Chasing Random – Investigating the "Gains" achieved through Instruction Selection Strategies at Scale

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Abstract

Prior work (Zhou et al., 2023a) has shown that language models can be trained to follow user instructions using only a small set of high-quality instructions. This has accelerated the development of general-purpose instruction selection strategies which enable sampling such high quality subsets from large instruction-tuning datasets. Typically, the performance of these strategies is not demonstrated across a uniform experimental setup and *thus their generalization capabilities are not well established*. In this work, we analyze popular selection strategies across different source datasets, selection budgets and evaluation benchmarks: Our results indicate that selection strategies generalize poorly, often failing to consistently outperform even random baselines. Further, since such strategies are often designed to reduce the cost of fine-tuning, we also study their cost-performance trade-off. Here, our findings reveal that data selection can often exceed the cost of full fine-tuning, yielding only marginal—and sometimes negative—performance gains compared to full fine-tuning and random sampling.

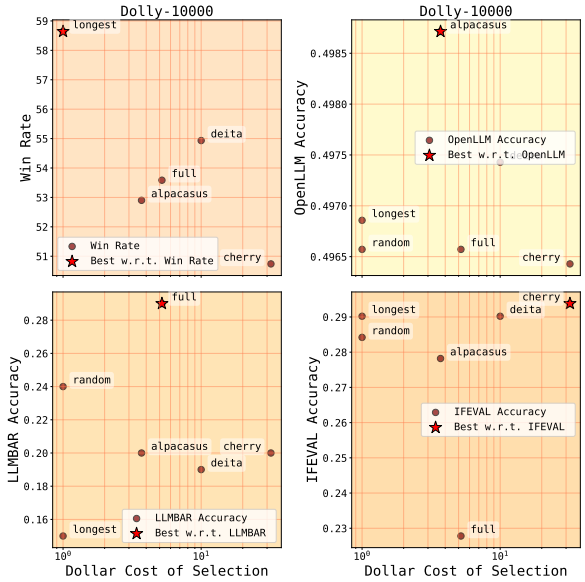


Figure 1: Selection Cost Versus Performance on different benchmarks when selecting 10000 samples from DOLLY (Conover et al., 2023): Upper Right Region (low cost, high performance) is ideal. Key Takeaways are: (a) Random baselines are reasonably competitive whilst incurring the least cost (b) Depending on the evaluation metric, the best strategy varies significantly with the setup (⋆indicates best selection strategy on the benchmark).

1 Introduction

Instruction fine-tuning is often considered a crucial step in training large language models, LLMs, to effectively meet the needs of users. By training LLMs over tens of thousands instruction-response tuples that highlight user preferences, models can demonstrate *instruction-following capabilities* which position them as useful tools for a wide variety of tasks. There has been a rapid increase in the development of *instruction selection* strategies (Qin et al., 2024b; Wang et al., 2024) to curate a subset of high-quality instructions to train competitive instruction following models more efficiently.

The experimental setups of general-purpose instruction data selection can be very varied (Qin et al., 2024b); Unlike task-specific data selection, they are not geared towards optimizing performance for some specific goal (Xie et al., 2023a). Therefore, their utility is strongly tied to their generalization beyond a few limited setups endorsed by their designers.

Measuring this generalization is hard for several reasons. Firstly, proposed strategies are often applied across arbitrary experimental setups including different source datasets, selection budgets and testing benchmarks. Additionally, since there isn't a single set of behaviors expected of instruction tuned models, it is unclear performance gained through selection on one instruction-following benchmark will induce correlated gains on other

instruction-following benchmarks. Finally, the time and resources necessitated by a selection strategy vary significantly. While strategies like Chen et al. (2023) can directly incur dollar-cost through their dependence on API-accessible commercial large language model APIs, others like (Li et al., 2023a; Liu et al., 2023) design selection methods that involve finetuning or inferencing on LLMs, thus mandating a GPU-reliant infrastructural cost.

Contribution: In this work, we carry out an exhaustive investigation for over 60 experimental configurations across 4 evaluation benchmarks each to provide evidence for the following findings (a) Instruction Selection Strategies don't generalize to reasonably similar experimental configurations. Consequently, **no selection strategy beats random selection *consistently***. (b) **Competence in General Purpose Instruction Following is a subjective goal** and hence, comparing selection strategies on different facets of this goal can produce contradictory trends. (c) **Many strategies scale poorly as the budget of selection increases**: Incurred selection costs can often overshoot the cost of training with the entire dataset and do not give consistently high gains over random selection carried out at negligible cost.

We argue that the lack of generality and consistent performance over a naive baseline makes it difficult to use existing example selection strategies in the wild: if selection through a strategy is not *consistently cost-effective* over a naive form of subsampling (random-sampling) across *reasonably similar experimental configurations*, it is unclear if selection is an advantageous step in the process of training competitive LLMs.

2 Literature Review

We focus on general-purpose instruction selection methods, which aim to equip models to follow user queries that aren't specific to a fixed task, capability or domain (Wang et al., 2024).Such methods involve strategies including rule-based metrics (Cao et al., 2023), length (Zhao et al., 2024), diversity (Liu et al., 2023) and model derived uncertainity measurements (Li et al., 2023a) to subsample large instruction tuning datasets. They sit in contrast to task-specific data selection strategies which optimize for performance on a known test distribution or task specification (Xia et al., 2024; Xie et al., 2023b; Pan et al., 2024).

Due to broad definition of instructing following, experimental setups for such work can show significant variation: While some adopt source distributions of varying origins (Zhou et al., 2023a; Li et al., 2024; Shen, 2024), some even synthetically augment subsets of data during their selection process (Liu et al., 2023). Similarly, the selection budget applied on these datasets can vary from anywhere between a mere 200 samples (Wei et al., 2023) to 15K (Du et al., 2023; Xia et al., 2024). We summarize some of the most popular choices in Table 1 and utilize these for our experiments.
A few methods also (Mekala et al., 2024; Xia et al., 2024) acknowledge and accordingly attempt to address the relatively high cost of selection by either exploring the use of cheaper proxies like smaller models, low-rank approximations or adopting sequential pipelines (Ge et al., 2024) to make instruction data selection more efficient. Finally, work like (Liu et al., 2024; Wang et al., 2024) have highlighted the concerns in comparing between the performance of instruction selection strategies. Through a unified comparison based on efficiency and feasibility, Liu et al. (2024) provide strong evidence that the comparison between instruction selection strategies needs to be more holistic. Distinctive from this work though, their evaluation does not focus on a comparison including random baselines.

