Active Example Selection for In-Context Learning

Yiming Zhang and **Shi Feng** and **Chenhao Tan**
{yimingz0, shif, chenhao}@uchicago.edu
University of Chicago

Abstract

With a handful of demonstration examples, large-scale language models show strong capability to perform various tasks by *in-context* learning from these examples, without any fine-tuning. We demonstrate that in-context learning performance can be highly unstable across samples of examples, indicating the idiosyncrasies of how language models acquire information. We formulate example selection for in-context learning as a sequential decision problem, and propose a reinforcement learning algorithm for identifying generalizable policies to select demonstration examples. For GPT-2, our learned policies demonstrate strong abilities of generalizing to unseen tasks in training, with a 5.8% improvement on average. Examples selected from our learned policies can even achieve a small improvement on GPT-3 Ada. However, the improvement diminishes on larger GPT-3 models, suggesting emerging capabilities of large language models.

1 Introduction

Large language models demonstrate the capability to learn from just a few examples (Radford et al., 2019; Brown et al., 2020; Rae et al., 2022; Zhang et al., 2022). The possibility to train a model without any parameter update has inspired excitement about the in-context learning paradigm.

Intuitively, high in-context learning performance should require carefully chosen demonstration examples, but a recent line of work suggests otherwise — that demonstration examples are not as important as we expected, and that few-shot performance can be largely attributed to the model's zero-shot learning capacity (Min et al., 2022), across GPT-2 and GPT-3. This insight is corroborated by a parallel line of work that brings significant improvements to in-context learning performance without example selection, for example, by reordering randomly selected examples and using calibration (Lu et al., 2022; Zhao et al., 2021; Kojima et al., 2022). Another notable approach is to use best-of-n sampling, which requires a labeled set for validation (Nakano et al., 2022).

Our contribution in this paper is twofold. First, we revisit the effect of example selection on in-context learning. We show that even with reordering and calibration, we still observe a large variance across sets of demonstration examples, especially for GPT-2, while calibration reduces the variance for GPT-3 models. The high variance needs further investigation, as we take it as evidence that large language models are still not capable of efficiently and reliably acquire new information in-context. Understanding what makes good demonstration examples sheds some light on the mechanisms that large language models use to process information.

Second, we seek to discover general trends in example selection for in-context learning across different tasks. Concretely, we use reinforcement learning to optimize example selection as sequential decision making problem. We argue that active example selection from unlabeled datasets is the most appropriate setting for in-context learning because fine-tuning with an existing labeled set leads to great performance with low variance. For GPT-2, we validate our learned policy on a seen task with labeled dataset and observe a 12.1% improvement over a max-entropy active learning baseline. Moreover, our learned policy is able to generalize to new tasks with 5.8% improvement, suggesting that the policy is able to capture systematic biases in how GPT-2 acquires information. Examples selected from our learned policies can even achieve a small improvement on GPT-3 Ada. However, the improvement diminishes on larger GPT-3 models. We provide further analyses to understand the properties of useful examples.

Overall, our work explores how large language models process information through the perspective of example selection and formulate active ex-

Proceedings of the 2022 Conference on Empirical Methods in Natural Language Processing, pages 9134 - 9148

ample selection as a sequential decision making problem. We investigate divergent behaviors between GPT-2 and GPT-3, which echoes the emerging abilities of large language models, and suggest that researchers in the NLP community should collectively build knowledge and research practice in the era of large language models.[1]

2 The Effect of Example Selection

In this section, we demonstrate the instability of in-context learning performance due to the selection of demonstration examples. We further show that existing methods (e.g., calibration, reordering) are insufficient for addressing this stability for GPT-2. In comparison, the variance of GPT-3 models can be mitigated with calibration.

2.1 In-context Text Classification with Demonstration Examples

We start by formally defining in-context learning. We focus on in-context learning for text classification with a left-to-right language model. All supervision is given through a "prompt" which we denote as s. The prompt typically contains natural language instructions and a few demonstration examples. To make a prediction for a test example x, we concatenate the prompt and the test example as prefix, and use the language model to predict the next token: $\arg\max_y \mathbf{P}_{\text{LM}}(y|s+x)$, where $+$ denotes concatenation. Typically, instead of taking the $\arg\max$ from the whole vocabulary, we restrict the model's output to a set of special tokens which corresponds to the set of labels, e.g., with the word "positive" corresponding to the positive class in binary sentiment classification. In our formulation, we omit a separate variable for the special tokens, and use $\mathcal{Y}$ to refer to both the label set and the set of proxy tokens for simplicity.

To summarize, a prompt in this paper is a sequence of k **labeled** examples concatenated together: $s = (x_1, y_1), (x_2, y_2), \ldots, (x_k, y_k)$. And the prediction for a test input x is the label with the highest likelihood of being by the language model: $\arg\max_{y \in \mathcal{Y}} \mathbf{P}_{\text{LM}}(y|s+x)$.[2]

Experiment setup. Following Zhao et al. (2021), we conduct our experiments on AGNews (Zhang et al., 2015), SST-2 (Socher et al., 2013) and TREC (Voorhees and Tice, 2000). We additionally include Amazon (Zhang et al., 2015) since it contains longer texts than the remaining datasets. Table 1 give basic information of the tasks.

Dataset	Domain	#classes	avg. length
AGNews	Topic cls.	4	37.8
Amazon	Sentiment cls.	2	78.5
SST-2	Sentiment cls.	2	19.3
TREC	Question type cls.	6	10.2

Table 1: Dataset information.

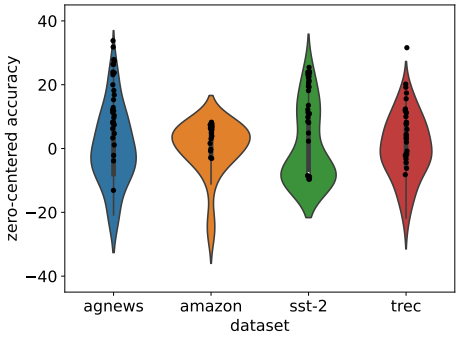


Figure 1: Zero-centered in-context learning accuracy of GPT-2 on 30 random sets of 4 demonstration examples. Each dot indicates performance of the best permutation for one set of demonstration examples. y-axis represents the accuracy difference with the mean accuracy of random demonstration examples.

Using GPT-2 345M (GPT-2), GPT-3 Ada (ADA) and GPT-3 Babbage (BABBAGE) as the in-context learning models, we report 4-shot example selection performance across all experiments.

2.2 Sensitivity to Example Selection

We first highlight the sensitivity of GPT-2 due to example selection. In Figure 1, we plot the in-context learning performance of 30 random sequences of demonstration examples with length 4. Across all 4 tasks, the maximum and minimum performance due to random sampling differs by $> 30\%$. Additionally, for 3 out of the 4 tasks (AGNews, SST-2 and TREC), performance of the worst set of demonstration examples lead to in-context learning performance below random guessing (e.g., it is 10.0% on TREC, below 16.7% accuracy of guessing randomly among 6 labels in TREC).

Reordering sequence alone cannot address the instability. Lu et al. (2022) identifies the ordering of demonstration examples as the cause for variance, and proposed heuristics to reorder demonstra-

[1] Our code is available at https://github.com/ChicagoHAI/active-example-selection.

[2] If a label is represented as multiple tokens in the LM, e.g., negation=neg+ation, we simply use the first unambiguous token, e.g., neg for negation and ent for entailment.

