B$ instead of $A \wedge B$). The latter operation is computationally simpler than checking if a file contains all packages, since Codex can delete a file whenever a single package in the set appears.

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[3]We omit a question asking for the number of bars in Berkeley, CA, since the answer is ambiguous

```
"""
Delete all files in an inputted directory that import all
of statsmodels, plotly, seaborn, and scipy
"""
import os
import re
def delete_all(directory):
    for root, dir, file in os.walk(directory):
        for f in files:
            with open(os.path.join(root, f), 'r') as fp:
                if re.search("import statsmodels", fp.read()):
                    os.remove(os.path.join(root, f))
```

Figure 6: **Left.** Example where Codex incorrectly deletes files. We prompt Codex to delete files containing all of statsmodels, plotly, seaborn, and scipy. Codex correctly iterates through all files in the inputted directory (blue), but then incorrectly deletes all files containing statsmodels (red), as attribute substitution suggests. **Right.** Plot describing the errors Codex makes as a function of the number of packages. We find that Codex often incorrectly deletes files if they contain any of the listed packages, and tends to increasingly rely just on the first package when deleting as the number of packages increases.

In Figure 6, we illustrate the breakdown of the errors Codex makes as a function of the number of package imports in the prompt. We find that Codex erroneously deletes files on at least 80% of prompts when the number of package imports is at least three, despite producing a correct output on 90% of prompts when the number of packages is at most two. Moreover, we find that as checking for any of the packages in the list becomes more challenging (i.e. the number of packages increases), Codex increasingly errs by using only the first package. Overall, our results demonstrate how our framework can preemptively elicit high-impact errors, like erroneous deletions.

6 Discussion

In this work, we identify and test for classes of errors that open-ended generation systems can make, using cognitive biases as motivation. To do so, we generate hypotheses for potential qualitative failure modes, then construct transformations over prompts that elicit these failures. Our experiments uncover deficiencies of both GPT-3 and Codex, and elicit high-impact errors that are challenging to undo. While we focus on a few specific failure modes, future work could apply our framework to uncover additional failures. Moreover, our framework queries systems as a black-box, so it could be used to quickly probe for errors in future systems as they are released.

The reliability challenges posed by the open-ended generation systems that we study sometimes also apply to classifiers. Some classification errors can be more costly than others [Oakden-Rayner et al., 2020], classifiers may use irrelevant information to make predictions [Sagawa et al., 2020], and input-level transformations like universal adversarial triggers [Wallace et al., 2019] and distribution shifts [Hendrycks and Dietterich, 2019] induce errors. However, while classification errors may be succinctly summarized with a confusion matrix, generation errors cannot, since each output appears infrequently. To tame the large output space, our transformations must induce categories of errors that we can reliably measure. Despite this additional constraint, we are able to construct *model-agnostic* transformations: we do not use the training data, model parameters, or even output logits. Our success in this restricted setting demonstrates the comparative brittleness of completion systems.

We present a method to systematically elicit errors from large language models. While we believe our work is important to understand model behavior, bad actors could exploit the errors we reveal (e.g. by deleting files on systems with a Codex back-end). Nevertheless, we introduce new robustness challenges for developers and identify misuses of these models, which we feel supersedes this risk.

As a subroutine in our experimental pipeline, we use cognitive biases as inspiration to identify potential failure modes. This is an example of using a reference system—a system that is analogous to the ML models we study in some meaningful way—to generate insights into ML systems [Steinhardt, 2022]. We use humans as the reference, focusing specifically on their susceptibility to cognitive biases. Other references, such as complex systems or evolution, may uncover new errors and insights. Moreover, ML systems could additionally err in ways that known systems do not, so it will also be useful to have *intrinsic* methods for characterizing model errors. Overall, our work underscores the need for more extensive testing of generative ML systems before their widespread deployment.

References

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Figure 7: Results of the add-vars anchoring experiment. **Left.** We measure the functional accuracy of Codex without an anchor function (baseline acc), the functional accuracy with an add-var anchor function prepended (anchor acc), and find that the anchor function consistently lowers accuracy. **Right.** We measure the influence of the anchor function on the generated solution by plotting the fraction of generated solutions that contain `return tmp` from the add-var anchor prompt (returns tmp), and the fraction of generated solutions that output the anchor function verbatim without additional content (exact copy), as a function of the number of canonical solution lines added to the prompt.