Our Work Distinct from prior work, we focus on calibrating both, the performance gain and the cost benefit of various instruction selection strategies against the negligible cost alternative, random baselines. In the process, we also uncover the sharp sensitivity of selection strategies to their experimental setups which can significantly harm the ease of their adoption.

3 Experimental Setup

In this section, we briefly describe our source datasets, the selection strategies we study and our evaluation setup.

3.1 Source Datasets and Evaluation Setups

Table 1 provides a concise description of all our datasets and evaluation benchmarks. For FLAN, as a precaution against including disproportionately high representations towards tasks that are overly-represented in the original composition, we curate a smaller subset of FLAN, by limiting the datapoints sampled per task to 50 for our evaluation. The

Source Distribution	Authorship	Number of Samples	Brief Description
FLAN (Longpre et al., 2023)	Automatic	88k	Includes Flan 2021, P3, Super-Natural Instructions among other datasets.
DOLLY (Conover et al., 2023)	Human	15k	Instruction-responses crafted by Databricks employees.
EVOL (Xu et al., 2023)	LLM	196k	Modifying seed instructions using ChatGPT.
ALPACA (Taori et al., 2023)	LLM	52k	ChatGPT[1] driven generation with Self-Instruct's pipeline.
Evaluation Setup	**Number of Samples**	**Metric**	**Brief Description**
IFEVAL (Zhou et al., 2023b)	500	Instruct, Prompt Level Accuracy	Instructions have verifiable prompts to check if model fulfills all prompts in an instruction.
ALPACAEVAL (Li et al., 2023b)	805	Length Controlled Win Rate	Judges LLM responses by an automatic annotator with high human-correlation.
LLMBAR (Zeng et al., 2023)	100 (Natural Set)	Accuracy	Checks model preference over instruction responses to check if a model identifies faithful responses.
OPENLLM (Gao et al., 2023)	Task-Specific	Accuracy	MMLU, ARC-Easy, ARC-Challenge, Wino-Grande, TruthfulQA, HellaSwag

Table 1: A brief overview of the source distributions we investigate and the Evaluation Setups we consider.

resulting dataset contains 88K examples and we refer to this version as FLAN. For ALPACAEVAL, we use a fixed randomly sampled subset of 300 samples to reduce the cost overhead of our evaluations. We use the default recommended annotator configuration using GPT-4-Turbo.

3.2 Selection Strategies

Alpagasus ($S_{\text{alpacasus}}$) Chen et al. (2023) use GPT-3.5 as scorer (between 1-5) to score samples from ALPACA and include the highest scoring samples.

Longest (S_{longest}) Zhao et al. (2024) include the instructions with the longest responses.

Cherry (S_{cherry}) Li et al. (2023a) use a sequential approach of selecting instructions: they apply k-means clustering to the last hidden state embeddings of all instruction in a source dataset to get a set of 1000 instructions (100 clusters and 10 samples per cluster). Then, they use this subset of instructions to finetune a model, referred to as the pre-experienced model. Finally, this model scores each sample with an Instruction Difficulty or IFD and the highest scoring samples are included in the selected subset.

DEITA (S_{deita}) Liu et al. (2023) train a scorer akin to Alpagasus to first score the entire dataset cheaply and then, rather than choosing the entire budget of instructions in one shot - iteratively construct the selected subset by checking the similarity of a candidate instruction to the current pool of instructions.

Uniform Random (S_{random}) This is the naivest form of sampling and acts as our baseline. We report numbers with error bars for trials across 3 such random seeds. We also resample for any random subset that ends up having more than 30% overlap with the data sampled with any strategy for all datasets expect dolly (due to Dolly's limited size, a maximum overlap of about 50% is possible only for the highest budget 10000).

Strict Random ($S_{\text{strictrandom}}$) We also create a special variant of our random-baselines called the "strictrandom" baselines which is created by sampling from the dataset after removing **all** the target instructions that have been deemed high-quality by *any* of the selection strategies. In practice, the strictrandom baselines can also be considered as sampling data from the complement set of all strategies' "high-quality" subsets of budget 10000.

Full Dataset: The entire dataset is used to train the model. Note that we include this variant without tuning optimally for each dataset and include this only to compare the gains that can be naively procured by avoiding selection altogether.

3.3 Base Model

We use the LLaMa-7B (Touvron et al., 2023) for all our experiments.This model's choice is dictated by it's use as a common choice for demonstrating and ablating the performance of the instruction selection strategies that we study (Table 2 (Qin et al., 2024b)). We provide all details of the 3 hyperparameter sets we test in the Appendix A.1).

4 Results

In this section, we present evidence supporting our conclusions on the brittle generalization of instruction selection strategies (§4.1 and §4.2) as well as the negative utility of expending cost on data selection §4.3.

4.1 Most Strategies Fail to Beat Random Sampling Consistently

In the space of instruction data selection, it is very common to show that M_{selected} outperform $M_{\text{full-dataset}}$ by over 50% (i.e., an LLM judge prefers the outputs of the M_{selected} more than the $M_{\text{full-dataset}}$). We modify this experimental setup to perform these comparisons between the M_{random} and M_{selected} on ALPACAEVAL. Specifically, for each model in M_{selected}, we pair the output of the M_{selected} with a randomly chosen inference generated by a random baseline from the M_{random} trained for the same budget and dataset. We then compute the Mean-Adjusted Win-Rate by taking the signed difference between the win-rate[2] of the M_{selected} from 50%. Our results across two budgets are summarized in Figure 2.

Findings on ALPACAEVAL No strategy except S_{deita}, consistently dominates over the M_{random} across all experimental configurations. To illustrate the practical implications of this observation, consider an NLP practitioner who intends to apply data selection on the DOLLY dataset with a budget of 10,000 samples. They evaluate the performance of various selection strategies on DOLLY at a smaller budget of 5,000 samples and conclude that S_{cherry} is the most effective strategy (Figure 2). However, when this strategy is applied and empirically tested at the intended budget of 10,000 samples, the results are the opposite: S_{cherry} delivers the lowest performance among all strategies (Figure 2).