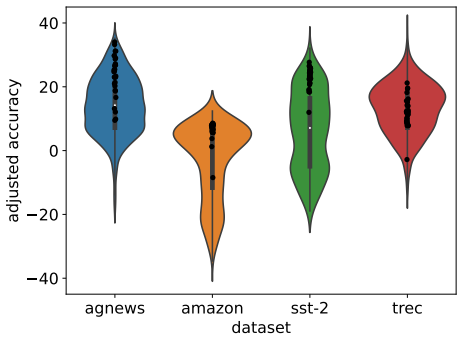


Figure 2: In-context learning accuracy of 30 random sets of 4 demonstration examples **with calibration**. Each dot indicates performance of the best permutation for one set of demonstration examples. Accuracy over random examples (**no calibration**) is plotted.

tion examples. For such an approach to be effective, the underlying assumption is that there exists good orderings for most sets of demonstration examples.

In Figure 1, we additionally report the highest possible performance among $4! = 24$ permutations for each of the 30 sets using a validation set of 100 examples. The reordering performance reported here is highly optimistic for a true few-shot setting (Perez et al., 2021) since a validation set cannot be assumed available. As expected, taking the best permutation on a validation set improves test performance: we observe an average of 8.1% increase on average over random demonstration examples.

However, these best orderings of examples still lead to a wide range of possible performance. On AGNews, we observe a maximum accuracy of 79.6% and a minimum accuracy of 32.7% after considering the best possible orderings. On TREC, the best ordering for 9 out of 30 sets of examples lead to performance below random examples. These observations suggest that there are simply no good orderings for considerable proportions of demonstration sets, motivating the need for selecting examples beyond merely reordering.

Calibration does not decrease variance for GPT-2, either. Zhao et al. (2021) finds that language models are poorly calibrated when used directly as in-context classifiers, and argues that calibration is the key missing piece to improve and stablize in-context learning performance. It proposes using dummy examples (e.g., "N/A") as anchors for calibrating the language model since a calibrated language model should make neutral predictions for these content-free examples.

Figure 2 demonstrates the effectiveness of calibration in improving few-shot performance. With calibration, we observe an increase in average performance of varying magnitude on 3 out of the 4 tasks (AGNews, SST-2 and TREC), but a marginal decrease of performance on Amazon. For example, on AGNews where calibration improves performance the most, we observe a maximum accuracy of 79.5% and a minimum accuracy of 26.1%, resulting in a gap of over 53.4%.

Model	AGNews	Amazon	SST-2	TREC
GPT-2	$44.5_{9.3}$	$87.5_{3.7}$	$61.7_{14.4}$	$29.4_{12.8}$
GPT-2 (**C**)	$55.2_{12.0}$	$76.3_{14.0}$	$66.2_{14.7}$	$40.8_{5.4}$
ADA	$62.9_{17.5}$	$87.0_{6.1}$	$65.0_{10.2}$	$21.2_{6.6}$
ADA (**C**)	$64.0_{4.0}$	$90.0_{1.2}$	$73.8_{9.7}$	$22.1_{5.3}$
BABBAGE	$68.0_{14.0}$	$93.4_{0.8}$	$92.2_{2.7}$	$27.4_{5.8}$
BABBAGE (**C**)	$78.1_{6.1}$	$92.7_{1.6}$	$90.8_{1.1}$	$36.0_{4.0}$

Table 2: Performance of GPT-2, ADA and BABBAGE across 5 random sets of 4-shot demonstration examples. **C** indicates calibration. Standard deviation is reported as subscripts.

Interestingly, we observe varying behavior when combining calibration with demonstration reordering. On the binary tasks (Amazon and SST-2), we observe prompt reordering to be quite effective, consistently leading to performance above random examples. On the other hand, for AGNews (4 labels) and TREC (6 labels), we observe much greater variance.

In summary, with GPT-2, existing methods do not provide satisfactory solutions to the sensitivity of in-context learning to demonstration examples. Reordering demonstration requires a well-behaving demonstration set, which is often not the case, and does not reduce variance. Calibration, though improves performance, does not reduce variance, and its effectiveness deteriorates with a large label set. These findings motivate the need for identifying high quality demonstration examples for consistent and performant in-context learning.

Variance persists to some degree with GPT-3. In Table 2, we report the performance of GPT-2, ADA and BABBAGE on 5 random sets of demonstration examples.[3] GPT-3 models are not immune to instability due to resampling demonstration examples. On multi-labeled tasks including AGNews and TREC, we observe both ADA and BABBAGE demonstrate significant variance, and on binary

[3]We do not use the same sample size or examine the effect of re-ordering for cost considerations.

tasks such as Amazon and SST-2, much smaller variance is observed. This difference is potentially due to the difficulty of the task and the multi-class nature of AGNews and TREC. We will address the latter in §4.3. Another interesting observation is that variance diminishes with calibration. However, one may argue that calibration no longer reflects the model's innate ability to acquire information.

Overall, the differences in model behavior between GPT-2 and GPT-3 add evidence to the emergent ability of large language models (Wei et al., 2022; Bowman, 2022). We hypothesize that the variance will be even smaller with GPT-3 Davinci.

3 Active Example Selection by RL

Given a set of *unlabeled* examples, can we choose the right ones to be annotated as demonstration examples? In this section, we formulate the problem of active example selection for in-context learning. Following the definition of in-context learning in §2.1, constructing a prompt for in-context learning boils down to choosing a sequence of demonstration examples.

We emphasize that by selecting from *unlabeled* examples, our setup is analogous to active learning, where we select examples to label. We think that this is the most appropriate setting for in-context learning because fine-tuning can lead to great performance with low variance if we already have a moderately-sized labeled set (e.g., 100 instances).

As in-context learning uses a small number of examples, we formulate active example selection as a sequential decision making problem, where prompt is constructed by selecting and annotating one demonstration example at a time. We use a Markov Decision Process (MDP) to formalize the problem, discuss our design of the reward function, and introduce our solution to example selection using reinforcement learning (RL).

3.1 Active Example Selection as a MDP

Given a set of unlabeled examples, we want to maximize the expected accuracy on unseen test examples by getting up to k annotations. The space of possible prompts grows exponentially with the number of unlabeled example and is intractable to enumerate, so we treat it as a sequential decision making problem: given the pool of unlabeled examples $\mathbf{S}_{\mathcal{X}} = \{x_i\}$, choose one example x_i, obtain its groundtruth label y_i, append the pair (x_i, y_i) to our prompt, and repeat this process until either the budget k is exhausted or the policy takes a special action $\perp$ indicating early termination.

Action space and state space. The action space of the MDP is the set of unlabeled examples plus the special end-of-prompt action: $\mathcal{A} = \mathbf{S}_{\mathcal{X}} \cup \{\perp\}$. After choosing an action x_i we observe its label y_i, and the state is defined by the prefix of the prompt $s = (x_1, y_1), (x_2, y_2), \ldots, (x_i, y_i)$.

Reward. The reward r can be defined based on an arbitrary scoring function f of the language model LM when conditioned on the prompt s, denoted $r = f(\text{LM}_s)$. In practice, we use the accuracy on a labeled validation set as reward.

It follows that we need to have access to a validation set during training, which we refer to as *reward set*. Similarly, we also have a labeled set from which our policy learns to select examples. We refer to this labeled set as *training set*. Ideally, our learned policies identify generalizable qualities of demonstration examples and can select useful unlabeled examples in a task where the policy has not observed any labeled examples. We will explore different setups to evaluate our learned policies.

It is useful to emphasize how active example selection deviates from the standard reinforcement learning setting. First, the action space is the examples to be selected, which can be variable in size. Furthermore, the actions during test time can be actions that the policy has never observed during training. Similarly, the classification task can differ from training, analogous to a new environment. Such generalizations are not typically assumed in reinforcement learning, due to the challenging nature of the problem (Kirk et al., 2022).

3.2 Active Example Selection by Q-learning

Framing active example selection as a sequential problem allows us to use off-the-shelf RL algorithms to train a policy. We opt to use Q-learning (Mnih et al., 2013) for its simplicity and effectiveness.

