Pythia: A Suite for Analyzing Large Language Models Across Training and Scaling

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Abstract

How do large language models (LLMs) develop and evolve over the course of training? How do these patterns change as models scale? To answer these questions, we introduce *Pythia*, a suite of 16 LLMs all trained on public data seen in the exact same order and ranging in size from 70M to 12B parameters. We provide public access to 154 checkpoints for each one of the 16 models, alongside tools to download and reconstruct their exact training dataloaders for further study. We intend *Pythia* to facilitate research in many areas, and we present several case studies including novel results in memorization, term frequency effects on few-shot arithmetic performance, and reducing gender bias. We demonstrate that this highly controlled setup can be used to yield novel insights toward LLMs and their training dynamics. Trained models, analysis code, training code, and training data can be found at [url redacted for anonymity].

1. Introduction

Over the past several years, large transformer models have established themselves as the premier methodology for generative tasks in natural language processing (Brown et al., 2020; Sanh et al., 2021; Chowdhery et al., 2022). Beyond NLP, transformers have also made big splashes as generative models in areas as diverse as text-to-image synthesis (Ramesh et al., 2022; Crowson et al., 2022; Rombach et al., 2022), protein modeling (Jumper et al., 2021; Ahdritz et al., 2022), and computer programming (Chen et al., 2021; Xu et al., 2022; Fried et al., 2022). Despite these successes, very little is known about how and why these models are so successful.

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Critical to understanding the functioning of transformers is better understanding how these models behave along two axes: training and scaling. It is well established that there are regular and predictable patterns in the behavior of trained language models as they scale (Kaplan et al., 2020; Henighan et al., 2020; Hernandez et al., 2021; Mikami et al., 2021; Pu et al., 2021; Sharma & Kaplan, 2020; Ghorbani et al., 2021), but prior work connecting these "Scaling Laws" to the learning dynamics of language models is minimal. One of the driving reasons for this gap in research is a lack of access to appropriate model suites to test theories: although there are more publicly available LLMs than ever, they do not meet common requirements for researchers, as discussed in Section 2 of this paper. A notable counterexample is the simultaneous work Xia et al. (2022), which studies the evolution of the OPT (Zhang et al., 2022) models over the course of training. However, that paper relies on model checkpoints not available to people outside of Meta, further emphasizing the importance of having publicly available model suites for scientific research.

In this paper we introduce *Pythia*, a suite of models ranging from 70M to 12B parameters designed specifically to facilitate such scientific research. The *Pythia* suite is the only publicly released suite of LLMs that satisfies three key properties:

1. Models span several orders of magnitude of model scale.
2. All models were trained on the same data in the same order.
3. The data and intermediate checkpoints are publicly available for study.

We train 8 model architectures each on both the Pile (Gao et al., 2020; Biderman et al., 2022) and the Pile after deduplication, providing 2 copies of the suite which can be compared.

We use these key properties of *Pythia* in order to study for the first time how properties like gender bias, memorization, and few-shot learning are affected by the precise training data processed and model scale. We intend the following

Model Size	Non-Embedding Params	Layers	Model Dim	Heads	Learning Rate	Equivalent Models
70 M	18,915,328	6	512	8	10.0×10^{-4}	—
160 M	85,056,000	12	768	12	6.0×10^{-4}	GPT-Neo 125M, OPT-125M
410 M	302,311,424	24	1024	16	3.0×10^{-4}	OPT-350M
1.0 B	805,736,448	16	2048	8	3.0×10^{-4}	—
1.4 B	1,208,602,624	24	2048	16	2.0×10^{-4}	GPT-Neo 1.3B, OPT-1.3B
2.8 B	2,517,652,480	32	2560	32	1.6×10^{-4}	GPT-Neo 2.7B, OPT-2.7B
6.9 B	6,444,163,072	32	4096	32	1.2×10^{-4}	OPT-6.7B
12 B	11,327,027,200	36	5120	40	1.2×10^{-4}	—

Table 1. Models in the Pythia suite and select hyperparameters. For a full list of hyper-parameters, see Appendix C. Models are named based on their total number of parameters, but for most analyses we recommend people use the number of non-embedding parameters as the measure of "size." Models marked as "equivalent" have **exactly** the same architecture and number of non-embedding parameters.

to be case studies demonstrating the experimental setups *Pythia* enables, and to additionally provide directions for future work.

Mitigating Gender Bias There is much work cataloging how language models reflect the biases encoded in their training data. However, while some work has explored finetuning's effects on bias in language models (Gira et al., 2022; Kirtane et al., 2022; Choenni et al., 2021), or the relationship between the corpus statistics and the measured bias (Bordia & Bowman, 2019; Van der Wal et al., 2022b), researchers have generally lacked the tools to study the role of the training data on the learning dynamics of bias in large language models of different sizes. To demonstrate what is now possible with Pythia, we analyze whether deliberately modifying the frequency of gendered terms in the pretraining data of a language model can have an impact on its downstream behavior and biases. We leverage the known pretraining data and public training codebase of our model suite, and counterfactually retrain models such that the last 7% and 21% of model training has a majority of pronouns modified such that their grammatical gender is feminine rather than masculine. We demonstrate that such interventions are successful at reducing bias measures on a targeted benchmark, and propose these counterfactual interventions and retrainability of portions of our models as a key tool for future study of the influence of training corpora on model behavior.

Memorization is a Poisson Point Process Building on the extensive literature on memorization in large language models (Carlini et al., 2019; 2021; Hu et al., 2022), we ask the following question: does the location of a particular sequence in the training dataset influence the likelihood of it being memorized? Leveraging *Pythia*'s reproducible dataloader setup we answer this question in the negative, and furthermore find that a Poisson Point Process is a very good model for the occurrence of memorized sequences over the course of training.

Emergence of the Impact of Pretraining Frequencies Recent work has identified the frequency of specific facts within a corpus as an important factor in how likely a model is capable of applying that fact in response to a natural language question (Razeghi et al., 2022; Elazar et al., 2022; Kandpal et al., 2022; Mallen et al., 2022). Existing work has been heavily dependent on the handful of models trained on public data, such as GPT-J (Wang & Komatsuzaki, 2021) and BLOOM (Scao et al., 2022), which lack frequent intermediate checkpoints, so none of these papers are able to look at the evolution of this phenomenon over the course of training. To address this gap in the literature, we examine how the role of pretraining frequencies changes over the course of training. We find that significant phase change occurs after 65,000 training steps (45% through training): the models with 2.8 billion parameters or more start to exhibit a correlation between task accuracy and occurrence of task-relevant terms which is not present in prior checkpoints and is largely absent from smaller models.

2. The Pythia Suite

Following the advice of Birhane et al. (2021), in this section we seek to explicitly document our values as well as our choices in designing and implementing *Pythia*.

2.1. Requirements for a Scientific Suite of LLMs

Pythia is envisioned as a suite for enabling and empowering scientific research on the capacities and limitations of large language models. After surveying the existing literature, we found no existing suites of models which satisfied all the following conditions:

Public Access Models are publicly released and are trained on publicly available data.

Training Provenance Intermediate checkpoints are available for analysis, all models are trained with the same data

ordering, and intermediate checkpoints can be linked with the exact data seen up to that checkpoint. Training procedure as well as model and training hyperparameters are well-documented.

Consistency Across Scale Models scaling sequences should have self-consistent design decisions that reasonably adhere to common practice for training state-of-the-art large models. Model sizes should cover a variety of scales across multiple orders of magnitude.

Table 2 provides our assessment of a number of popular language model suites along these criteria. We note that for "number of checkpoints" we go with the number of checkpoints by the model in the model suite with *the fewest checkpoints*. While some model suites (e.g., GPT-Neo, OPT, BLOOM) have a subset that have more available, for most research purposes this is insufficient. This is exacerbated by the fact that typically smaller models are the ones with more checkpoints; the only model suite from the above list whose largest model has more checkpoints than smaller ones is GPT-Neo.

2.2. Training Data

We train our models on the Pile (Gao et al., 2020; Biderman et al., 2022), a curated collection of English language datasets for training large language models that is popular for training large autoregressive transformers. This dataset has three major benefits over its competitors: first, it is freely and publicly available; second, it reports a higher downstream performance (Le Scao et al., 2022) than popular crawl-based datasets C4 (Raffel et al., 2020; Dodge et al., 2021) and OSCAR (Suárez et al., 2019); and third, it has been widely used by state-of-the-art models including GPT-J-6B (Wang & Komatsuzaki, 2021), GPT-NeoX-20B (Black et al., 2022), Jurassic-1 (Lieber et al., 2021)[1], Megatron-Turing NLG 530B (Smith et al., 2022), OPT (Zhang et al., 2022), and WuDao (Tang, 2021). We use the tokenizer developed by Black et al. (2022), which is a BPE tokenizer that is trained specifically on the Pile.

While we considered training on a multilingual corpus instead of a monolingual one, we ultimately opted against doing so for the following reasons:

1. While we are confident that we are generally aware of the contents and quality of the Pile, we cannot say the same for multilingual datasets. Existing massive multilingual datasets can be of dubious quality (Caswell et al., 2020; Kreutzer et al., 2021) and we do not feel qualified to vet existing multilingual datasets well enough to determine issues that may arise due to using them. ROOTS (Laurençon et al., 2022), the dataset that BLOOM (Scao et al., 2022) was trained on, was styled after the Pile and would potentially be a good candidate, but it was not publicly available when we started training our models.
2. As this framework is intended to be used as a baseline for future research, we feel it is important to stay close to currently accepted common practices. While the Pile is widely used for training English-language models, there is no equally widespread multilingual dataset. In particular, ROOTS has not been used to train models beyond BLOOM.
3. We do not have access to a multilingual evaluation framework that is anywhere near as comprehensive as Gao et al. (2021).

We train 2 copies of the *Pythia* suite using identical architectures. Each suite contains 8 models spanning 8 different sizes. We train one suite of 8 models on the Pile, and the other on a copy of the Pile after applying near-deduplication with MinHashLSH and a threshold of 0.87, following the advice that LLMs trained on deduplicated data are better and memorize less of their data (Lee et al., 2021). After deduplication, the deduplicated Pile is approximately 207B tokens in size, compared to the original Pile which contains 300B tokens.

2.3. Architecture

Our model architecture and hyperparameters largely follow Brown et al. (2020), with a few notable deviations based on recent advances in best practices for large scale language modeling (Black et al., 2022; Chowdhery et al., 2022; Zeng et al., 2022):

1. Brown et al. (2020) describes using sparse and dense attention layers in alternation, while we follow all subsequent work and use fully dense layers for our models.
2. We use Flash Attention (Dao et al., 2022) during training for improved device throughput.
3. We use rotary embeddings introduced by Su et al. (2021) and now in widespread use (Black et al., 2022; Chowdhery et al., 2022; Zeng et al., 2022) as our positional embedding type of choice.
4. We use the parallelized attention and feedforward technique and model initialization methods introduced by Wang & Komatsuzaki (2021) and adopted by (Black et al., 2022; Chowdhery et al., 2022), because it improves training efficiency and does not harm performance.