[2]In all our experiments we use length controlled win-rate to negate the effects of length-bias in LLM judges.

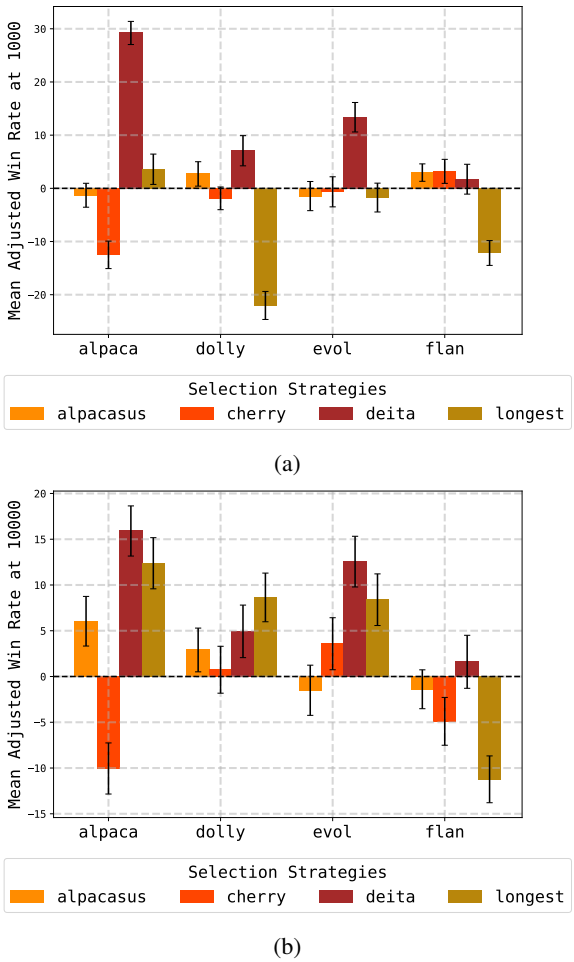
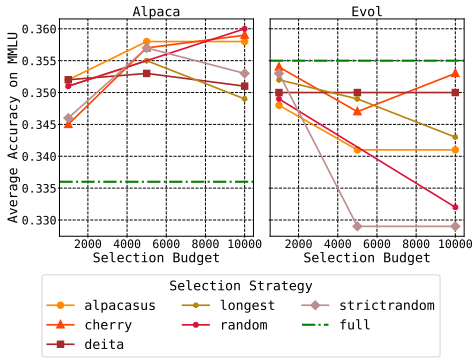


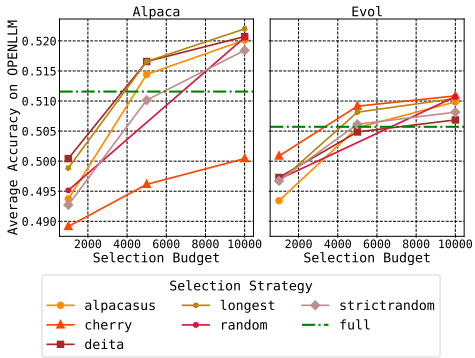
Figure 2: Mean Adjusted Win Rates on ALPACAEVAL for budgets (a) 1000 (b) 10000. A bar along the negative y-axis indicates that the M_{random} responses are preferred more than 50% of the time by GPT-4. No strategy except S_{deita} beats random baselines consistently. No strategy shows consistent performance trends across budgets as well (Section §4.1) for more details.

While we give an example with S_{cherry}, it is reasonable to assume that other strategies experience similar inflection points in their performance with the change in budget. For example, even though S_{deita}consistently outperforms random in this evaluation, it loses nearly 15% of its dominance over M_{random} at budget 10000 (when scaled from 5000) indicating the potential for an inflection point in performance for some larger budget.

Takeaway This evaluation exemplifies that the performance estimate for a selection strategy is heavily influenced by the budget and source datasets on which it is tested, and purported gains may not transfer consistently across selection budgets or data sources.



(a) Performance Trends According to MMLU



(b) Performance Trends According to Average Performance on Subset of OpenLLM

Figure 3: There is a stark difference between the performance trends of selection strategies depending upon what subset of OPENLLM tasks are chosen for evaluation. S_{random} is the worst performing strategy across all datasets when performance is gauged on MMLU, while S_{random} shows competitive performance as more tasks from OPENLLM are considered. Details in §4.1 and 9b.

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Findings with OPENLLM To corroborate this trend, we evaluate M_{selected} with M_{random} on OPENLLM. In Figure 3, we demonstrate the performance of M_{selected} across different budgets on both (a) MMLU (the only task evaluated by (Chen et al., 2023)) and (b) average performance on 7 tasks from OpenLLM (the largest union of tasks considered by (Zhao et al., 2024; Li et al., 2023a)). Not surprisingly, we find extreme divergence in the observed performance trends of selection strategies depending upon which setup is adopted: While S_{random} subsampling performs the worst by a significant margin against all selection strategies when evaluated using only MMLU (Fig 3 (a)), it performs far more competitively when more tasks from OpenLLM are considered (Fig 3 (b)), especially performing competitively at larger budgets. Note that this setup only highlights the difficulty arising out of using a non-standard subset of evaluation tasks and does not question if its even appropriate to consider *any* of these tasks as a reasonable indicator of a model's instruction following capabilities. MMLU, for example, has been shown to demonstrate several contextual limitations (Gema et al., 2024) in addition to being a multiple choice format task which significantly deviates from the traditional long-form generation format of instruction following benchmarks. Hence, it would not be too unreasonable to assume that it may not be a sufficiently aligned choice for demonstrating that a M_{selected} demonstrates instruction following capabilities in the first place.

4.2 Measuring Instruction Following for M_{selected} produces contradictory trends

Measuring instruction following capabilities is generally more complex than task-specific accuracy evaluation as instruction following models are expected to demonstrate a wide range of capabilities (Lou et al., 2024). Consequently, the subjectivity in the coverage of topics and the performance ranges of each instruction following benchmark can further influence our estimates of a selection strategy's performance. Recently, an emerging class of benchmarks recommend evaluating models with instructions which have more objective requirements (Qin et al., 2024a; Zhou et al., 2023b). Accordingly, we conduct an evaluation of M_{selected} on another popular instruction following benchmark that complies with this format, IFEVAL (Zhou et al., 2023b). IFEVAL defines its own metrics, prompt-level and instruction-level accuracy, to measure how well a model response covers all the requirements delineated by each prompts and ultimately the test instruction. As in our previous evaluation with AlpacaEval and OpenLLM, we compare the performance of M_{selected} and M_{random} on this benchmark.