The objective of Q-learning is to approximate the optimal state-value function $Q^\star(s, a)$, i.e., the maximum (discounted) future reward after taking action a in state s. The Bellman equation (Bellman, 1957) allows a recursive formulation of the optimal state-value function $Q^\star$ as

$$Q^\star(s, a) = \mathbb{E}_{s \sim \mathcal{S}} \left[r(s, a) + \gamma \max_{a'} Q^\star(s', a') \right].$$

We collect off-policy training data in our implementation and thus use offline Q-learning to lever-

age off-policy data (Prudencio et al., 2022). Specifically, We use conservative Q-learning (CQL) (Kumar et al., 2020), which uses regularization to prevent the overestimation of Q-values for unobserved actions in training data, contributing to a robust policy when evaluated in an unfamiliar environment. More details about CQL can be found in the Appendix A.

Generation of off-policy data. Offline learning requiers off-policy training data. We run a random policy for a fixed number (2,000) of episodes to create the off-policy data. For every episode, we randomly sample 4 demonstration examples, and compute features and intermediate rewards. Then, we store the trajectory as training data.

Feature-based representation of actions. In our framework, a state s is a sequence of examples, and we simply use the number of already selected examples $|s|$ as the feature representation. To enable our method to be deployed in an active example selection process, we assume no access to labels prior to selecting an example. That is, when representing a example to be selected $a = (x, y)$, we omit the label y and simply use predicted label probabilities conditioned on the current examples $\mathbf{P}_{\text{LM}}(\cdot \mid s + x)$. We additionally include entropy of the prediction.[4]

Reward shaping. The previously defined reward function only rewards a completed prompt, while intermediate states receive zero reward.Sparse reward schemes are known to make learning difficult (Pathak et al., 2017). Therefore, we propose an alternative reward function based on the marginal utility of actions (Von Wieser, 1893). At time step t we define $r : \mathcal{S} \times \mathcal{A} \to \mathbb{R}$ as

$$r(s, a) = f(\text{LM}_{s+a}) - f(\text{LM}_s).$$

Intuitively, r measures the "additional gain" on objective f by acquiring the label of example a. Notice that $f(\text{LM}_\varnothing)$ can be conveniently interpreted as the zero-shot performance of the language model. Maximizing this marginal utility reward function is indeed equivalent to optimizing the true objective f: observe that the summation of rewards along a trajectory is a telescoping series, leaving only the final term $f(\text{LM}_{s_\perp})$ minus a constant term that does not affect the learned policy.[5] It turns out that r is a **shaped reward** (Ng et al., 1999), a family of transformed reward functions that preserves the invariance of optimal policies.

Target network with replay buffer. Our algorithm uses separate policy and target networks (Hasselt, 2010) with a replay buffer (Lin, 1992). Both are standard extensions to vanilla DQN (Arulkumaran et al., 2017), and are demonstrated to improves performance while alleviating certain optimization issues (Hessel et al., 2017). After concatenating state and action representations, we use a 3-layer MLP as the Q-network: $\hat{Q}(s, a) = \text{MLP}([s \parallel a])$. We report hyperparameters details in Appendix B.

4 Results

In this section, we investigate the performance of our learned policies for GPT-2. Due to the significant costs of generating episodes, we only apply the policies learned from GPT-2 and examine direct transfer results on GPT-3. Baselines, oracles and our method have access to the same underpinning calibrated GPT-2 model.

4.1 Setup

Following our framework in §3, during training, we use a **training set** from which the trained policy picks 4 examples for demonstration, as well as a **reward set**, which is a validation set where we compute rewards for the learning agent. Each set has 100 examples and our training scheme uses a total of 200 examples.

Depending on the availability of a reward set, we consider three evaluation settings:

- **SEEN EXAMPLES, SAME TASK.** In this setting, we use the learned policy to pick demonstration examples from the **training set**. We expect our method to be competitive with oracle methods that select examples based on rewards.
- **NEW EXAMPLES, SAME TASK.** We consider a more challenging setting where the learned policy picks from an **unlabeled set** of 100 or 1000 previously unseen examples. The learned policy still benefits from access to the reward set during training as the classification task is the same, but it cannot perform well simply by memorizing good sequences.
- **NEW EXAMPLES, NEW TASK.** Finally, we ask the learned policy to pick examples on a new task that it has never seen. Specifically, we adopt a multi-task learning approach, allowing the policy

[4] Other features can be used, such as embeddings of the language model. We use minimal features so that policies could be evaluated across models (GPT-2 and GPT-3).

[5] Requires the discount factor $\gamma = 1$, which we use in across all experiments.

Method	Average	AGNews	Amazon	SST-2	TREC
random	59.6	$55.2_{10.5}$	$76.3_{12.3}$	$66.2_{12.9}$	$40.8_{4.7}$
max-entropy	59.3	$58.8_{11.3}$	$74.8_{5.1}$	$65.7_{10.7}$	$37.8_{6.7}$
reordering	63.5	$63.3_{6.8}$	$89.8_{3.8}$	$67.9_{11.1}$	$33.0_{4.2}$
best-of-10	72.5	$72.1_{1.9}$	$91.1_{0.6}$	$81.1_{4.4}$	$45.6_{3.5}$
greedy-oracle	78.0	$80.6_{1.7}$	$91.8_{1.1}$	$81.7_{3.9}$	$58.0_{7.5}$
our method (**seen examples**)	71.4	$70.8_{7.8}$	$90.4_{1.9}$	$81.0_{3.5}$	$43.3_{2.0}$
our method (**100 new examples**)	71.6	$71.3_{7.4}$	$89.2_{3.9}$	$81.8_{2.6}$	$44.0_{4.6}$
our method (**1000 new examples**)	69.0	$65.5_{7.4}$	$88.5_{4.2}$	$76.7_{7.5}$	$45.4_{5.0}$

Table 3: **SAME TASK** accuracy on AGNews, Amazon, SST-2 and TREC, across 5 random seeds. 95% confidence intervals are reported as subscripts.

to simultaneously learn from all but one tasks. Then, we evaluate the held-out task (e.g., train on AGNews, SST-2, TREC and test on Amazon). The learned policies use 600 examples from training (3×100 each for the **training set** and **reward set**). During evaluation, the policy picks examples from an **unlabeled set** of examples in the held-out task, and we experiment with either 100 or 1000 unlabeled examples.

SEEN EXAMPLES, SAME TASK and NEW EXAMPLES, SAME TASK serve as sanity check of our learned policies, while NEW EXAMPLES, NEW TASK is the most appropriate setting for evaluating in-context learning.

Baselines and oracles. We consider three baseline methods for example selection. The **random** strategy simply picks demonstration examples randomly. Our second baseline (**max-entropy**) is a standard approach in active learning (Settles, 2009; Dagan and Engelson, 1995) which greedily picks the example maximizing classification entropy. We additionally consider a strong example reordering heuristic by Lu et al. (2022) , dubbed **reordering**;[6] **reordering** first uses the language model to generate a set of fake examples that resemble demonstration, and then chooses an ordering that maximizes classification entropy on these fake examples. Intuitively, **max-entropy** and **reordering** both encourages class balance during prediction. All three baselines can be used in active example selection, namely, example selection that does not have label access to examples before they are selected.

We further consider two oracle methods that require a labeled candidate set and a reward set. The **best-of-10** strategy randomly samples 10 times and keeps the sample that maximizes performance on the reward set as the final demonstration sequence. In addition, we use a greedy strategy to iteratively choose the example that results in the highest performance on the reward set, and we refer to this strategy as **greedy-oracle**. The oracles do not work for active example selection and cannot be used in NEW TASK as the assumption is that we do not have any labeled examples, so we do not compare our learned policies with oracles in NEW TASK.

We use baselines and our methods to select 4 demonstration examples for every task, and we average model performances across 5 random runs.

4.2 Main results

We analyze the effectiveness of applying our method in both SAME TASK and NEW TASK.