[1]While the paper discusses the Pile at length, it does not explicitly state that Jurassic-1 was trained on the Pile. We originally discovered this fact by executing data extraction attacks on the API, and confirmed with private communication with the authors.

	GPT-2	GPT-3	GPT-Neo	OPT	T5	BLOOM	Pythia (ours)
Public Models	●	◖	●	●	●	●	●
Public Data			●		●	◖	●
Known Training Order			●			◖	●
Consistent Training Order				●		◖	●
Number of Checkpoints	1	1	30	2	1	8	154
Smallest Model	124M	Ada	125M	125M	60M	560M	19M
Largest Model	1.5B	DaVinci	20B	175B	11B	176B	12B
Number of Models	4	4	6	9	5	5	8

Table 2. Commonly used model suites and how they rate according to our requirements. Further information can be found in Appendix D.

5. We use untied embedding / unembedding matrices, as prior work has suggested that this makes interpretability research easier (Belrose et al., 2023).

2.4. Training

We train our models using the open source library GPT-NeoX developed by EleutherAI (Andonian et al., 2021). We train using Adam (Zhang, 2018) and leverage the Zero Redundancy Optimizer (ZeRO) (Rajbhandari et al., 2020) to efficiently scale to multi-machine set-ups. We additionally leverage data parallelism (Goyal et al., 2017) and tensor parallelism (Shoeybi et al., 2019) as appropriate to optimize performance. We use Flash Attention (Dao et al., 2022) for improved hardware throughput.

The most notable divergence from standard training procedures is that we use a much larger batch size than what is standard for training small language models. According to prior work (McCandlish et al., 2018; Zhang et al., 2019; Kaplan et al., 2020; Brown et al., 2020; Hoffmann et al., 2022), using larger batch sizes is desirable, but it is not possible to train smaller ($<$ 1B parameter) models with such large batch sizes. Contrary to this literature, we find no convergence issues with using batch sizes $4\times$ to $10\times$ what is considered standard (0.5M tokens). Consequently, we use a batch size of 1024 samples with a sequence length of 2048 (2,097,152 tokens) for all models, in order to maintain consistency across all *Pythia* model training runs.

A large batch size is essential to training models quickly: in a regime where one is not bottlenecked by access to GPUs or high quality interconnect, doubling the batch size halves the training time. A maximum batch size therefore directly implies a *minimum* wall-clock training time and *maximum* number of compute-saturated GPUs. By inflating batch sizes beyond previous standards, we achieve wall-clock speed-ups of factors as large as $10\times$ compared with standard batch sizes on our smaller models (Table 3). We also note that our models still perform on par with widely used models of the same size like GPT-Neo (Black et al., 2021) or OPT (Zhang et al., 2022) (see Appendix E for plots on common benchmarks).

Model Size	GPU Count	OPT GPUs	Speed-Up
70 M	32	4	8×
160 M	32	8	4×
410 M	32	8	4×
1.0 B	64	16	4×
1.4 B	64	32	2×

Table 3. Models in the Pythia suite, number of GPUs used during training, and the number of GPUs (assuming the same utilization and microbatch size per GPU) we would have been able to use had we used OPT's batch sizes. All GPUs are A100s with 40 GiB VRAM.

We save model checkpoints at initialization and every 2,097,152,000 tokens (or 1,000 iterations), resulting in 144 checkpoints evenly spaced throughout training. Additionally, we save log-spaced checkpoints early in training at iterations $\{1, 2, 4, 8, 16, 32, 64, 128, 256, 512\}$. This gives a total of 154 checkpoints per model, far more than any other suite of publicly available language models.

We train all models for 299,892,736,000 $\sim$ 300B tokens. This equates to 1 epoch on the original Pile, and $\sim$1.5 epochs on the deduplicated Pile, which is 207B tokens in size. We refer to the models trained on the original Pile as "Pythia-xxx", where 'xxx' is the model's total parameter count rounded to 2 significant figures, and their counterparts trained on the deduplicated Pile as "Pythia-xxx-deduped".

2.5. Evaluation

While the primary focus of this work is to promote scientific research on the behaviors of large language models, and state-of-the-art performance is not necessarily a core requirement, we find that Pythia and Pythia (Deduplicated) perform very similarly to OPT and BLOOM models on a variety of NLP benchmarks. These results are presented in

Appendix E. We use the Language Model Evaluation Harness (Gao et al., 2021) to run evaluations on eight common language modeling benchmarks: OpenAI's LAMBDADA variant, PIQA, the Winograd Schema Challenge, WinoGrande, ARC (easy and challenge sets separately), SciQ, and LogiQA. We consistently find that Pythia and Pythia (Deduplicated) perform very similarly to OPT and BLOOM models.

2.6. Novel Observations in Evaluation

We find three interesting phenomena that run counter to the prevailing narratives in the literature. Firstly, we find that deduplication of our training data has no clear benefit on language modeling performance. This is consistent with the results of Black et al. (2022), but inconsistent with other papers. This may indicate that the upsampling of certain subsets of the Pile does not accord with conventional assumptions about duplicated data, or that the general tendency of deduplicated data to outperform non-deduplicated data is primarily a statement about the quality of the data used in other works. Secondly, we find that we achieve (equi-token and equi-parameter) performance on-par with OPT despite the use of parallel attention + MLP sublayers at all model scales. Both Black et al. (2022) and Chowdhery et al. (2022) state that this architecture choice causes a performance regression at scales < 6B parameters. Thirdly, we find a minimal and inconsistent "curse of multilinguality" (Conneau et al., 2020; Pfeiffer et al., 2022) for BLOOM. While BLOOM certainly underperforms other models on LAMBADA, PIQA, and WSC, it does not appear to do so on WinoGrande, ARC-easy, ARC-challenge, SciQ, and LogiQA. We interpret this as a sign that some of the existing literature on the curse of multilinguality may need to be revisited using more diverse evaluation benchmarks.

2.7. Public Release and Reproducibility

To ensure that our work is fully reproducible, we seek to only make use of codebases and dependencies that are freely and publicly available. As previously mentioned, we use the open source GPT-NeoX and DeepSpeed libraries for training. For evaluating our models we use the Language Model Evaluation Harness (Gao et al., 2021) and run all evaluations ourselves instead of copying claimed results from previous papers.

We release all of our models and checkpoints to the public under the Apache 2.0 license via the HuggingFace Hub (Wolf et al., 2019)[2]. We additionally release the code used for all evaluations and the raw benchmark scores generated on GitHub [3].

[2]link omitted for anonymity
[3]link omitted for anonymity

In addition to training our models on the public Pile dataset , we also provide a tool for downloading the pre-tokenized data files utilized by our dataloader in the GPT-NeoX library, as well as a script that can be used to reproduce the exact dataloader setup used by our models during training, so that the contents of each batch at each training step can be read out or saved to disk by researchers.

3. Case Studies

We perform three case studies in language modeling research that would not have been possible to perform using any pre-existing model suites. These case studies were chosen to cover a variety of topical domains and address small but important questions in their respective fields. We especially seek to leverage the public training data order to derive novel insights about these models that have not been previously studied.

3.1. How Does Data Bias Influence Learned Behaviors?

Large language models are typically trained on minimally curated human-authored data. While it is widely known that models typically learn the biases encoded in their training data, virtually nothing is known about the actual learning dynamics of how these biases develop throughout training. This is particularly concerning as one of the best established phenomena in the study of bias in deep learning models is *bias amplification*—the fact that social biases in deep learning models tend to be more extreme than those found in their training data (Zhao et al., 2017; Hirota et al., 2022; Hall et al., 2022). To mitigate the biases learned from data, previous works have used finetuning on balanced datasets to reduce the gender bias of language models with some success (Levy et al., 2021; Gira et al., 2022; Kirtane et al., 2022), yet little is known about the role of specific corpus statistics in the emergence of bias during pretraining.

We seek to investigate a counterfactual claim—if we were to train our models on a corpus with different properties, how would these models' properties change downstream? To test the effects of corpus statistics on the biases learned by language models, we repeat segments of pretraining on specific models, with altered corpus statistics. In particular, for the Pythia-70M-deduped, Pythia-400M-deduped, Pythia-1.4B-deduped, and Pythia-6.9B-deduped models, we take a checkpoint and optimizer state 21B tokens (7%) prior to the end of training, and resume training of the model such that it sees the exact same dataset and data ordering until the end of training, but with morphologically masculine pronouns replaced by their feminine counterparts. We also repeat this intervention for 63B tokens (21%) prior to the end of training on just the Pythia-1.4B-deduped model. We then measure model performance on the WinoBias (Zhao et al., 2018) benchmark and the English subset of the multilingual

CrowS-Pairs[4] to observe whether this altered pretraining data affects downstream gender bias. Neither of these benchmarks were originally intended for autoregressive language models or text generation, so we describe our modifications to the evaluation setups in Appendix B.1.

The controlled setup provided by Pythia—with precise access to the data samples seen during training—enables us to isolate the effect of the pronoun frequency by pretraining on the exact same data with only pronouns modified. If instead we chose to compare two different training datasets, we would change a large number of potential explanatory factors that we cannot control for. In fact, it has been suggested that even the choice of hyperparameters, such as the data ordering, can have an effect on the resulting bias (D'Amour et al., 2020). Therefore, without being able to resume pretraining on the exact same data in the exact same order, we could not be confident our experiment was indeed measuring only the effect of particular gendered terms' frequency.

For WinoBias, we do not see a clear effect of the intervention (see Appendix B.1). We suspect that this is because of our atypical setup on this benchmark. The poor results we observe on the accuracy reinforce our suspicion that the current implementation of WinoBias using PromptSource is not a reliable bias measure, and that comparing log-likelihoods as a classification problem would be more reliable than recording how often the model generates the exact desired answer. This is yet another example of the importance of testing the validity and reliability of bias measures (Van der Wal et al., 2022a; Bommasani & Liang, 2022; Orgad & Belinkov, 2022). In fact, the extensive availability of checkpoints, consistent training order, and retrainability that *Pythia* provides could be useful in assessing the *test-retest reliability* of existing bias measures (Van der Wal et al., 2022a).

Figure 1 shows the progression of the CrowS-Pairs gender bias metric and the effect of the interventions. We can clearly see a reduction in the bias as result of swapping the gendered pronouns in the last 7% or 21% of the training for all model sizes, but most prominently for the larger ones, although these are also more biased to begin with. We hypothesize that because larger models are better at modeling correlations and distributions within their corpora, their increased capacity causes features of bias to be more strongly or robustly learned. We also see that the interventions only lead to a marginal decrease in the model perplexity on LAMBADA (Paperno et al., 2016) (Appendix B.1), which demonstrates the effectiveness of the bias mitigation without hurting language modeling performance downstream to a large degree. Whether the noisiness of the progression reflects actual changes in the language model's bias or poor reliability of CrowS-Pairs is an open question we leave for future work.

[4]While previous works have found the original version of CrowS-Pairs (Nangia et al., 2020; Névéol et al., 2022) benchmark of questionable validity (Blodgett et al., 2021), Névéol et al. (2022) have revised the English dataset to take care of the raised concerns.