Findings from IFEVAL We include complete results on IFEVAL in the Appendix (Figure 10), where we observe similarly competitive performance from M_{random}; Here, we highlight another interesting observation derived through this benchmark: In Figure 4, we show the correlation between the Win-Rates for M_{selected} and their IFEVAL accuracy 4. The performance trends on both benchmarks appear very weakly correlated for our lowest budget, and show almost negative correlation after scaling M_{selected} to the larget budget. This is particularly concerning as both benchmarks are widely used as indicators of instruction follow-

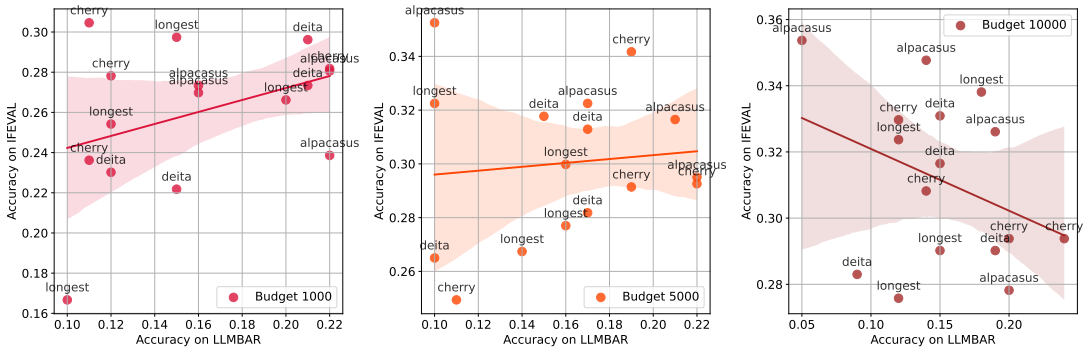

Figure 4: Mean Instruct-Level Accuracy of M_{selected} on IFEVAL versus Win-Rate on ALPACAEVAL: The correlation between Win-Rate and IFEVAL is entirely non-existent or weakly correlated at best. As budget increases these also appear to diverge: as performance drops on Win-Rate as IFEVAL accuracy improves. (§4.2 for further details.)

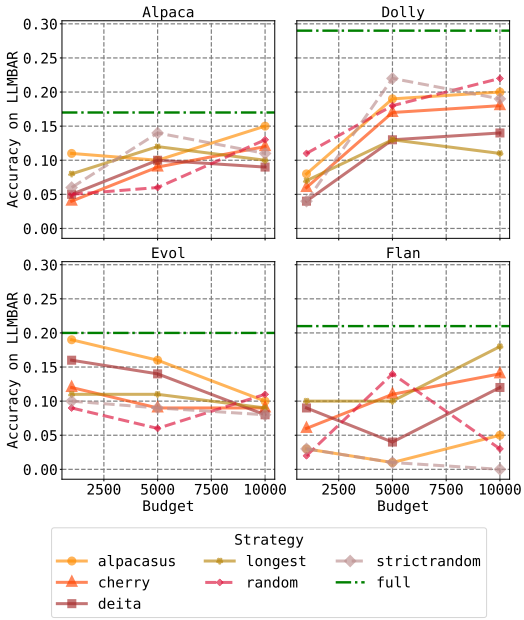

Figure 5: Performance on LLMBAR: Both M_{random} and M_{selected} consistently underperform $M_{\text{full-dataset}}$.

ing capabilities and hence, at least by definition it is hard to pick the conclusions of one over the other. The practical implication of this correlation is observed when these setups disagree on what the most appropriate selection strategy for a setup is. In this case for instance, we see that a lower budget (Figure 4(, we can claim with reasonable confidence that S_{deita} would give high performance across both benchmarks but as budget scales the trade-off between the performances on both benchmarks increases significantly making it hard to conclude which strategy has higher utility. We also observe similarly poor correlations between the performance trends of selection strategies when we do a pair wise comparison between other studied benchmarks like OPENLLM and LLMBAR (Figure 11).

To demonstrate this more concretely, we conduct a final evaluation on another instruction following benchmark, LLMBAR.

Findings from LLMBAR In Figure 5, we observe that both M_{selected} and M_{random} perform poorly on this benchmark. Interestingly, unlike all other benchmarks we study where $M_{\text{full-dataset}}$ are either comparable in performance or even underperform M_{selected}, on LLMBAR we clearly see consistent performance improvement when the model is trained on the entire data. This result, hence, sits in complete contrast to all other benchmark evaluations as it exposes another facet of evaluation where selection is not advantageous at all.

> ***Takeaways*** Benchmarks do not show agreement on the performance trends of selection strategies (§B.2). Further, choosing representative tasks that are aligned with a subjective measure of instruction following can significantly alter the observed performance trends (seen through §B.2). In such a case, it seems more useful to focus on data selection when we have prior objectives to optimize for as in test-distribution or task-specific selection.

4.3 Cost of Instruction Data Selection is Non-Trivial when compared to the cost of Tuning on the Entire Data

A strong motivation for designing instruction selection strategies, and more broadly, data selection strategies draws from the need to train competitive models efficiently, both in terms of time and resource consumption. While the advantages towards this goal are more explicitly observed when

Dataset	Samples (as multiples of 1k)	Alpacasus	Cherry		DEITA		Entire Dataset	
Costing Categories		API Inference Cost (USD)	Rent Time (min)	Rent Cost (USD)	Rent Time (min)	Rent Cost (USD)	Rent Time (min)	Rent Cost (USD)
EVOL	200	50	3290	427	1000	130	1620	216
ALPACA	52	12.66	855	111.15	260	33.8	120	15.6
DOLLY	15	3.7	246.75	32.07	75	9.975	40	5.2
FLAN	88	21.46	1447.6	188.2	440	57.2	220	28.6

Table 2: Random and Longest incur negligible time and compute cost on our setups and hence, they are not included here. For all other strategies, the effective cost of data selection is non-trivial in comparison to training on the full-dataset. In three out of four strategies, it is possible to overshoot the cost of finetuning on the full dataset.

source datasets are very large (pretraining datasets of the order of billions of tokens), instruction tuning datasets are typically much smaller in magnitude and thus the efficiency gains of selection can be less obvious to gauge. Accordingly, we evaluate if the proposed selection strategies *consistently* provide this intended benefit by comparing the effective cost of selection against the performance of M_{selected} and $M_{\text{full-dataset}}$.