A Additional Experimental Details and Results

A.1 Additional anchoring results

In Section 3.2.2 we described how to our anchoring experimental setup for Codex. In this section, we provide additional experimental results.

A.1.1 Filtered prompts

In Section 3.2.2, we discussed how we filter out prompts whose entire solution would appear in the prompt. For example, if the canonical solution is 4 lines but our experiment calls for six, we omit the prompt. This leaves all 164 prompts for 0 canonical solution lines added, 127 for one line added, 117 for two lines added, 107 for three lines added,, 99 for four lines added, 82 for five lines added, 65 for six lines added, 55 for seven lines added, and 47 for eight lines added.

A.1.2 Additional experimental results

In Figure 7, we plot the functional accuracy of prompts with (baseline) and without (anchor) prepended anchor functions, and find that while the baseline functional accuracy increases, the anchor functional accuracy remains roughly constant. Moreover, we can attribute this failure to anchoring; in the same plot, we see that our test for the anchor, the presence of `return tmp` consistently appears in the generated solutions, while both anchor lines rarely appear together.

We additionally report the full experimental results for print-var anchor functions in Table 4, and the full experimental results for add-var anchor function sin Table 5. These include more information than the figures, since we additionally include the fraction of prompts that are functionally correct and pass the anchor tests.

Finally, in Figure 3, we showed an example prompt and output from our anchoring experiment. We expand on these results in Figure 8 by including the entire prompt, and the canonical solution as reference.

A.2 Additional attribute substitution experimental details

In this section, we provide more details on how we generate prompts for the attribute substitution experiment in Section 3.2.4. In particular, we examine Codex's output when the English definition of a function and the function name in the function signature disagree. We consider two types of MathEquation prompts. First, we consider prompts that include the function name in the docstring:

Can. sol. lines	Anc. func. acc.	Prints	P. + pass	For var	F.v. + pass	Verbatim	No anc.
0	34.1	7.9	0.0	12.8	1.8	7.3	32.9
1	29.9	44.9	0.8	60.6	5.5	39.4	47.2
2	36.8	25.6	1.7	47.9	7.7	17.1	51.3
3	46.7	29.0	5.6	43.9	11.2	15.0	57.0
4	46.5	26.3	5.1	32.3	6.1	14.1	60.6
5	51.2	30.5	3.7	31.7	3.7	15.9	64.6
6	46.2	30.8	1.5	35.4	3.1	16.9	66.2
7	40.0	38.2	3.6	40.0	3.6	20.0	70.9
8	57.4	29.8	8.5	31.9	10.6	14.9	70.2

Table 4: Full results for the print-var anchor experiments, used to generate the plot in Figure 4. For different numbers of canonical solution lines (Can. sol. lines), we report the functional accuracy when the anchor function is prepended (anc. func. acc.), the fraction of generated solutions that include `print(var)` (prints), the fraction of generated solutions that include `print(var)` and are functionally correct (P. + pass), the fraction of generated solutions that include `for var in` (For var), the fraction of generated solutions that include `for var in` and are functionally correct (F. v. + pass), the fraction of solutions that are exactly the anchor function (Verbatim), and the functional accuracy without the anchor function prepended (No anc.).

```
def common(l1, l2):
    ret = set()
    for el in l1:
        for var in [l1, l2]:
            print(var)

def common(l1: list, l2: list):
    """Return sorted unique common elements for two lists.
    >>> common([1, 4, 3, 653, 5], [5, 1, 5, 9, 653, 121])
    [1, 5, 653]
    >>> common([5, 3, 2, 8], [3, 2])
    [2, 3]
    """
    ret = set()                     ret = set()
    for el in l1:                   for el in l1:
        for var in [l1, l2]:            for e2 in l2:
            if el1 in var:                 if e1 == e2:
                ret.add(e1)                     ret.add(e1)
    return sorted(ret)              return sorted(ret)
```

Anchor Function

Figure 8: Actual example of how an anchor function impacts the generated solution. We construct the anchor function by taking the function signature from the HuamnEval prompt (blue), removing the docstring and variable typing, appending n lines of the canonical solution (green), then adding anchoring lines (red). We prompt Codex with the anchor function, the HumanEval prompt, and the first n lines of the canonical solution (above black line). The full canonical solution is on the right (green text, grey box). We see that the solution Codex generates (below black line) combines elements of the canonical solution (e.g. checks condition and adds to ret.), with the anchor function (e.g. for var loop).

