The Benchmark Lottery

Abstract

The world of empirical machine learning (ML) strongly relies on benchmarks in order to determine the relative effectiveness of different algorithms and methods. This paper proposes the notion of a benchmark lottery that describes the overall fragility of the ML benchmarking process. The benchmark lottery postulates that many factors, other than fundamental algorithmic superiority, may lead to a method being perceived as superior. On multiple benchmark setups that are prevalent in the ML community, we show that the relative performance of algorithms may be altered significantly simply by choosing different benchmark tasks, highlighting the fragility of the current paradigms and potential fallacious interpretation derived from benchmarking ML methods. Given that every benchmark makes a statement about what it perceives to be important, we argue that this might lead to biased progress in the community. We discuss the implications of the observed phenomena and provide recommendations on mitigating them using multiple machine learning domains and communities as use cases, including natural language processing, computer vision, information retrieval, recommender systems, and reinforcement learning.

Contents

1 Introduction ........................................ 2
2 Background .......................................... 4
   2.1 Life of a benchmark .............................. 5
3 Task selection bias .................................. 6
   3.1 Case Studies .................................... 6
      3.1.1 SuperGLUE ................................. 6
      3.1.2 Visual Task Adaptation Benchmark (VTAB) .... 7
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1 Introduction

Quantitative evaluation is a cornerstone of machine learning research. As a result, benchmarks, including those based on data sets and simulations, have become fundamental to tracking the progress of machine learning research.

Benchmarks have a long history in artificial intelligence research generally. There have been several attempts at designing milestones to capture progress toward artificial intelligence (e.g., human level game performance, the Turing test \cite{Turing1950}). Specific system properties are measured through specialized benchmarks (e.g. for vision, natural language processing, robotics). All of these benchmarks, by design, encode values about what is salient and important, both across domains (e.g. natural language processing benchmarks versus robotics benchmarks) and within them (e.g. which
languages are considered in an NLP benchmark, which environments are considered in a robotics benchmark).

As benchmarks become widely accepted, researchers adopt them, often without questioning their assumptions, and algorithmic development becomes slowly tied to these success metrics. Indeed, over time, the research community makes collective decisions about what shared tasks—and values—are important (through peer review norms and resource investment) and which are not.

Because of this, it is important for the research community to understand the individual, community, social, and political pressures that influence why some benchmarks become canonical and others do not. This paper shares some opinions on this topic along with case studies calling for discussion and reconsiderations on several issues with benchmarking in machine learning and argues that a meta-level understanding of benchmarks is a prerequisite for understanding how the progress in machine learning is made.

This paper presents analyses on how benchmarks may affect the direction and pace of progress in machine learning and puts forward the notion of a benchmark lottery. We argue that many factors other than the algorithmic superiority of a method may influence the emergence of algorithms that are perceived as better. Moreover, we claim that for a method to emerge successful, it has to first win the benchmark lottery. Out of the many potential trials in this lottery, a method has to be first well-aligned with the suite of benchmarks that the community has accepted as canonical. We refer to the alignment between the tasks brought forth by the community and successful algorithms as the task selection bias. We empirically show that the task selection process has a great influence over the relative performance of different methods. Moreover, we argue that benchmarks are stateful, meaning that the method has to also participate in the lottery at the right moment, and to align well with existing techniques, tricks, and state-of-the-art. Related to this, we also briefly discuss how benchmark reuse may affect the statistical validity of the results of new methods.

As a whole, as we researchers continue to participate in the benchmark lottery, there are long-term implications, which we believe are important to be explicitly aware of. As such, the main goals of this paper are to (i) raise awareness of these phenomena and potential issues they create; and to, (ii) provide some recommendations for mitigating these issues. We argue that community forces and task selection biases, if left unchecked, may lead to unwarranted overemphasis of certain types of models and to unfairly hinder the growth of other classes of models - which may be important for making fast and reliable progress in machine learning.

Additionally, we discuss the trend of consolidating several benchmarks into a meta-benchmark (or benchmark suite), e.g. GLUE [Wang et al., 2018], XTREME [Hu et al., 2020], VTAB [Zhai et al., 2019], RL Unplugged [Gulcehre et al., 2020] to name a few. In this context, using the most common score aggregation technique, i.e., averaging, a model that performs poorly on any of the bundled tasks may be set up for failure. We argue that the and operator is an inductive bias that favors well-rounded models as opposed to models that may do really well on one task, making unfair assumptions about what practitioners might really need or want. Finally, in alignment with the task selection biases, we show that the selection of a subset of the tasks used in the benchmark suite plays a substantial role in determining the winning algorithms, providing further evidence that researchers are participating in the benchmark lottery.

The notion of what makes a benchmark canonical, in the sense that is widely accepted by the community, is also diverse depending on the field of study. On one hand, fields like natural language processing (NLP) or computer vision (CV) have well-established benchmarks for certain problems. On the other hand, fields such as recommender systems or reinforcement learning tend to allow researchers more freedom in choosing their own tasks and evaluation criteria for comparing methods. We show how this may act as rigging the lottery, where researchers can “make their own luck” by fitting benchmarks and experimental setups to models instead.

Overall, this paper explores these aspects of model evaluation in machine learning research. We frame this from a new perspective of the benchmark lottery. While there has been recent work that peers deeply into the benchmark tasks themselves [Bowman and Dahl, 2021], this work takes meta- and macro-perspectives to encompass factors that go beyond designing reliable standalone tasks.

The remainder of the paper is organized as follow:
• Section 2 discusses how benchmarks can influence long-term research directions in a given (sub-)field, and describes the life cycle of a benchmark.
• Section 3 introduces the task selection bias and using established benchmarks as examples shows how relative performance of algorithms is affected by the task selection process.
• Section 4 takes another view of the task selection bias and proposes community bias as a higher-level process that influences task selection. We show that forces from the broader research community directly impact the task selection process and as a result, play a substantial role in creating the lottery.
• Section 5 posits that benchmarks are stateful entities and that participation in a benchmark differs vastly depending upon its state. We also argue continual re-use of the same benchmark may be problematic.
• Section 6 discusses rigging the lottery, the issue that some communities (e.g. recommender systems and reinforcement learning) face, where the lack of well-established community-driven sets of benchmarks or clear guidelines may inadvertently enable researchers to fit benchmarks to model. We highlight the potential drawbacks of such an approach.
• Finally, in Section 7 we provide recommendations for finding a way out of the lottery by building better benchmarks and rendering more accurate judgments when comparing models.

Overall, unified benchmarks have led to incredible progress and breakthroughs in machine learning and artificial intelligence research [Kingma and Welling, 2013, Mikolov et al., 2013, Sutskever et al., 2014, Bahdanau et al., 2014, Goodfellow et al., 2014, Hinton et al., 2015, Silver et al., 2016, He et al., 2016a, Vaswani et al., 2017, Devlin et al., 2018, Brown et al., 2020, Dosovitskiy et al., 2020].

There is certainly a lot of benefits of having the community come together to solve shared tasks and benchmarks. Given that the role of benchmarks is indispensable and highly important for measuring progress, this work seeks to examine, introspect and find ways to improve.

2 Background

Measuring progress is one of the most difficult aspects of empirical computer science and machine learning. Such questions as “What are the best setup and task to use for evaluation?” [Ponce et al., 2006, Machado et al., 2018, Lin, 2019, Bowman and Dahl, 2021, Recht et al., 2019, Lin et al., 2021, Gulcehre et al., 2020, Perazzi et al., 2016, Vania et al., 2020, Musgrave et al., 2020], “Which data or benchmark are most applicable?” [Metzler and Kurland, 2012, Beyer et al., 2020, Northcutt et al., 2021, Gulcehre et al., 2020, DaCunha et al., 2019], “Which metrics are suitable?” [Machado et al., 2018, Bouthillier et al., 2021, Balduzzi et al., 2018, Bouthillier et al., 2019, Musgrave et al., 2020], or “What are the best practices for fair benchmarking?” [Torralba and Efros, 2011, Armstrong et al., 2009, Machado et al., 2018, Sculley et al., 2018, Lin, 2019, Bowman and Dahl, 2021, Bouthillier et al., 2021, Recht et al., 2019, Lin et al., 2021, Balduzzi et al., 2018, Lipton and Steinhardt, 2018, Bouthillier et al., 2019, Vania et al., 2020, Mishra and Arunkumar, 2021, Marie et al., 2021, Dodge et al., 2019] are of utmost importance to correct empirical evaluation of new ideas and algorithms, and have been extensively studied. Nevertheless, the jury is still out on most of these questions.

Owing to this complexity at no point in time can the claim of superiority of a certain algorithm or model on a specific task be made with full confidence. However, providing any answer to the question of “Which algorithm is currently the best?” can play a major role in determining the direction of research and the progress in the field in the long run.