Setup We compute the Cost of Selection as a product of the per-hour cost to user for renting a fixed compute infrastructure and the wall clock run time for running the selection for that strategy end-end. §B describes the full details of this computation including the wall clock time of running each selection (Table 5), while the total cost to user in summarized in Table 2. In Figure 6, we plot the cost of selection per dataset compared to the performances of M_{selected} on IFEVAL (all budgets are included in §B.3 in the Appendix).

Finding The effective cost of selecting data can often overshoot the cost of finetuning $M_{\text{full-dataset}}$ in some cases and the gains achieved through selection are marginal in comparison to the additional cost expended at carrying out the selection. While one potential cause of this could be the lack of more aggressive strategy-specific hyperparameter tuning, that is impractical for multiple reasons; For one, hyperparameter tuning in this space involves tuning for strategy dependent parameters such as the similarity threshold, λ in S_{deita}, the number of pre-experienced samples in S_{cherry}, etc. in addition to traditional model training parameters like learning rate, scheduler and batch size. Jointly optimizing for both these class of hyperparameters can significantly bloat the set of combinations to explore for hyperparameter optimization thus significantly increasing the cost of tuning. Secondly, under a practical setup where an NLP pracitioner expects to choose the best selection strategy *amongst* several candidate strategies, a hyperparameter sweep for each candidate strategy would mandate tuning **all the strategies being examined**. From 2, this would imply summing the cost estimates across any row. We can clearly see that such an estimate would quickly overshoot the cost of full finetuning for any strategy.

One interesting and consistent observation from this cost-benefit analysis is the surprising performance gain shown by M_{selected} over $M_{\text{full-dataset}}$. Both, M_{selected} and M_{random} often beat the $M_{\text{full-dataset}}$ across several experimental configurations. While some of these gains may be attributed to the lack of hyperparameter tuning for $M_{\text{full-dataset}}$, supporting evidence from literature in the space of instruction data selection ((Qin et al., 2024b; Zhou et al., 2023a; Zhao et al., 2024; Ge et al., 2024) does imply that training on selected data can be beneficial (even though not necessarily cost effective). Empirically, this is also visible from the performance of our $S_{\text{strictrandom}}$ baselines: through the majority of our evaluation, the $S_{\text{strictrandom}}$ baselines underperform all other strategies indicating that systematically excluding datapoints that are selected by selection strategies definitely harms performance.

Takeaways Models can be trained to follow user instruction with relatively small subsets of data. It remains unclear though, if this selection is significantly more performant and cost-effective if carried out using selection strategies other than naive random sampling.

5 Discussion and Conclusion

This work demonstrates that selection strategies are not consistently competitive across setups and **this puts them at a risk of falling short of even ran-**

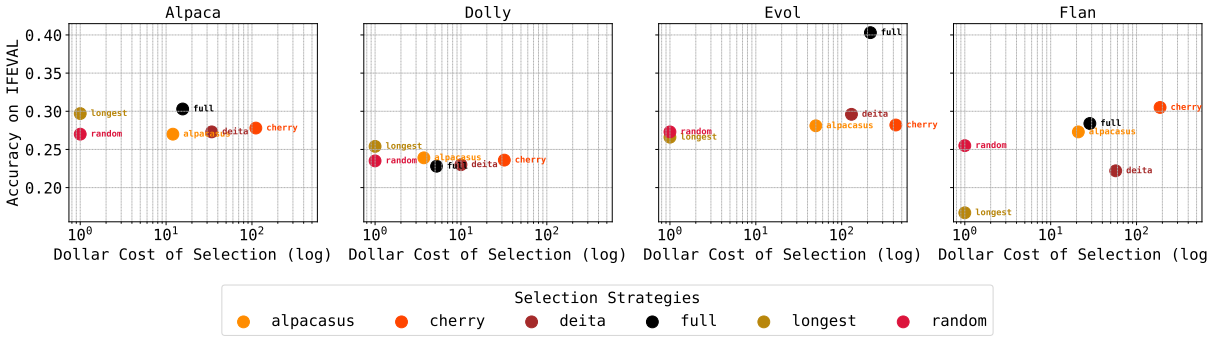

Figure 6: Cost Versus Performance Trade-Off at Selection Budget of 1000: Rather than reporting the average performance of random - we report the **lowest** performance amongst all our random trials to give the most pessimistic estimate of the performance of our random baseline.

dom sampling under a wider range of instruction tuning datasets, selection budgets and benchmarks. We also highlight that selection cost often surpasses the cost of full fine-tuning, without consistently delivering proportional benefits.

Random Baselines offer consistency, reasonable and cost-effective performance: Our conclusions on the performance of random baselines in this setting can be considered aligned to contemporary work demonstrating the unreasonable effectiveness of random baselines in several other domains; Yauney and Mimno (2024) discuss the significant competence of maximum expectancy random baselines in in-context learning by highlighting how standard random baselines may be severely underestimated on validation sets that are smaller in size. Similarly, Lu et al. (2023) find that random baselines for prompt optimization can prove to be effective separators for prompt-style classification even challenging the assumptions that mandate task relevance and human readability in such tasks. Accordingly, our construction of random baselines must improve at scale to get a realistic calibration of the performance of our proposed methods.

Instruction selection performance claims do not stand agnostic to the adopted experimental configurations This dependence significantly harms their ease of adoption. Conversely, proposed instruction selection strategies may be more usable to NLP practioners if the efficacy of methods are tested across a wider range of experimental parameters (more budgets, datasets of differing distributions, etc.).