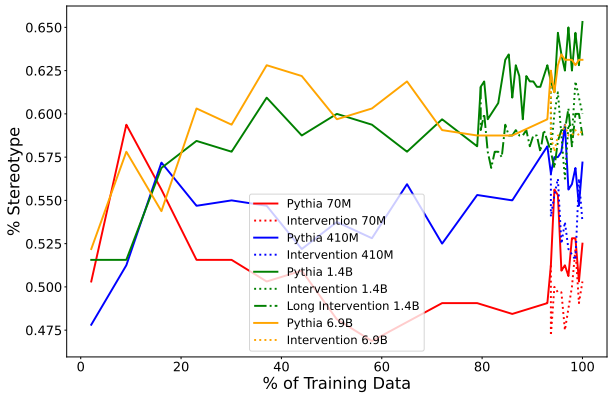

Figure 1. The CrowS-Pairs gender bias, shown as the percentage of times that the perplexity of the stereotyping sentence is lower than its less stereotyped counterpart (% Stereotype) for the Pythia models of different sizes at the end of training. We also show the effect of the gender swapping intervention on the measured bias for the partially retrained models.

We propose that performing such modifications to portions of language model training data, retraining, and comparing to the baseline model ("interventions") should be studied further for applications including but not limited to investigating bias amplification and devising new mitigation strategies. For example, while not explored in this case study, we think that the finegrained information that Pythia provides on the data seen during training could benefit the promising literature on influence functions to estimate the role of specific training samples on the encoded bias (Brunet et al., 2019; Silva et al., 2022).

3.2. Does Training Order Influence Memorization?

Although memorization in neural language models is widely studied, many basic questions about the dynamics of memorization remain unanswered. Prior work on the dynamics of memorization is generally limited to a few models in isolation (Jagielski et al., 2022; Elazar et al., 2022) or papers which train (but do not release) custom models for their studies (Tirumala et al., 2022; Hernandez et al., 2022). Carlini et al. (2022) studies the impact of scaling on memorization and repeatedly remark on the lack of suitable model suites for their study. They ultimately focus on the GPT-Neo model suite (Black et al., 2021; Wang & Komatsuzaki, 2021; Black et al., 2022), despite the fact that these models were trained on slightly different datasets, in different orders, and with inconsistent checkpointing.

In this experiment we test whether training order influences memorization. This is an explicitly theoretically-driven experiment: several authors realized that their mental model of transformers was that they work iteratively—by adding new information to a latent space and then processing the space as a whole to obtain a better representation. This mental model predicts that data encountered later in training will be memorized more, as the model has had less time to incorporate it more fully into its representation space. If true, this would potentially be highly useful for mitigating the memorization of sequences for which verbatim memorization would be undesirable, by intentionally modifying a model's training data order prior to training.

To test our hypothesis, we measure the memorization of an initial segment of each document in the training corpus. While there are several reasonable definitions of memorization, we use the one from Carlini et al. (2021) as it has received considerable attention in the literature (Yoon & Lee, 2021; Huang et al., 2022; Ginart et al., 2022; Ippolito et al., 2022; Biderman et al., 2023). In their context, a string is (k, ℓ)-memorized if prompting the model with a string of length k from the training data induces the model to generate the next ℓ tokens from the training data correctly. We choose $k = \ell = 32$ largely arbitrarily, and note that doing all reasonable pairs of (k, ℓ) would have a computational cost comparable retraining all of our models from scratch. To avoid potential covariate effects, we only use the first 64 tokens from each context seen during training.

Surprisingly, we find that a Poisson model fits the data extremely well, indicating that training order has little impact on memorization. This model implies that memorized sequences are not spaced more densely toward the beginning or end of training, and that between each checkpoint roughly the same number of memorized sequences can be found.

This finding is important for practitioners seeking to control which sequences are memorized by a model. It implies that one cannot simply place sequences that are undesirable to memorize at the beginning or end of training and successfully reduce the chance of memorization. However, we propose that a practitioner especially worried about the memorization of certain sequences place those sequences at the beginning of training, thus increasing the odds that the practitioner may observe prior to the completion of the training run that undesirable memorization behavior occurs in the partially-trained model.

3.3. Do Pretraining Term Frequencies Influence Task Performance Throughout Training?

Recent work has explored the effect of term frequencies in language model corpora on numerous tasks. Findings presented in Shin et al. (2022) demonstrate how the pretraining corpus can impact few-shot performance, while

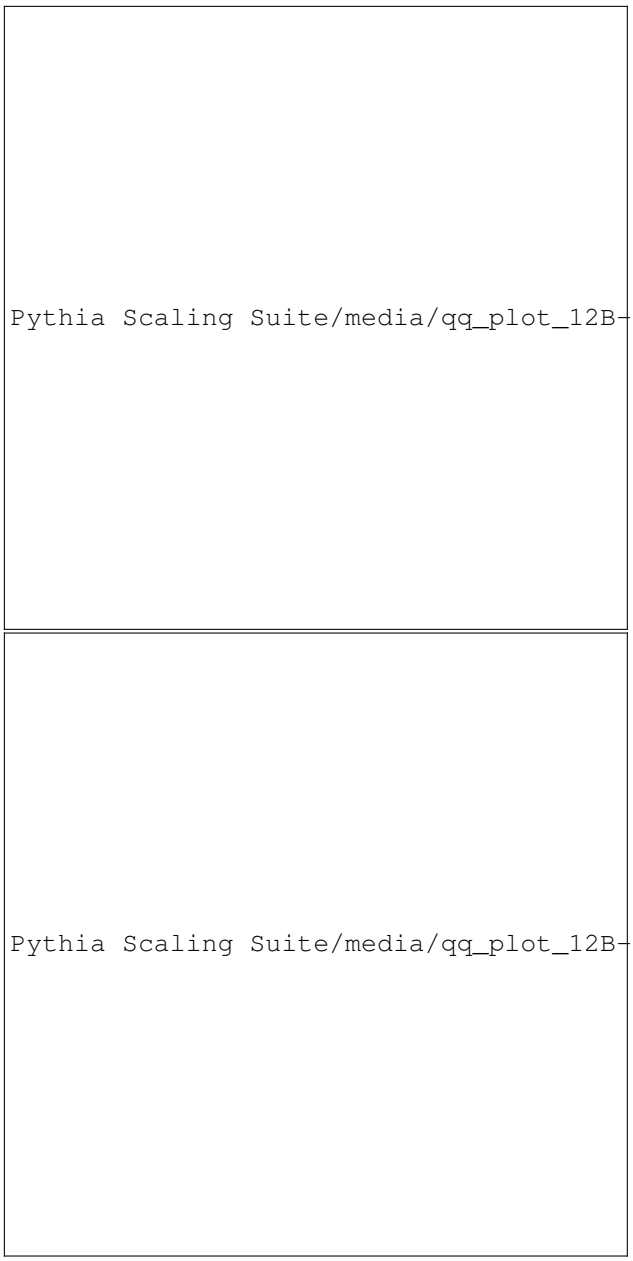

Figure 2. Quantile-Quantile plot of rate of occurrence of memorized sequences in 12B model compared to a Poisson Point Process, with (top) and without (bottom) deduplication. Color and dot size indicates number of points.

Razeghi et al. (2022) investigates how models are able to perform numerical reasoning from in a few-shot setting. By charting the performance of a arithmetic task given an input operand and the frequency in which it is found in the pretraining corpus, they concluded that accuracy tends to be higher for terms that are found more frequently compared to terms that are less frequent. Other works also suggest that the pretraining corpus has a significant impact on few-

shot behavior (Elazar et al., 2022; Kandpal et al., 2022). These works observe a correlational and causal relationship between the ability to answer factual questions and the frequency of salient entities found in the pretraining corpus. While the aforementioned works experiment with various model sizes, it is not yet studied when during training and at what model sizes this effect occurs. We further investigate this phenomenon across model checkpoints and model sizes by adapting arithmetic tasks of multiplication and addition Razeghi et al. (2022) and a QA task Kandpal et al. (2022) using natural language prompts evaluated over a set of k-shot settings. We calculate the relevant term frequencies for each checkpoint, which means counting through each subset of the pretraining corpus seen up to each model checkpoint. Model evaluation was performed on the deduped versions using the LM Evaluation Harness (Gao et al., 2021).

In adapting the arithmetic tasks in Pythia, the task consists of input operands $x_1 \in [0, 99]$ and $x_2 \in [1, 50]$ and an output y. The input operands are converted into a prompt with the prompt template *"Q:What is* x_1 *#* x_2*? A:"* with # being *"plus"* for addition and *"times"* for multiplication. We measure the accuracy of a prompt instance by checking the model's prediction against y. To measure the term frequency and task performance correlation, the average accuracy of all prompts with the same x_1 over all values of x_2 is mapped to the number of times x_1 is found in the pretraining corpus. In few-shot settings, we sample examples with digits that differ from the x_1 values being measured.

The QA task uses TriviaQA (Joshi et al., 2017) we use a simple template of *"Q:* x_1 *n A:* x_2*"* and map x_1 and x_2 with the question and answer pairs for the k-shot examples with the sa along with y being the possible answer set.

We follow the original experiment using 4-shots and evaluate both the training and the validation split of the dataset. Performance is averaged over a group of log-spaced bins.

After 65,000 steps (Figure 3), the models of sizes 2.8 billion parameters and above start to show strong links between average performance and the term frequencies, indicating that this is an emergent ability that happens in larger models. Models below this size rarely produce accurate results on the task despite being given up to 16 few-shot examples, suggesting that models below 2.8 billion parameters are not successful at learning the required pattern-matching. To further measure the impact of term frequencies, we calculate the performance discrepancy between the top 10% most frequent input operands and the bottom 10% least frequent input operands also following Razeghi et al. (2022) (see Table 4 in Appendix B.2). Performance affected by the number of terms in a pretraining corpora should have a wide gap. We report the performance gap for the arithmetic multiplication task and find that the performance gap widens over the course of training.

We are thus able to connect emergent phenomena over the course of training with term frequencies in pretraining. We hope that this analysis will inspire further follow-up work showing how pretraining data drives the acquisition and emergence of capabilities across more complex tasks. We additionally note that experimental setups similar to this case study were exactly why we developed the *Pythia* suite: though prior work has investigated term frequency's effects on a fully-trained model (Razeghi et al., 2022), it is only via accessing the ordering of the data and viewing statistics over all the data seen *thus far* by a model's partial checkpoint.

4. Conclusion

We release *Pythia*, a suite of language models trained with consistent data ordering and model architecture across multiple orders of magnitude of scale. We demonstrate how Pythia can be used to empower experiments at unprecedented levels of detail for a public model suite by presenting novel analyses and results on gender debiasing, memorization, and term frequency effects. We hope that these models and their dataset tooling will be broadly useful for a variety of practitioners, and recommend using the suite as a framework for novel experimental setups on LLMs.