We argue that some models and algorithms are not inherently superior to their alternatives, but are instead perceived as such by the research community due to various factors that we discuss in this paper. One of these factors is the software and hardware support for an idea, as captured in the concept of hardware lottery by [Hooker, 2020]. Here however we focus mainly on benchmark-related factors, and discuss the role they play in the selection of a model as “fashionable” in the research world, and how this is often conflated with the model being better. When a class of models or algorithms gets recognition in the community, there will be more follow up research, adaption to more setups, more tuning and discovery of better configurations, which lead to better results. This is a valid way of propelling the field further. However, a question that we should also ask is how much progress could have been made by investing the same amount of time, effort, computational
resources and talent in a different class of models. In other words, assuming model development as a complex high-dimensional optimization process, in which researchers are exploring a fitness surface, the initial point, as well as the fitness function, are the key factors for ending up with better optima, and both these factors are highly affected by the benchmarks used for evaluation.

Before diving into various aspects of the benchmark lottery, we begin by describing the life cycle of a benchmark and set the stage by describing several widely adopted benchmarks/tasks and their characteristics.

2.1 Life of a benchmark

A benchmark is often proposed as a means to evaluate relative model quality. However, it can sometimes also be employed as a diagnostic tool or suite that allows practitioners to peer into the behavior of their models. This section examines the life cycle of a benchmark.

Inception. A benchmark may be first proposed as an example task in its debut paper (implicit), or be directly framed as a shared task (explicit) where the main contribution may be the dataset itself. A recent trend is also for a leaderboard or competition website to be set up to allow submissions from the community. An example of the former is the Stanford Sentiment Treebank (SST) dataset, which was proposed as part of a methods paper [Socher et al., 2013]. Conversely, examples of an explicit shared task are benchmarks like SQuAD [Rajpurkar et al., 2016], GLUE [Wang et al., 2018, 2019] or XTREME [Hu et al., 2020].

Gaining traction or fizzling out. certain benchmarks may gain traction at an early stage, encouraging a large number of submissions or publications that evaluate their ideas against them. Benchmarks that are gamified with leaderboards [Rajpurkar et al., 2016, Bowman et al., 2015, Hu et al., 2020, Wang et al., 2019, 2018] tend to be of this nature, encouraging dataset creators to always setup these competition scenarios. Similar to how posts become viral on social media, early tractions and citations on benchmark are likely to influence future adoption. That said, benchmarks may also fizzle out due to one or many issues. Oversaturation on a certain domain is of the leading factors, along with usage difficulties (such as having evaluation metrics or setups that may be inconvenient or tedious, etc). On the flip side, benchmarks can suddenly gain traction if a highly influential method paper uses them in its experiments. As an example, the subject-verb agreement task [Linzen et al., 2016] became more popular, when it started being used for showcasing the ability of recurrent neural networks over Transformers on modeling hierarchical structure [Tran et al., 2018, Dehghani et al., 2019, Abnar et al., 2020].

Active and in-progress. This stage may take a long time depending on how much progress is being made on the proposed task. An active task will typically have a large number of submissions and publications attempting to make improvements on the task. Progress may be optimistic and the bar for improvement is usually higher at this stage. A Stage 3 benchmark may even be considered “hot” where any progress or state-of-the-art result is poised to generate a lot of interest and publicity. An example of such a benchmark is the Long Range Arena benchmark [Tay et al., 2020b], whose usage continues to increase leading to the emergence of new ideas for improving the efficiency of Transformer models on long sequences.

Maturity, degeneration or deprecation. A benchmark may reach its maturity if the community considers the task to be solved, or if meaningful progress on the task can no longer be made (degeneration). This can happen once human parity is achieved (e.g. SQuAD). Eventually, the community largely stops paying attention to the task, and what is left is researchers optimizing for the last 0.01% percentage improvement. An example of continued hill-climbing on a mature benchmark can be found at the SQuAD leaderboard.3

Renewal. Once a benchmark degenerates or reaches full maturity, it is not uncommon for the original authors (or an independent set of authors) to propose new variations or extensions of the benchmark. This might be to resolve issues with the existing version, or simply to improve the challenge, difficulty, or other aspects of the benchmark. We saw this evolution when SQuAD benchmark graduated to V2 [Rajpurkar et al., 2018], or when the SNLI [Bowman et al., 2015] benchmark slowly evolved into MultiNLI [Williams et al., 2017]. Another example is the of ImageNet-A and O [Hendrycks et al., 2021], ImageNet-C [Hendrycks and Dietterich, 2019], and ImageNet-R [Hendrycks et al., 2020] for evaluating robustness of algorithms. As a more known example, QMNIST [Yadav and Bottou, 2019] is proposed as a larger and more accurate dataset as a replacement for the MNIST dataset.

3https://rajpurkar.github.io/SQuAD-explorer/
Figure 1: Disagreement of model rankings on the SuperGLUE benchmark as a function of the number of selected benchmark tasks. The x-axis represents the number of tasks in each sub-selection of tasks and each line corresponds to a different value of k for the Top-k in the rankings. Points are labels as A/B, where A is the number of unique model rankings and B is the total number of possible task combinations for this subset size. If A = 1, then all rankings are equivalent and consistent across all task selections; higher values of A correspond to higher degrees of disagreement between models rankings.

3 Task selection bias

As we show in this section, relative model performance is highly sensitive to the choice of tasks and datasets it is measured on. As a result, the selection of well-established benchmarks plays a more important role than is perhaps acknowledged, and constitutes a form of partiality and bias - the task selection bias.

3.1 Case Studies

In this section, we study four different popular benchmarks or natural language processing, computer vision, efficient sequence modeling, and offline reinforcement learning and use the data from the leaderboards of these benchmarks and run analyses to highlight the effect of task selection bias.

3.1.1 SuperGLUE

In order to study the effect of aggregated scores and how findings change by emphasizing and de-emphasizing certain tasks, we explore the SuperGLUE dataset [Wang et al., 2019]. To demonstrate the task selection bias on this benchmark, we re-compute the aggregated scores using different combinations of eight SuperGLUE tasks.

Model Runs. We consider over 55 different top performing models that are studied in [Narang et al., 2021], including transformer-based models with various activation functions, normalization and parameter initialization schemes, and also architectural extensions (e.g., Evolved Transformers [So et al., 2019], Synthesizers [Tay et al., 2020a], Universal Transformer [Dehghani et al., 2019], and Switch Transformers [Fedus et al., 2021]) as well as convolution-based models (e.g. lightweight and dynamic convolutions). We consider the fine-grained scores of these models on the 8 individual tasks of SuperGLUE and their different combinations. For each combination of tasks, we take a mean-aggregate model performance for all models on the selected tasks and produce a ranking of all 55 models. To make this ranking more meaningful, we only consider its Top-k entries, where k ∈ {1, 3, 5, 10}.

Ranking inconsistency. Figure 1 gives a concise overview of the number of unique Top-k rankings produced obtained from fixed-size subsets of tasks. For example among the 70 different possibilities of selecting 4 out of 8 tasks, there are 6 distinct model ranking orders produced for Top-1 (i.e. there are 6 different possible top models). Moreover, when considering Top-3 or even Top-5, almost 60 out of 70 rankings do not agree with each other. Overall, the rankings become highly diverse as the subset of tasks selected from the benchmark is varied. This forms the core of the empirical evidence
of the task selection bias. More analyses on ranking of models on all possible combinations of tasks, rank correlation between SuperGLUE score and individual tasks, effect of relative ranking of models in Appendix A, B, and C.

### 3.1.2 Visual Task Adaptation Benchmark (VTAB)

A similar situation can be observed for the Visual Task Adaptation Benchmark (VTAB; [Zhai et al., 2019]) benchmark. VTAB is used for evaluating the quality of representations learned by different models in terms of their ability to adapt to diverse, unseen tasks with few examples. In addition to standard natural image tasks, like classification on ImageNet or CIFAR datasets, VTAB includes tasks that are related to sensorimotor control, medical imaging, and scene understanding. The benchmark defines the score of an algorithm as its expected performance over a known distribution of tasks that includes those that a human can solve, from visual input alone.

VTAB defines a total of 19 tasks, grouped into three categories: (i) **Natural**, which contains natural images captured using standard cameras that represent generic, fine-grained, or abstract objects ([Caltech101] [Fei-Fei et al., 2006], [CIFAR100] [Krizhevsky et al., 2009], [DID] [Cimpoi et al., 2014], [Flowers102] [Nilsback and Zisserman, 2008], [Pets] [Parkhi et al., 2012], [Sun397] [Xiao et al., 2010], and [SVHN] [Netzer et al., 2011];) (ii) **Specialized**, which contains images of the world that captured through specialist equipment [Remote sensing: Resisc45 [Cheng et al., 2017] and EuroSAT [Helber et al., 2019]: aerial images of the Earth captured using satellites or aerial photography; Medical: Patch Camelyon [Veeling et al., 2018], metastases detection from microscopy images, and Diabetic Retinopathy [Kaggle and EyePacs, 2015], retinopathy classification from fundus images;) and finally (iii) **Structured**, which contains tasks that designed to assess comprehension of the structure of a scene, mostly generated syntactically using simulated environments [CLEVR [Johnson et al., 2017]: Simple shapes rendered in a 3D scene, with two tasks: counting and depth prediction, dSprites [Higgins et al., 2016]: Simple black-and-white shapes rendered in 2D, with two tasks: location and orientation prediction, SmallNORB [LeCun et al., 2004]: Artificial objects viewed under varying conditions, with two tasks: object azimuth and camera-elevation prediction, DMLab [Beattie et al., 2016]: Frames from a rendered 3D maze. The task involves predicting the time for a pre-trained RL agent to navigate to an object, KITTI [Geiger et al., 2013]: frames captured from a car driver’s perspective and the task is to predict the depth of the nearest vehicle.]. We have evaluated 32 different models against all the 19 VTAB tasks. The difference between models is on their architectures (e.g. WAE-GAN [Tolstikhin et al., 2017] vs. VIVI[Tschannen et al., 2020]), their sizes (e.g. ResNet-50 vs. ResNet-101 [Kolesnikov et al., 2019]), or the dataset they were pre-trained on (e.g. ResNet-50 pretrained on ImageNet-21k vs. ResNet-50 pretrained on JFT [Kolesnikov et al., 2019]). Models we considered in our study are those that are introduced as “representation learning algorithms” in [Zhai et al., 2019].