SAME TASK. Our method evaluated by picking from **seen examples** demonstrates strong performance. Across all 4 tasks, our method outperforms random, max-entropy and reordering baselines by an average of 11.8%, 12.1% and 7.9%, respectively, as well as $> 10\%$ improvements on 2 tasks.

Beyond performance gains, it is clear that our method helps reduce variance. We present 95% confidence intervals as a proxy for variance. Across all 4 tasks, we observe consistent decrease in variance compared to the baselines.

Picking from both 100 and 1000 **new examples** largely retains the performance gains and variance reductions. Interestingly, we notice a higher overall performance of picking from 100 over 1000 new examples. This can be attributed to the large variance (see Appendix C.1 for more results).

Comparing with oracle methods, our methods perform relatively closely to **best-of-10**, while **greedy-oracle** significantly outperforms the other methods. Since we want the policies to learn generalizable example selection strategies, we intention-

[6] Lu et al. (2022) experiment with two metrics for selecting the best ordering. In the **reordering** baseline, we use the "Global Entropy" metric since it performs better on average in the original paper.

Method	Average	AGNews	Amazon	SST-2	TREC
random	59.6	$55.2_{10.5}$	$76.3_{12.3}$	$66.2_{12.9}$	$40.8_{4.7}$
max-entropy	59.3	$58.8_{11.3}$	$74.8_{5.1}$	$65.7_{10.7}$	$37.8_{6.7}$
reordering	63.5	$63.3_{6.8}$	$89.8_{3.8}$	$67.9_{11.1}$	$33.0_{4.2}$
our method (**100 examples**)	63.8	$63.4_{10.4}$	$86.8_{6.7}$	$65.9_{13.4}$	$38.9_{5.1}$
our method (**1000 examples**)	65.4	$66.7_{5.7}$	$89.9_{1.6}$	$61.9_{7.7}$	$43.3_{4.4}$

Table 4: **New-task** accuracy on AGNews, Amazon, SST-2 and SST-2, across 5 random seeds. 95% confidence intervals are reported as subscripts.

ally use simple features, which may explain why our method, even when picking from seen examples, does not outperform oracles. Thanks to the high variance of random sampling, **best-of-10** is a very performant strategy despite its simplicity, and a reasonable choice if validation is possible. At the cost of an exponential runtime, **greedy-oracle** shows the strong in-context learning performance attainable with just example selection, motivating the framing of in-context learning optimization as a pure example selection problem. In fact, the average performance from **greedy-oracle** with GPT-2 (345M) is better than that of GPT-3 Curie, a 20x larger model (see Appendix C.2).[7]

NEW TASK. We further evaluate our methods under the new task setting, where we train the example selection policy on 3 tasks, and evaluate on a previously unseen task. On average, we observe a smaller, but still significant improvements over both random and max-entropy baselines, suggesting the existence of learnable insights about good demonstration examples that generalize across tasks. On the other hand, we observe limited gains over reordering, signifying the challenge of finding good examples in an unknown task.

Interestingly, when picking from 1000 examples, we observe a much greater effect of variance reduction compared to baselines. In comparison, the variance reduction effect is minimal when picking from 100 examples and the performance gain is slightly smaller likely due to randomness.

We continue this discussion on the effect of size of selection set on transfer performance in Appendix C.1.

GPT-3 transfer. Training example selection policies directly on GPT-3 models is not viable since it requires sample a significant number of trajectories while computing rewards. Therefore, we instead evaluate if policies and examples trained on GPT-2 generalize to GPT-3. Overall, we find mixed transfer results. On the smaller GPT-3 ADA model, we observe small gains ($\sim 1\%$) by transferring both policies and examples, which is impressive consider the architectural differences between GPT-2 and GPT-3. However, we observe mixed results in transfer to BABBAGE and CURIE. We report further details in Appendix C.2.

4.3 What Makes Good Examples?

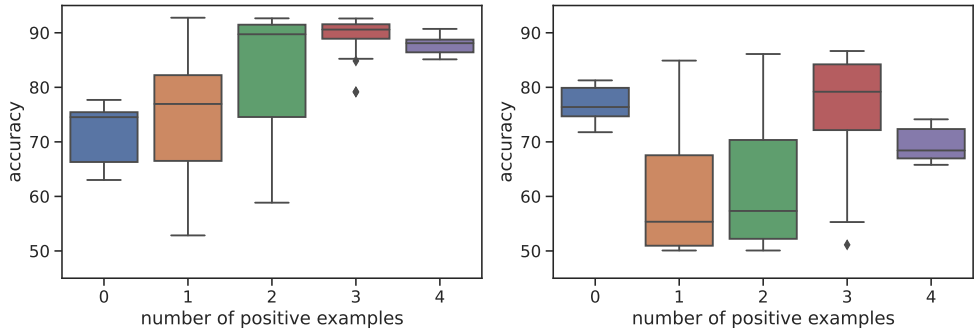
To understand what makes good examples, we explore properties of the learned policy and design additional experiments based on our qualitative examination of the selected examples. In the interest of space, we focus on label balance and coverage, and present other results based on linear policies (C.3) and length (C.4) in the Appendix.

On Amazon and SST-2, both binary sentiment classification tasks, we focus on label balance, measured by the number of **positive** labels in the demonstration set. For AGNews (4 labels) and TREC (6 labels), we instead focus on the distinct number of labels covered in demonstration. We present the results in Figure 3 and Figure 4.

Perhaps surprisingly, a well-balanced demonstration set does not consistently lead to greater performance or less variance. In Amazon, we notice that having all 4 examples being positive actually leads to good in-context learning performance, with an average accuracy of 87.8% and 4.5% greater than that of a perfectly balanced demonstration set (83.3%). A similar trend is demonstrated in SST-2, where having all positive or all negative labels leads to much smaller variance compared to more balanced sets, while outperforming perfectly balanced sets on average.

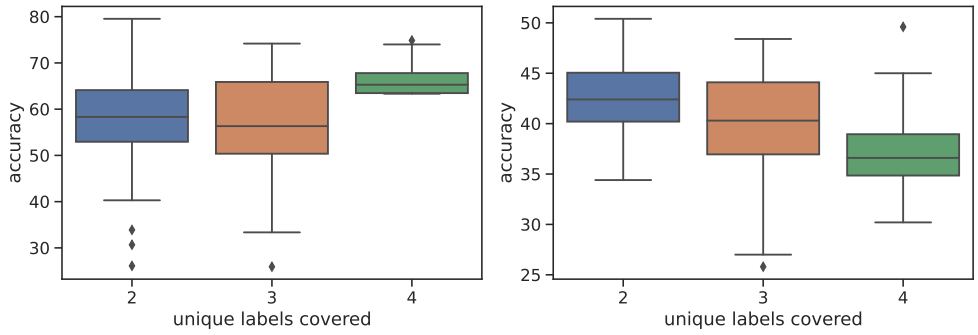
In TREC, we again observe that the model does not need to observe the entire label space to perform well. The greatest performance occurs when

[7]The sizes of GPT-3 models hosted by OpenAI are not publicly known, and we use estimations at https://blog.eleuther.ai/gpt3-model-sizes.



(a) Amazon (b) SST-2

Figure 3: Accuracies of Amazon and SST-2 with varying **label balance** (number of positive examples in demonstration), across 100 total random samples of 4 demonstration examples.



(a) AGNews (b) TREC

Figure 4: Accuracies of AGNews and TREC with varying **label coverage** (number of unique labels covered in demonstration), across 100 total random samples of 4 demonstration examples. Demonstration set that only covers 1 label is very unlikely and does not appear in our experiments.

exactly two labels are covered by demonstration, and the performance deteriorates as label coverage increases. AGNews demonstrates a somewhat expected pattern. When 4 label are covered, we observe the best performance along with a small variance. That said, covering three labels does not improve over covering two labels.

Overall, our analysis highlights the idiosyncrasies of how GPT-2 acquires information in in-context learning. The sequences that lead to strong performance may not align with human intuitions.

