FEAR: A Simple Lightweight Method to Rank Architectures

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Abstract

The fundamental problem in Neural Architecture Search (NAS) is to efficiently find high-performing architectures from a given search space. We propose a simple but powerful method which we call FEAR, for ranking architectures in any search space. FEAR leverages the viewpoint that neural networks are powerful non-linear feature extractors. First, we train different architectures in the search space to the same training or validation error. Then, we compare the usefulness of the features extracted by each architecture. We do so with a quick training keeping most of the architecture frozen. This gives fast estimates of the relative performance. We validate FEAR on Natsbench topology search space on three different datasets against competing baselines and show strong ranking correlation especially compared to recently proposed zero-cost methods. FEAR particularly excels at ranking high-performance architectures in the search space. When used in the inner loop of discrete search algorithms like random search, FEAR can cut down the search time by $\approx 2.4X$ without losing accuracy. We additionally empirically study very recently proposed zero-cost measures for ranking and find that they breakdown in ranking performance as training proceeds and also that data-agnostic ranking scores which ignore the dataset do not generalize across dissimilar datasets.
1 Introduction

Neural Architecture Search (NAS) [Elsken et al., 2019, Ren et al., 2021] is a sub-field of automatic machine learning (AutoML) [He et al., 2021, Hutter et al., 2018] where the aim is for algorithms to search for high-performing architectures instead of humans manually trying out many possibilities. Given the rapid advances in differentiable operator types and usual application-dependent open questions of number of layers, channels, input resolution, etc, this leads to an explosion of combinatorial choices in architecture design space. Indeed, even ubiquitously used search spaces like the DARTS [Liu et al., 2019] search space contain approximately $10^{18}$ architectures [Siems et al., 2020].

The fundamental problem in NAS is to search this combinatorially exploding space of architectures as efficiently as possible.

Current NAS methods conduct this search in one of mainly two different ways: 1. **Discrete methods** use methods that sample architectures from a search space, estimate the final performance of the architectures by training them partially and then update the sampling rule based on that estimate [Zoph and Le, 2017, Liu et al., 2020]. These methods can be notoriously computationally expensive due to the cost of training each sampled architecture individually [White et al., 2019, 2020a]. 2. **One-shot methods** on the other hand train a single large graph (colloquially known as ‘supergraph’) that contains all possible architectures in one single graph by sharing weights amongst common edges in architectures. Even though weight-sharing has many issues like optimization gap between training and evaluation and shallow sub-graphs training faster [Xie et al., 2020, Wang et al., 2021, Shu et al., 2020, Zela et al., 2020], by training a single graph the cost of gradient computation via backpropagation can be amortized over exponentially many sub-graphs. Popular approaches include bilevel optimization [Liu et al., 2019], single level optimization approaches [Chang et al., 2019] utilizing the Gumbel-Softmax trick [Jang et al., 2017, Maddison et al., 2017] or even direct optimization without architecture weights [Wang et al., 2021]. Note that for both with and without weight-sharing one can use various classes of techniques. For example RL-based [Pham et al., 2018], evolutionary search [Liu et al., 2020], random search [Li and Talwalkar, 2020], local search [White et al., 2020a] and variants of Bayesian optimization [White et al., 2019].

We focus on the evaluation phase of discrete methods where it is often an ad-hoc choice on how and for how long to evaluate each sampled architecture for a given dataset. As Li and Talwalkar [2020] note, the partial training
phase can often be critical in deciding how well a method performs. Crucially, they note that using partial training or validation error after many epochs of training can still not get the correct final rank of architectures. We propose a simple but powerful architecture ranking methodology that enables fast architecture ranking by leveraging the fact that neural networks are powerful feature extractors and the power of an architecture is dependent on how effective it is at extracting useful features from the inputs for the given task. We term this ‘recipe’ (see Figure 1 for an overview) for ranking architectures FEAR (FEATure-extraction Ranking). At a high level, first trains any architecture regularly till the architecture attains a specified training or validation accuracy threshold in the first stage. In the second stage, most of the architecture is frozen and the final few layers continue training for a bit longer. Architectures can be relatively ranked via their training or validation error after these two stages. We leverage insights in training dynamics of neural networks [Kornblith et al., 2019, Raghu et al., 2017] that show that early layers train very fast and become stable within a few epochs but later layers spend most of their time learning to make decisions from the features extracted by the earlier layers. A crucial aspect of our procedure is that we train architectures up to a specified threshold accuracy instead of committing apriori to fixed number of epochs. This exposes first order dynamics in neural network training where invariably (empirically) weak architectures take much longer to reach threshold accuracy as opposed to stronger architectures (see Figure 2). Such first order training dynamics can be exploited in any sampling-based discrete NAS algorithm. FEAR is discussed at length in Section 3.