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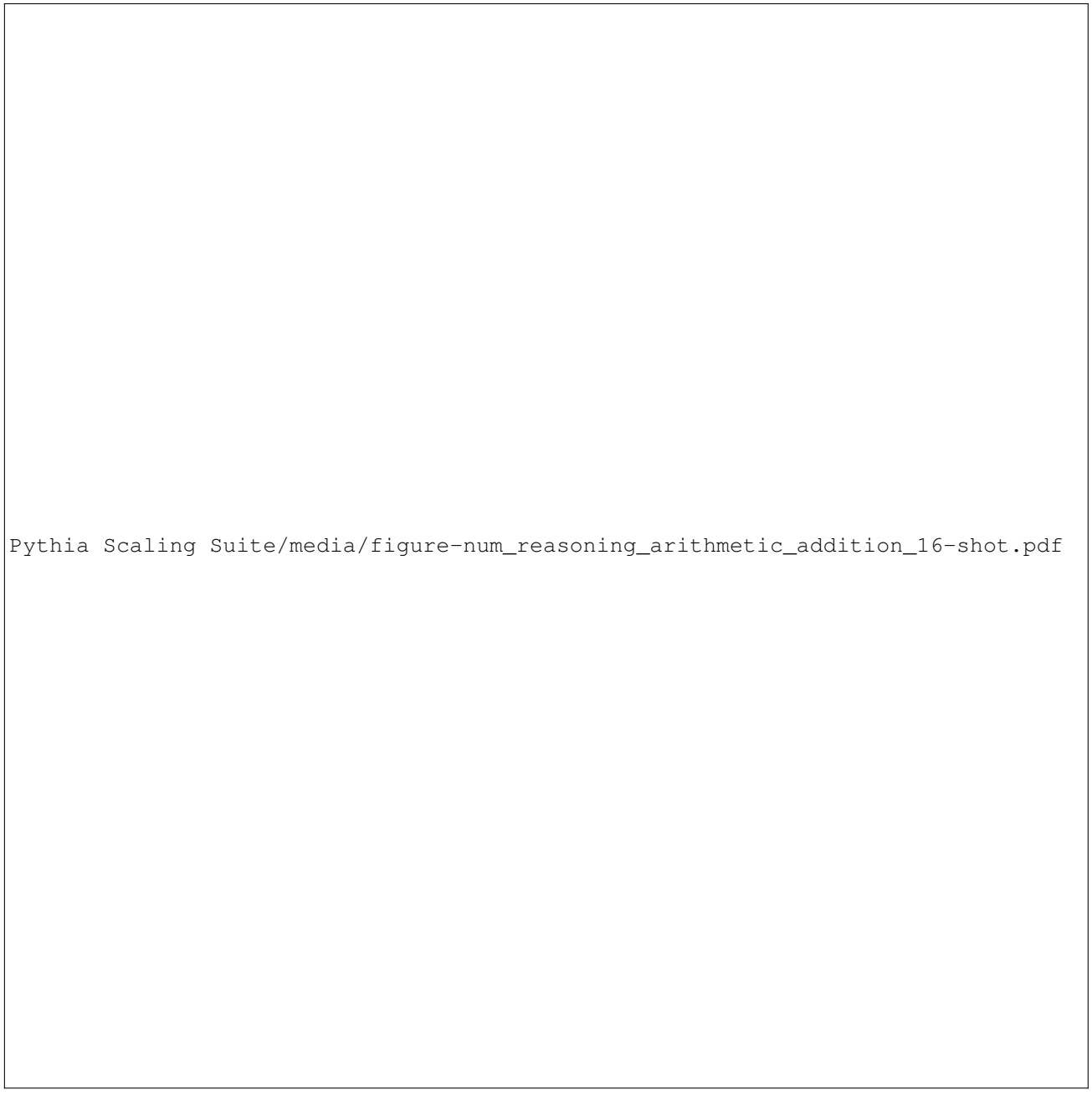


Figure 3. Accuracy of the arithmetic addition task with 16 shots, across various model sizes (divided by subfigure). For each model, multiple intermediate checkpoints (differentiated by color and their step number) are plotted. Each point represents the average accuracy (y-axis) of binned term frequency (x-axis).

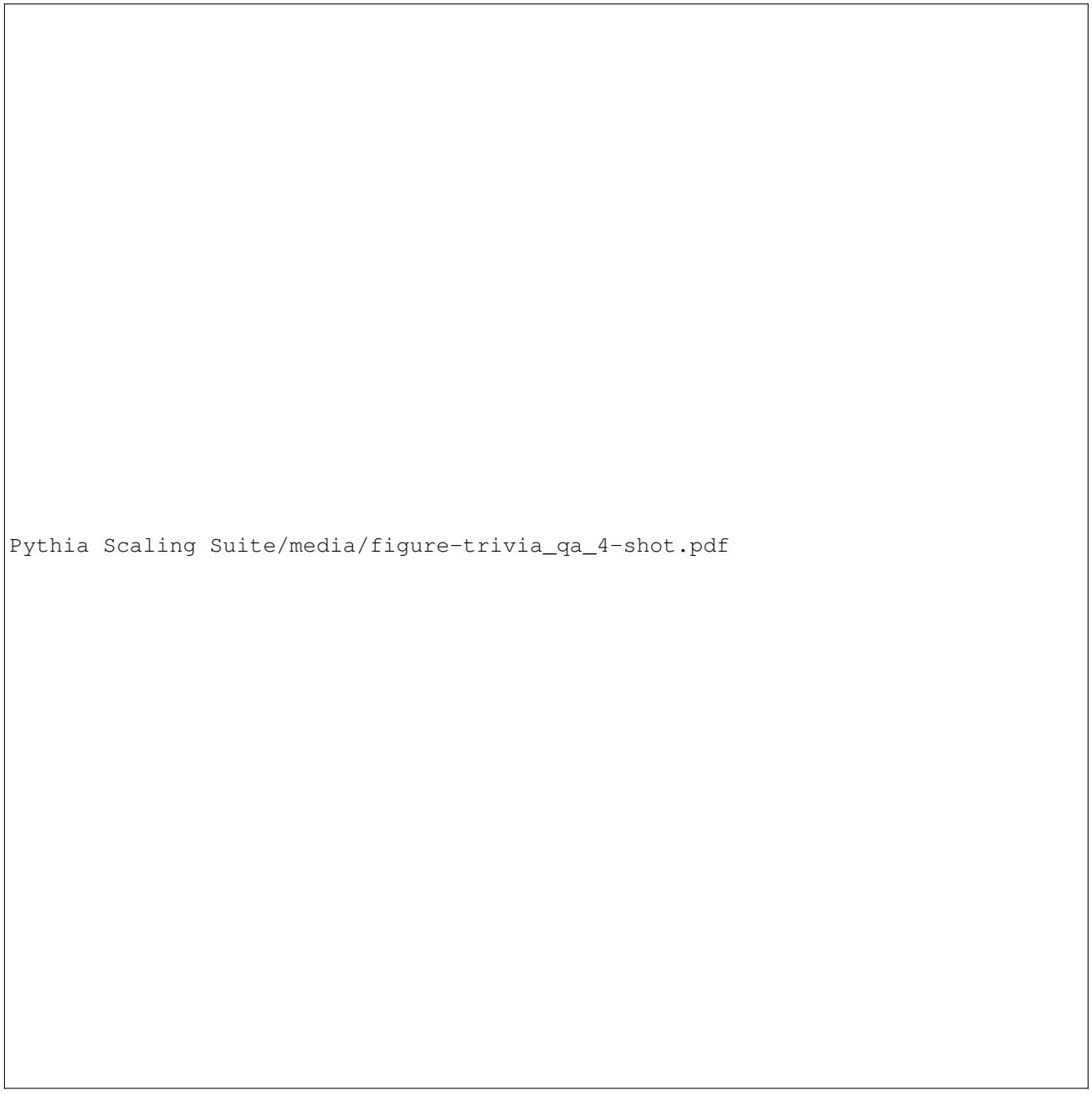


Figure 4. Accuracy on Trivia QA plotted againts the number of relevant entity counts found in a QA-pair. Each subfigure shows the impact of performance across various model sizes over multiple intermediate checkpoints. Each point represents the average accuracy (y-axis) of binned counts (x-axis).

Author Contributions

All authors other than the first two are listed in alphabetical order.

Stella Biderman Conceived, organized, and lead the project. Designed the experiments for the memorization and pretraining frequencies case studies. Lead the writing of the paper.

Hailey Schoelkopf Trained the models, wrote large portions of the paper, uploaded and converted all model checkpoints for hosting, and planned the gender bias case study.

Quentin Anthony Optimized the model implementation, advised the choice of hyper-parameters, and wrote the paper.

Herbie Bradley Carried out the WinoBias analysis and wrote portions of the gender bias case study.

Kyle O'Brien Conducted zero- and five-shot evaluations of several of the models on NLP benchmarks.

Eric Hallahan Evaluated the models on standard NLP benchmarks and authored most plots in the paper.

Mohammad Aflah Khan Helped in implementing the CrowS-Pairs evaluation and performed analysis on the results.

Shivanshu Purohit Optimized the model implementation, advised the choice of hyper-parameters.

USVSN Sai Prashanth Conducted the memorization case study, evaluated the models on standard NLP benchmarks and wrote the paper.

Edward Raff Advised on the project and wrote the paper.

Aviya Skowron Wrote documentation for the model suite and analysis, including the model card. Edited the paper.

Lintang Sutawika Conducted the experiments and analysis for the pretraining frequencies case study.

Oskar van der Wal Helped with the CrowS-Pairs evaluation and writing up the gender bias case study.

A. Corrections and Updates

Following the value of "doing science in the open" (Phang et al., 2022), we released a variety of artifacts over the course of training our models for the public to use. However, after this initial release of preliminary versions of the *Pythia* suite, we decided that in order to

The updated version of the *Pythia* suite features several small changes to hyperparameters in a redone version, detailed below:

- All model sizes are now trained with uniform batch size of 2M tokens. Prior, the models of size 160M, 410M, and 1.4B parameters were trained with batch sizes of 4M tokens, but in the course of training the initial suite we discovered that it was feasible to train all models with uniform batch size, though based on prior literature we had not been certain of this fact before performing our own experiments on batch size.
- We configured model checkpoint saving in order to obtain checkpoints at initialization (step 0) and steps {1,2,4,8,16,32,64,128,256,512} in addition to every 1000 training steps. This enables practitioners to use our new suite to study training dynamics and emergent behaviors early in training, as well as access the random weight initializations easily.
- Before retraining the suite, we received a contribution to our codebase integrating Flash Attention (Dao et al., 2022). Utilizing the Flash Attention greatly increased per-device throughput for the second set of training runs.

- We remedied a minor inconsistency that existed in the original suite: all models of size 2.8B parameters or smaller had a learning rate (LR) schedule which decayed to a minimum LR of 10% the starting LR rate, but the 6.9B and 12B models all used an LR schedule which decayed to a minimum LR of 0. In the redone training runs, we rectified this inconsistency: all models now were trained with LR decaying to a minimum of $0.1\times$ their maximum LR.

We did not expect these changes to significantly impact any experimental findings in the paper, and we reran all analyses and evaluations on the new models to confirm this was indeed the case. All experiments in the paper report results from this updated version of the suite. We chose to rerun the training runs in order to make the *Pythia* suite maximally useful to practitioners, and report this change for full transparency

B. Additional Plots for Case Studies

B.1. Gender Bias Interventions

Figure 5 shows the net bias and stereotype co-reference accuracy on the WinoBias benchmark across scale and Pythia checkpoints. We do not see a clear effect of either intervention in the accuracy or bias, and we also see a low absolute accuracy even at 6.9B scale. We suspect this is due to the PromptSource (Bach et al., 2022) prompt-based implementation and the use of exact match accuracy (see Section 3.1).