First, we study the agreement of the aggregated score across all 19 tasks with the aggregated scores obtained from different combinations of the three task categories: natural (NA), specialized (SP), and structured (ST). Figure 24 shows the Kendall rank correlation, when ranking different models based on the full VTAB score and based on the category (combination) score. It can be seen that rankings of models based on different combinations of categories are not always perfectly correlated. For instance, the structured (ST) subcategory has a correlation of $\approx 0.7$ with the full VTAB score, thus highlighting rather different aspects of the competing models. A more striking point is the full disagreement of different subcategories on the winning model, i.e. top-1 that is shown in Appendix D where we future present the results that show disagreement in the top-1, 2, and 3 rank positions based on different combinations of sub-categories and tasks. This shows that crowning a model as the winner based on a single score can be suboptimal, and demonstrates how the random nature of task selection can become a lottery that algorithms need to win.

Figure 25 also presents the correlations between the rankings based on the individual tasks and the aggregated VTAB score. Unsurprisingly, an even stronger disagreement between rankings is observed (mean Kendall correlation of $\approx 0.60$), including tasks with negative correlation.

### 3.1.3 Long Range Arena

The Long Range Arena (LRA; [Tay et al., 2020b]) is a benchmark designed for aggregated evaluation of long-range Transformer models [Tay et al., 2020c]. Similar to other benchmark suites, LRA consists of six tasks: ListOps, Long Text Classification, Long Text Retrieval, Pixel-wise Image
Table 1: Top-3 best performing models on the LRA for three cases: the full LRA score, individual tasks and leave-one-out scores. Results on all the possible combinations along with complete model names are reported in Table 3 in the Appendix E.

| Task Score | Best Model | Rank-2 | Rank-3 |
|------------|------------|--------|--------|
| LRA Score  | B          | L      | L      |
| $t_1$ Text only | L | P | T |
| $t_2$ Retrieval only | S | B | L |
| $t_3$ ListOps only | R | Z | T |
| $t_4$ Image only | S | P | T |
| $t_5$ Path only | P | L | L |
| $t_1 + t_2 + t_3 + t_4$ | T | B | L |
| $t_1 + t_3 + t_4 + t_5$ | B | T | L |
| $t_1 + t_2 + t_4 + t_5$ | S | P | B |
| $t_2 + t_3 + t_4 + t_5$ | B | T | L |

Classification, and two variants of spatial reasoning based on the path-finder task. The authors rank eleven efficient transformer models by aggregating performance across all six tasks. To demonstrate that here too task selection matters, we computed Top-3 rankings of models for each task combination displayed in Table 1. Model name abbreviations are used for brevity and because the actual model names are not important for the purpose of this analysis. Notably, it is easy to see that the identity of each of the top-3 changes frequently as the subset of evaluation tasks is changed.

3.1.4 RL Unplugged

(a) Atari 2600

(b) DM Control Suite

Figure 3: Rank correlation between the aggregated score and scores from each individual dataset. Note that the common approach in the literature to ingrate scores is “median human normalized performance” for Atari and “mean” for DM controls.
RL Unplugged \cite{Gulcehre2020} is a suite of benchmarks for offline reinforcement learning, where the task for the agent is to learn a policy directly from some logged data that is produced by a system as part of its normal operation, without interacting with the environment at the time of learning. In reinforcement learning, in general, it has been shown that varying random seeds alone can lead to a high variance between runs \cite{Henderson2018}, and this seed lottery is introducing difficulty in comparing different methods and making conclusions. Here, we study offline RL, where the results are more stable for the sake of focusing a bit more on the task selection bias problem. We will discuss online RL and expand on some other aspects in the context of Section 6.2.

RL Unplugged introduces a collection of task domains and associated datasets together with a clear evaluation protocol. It includes some widely used domains such as the DM Control Suite \cite{Tassa2018} and Atari 2600 games \cite{Bellemare2013}, as well as Real-World RL (RWRL) tasks \cite{DulacArnold2019} and DM Locomotion tasks \cite{Heess2017}.

Here, we study the aggregated scores over multiple tasks in Atari 2600 and DM Control from RL-Unplugged. Atari 2600 consists of 46 Atari games, and DM Control has 9 different tasks. We use the performance of 7 different baselines\footnote{For our analysis we used the data from the ancillary files of \cite{Schrittwieser2021}, which can be found at \url{https://arxiv.org/src/2104.06294v1/anc}.} in our analysis.

Figure 3a presents the Kendall rank correlation when ranking different models based on their human normalized performance on each task vs the median human normalized performance across all tasks. We also show the correlation between median and mean human normalized performance on Atari. Although many papers reported mean performance on Atari as the aggregated score, it is becoming a standard to report median since the mean is potentially less informative, as it is dominated by a few games (e.g. Atlantis) where agents achieve scores orders of magnitude higher than humans do. Figure 3b also shows the Kendall rank correlation of the mean performance across all tasks with performance on each task as well as the median. First of all, in both cases, it can be seen that the ranking of models based on individual tasks can widely disagree the ranking from the aggregated score (average rank correlation in Figure 3a is $\approx 0.49$ and in Figure 3b is $\approx 0.54$), indicating how solely reporting the aggregated score can send a potentially wrong signal for choosing the best model. Moreover, the aggregation strategies, i.e. mean and median in this case do not agree which shows standardizing one over another with the intention of considering only one of them comes at the cost of losing some information.

### 3.2 Score and rank aggregation

So far, we highlighted the issue with reporting a single aggregated score that is supposed to reflect the performance on multiple tasks, by showcasing the disagreement between different subsets of tasks. One of the main difficulties for aggregating scores of multiple tasks is the lack of a clear mechanism for incorporating the difficulty of tasks into account. This is made more complex by the fact that there are multiple facets to what makes a task difficult. For instance, the size of the training data for different tasks, the number of prediction classes (and consequently the score for a random baseline for the task), distribution shift between the pretraining dataset and the downstream tasks, different performance ranges across tasks, or overrepresenting particular aspects by multiple tasks that introduces biases into averages \cite{Balduzzi2018}. As a concrete example, in the case of VTAB some tasks use the same input data thus upweighting those domains, e.g. CLEVR-Count and CLEVR-Dist use the same data for different tasks, and for this particular example, given the negative correlation between CLEVR-Dist and the mean score, this upweighting effect makes the aggregated score even noisier.

To address some of these issues, there are alternative ways for ranking models instead of using the mean score across all tasks as the model performance on the benchmark. For instance, One can grouping tasks based on their domain) and use macro-averaging to account for the effect upweighting some domains \cite{Zhai2019}. Given that using simple averaging for aggregation across multiple tasks, the maximum score is bounded, this may limit the range of performances, implicitly upweighting tasks with more headroom. To address this issue, one can use geometric mean instead of arithmetic mean. There are also solutions for rank aggregation that ignore absolute score differences in favor of relative ordering \cite{Dwork2001, Tabrizi2015}: For instance, the “average rank” that is obtained by ranking the methods for each task based on their score and then computing the average ranks across tasks. Another alternatives are, for instance, robust average
rank, where, before averaging ranks across tasks, the accuracy is binned into buckets of size 1% and all methods in the same bucket get the same rank or elimination ranking (which is equivalent to an exhaustive ballot voting system) [Hao and Ryan, 2016].

3.3 Human evaluation bias

Related to the task selection bias we discussed in this section, human evaluation bias within a task can also play a role in model selection in some tasks like natural language generation. Lack of consistency in how human evaluation, e.g. due to different levels of expertise, cognitive biases, or even inherent ambiguity in the annotation task can introduce a large variability in model comparisons [Schoch et al., 2020]. In the context of measuring the reliability in human annotation, it has been shown that selecting a subset of annotators for evaluation may change the performance of models [Van Der Lee et al., 2019, Amidei et al., 2018, Schoch et al., 2020, Amidei et al., 2020], which can be framed as “annotation bias” that also contribute to the benchmark lottery.