The Limits of *Selective* Training in *General*-Purpose Instruction Following General purpose instruction following is an unbounded recall problem as it can involve a fairly vast set of capabilities depending upon the context. There isn't a clear consensus on what are the sufficient conditions for claiming competence in general purpose instruction following: Models trained on selected data may show performance improvement against few limited facets but degrade it on unseen ones. Even using automatic metrics that act as proxies for human judgement seems unreliable as these metrics are also fallible (Zheng et al., 2024) and susceptible to bias (Panickssery et al., 2024). Finally, since instruction following has evolving expectations, standardizing the choice of evaluation through human corroboration may only provide a stopgap solution (van der Meer et al., 2024; Shen et al., 2023). As the complexity of such evaluation can be simplified for known test distributions, selection design effort may be more reliable and consistent in such fields.

6 Limitations

Since our work's goal is study the competency of models on a highly subjective goal, general purpose instruction following, conducting a comprehensive human evaluation to support our conclusions was not feasible. Our work also does not address other attributes that selection strategies differ by: including but not limited to the use of different base models, their impact on tuning strategies (like preference optimization) and alignment objectives.

7 Ethics Statement

This work highlights the potential of availing negative utility in the field of instruction data selection. Through the evidence in this work, we encourage a more conscious allocation of compute and dollar cost to reduce unnecessary computational over-

heads. Our code base and training logs (to validate wall clock times) will be released under the MIT License.

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A Appendix

A.1 Hyperparameter Configurations

We do our evaluations across 3 setups, trying to maximize the coverage of training setups that have been adopted by the strategies we reproduce. Additionally, we carried out one evaluation with LORA (Hu et al., 2021) to test if some weak correlation about the performance trends of selection strategies could be gleaned from low-rank finetuned models. The results for that evaluation are shown in §7. We present results from the hyperparameter configurations that matches the MMLU performance of reported for each strategy. The standard deviation with reported numbers along with confidence values for our hyperparameter runs across MMLU are provided in Table 4. Since the work we study did not report IFEVAL, LLMBAR or ALPACAEVAL length-controlled win-rates - we were only able to utilize MMLU numbers (reported by all) as our sanity check for replication.

To replicate S_{cherry}, we used the code open-sourced by the authors on Github, making minor adaptations to add support for new datasets. Following the default setup advised in (Li et al., 2023a), we train our pre-experienced model for 1000 samples using the training configurations

specified by the authors. For S_{deita} also, we adopt the code opensourced by the authors on Github. We use the Mistral-7B-v0.1 for embedding generation, along with the quality scorer. For our similarity metrics, we used the same distance metric: cosine but different thresholds as keeping the default threshold led to an underflow for few of the models. We carry out inference using VLLM to improve efficiency of our inference in S_{deita}.

B Detailed Cost Estimation Across Data Selection Budgets

All our estimates are provided assuming the following infrastructure: 8 A6000s, 128 CPUs provided by Google Cloud Estimator. The Dollar Cost of renting our infrastructure per hour is about 8 USD.

A detailed breakdown of the costs associated with each step of the selection is provided in Table 5. Note that the cost of selection doesn't vary significantly with the change in the selection budget as the entire dataset needs to be sorted in accordance with the strategy guided metric, irrespective of the final budget.

B.1 Estimating Performance Using Cost-Effective Proxies

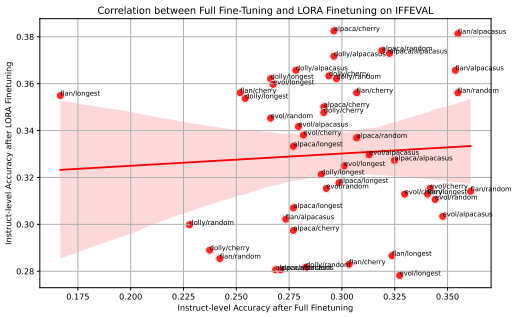


Figure 7: There isn't any observable correlation between the performance of models finetuned with and without LORA across our setups indicating that we cannot reliably predict the optimal selection strategy on a faster, cost-effective parameter efficient setup.

While it is not possible to largely modify the cost of a selection strategy, it might be possible to offset the cost of finetuning the models on subsets generated via different selection strategies through parameter efficient techniques. If such trends are correlated with the performance of the selected data on the full variant of the model, NLP practioners can potentially design a set of relatively low-cost experiments to rapidly identify the optimal selection strategy to further carry out their selection. Recent work like (Xia et al., 2024), even leverage such correlation to achieve great efficiency in task-specific instruction selection. For preliminary experimentation, we rerun all our experiments with the modification of including LORA modules in our finetuning. This reduces the memory footprint of training by to only 0.0038 times of the memory footprint of full finetuning along with faster training by half of its full-finetuning counterpart. In 7 we plot the correlation between the instruct-level-accuracy on IFEVAL for models trained with and without LORA. However, we don't find any reasonable correlation between these performances highlighlting a need to identify cost-effective methods of predicting the suitability of a custom budget and source distribution to a given selection strategy.

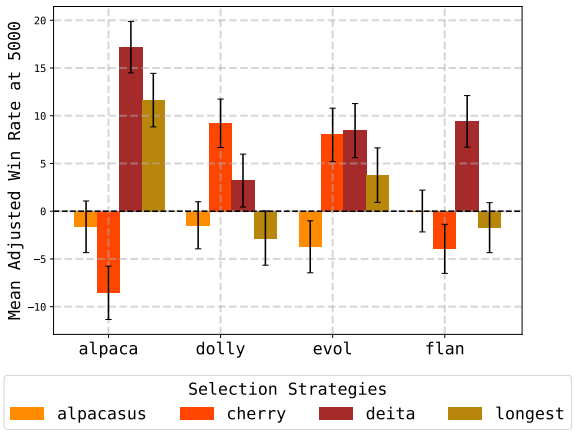


Figure 8: AlpacaEval Length Controlled Win Rate at 5K: Models do not show a consistent trend in performance when scaled from 1000 samples or across datasets.

