A Pretrainer's Guide to Training Data: Measuring the Effects of Data Age, Domain Coverage, Quality, & Toxicity

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Abstract

Pretraining data design is critically under-documented and often guided by empirically unsupported intuitions. We pretrain models on data curated (1) at different collection times, (2) with varying toxicity and quality filters, and (3) with different domain compositions. First, we find that temporal shift between evaluation data and pretraining data leads to performance degradation, which is not overcome by finetuning. Second, we measure the effect of quality and toxicity filters, showing a trade-off between performance on standard benchmarks and risk of toxic generations. We also find that the effects of different types of filtering are not predictable from text domain characteristics. Third, we empirically validate that heterogeneous data sources, like books and web, are beneficial and warrant greater prioritization. To date, these experiments constitute the single largest publicly documented empirical study of the effects of pretraining data. Spanning 28 unique 1.5 billion parameter models pretrained from scratch, these findings validate, quantify, and expose many undocumented intuitions about text pretraining, which ultimately support more informed data-centric decisions in model development.

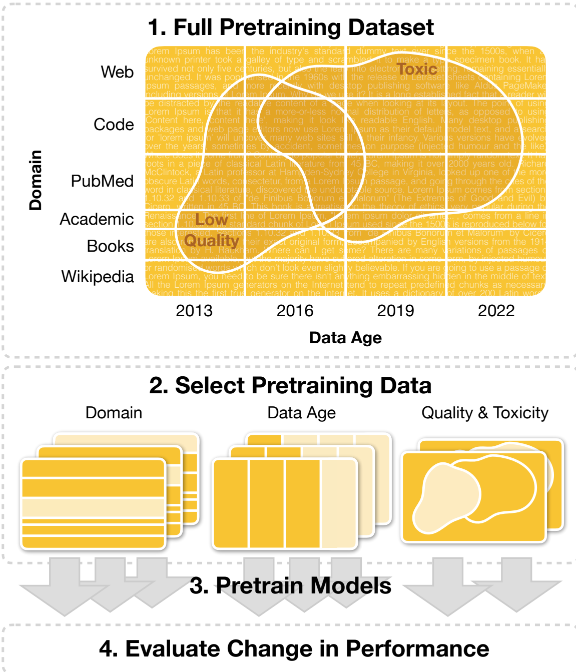

Figure 1: The experimental pretraining curation pipeline sub-selects data from C4 or the Pile, pretrains language models on each data split, and evaluates the change in performance over benchmarks.

1 Introduction

The strong performance (Chowdhery et al., 2022; Nostalgebraist, 2022; OpenAI, 2023; Google, 2023), and emergent abilities (Wei et al., 2022) of modern language models (LMs) depend on self-supervised pretraining on massive text datasets. All model developers implicitly or explicitly decide the composition of these datasets: what data sources to include, whether to filter for attributes such as quality and toxicity, and when to gather new documents. While many of the most prominent models do not document their curation procedures (OpenAI, 2023; Google, 2023), or only document *which* procedures they used (Brown et al., 2020; Nostalgebraist, 2022; Scao et al., 2022; Touvron et al., 2023), they rarely document *why* they chose those protocols or what effect those had. This documentation debt leaves practitioners to be guided by intuitions and precedents, neither thoroughly evaluated (Bandy and Vincent, 2021; Sambasivan et al., 2021). Given the outsized and fundamental role of pretraining data in modern LMs, we believe this practice has

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detracted from responsible data use and hampered effective model development (Rogers, 2021; Gebru et al., 2021; Bender and Friedman, 2018).

Among the small number of general-purpose LMs dominating community use and discussion, the prevailing focus has been on the scale of pretraining data and number of optimization steps (Brown et al., 2020; Nostalgebraist, 2022; Google, 2023). In this work, we select three common data design decisions and systematically test how they affect model performance—specifically, we choose the time of collection, content filtering strategy (toxicity/quality), and domain composition. We study the impacts in two ways. First, we present observational measurements of the effect of existing quality and toxicity filtering methods (Section 3). We document how these filters affect a range of characteristics in two major pretraining datasets, C4 (Raffel et al., 2020) and the Pile (Gao et al., 2020). Second, we rigorously evaluate these dataset decisions on downstream tasks by evaluating decoder-only autoregressive 1.5B-parameter LMs each pretrained on a dataset modified along one dimension of time, toxicity, quality, or domain composition. We summarize our findings and recommendations to model developers as follows:

Dataset Age (Section 4). Performance degrades when the age of evaluation data does not match pretraining data, both when evaluation data is newer *and* older. This phenomenon is not eliminated by substantial finetuning and is exacerbated in larger models. The effect can meaningfully complicate comparisons between new and old models: for example, frozen benchmark datasets give a subtle advantage to models pretrained on older data.

Quality and Toxicity Filters (Section 5). Though defining document quality and toxicity is difficult, most language models use heuristics for filtering documents by quality and/or toxicity (see Appendix, Table 4). We evaluate the most common heuristics for content filtering used in practice, finding that quality and toxicity filtering have significant but opposite effects on model behaviour. Quality filtering, removing "low-quality" text, substantially increases both toxic generation and downstream performance across tasks we tested, *despite reducing the amount of training data*. In contrast, removing "toxic" data trades-off fewer toxic generations for reduced generalization performance. Inverse toxicity filters, which remove the least toxic content, demonstrate targeted benefits. Removing low-quality text from the dataset does not necessarily improve results on datasets with high quality text. Quality filtering shows mostly positive effects, but the benefits are not predictable from text characteristics. These findings demonstrate that *one size filter does not fit all*. Practitioners should develop more targeted quality or inverse toxicity filters for their tasks.

Domain Composition (Section 6). The best performing domains comprise high-quality (books) and heterogeneous (web) data, corroborating Brown et al. (2020); Chowdhery et al. (2022); Xie et al. (2023a). However, these text sources contribute most to toxic generation. Still, we found that the benefit of training on these data sources is often greater than data collection for a targeted domain, and so recommend practitioners focus future collection on diverse, well-edited data. Additionally, our best performing models still use all data sources (even at the relatively small scale of 1.5B parameters); thus, we recommend practitioners include broad data sources, even those less relevant to their downstream tasks (Madaan et al., 2022).

As the majority of the community has adopted a small set of models for most research and applications (BERT, T5, GPT-2, GPT-3), pretraining data curation decision have long-term ramifications. Our findings *empirically quantify, validate, and, occasionally, challenge* an entrenched set of underexamined pretraining assumptions.

We hope these results better inform model developers training the next wave of LMs and set a precedent for more exploration of pretraining decisions. To our knowledge, these constitute the largest publicly documented LM data curation study, spanning 28 1.5B parameter models. While these models come at substantial computational cost (Section 8), we argue that the cost of *not* publicly evaluating pretraining decisions is greater.

2 Methodology

We measure how pretraining data curation choices affect downstream performance. Figure 1 illustrates our approach: each experiment starts with a pretraining dataset, applies a filter that removes documents, pretrains a language model on the curated dataset, and finetunes and evaluates the model on downstream tasks.

2.1 Pretraining Datasets

We use two common publicly available pretraining datasets: C4 (Raffel et al., 2020) and the Pile (Gao et al., 2020).[1] Both start with heuristic filtering for English language and content quality; we also deduplicate (Lee et al., 2022). Table 4 shows that these two datasets are widely used. Many other pretraining experiments are limited to only one of these datasets (Dodge et al., 2021; Biderman et al., 2023; Welbl et al., 2021; Xu et al., 2021).