We also describe our modifications to the evaluation setups in the gender bias case study (see Section 3.1), as neither of the benchmarks were originally intended for autoregressive language models or text generation.

WinoBias is a coreference resolution benchmark testing how a model links gendered pronouns to stereotypical occupations for each gender (Zhao et al., 2018). WinoBias contains both pro and anti-stereotypical versions of these tasks (the latter created by swapping pronouns), and the bias of a model is calculated by subtracting the model's accuracy on the pro-stereotyped examples from the accuracy on the anti-stereotyped examples. To use this benchmark with our autoregressive language models, we use PromptSource (Bach et al., 2022) to prompt our models with templates: Given a sentence containing two occupations and a pronoun, the model is asked who the pronoun refers to. We then measure the *exact match accuracy*.[5] This formulation is different from the original WinoBias setup (Zhao et al., 2018), which measured the gender bias of older co-reference approaches such as rule-based systems that do not require prompting.

CrowS-Pairs is a stereotype benchmark that presents a model with two versions of a sentence: a stereotyped version and a version which is less stereotyping (Névéol et al., 2022). While the original task was designed for masked language models (Nangia et al., 2020), we measure the percentage of sentences for which the language model assigns a lower perplexity for the stereotyping sentence over the less stereotyping sentence. We evaluate our models only on the English subset for gender bias, since our models are monolingual and we intervene on gendered pronouns.

Figure 6 demonstrates the performance of different models in the *Pythia* suite on the LAMBADA Dataset (Paperno et al., 2016). The plots also show how intervening by swapping gendered pronouns does not lead to major dips in accuracy. Hence the interventions are successful in reducing bias while preserving the text understanding capabilities of the model.

B.2. Pretraining Term Frequency

[5]For example, to query the model for an occupation linked with the pronoun 'her', we might start with a sentence such as 'The mover greeted the librarian and asked her where the books were.', then append 'Here, what does her stand for?' before prompting the model with the concatenation. The target completion for the model is then 'the librarian'.

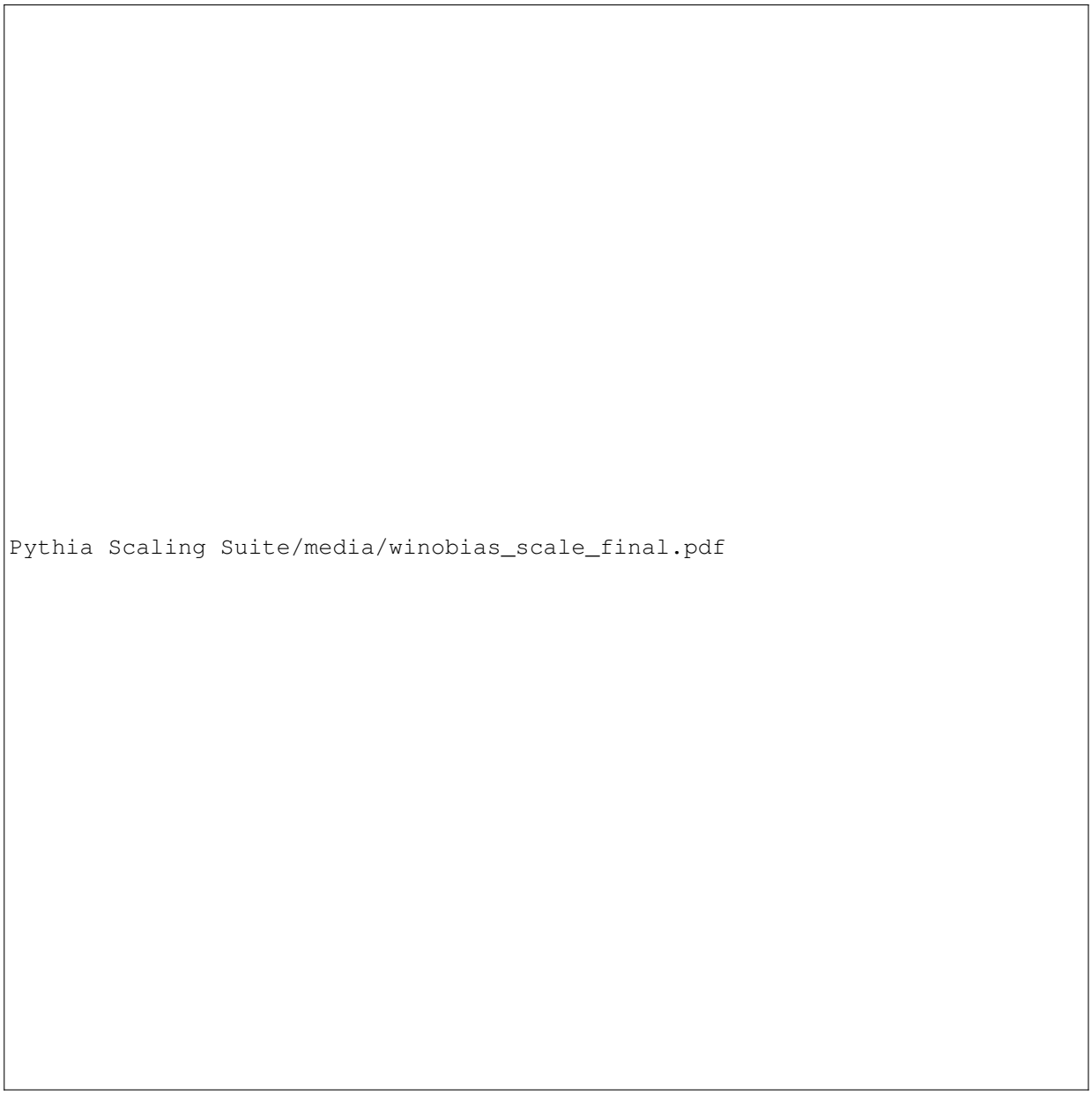


Figure 5. Results from the WinoBias benchmark, showing the accuracy difference between pro-stereotypical and anti-stereotypical examples. Positive values indicate greater gender bias towards traditional stereotypes.

checkpoint	160 M			1.0 B			2.8 B			12 B		
	$\Delta_{k=0}$	$\Delta_{k=4}$	$\Delta_{k=16}$	$\Delta_{k=0}$	$\Delta_{k=4}$	$\Delta_{k=16}$	$\Delta_{k=0}$	$\Delta_{k=4}$	$\Delta_{k=16}$	$\Delta_{k=0}$	$\Delta_{k=4}$	$\Delta_{k=16}$
13000	10.2	2.8	0.6	13.2	7.8	6.4	8.8	12.6	14.0	5.4	13.2	11.6
39000	7.4	7.0	5.4	12.0	11.8	16.0	9.0	33.6	30.6	16.2	29.0	37.8
65000	9.0	4.0	2.8	13.0	12.8	11.0	10.8	34.4	24.8	20.2	47.0	49.2
91000	13.8	11.2	3.2	14.2	11.0	12.8	5.2	46.4	47.0	26.0	58.0	54.2
117000	5.8	4.0	2.0	16.6	11.0	10.4	6.8	66.6	64.4	36.2	72.4	63.4
143000	12.2	8.6	3.0	15.2	12.8	12.2	4.0	66.0	66.6	42.2	75.6	62.4

Table 4. Performance gap on the arithmetic multiplication task for various model sizes with varying number of shots across checkpoints.

Figure 6. Zero-shot evaluations of Pythia models over training, as well as their intervened counterparts, on the LAMBADA dataset.

C. Full Configuration Details

In Table 5 we attach the full configuration details to train the models in this paper. Individual configuration files are available in the config files in our GitHub Repository.

Configuration Key	Value
attention-config	[[”flash”], n-layers]]
attention-dropout	0
bias-gelu-fusion	True
checkpoint-activations	True
checkpoint-num-layers	1
data-impl	mmap
distributed-backend	nccl
eval-interval	143000
eval-iters	10
fp16.enabled	True
fp16.fp16	True
fp16.hysteresis	2
fp16.initial-scale-power	12
fp16.loss-scale	0
fp16.loss-scale-window	1000
fp16.min-loss-scale	1
global-batch-size	1024
gpt-j-residual	True
gradient-accumulation-steps	–
gradient-clipping	1.0
hidden-dropout	0
hidden-size	–
init-method	small-init
log-interval	10
lr-decay-iters	143000
lr-decay-style	cosine
max-position-embeddings	2048
min-lr	$0.1 *$ optimizer.params.lr
model-parallel-size	–
no-weight-tying	True
norm	layernorm
num-attention-heads	–

Configuration Key	Value
num-layers	–
optimizer.params.betas	[0.9, 0.95]
optimizer.params.eps	1e-08
optimizer.params.lr	–
optimizer.type	Adam
output-layer-init-method	wang-init
output-layer-parallelism	column
partition-activations	False
pipe-parallel-size	1
pos-emb	rotary
rotary-pct	0.25
save-interval	1000
scaled-upper-triang-masked-softmax-fusion	True
seq-length	2048
split	969,30,1
steps-per-print	10
synchronize-each-layer	True
tokenizer-type	HFTokenizer
train-iters	143000
train-micro-batch-size-per-gpu	–
vocab-file	20B-tokenizer.json
wall-clock-breakdown	True
warmup	0.01
weight-decay	0.01
zero-optimization.allgather-bucket-size	–
zero-optimization.allgather-partitions	True
zero-optimization.contiguous-gradients	True
zero-optimization.cpu-offload	False
zero-optimization.overlap-comm	True
zero-optimization.reduce-bucket-size	–
zero-optimization.reduce-scatter	True
zero-optimization.stage	1

Table 5. The full configuration details for *Pythia* training. Exact model config files are also made available via our Github repository.

Configuration values marked with “–” differ between models. Table 1 provides particular model dimensions. Additionally, some modifications are necessary to enable appropriate parallelism: while most models are trained with “model-parallel-size = 1”, the 6.9b models were trained with “model-parallel-size = 2” and the 12b models were trained with “model-parallel-size = 4”. Both these larger models were trained using “zero-optimization.allgather-bucket-size = zero-optimization.reduce-bucket-size = 1260000000”, while all other models were trained with a value of 500000000. Exact number of GPUs, microbatch size per accelerator, and gradient accumulation steps per model, are available in the config files in our Github repository.

D. Assessment of Existing Suites

Existing model suites were assessed in consultation with the authors.

GPT-2 (Radford et al., 2019) No further notes.