5 Related Work

Our paper builds on top of prior work that uses RL to solve the active learning problem (Fang et al., 2017; Liu et al., 2018), and is made possible by the recent advances in pre-trained language models (Devlin et al., 2019; Liu et al., 2019; Raffel et al., 2020; Gao et al., 2021). In-context learning, the observation that LMs (Radford et al., 2019; Brown et al., 2020; Rae et al., 2022; Zhang et al., 2022) can "learn" to perform a task when conditioned on a prompt. Xie et al. (2022) explains the emergenece of in-context learning by inferring the shared latent concept among demonstration examples, while Min et al. (2022) finds the success of in-context learning is largely independent of access to gold labels.

A variety of issues with in-context learning is discovered, including surface form competition, the phenomenon that multiple words referring to the same concept fighting for probability mass (Holtzman et al., 2021), and sensitivity of LMs due to changes in prompt (Lester et al., 2021), instruction (Mishra et al., 2022), or ordering of demonstration examples (Zhao et al., 2021; Lu et al., 2022). To optimize the performance of in-context learning, methods with varying levels of granularity are proposed. Such methods include prompt tuning (Lester et al., 2021; Vu et al., 2022; Wu et al., 2022), and instruction optimization (Mishra et al., 2022; Kojima et al., 2022). Liu et al. (2021) approaches the example selection problem by searching for nearest neighbors of test examples in the embedding space, while Rubin et al. (2022) uses a scoring LM for example retrieval.

6 Discussion

Inspired by Pang and Lee (2005), we adopt a Q&A format to discuss the implications of our work.

Q: Are GPT-2 results still relevant?

A: We believe that it is relevant for three reasons. First, GPT-2 is public and economically feasible options for many researchers. Our knowledge about GPT-2 is far from complete and expanding this understanding is useful on its own. Second, in the long term, it is unclear that everyone will have access to large models or that it is appropriate to use the largest model available in every use case. Models of moderate sizes are likely still useful depending on the use case. Third, it is important to highlight the emerging abilities over different sizes of language models. By understanding the phase change, i.e., when emerging abilities happen, we will better understand the behavior of large-scale language models.

That said, one should caution against making generalizing claims based on results from GPT-2, because the results may not generalize to GPT-3 (Bowman, 2022). This is why we present negative results from GPT-3. Differing results between GPT-2 and GPT-3 or more generally models of different sizes will be a reality in NLP for a while. It is important for the NLP community to collectively build knowledge about such differences and develop the future ecosystem of models.

Q: Why did you not experiment with GPT-3-Davinci?

A: The goal of this work is twofold: 1) assessing the ability of large-scale language models to acquire new information and 2) exploring whether reinforcement learning can identify reliable strategies for actively selecting examples. Our results are generally positive on GPT-2. Meanwhile, we observe relatively small variance after calibration with GPT-3-Babbage, so it does not seem economically sensible to experiment with even bigger models.

Q: Why did you choose $k = 4$? Is this generalizable?

A: Our experiments are limited by the context window of GPT-2 (1024 tokens) and GPT-3 (2048) tokens. Using k beyond 4 would frequently leads to demonstration examples overflowing the token limit and need to be truncated. Additionally, prior work (Zhao et al., 2021; Brown et al., 2020) shows diminishing improvements of in-context learning performance by adding the number of demonstration examples beyond 4. Therefore, we believe experimenting with $k = 4$ is a reasonable choice. We are optimistic that our framework and method can generalize to different shots.

7 Conclusion

In this work, we investigate how large language models acquire information through the perspective of example selection for in-context learning. In-context learning with GPT-2 and GPT-3 is sensitive to the selection of demonstration examples. In order to identify generalizable properties of useful demonstration examples, we study active example selection where unlabeled examples are iteratively selected, annotated, and added to the prompt. We use reinforcement learning to train policies for active example selection. The learned policy stablizes in-context learning and improves accuracy when we apply it to a new pool of unlabeled examples or even completely new tasks unseen during training for GPT-2. Our analyses further reveal that properties of useful demonstration examples can deviate from human intuitions.

Examples selected from GPT-2 can still lead to a small improvement on GPT-3 Ada, however, the gain diminishes on larger models (i.e., Babbage and Curie). Our results highlight the challenges of generalization in the era of large-scale models due to their emerging capabilities. We believe that it is important for the NLP community to collectively build knowledge about such differences and develop the future ecosystem of models together.

Ethics Statement

Our primary goal is to understand how large language models acquire new information in in-context learning through the perspective of example selection. A better understanding can help develop more effective strategies for in-context learning as well as better large-scale language models. However, these strategies can also be used in applications that may incur harm to the society.

Acknowledgments

We thank all anonymous reviewers for their insightful suggestions and comments. We thank all members of the Chicago Human+AI Lab for feedback on early versions of this work. This work was supported in part by an Amazon research award, a Salesforce research award, a UChicago DSI discovery grant, and an NSF grant IIS-2126602.

References

Kai Arulkumaran, Marc Peter Deisenroth, Miles Brundage, and Anil Anthony Bharath. 2017. A Brief Survey of Deep Reinforcement Learning. *IEEE Signal Processing Magazine*, 34(6):26–38.

Richard Bellman. 1957. *Dynamic Programming*, first edition. Princeton University Press, Princeton, NJ, USA.

Samuel Bowman. 2022. The dangers of underclaiming: Reasons for caution when reporting how NLP systems fail. In *Proceedings of the 60th Annual Meeting of the Association for Computational Linguistics (Volume 1: Long Papers)*, pages 7484–7499, Dublin, Ireland. Association for Computational Linguistics.

Tom Brown, Benjamin Mann, Nick Ryder, Melanie Subbiah, Jared D Kaplan, Prafulla Dhariwal, Arvind Neelakantan, Pranav Shyam, Girish Sastry, Amanda Askell, Sandhini Agarwal, Ariel Herbert-Voss, Gretchen Krueger, Tom Henighan, Rewon Child, Aditya Ramesh, Daniel Ziegler, Jeffrey Wu, Clemens Winter, Chris Hesse, Mark Chen, Eric Sigler, Mateusz Litwin, Scott Gray, Benjamin Chess, Jack Clark, Christopher Berner, Sam McCandlish, Alec Radford, Ilya Sutskever, and Dario Amodei. 2020. Language Models are Few-Shot Learners. In *Advances in Neural Information Processing Systems*, volume 33, pages 1877–1901. Curran Associates, Inc.

Ido Dagan and Sean P. Engelson. 1995. Committee-based sampling for training probabilistic classifiers. In *Proceedings of the Twelfth International Conference on International Conference on Machine Learning*, ICML'95, pages 150–157, San Francisco, CA, USA. Morgan Kaufmann Publishers Inc.

Jacob Devlin, Ming-Wei Chang, Kenton Lee, and Kristina Toutanova. 2019. BERT: Pre-training of Deep Bidirectional Transformers for Language Understanding. In *Proceedings of the 2019 Conference of the North American Chapter of the Association for Computational Linguistics: Human Language Technologies, Volume 1 (Long and Short Papers)*, pages 4171–4186, Minneapolis, Minnesota. Association for Computational Linguistics.

Meng Fang, Yuan Li, and Trevor Cohn. 2017. Learning how to Active Learn: A Deep Reinforcement Learning Approach. In *Proceedings of the 2017 Conference on Empirical Methods in Natural Language Processing*, pages 595–605, Copenhagen, Denmark. Association for Computational Linguistics.

Tianyu Gao, Adam Fisch, and Danqi Chen. 2021. Making Pre-trained Language Models Better Few-shot Learners. In *Proceedings of the 59th Annual Meeting of the Association for Computational Linguistics and the 11th International Joint Conference on Natural Language Processing (Volume 1: Long Papers)*, pages 3816–3830, Online. Association for Computational Linguistics.