2.2 Pretraining Data Variations

We evaluate variations in the pretraining data based on three categories of interventions: (1) To test impact of the age of a pretraining dataset, we create four versions of C4 using snapshots of the Common Crawl from different years: 2013, 2016, 2019, and 2022. (2) To examine the source composition of a pretraining dataset, we partition the Pile's 22 distinct sources into 9 parts: Common Crawl, OpenWebText, books, Wikipedia, legal, social, biomedical, academic, and code (see Appendix, Table 5). We then ablate each source from the Pile one-at-a-time and measure the performance change in diverse QA tasks. (3) To examine the effect of toxicity and quality filters, now a staple in pretraining (Appendix, Table 4), we use two document-level, classifier-based filters and vary the confidence thresholds to show different filter strengths.

Quality Filters We employ the proprietary quality filter used by PaLM and GLaM, which assigns each document a score from 0 (high quality) to 1 (low quality). We remove low-quality documents that fall above four quality thresholds: 0.975, 0.95, 0.9, 0.7. We separately invert this filter to instead remove the *highest* quality documents *below* a threshold.

Toxicity Filters We use Jigsaw's Perspective API[2], which was trained on comments from online forums and assigns toxicity scores based on whether annotators found the comment to contain profanity/obscenity, identity-based negativity, insults, or threats. Our experiments include five different toxicity threshold values 0.95, 0.9, 0.7, 0.5, and 0.3, as well as one inverse filter that removes documents with the *least* predicted toxicity *below* a threshold. In addition to the classifier-based filter, we also experiment with the n-gram based filter used by Raffel et al. (2020) in the original version of the C4 dataset. This filter removes all documents that contain any word present in the "List of Dirty, Naughty, Obscene, or Otherwise Bad Words".

There are several important limitations with the design and definition of "quality" and "toxicity" filters in general, which we discuss in Section 8. More details on pretraining data curation methodology are in Appendix B.

CURATION	TASK DESCRIPTION	DATASETS
Age	Temporal degredation	PubCLS, NewSum, PoliAff, TwiERC, AIC (Luu et al., 2022)
Toxicity/quality filtering	Toxic generation	RealToxicityPrompts (Gehman et al., 2020) RepBias (Chowdhery et al., 2022)
	Toxicity identification	Social Bias Frames (Sap et al., 2020) DynaHate (Vidgen et al., 2021) Toxigen (Hartvigsen et al., 2022)
	Question answering	MRQA (Fisch et al., 2019) UnifiedQA (Khashabi et al., 2020)
Domain composition	Question answering	MRQA (Fisch et al., 2019) UnifiedQA (Khashabi et al., 2020)

Table 1: **We evaluate the effect of different curation strategies on different downstream tasks.**

2.3 Evaluation

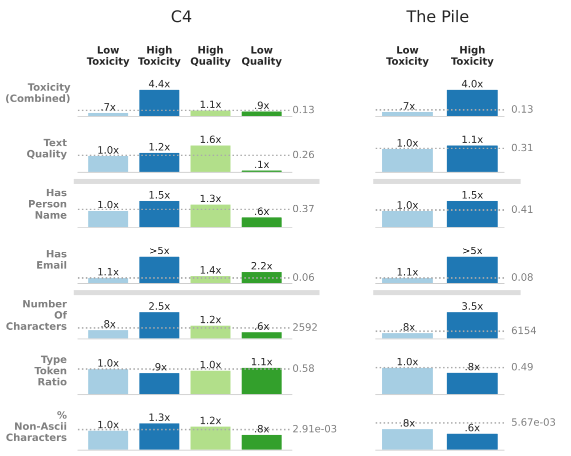
To measure the effects of time, domain, quality, and toxicity, we evaluate pretrained models on English-language tasks for toxicity identification, toxic generation, dozens of question answering (QA) tasks from diverse domains, and several tasks with temporal annotations (Table 1). These evaluations are chosen to broadly understand the impact of dataset ablations.

To evaluate domain generalization we combine two question-answering benchmarks: Machine Reading for Question Answering (MRQA) (Fisch et al., 2019) and UnifiedQA (Khashabi et al., 2020), which together consist of 27 unique QA datasets that we partition into domain categories (see Appendix, Table 9). To evaluate temporal shift, Luu et al. (2022) released several datasets in which increasing temporal distance between *finetuning* and evaluation time decreases test performance. We choose 5 of these datasets from varying domains to evaluate whether a similar phenomenon exists between *pretraining* and evaluation time: PubCLS, NewSum, PoliAff, TwiERC, and AIC.

To evaluate toxic generation tendencies, we use the RealToxicityPrompts benchmark (Gehman et al., 2020) and the Representational Bias benchmark used in Chowdhery et al. (2022) (see Appendix D.1 for details). To evaluate the model's ability to identify toxic content, which is critical in content moderation applications (NYT, 2020; Singh, 2019), we use Social Bias Frames (SBF, Sap et al., 2020), DynaHate (DH, Vidgen et al., 2021), and Toxigen (Hartvigsen et al., 2022).

[1] The datasets have the ODC-By and MIT Licenses, respectively. We use the datasets for the intended purpose.

[2] https://www.perspectiveapi.com



(a) (b)

Figure 2: **Feature differences across slices of the pretraining datasets.** Bars show the ratio between the mean feature value for the slice and the mean value for the dataset (the Pile or C4), which is indicated by a horizontal gray line. For example, high toxicity Pile text has 3.5x the number of characters per datapoint than average for the Pile.

		Finetuning				Pretraining			
		LM-Small		LM-XL		LM-Small		LM-XL	
Domain	Task	TD	r	TD	r	TD	r	TD	r
News	PubCLS	5.82	0.84	5.63	0.80	0.02	0.01†	0.59	0.67
	NewSum	0.80	0.82	2.91	0.92	-0.31	-0.29	0.73	0.45
Twitter	PoliAff	3.74	0.84	4.93	0.89	0.50	0.21	0.28	0.56
	TwiERC	0.49	0.73	0.53	0.82	0.05	0.27	0.23	0.72
Science	AIC	0.94	0.83	0.24	0.36	0.11	0.18†	0.23	0.66
	Mean	2.36	0.81	2.84	0.76	0.08	0.07	0.41	0.61

Table 2: Temporal Degradation (TD) measures the expected performance degradation from one year of temporal misalignment. We report TD first between finetuning and evaluation, then pretraining and evaluation, for LM-XL and LM-Small, across five tasks. Pearson correlation r indicates the correlation strength between performance change and temporal misalignment. **Temporal Degradation due to pretraining is significant and persistent across domains.** All correlations are significant at $p < 0.05$ unless marked with †.

2.4 Models

We use a decoder-only Transformer-based language model, trained using T5X (Roberts et al., 2022). Our main experiments use LM-XL, a 1.5B parameter model similar to the `t5.1.1-XL` architecture configuration trained with an autoregressive next-token-prediction objective. For experiments that measure scaling effects, we use LM-Small, a 20M parameter decoder-only model similar to the `t5.1.1-small` configuration. These configurations are popular, show decent performance (Wang et al., 2022), and can generate text without additional finetuning. We study one model family in order to focus on the effects of data curation due to the computational cost of pretraining. Additional details on pretraining and finetuning are available in Appendix C

3 Impact of Data Curation on Data Characteristics

We first present observational statistics on the pretraining datasets themselves. We find substantial interactions between curation choices. For more details on these features, see Appendix E.