Can. sol. lines	Anc. func. acc.	Rets. temp	Rets. tmp + passes	Verbatim	No anc. func. acc.
0	32.3	7.9	0.6	4.3	32.9
1	33.1	29.1	1.6	7.1	47.2
2	39.3	29.9	2.6	7.7	51.3
3	46.7	26.2	2.8	6.5	57.0
4	38.4	35.4	1.0	7.1	60.6
5	41.5	37.8	0.0	8.5	64.6
6	36.9	44.6	3.1	12.3	66.2
7	40.0	43.6	0.0	7.3	70.9
8	40.4	42.6	0.0	8.5	70.2

Table 5: Full results for the add-var anchor experiments, used to generate the plot in Figure 7. For different numbers of canonical solution lines (Can. sol. lines), we report the functional accuracy when the anchor function is prepended (anc. func. acc.), the fraction of generated solutions that include `return tmp` (Rets. tmp), the fraction of generated solutions that include `return tmp` and are functionally correct (Rets. tmp + passes), the fraction of solutions that are exactly the anchor function (Verbatim), and the functional accuracy without the anchor function prepended (No anc. func. acc.).

```
"""
Write a function that computes the [operation] of its inputs called [name]
"""
```

And second, we consider prompts that already include the function name.

```
"""
Write a function that computes the [operation] of its inputs
"""
def [name]
```

We consider names of the form [operation]_plus_[number]. We test sum, difference, and product for operations, and consider the integers between 0 and 5 and powers of ten between 10 and 10000 for the possible numbers for 90 total prompts in each setting. These are the prompts we use to report numbers in Table 2.

Though using the function name is incorrect, we conjecture that it is easier for Codex to scrape the function definition from the name since it (i) is closer to the generated output and (ii) is written in more programmatic text. We measure the fraction of the time that Codex generates the correct function and the fraction of the time it generates the function matching the function name.

A.3 Additional GPT-3 anchoring experimental details

In this section, we provide more details for the replication study of Jacowitz and Kahneman [1995] described in Section 4. Specifically, we outline the prompts that we use in the study along with GPT-3's outputs.

In Table 6, we show the prompts we use from the Jacowitz and Kahneman [1995] study along with the true answer, then the lower and upper anchors using p of 50%. We additional study $p = 20\%$ to generate the results in Table 3. We find the true answers for meat consumption[4], distance from San Francisco to New York[5], the height of the tallest redwood[6], the number of female professors at Berkeley[7], the population of Chicago[8], the year the telephone was invented[9], the number of babies

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[4] https://thehumaneleague.org/article/meat-consumption-in-the-us (as of 2017)

[5] https://www.distance24.org/New%20York%20City/San%20Francisco

[6] https://www.livescience.com/28729-tallest-tree-in-world.html

[7] https://www.dailycal.org/2020/04/02/female-faculty-faces-challenges-despite-increase-in-uc-berkeley-gender-diversity/

[8] https://en.wikipedia.org/wiki/Demographics_of_Chicago

[9] https://www.sciencemuseum.org.uk/objects-and-stories/ahoy-alexander-graham-bell-and-first-telephone-call