Hado Hasselt. 2010. Double Q-learning. In *Advances in Neural Information Processing Systems*, volume 23. Curran Associates, Inc.

Matteo Hessel, Joseph Modayil, Hado van Hasselt, Tom Schaul, Georg Ostrovski, Will Dabney, Dan Horgan, Bilal Piot, Mohammad Azar, and David Silver. 2017. Rainbow: Combining Improvements in Deep Reinforcement Learning.

Ari Holtzman, Peter West, Vered Shwartz, Yejin Choi, and Luke Zettlemoyer. 2021. Surface Form Competition: Why the Highest Probability Answer Isn't Always Right.

Robert Kirk, Amy Zhang, Edward Grefenstette, and Tim Rocktäschel. 2022. A Survey of Generalisation in Deep Reinforcement Learning.

Takeshi Kojima, Shixiang Shane Gu, Machel Reid, Yutaka Matsuo, and Yusuke Iwasawa. 2022. Large Language Models are Zero-Shot Reasoners.

Aviral Kumar, Aurick Zhou, George Tucker, and Sergey Levine. 2020. Conservative Q-Learning for Offline Reinforcement Learning.

Brian Lester, Rami Al-Rfou, and Noah Constant. 2021. The Power of Scale for Parameter-Efficient Prompt Tuning. *arXiv:2104.08691 [cs]*.

Long-Ji Lin. 1992. Self-Improving Reactive Agents Based on Reinforcement Learning, Planning and Teaching. *Machine Language*, 8(3-4):293–321.

Jiachang Liu, Dinghan Shen, Yizhe Zhang, Bill Dolan, Lawrence Carin, and Weizhu Chen. 2021. What Makes Good In-Context Examples for GPT-3?

Ming Liu, Wray Buntine, and Gholamreza Haffari. 2018. Learning How to Actively Learn: A Deep Imitation Learning Approach. In *Proceedings of the 56th Annual Meeting of the Association for Computational Linguistics (Volume 1: Long Papers)*, pages 1874–1883, Melbourne, Australia. Association for Computational Linguistics.

Yinhan Liu, Myle Ott, Naman Goyal, Jingfei Du, Mandar Joshi, Danqi Chen, Omer Levy, Mike Lewis, Luke Zettlemoyer, and Veselin Stoyanov. 2019. RoBERTa: A Robustly Optimized BERT Pretraining Approach.

Yao Lu, Max Bartolo, Alastair Moore, Sebastian Riedel, and Pontus Stenetorp. 2022. Fantastically Ordered Prompts and Where to Find Them: Overcoming Few-Shot Prompt Order Sensitivity. In *Proceedings of the 60th Annual Meeting of the Association for Computational Linguistics (Volume 1: Long Papers)*, pages 8086–8098, Dublin, Ireland. Association for Computational Linguistics.

Sewon Min, Xinxi Lyu, Ari Holtzman, Mikel Artetxe, Mike Lewis, Hannaneh Hajishirzi, and Luke Zettlemoyer. 2022. Rethinking the Role of Demonstrations: What Makes In-Context Learning Work? *arXiv:2202.12837 [cs]*.

Swaroop Mishra, Daniel Khashabi, Chitta Baral, Yejin Choi, and Hannaneh Hajishirzi. 2022. Reframing Instructional Prompts to GPTk's Language.

Volodymyr Mnih, Koray Kavukcuoglu, David Silver, Alex Graves, Ioannis Antonoglou, Daan Wierstra, and Martin Riedmiller. 2013. Playing Atari with Deep Reinforcement Learning.

Reiichiro Nakano, Jacob Hilton, Suchir Balaji, Jeff Wu, Long Ouyang, Christina Kim, Christopher Hesse, Shantanu Jain, Vineet Kosaraju, William Saunders, Xu Jiang, Karl Cobbe, Tyna Eloundou, Gretchen Krueger, Kevin Button, Matthew Knight, Benjamin Chess, and John Schulman. 2022. WebGPT: Browser-assisted question-answering with human feedback.

Andrew Y. Ng, Daishi Harada, and Stuart J. Russell. 1999. Policy Invariance Under Reward Transformations: Theory and Application to Reward Shaping. In *Proceedings of the Sixteenth International Conference on Machine Learning*, ICML '99, pages 278–287, San Francisco, CA, USA. Morgan Kaufmann Publishers Inc.

Bo Pang and Lillian Lee. 2005. Seeing stars: Exploiting class relationships for sentiment categorization with respect to rating scales. In *Proceedings of ACL*, pages 115–124.

Deepak Pathak, Pulkit Agrawal, Alexei A. Efros, and Trevor Darrell. 2017. Curiosity-Driven Exploration by Self-Supervised Prediction. In *2017 IEEE Conference on Computer Vision and Pattern Recognition Workshops (CVPRW)*, pages 488–489, Honolulu, HI, USA. IEEE.

Ethan Perez, Douwe Kiela, and Kyunghyun Cho. 2021. True Few-Shot Learning with Language Models. In *Advances in Neural Information Processing Systems*.

Rafael Figueiredo Prudencio, Marcos R. O. A. Maximo, and Esther Luna Colombini. 2022. A Survey on Offline Reinforcement Learning: Taxonomy, Review, and Open Problems.

Alec Radford, Jeff Wu, Rewon Child, David Luan, Dario Amodei, and Ilya Sutskever. 2019. Language models are unsupervised multitask learners.

Jack W. Rae, Sebastian Borgeaud, Trevor Cai, Katie Millican, Jordan Hoffmann, Francis Song, John Aslanides, Sarah Henderson, Roman Ring, Susannah Young, Eliza Rutherford, Tom Hennigan, Jacob Menick, Albin Cassirer, Richard Powell, George van den Driessche, Lisa Anne Hendricks, Maribeth Rauh, Po-Sen Huang, Amelia Glaese, Johannes Welbl, Sumanth Dathathri, Saffron Huang, Jonathan Uesato, John Mellor, Irina Higgins, Antonia Creswell, Nat McAleese, Amy Wu, Erich Elsen, Siddhant Jayakumar, Elena Buchatskaya, David Budden, Esme Sutherland, Karen Simonyan, Michela Paganini, Laurent Sifre, Lena Martens, Xiang Lorraine Li, Adhiguna Kuncoro, Aida Nematzadeh, Elena Gribovskaya, Domenic Donato, Angeliki Lazaridou, Arthur Mensch, Jean-Baptiste Lespiau, Maria Tsimpoukelli, Nikolai Grigorev, Doug Fritz, Thibault Sottiaux, Mantas Pajarskas, Toby Pohlen, Zhitao Gong, Daniel Toyama, Cyprien de Masson d'Autume, Yujia Li, Tayfun Terzi, Vladimir Mikulik, Igor Babuschkin, Aidan Clark, Diego de Las Casas, Aurelia Guy, Chris Jones, James Bradbury, Matthew Johnson, Blake Hechtman, Laura Weidinger, Iason Gabriel, William Isaac, Ed Lockhart, Simon Osindero, Laura Rimell, Chris Dyer, Oriol Vinyals, Kareem Ayoub, Jeff Stanway, Lorrayne Bennett, Demis Hassabis, Koray Kavukcuoglu, and Geoffrey Irving. 2022. Scaling Language Models: Methods, Analysis & Insights from Training Gopher.

Colin Raffel, Noam Shazeer, Adam Roberts, Katherine Lee, Sharan Narang, Michael Matena, Yanqi Zhou, Wei Li, and Peter J. Liu. 2020. Exploring the Limits of Transfer Learning with a Unified Text-to-Text Transformer.

Ohad Rubin, Jonathan Herzig, and Jonathan Berant. 2022. Learning To Retrieve Prompts for In-Context Learning.

Burr Settles. 2009. Active learning literature survey.