Toxicity and Quality Figure 2 shows that toxicity and quality are surprisingly not well-aligned with one another. There is little discernible difference in feature measurements for profanity, toxicity, and sexually explicit content between content classified as low vs. high quality. High toxicity documents have higher text quality than low toxicity documents. This is explained by the Books subset of the Pile having substantially more profane, toxic, and sexual content, but also greater predicted quality (see Appendix, Figure 7). While we might expect books to be high quality, in the sense that they typically contain meaningful, well-edited sentences, they also contain strong language and erotic subjects. This may also explain why documents classified as high toxicity in both C4 and the Pile are much longer (2.5x and 3.5x above dataset mean, respectively), more profane (5x and 4.4x), sexually explicit (4.6x and 4.2x), and toxic (3.6x and 3.5x).

Domains in the Pile Figure 7 in the Appendix shows that OpenWeb provides the most lexical and linguistic diversity, with the highest non-ASCII characters and type-token ratio. Wikipedia presents the highest quality text, before Books and OpenWeb. Technical domains such as PubMed, Code, and Academic score low on predicted quality, indicating that overly-specific positively-defined filters on web documents may remove substantial amounts of potentially useful specialized text.

4 Impact of Dataset Age on Pretrained Models

While models are frequently and cheaply updated with new finetuning data, the expense of pretraining means the NLP community has relied on relatively few static pretrained models that are rarely updated or exchanged. BERT, RoBERTa, GPT-2, and T5 variants, all pretrained prior to 2020, constitute the majority (estimated at ~58% as of April 16, 2023) of all models downloaded on Hugging Face. Prior work demonstrates that language use changes over time (Altmann et al., 2009; Labov, 2011) and that *temporal misalignment* between finetuning and evaluation datasets correlates with degraded performance, visible across settings and domains (Luu et al., 2022; Lazaridou et al., 2021; Agarwal and Nenkova, 2022; Jang et al., 2022). In contrast, we examine the effect of temporal misalignment between *pretraining data* and evaluation. In evaluating the impact of pretraining time across data domains, we can quantify the impact this design choice has on NLP broadly.

We pretrain four autoregressive language models on versions of C4 (2013, 2016, 2019, and 2022), beginning with Common Crawl data and removing all data that was scraped after the cutoff year. Following Luu et al. (2022), we measure the effect of temporal misalignment by using evaluation tasks that have training and test sets split by year (from News, Twitter, and Science domains). After pretraining, we finetune each model on each dataset's training-year split separately, then evaluate on every test-year split. Full details and results are in Appendix D.2 and Appendix F.1, respectively.

We estimate the effects of temporal misalignment between *pretraining* and evaluation (Appendix, Figure 9). Since all models were finetuned on the training sets of the evaluation tasks, we show that temporal misalignment during pretraining persists even with temporally-relevant finetuning data.

Performance degradation strongly correlates with pretraining misalignment and its effects are non-trivial. Luu et al. (2022) formalize a definition for Temporal Degradation (TD), which measures the performance change observed from a one year difference between the finetuning and evaluation years. We generalize TD to also measure the effect of a one year difference between pretraining time and evaluation time, as described in Appendix D.2. Furthermore, we quantify the strength of the relationship between performance difference and temporal difference using Pearson correlation. In Table 2 we find temporal degradation is highest for finetuning (2.8 on average), as expected, but also surprisingly high for one year of pretraining (0.4)—particularly for the News domain. The average Pearson correlation of 0.61 indicates a strong correlation between pretraining temporal misalignment and reduced performance. All five tasks pass a one-sided Wald test with $p < 0.05$, validating that the effect is significant.

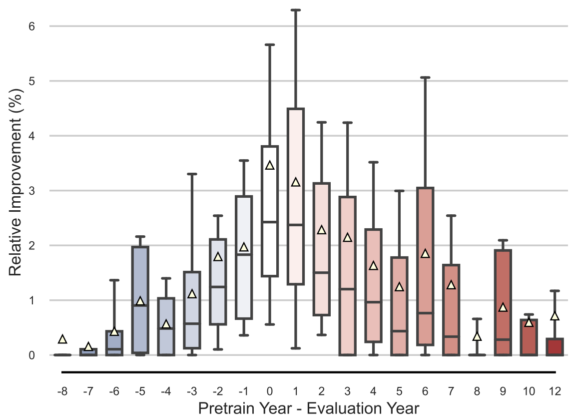
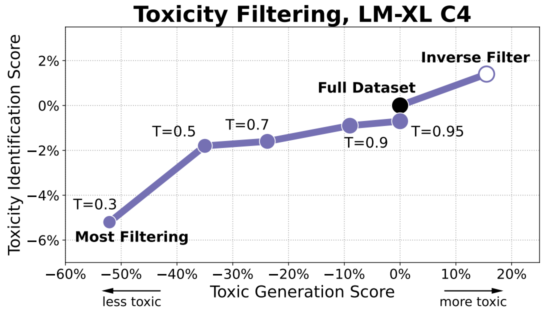


Figure 3: **Relative performance degrades when there is temporal mismatch between pretraining and evaluation data.** The boxplot indicates the median (solid line), mean (triangles), quartile range (boxes), and rest of the distribution (whiskers). Note that each dataset has different evaluation year ranges.

Pretraining misalignment is not overcome by significant finetuning. The temporal degradation due to pretraining suggests models pretrained on data from the same timeframe as target evaluations will have advantages over models trained on much older or newer data. Notably, this effect is observed for models which are finetuned on the full temporally-relevant training sets. This suggests that even substantial finetuning cannot overcome pretraining data that is temporally misaligned.

Pretraining misalignment effects are asymmetric and have implications for NLP evaluations. Figure 3 summarizes temporal results, each of which is associated with an evaluation dataset, a pretraining year, a finetuning year, and an evaluation year. Each result is compared to a baseline of the lowest performance for that dataset's evaluation year (across any pretraining and finetuning year combination). Zero on the y-axis represents the lowest performance, and points are plotted based on their percentage improvement over their corresponding baseline. We observe performance degradation regardless of whether the pretraining data was collected before or after the evaluation data. While we would not expect a 2019 model to perform well on questions about COVID, we also find that 2022 models perform less well on Obama-era evaluations than earlier models.



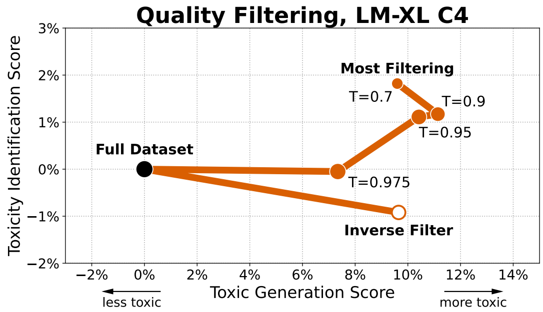


Figure 4: **Toxicity filtering the pretraining dataset decreases the ability of LM-XL to identify toxicity and to generate toxic text. Quality filtering surprisingly increases both abilities.** Documents with scores above a given threshold were filtered out.

Figure 3 also shows performance degradation is asymmetric: it is steeper when the evaluation year is after the pretraining year (blue bars) as opposed to the reverse (red bars). This finding suggests that both models and evaluations become stale: older models perform less well than newer models on new evaluations and newer models will perform less well on older evaluations. There are many possible causes; it could be because a lower fraction of the training dataset comes from the years that are relevant to the time-dependent task, or it might be because the facts change in pretraining data from later years. Regardless of the cause, this phenomenon has implications for NLP experiments comparing models pretrained at different times. For instance, newer evaluation sets may appear much more difficult than old evaluation sets when applied to established, but less fresh, models. Similarly, older evaluations may underestimate the capabilities of newer models.