We have two main contributions in this work:

- We propose a simple fast architecture evaluation method named FEAR and validate it on a variety of datasets on the Natsbench topological space benchmark against competing baselines.¹

- We also empirically find that a number of very recently proposed lightweight ranking measures [Abdelfattah et al., 2021, Mellor et al., 2021] degrade in ranking performance as network training progresses and that data-agnostic ranking measures don’t generalize across datasets. The performance of an architecture is a function of both the topology

¹Reproducible implementation of all experiments is available at https://github.com/microsoft/archai/tree/fear_ranking [Shah and Dey, 2020] under MIT license.
Figure 1: Overview of FEAR which first trains a fast but shallow learner to get a reasonable training/validation error threshold and then trains the architecture in a two-stage procedure. In the first stage the architecture is regularly trained until it achieves threshold accuracy. In the second stage most of the partially trained architecture from the first stage is frozen and training continues for a few more epochs. All candidate architectures can then be ranked by final training or validation accuracy obtained via this two stage procedure.

and the dataset (in addition to the training pipeline).

2 Related Work

NAS research has received a lot of attention recently. We refer the reader to continuously updated excellent surveys [Elsken et al., 2019, Ren et al., 2021] and a near-exhaustive list of papers at [Deng and Lindauer, 2016] for an overview of the field. Here we discuss the works that are directly relevant to fast evaluation and ranking of architectures.

**Architecture Performance Prediction:** Baker et al. [2017] propose training regressors which take in the architecture, training hyperparameters and the first few validation accuracies as features and try to predict the final validation accuracy. The proposed method has a “burn-in” phase where a sampled few architectures are first fully trained to gather data for training the regressor. This regressor is then utilized in the rest of the pipeline. Similarly Rorabaugh et al. [2021] propose fitting curves to the performance numbers of the first few training iterations and extrapolate to later accuracy values. White et al. [2019] study neural network performance prediction in the context of Bayesian Optimization. Orthogonally, White et al. [2020b] conduct an extensive study of architecture encodings for common NAS algorithm
subroutines including performance prediction which sheds light on the pros and cons of certain featurizations for this task. **FEAR** is orthogonal to the above body of work on architecture performance prediction and in fact can be used for further speeding up the performance prediction modules as one doesn’t have to train the architectures fully.

**Lightweight Architecture Evaluation:** Zhou et al. [2020] search for combinations of reduced input image resolution, fewer epochs, and number of stem channels to find computationally cheap proxies for evaluating architectures while keeping their relative ranks the same. They find an optimal configuration of resolution, epochs and number of channels on a bag of 50 models. They term their method as EcoNAS. Abdelfattah et al. [2021] note that the configuration found by EcoNAS suffers from degrading performance when evaluated on all 15625 models in Nasbench-201 CIFAR10 dataset [Dong and Yang, 2020]. Abdelfattah et al. [2021] conduct their own search and find a different configuration that works better on Nasbench201 CIFAR10 dataset. They caution that such proxies clearly don’t work on different search spaces even when the dataset and task are the same and also the importance of measuring actual wall clock run times as reduced flops often don’t translate into actual time savings due to different ways of accessing memory.

Cao et al. [2021] propose a lightweight architecture evaluation method based on the viewpoint that neural networks are rich feature extractors which are utilized by the last linear classifier layer. They propose training an architecture for a few epochs and recording the features per datapoint over a window of ‘k’ last epochs. A linear classifier is trained on each of these feature histories to produce ‘k’ linear classifiers. An ensemble of these ‘k’ classifiers is used to predict the class membership of each datapoint. The average error over all datapoints is used to rank architectures. This method is closest in spirit to **FEAR** but differs in a number of important ways. First of all maintaining feature histories for every datapoint is expensive in terms of memory. **FEAR** avoids this step by simply freezing most of the architecture and continuing training (which is much cheaper since gradients have to be computed only for a small part of the architecture). More critically **FEAR** does not apriori fix the number of epochs. Instead it trains the network in the first stage to a prespecified threshold training or validation accuracy and then freezes most of the network in the second stage. We describe in detail in Section 3 how to organically set this threshold in a task-dataset dependent
manner.

**Trainingless Proxies:** Mellor et al. [2021] propose a trainingless method for ranking architectures based on the KL divergence between an uncorrelated Gaussian distribution and the correlation matrix of local linear operators (for networks with ReLU activation functions) associated with every input data point. If the correlation between such local linear maps is low then the network should be able to model each data point well. Since this score can be computed with just a small sample of the dataset (typically a single minibatch), this takes negligible compute and time. In very recent work Abdelfattah et al. [2021] thoroughly empirically evaluate this trainingless method which they term as jacob\_cov along with an entire family of pruning-at-initialization schemes which they convert to trainingless architecture ranking methods by simply summing up the saliency scores at each weight of the architecture. The particular methods they evaluate include snip [Lee et al., 2019], grasp [Wang et al., 2020a], synflow [Tanaka et al., 2020] and fisher [Turner et al., 2020] in addition to other natural baselines like grad\_norm which is the sum of Euclidean norm of the gradients using a single minibatch of training data. On Nasbench-201, on all three datasets (CIFAR10, CIFAR100, ImageNet16-120) they find that synflow score performed the best with relatively high rank correlations with full training of architectures. jacob\_cov was second best. A majority vote amongst synflow , jacob\_cov and snip termed as vote performs the best. They also note that these trainingless methods don’t work satisfactorily when evaluated on other search spaces like Nasbench-101 [Ying et al., 2019].