4 Community bias

Even when viewed as a random process, the task selection bias described in Section 3 alone is sufficient for creating arbitrary selection pressures for machine learning models. We argue however that there is also a higher-level process in which the broader research community influences the task selection, and that counterintuitively leads to the lottery forces not being diminished, but instead more pronounced. This section takes a people perspective of the benchmark lottery and postulates that it is not only the “gamemasters” (benchmark proposers) but also the community that contribute to and reinforce it.

While researchers technically have the freedom to select any dataset to showcase their method, this choice is often moderated by the community. In short, the community determines which benchmarks are acceptable for measuring progress on particular problems. This process is implicit and not directly observable, as there are no hard rules on permissible benchmarks or tasks.

A common feedback in the review process of scientific publications that any ML researcher will face eventually is a criticism of the choice of benchmark. For example “the method was not evaluated on X or Y dataset” or “the method’s performance is not SOTA on dataset Z”. Over time, ML researchers tend to gravitate to safe choices of tasks and benchmarks. For example, most papers proposing new pretrained language models [Lan et al., 2019, Liu et al., 2019, Clark et al., 2020, Yang et al., 2020] evaluate on GLUE even if alternatives exist (see example below for further substantiation). In other words, the selection of tasks commonly used in publication is largely driven by the community. Moreover, whether a benchmark is selected as the canonical testbed or not, is not necessarily governed by the quality of the test examples, metrics, evaluation paradigm, or even what the benchmark truly measures.

In fact, an argument that the community is solely responsible for the task selection bias is not without merit, since the community is the final endorser and enforcer of these circumstances. There can be no task selection bias if there is no one to act upon it. To this end, the community might ‘double down’ on a benchmark where it becomes almost an unspoken rule for one to evaluate on a particular benchmark. Once a benchmark builds up a following and becomes well-established, it is not hard to imagine that reviewers would ask for results on these benchmarks, potentially regardless of suitability and/or appropriateness. This makes it difficult to fix potentially broken benchmarks.

As foreshadowed, commonly used benchmarks are not immune to containing errors. While these errors are likely to be small (as otherwise they would presumably be noticed early on), they do matter in close calls between competing methods. [Northcutt et al., 2021] identified label errors in test sets of 10 of the most commonly-used computer vision, natural language, and audio datasets; for example, there are label errors in 6% of the examples in the ImageNet validation set. They showed that correcting label errors in these benchmarks changes model ranking, especially for models that had similar performance. In the field of NLP, it was later found in SNLI [Bowman et al., 2015], which is a dataset for natural language inference (NLI), a large amount of annotation artifacts exists, and it is possible to simply infer the correct label by only using the premise and not the hypothesis [Gururangan et al., 2018]. It is worth noting that SNLI, being the canonical benchmark for NLI, was easily perceived as mandatory for almost any NLI based research.
The possibility of having such an issue is not only restricted to the peer review process, but it may extend to the public perception of papers after they are published regardless of whether they went through the peer review process or not. The community bias problem can be raised as the community collectively assigning a weighted impact score for doing well on arbitrarily selected tasks. Achieving state of the art on task $Y$ is then deemed significantly less meaningful than doing that for task $X$. Moreover, this is not necessarily done without any explicit reasoning as to why one task is preferred to the other, or even how such an “decision” was made.

The main concern with respect to the community bias is that research is becoming too incremental and biased toward the common expectations, since a completely new approach will initially have a hard time competing against established and carefully fine-tuned models.

4.1 Example: GLUE benchmark

The GLUE benchmark was pitched as a general language understanding benchmark and is an aggregation of 8 datasets that have been previously proposed [Williams et al., 2017]. We use this as an example of a community bias. To this date, the majority of pretrained LM paper evaluates on the GLUE benchmark. This includes widely recognized and cited papers such as BERT [Devlin et al., 2018], ALBERT [Lan et al., 2019], RoBERTa [Liu et al., 2019], XLNet [Yang et al., 2020], ELECTRA, and many others. Given the popularity of GLUE for evaluating these models, it is only imperative that newly proposed models have to shine on GLUE in order to increase their likelihood of acceptance (in the literal sense or by the community).

Here, it is good to note that seven out of eight tasks in GLUE are actually matching tasks that require modeling the relationship between two or more sequences. While it is still unclear how much this problem formulation has to do with natural language understanding, it is clear that this problem formulation favors a certain class of models (e.g., Transformers which has this baked-in cross attention in the encoder). It is easy to see that this conflates an actual advantage in problem formulation (and input setting) with the ability of an encoder model to learn textual representations. While one may argue that a method should reap rewards even for a problem formulation advantage, it is also good to note that many of these cross attention setups are infeasible in practice at scale [Guo et al., 2019, Seo et al., 2018]. It is also interesting that, if the tasks in GLUE were swapped for other equally plausible and practical tasks, we might encourage the development of alternative architectures such as pretrained ConvNets in NLP [Tay et al., 2021].

5 Benchmarks are stateful

With leaderboards and the continuous publication of new methods, it is clear that benchmarks are stateful entities. At any point in time, the attempt of a new idea for beating a particular benchmark depends on the information gathered from previous submissions and publications. This is a natural way of making progress on a given problem. But when viewed from the perspective of the selective pressures it causes, it creates another kind of lottery.

For many machine learning benchmarks, researchers have full access to the holdout set. Although not explicitly, this typically leads to the violation of the most basic datum of “one should not train on test/holdout set” by getting inspiration from already published works by others who presumably report only the best of the numerous models they evaluated on the test set.

Beyond that, it is common to copy-paste hyper-parameters, use the same code, and more recently to even start from pre-retrained checkpoints of previous successful models. In such setups, where the discovery of new models is built on top of thousands of queries, direct or indirect, to the test set, the error rate on test data does not necessarily reflect the true population error [Arora and Zhang, 2021, Blum and Hardt, 2015, Dwork et al., 2015]. The adaptive data analysis framework [Dwork et al.]

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4 We have manually checked the papers presented ideas to improve pretrained LMs with more than 500 citations that and in all these papers GLUE has been used for evaluation.

5 This is in particular common when a paper provides results based on large scale experiments that are not necessarily feasible to redo for many researchers. For instance, the majority of the papers that propose follow up ideas to Vision Transformer [Dosovitskiy et al., 2020] start by initializing weights from the released pretrained models and follow the setups of the original paper. Similarly, several NLP papers use BERT pretrained models and the same hyper-parameters as BERT in their experimental setup.
standard GLUE tricks” introduced/used by submissions to the leaderboard [Liu et al., 2019, Yang et al., 2019, Clark et al., 2020]. Check the Pre-training and fine-tuning details in the appendix of [Clark et al., 2020]. Another aspect of benchmarks being stateful is that participating in shared tasks at a later stage is often different from the time of its inception. By then the landscape of research with respect to the development of new models is shaped by the knowledge of the test errors of all models before it. First of all, there had been events in the past where accessing the test set more than others, intentionally, secured a margin for victory in the race. In other words, having the ability to access the test set more than others can be interpreted as buying more lottery tickets. Besides, even when there is no explicit intention, the tempting short-term rewards of incremental research polarize people to focus on short-term gains, not necessarily the long-term goal. Furthermore, the echo chamber effect - leading models are quickly adapted by re-using their code, pre-trained weights, and hyper-parameters are re-used to build something on top of them even faster. Unfortunately, this process makes no time for considering how it affects the statistical validity of the results reported on it? How far will we get by gaming the benchmark rather than making progress towards solving the original problem?

From the benchmark lottery point of view, the most important aspect of the above phenomena is that the development of new models is shaped by the knowledge of the test errors of all models before it. First of all, there had been events in the past where accessing the test set more than others, intentionally, secured a margin for victory in the race. In other words, having the ability to access the test set more than others can be interpreted as buying more lottery tickets. Besides, even when there is no explicit intention, the tempting short-term rewards of incremental research polarize people and reinforce the echo chamber effect - leading models are quickly adapted by re-using their code, pre-trained weights, and hyper-parameters are re-used to build something on top of them even faster. Unfortunately, this process makes no time for considering how it affects the statistical validity of results reported on the benchmark.

Another aspect of benchmarks being stateful is that participating in shared tasks at a later stage is vastly different from the time of its inception. By then the landscape of research with respect to the specific benchmark is filled with tricks, complicated and specialized strategies, and know-how for obtaining top performance on the task. The adapted recipes for scoring high are not necessarily universal and may be applicable only to a single narrow task or setup. For example, a publication might discover that a niche twist to the loss function produces substantially better results on the task. It is common for all papers subsequently to follow suit. As an example, the community realized that pre-training on MNLI is necessary for obtaining strong performance on RTE and STS datasets [Liu et al., 2019, Clark et al., 2020], and this became common practice later on. Experience shows that it is not uncommon for benchmark tasks to accumulate lists of best practices and tricks that are dataset- and task-specific. Whether a novel algorithm is able to make use of these tricks (or whether they are

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6 https://image-net.org/challenges/LSVRC/announcement-June-2-2015

7 As an example, for achieving scores that are comparable to top-ranked models on the GLUE benchmark, there are a series of extremely specific actions and setups used in pretraining/fine-tuning that are known as “standard GLUE tricks” introduced/used by submissions to the leaderboard [Liu et al., 2019, Yang et al., 2019, Lan et al., 2019]. Check the Pre-training and fine-tuning details in the appendix of [Clark et al., 2020].
available at all) is again a form of lottery, in which models that cannot incorporate any of the earlier tricks are significantly disadvantaged.