Temporal Degradation is greater for larger models. We find more temporal degradation for LM-XL (1.5B parameters) than for LM-SMALL (20M parameters). As shown in Table 2, we do not find the same temporal degradation effects of pretraining were significant for LM-SMALL models. This suggests that larger models may have a greater sensitivity to temporal information than smaller models, which may not have the capacity to take advantage of subtle temporal features at all. Full results for LM-SMALL experiments are provided in Appendix F.1.

Filter	% Data	Wiki	Web	Acad.	C.S.	Avg
Full Data	100	0.0	0.0	0.0	0.0	0.0
QUALITY FILTERS						
Inv T=0.5	73	-5.0	-4.5	-2.7	-6.4	-3.1
T=0.975	91	+1.2	+0.7	+6.4	+6.1	+2.5
T=0.9	73	-0.3	+0.8	+0.8	+6.8	+1.2
TOXICITY FILTERS						
Inv T=0.06	92	+0.4	-1.4	+4.9	+2.7	+1.7
T=0.9	95	-2.2	-1.1	+0.2	+0.2	-0.7
T=0.5	76	-4.2	-2.4	-1.1	-3.5	-2.7

Table 3: **Quality filters improve performance, toxicity filters reduce performance.** Quality and toxicity filters on C4 (y-axis) reduce the pretraining data (% Data), and impact LM-XL's relative performance on QA evaluations from domains Wiki, Web, Academic, and Contrast Sets. Full results shown in Figure 10 and Figure 11.

5 Impact of Quality & Toxicity Filters on Pretrained Models

Most modern large language models use some form of quality and/or toxicity filtering for their pretraining datasets (Appendix, Table 4). To curb toxicity, T5 uses n-gram filters; Gopher and Chinchilla use SafeSearch filters; and LaMDA uses "safety discriminators". Quality heuristics are universally applied for web-scraped data, with newer models like LLaMA, the GPT-series and the PaLM-series all relying on quality classifiers. To compare and quantify the effects of these two filter types, we implement quality and toxicity filters at various thresholds, as described in Section 2, to vary toxic and low-quality text present when pretraining models on the Pile and C4.

Quality filters significantly improve performance across nearly all tasks, despite reducing training data quantity and variety. Quality filters improve nearly all downstream tasks: toxicity identification by 2% (Figure 4, bottom) and most QA task categories by 1-6% (Table 3). These improvements are realized despite removing 10%+ of the training data, even though we find that removing data usually leads to a decrease in performance (Section 6). While the average performance peaks

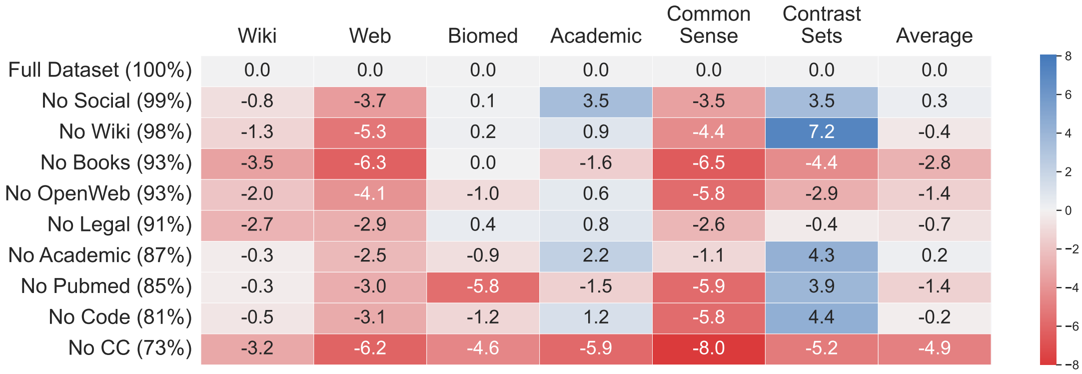


Figure 5: **QA tasks are affected by removing domains when pretraining LM-XL.** Each row represents a model with one domain removed, the size of the remaining dataset is shown at the left in parentheses. Each column represents a set of QA evaluations from a domain. The FULL DATASET model represents the unfiltered Pile LM-XL, and all scores are relative to this baseline model.

at $T = 0.975$ for the QA tasks, greater quality filtering still outperforms the unfiltered baseline on average. The performance on toxicity identification is still improving after $T = 0.7$, where 55% of the dataset has been filtered out.

The effect of quality filtering varies by dataset. In Section 3, Books, Wikipedia, and Web data are classified as highest quality. Table 3 shows that quality filtering provides the least benefit to QA tasks in these categories, even hurting the performance for Books. Academic and biomedical data have lower initial estimated quality, and their QA tasks benefit the most from quality filtering. When we remove the highest estimated quality documents, Wikipedia and Web QA tasks are the most hurt, suggesting that these domains are not affected as much by the absence of the lowest quality data as the presence of the highest quality data. Unexpectedly, both the quality and inverse quality filters led to models with higher toxic generation tendencies (Figure 4, bottom). Different segments of data along this classifier's quality spectrum can have strong but varied effects on different domains. "Quality" is a complex, situational concept, for which it is unlikely that one measure is sufficient.

Toxicity filtering leads to a trade-off between toxicity identification and toxic generation goals. We find that filtering using a toxicity classifier leads to a trade-off: models trained from heavily filtered pretraining datasets have the least toxic *generation* but also the worst toxicity *identification* (Figure 4, top). Similarly, Table 3 shows the performance of QA tasks unrelated to toxicity are hurt by toxicity filtering, though this may be due to the overall decrease in training data. Ultimately, the filtering strategy should match the intended behaviour of the model. The strongest performance on toxicity identification for every dataset comes from the inverse toxicity filter. **Practitioners optimizing for performance on toxic domains should intentionally apply inverse filters**.

6 Impact of Domain Composition on Pretrained Models

As shown in Table 4 (Appendix), pretraining datasets combine diverse data sources to generalize widely. How does the choice of source domains impact downstream performance? We empirically answer this question by ablating pretraining sources from the Pile one-at-a-time and measuring the performance change in 27 diverse QA tasks.

As described in Section 2, we partition the Pile data sources into 9 conceptual domains (see Appendix, Table 5), and the QA datasets into 7 domains (see Appendix, Table 9). We choose to maintain the size disparities in the source domains, simply because they reflect reality: curated Wikipedia content is finite, while web and books are much more abundant. After pretraining each model, we finetune on Natural Questions to prepare the model for the QA task, then evaluate on all QA datasets. Full details are in Appendix D.3.

Common Crawl, OpenWeb, and Books have the strongest positive effects on downstream performance. Figure 5 shows that average downstream performance degrades the most when we remove web-based domains like CC, Books, and OpenWeb, corroborating recent findings by Xie et al. (2023a). These sources improve performance on challenging Common Sense and Contrast Sets tasks. While CC is the largest chunk of text in the Pile, Books and OpenWeb are smaller but more diverse and high quality (see Section 3). These results suggest that a combination of heterogeneity and quality is more impactful than raw dataset size.

Domain heterogeneity is often more beneficial than targeted data, even for targeted evaluations. Performance degrades when we remove domains with close alignment between the pretraining and downstream data sources: removing PubMed hurts the BioMed QA evaluations, dropping Wikipedia hurts the Wikipedia benchmarks, and removing web content hurts web evaluations. However, removing the large heterogeneous domains can have even more effect than the targeted domains. For instance, removing CC from pretraining reduces performance on downstream Academic QA tasks to a much greater extent than removing the Academic domain. We speculate that CC, OpenWeb and Books cover many topics, so removing the Academic-specific category of sources does not remove all relevant academic information.