Richard Socher, Alex Perelygin, Jean Wu, Jason Chuang, Christopher D. Manning, Andrew Ng, and Christopher Potts. 2013. Recursive Deep Models for Semantic Compositionality Over a Sentiment Treebank. In *Proceedings of the 2013 Conference on Empirical Methods in Natural Language Processing*, pages 1631–1642, Seattle, Washington, USA. Association for Computational Linguistics.

Friedrich Freiherr Von Wieser. 1893. *Natural Value*. Macmillan and Company.

Ellen M. Voorhees and Dawn M. Tice. 2000. Building a question answering test collection. In *Proceedings of the 23rd Annual International ACM SIGIR Conference on Research and Development in Information Retrieval*, SIGIR '00, pages 200–207, New York, NY, USA. Association for Computing Machinery.

Tu Vu, Brian Lester, Noah Constant, Rami Al-Rfou, and Daniel Cer. 2022. SPoT: Better Frozen Model Adaptation through Soft Prompt Transfer. *arXiv:2110.07904 [cs]*.

Jason Wei, Yi Tay, Rishi Bommasani, Colin Raffel, Barret Zoph, Sebastian Borgeaud, Dani Yogatama, Maarten Bosma, Denny Zhou, Donald Metzler, et al. 2022. Emergent abilities of large language models. *arXiv preprint arXiv:2206.07682*.

Zhuofeng Wu, Sinong Wang, Jiatao Gu, Rui Hou, Yuxiao Dong, V. G. Vinod Vydiswaran, and Hao Ma. 2022. IDPG: An Instance-Dependent Prompt Generation Method. *arXiv:2204.04497 [cs]*.

Sang Michael Xie, Aditi Raghunathan, Percy Liang, and Tengyu Ma. 2022. An Explanation of In-context Learning as Implicit Bayesian Inference.

Susan Zhang, Stephen Roller, Naman Goyal, Mikel Artetxe, Moya Chen, Shuohui Chen, Christopher Dewan, Mona Diab, Xian Li, Xi Victoria Lin, Todor Mihaylov, Myle Ott, Sam Shleifer, Kurt Shuster, Daniel Simig, Punit Singh Koura, Anjali Sridhar, Tianlu Wang, and Luke Zettlemoyer. 2022. OPT: Open Pre-trained Transformer Language Models.

Xiang Zhang, Junbo Zhao, and Yann LeCun. 2015. Character-level Convolutional Networks for Text Classification. In *Advances in Neural Information Processing Systems*, volume 28. Curran Associates, Inc.

Tony Z. Zhao, Eric Wallace, Shi Feng, Dan Klein, and Sameer Singh. 2021. Calibrate Before Use: Improving Few-Shot Performance of Language Models.

A Conservative Q-Learning

The objective of standard Q-learning is to minimize the Bellman Error (BE):

$$\text{BE}(Q) = \mathbb{E}_{s,a,s' \sim \mathcal{D}} \Big[r(s,a) + \gamma \max_{a'} Q(s',a') - Q(s,a) \Big].$$

An issue with offline Q-learning is there are OOD actions that do not appear in the training data. Learned Q-networks often overestimate these Q-values, resulting in the policy taking unfamiliar actions during evaluation and hurts performance. To mitigate this issue, conservative Q-learning (CQL) adds a penalty term to regularize Q-values:

$$\min_Q \alpha \mathbb{E}_{s \sim \mathcal{D}} \Big[\log \sum_a \exp(Q(s,a)) - \mathbb{E}_{a \sim \hat{\pi}_\beta} \big[Q(s,a) \big] \Big] + \frac{1}{2} \text{BE}(Q)^2,$$

where α is a weight term, and $\hat{\pi}_\beta$ is the *behavior policy*, under which the offline transitions are collected for training. Notice this objective penalizes all unobserved actions under $\hat{\pi}_\beta$. Intuitively, this regularizer leads to a policy that avoids unfamiliar actions during evaluation. We refer the interested reader to the original paper for theoretical guarantees and further details (Kumar et al., 2020).

B Hyperparameters

We report the list of hyperparameters for the hyperparameter search in Table 5. We use grid search over these hyperparameters to determine the combination that maximizes validation performance.

Hyperparameter	Value
Train steps	8000
Batch size	16
Hidden dim (MLP)	16
Replay memory size	50000
Learning rate	1e-4, 3e-4, 5e-4
CQL regularization weight α	0, 0.1, 0.2
Target network update steps	100, 200, 400
Dropout rate	0, 0.25

Table 5: List of hyperparameters used in our experiments.

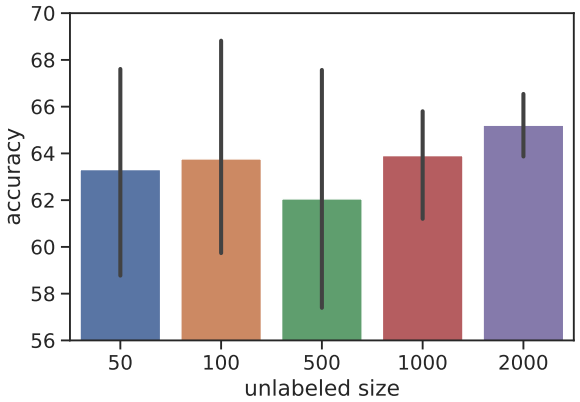


Figure 5: Average NEW TASK (transfer) accuracy on 4 tasks across 5 random seeds. 95% confidence intervals are reported as error bars.

During validation, the policy picks from the **reward set**, and is evaluated on the **training set**, whereas in training, we pick from the **training set** and evaluate on the **reward set**. We point out that our validation scheme does not use extra data.

Table 6 further includes the performance of linear policies. The performance of linear policies is better than the baselines, but clearly worse than the MLP policy.

C Additional Results

We present results on the effect of unlabeled size and on transfer GPT-3. We also provide additional analysis towards understanding what makes good examples for in-context learning.

C.1 Effect of Unlabeled Size

In §4.2, we noticed the number of unlabeled examples available for selection plays a role in the performance our policies. One might expect the transfer performance in the NEW TASK setting scales with unlabeled size, simply because there are additional examples to pick from.

Method	Average	AGNews	Amazon	SST-2	TREC
random	59.6	$55.2_{10.5}$	$76.3_{12.3}$	$66.2_{12.9}$	$40.8_{4.7}$
max-entropy	59.3	$58.8_{11.3}$	$74.8_{5.1}$	$65.7_{10.7}$	$37.8_{6.7}$
best-of-10	72.5	$72.1_{1.9}$	$91.1_{0.6}$	$81.1_{4.4}$	$45.6_{3.5}$
greedy-oracle	78.0	$80.6_{1.7}$	$91.8_{1.1}$	$81.7_{3.9}$	$58.0_{7.5}$
Linear policy (**seen examples**)	65.6	$62.8_{7.8}$	$82.7_{8.6}$	$74.2_{5.8}$	$42.8_{2.9}$
Linear policy (**1000 new examples**)	65.9	$69.5_{6.0}$	$83.7_{6.2}$	$65.2_{4.9}$	$45.2_{2.8}$
MLP policy (**seen examples**)	71.4	$70.8_{7.8}$	$90.4_{1.9}$	$81.0_{3.5}$	$43.3_{2.0}$
MLP policy (**1000 new examples**)	69.0	$65.5_{7.4}$	$88.5_{4.2}$	$76.7_{7.5}$	$45.4_{5.0}$

Table 6: **SAME TASK** accuracy on AGNews, Amazon, SST-2 and TREC, across 5 random seeds, with our methods (using MLP and Linear networks as policies). 95% confidence intervals are reported as subscripts.

In Figure 5, we plot average accuracies in the NEW TASK setting, where we train our policies on three datasets and evaluate on a held-out dataset. Here, we notice the benefit of a larger unlabeled set is twofold, both in increasing transfer performance, and in reducing variance. That said, the improvement is not necessarily monotonic due to the large variance. Interestingly, our learned policy is performant even when the unlabeled set is small. Picking from 50 unlabeled examples, our policies reaches an average accuracy of 63.3%, still manage to outperform random demonstration (59.6%).