6 Rigging the lottery: making your own luck

For some tasks and problems, there are already standard benchmarks and established setups that are followed by most of the community. However, for some others, inconsistencies in the employed benchmarks or reported metrics can be observed. This diversity of evaluation paradigms makes comparisons between publications extremely difficult. Alternatively, in some cases, there is simply no standard benchmark or setup, either because the problem is still young, or because there has never been an effort to unify the evaluation. Sometimes this is due to the high computational cost of proper evaluation, like when reporting variance over multiple random seeds is important [Bouthillier et al., 2019]. While in other instances, the root cause is of behavioral nature, where researchers prefer to showcase only what their method shines at - oftentimes to avoid negative reviews, unsuccessful experiments, although performed, are simply not reported. Here, we study two known examples of this issue, which we refer to as rigging the lottery.

6.1 Recommender systems and benchmark inconsistencies

The goal of recommender systems (RecSys) research is to build systems that understand users’ interests and behaviors towards a list of potential items to recommend. Deep learning-based recommender systems are trained on large quantities of implicit user feedback, such as users clicking on or otherwise interacting with items [Zhang et al., 2019].

Unlike the fields of NLP or CV, there are no well-established evaluation setups for recommender systems that provide canonical ranked lists of model performance. While there has been a famous Netflix prize challenge[^8], this dataset has not been extensively used in academic research or for benchmarking new models. Moreover, even popular datasets like MovieLens [Harper and Konstan, 2015] or Amazon Reviews [He and McAuley, 2016] generally do not have a canonical test split, metric or evaluation method. Therefore, it is still quite unclear about which modern RecSys method one should adopt, as model comparisons are difficult to interpret [Dacrema et al., 2019].

Furthermore, RecSys evaluation is also very challenging for a number of reasons. (i) Different recommendation platforms tackle slightly different problems (e.g. retrieval [Yi et al., 2019], ranking [Pei et al., 2019]), or multitask learning ([Zhao et al., 2019]), and each requires their own evaluation setup. (ii) As is common for user interacting systems, user’s reaction towards different algorithms can be different. Constructing offline datasets of user behaviors from an existing system creates an off-policy evaluating challenge [Swaminathan et al., 2016]. (iii) A real-world recommendation system trains on billions of users and items, the scale of user-item interactions makes it extremely difficult to create a complete dataset containing all possible user-item interactions [He et al., 2016b]. As a result, evaluation setups in many recommender system papers tend to be arbitrary.

There exists a small number of public datasets (see Appendix F), such as MovieLens [Harper and Konstan, 2015] or Amazon Product Review [He and McAuley, 2016] that are commonly used for evaluating recommender systems. However, even these datasets are tweaked differently in various publications, leading sometimes to contradictory results [Rendle et al., 2020; Zheng et al., 2019]. For example, some papers use Hit Ratio and NDCG as evaluation metrics [He et al., 2017], while others resort to using Recall@K [Zheng et al., 2019]. Interestingly, in this particular example, the same methods reverse their performance when a different metric is used. Holdout test sets can also be created differently, with some papers for example using random split [Beutel et al., 2017] and others using an out-of-time split [Zhang et al., 2020].

While the majority of this paper discusses cases where a standardized benchmark may lead to biased progress in the ML community, here we instead discuss the exact opposite - implications of having no consensus datasets or evaluation setups. Having no unified benchmark for the community to make progress on has numerous flaws. To name a few, (i) this hinders progress in the field, while possibly (ii) creating an illusion of progress. It is not surprising that under these circumstances researchers (potentially unknowingly) tend to find good experimental setups that fit their models instead of the other way around.

[^8]: https://netflixprize.com/index.html
6.2 ALE and evaluation setup inconsistencies

An example of a benchmark that hundreds of papers have used as a testbed, while simultaneously employing a number of distinct experimental evaluation protocols is the Arcade Learning Environment (ALE) which is based on Atari 2600 games [Mnih et al., 2013]. The main aspects in which evaluation setups in different papers using ALE diverge are different metrics used for summarizing agent performance, and the different mechanisms used for injecting stochasticity in the environment [Machado et al., 2018].

For example, different assumptions can be made for determining episode termination. While in some publications episodes terminate when the game is over [Bellemare et al., 2013, Hausknecht et al., 2014, Liang et al., 2015, Lipovetzky et al., 2015, Martin et al., 2017], while others papers choose to terminate the training episodes for a subset of the games when the agent loses a life [Mnih et al., 2016, Nair et al., 2015, Wang et al., 2016, Van Hasselt et al., 2016].

Another major disagreement in evaluation strategies for ALE, also comes from using different parameters used for the evaluation setup. For example, some papers use a non-default value for the skipframe parameter in their baseline models [Mnih et al., 2015]. Alternatively in some publication, methods are evaluated for each $2 \times 10^5$ frames [Pritzel et al., 2017], while in others methods are evaluated every $10^6$ frames [Mnih et al., 2013, 2016]. Another observation is the difference between the number of games used in the evaluation setups. For instance, [Mnih et al., 2015] use 49 games, while [Van Hasselt et al., 2016, Wang et al., 2016] use 57. Moreover, for the hyper-parameter tuning, sometimes papers use the entire suite of games as the validation set [Bellemare et al., 2013], while in other cases hyperparameters are optimized on a per-game basis [Jaderberg et al., 2016].

Yet another inconsistency is in reporting the results in terms of the variety of different summary statistics used to describe them, which makes direct comparisons between ideas difficult [Machado et al., 2018]. To make matters worse sometimes sufficient statistics to make a judgment on the quality of the models are not provided. As an example, in [Bellemare et al., 2013], the main results are reported as the average performance of the method as well as the best run without mentioning the variance or the standard error of the mean. This is particularly problematic for reinforcement learning, where it has been shown that often the variance between runs can be so large as to create statistically different distributions just by varying random seeds [Henderson et al., 2018].

The final contentious aspect of ALE that we highlight is the way that various publications choose to inject stochasticity into the environment. ALE is fully deterministic, thus it is possible to get good scores by simply memorizing the “right” action sequence, rather than learning to make good decisions in a variety of game scenarios (i.e. learning an open-loop policy). With this in mind, to encourage and evaluate agent robustness, various ideas were developed to add forms of stochasticity to ALE [Bellemare et al., 2013]. Unfortunately, these methods are not necessarily consistent with each other.

7 What can we do?

While the previous sections of the paper focused on the challenges that arise from the lottery-like interaction between ML benchmarks and the research community, here we would like to show that there are reasons to be optimistic about future developments in this regard. We present suggestions for improving the idea benchmarking process in ways that make it less of a lottery. These recommendations can be also framed as checklists for different parts of the process, like making benchmarks, using benchmarks, evaluation of a new ideas. Appendix G presents a proposed benchmarking checklist for the review process.

7.1 Investing in making guidelines

Assessing the quality of ML algorithms is a complex task, not the least due to the diversity of aspects and contexts in which algorithms are compared (e.g. based on performance, theoretical computational

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9 When predicting the action given the state, it is often done for every $k$-th frame, where $k$ is the skipframe hyper-parameter.

10 Similar to the reproducibility checklist [https://www.cs.mcgill.ca/~jpineau/ReproducibilityChecklist.pdf] [Dodge et al., 2019].
efficiency, efficiency on specific hardware, fairness w.r.t. selected attributes, etc). This adds an extra dimension of complexity to the already difficult task of setting up a benchmark.

Given the pitfalls discussed in this paper, it is not impossible to imagine a benchmark being set up that inadvertently misrepresents the progress and causes an ill-fated shift in research questions and priorities for (a part of) the research community. The risks of this happening can be minimized by standardizing the recipe for creating new benchmarks. We believe that the development of shared standards and guidelines for future benchmarks can be the first step towards solving the “rigging the lottery” problem described in Section 6. We discuss such guidelines in the remainder of this section.

**Benchmark building blocks.** The ultimate goal of a benchmark is to provide an opportunity to learn more about a specific problem, which is achieved by quantifying progress made on that problem. So the problem or the task itself has a central role when designing a new benchmark. Underinvestment in a clear definition of the problem can offset the outcome of the comparisons, while having a clear set of objectives can assist in identifying the right datasets and usage guidelines for the benchmark.

Datasets form the core of the benchmark, and great care should be taken to collect or select data for the benchmark that appropriately captures the problem of interest, has the “right” dataset size, and faithfully captures the distribution of the larger population.

Finally, quantifiable progress necessitates the use of shared metrics, which similarly are an essential part of a benchmark. Their choice requires great care, as at the end of the day, metrics reflect the progress and dictate future research directions.

The gola, datasets, and metrics form the essential building blocks of a benchmark (suite), when chosen without deliberation, can respectively feed into the community (Section 4) and task selection biases (Section 3) and rigging the lottery (Section 6) behaviours.