The best performing models pretrain on *all* data sources. Despite the importance of data heterogeneity, the best mean performance still comes from models that train on all, or nearly all, available data. The exceptions are the removal of targeted source domains like the Pile's Code or Academic (advanced science and math journals) domains. These are both large but perhaps not well matched with the QA evaluation sets, which do not require coding skills or scientific rigour beyond that found on Wikipedia and from web-based sources. This finding suggests that both the quantity and diversity of open source data remain a bottleneck for current pretraining methods.

Web and Books domains cause the biggest trade-off between toxic identification and generation. Section 5 identifies a trade-off: better performance on QA and toxicity identification comes at the cost of more toxic generation. Table 17 in the Appendix shows that the largest decreases in *both* toxicity generation and identification were caused by removing CC (26.9% of the data), OpenWeb (6.9%), and Books (6.9%). This is consistent with our observation that Web and Books data had the most text predicted to be toxic.

7 Discussion

Model developers often neglect to share empirical insights, maintaining a knowledge gap often referred to as "documentation debt" (Bandy and Vincent, 2021). As a result, pretraining dataset curation is frequently guided by intuitions or precedents that have not been thoroughly evaluated (Sambasivan et al., 2021). Our results show that choices made in pretraining curation affect models in ways that are not easily erased by subsequent finetuning. We urge both model producers and users to think of dataset curation policies as a form of hyperparameter, much like learning rates or network dimensions. Exhaustive search methods that work for single scalar values will not, however, scale to curation policies that affect terabytes of data. In this section, we distill our findings into a few specific recommendations.

Age of the pretraining corpus. Model creators must choose between model staleness and expensive continuous data collection and training. Even with sufficient compute, newer data can add a "presentist" bias when evaluating retrospective tasks. Our findings suggest the temporal properties of pretraining corpora are increasingly essential to consider for larger models, for more novel tasks (less finetuning data), and for instruction tuning models. For instance, Schulman (2023) suggests that finetuning on new information not represented during pretraining can encourage model hallucination. Retrieval augmentation may offer some mitigation but presents its own challenges.

We recommend model creators report the temporal distribution of pretraining data, which is not currently standard practice (Hoffmann et al., 2022; Thoppilan et al., 2022; Anthropic AI, 2023; Cohere AI, 2023). Users should be able to predict otherwise unforeseen performance degradations on much newer datasets, or be aware of the potential side effects of finetuning models on information not covered in pretraining.

Data source composition. We find that corpora should be as diverse as possible, but we recognize that this is time consuming and requires a wide range of area expertise to ensure quality. Our results suggest that practitioners should not omit any data sources if generalization to as many text-to-text tasks is the goal, and that future work should focus on collecting more diverse, high quality web and books content, which yield the largest benefits.

These findings are somewhat consistent with hypotheses that the volume of training data remains a limiting factor, especially given licensing constraints (Nostalgebraist, 2022).

Filtering for toxicity and quality. The Common Crawl contains an enormous amount of toxic and low quality text (spammy, repetitive, non-human-readable, etc.). Many state-of-the-art language models filter out this text before training, either using bad words lists (Raffel et al., 2020), heuristics, or classifiers (Du et al., 2022; Brown et al., 2020; Chowdhery et al., 2022). Deciding on how much and what kind of text to filter out requires non-trivial normative decisions that affect the biases of their datasets and thus their models.

In our experiments, we expose an implicit trade-off between a model's generalization abilities and its tendency to generate toxic content, modulated by content filters. In fact, over-sampling *more* toxic documents leads to the best performance on toxicity identification. This observation, coupled with evidence that recent work is using post-hoc methods to curb unwanted toxic generation (e.g. instruction tuning (Chung et al., 2022) or steerable decoders (Dathathri et al., 2020; Welbl et al., 2021)), suggests that pretraining should target toxicity identification rather than curbing toxic generation.

We find that our quality filter (the same used by PaLM, trained to keep content resembling Wikipedia and Books) significantly improves performance across domains, despite removing large portions of the training data. But surprisingly, observational quality characteristics of the data are not sufficient to predict which domains will benefit most from quality filtering. Our analysis suggests that performance on a task/domain is not influenced *only* by how much poor quality data (i.e. that which is unlike Wikipedia/Books) is removed, but also by other aspects of quality, such as how much of the highest or mid-quality data is represented along this specific measurement dimension.

Conclusion. We empirically show that pretraining data curation decisions for dataset age, composition, and content filtering have systematic impact on downstream performance. Though we chose these curation axes in order to analyze widespread current practices, the scale and variety of pretraining data are so vast that no one paper can address all possible variations. We hope this paper provides the foundation for further work linking properties of pretraining data to properties of models.

8 Limitations

English vs Multilingual Data Our analysis was limited to two English pretraining datasets. It's important to note that training composition is an even more crucial question for multilingual and non-English models, where optimally balancing corpora from different languages and finding large-enough high-quality corpora can be very challenging (Chung et al., 2023).

That said, our experiments are among the most comprehensive publicly available. Pretraining is extremely expensive, and we evaluate the intersection of multiple factors, carefully chosen because they are under-studied, and we lack empirical evidence on their effects. Each of these factors has multiple options, and interacting them with other features in the experimental design can have exponential impact on cost and running time. Prior work has typically studied only one of the Pile/C4 at a time (Dodge et al., 2021; Biderman et al., 2023; Welbl et al., 2021; Xu et al., 2021). We hope future work can study additional pretraining datasets.

Compute Expense & Single Shot Experiments To our knowledge, this is the largest publicly documented LM pretraining data ablation study, spanning 28 1.5B parameter models—training more models with different data variants from scratch than GLaM (Du et al., 2022), miniBertas (Warstadt et al., 2020), MultiBerts (Sellam et al., 2022), and even Pythia (Biderman et al., 2023), which focuses on preserving data composition and order. It is important to acknowledge each of these pretrainings, with their corresponding finetuning and evaluations, is computationally and environmentally costly. With this in mind, we made the careful decision on what experiments to pursue, narrowing our list to: age of the corpora, quality filters, toxicity filters, and the choice of source domains. We carefully curated the choice of experiments in advance, without the luxury of multiple rounds of reflection and repetition, common in many NLP experimental settings. As a result, we struck a balance as best we could between the computational costs and reproducible validity.

Quality & Toxicity Filters Throughout the paper, we refer to document 'quality' and 'toxicity' purely as the decision made by the classifiers, as used in prior work (Brown et al., 2020; Chowdhery et al., 2022; Du et al., 2022; Touvron et al., 2023). However, it must be acknowledged these classifiers are imperfect (Friedl, 2023; Gargee et al., 2022; Lees et al., 2022), and the underlying definitions of quality and toxicity are shown to have high variance among human judges (Cortes and Lawrence, 2021). Though using these limited definitions risks reinforcing them, a concrete conclusion of our and others' work is that a single measurement of each of these is insufficient to capture either broader human values or practical objectives.

Blackbox APIs An additional limitation is our use of Perspective's API for evaluating the toxicity of generations. While most of our toxicity filters and evaluations were in a compressed time period, Pozzobon et al. (2023) have since demonstrated the irreproducibility of black-box APIs, which may have shifting implementations over time. We also believe that while this is the standard procedure for popular toxic generation benchmarks like RealToxicityPrompts, the reliance on APIs and narrow evaluation setting can have limited implications for toxic generation in real applications. For the time being, these are the best proxies we have.