B.2 Benchmark Evaluations for All Configurations

B.3 Correlation between all Benchmarks

B.4 Cost Versus Performance Trade-Offs for All Budgets

Setup	LR	Optimizer	BS	MSL	Epochs	Warmup Ratio	LR Scheduler
Set 2	2e-5	Adam	128	512	3	0.03	Cosine
Set 1	2e-5	Adam	128	512	3	0.03	Linear
Set 3	1e-5	Adam	128	512	3	0.03	Linear
Set 4 [LORA]	2e-5	Adam	128	1024	5	0.3	Linear

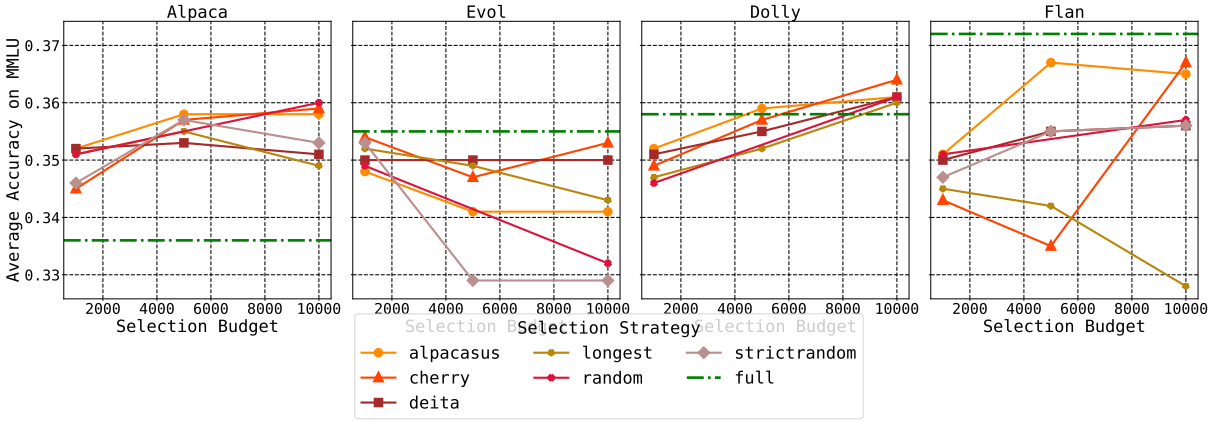
Table 3: Hyperparameter Configurations for our experimental setup

Dataset	Strategy	MMLU (Set 1)	MMLU (Set 2)	MMLU (Set 3)	Standard Deviation	Confidence Interval
alpaca	alpacasus	0.351	0.352	0.345	0.004	0.012
evol	cherry	0.354	0.354	0.348	0.003	0.011
evol	longest	0.352	0.352	0.349	0.002	0.005
flan	alpacasus	0.351	0.351	0.347	0.002	0.007
dolly	alpacasus	0.352	0.352	0.347	0.003	0.009
alpaca	longest	0.353	0.351	0.345	0.004	0.013
evol	alpacasus	0.348	0.348	0.349	0.001	0.002
flan	cherry	0.344	0.343	0.344	0.001	0.002
dolly	longest	0.348	0.347	0.345	0.002	0.005
dolly	cherry	0.348	0.349	0.345	0.002	0.006
alpaca	cherry	0.346	0.345	0.344	0.001	0.003
flan	longest	0.345	0.345	0.345	0.000	0.000

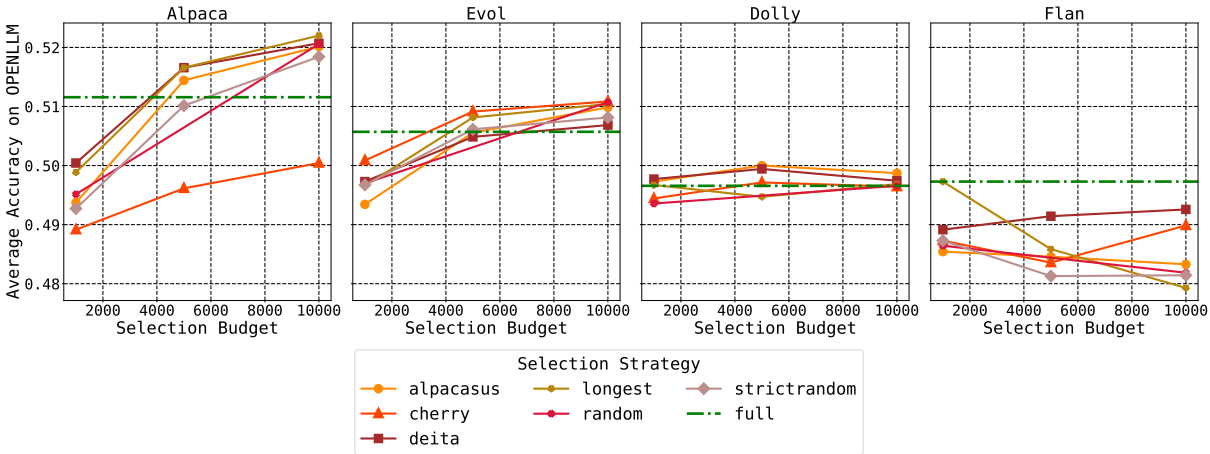
Table 4: MMLU Values for Budget 1000 across all hyperparameter setups. Since we saw the highest (relative) correlation between all benchmarks at this budget, we chose the final hyperparameter set based on this budget's value.

S_{strategy}	**Wall Clock Time on Rented Infrastructure (hr)**
$S_{\text{alpacasus}}$	0
S_{longest}	Total Time per 1000 samples: 1 minute
S_{cherry}	1.457 minutes minutes for 1000 samples embedding construction + 7 mins for training pre-experienced model on 1000 samples (One-time cost, so ignored) + 15 minutes for computing token loss over 1000 samples Total Time per 1000 samples: 16.45 minutes
S_{deita}	2 minutes for 1000 samples for embedding construction (Mistral 7B) + 120 minutes for scoring 1000 samples w/o VLLM 2 minutes for scoring 1000 samples with VLLM + 1[3] minute *at least* for filtering 1000 sample. Total Time per 1000 samples: 5 minutes

Table 5: Wall Clock Times for Each Selection Strategy: We offset the time of computatation we subsample 100K samples from EVOL and then select samples from that subset.



(a) M_{random}versus M_{selected}on MMLU



(b) M_{random}versus M_{selected}on OPENLLM

Figure 9: Comparison of $M_{\text{random}}$and M_{selected}on different models

Paper	**Reported Value**	**Our Value (Budget - 1k)**
Alpagasus at 9K	36.93	35.2
Cherry at 3.5K	36.51	35.2
Cherry at 7K	33.08	35.2

Table 6: Reported Performance versus replicated performance; While S_{deita}also used the same base model as us, they use a 13B parameter model and hence, we do not compare with their numbers. Set 2 was the closest in evaluation to these numbers so we chose Set 2 for reporting our results.