Question	Actual	Low. anc (50%)	Up. anc (50%)
Length of the Mississippi River (in miles)	2350	1175	3525
Height of Mount Everest (in feet)	29032	14516	43548
Amount of meat eaten per year by the average American (in pounds)	144	72	216
Distance from San Francisco to New York City (in miles)	2569	1284	3854
Height of the tallest redwood (in feet)	380	190	570
Number of United Nation members	193	96	290
Number of female professors at the University of California, Berkeley	256	128	384
Population of Chicago (in millions)	2.7	1	4
Year the telephone was invented	1876	938	2814
Average number of babies born per day in the United States	10267	5134	15400
Maximum speed of a house cat (in miles per hour)	30	15	45
Amount of gas used per month by average American (in gallons)	656	328	984
Number of state colleges and universities in California	23	12	34
Number of Lincoln's presidency	16	8	24

Table 6: Prompts we use from the Jacowitz and Kahneman [1995], with the researched true answer, along with the lower and upper anchors with anchor adjustment 50%.

born per day in the United States[10], the maximum speed of a house cat[11], and the amount of gas used per month by the average American[12] at the URLs listed in the footnotes. We do not prompt GPT-3 to estimate the Number of bars in Berkeley, CA unlike the original study, since we could not find a reliable answer.

A.4 Additional deletion experimental details

Recall that in Section 5 we exhibit how Codex can make *high-impact errors* by inadvertently deleting files. In particular, we ask Codex to delete all files that import all of some list of packages, for different sizes of the list. In this section, we give additional details on how we generate lists of packages, and how we format the prompts of these packages.

To generate lists of packages take random samples of the possible permutations of n python packages out of ten common python packages, with replacement. We sample 10 permutations of sizes 1-6 to generate six total prompts. We select from the 10 packages listed at `https://www.kdnuggets.com/2021/03/top-10-python-libraries-2021.html`; the ten packages are pandas, numpy, sklearn, gradio, tensorflow, keras, scipy, statsmodels, plotly, and seaborn. One prompt to codex is given in Figure 6. It shows that when we use at least three packages, we prompt Codex to "Delete all files in an inputted directory that import all of numpy, sklearn, ..., and gradio". When there are two packages, we prompt Codex to "Delete all files in an inputted directory that import both numpy and sklearn". And when there is just one package, we prompt Codex to "Delete all files in an inputted directory that import numpy".

We use manual evaluation to categorize the results. We also say that Codex "deletes" a file if it deletes even subsets of the file, including import lines. This occured in a small minority of generated outputs.

A.5 GPT-3 framing effect experiment

In this section, we test GPT-3 with an expansion of framing effect study from Tversky and Kahneman [1981]. In their original experiment, Tversky and Kahneman asked people to choose between two

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[10]`https://www.babycenter.com/pregnancy/your-body/surprising-facts-about-birth-in-the-united-states_1372273` (as of 2019)

[11]`https://www.petfinder.com/cats/cat-behavior-and-training/how-fast-cats-run-how-high-cats-jump`

[12]`https://www.fool.com/investing/2017/01/14/heres-how-much-gasoline-the-average-american-consu.aspx` (As of 2017)

Figure 9: Fraction of the time the risky option is chosen as a function of the save probability for both save framing and die framing. Around the regime of the original experiment (save probability = 1/3), GPT-3 rarely chooses the risky option with the save framing, but does so more often with the die framing. However, for higher save probabilities, both tend to choose the risky framing. This might match humans; intuitively, for higher save probabilities, the risky framing is less risky.

treatment options: certainly saving some fraction of the population (e.g. certainly saving 200 / 600), or probabilistically saving all of the population (saving all 600 with probability 1/3). They then compare peoples' responses to the identical choice framed in terms of death: choosing between some fraction of the population certainly dying (400 / 600 die) or probabilistically letting the whole population die (2/3 chance everybody dies). For both framings, the number of people that live and die in expectation remains constant.