GPT-3 (Brown et al., 2020) These models receive a half-mark for "Public Models" because they have a publicly accessible API, the API costs money and OpenAI places substantial limitations on the research they allow you to do with the API. While these models are known to be similar to the models described in Brown et al. (2020), they are not the same models. Gao (2021) estimates the size of these models as being 350M, 1.3B, 6.7B, and 175B parameters respectively, which

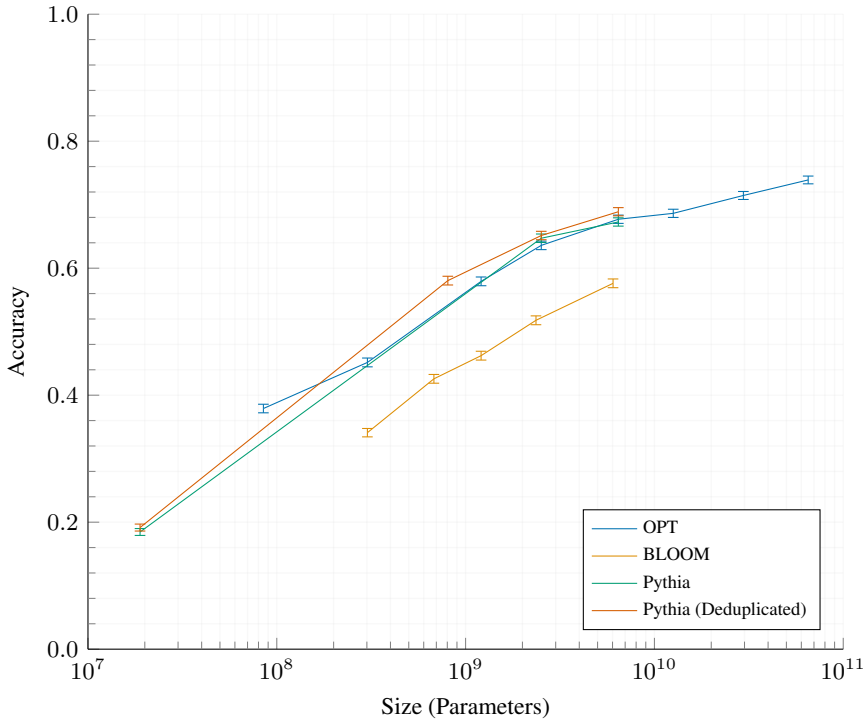
GPT-Neo (Black et al., 2021; Wang & Komatsuzaki, 2021; Black et al., 2022) These models strictly speaking do not form a suite and have some substantial differences between them. Despite that, they are commmonly used *as if they were* a consistent model suite.

OPT (Zhang et al., 2022) While more checkpoints of OPT models exist (as is seen by their use in Xia et al. (2022)) they largely are not publicly available (less than 10 checkpoints available, only for the 2.7b, 6.7b, and 13b parameter models). Additionally, the training dataset for OPT is not public.

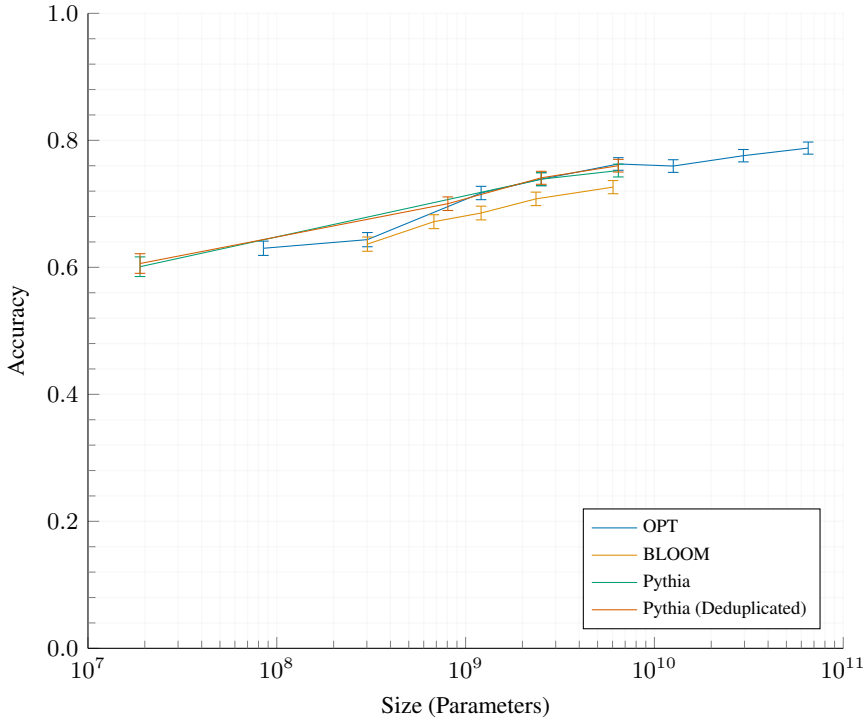
T5 (Raffel et al., 2020) The original paper did not release its training data, but it did release code for producing it which was subsequently run and released by Dodge et al. (2021).

BLOOM (Scao et al., 2022) The ROOTS dataset that BLOOM was trained on is available via application to researchers, but the authors suggest that they may not make the full data indefinitely in accompanying work (Jernite et al., 2022; McMillan-Major et al., 2022). The BLOOM models were *mostly* trained in a known and consistent order, however they handled training divergences by rewinding and skipping the offending sequences. Thus there are small (and undocumented) differences in the exact training composition and ordering across BLOOM models.

E. Evaluations

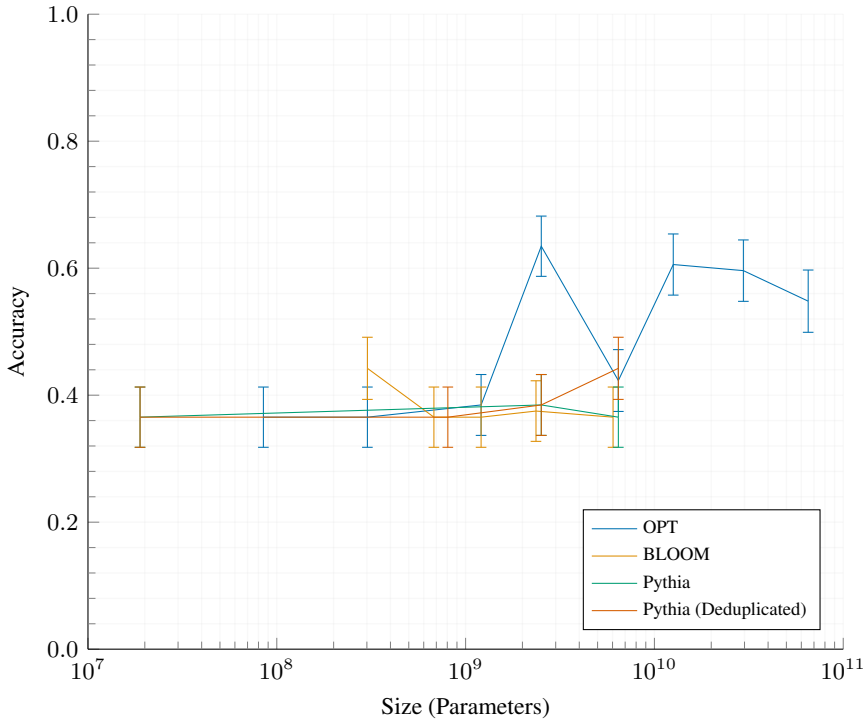


(a) LAMBADA (OpenAI)

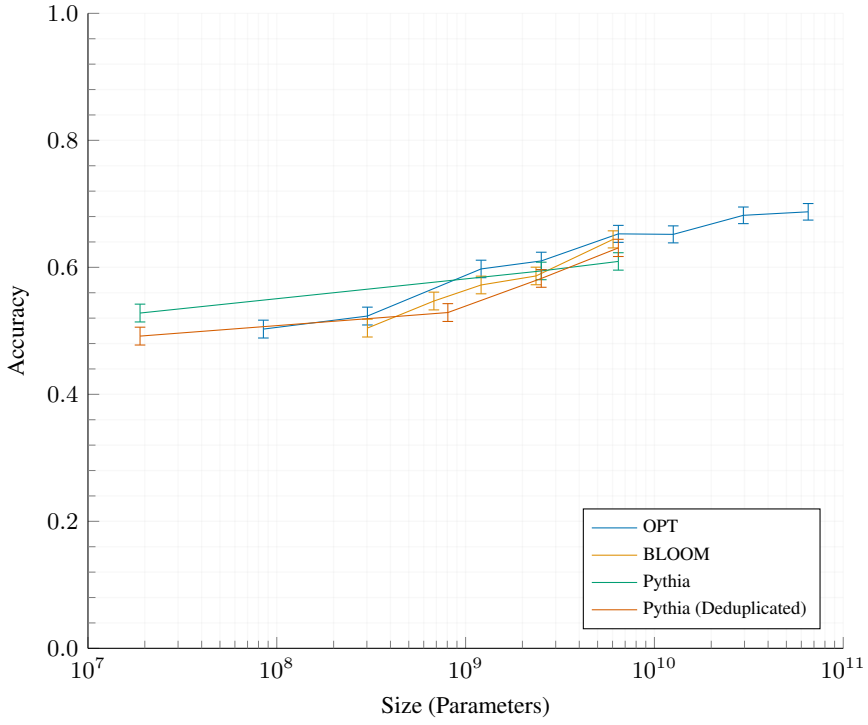


(b) PIQA

Figure 7. Zero-shot evaluations of final Pythia checkpoints against OPT and BLOOM (continued).