Reproducibility Due to organizational constraints, we are unable to release the trained models or code in these experiments. As a central goal of this work is to bring greater shared knowledge and empirical analysis to poorly documented practices, we certainly feel this constraint is unfortunate and suboptimal. Nonetheless, we have closely documented all of the hyperparameters in pretraining, finetuning, and evaluation, and used (almost entirely) publicly available data and evaluations. We hope this careful documentation serves to improve the reproducibility of these experiments, and would defend the overarching contribution as greatly improving visibility and understanding into underdocumented pretraining practices.

Relevance to Zero- & Few-Shot Prompted Settings Our experiments focus on finetuned settings rather than zero- or few-shot prompting. This choice is motivated by finetuning being more applicable for 1.5B parameter models and also in many applied settings.

New & Contemporaneous Data Concurrent with our work, new pretraining datasets have been released: MPT (Team, 2023), RefinedWeb (Penedo et al., 2023), RedPajama (Computer, 2023), and Dolma (Soldaini et al., 2024). We expect our findings to generalize as these datasets contain largely the same ingredients as C4 and the Pile.

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A Expanded Literature Review

Pretraining Dataset Curation In Table 4 we highlight the data characteristics of some popular models. There have been dozens of general-purpose models trained for natural language understanding and generation tasks. Early models in this space, such as ELMO (Peters et al., 2018), BERT (Devlin et al., 2019), and BERT's various descendants (Liu et al., 2019; Lan et al., 2020), focused on strong finetuning performance for a variety of natural language inference tasks, as well as semantically meaningful language embeddings. These systems were trained on semi-curated datasets such as Wikipedia, BookCorpus (Zhu et al., 2015), and news articles from the One Billion Word Benchmark (Chelba et al., 2013). XLNet (Yang et al., 2019) broke away from this use of curated datasets to include documents from Common Crawl into their pretraining dataset. T5 (Raffel et al., 2020), which introduced the C4 dataset, was one of the first pretrained language models to train exclusively on Common Crawl data. Multilingual versions of T5 (Xue et al., 2021) and BERT were trained on Common Crawl and Wikipedia, respectively.

GPT-2 was one of the first models intended primarily for generation (Radford et al., 2019). Deeming Common Crawl too noisy to be practical for training generative models, they developed WebText, a dataset containing websites linked to from highly-ranked posts on Reddit. Subsequent generative models proposed mixing large amounts of noisy Common Crawl data with smaller corpora perceived as high-quality. The GPT-Neo model family (Black et al., 2022) trained on the Pile, which augments the Common Crawl with ArXiV, Stack Exchange, legal documents, books, Github, and other more curated sourced (Gao et al., 2020). More recently, OPT (Zhang et al., 2022) trained on the Pile augmented with social media data (Baumgartner et al., 2020), and LLaMA (Touvron et al., 2023) trained on C4 augmented with Github, Stack Exchange, books, and other sources. Pythia trained on the Pile, with and without duplication (Biderman et al., 2023). The BLOOM model family (Scao et al., 2022) trained on the ROOTS Corpus, which crowd-sourced a collection of "identified" datasets, coming from known, high-quality sources in a variety of languages. Finally, the OLMo model family (Groeneveld et al., 2024) trained on Dolma, a cleaned English-language dataset of three trillion tokens which augments C4 with sources spanning the web, code, social media, scientific papers, and Wikipedia (Soldaini et al., 2024).

All of the models mentioned so far are publicly available. However, companies are increasingly training their best models on proprietary datasets, with only limited hints as to the data composition. At Alphabet, models such as Gopher (Rae et al., 2021), GLaM (Du et al., 2022) , LaMDA (Thoppilan et al., 2022), and PaLM (Chowdhery et al., 2022) have been trained on mixtures of web text, books, news, code, Wikipedia, and dialog data. At OpenAI, GPT-3 (Brown et al., 2020) was trained on Common Crawl, WebText (GPT-2's training set), books, and Wikipedia. Subsequent versions of their model have also included code. Most of these models have acknowledged using various forms of filtering techniques to improve the quality of web-derived training data. These include classifiers designed to exclude content which looks least like "high-quality" sources such as books or Wikipedia (Chowdhery et al., 2022; Ouyang et al., 2022), us-

	Represented Domains (%)									Filters		Data	
Model	Wiki	Web	Books	Dialog	Code	Acad	Pile	C4	M-L	Tox	Qual	Pub	Year
BERT	76		24				✗	✗			H	Part	2018
GPT-2		100					✗	✗			H	Part	2019
RoBERTa	7	90	3				✗	✔			H	Part	2019
XLNet	8	89	3				✗	✔			H	Part	2019
T5	<1	99					✗	✔		H	H	✔	2019
GPT-3	3	82	16				✗	✔	7%		C	✗	2021
GPT-J/Neo	1.5	38	15	4.5	13	28	✔	Part			C	✔	2020
GLaM	6	46	20	28			✗	✔			C	✗	2021
LaMDA	13	24		50	13		✔	✔	10%	C	C	✗	2021
AlphaCode					100		✗	✗			H	✗	2021
CodeGen	1	24	10	3	40	22	✔	Part			H	Part	2020
Chinchilla	1	65	10		4		✔	✔		H	C	✗	2021
Minerva	<1	1.5	<1	2.5	<1	95	✔	✔	<1%		C	✗	2022
BLOOM	5	60	10	5	10	10	✔	✔	71%	H	C	Part	2021
PaLM	4	28	13	50	5		✗	✔	22%		C	✗	2021
Galactica	1	7	1		7	84	✔	Part			H	Part	2022
LLAMA	4.5	82	4.5	2	4.5	2.5	Part	✔	4%		C	Part	2020
Pythia	1.5	38	15	4.5	13	28	✔	Part			C	✔	2023
OLMo	<1	81	<1	3	13	2	Part	✔		C	C	✔	2024

Table 4: A list of **well-known language models and a quantitative breakdown of their pretraining data**, including represented domains; if the Pile or C4 are used, the percent of multilingual (M-L) data (meaning non-English and non-code); if Toxicity or Quality data filters were used, as either automatic Heuristics (H) or Classifiers (C); if the dataset is public (Pub), and what year the data was collected up to. If a dataset is "Part" public, then all of its constituent corpora are public, but not the final mixture. In Represented Domains, extended from (Zhao et al., 2023), Web includes the Common Crawl and other web scrapes; Dialog includes forum, social media and conversations; Academic includes research papers, textbooks, and mathematics.

ing Google's SafeSearch for identifying toxic content (Rae et al., 2021), and various heuristics based on document length and the presence or absence of certain words or characters.

Pretraining Dataset Analysis Dodge et al. (2021) find significant amounts of low-quality patent, military, and machine-generated text in C4, and a dearth of English text from American minority communities as well as from non-Western communities like India or Nigeria post-filtering, and so recommend against filtering. In contrast, Luccioni and Viviano (2021) recommend more robust filtering practices to curb the significant presence of hate speech and sexually explicit content they find in C4 even after filtering. Similarly, Kreutzer et al. (2022) find that multilingual pretraining corpora are also dominated by low-quality text, particularly for lower resource languages. Lee et al. (2022); Kaddour (2023) show the benefits of deduplicating pretraining datasets, which often contain a great deal of repeated content. Lastly, Zhao et al. (2023) reviews pretraining data sources, strategies for quality filtering, and the importance of data distribution. Their summary corroborates our findings regarding domain composition and quality filtering, in particular.