Note that FEAR is not a trainingless method and does use more computation than the trainingless proxies outlined above. But we empirically show in Section 4 that FEAR outperforms these proxy measures as well as the natural baselines of reduced number of training epochs. Furthermore in the process of experimentation we identify some curious properties of trainingless proxies such as degradation in performance as the network trains more which is counterintuitive and also the curious phenomenon of synflow-based ranking (which is a data-agnostic scoring mechanism) in particular not generalizing across datasets. This supports the intuition that architecture performance is not an intrinsic property of just its topology (and training procedure) but crucially also dependent on the task and dataset at hand. This has been empirically validated by the very recent work of Tuggener et al. [2021] who
show that architectures which perform well on ImageNet [Deng et al., 2009] do not necessarily perform as well on other datasets. In fact on some datasets their ranks are negatively correlated with ImageNet ranks. This further suggests that a data-agnostic scoring mechanism such as synflow may not work well at ranking architectures.

3 Approach

Here we describe FEAR in more detail. Figure 1 shows a high level schematic of the approach.

Finding training accuracy threshold: FEAR first trains a fast but shallow learner on the dataset and task of choice to learn a training or validation accuracy threshold. For example, for the task of image classification one can use a number of fast shallow learners like random forest, linear classifier, with handcrafted visual features such as Histogram-of-Gradients (HoG) [Dalal and Triggs, 2005] or random features such as random kitchen sink [Rahimi and Recht, 2008]2. We emphasize that the role of this threshold is to be both non-trivial yet not too difficult to beat with a neural network architecture. We explain the intuition behind this choice below.

Stage 1: Regular training till threshold accuracy: FEAR then trains the candidate architecture till it achieves this threshold accuracy.

Stage 2: Using architecture as feature extractor: FEAR then freezes most of the layers of the architecture other than the last few layers and trains it for a few more steps. This freezing has the advantage of being several times faster per step than training the entire architecture as gradients don’t have to be computed for most of the layers. This stage essentially treats the network as a feature extractor and trains a relatively shallow network utilizing these features for a few more epochs. A pool of candidate architectures are then ranked by their final training or validation accuracies on the dataset under consideration. Intuitively, FEAR ranks architectures on their ability to extract useful features from inputs.

2See [Mikolajczyk and Schmid, 2005] for a survey of handcrafted visual features.
A question that may naturally arise is by cutting off training of most of the layers at a relatively early stage of training, are we not hurting the architecture’s ability to potentially distinguish itself at feature extraction? Raghu et al. [2017] and Kornblith et al. [2019] dive deep into the training dynamics of neural networks and show that networks train ‘bottom-up’ where the bottom layers (near the input) train quite fast early-on in training and become stable. As training progresses these bottom layers rarely change their representation and mostly the top layers change to learn the decision-making rules using the bottom layers as rich feature extractors. **FEAR** leverages this insight by first training all layers for a few epochs and then freezing most of the layers but the last few layers.

**Role of training till threshold accuracy:** We would like to re-emphasize that **FEAR** does not fix the number of epochs apriori. Instead it trains the architecture until a threshold accuracy has been reached. This has a number of advantages. First, it makes architectures **comparable** to each other and makes sure that every architecture gets ample time to learn the best features it can for the task. Secondly, it exposes first-order dynamics of training i.e. weak architectures empirically take longer time to reach the same training or validation error compared to the stronger architectures. See Figure 2 where we plot for three datasets the time taken by 1000 architectures to reach a threshold accuracy (x-axis) against the final test error (y-axis). Invariably, we find that architectures which go on to attain good final test accuracy achieve threshold training accuracy much faster than weaker ones. This alone is not enough for good ranking as some weaker architectures can achieve the threshold accuracy fast as well and hence ranking by training a bit longer to resolve the power of features is necessary. Also note that there are no architectures that train slowly but go on to achieve good final test accuracy after full training (‘late-bloomers’, these would have been on the upper-right hand part of the plots). This effect can be utilized to early-stop evaluation of candidates that take much longer than the fastest architecture encountered to reach threshold accuracy. This is in fact crucial to obtain large speedup when combining our ranking method with standard discrete neural architecture search techniques.