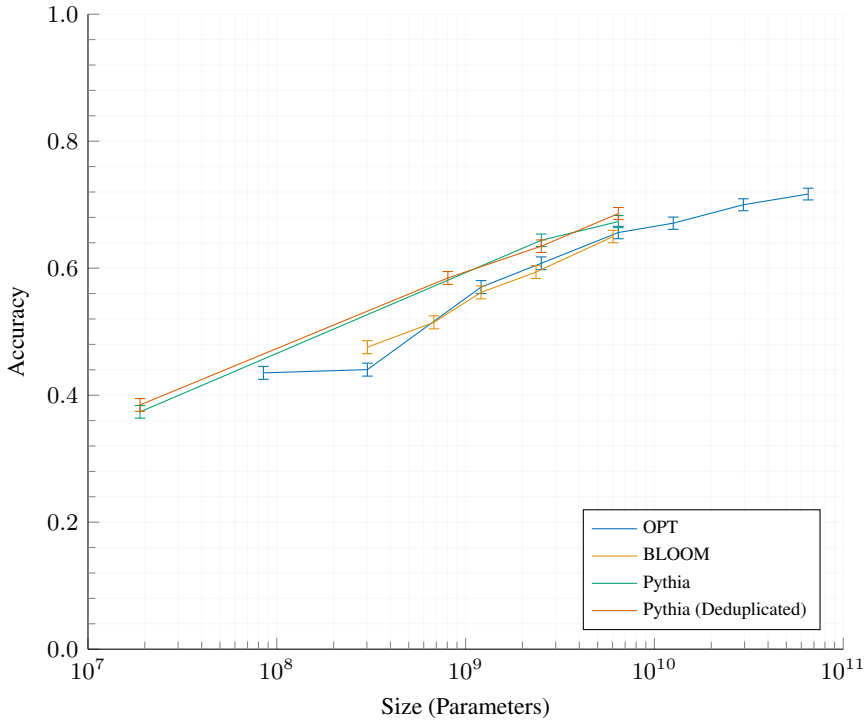


(c) Winogrand Schema Challenge

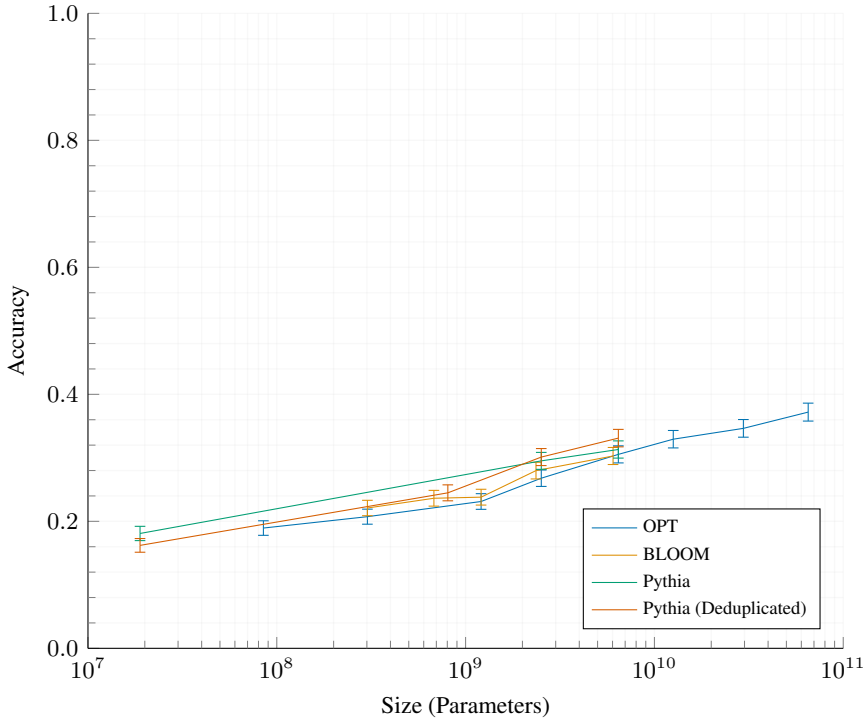


(d) WinoGrande

Figure 7. Zero-shot evaluations of final Pythia checkpoints against OPT and BLOOM.

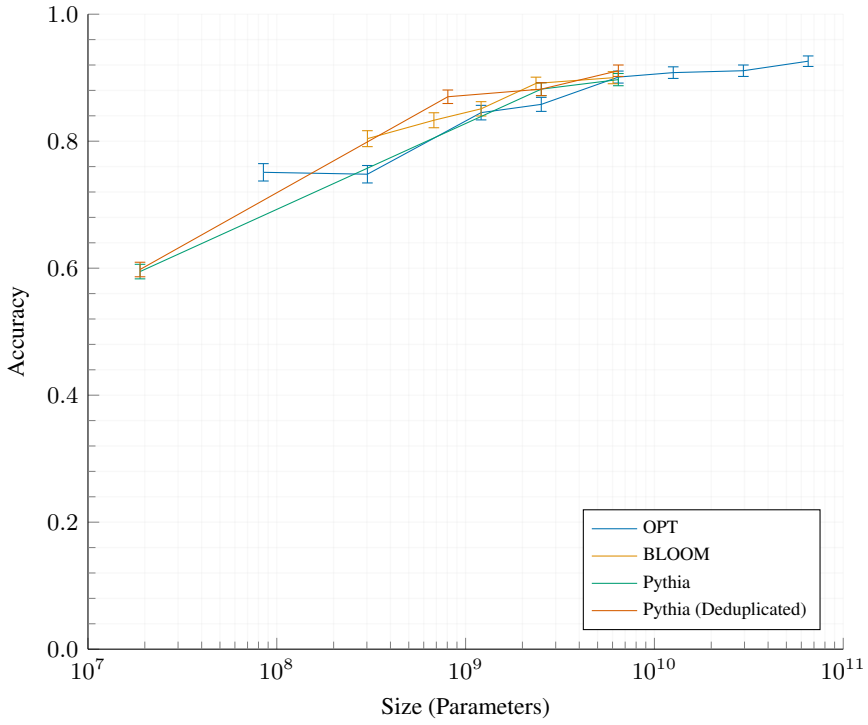


(e) AI2 Reasoning Challenge — Easy Set

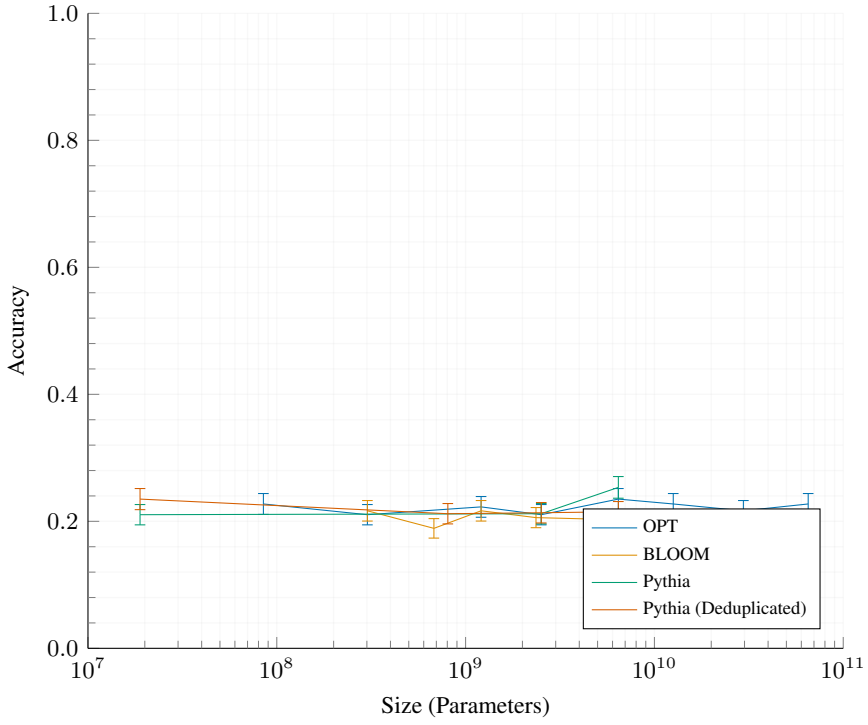


(f) AI2 Reasoning Challenge — Challenge Set

Figure 7. Zero-shot evaluations of final Pythia checkpoints against OPT and BLOOM (continued).