Data, Toxicity, & Quality Research into the quality and toxicity of datasets and their resulting models has seen mixed findings. All of the major models report using significant data pre-processing and toxicity/quality filters, including BERT, T5, BLOOM, OPT, ChinChilla, PaLM, LaMDA, and the GPT-3 series, with the largest of these now using classifiers. This widespread adoption suggests there are significant implicit benefits, even though they not often externally reported. GLaM does empirically report performance improvements from filtering, particularly on Natural Language Generation (NLG) tasks (Du et al., 2022).

However, in academia, a few works caution against the use of detoxification techniques, including data filters, which can increase model perplexity on underrepresented communities (Xu et al., 2021; Welbl et al., 2021). Welbl et al. (2021) also reports that a toxicity classifier reduces toxicity more than than applying data toxicity data filters, but Xu et al. (2021) show this yields the worst perplexity on underrepresented communities. Meade et al. (2022) further corroborate that improvements on bias benchmarks correlates with deteriorations in general language modeling abilities. Furthermore, investigating GPT-3's described quality filter, Gururangan et al. (2022) find its quality judgments are unaligned with factuality or literary acclaim but are instead aligned with some notion of langauge ideology more correlated with wealthier zip codes. Works in the vision domain show data filtering has important detoxification benefits but can reduce performance (Nichol et al., 2022) or introduce other biases (Nichol, 2022). In summary, pretraining data filters are ubiquitous in the development of non-toxic and high-quality models, but they are prone to reducing their abilities to serve underrepresented communities and may introduce new biases.

Additional work has shown that instruction tuning (Chung et al., 2022; Longpre et al., 2023) and forms of alignment tuning (Ouyang et al., 2022; Bai et al., 2022) have both reduced unwanted toxic generation.

Data & Time Natural language is known to evolve and change over time (Altmann et al., 2009; Labov, 2011; Eisenstein et al., 2014; Jaidka et al., 2018). As language's distribution shifts, the ability of models to perform well on new test sets has also been shown to degrade, due to their static knowledge of recent events, syntactic and semantic practices (Lazaridou et al., 2021; Agarwal and Nenkova, 2022; Longpre et al., 2021). Luu et al. (2022); Lazaridou et al. (2021); Liska et al. (2022); Yao et al. (2022); Zhang and Choi (2021); Jang et al. (2022) offer evaluation sets to measure this phenomena. Proposed remedies include finetuning on more recent data (Luu et al., 2022), adaptive/continuous pretraining (Lazaridou et al., 2021; Röttger and Pierrehumbert, 2021), data augmentation (Singh and Ortega, 2022), modeling text with its timpestamps (Dhingra et al., 2022). To our knowledge, no work has thoroughly investigated the effects of temporal degradation when pretraining from scratch.

Data & Domains The composition of public datasets, like C4 and the Pile, is guided mostly by licensing, which severely restricts availability. Even so, Villalobos et al. (2022); Nostalgebraist (2022); Hoffmann et al. (2022) suggest we are imminently exhausting high-quality text data on the web to train compute-optimal larger LMs, at least with existing training efficiency. This poses a challenge, given the demonstrated importance of high quality and diverse training data to strong generalization (Gao et al., 2020; Papadimitriou and Jurafsky, 2020). A great deal of literature has dedicated itself to adapting static pretrained models to new downstream domains, using domain adaptive pretraining (Gururangan et al., 2020), finding intermediate finetuning tasks (Pruksachatkun et al., 2020), dynamically balancing data sources (Wang et al., 2020), data selection (Iter and Grangier, 2021; Albalak et al., 2023), augmentation (Longpre et al., 2019), and active learning (Longpre et al., 2022). Another line of work demonstrates the potential of pretraining on carefully crafted synthetic data (Wu et al., 2022).

Most similar to this section of our work, Xie et al. (2023a) re-balance mixtures of the Pile to achieve more performant and efficient convergence. Xie et al. (2023b) use importance sampling to select subsets of the Pile most useful for target downstream tasks, in lieu of quality filters, to achieve 2% improvement on downstream tasks. Pruksachatkun et al. (2020) systematically benchmark the effects of intermediate finetuning tasks, similar to how we benchmark different compositions of pretraining tasks.

Model & Data Scaling Prior work has explored scaling model size (Kaplan et al., 2020; Tay et al., 2022; Du et al., 2022), the amount of pretraining data or the number of pretraining steps (Liu et al., 2019; Chowdhery et al., 2022; Brown et al., 2020). Chinchilla investigated and reported optimal compute scaling laws, expressing a relationship between model and data size (Nostalgebraist, 2022). Recent work has demonstrated that new abilities emerge at greater scale (Wei et al., 2022), but also that many of these benefits can be distilled or compressed into smaller models (Taori et al., 2023; Movva et al., 2022). In this work, we investigate how temporal pretraining misalignment varies on different model sizes, which to our knowledge was previously unanswered.

B Detailed Pretraining Data Experiments

We begin with two publicly available pretraining datasets: C4 (Raffel et al., 2020) and the Pile (Gao et al., 2020). Both have received basic initial heuristic filtering for English language and content quality. We further deduplicate both datasets using the approximate deduplication method described in Lee et al. (2022).

C4 (Raffel et al., 2020) The English Colossal Clean Crawled Corpus (C4) is a snapshot of Common Crawl from 2019, which includes a mix of news, legal, wikipedia, and generic web documents (Dodge et al., 2021), filtered for well-formed En-

CATEGORY	COMPONENTS	SIZE	DESCRIPTION
CC	Pile-CC	227 GB	A filtered set of Common Crawl websites, scraped with JusText (Endrédy and Novák, 2013).
OPENWEB	OpenWebText2	63GB	Scraped OpenWebTextCorpus using upvoted Reddit outgoing links.
WIKIPEDIA	Wikipedia (en)	6 GB	The English scrape of Wikipedia.
BOOKS	Books3, BookCorpus2, Gutenberg (PG-19)	118 GB	The Bibliotik general literature collection, PG-19's pre-1919 western classics, and BookCorpus's set of yet unpublished works.
PUBMED	PubMed Central, PubMed Abstracts	109 GB	Biomedical articles from 1946 to present
ACADEMIC	ArXiv, PhilPapers, NIH ExPorter	60 GB	Preprint academic papers in Math, Computer Science, Physics, and Philosophy.
CODE & MATH	Github, StackExchange, DM Mathematics	135 GB	Code repositories, documentation, coding questions and answers, and mathematical problems.
LEGAL	FreeLaw, USPTO Backgrounds	74 GB	Court filings, judicial opinions, and patents
SOCIAL	Ubuntu IRC, EuroParl, Enron Emails, HackerNews, OpenSubtitles, Youtube-Subtitles	33 GB	Movie and video subtitles, chat logs, emails, and text from social news websites.
BASE	All	825 GB	A wide mix of online text from the web, wikipedia, books, academic articles, code, legal, and social sources.

Table 5: **Partitions of the Pile's Data Sources into Domains** The Pile contains 22 distinct sources of data, which we manually partition into 9 thematically similar domain clusters.

glish text.[3] While the original version of C4 filtered out any documents containing words from a "bad words list", our version does not. C4 remains one of the most widely adopted fully open source datasets for textual training, given its permissive license. It is a key component of many LMs, as shown in Table 4.

The Pile (Gao et al., 2020) is an 800GB dataset consisting of data from 22 sources. These include a Common Crawl web scrape as well as more diverse collections of academic, books, coding, medical, legal and social sources (see Table 5), which more closely resemble the reported data sources in larger non-open source models like PaLM (Chowdhery et al., 2022), Chinchilla (Hoffmann et al., 2022), and the GPT-3 series (Brown et al., 2020). Note that the Pile's corpora composition was manually selected, and some options were excluded on the grounds of being too toxic or explicit.