**Motivation:** The motivation for our method comes from the emerging theoretical understanding of gradient learning on neural networks. In a
Figure 2: Time to reach threshold training accuracy (x-axis) vs. final test accuracy on 1000 uniformly sampled architectures on Natsbench. They show a clear relationship where ultimately worse performing architectures take longer time to reach threshold accuracy than stronger ones.

number of works (e.g., [Hu et al., 2020, Chizat and Bach, 2020, Nakkiran et al., 2019, Allen-Zhu and Li, 2020]) it has been observed that at first training happens in the so-called “neural tangent kernel” (NTK) [Jacot et al., 2018] regime, where the network basically uses its initialization as a sort of kernel embedding and performs a kernel regression. Our key hypothesis is that this phase should stop a bit before reaching the fixed threshold accuracy, since this threshold has been obtained with a good kernel method (or something slightly more powerful like a random forest model). To put it differently, in our method, when we stop the training after reaching the fixed threshold accuracy, it should be that the network has already escaped the NTK regime and is currently actively training the features (second phase of learning). Our second (empirical) hypothesis is that the quality of the features learned in the early part of this second phase is predictive of the final quality of the network. We measure the quality of the learned features via the freezing technique, whose extreme case is to only continue training the final layer (i.e., train a linear model on top of the current embedding).

4 Experiments

Search space: We uniformly randomly sample 1000 architectures from the 15625 architectures of the topology search space of the Natsbench [Dong et al.,
and hold them constant for all following experiments. The topology search space is similar to that used in DARTS [Liu et al., 2019]. All cells have the same topology. Edges are different operator choices (the set of operators are zeroize, skip connection, $1 \times 1$ convolution, $3 \times 3$ convolution and $3 \times 3$ average pooling). Nodes are tensors. Each cell has 4 nodes. There are three stages in the outer macro skeleton with a downsampling operation after each stage. Each cell is repeated 5 times in each stage. There are respectively 16, 32 and 64 channels in each stage. See Figure 1 in [Dong et al., 2021] for a visualization of the search space. Natsbench topology search space has trained each architecture on CIFAR10, CIFAR100 [Krizhevsky et al., 2014] and ImageNet16-120 [Chrabaszcz et al., 2017] image classification datasets.

4.1 Performance criteria

Spearman’s rank correlation vs. evaluation wall-clock time: We report performance of FEAR and baselines by first binning architectures into several buckets of increasing size. For example Top 10% in Figure 6 shows the average wall-clock time taken by any method (x-axis) vs. Spearman’s rank correlation [Spearman, 1904] of the method with the groundtruth rank of architectures (by test accuracy) after full final training procedure over the top 10% of architectures. Similarly the bin of Top 20% architectures includes top 20% of candidates and so on. We break-up the performance of methods over such cumulative bins to highlight how methods perform in discriminating amongst high-performing candidates and not just over the entire population. It is crucial for any reduced-computation proxy ranking method to hone-in on good ones and not just the entire population.

Percentage overlap with groundtruth ranking vs. evaluation wall-clock time: While Spearman’s rank correlation over cumulative bins of candidates by groundtruth performance is useful for showing the ability of methods to discriminate amongst the top x% of architectures, it is also important to evaluate what percentage of architectures are common between the top x% of groundtruth architectures and the architectures that are ranked

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3Note that Natsbench topology search space is the same as Nasbench-201 [Dong and Yang, 2020].

4Spearman’s rank correlation is between $(-1, 1)$ with 1 implying perfect correlation and $-1$ anti-correlation of the ranks of candidate architectures.
by a method. When ranking the entire population of candidates, this metric evaluates if the high ranking architectures in groundtruth are also highly ranked by a method (and vice-versa). By definition this is a quantity between $(0, 1)$.

### 4.2 Baselines

**Regular training with reduced epochs: shortreg**  The most natural baseline is to compare rank correlation of FEAR against reduced epochs of training. Most NAS methods use a reduced number of training epochs [Li and Talwalkar, 2020] in the inner loop to decide the relative ranks of architectures as a proxy for final performance after undergoing the complete training procedure. We term this reduced training proxy as shortreg. We show that FEAR consistently outperforms the pareto-frontier of wall-clock time vs. Spearman rank correlation and the ratio of common architectures over cumulative bins of candidate architectures by groundtruth (test accuracy).

**Zero-cost Proxies:** As detailed in Section 2, in very recent work [Abdelfattah et al., 2021] a number of nearly negligible cost proxies for ranking architectures are presented. We evaluate these zero-cost proxies for ranking and observe a number of mysterious phenomena where such measures break-down across datasets or as networks are trained. This shows that such measures while exhibiting reasonable prima-facie performance on NAS benchmarks are not generalizing across datasets. We detail our investigation in Section 4.4.

**Reduced Resolution Proxies:** While reduced resolution proxies as proposed in [Zhou et al., 2020] and [Abdelfattah et al., 2021] can significantly speed up architecture evaluation, note that they are orthogonal to our approach as they equally speed-up both shortreg and FEAR. For use in production pipelines they should be combined with FEAR to get even more speedup.