**Guidelines for creating benchmarks.** Given this, we believe that investing more into shared guidelines for creating new benchmarks can be extremely beneficial to the long-term health of the research community. In our view, such guidelines should include the current best practices and aspects that require special attention; and should highlight potential concerns for issues that may emerge in the future when different models and algorithms are applied to the benchmarks.

Fortunately, there have been some efforts in providing guidelines and best practices for making new benchmarks. For example, Zhang [2021] discusses the need for how robotic warehouse picking benchmarks should be designed to assess the ability of robots in terms of their out of the box performance (i.e. their success rate the first time a new item or unknown scene is encountered), learning speed (i.e. the time required to adapt to new experiences, objects or scenarios) and learning potential (i.e. ability to master new experiences), and provides tips on object selection, scene design, execution, and type of analysis that should be done for proper evaluation. Kiela et al. [2021] proposed a framework for benchmarking in NLP that unifies dataset creation, model development, and model assessment, in a dynamic way with humans and models in the loop. Although their proposal aims at addressing issues with traditional static benchmarks, it also sets clear standards for making new tasks and benchmarks. Denton et al. [2020] look at the dataset construction process with respect to the concerns along the ethical and political dimensions of what has been taken for granted, and discuss how thinking about data within a dataset must be holistic, future-looking, and aligned with ethical principles and values. Bender and Friedman [2018] also proposed using data statements for NLP datasets in order to provide context that allows users to better understand how experimental results on that dataset might generalize, how software might be appropriately deployed, and what biases might be reflected in systems built on the software.

Another issue that is rather common in many of the ML benchmarks is that the top-scoring models are often overly complex or specialized. Sometimes they are ensembles of different methods or there are fancy techniques involved in the training process. Sometimes these complex methods grand-father the benchmark by staying at the top position for a long time, which might kill the potential of elegant models having their moment of fame and get the chance of shining and being picked by the researchers as good starting points. One idea to address this issue can be rewarding simplicity and ease of adaptation, and different benchmarks and tasks, there may be ways to define a quantitative metric to be taken into account when ranking models. From a more general point of view, one of the main issues here is the assumption that there is a single ideal model, while the appropriate model is, in fact, a function of context. In other words, dimensions considered for ranking models are inherently dynamic and can change as the application changes. Thus besides simplicity, many factors can play a role in choosing the best model, given the requirements of the context.
In Section 6, we pointed out that sometimes the blocking factor or conduction rigorous evaluation is the high computational costs, in particular for the academic environment. For instance, analyzing all possible sources of variance in the performance is prohibitively expensive. As a potential solution to this problem, the community can invest more in setting up initiatives like reproducibility challenge\footnote{For instance https://paperswithcode.com/rc2020 https://reproducibility-challenge.github.io/iclr_2019/ or https://reproducibility-challenge.github.io/neurips2019/ https://2020.emnlp.org/blog/2020-05-17-write-good-reviews.} or specialized tracks at conferences that offer help in terms of expertise, infrastructure, and computational resources for extensive evaluation to the papers submitted to that conference.

**Guidelines for benchmark usage.** Besides the necessity of making guidelines for “how to make new benchmark”, it is important to have clear guidelines for “how to use a benchmark”, which for instance includes the exact setup that the benchmark should be used for evaluation or how the results should be reported. This would be a great help with reducing the instances of rigging the lottery prevalent in some domains (Section 6).

Most modern benchmarks have a “how to” section that focuses on the technical aspects, such as loading the data, or installing helper libraries, and using utility functions, running the baselines or the evaluation scripts, or submitting entries to the leaderboard. This is very helpful with getting started with a new benchmark, but is insufficient to prevent inconsistencies between publications in the adapted setups or the way results are presented. Such inconsistencies make it difficult to understand the progress in the field based on these benchmarks. We believe that these “how to” sections can be expanded into guidelines providing a set of recommendations for a process to follow when using the benchmark in order to avoid shortcuts that undermine the validity of findings. Furthermore, because in some cases, like those that the method is supposed to work with open data, the difficulty of using the default benchmark setup is the main cause for researchers to diverge from that setup, the guidelines could also focus on establishing standard methodologies for principled comparison and analysis of experimental results (see also Section 7.2).

Top scoring approaches on any benchmark are often overly complex, overly-specialized models, heavy multi-scale ensembles, fancy training techniques, etc. How do we organize benchmarks/metrics for the "simplest baseline that just works".

There are also several efforts targeting this goal. For instance, Albrecht et al. [2015], Machado et al. [2018] propose specific standards for the ALE benchmark (discussed in Section 6.2) to have more comparable numbers across different baselines. There is also literature highlighting the issue of reporting a single number as the quality of a model, which encourages blindly optimizing for climbing a leaderboard without taking other aspects into account. Ethayarajh and Jurafsky [2020] argue against ranking models merely based on their performance and discuss the example of a highly inefficient model that provides little utility to practitioners but ranks well on leaderboards. To shed light on such instances, they propose to always report model size, energy efficiency, inference latency, and metrics indicating model robustness and generalization to the out-of-distribution data. Gebru et al. [2018] proposed that every dataset be accompanied by a datasheet that documents its motivation, composition, collection process, recommended uses, etc with the goal of increasing transparency and accountability, mitigating unwanted biases in ML systems, facilitating greater reproducibility, and helping researchers and practitioners select more appropriate datasets for their chosen tasks.

Another important problem that can benefit from established regulation is the hyper-parameter tuning budget used by researchers to improve their model performance. Spending enough time and compute to precisely tune hyper-parameters of the model or the training process can improve the results a great deal [Li et al., 2018, Bello et al., 2021, Steiner et al., 2021]. Given that, a guideline on limiting the budget for the hyper-parameter tuning can curb the improvements that are solely based on exhausting hyper-parameter search and gives a chance to have comparisons that are tied less to the computational budget of the proposing entity, but more to the merits of the methods themselves.

**Guidelines for conferences and reviewers.** There have been attempts to ameliorate the problems related to the benchmark lottery, especially its community biases and the statefulness aspects (Sections 4 and 5). For example, NLP conferences have specially called out “not being SOTA” as an invalid basis for paper rejection.\footnote{We believe it is possible to leverage education through the review process in order to alleviate many negative aspects of benchmark lottery.}
As discussed in Bouthillier et al. [2021], obtaining such evidence amounts to running multiple trials. As an example, we can make sure that in the review process, scores on a particular benchmark are not biased in favor of the new model. The presence of established benchmarks and metrics alone does not necessarily lead to a steady improvement of research ideas; it should also be accompanied by rigorous procedures for comparing these ideas on the said benchmarks. For example, Armstrong et al. [2009] discuss the importance of comparing improvements to the strongest available baselines, and how the lack of such practice was responsible for a decade of stagnation experienced by the field of information retrieval in the early 2000s. Luckily modern day ML research largely overcame this issue by setting up strong baseline expectations and incentives for making research easily reproducible (e.g. citations and general interest in publications sharing source code). However, the question of how do we know that if a new model B is significantly better than its predecessor model A remains anything but solved 10 years later [Lin et al., 2021].

**Benchmark results as random samples.** Machine learning models are usually trained on a training set and evaluated on the corresponding held out test set, where some performance metric $m$ is computed. Because model training is subject to sources of uncontrolled variance, the resulting metric $m$ should be viewed as a single sample from the distribution describing the model’s performance. Because of that, deciding which of the two models is better based on point estimates of their performances $m_A$ and $m_B$ may be unreliable due to chance alone. Instead distributions of these metrics $p(m_A)$ and $p(m_B)$ can be compared using statistical significance testing to determine whether the chance that model $A$ is at least as good as model $B$ is low, i.e. $p(A \leq B) < \alpha$ for some $a$ priori chosen significance level $\alpha$. Estimation of $p(A \leq B)$ forms for the crux of statistical significance testing. It can be done either by using parametric tests that make assumptions on distributions $p(m_A)$ and $p(m_B)$ and thus often need fewer samples from these distributions, or by using non-parametric tests that rely on directly estimating the metric distributions and require more samples.

The popularity of standardized benchmarks and exponential growth in the amount of research that the ML community has experienced in recent years exacerbate the risk of inadvertently misguiding research through lax standards on declaring a model as an improvement on the SOTA. Indeed, if point estimates are used in place of statistical significance testing procedures, sampling $m_A' \sim p(m_A)$ and $m_B' \sim p(m_B)$ such that $m_A' > m_B'$ is only a matter of time, even if performance of the two models is not actually different. Note that this is not the same as the issue described in Section 5 but could instead be thought of as winning a lottery if you purchase enough lottery tickets.

**Beyond a single train-test split.** Unfortunately, researchers rarely go through the process of collecting strong empirical evidence that model $B$ significantly outperforms model $A$. This is not surprising. As discussed in Bouthillier et al. [2021], obtaining such evidence amounts to running multiple trials of hyper-parameter optimization over sources of variation such as dataset splits, data ordering, data augmentation, stochastic regularisation (e.g. dropout), and random initialization to understand the models’ variance, and is prohibitively expensive. If studied at all, mean model performance across several random parameter initializations is used for declaring that the proposed model is a significant improvement. This is vastly sub-optimal because dataset split contributes the most to model variance compared to other sources of variation [Bouthillier et al., 2021]. However, providing multiple dataset splits to estimate this variance is not standard practice in benchmark design.