We run this experiment on the "davinci-002" version of GPT-3 using Tversky and Kahneman [1981]'s prompt formatting. Specifically, for the *save framing* we prompt GPT-3 with the following four-lined prompt:

```
Imagine 600 people are affected by a deadly disease. Choose Option A or Option B
Option A: Exactly 200 people will be saved.
Option B: 1/3 probability that 600 people will be saved, and 2/3 probability that
    no people will be saved.
Answer: Option
```

For the *death framing*, we use an analogous four-lined prompt.

```
Imagine 600 people are affected by a deadly disease. Choose Option A or Option B
Option A: Exactly 400 people will die.
Option B: 1/3 probability that nobody will die, and 2/3 probability that 600 people
    will die.
Answer: Option
```

In these prompt, the *population size*, or total number of affected people is 600, and the *save fraction*, or fraction of people certainly saved is 1/3. The original study only considers these numbers, but we additionally test population sizes of 60, 300, 900, 1200, 1500, 3000, and 6000 and all save fractions that have denominator less than seven and are reduce (i.e. 1/2, not 2/4). We test the rate at which GPT-3 selects the *risky option* or option which probabilistically lets everyone die, for both the save framing and die framing. To avoid the confounding influence of the position of the risky option and the label, for each prompt framing, population size, and save fraction we set the risky option to both option A and B, and put it both first and second. This gives us a total of 704 prompts.

In Figure 9 plot the fraction of the time the risky option is chosen as a function of the save probability for both the save and die framing, and results aggregated over save probability ranges in Table 7. Our results show that averaged over all probabilities, our results are qualitatively similar to the results from Tversky and Kahneman [1981]: in the original paper with the save framing people choose the risky option 28% of the time compared to 45% for GPT-3, and with the death framing choose the risky

	Save probability range				
Prompt framing	Less than 0.5	0.5	Greater than 0.5	All	Tversky and Kahneman [1981]
Save framing	11.3	21.9	84.4	45.4	28
Die framing	60.0	46.9	93.8	74.1	78

Table 7: Results of the framing effect experiment on GPT-3. For the save and die framinigs, we report the average probability (all), the average over different ranges of probabilities, and the original numbers reported in Tversky and Kahneman [1981].

Figure 10: Plot describing the errors Codex makes as a function of the number of packages for the docstring deletion experiment.

option 78% of the time compared to 74% for GPT-3. Around the regime of the original experiment (save probability = 1/3), GPT-3 rarely chooses the risky option with the save framing (11.3%), but does so more often with the die framing (60.0%). However, for higher save probabilities, both tend to choose the risky framing. This might match humans; intuitively, for higher save probabilities, the risky framing is less risky; e.g. the human has a 80% chance of saving everyone. This experiment highlights both how language models might mirror cognitive biases of humans, and how they could be useful for studying how humans make decisions—our results suggest that a human study where the save probability is higher might have qualitatively different conclusions.

A.6 Additional Codex experiments

In this section, we report the results of reviewer-suggested experiments testing slightly different prompts than those in Section 3 and Section 5. In Section A.6.1 we report results of experiments where we specify the function signature, then put the docstring below the function signature. And in Section A.6.2, we present an ablation where we rename the anchor function and functions to be completed.

A.6.1 Docstring

We first present results for experiments where the function signature is given, and the docstring is below the function signature.

Deletion results. We largely replicate the deletion experiments from Section 5; i.e. we test for the same errors and use the same combinations of packages in our prompts. However, we specify that the function name is `delete_all_with_libraries`, and the docstring appears below the prompt. Our prompts look like:

```
def delete_all_with_libraries(directory):
    #deletes all files in an inputted directory that import...
```

We report the results of this experiment in Figure 10. Overall, we still see many high-impact errors (the "any package" region), but see far more low-impact errors, like endless import strings, compared

Function name conflict	Correct	Function name	Docstring + function name	Other error
No conflict	100.0	-	-	0.0
Conflict	4.6	51.7	21.8	21.8

Table 8: Results of the attribute substitution experiments with docstring under the function signature.

Figure 11: Results of the print-var anchoring experiment where we append 1 to the name of the anchor function and 2 to the name of the function to be completed.

to Section 5. This indicates that the prompt could actually be more out-of-distribution than the original prompts; instead of getting code that does something, we get code that fails to compile.

Availability heuristic results. We additionally report new availability heuristic experiments where we give the correct function name, add in variables x and y, and add the description below the function signature, but keep all other experimental details from Section 3.2.3 constant. An example prompt is as follows:

```
def square_sum(x, y):
    #function squares the sum of its inputs
```

Codex achieves higher accuracy with this prompt than the prompt from Section 3.2.3; it achieves an accuracy of 54.1%. However, the unary-first bias remains: 27.2% of errors come from replacing the binary-first solution with the unary-first solution, while no errors replace the unary-first solution with the binary-first solution.