**Training procedure hyperparameters and hardware:** In all our experiments we use the same hyperparameter settings as Natsbench, specifically cosine learning rate schedule with starting learning rate of 0.1, minimum learning rate of 0.0, SGD optimizer, decay of 0.0005 for both regular weights and batch-norm weights, momentum 0.9 and Nesterov enabled. For shortreg
baseline we run experiments with varying number of epochs and batch sizes 256, 512, 1024, 2048 to find a pareto-frontier of wall-clock time vs. Spearman’s correlation and common ratio. We especially investigate varying the batch size since that can have a large effect on wall-clock time. When architectures are evaluated by FEAR , for the second phase we froze the network up to cell113 out of 16 total cells for all architectures in the search space. This usually corresponds to $\approx 53\%$ of parameters. All experiments were conducted on Nvidia V100 GPUs with 16 GB of GPU memory.

4.3 Main Results

Figure 3: [Left] Average duration per architecture vs. Spearman’s correlation and [Right] average duration per architecture vs. common ratio over the top $x\%$ of the 1000 architectures sampled from Natsbench topological search space on CIFAR100. We also show the various zero-cost measures from Abdelfattah et al. [2021] in green. Recall that in Section 4.4 these measures will be shown to be non-robust to change of task as well as degradation when the training progresses.

Figure 3 shows the ranking efficiency of FEAR vs. shortreg on the uniform random sample of 1000 architectures on CIFAR100 up to the top 30% of architectures. For elucidation purposes we give out detailed versions in Appendix A.3 (figures 6, 7 and 8) on Natsbench CIFAR10, CIFAR100 and
ImageNet16-120 respectively. We sort architectures in descending order of test accuracy, and bin them cumulatively into top 10%, 20%, . . . bins. For each bin we report the two performance criteria detailed above of Spearman’s rank correlation and the common ratio of architectures in the global ranking. For CIFAR10 and CIFAR100 we find that FEAR consistently places above the pareto-frontier of shortreg especially at higher ranked architectures. This means that FEAR is able to both discriminate better amongst high-performing architectures (better Spearman’s correlation) with shorter wall-clock time as well as achieve global ranking which overlaps more with the groundtruth ranking of architectures using test accuracy. Note that as the bin increases to encompass the entire set of 1000 architectures (Top 100%), by construction, FEAR will start taking more time as low-performing architectures take more time to reach threshold accuracy (recall Figure 2 and associated explanation) and hence increase the total/average wall-clock time. In practice, this extra time for lower-performing architectures will not be paid since low-performing ones can be simply removed from consideration when they exceed some multiple of the fastest time so far by other architectures to achieve the same threshold accuracy. On ImageNet16-120 the gap between FEAR and shortreg is not as big but nevertheless it doesn’t degrade in performance below shortreg and over the high-performance bins is marginally better.

Since figure 3 and corresponding detailed versions 6 7 and 8 contain dense information, for ease of elucidation, in Table 2 for each bin we note FEAR and the nearest point on the pareto-frontier generated by shortreg to rank them amongst each other using Spearman’s correlation (denoted as ‘spe’) and common ratio of architectures in groundtruth ranking against the average time in seconds. Especially on CIFAR10 and CIFAR100 large gaps in performance can be seen at high-performing architecture bins.

**Finding the training threshold:** We construct a shallow pipeline using Histogram-of-Oriented-Gradients (HoG) [Dalal and Triggs, 2005] as image features and construct a relatively shallow learner by passing the features through two hidden fully connected layers. This simple pipeline achieves 0.6 training accuracy on CIFAR10, 0.3 on CIFAR100 and 0.2 accuracy on ImageNet16-120. These numbers were used as the training accuracy threshold for stage 1 of FEAR with respective datasets.
Random Search with FEAR: On CIFAR100 and ImageNet16-120 we ran random search (RS) with FEAR (RS-FEAR) 10 times with different random seeds where the search cut-off any architecture which exceeded 4.0 times the fastest time to reach threshold training accuracy encountered so far. Each method got a budget of 500 architectures. As shown in Table 1 RS-FEAR can get similar final accuracy by being ≈ 2.4 times faster. See Figure 5 in Appendix A.2 for an intuitive explanation of how FEAR early rejects weaker architectures.

|          | FEAR (4.0 * fastest) | shortreg (50 epochs) |
|----------|-----------------------|-----------------------|
|          | top1 (%), duration (s) | top1 (%), duration (s) |
| CIFAR10  | 93.97±0.08, 90560±845  | 94.10±0.10, 347643±2287 |
| CIFAR100 | 72.04±0.29, 142550±3106 | 72.08±0.30, 347640±1674 |
| ImageNet16-120 | 45.97±0.17, 214824±4674 | 45.64±0.21, 528454±4901 |

Table 1: When used as the architecture evaluation scheme in random search, FEAR can achieve same or more top-1 accuracy as shortreg while using ≈ 2.4 times less search duration on CIFAR100 and ImageNet16-120. On CIFAR10 it takes ≈ 3.8 times less duration. Each experiment was run 10 times with different random seeds. (*shortreg on CIFAR10 had finished 7 runs at the time of writing).