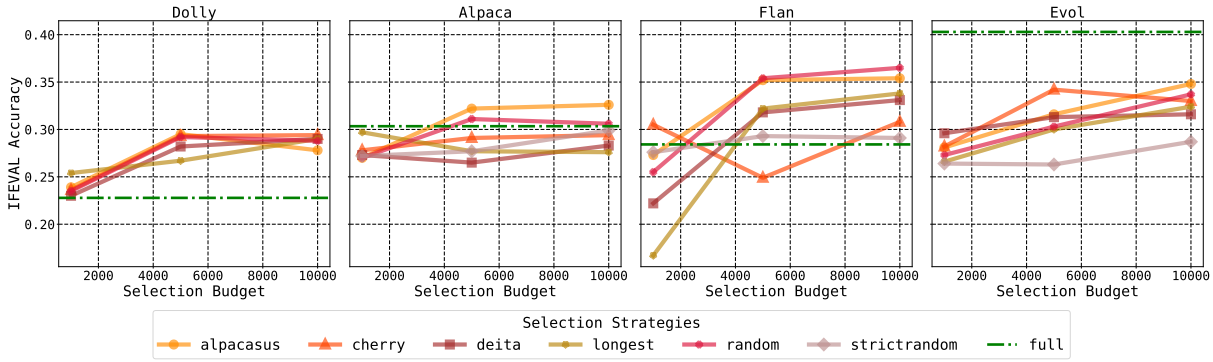


Figure 10: Performance Comparison between $M_{\text{random}}$and M_{selected}on IFEVAL: We report the average of all random runs (both random and strict random) for a particular configuration in any result.

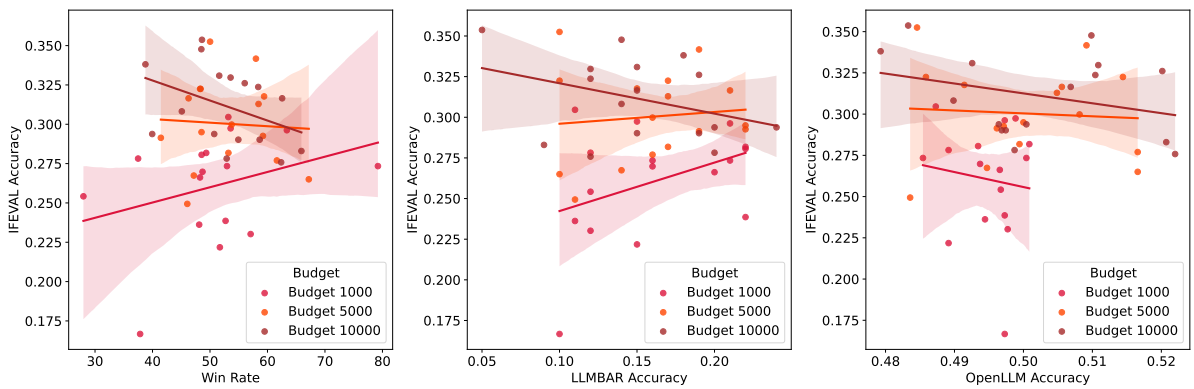


Figure 11: Correlation between Benchmarks: All benchmarks show poor correlation, especially at larger budgets (almost negatively correlated performances)

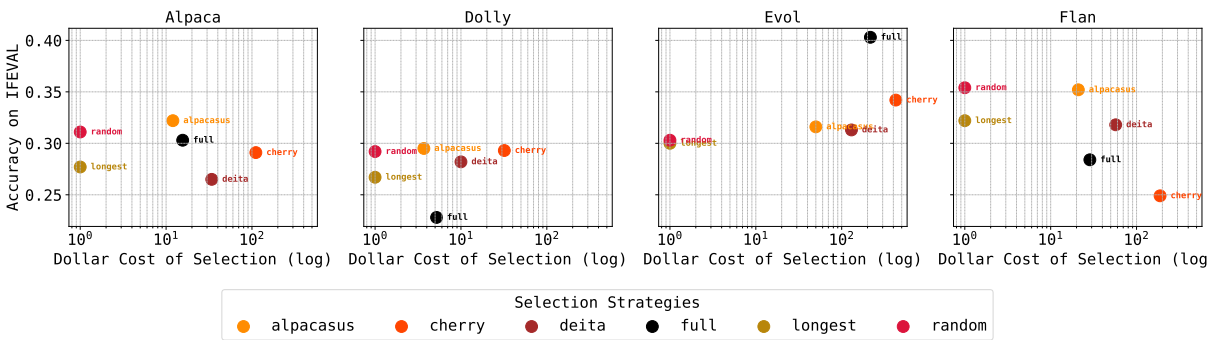


Figure 12: Cost Versus Performance Comparison at 5K budget: We highlight that random performs competitively across most setups

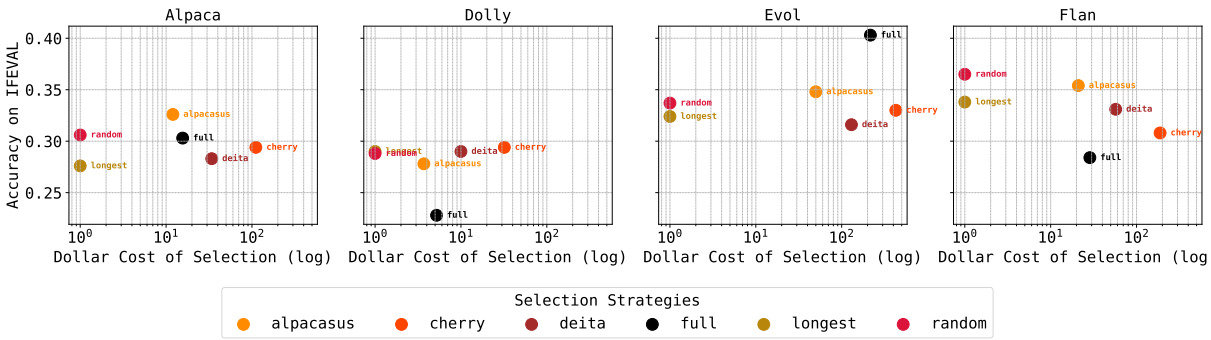


Figure 13: Cost Versus Performance Comparison at 10K:We highlight that random performs competitively across most setups