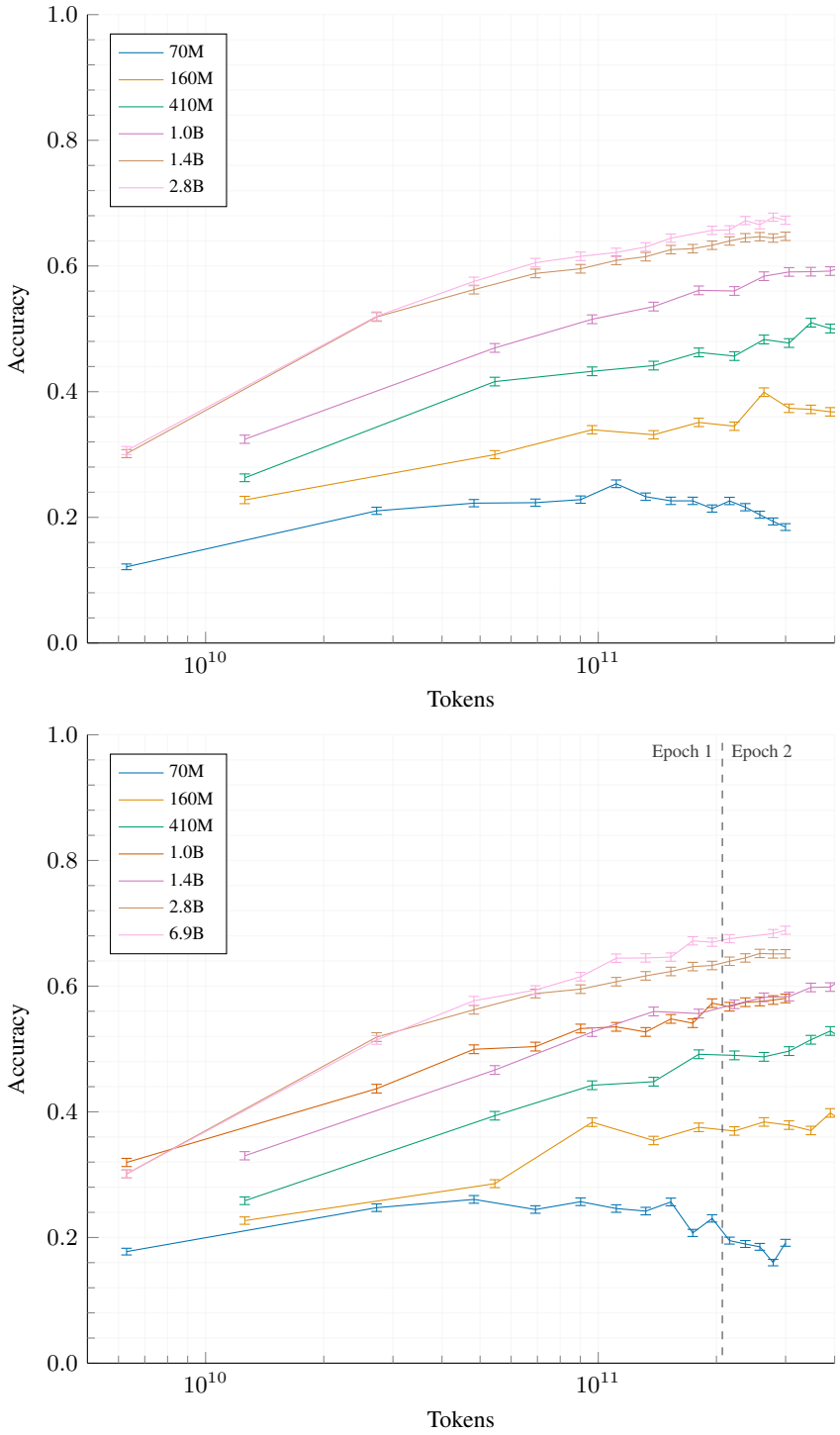


(g) SciQ

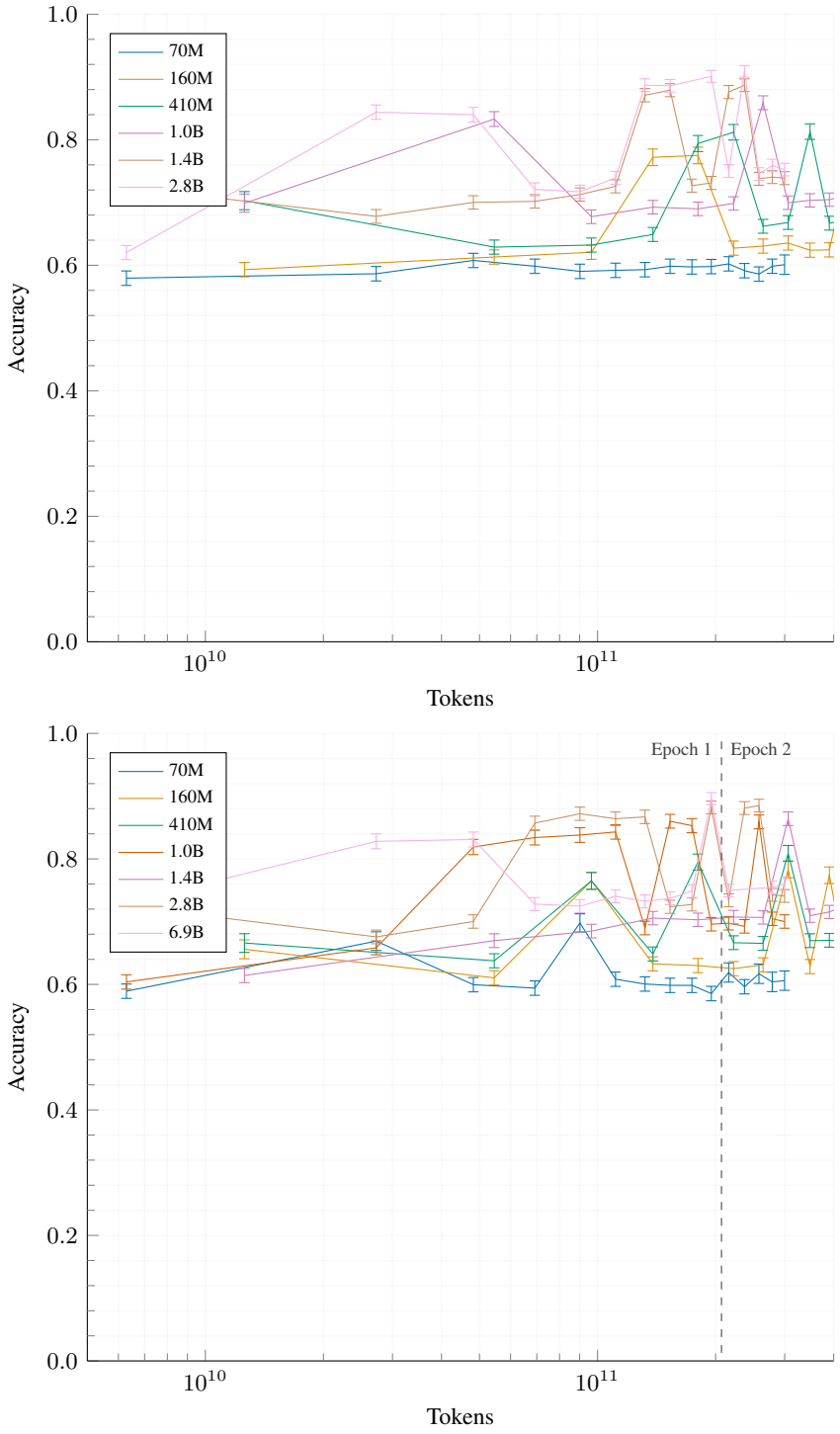


(h) LogiQA

Figure 7. Zero-shot evaluations of final Pythia checkpoints against OPT and BLOOM (continued).

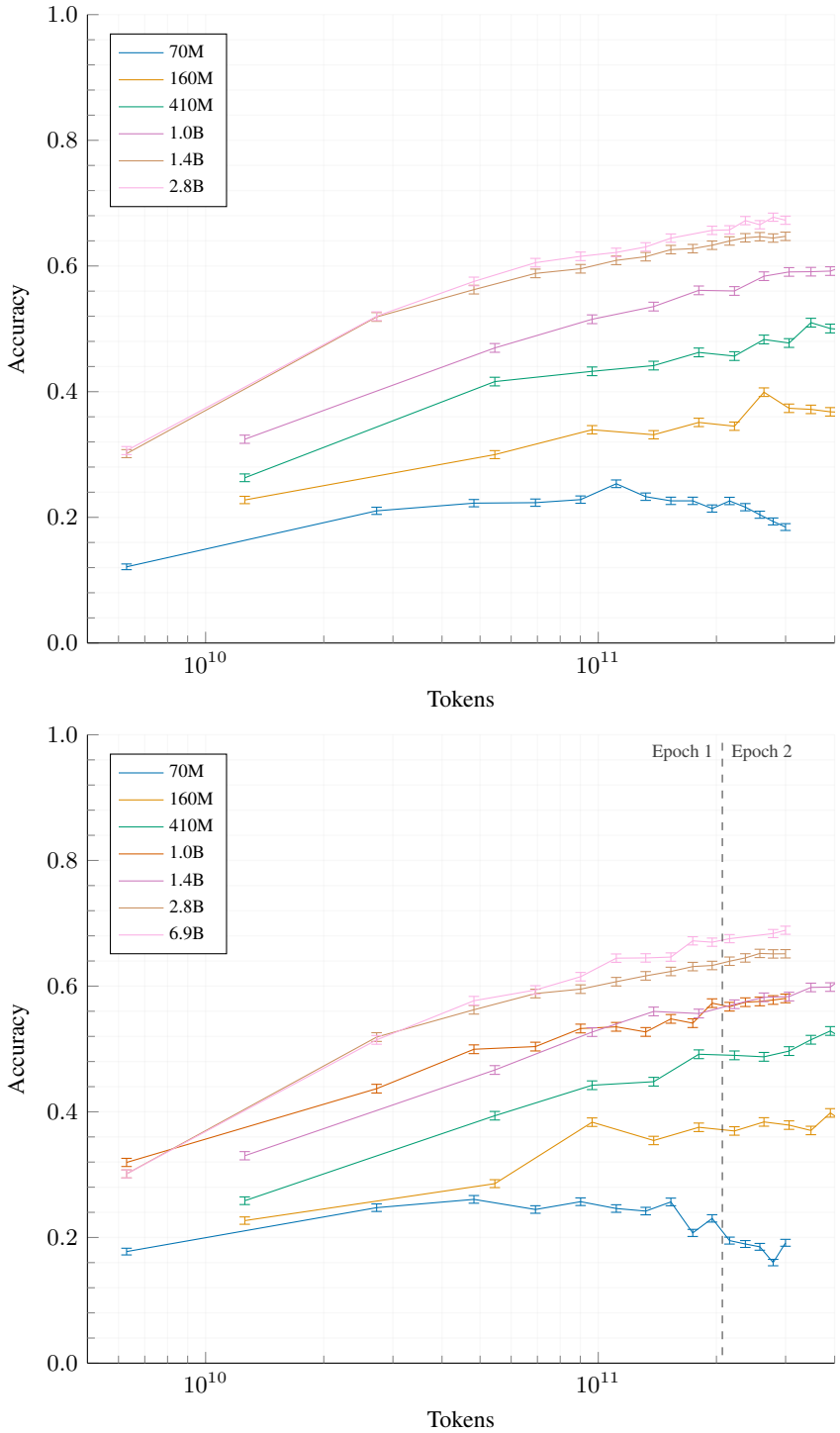


(a) LAMBADA (OpenAI)

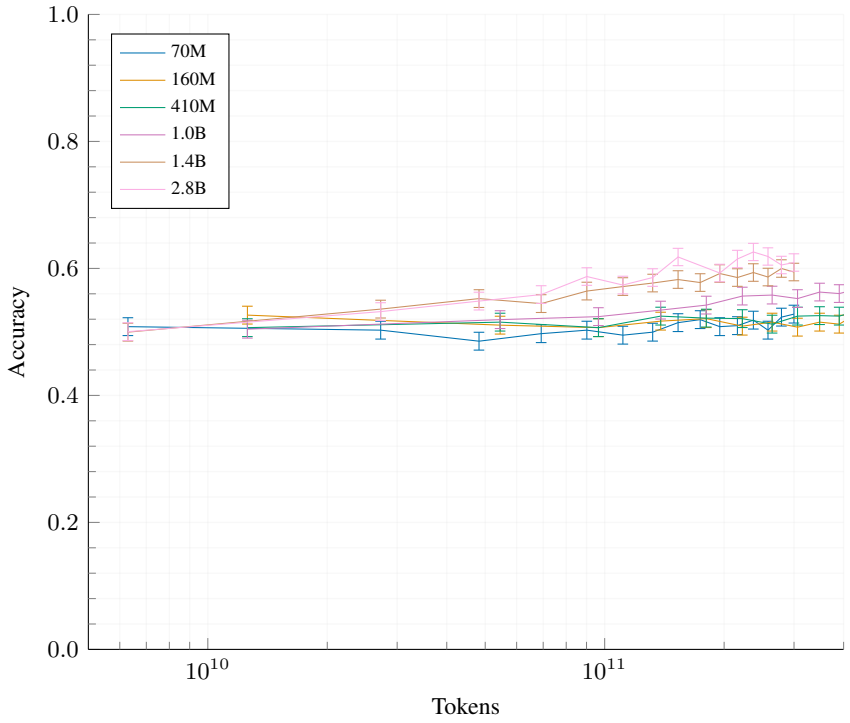


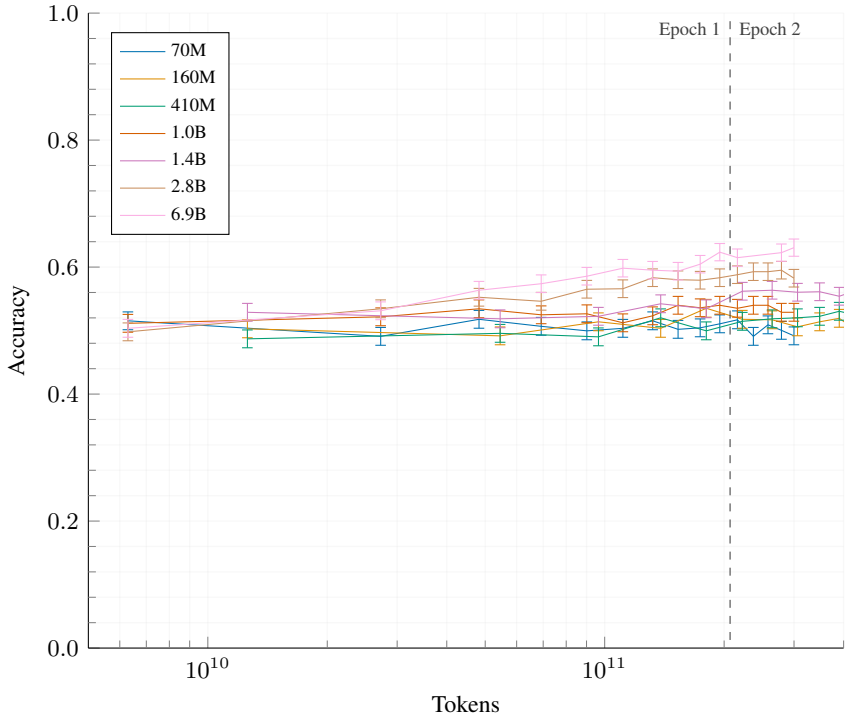
(a) PiQA

Figure 9. Zero-shot evaluations of Pythia checkpoints across training.



(a) LAMBADA (OpenAI)





(a) WinoGrande

Figure 11. Zero-shot evaluations of Pythia checkpoints across training.