4.4 Deeper Dive into Zero-Cost Measures

As discussed in Section 2, Abdelfattah et al. [2021] propose using pruning-at-initialization methods like synflow, snip, grasp, fisher etc for ranking architectures without any training by summing up the per-weight saliency scores to come up with an overall architecture score. In thorough experiments, synflow emerged as a good ranking measure with a majority voting scheme with synflow, jacob_cov and snip emerging as the best overall. As discussed in Section 2, synflow’s good performance is a bit perplexing since it is a data-agnostic measure. It suggests that there are inherently good and bad architectures and the particulars of the dataset should not matter. In order to investigate this we created a synthetic dataset with properties such that it would be a drop-in for CIFAR10. Specifically we created random Gaussian images of dimension [32, 32, 3] and mean 0 and variance 1. Each image
Table 2: (Left) Spearman’s correlation (‘spe’) comparison between FEAR and the nearest point on the pareto-frontier etched out by shortreg variants with respect to average wall-clock time (s). (Right) Ratio of overlap in bins between rankings of FEAR and nearest pareto-frontier point of shortreg. The nearest pareto-frontier points are found by inspecting figures 6, 7 and 8.

| Top % | FEAR (spe, s) | Nearest Pareto (spe, s) | FEAR (common, s) | Nearest Pareto (common, s) |
|-------|---------------|-------------------------|-------------------|----------------------------|
| 10    | 0.22, 133.22±4.09 | 0.16, 138.28±2.25       | 0.59, 133.22±4.09 | 0.40, 138.28±2.25          |
| 20    | 0.40, 133.04±2.76  | 0.19, 134.68±1.50       | 0.60, 133.04±2.76 | 0.57, 134.68±1.50          |
| 30    | 0.41, 132.76±2.24  | 0.28, 140.13±1.46       | 0.70, 132.76±2.24 | 0.70, 132.77±2.24          |
| 40    | 0.43, 131.41±2.00  | 0.34, 135.31±1.32       | 0.78, 131.41±2.00 | 0.72, 135.31±1.32          |
| 50    | 0.55, 134.48±1.97  | 0.50, 136.26±1.14       | 0.79, 134.48±1.97 | 0.79, 136.26±1.14          |
| 100   | 0.83, 236.36±9.39  | 0.90, 218.97±1.44       | 1.0, 236.36±9.39  | 1.00, 218.97±1.44          |

(a) Natsbench CIFAR 10

| Top % | FEAR (spe, s) | Nearest Pareto (spe, s) | FEAR (common, s) | Nearest Pareto (common, s) |
|-------|---------------|-------------------------|-------------------|----------------------------|
| 10    | 0.61, 213.07±6.33 | 0.42, 264.68±3.52       | 0.63, 213.04±6.33 | 0.63, 264.68±3.52          |
| 20    | 0.51, 208.84±3.36 | 0.25, 257.12±2.43       | 0.79, 208.84±3.36 | 0.79, 257.12±2.43          |
| 30    | 0.66, 207.19±3.63 | 0.15, 249.86±2.03       | 0.79, 207.19±3.63 | 0.79, 249.86±2.03          |
| 40    | 0.70, 205.13±3.16 | 0.27, 253.67±2.18       | 0.85, 205.13±3.16 | 0.85, 244.96±1.85          |
| 50    | 0.76, 201.65±2.78 | 0.26, 247.37±1.97       | 0.90, 201.65±2.78 | 0.90, 239.41±1.66          |
| 100   | 0.93, 313.52±9.31 | 0.90, 329.79±2.19       | 1.0, 313.52±9.31  | 1.00, 281.30±2.42          |

(b) Natsbench CIFAR 100

| Top % | FEAR (spe, s) | Nearest Pareto (spe, s) | FEAR (common, s) | Nearest Pareto (common, s) |
|-------|---------------|-------------------------|-------------------|----------------------------|
| 10    | 0.56, 481.75±17.14 | 0.56, 468.17±6.07       | 0.76, 481.75±17.14 | 0.72, 510.86±6.77          |
| 20    | 0.73, 498.91±14.52 | 0.64, 530.86±5.98       | 0.82, 498.91±14.52 | 0.81, 445.89±5.05          |
| 30    | 0.78, 486.40±11.56 | 0.69, 514.65±4.99       | 0.83, 486.40±11.56 | 0.85, 470.47±4.58          |
| 40    | 0.79, 491.91±10.74 | 0.75, 493.66±4.61       | 0.90, 491.91±10.74 | 0.91, 493.66±4.61          |
| 50    | 0.84, 571.37±10.73 | 0.83, 532.04±4.70       | 0.91, 511.37±10.73 | 0.88, 510.74±5.04          |
| 100   | 0.95, 1070.88±35.88 | 0.96, 822.05±8.14       | 1.0, 1070.88±35.88 | 1.00, 822.05±8.14          |

(c) Natsbench ImageNet16-120
was assigned a class label in (0, 9) by passing each image through 10 randomly initialized neural networks and picking the id of the network which assigned the image a maximum score. Each of the networks has a simple architecture of a linear layer with dimension 3072, followed by a ReLu layer, followed by a linear layer which produces a single scalar output. A dataset of 60000 images was generated with 50000 training and the rest held-out as a test set. Each class has 6000 examples. We refer to this dataset as Synthetic CIFAR10.