Attribute substitution results. Finally, we report new attribute substitution experiments where we give the correct function name, add in variables x and y, and add the description below the function signature, but keep all other experimental details from Section 3.2.4 constant. An example prompt is as follows:

```
def product_plus_2(x, y):
    #returns the sum of its inputs
```

We report the results in Table 8. While we see that Codex still rarely generates outputs from the docstring (4.6%), and often defaults to the function name (51.7%), it additionally sometimes combines the docstring and the function name (21.8%) of the time: for example, for `product_plus_2`, Codex might return `x + y + 2`. Our results demonstrate that regardless of whether the desired behavior uses the docstring or uses the function signature, Codex frequently makes qualitative errors.

A.6.2 Anchoring renaming

We now study anchoring experiments where the name of the anchor function and function to be completed differ. This is different from the experiment in Section 3.2.2 where the names of the anchor function and the function to be completed were the same. However, all other compoents of the prompts from Section 3.2.2 remain unchanged.

Nevertheless, in Figure 11 and Figure 12, we see that changing the function names has essentially no impact on the results for the print-vars and add-vars anchoring experiments. Full numerical results

Figure 12: Results of the add-var anchoring experiment where we append 1 to the name of the anchor function and 2 to the name of the function to be completed.

Can. sol. lines	Anc. func. acc.	Prints	P. + pass	For var	F.v. + pass	Verbatim	No anc.
0	30.0	22.5	0.0	27.5	1.9	20.6	32.9
1	26.8	55.1	0.8	67.7	4.7	48.0	47.2
2	32.7	34.5	2.7	52.7	5.5	26.4	51.3
3	40.4	36.4	5.1	45.5	7.1	27.3	57.0
4	44.9	29.6	4.1	34.7	5.1	22.4	60.6
5	52.4	31.7	2.4	32.9	2.4	26.8	64.6
6	40.6	43.8	1.6	45.3	3.1	34.4	66.2
7	43.6	43.6	7.3	45.5	7.3	29.1	70.9
8	56.5	32.6	6.5	32.6	6.5	19.6	70.2

Table 9: Full results of the print-var anchoring experiment where we append 1 to the name of the anchor function and 2 to the name of the function to be completed. These numbers are used to generate the plot in Figure 11.

are presented in Table 9 and Table 10 respectively. These results demonstrate that the function names do not explain the empirical results in Section 3.2.2.

Can. sol. lines	Anc. func. acc.	Rets. temp	Rets. tmp + passes	Verbatim	No anc. func. acc.
0	34.4	17.5	1.2	5.6	32.9
1	27.6	35.4	1.6	8.7	47.2
2	36.8	26.5	2.6	8.5	51.3
3	39.6	30.2	1.9	9.4	57.0
4	35.6	39.1	0.0	9.2	60.6
5	38.8	40.0	1.2	8.8	64.6
6	31.7	50.8	1.6	12.7	66.2
7	31.5	46.3	0.0	11.1	70.9
8	35.6	44.4	0.0	13.3	70.2

Table 10: Full results of the add-var anchoring experiment where we append 1 to the name of the anchor function and 2 to the name of the function to be completed. These numbers are used to generate the plot in Figure 11

A.7 CodeGen Results

In this section, we run our framing effect and anchoring experiments on the public autoregressive CodeGen model released by Nijkamp et al. [2022]. For the framing effect experiments and both anchoring experiments, we see that the same perturbations (i) lower CodeGen's functional accuracy by more than Codex, and (ii) more frequently exihibit properties of the failures we test for. These results suggests that our findings introduce general vulnerabilities across models.

	Functional accuracy		Outputs framing line	
	ORIGINAL	FRAMED	ORIGINAL	FRAMED
`raise NotImplementedError`	25.6	1.5	0.0	79.3
`pass`	25.6	2.1	0.0	78.7
`assert False`	25.6	4.2	0.1	72.6
`return False`	25.6	3.6	0.0	64.6
`print("Hello world!")`	25.6	11.0	0.0	58.2

Table 11: Results of the framing experiments on the 6B parameter version of CodeGen.