Benchmarks typically come with a single fixed test set, and thus could even be said to unintentionally discourage the use of accurate statistical testing procedures. This is particularly problematic for mature benchmarks (i.e. Stage 4; see Section 2.1), where the magnitude of model improvements may become comparable to the model variance. Systematic variance underestimation may lead to a series of false positives (i.e. incorrectly declaring a model to be a significant improvement) that stall research progress, or worse - lead the research community astray by innovating on “improved overfitting” in

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1. [https://neuripsconf.medium.com/what-we-learned-from-neurips-2020-reviewing-process-e24549e0e382](https://neuripsconf.medium.com/what-we-learned-from-neurips-2020-reviewing-process-e24549e0e382)

2. Although Bouthillier et al. [2021] also propose a pragmatic alternative to the exhaustive study of all source of variation.
place of algorithmic improvements. Going forward, one way of addressing this limitation is to design benchmarks with multiple fixed dataset splits. As an added benefit, model performance reported across such standardized splits would also enable the application of a variety of statistical tests not only within the same study, but also across publications.

Contrary to the above concern, a recent study of the progress on the ImageNet benchmark [Recht et al., 2019, Deng et al., 2009] found that in image classification overfitting on the test does not appear to be an issue despite the benchmark’s popularity. Recht et al. found that algorithms ranked consistently when re-trained on a different train-test split, and that observed drops in performance on the new test split could be explained by the “difficulty” of the test images rather than overfitting on the original test split. It may thus also be acceptable to take a shortcut and rely on a single train-test split for benchmarks so long as there are also periodic studies such as [Recht et al., 2019] that double check that the research community is still making progress. Having said that, the decision of whether such shortcut can be used should be made on a case-by-case basis, and there is no harm in producing multiple standardized train-test splits for a new benchmark even if a single split ends up being used.

**Benchmark design with statistical testing in mind.** The choice of a suitable statistical testing procedure is non-trivial. It must consider the distribution of the metric \( m \) that is being compared, the assumption that can be safely made about the distribution (i.e. whether a parametric test is applicable or a non-parametric test should be used), the number of statistical tests performed (i.e. whether multiple testing correction is employed) and can also change as the understanding of the metric evolves [Demšar, 2006, Bouthillier et al., 2021, Lin et al., 2021]. We, therefore, recommend that benchmark design is accompanied by the recommendation of the suitable statistical testing procedures, including the number dataset splits discussed above, number of replicates experiments, known sources of variance that should be randomized, the statistic to be computed across these experiments and the significance level that should be used for determining statistically significant results. This would not only help the adoption of statistical testing for ML benchmarks, but also serve as a centralized source for best practices that are allowed to evolve. A detailed discussion of statistical testing is outside of the scope of this paper, and we refer interested readers to [Bouthillier et al., 2021, Dror et al., 2017] for an overview of statistical testing procedures for ML.

**Beyond a single dataset.** Often we are interested in understanding whether model \( B \) is significantly better than model \( A \) across a range of tasks. These kinds of comparisons are facilitated by benchmarks that span multiple datasets (e.g. VTAB or GLUE). Already the question of what it means to do better on a multi-task benchmark is non-trivial due to the task selection bias (see Section 5) - is it sufficient for model \( B \) to do better on average; or should it outperform model \( A \) on all tasks? It is not surprising that the statistical testing procedures for such benchmarks are also more nuanced - the answer to this question leads to different procedures. It is unclear whether the average metric across datasets, a popular choice for reporting model performance, is meaningful because the errors on different datasets may not be commensurable, and because models can have vastly different performance and variances across these datasets. For this reason, more elaborate procedures are required. For example, for the case when we are interested in seeing whether \( B \) outperforms \( A \) on average Demšar [2006] propose to ignore the variance on individual datasets and treat the model \( A \) and \( B \)’s performance across datasets as samples from two distributions that should be compared. They recommend that the Wilcoxon signed-rank should be used in such a setup; but the recommended can have limited statistical power when the number of datasets in the benchmark is small. Alternatively, for cases when we are interested in seeing whether \( B \) is better than \( A \) on all datasets Dror et al. [2017] propose to perform statistical testing on each of the datasets separately while performing multiple testing corrections. Here again the “right” statistical testing procedure depends on the benchmark, its composition, and the criteria for preferring one model over another; and we believe that the community would benefit if these questions were explicitly answered during benchmark design.

### 7.3 Rise of living benchmarks

Another major issue for many popular benchmarks is “creeping overfitting”, as algorithms over time become too adapted to the dataset, essentially memorizing all its idiosyncrasies, and losing the ability to generalize. This is essentially related to the statefulness of benchmarks discussed in Section 5.

Besides that, measuring progress can be sometimes chasing a moving target since the meaning of progress might change as the research landscape evolves.

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15 In fact for that reason it was not a popular choice until recently Demšar [2006].
This problem can be greatly alleviated by for instance changing the dataset that is used for evaluation regularly, as it is done by many annual competitions or reoccurring evaluation venues, like WMT\textsuperscript{16} or TREC\textsuperscript{17}. Besides that, withholding the test set and limiting the number of times a method can query the test set for evaluation on it can also potentially reduce the effect of adaptive overfitting and benchmark reuse. In a more general term, an effective approach is to turn our benchmarks into “living entities”. If a benchmark constantly evolves, for instance, adds new examples, adds new tasks, depreciates older data, and fixes labeling mistakes, it is less prone to “tricks” and highly robust models would find themselves consistently doing well across versions of the benchmark. As examples of a benchmark with such a dynamic nature, GEM is a living benchmark for natural language generation [Gehrmann et al., 2021] or Dynabench [Kiela et al., 2021] proposes putting humans and models in the data collection loop where we continuously reevaluate the problem that we really care about.

8 Epilogue

Ubiquitous access to benchmarks and datasets has been responsible for much of the recent progress in machine learning. We are observing the constant emergence of new benchmarks. And on the one hand, the development of benchmarks is perhaps a sign of continued progress, but on the other hand, there is a danger of getting stuck in a vicious cycle of investing in making static benchmarks that soon will be rejected due to the inflexible flaws in their setup, or lack of generality and possibility for expansion and improvements. We are in the midst of a data revolution and have an opportunity to make faster progress towards the grand goals of artificial intelligence if we understand the pitfalls of the current state of benchmarking in machine learning. The “benchmark lottery” provides just one of the narratives of struggling against benchmark-induced model selection bias. Several topics we touched upon in this paper are discussed in the form of opinions or with a minimum depth as a call for further discussion. We believe each subtopic deserves a dedicated study, like how to better integrate checks for ethical concerns in the mainstream evaluation of every existing benchmark, how to develop tools and libraries that facilitate the rigorous testing of the claimed improvements, or a deep investigation of the social dynamics of the review process and how to improve it. In the end, there are many reasons to be excited about the future - the community is continuously taking positive delta changes that contribute to fixing issues with measuring progress in the empirical machine learning.

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9 Appendix

A SuperGLUE: Ranking of models on different combinations of tasks

Figure 4 shows the performance of different models on different combinations of tasks in terms of their rank in the list of all models. The very top row in the heatmap is the ranking on the SuperGLUE (considering all tasks) and models on the x-axis are sorted based on their rank on “All” tasks. There is no strong pattern observable in the plot. For instance, the top ranked model on “All” does not newsreel perform best on other combinations of tasks.

Figure 4: Performance of different models on different combinations of tasks in terms of their rank on the SuperGLUE benchmark. Models are sorted on the x-axis based on their rank when evaluated on “ALL” tasks. We can observe that there no clear and strong pattern or correlation in different combinations of tasks compared to the full benchmark, indicating that there is no “best” model, while most of the time, the top-ranked model is simply taken as the absolute winner.
B SuperGLUE: Rank correlation between SuperGLUE score and individual tasks

Figure 5 shows the rank correlation between the SuperGLUE score and each of the 8 tasks in the benchmark, given 55 different models described in [Narang et al., 2021]. The average Kendall rank correlation of tasks with the SuperGLUE score is 0.648. This correlation is not perfect, but a more important point in the SuperGLUE benchmark is the disagreement of the top-k models across all tasks. This point is highlighted in Figure 1, where for instance, in 6 out of 8 individual tasks, we have different models as the winner. Thus using the mean score for a practitioner to choose a model to adapt it for their own application can be sub-optimal based on the context.

C SuperGLUE: Effect on relative ranking of models

A significant amount of work in machine learning modeling is to determine the relative performance of a set of different inductive biases or model architectures. We show that the ranking of models can be drastically altered based on the choice of the subset of the benchmark considered. In other words, the relative performance of models can be easily manipulated by task selection. In order to show this phenomenon, we select ten models, namely vanilla Transformers, Weighted Transformers, Funnel Transformers, Switch Transformers, Lightweight Convolutions, Dynamic Convolutions, Universal Transformers and Adaptive Softmax. The results are similarly obtained from [Narang et al., 2021].