The same set of 1000 randomly sampled architectures used in above experiments were evaluated on this dataset using FEAR and the various zero-cost measures. Table 3 shows that the zero-cost measures have almost no correlation with rankings while FEAR still works reasonably. Also note that the rankings via synflow which ignore the dataset are no longer valid on Synthetic CIFAR10. This means that architectures which performed really well on CIFAR10 don’t work as well on Synthetic CIFAR10. This is at least an existence proof of the fact that performance of an architecture is also a function of the dataset and task and is not an inherent property only of the topology of the network. This is also empirically shown recently by Tuggener et al. [2021].

In Figure 4 we evaluated zero-cost measures after each epoch of training on CIFAR10 for the 1000 randomly sampled architectures from Natsbench topological search space. We find that measures like snip and grad_norm gradually degrade in rank correlation as the network trains. jacob_cov and grasp at initialization have Spearman of 0.69 and 0.63 respectively but after

| Method     | Spearman Corr. |
|------------|----------------|
| synflow    | -0.000437      |
| jacob_cov  | -0.13          |
| snip       | -0.31          |
| fisher     | -0.42          |
| grasp      | -0.25          |
| synflow_bn | 0.18           |
| FEAR       | 0.55           |

Table 3: Zero-Cost measures on Synthetic CIFAR10 achieve low correlation while FEAR still gets reasonable performance. This phenomenon is especially not amenable for data-agnostic measures like synflow which ignore the dataset completely.
Figure 4: Evaluating the ranking performance of zero-cost measures after each epoch of training. Measures like \texttt{grasp} and \texttt{jacob\_cov} demonstrate large degradation in performance after even a single epoch of training. \texttt{snip} and \texttt{grad\_norm} decay gradually.

even one epoch of training drastically degrade to $-0.02$ and $-0.41$. Note from figures 3, 6, 7, and 8 that ranking architectures via training error even after one or two epochs of training leads to much better ranking correlation.

Note that measures derived from \texttt{snip}, \texttt{grasp}, \texttt{synflow} are intended originally for the task of \texttt{pruning} architectures at initialization. So it is perhaps not surprising that the saliency scores when summed-up over individual weights to provide a global architecture score doesn’t exhibit good ranking performance as the network is trained.

5 Conclusion

We have presented a simple but powerful fast architecture ranking scheme (\texttt{FEAR}) which can be used in the inner loop of \textit{any} discrete NAS algorithm to speed-up architecture evaluation. We have shown on standard NAS benchmarks that \texttt{FEAR} is effective and in the inner loop of even random search can drastically speed-up evaluation without loss of accuracy.

In future work we aim to validate \texttt{FEAR} on the state-of-the-art discrete
search methods like Bayesian optimization-based techniques [White et al., 2019] or even simpler techniques which have been surprisingly beneficial on NAS benchmarks like [White et al., 2020a]. With enough compute we will aim to validate on larger search spaces like DARTS via Nasbench-301 [Siems et al., 2020] and search spaces around Transformer-like architectures [Wang et al., 2020b, Tsai et al., 2020].

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A Appendix

A.1 Reproducibility and Best Practices Checklist in NAS

We use the best practices by [Lindauer and Hutter, 2019] to foster reproducibility and do better empirical NAS research.

1. Best practices for releasing code.
   (a) Code for the training pipeline used to evaluate the final architectures. - Yes.
   (b) Code for the search space. - Yes.
   (c) The hyperparameters used for the final evaluation pipeline as well as random seeds. - Yes.
   (d) Code for your NAS method. - Yes.
   (e) Hyperparameters for your NAS method, as well as random seeds. - Yes.

2. Best practices for comparing NAS methods.
   (a) For all NAS methods you compare, did you use exactly the same NAS benchmark, including the same dataset (with the same training-test split), search space and code for training the architectures and hyperparameters for that code? - Yes.
   (b) Did you control for confounding factors (different hardware, versions of DL libraries, different runtimes for the different methods)? - Yes. Specifically we run all the baselines ourselves on the same hardware.
   (c) Did you run ablation studies? - Yes.
   (d) Did you use the same evaluation protocol for the methods being compared? - Yes.
   (e) Did you compare performance over time? - Yes.
   (f) Did you compare to random search? - Yes.
   (g) Did you perform multiple runs of your experiments and report seeds? - Yes.
(h) Did you use tabular or surrogate benchmarks for in-depth evaluations? - Yes.