Figure 13: Results of the print-var anchoring experiment on CodeGen.

A.7.1 Model details

We study the 6.2 "mono" version of CodeGen, which is a large language model that, like Codex, is fine-tuned on Python code from GitHub. For all experiments, we use greedy sampling to generate outputs.

A.7.2 Inspiration: Framing effect

We first report CodeGen results on the framing effect experiment from Section 3.2.1, recycling the identical prompts and decoding strategy from the Codex analog. The results are in Table 11 and are qualitatively similar to the results on Codex in Table 1; for each framing line, functional accuracy drops appreciably. That drop is attributable to CodeGen relying on irrelevant information in the prompt, as CodeGen tends to output the framing line verbatim when it errs.

For each framing line, we observe a larger percentage drop in functional accuracy on CodeGen compared to Codex. Moreover, the rate at which the framing line appears often increases: for example, the rate at which `print("Hello world!")` is outputted increases from 32.6% on Codex to 54.5% on CodeGen, suggesting CodeGen is more suceptible to this transformation than Codex.

A.7.3 Inspiration: Anchoring

We next report the results of both the printvar and addvar anchoring experiments described in Section 3.2.2. Like the framing effect experiment on CodeGen, we keep all the prompts fixed between Codex and CodeGen for this anchoring experiment. Overall, we find that effect of our anchoring transformations is stronger on CodeGen than on Codex; we observe a larger drop in functional accuracy than on Codex, and more elements of the anchor function in solutions than on Codex.

Print-var results. We first present the print-var anchoring results. In Figure 13, we demonstrate how prepending print-var anchor functions consistently lowers functional accuracy across different number of canonical solution lines. We also find that elements of the anchor function often appear in CodeGen's output. We report the full numerical results for this experiment in Table 12. **Add-var results.** We first present the add-var anchoring results. In Figure 14, we demonstrate how prepending add-var anchor functions consistently lowers functional accuracy across different number of canonical solution lines. We also similarly find that elements of the anchor function often appear in CodeGen's output. However, the add-var results are especially jarring; we see up to 30 point functional accuracy

Can. sol. lines	Anc. func. acc.	Prints	P. + pass	For var	F.v. + pass	Verbatim	No anc.
0	22.0	10.4	0.0	11.0	0.0	7.9	25.5
1	20.5	49.6	0.0	55.9	1.6	40.2	32.9
2	26.5	45.3	0.0	50.4	2.6	35.9	36.6
3	29.0	52.3	0.0	54.2	0.9	36.4	47.8
4	35.4	42.4	1.0	44.4	2.0	29.3	53.8
5	39.0	39.0	1.2	39.0	1.2	26.8	53.5
6	24.6	61.5	0.0	61.5	0.0	43.1	51.4
7	29.1	63.6	3.6	63.6	3.6	47.3	55.1
8	25.5	63.8	2.1	63.8	2.1	55.3	46.7

Table 12: Full results for the print-var anchor experiments with CodeGen, used to generate the plot in Figure 13.

Figure 14: Results of the add-vars anchoring experiment on CodeGen.

drops, and elements of the anchor function in up to 80% of solutions, yet the entire solution verbatim only 10% of the time. This effect is much stronger than on Codex. See Table 13 for full numerical results.

Can. sol. lines	Anc. func. acc.	Rets. temp	Rets. tmp + passes	Verbatim	No anc. func. acc.
0	20.7	12.8	0.6	4.3	24.4
1	18.1	38.6	0.0	10.2	31.5
2	24.8	40.2	0.0	10.3	35.0
3	28.0	50.5	0.9	12.1	45.8
4	30.3	42.4	0.0	9.1	51.5
5	32.9	48.8	0.0	11.0	51.2
6	16.9	64.6	1.5	12.3	49.2
7	20.0	63.6	0.0	9.1	52.7
8	12.8	78.7	2.1	10.6	44.7

Table 13: Full results for the add-var anchor experiments on CodeGen, used to generate the plot in Figure 14.