C.2 Transfer to GPT-3

Despite demonstrating abilities to generalize across tasks, it is yet clear whether learned policies on GPT-2 can generalize to other models, such as GPT-3. In table 7, we report the performance of transferring both learned policies and selected examples from GPT-2 to GPT-3 ADA, BABBAGE and CURIE.

We observe mixed results when transferring to GPT-3. With an uncalibrated ADA model, we observe a small, but measurable improvement of transferring either policy (1.1%) or examples directly (0.9%). Such a trend holds for the calibrated ADA model too (0.4% and 1.9%). Despite the improved performance, the benefits of variance reduction is diminished. Perhaps surprising is the generalization of learned policies: it suggests different models could indeed share similar preferences for demonstration examples.

On the other hand, we observe negative results when transferring to BABBAGE. When transferring learned policy to an uncalibrated BABBAGE model, we notice the performance drops by 1.6%. For cost considerations, we run CURIE experiments for one random set and do not report variance. Marginal gains are observed when transferring policy to the uncalibrated model (1.8%) and examples to the calibrated model (1.0%). In other scenarios, transfer results match or underperform base models. As the observed results could be attributed to randomness, we hold short of drawing conclusions.

C.3 Coefficients in Linear Policies

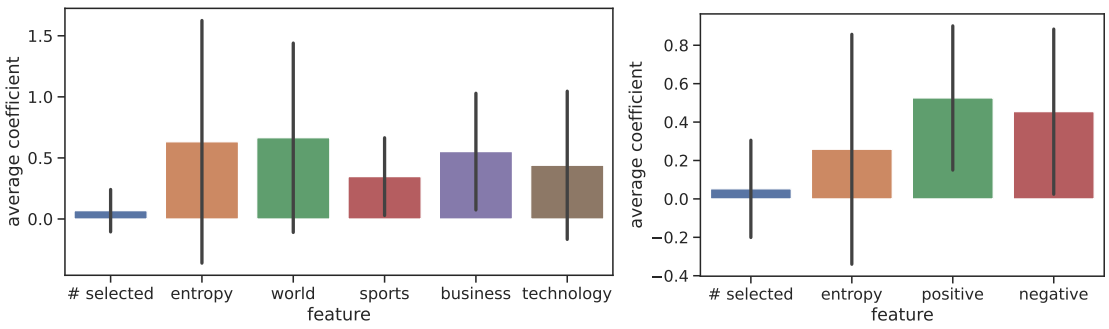
Although linear policies perform worse than the MLP, they are more interpretable. Figure 6 shows the coefficients of feature representations of actions for AGNews and SST-2. The average coefficient of entropy is indeed positive, suggesting that strategies encouraging class balance have some value. However, it is often not the most important feature. For example, positive examples in SST-2 matter more, which is consistent with our observation in the main paper. Moreover, the variance is large, highlighting the challenges in learning a generalizable policy.

C.4 Effect of Length

We also examine the effect of length on in-context learning. Intuitively, one might expect longer examples to be more meaningful. However, we do not see a correlation between length and accuracy in AGNews and TREC, and a non-significant negative correlations in SST-2. In Amazon, we observe a statistically significant (p-value = 0.019), but weak correlation between length and accuracy. Overall, there is no evidence suggesting longer examples improve in-context learning performance.

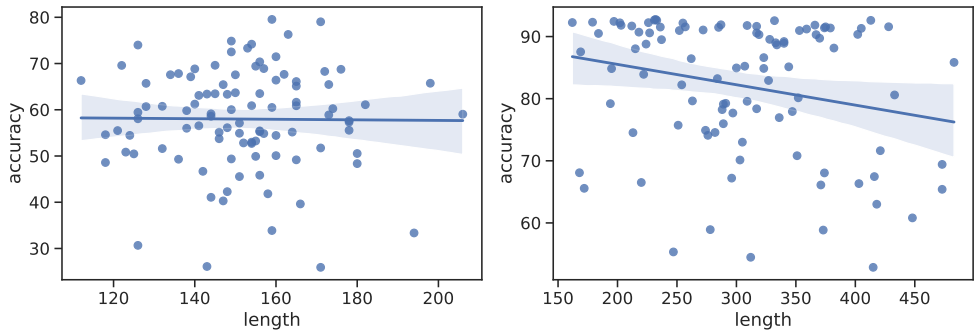
Model	Average	AGNews	Amazon	SST-2	TREC
ADA	59.0	$62.9_{15.3}$	$87.0_{5.3}$	$65.0_{8.9}$	$21.2_{5.8}$
ADA (**C**)	62.5	$64.0_{3.5}$	$90.0_{1.1}$	$73.8_{8.5}$	$22.1_{4.6}$
GPT-2 policy → ADA	60.1	$51.8_{15.5}$	$89.1_{1.7}$	$73.3_{15.0}$	$26.2_{3.9}$
GPT-2 policy → ADA (**C**)	62.9	$55.6_{5.9}$	$89.7_{2.2}$	$86.7_{1.6}$	$19.5_{1.4}$
GPT-2 examples → ADA	59.9	$48.9_{12.5}$	$89.3_{2.5}$	$74.8_{11.4}$	$26.6_{3.9}$
GPT-2 examples → ADA (**C**)	64.4	$62.0_{8.3}$	$88.7_{3.2}$	$84.0_{3.6}$	$23.0_{5.3}$
BABBAGE	70.3	$68.0_{12.3}$	$93.4_{0.7}$	$92.2_{2.4}$	$27.4_{5.1}$
BABBAGE (**C**)	74.4	$78.1_{5.3}$	$92.7_{1.4}$	$90.8_{1.0}$	$36.0_{3.5}$
GPT-2 policy → BABBAGE	68.7	$58.0_{5.9}$	$93.6_{2.2}$	$90.6_{1.6}$	$32.5_{1.4}$
GPT-2 policy → BABBAGE (**C**)	74.4	$75.1_{5.3}$	$93.4_{0.5}$	$90.3_{1.7}$	$38.8_{6.1}$
GPT-2 examples → BABBAGE	65.8	$42.6_{10.0}$	$93.0_{0.4}$	$91.1_{2.9}$	$36.6_{8.4}$
GPT-2 examples → BABBAGE (**C**)	73.6	$73.9_{7.3}$	$93.1_{0.5}$	$91.1_{1.8}$	$36.2_{2.6}$
CURIE	74.2	76.7	94.7	93.8	31.4
CURIE (**C**)	76.3	69.8	94.8	93.4	47.0
GPT-2 policy → CURIE	76.0	81.2	95.7	96.0	31.0
GPT-2 policy → CURIE (**C**)	75.4	75.8	95.4	93.0	38.2
GPT-2 examples → CURIE	74.4	77.7	93.8	94.3	31.8
GPT-2 examples → CURIE (**C**)	77.3	79.8	93.1	94.6	41.8

Table 7: Transfer of policies and examples learned on GPT-2 to various GPT-3 models across 5 random sets of 4-shot demonstration examples. **C** indicates calibration. 95% confidence intervals are reported as subscripts. Due to resource constraints, we limit experiments with CURIE to 1 random set.



(a) AGNews (b) SST-2

Figure 6: Average coefficients of linear policies trained on AGNews and SST-2 across 5 runs. Error bars show the standard deviation.



(a) AGNews ($r = -0.01$)

(b) Amazon ($r = -0.23^{*}$)

(c) SST-2 ($r = -0.08$)

(d) TREC ($r = -0.00$)

Figure 7: Correlation between length (number of words) of the demonstration prompt and in-context learning performance across 100 sets of randomly sample 4-shot demonstration. $*$ indicates a p-value < 0.05.