3. Best practices for reporting important details. - Yes

   (a) Did you report how you tuned hyperparameters, and what time and resources this required? - Yes.

   (b) Did you report the time for the entire end-to-end NAS method (rather than, e.g., only for the search phase)? - Yes.

   (c) Did you report all the details of your experimental setup? - Yes.

A.2 Detailed Random Search Experiments

Algorithm 1 shows the simple modifications made to vanilla random search to enable architecture evaluation with FEAR and early-reject weaker architectures which inevitably take much longer to reach threshold accuracy than stronger candidates. See Figure 5 and associated caption for an intuitive illustration of how one can early reject most architectures from the search space.
Figure 5: In this illustration on 1000 architectures from Natsbench topology search space on CIFAR10 we show how one can use FEAR in the inner loop of any discrete search algorithm to early-reject many architectures thus saving time without losing final accuracy. For simplicity we consider random search here. A simple modification to random search is made where the fastest time for any architecture to reach threshold accuracy is tracked. If any subsequent architecture improves on this time then it is updated. If any subsequent architecture during evaluation takes more than \( x \times \text{fastest\_till\_now} \) (where \( x \) is usually 4.0 in our experiments as that leads to retaining most of the top ones) then evaluation is terminated and control moves on to the next randomly sampled architecture. Such architectures which are early rejected from evaluation are represented by the region to the right of the green vertical dotted line in the figure. As more and more architectures are evaluated the estimate of the \( \text{fastest\_till\_now} \) improves and so does early termination of weaker architectures. And this benefit of early stopping weaker architectures is evident in our experiments where we see that one can reduce the search time by \( \approx 2.4 \) times on CIFAR100 and ImageNet16-120 without losing accuracy as detailed in Section 4.
Algorithm 1 Random Search with FEAR

Input: Architecture search space \( \mathcal{A} \), ratio of fastest time to reach threshold \( r \), maximum budget of architectures: \( B \), threshold accuracy to train to: \( t \)

Output: Best architecture \( a \in \mathcal{A} \)

\( \text{fastest} \_\text{till} \_\text{now} = \infty \)
\( \text{best} \_\text{train} \_\text{till} \_\text{now} = -\infty \)
\( \text{best} \_\text{arch} = \text{None} \)

\( \text{for } i = 1 \text{ to } B \text{ do} \)
\( \quad a = \text{random} \_\text{sample}(\mathcal{A}) \) \{Uniformly random sample an architecture.\}
\( \quad \text{time} \_\text{allowed} = r \times \text{fastest} \_\text{till} \_\text{now} \) \{Time allowed for architecture evaluation by FEAR.\}
\( \quad \text{result}, \text{time} \_\text{taken} = \text{FEAR}(a, t, \text{fastest} \_\text{till} \_\text{now}) \) \{FEAR evaluate architecture.\}
\( \quad \text{if not result then} \)
\( \quad \quad \text{continue} \) \{If FEAR early-rejected then move on to next architecture.\}
\( \quad \text{else} \)
\( \quad \quad \text{if result} > \text{best} \_\text{train} \_\text{till} \_\text{now} \text{ then} \)
\( \quad \quad \quad \text{best} \_\text{train} \_\text{till} \_\text{now} = \text{result} \) \{If a better architecture was found then update.\}
\( \quad \quad \quad \text{best} \_\text{arch} = a \)
\( \quad \quad \text{if time} \_\text{taken} < \text{fastest} \_\text{till} \_\text{now} \text{ then} \)
\( \quad \quad \quad \text{fastest} \_\text{till} \_\text{now} = \text{time} \_\text{taken} \) \{Update fastest time if faster than current fastest.\}
\( \quad \text{end if} \)
\( \quad \text{end if} \)
\( \text{end if} \)
\( \text{end for} \)
\( \text{return} \ \text{best} \_\text{arch} \)
A.3 Detailed Ranking Plots on Natsbench Topological Search Space

Figure 6: [Left] Average duration vs. Spearman’s correlation and [Right] average duration vs. common ratio over the top $x\%$ of the 1000 architectures sampled from Natsbench topological search space on CIFAR10. We also show the various zero-cost measures from Abdelfattah et al. [2021] in green.
Figure 7: [Left] Average duration vs. Spearman’s correlation and [Right] average duration vs. common ratio over the top $x\%$ of the 1000 architectures sampled from Natsbench topological search space on CIFAR100. We also show the various zero-cost measures from Abdelfattah et al. [2021] in green.
Figure 8: [Left] Average duration vs. Spearman’s correlation and [Right] average duration vs. common ratio over the top $x\%$ of the 1000 architectures sampled from Natsbench topological search space on ImageNet16-120. We also show the various zero-cost measures from Abdelfattah et al. [2021] in green.