Table 2 reports a sample of different selection of tasks. We show that for a different selection of tasks, the relative order of model performance is very different. Notably, models such as Universal, MoE, Adaptive Softmax, Switch all take turns to become the best performing model on multiple task configurations. Consequently, it is easy to see that with some manipulation of the benchmark configuration, it is easy to endorse and favor the performance of one model over another.

| Tasks | Top-5 Performing Models (In Order) |
|-------|-----------------------------------|
| H     | Universal, Switch, Adaptive Softmax, Weighted, Vanilla |
| G     | MoE, Switch, Vanilla, Funnel, Universal |
| A, B  | Adaptive Softmax, Vanilla, MoE, Switch, Weighted |
| A, C  | MoE, Switch, Adaptive Softmax, Vanilla, Universal |
| D, H  | Switch, Universal, Adaptive Softmax, MoE, Weighted |
| B, E, H | Adaptive Softmax, Switch, MoE, Vanilla, Weighted |
| F, G, H | Switch, MoE, Adaptive Softmax, Universal, Vanilla |
| A, F, G | MoE, Switch, Vanilla, Adaptive Softmax, Vanilla |
| C, F, G, H | Switch, MoE, Adaptive Softmax, Vanilla, Universal |
| A, C, D, G | MoE, Switch, Adaptive Softmax, Vanilla, Universal |
| All   | Switch, MoE, Adaptive Softmax, Vanilla, Universal |

Optimistically, we also note that even under the notion of a lottery, not all models have equal odds. Models that perform poorly across all tasks generally tend to not have a chance to qualify for the Top-5 of any of the above benchmark configurations. We note that Lightweight convolutions, Dynamic Convolutions, and Transparent Attention never made it to any of the Top-5 rankings. Models such as Funnel and Weighted also make very limited appearances. In short, we show empirically that benchmark suites can do pretty well in filtering model architectures that do poorly on most tasks.
D VTAB: Agreement on top-ranked models across sub-categories and tasks

Figure 6: Disagreement of model rankings on the VTAB benchmark as a function of the number of selected benchmark sub-categories (3 sub-categories: Natural, Specialized, Structured) or tasks (19 different tasks).

Similar to Figure 1, we looked into the disagreement of Top-1, 2, and 3 models based on different combinations of three VTAB sub-categories as well as different combinations with sizes 1, 2, and 3 of VTAB tasks. The \( x \)-axis represents the number of sub-categories/tasks in each sub-selection and each line corresponds to a different value of \( k \) for the Top-\( k \) in the rankings. Points are labels as \( A/B \), where \( A \) is the number of unique model rankings and \( B \) is the total number of possible sub-category combinations for this subset size.

In Figure 6a, we can see that all categories disagree on the winning model (top-1) and there is no full agreement on the set of top-2 or top-3 models. We can see a similar disagreement between subsets of tasks in Figure 6b. For instance, out of 19 individual tasks (the subset of size 1), there are 12 different winners (top-1) model or looking at the subsets of size 2 tasks, there are 20 different winners. Note that although the disagreement portion is 20/171 is rather small, at the end of the day, we have 20 different models performing best in different situations, and taking a single model based on the VTAB score as the best one can be easily a sub-optimal choice for many scenarios.
### E LRA: Comparing the top-scoring models on all possible task subsets

Table 3 presents the Top-1, 2, and 3 ranked results based on all possible combinations of tasks in Long Range Arena benchmark. This in fact is the complete version of Table 1 in Section 3.1.3.

**Table 3: Top 3 performing models on LRA depending on which subset of tasks we select.**

| Task          | Best Model       | Rank-2 Model     | Rank-3 Model     |
|---------------|------------------|------------------|------------------|
| $t_1$ (Text only) | Linear Transformers | Performer | Transformer |
| $t_2$ (Retrieval only) | Sparse Transformers | BigBird | Longformer |
| $t_3$ (ListOps only) | Reformers | Synthesizer | Transformer |
| $t_4$ (Image only) | Sparse Transformer | Performer | Transformer |
| $t_5$ (Path only) | Performer | Linformer | Linear Transformers |

- $t_1$ + $t_2$: BigBird, Sparse Transformer, Transformer
- $t_1$ + $t_3$: Transformer, BigBird, Synthesizer
- $t_1$ + $t_4$: Linear Transformer, Performer, Transformer
- $t_1$ + $t_5$: BigBird, Transformer, Longformer
- $t_2$ + $t_3$: Sparse Transformer, BigBird, Transformer
- $t_2$ + $t_4$: BigBird, Sparse Transformer, Performer
- $t_2$ + $t_5$: Linformer, BigBird, Transformer
- $t_3$ + $t_4$: Transformer, Synthesizer, Longformer
- $t_4$ + $t_5$: Performer, Linear Transformer, Sparse Transformer

- $t_1$ + $t_2$ + $t_3$: BigBird, Transformer, Synthesizer
- $t_1$ + $t_2$ + $t_4$: Sparse Transformer, Transformer, BigBird
- $t_1$ + $t_2$ + $t_5$: Performer, Linear Transformer, Transformer
- $t_2$ + $t_3$ + $t_4$: Transformer, Longformer, Synthesizer
- $t_2$ + $t_3$ + $t_5$: BigBird, Transformer, Longformer
- $t_3$ + $t_4$ + $t_5$: BigBird, Transformer, Longformer

- $t_1$ + $t_2$ + $t_3$ + $t_4$ + $t_5$ (LRA Score): BigBird, Transformer, Longformer
Popular public benchmarks for evaluating recommend systems

Table 4 presents the list of publicly available datasets for recommender systems used by the community for evaluation.

| Dataset                              | Number of examples | Users | Items | Sparsity |
|--------------------------------------|--------------------|-------|-------|----------|
| MovieLens 1M                         | 1,000,209          | 3706  | 6040  | 95.53%   |
| Movielens 20M                        | 13,501,622         | 138,159 | 16,954 | 99.42%   |
| Amazon Product Review (Movies & TV)  | 505K               | 22,147 | 178,086 | 99.98%   |
| Amazon Product Review (Video Games)  | 46K                | 2,670  | 47,063 | 99.96%   |
| Yahoo Movies                         | 221,367            | 7,642  | 11,915 | 99.76%   |
| Pinterest                            | 1.5M               | 9916   | 55187  | 99.73%   |
| Xing                                 | 1,450,300          | 65,347 | 20,778 | 99.89%   |
| Taobao                               | 100M               | 968K   | 4M     | 99.98%   |
| Last.FM                              | 42,346             | 1,872  | 3,846  | 99.41%   |
| Book-Crossing                        | 172,576            | 19,676 | 20,003 | 99.96%   |

1. https://grouplens.org/datasets/movielens/1m/
2. https://grouplens.org/datasets/movielens/20m/
3. https://jmcauley.ucsd.edu/data/amazon/
4. https://webscope.sandbox.yahoo.com/catalog.php?datatype=r
5. https://paperswithcode.com/dataset/pinterest
6. http://www.recsyschallenge.com/2017/
7. https://tianchi.aliyun.com/dataset/dataDetail?dataId=649
8. http://ocelma.net/MusicRecommendationDataset/
9. http://www2.informatik.uni-freiburg.de/~cziegler/BX/
G Benchmarking checklist for the review process

This section presents a proposal for a checklist that can be used in the review process with the hope of reducing the benchmark lottery effect. Note that as we discussed in the paper, the benchmark lottery effect can be rooted in various aspects. As an example, Gebru et al. [2018] provided a list of questions for the benchmark creation process that covers motivation of the benchmark, composition, collection process, and recommended uses. Such data can be also framed as checklists for benchmark creation.

### Benchmarking checklist for reviewers and area chairs

| Question                                                                                                                                                                                                 | 
|----------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
| □ If there is written dissatisfaction about the author’s choice of baselines, tasks, or benchmarks in the reviews, are there rationals beyond the fact that these requested datasets are “must-have” benchmarks? |
| □ Are the reviews considering potential benefits like efficiency, fairness, and simplicity of the proposed model outside the commonly evaluated performance metrics (e.g., accuracy)? |
| □ Are there any negative points in the reviews due to the paper proposing a method that deviates from the current trend/hype. If so, are there rational justifications for this? |
| □ If the reviews penalizing the paper due to the proposed method not performing well only on a subset of tasks, is there enough logical elaboration on such criticism in the reviews? |
| □ Are the reviews assessing the evaluation strategy in terms of studying the effect of different sources of variance (e.g., multiple splits, multiple random seeds, etc.)? |
| □ If there are analyses on statistical significance testing, are they appreciated in the reviews? If there is no such analysis, are there recommendations on this provided in the reviews? |
| □ If the paper is claiming SOTA or improvements over baselines on a benchmark, are there ablations on how much such improvement is secured by the tricks that are not tied to the main contributions? |
| □ If the reviews are asking for more experiments, analysis, or evaluation on more benchmarks, are the potential blockers are considered for such requests? E.g. those experiments being out of reach in terms of computing budget (pre-training or extremely large datasets). |
| □ If the paper is proposing a new idea while deviating from the common paradigms, is the “out of the hype” thinking valued in the reviews as opposed to solely recognizing SOTA performance? |