B.1 Data Curation Choices

We evaluate variations in the pretraining data based on three categories of interventions.

Dataset Age We create new versions of C4 by regenerating snapshots of the Common Crawl from different years. Multiple time-based collections are not available for the Pile.

Domain Filtering Both C4 and the Pile draw from multiple distinct data sources, but the Pile explicitly delineates 22 distinct sources from web pages, wikipedia articles, code repositories, online forums, legal texts, and research paper archives. To control for the topical content of the pretraining collection, we selectively remove documents from different domains (see Table 5).

Content Filtering Datasets derived from the Common Crawl and other weakly curated internet sources tend to contain large amounts of low-quality, toxic, or offensive content. As a result, curators often apply content-based filters. Deciding what to include and what not to include is a challenging and context-dependent problem: A "high-quality" Reddit post does not look like a "high-quality" academic paper; and even with academic papers, quality measured by peer review has high variance (Cortes and Lawrence, 2021).

There are several approaches to determining document appropriateness. The simplest filters use features such as sentence length, presence of stopwords and punctuation, and repetitiousness to identify pages that do not contain usable text (Rae et al., 2021; Yang et al., 2019; Laurençon et al., 2022; Zhang et al., 2022). Negatively-defined filters identify a category of text to be removed, and assume that everything else is usable. For example, Raffel et al. (2020) remove documents that contain words from a list of "bad words". Positively-defined filters identify a category of text to keep, and remove everything else (Du et al., 2022; Touvron et al., 2023; Brown et al., 2020).

[3] https://commoncrawl.org/

In this work, we evaluate the impact of two document-level, classifier-based filters that have been used widely in the development of state-of-the-art language models. These include negatively-defined, *toxic* content (text that is profane, explicit, insulting, or threatening) and postively-defined *quality* content (text similar to known "high-quality" sources). It is important to emphasize that we do not have ground truth: for the purposes of this paper we will use the description *toxic* or *quality* to refer to a document that triggers one of these automated classifiers, *not* to indicate a document that achieves those characteristics for a human reader.

Quality Filters Most recent language models create quality classifiers to distinguish between "high-quality" corpora and other documents (Table 4). These are usually then applied to crawled web pages. Examples of high-quality reference corpora are (1) Wikipedia, WebText and books for GPT-3 (Brown et al., 2020), (2) Wikipedia, books and a few selected websites for PaLM (Chowdhery et al., 2022) and GLaM (Du et al., 2022), and (3) pages used as references in Wikipedia for LLaMA (Touvron et al., 2023). In our work, we use the classifier employed by PaLM and GLaM, which assigns each document a score from 0 (high quality) to 1 (low quality). We experiment with removing documents that fall above four quality thresholds: 0.975, 0.95, 0.9, 0.7, along with an inverse filter that instead removes the *highest* quality documents *below* a threshold.

Toxicity Filters To identify toxic content, we use Jigsaw's Perspective API [4], which was trained on comments from online forums and assigns toxicity scores based on whether annotators found the comment to contain profanity/obscenity, identity-based negativity, insults, or threats. While the Perspective API, as with any classifier, has been shown to be imperfect—it falsely labels some neutral text as toxic and its training data reflects the normative values of its annotators—it has been shown to be far more accurate than heuristic and rule-based classifiers (Friedl, 2023; Gargee et al., 2022; Lees et al., 2022).

The Perspective API outputs a score from 0 (unlikely to be toxic) to 1 (very likely to be toxic). The documentation recommends using a score threshold of anywhere from 0.3 to 0.9 to filter documents, depending on the practitioner's goals.[5] We experiment with removing documents with scores above five different toxicity threshold values 0.95, 0.9, 0.7, 0.5, and 0.3. Documents above a given threshold are filtered out, along with an inverse filter that removes documents with the *least* predicted toxicity *below* a threshold.

In addition to the classifier-based filter, we also experiment with the n-gram based filter used by Raffel et al. (2020) in the original version of the C4 dataset. This filter removes all documents that contain any word present in the "List of Dirty, Naughty, Obscene, or Otherwise Bad Words".[6]

CURATION	BASE DATASET	VARIANT	TOKENS
Age	**C4**	2013	246B
		2016	206B
		2019	226B
		2022	360B
Quality filtering	**C4**	Full	226B
		$T = 0.975$	205B
		$T = 0.950$	190B
		$T = 0.900$	166B
		$T = 0.700$	103B
Toxicity filtering	**C4**	Full	226B
		$T = 0.95$	221B
		$T = 0.90$	215B
		$T = 0.70$	194B
		$T = 0.50$	171B
		$T = 0.30$	137B
Domain ablations	**The Pile**	Full	157B
		No Social	155B
		No Wiki	153B
		No Books	146B
		No OpenWeb	146B
		No Legal	143B
		No Academic	136B
		No PubMed	133B
		No Code	127B
		No Common Crawl	114B

Table 6: **Pretraining dataset sizes in tokens.**

B.2 Final pretraining dataset sizes

Table 6 shows the final size of each curated dataset variant in billions of tokens.

C Training details

This section provides further details on the methodology and hyperparameter settings used for pretraining, finetuning, and evaluation.

We use standard T5 SentencePiece tokenizers with a subword vocabulary of 32,128 (Kudo and Richardson, 2018). To allow for a model that can generate without finetuning but also perform well

[4] https://www.perspectiveapi.com

[5] See https://developers.perspectiveapi.com/s/about-the-api-score

[6] https://github.com/LDNOOBW/List-of-Dirty-Naughty-Obscene-and-Otherwise-Bad-Words

Parameter	LM-XL	LM-Small
TPUs	8x8x8	8x8
Batch Size	4096	4096
Sequence Length	512	512
Training Steps	88,064	88,064
Dropout	0.0	0.0
Base Learning Rate	0.5	
Decay Factor	0.5	
Warmup Steps	1000	
Steps per Decay	20000	

Table 7: **Pretraining hyperparameters** We adopt default pretraining hyperparameters from Wang et al. (2022), who select their parameters to fairly compare across a wide range of T5-based pretraining and architecture experiments.

Parameter	Tox-Identify	Natural Qs	SuperGLUE	Time
LM-XL				
TPUs	8x8	8x8	8x8	8x8
Sequence Length	128	512	512	128
Batch Size	128	128	128	128
Dropout	0.1	0.1	0.1	0.1
Training Steps	10k	50k	100k	See Table 10
Learning Rate	1e-3	1e-3	1e-3	See Table 10
Eval Metric	AUC-ROC	Acc	(By Dataset)	See Table 10
LM-Small *(where different)*				
Training Steps	30k	50k	100k	See Table 10

Table 8: **Finetuning and Evaluation Parameters for each set of Downstream Tasks.** We report the finetuning hyperparameter settings and evaluation metric used for finetunting and evaluating the pretrained models. We conduct finetuning for four sets of tasks: toxicity identification tasks (Toxigen, Social Bias Frames, and DynaHate), Natural Questions (for pretraining domain transfer analysis), general NLU performance (SuperGLUE), and the Time tasks (including PubCLS, NewSum, PoliAff, TwiERC, and AIC). For T5 Small models, we modify the number of training steps accordingly, as shown in the last row.

after finetuning, we rely on the extensive experiments of Wang et al. (2022). Their empirical results suggest these criteria are met with a Causal Decoding architecture with a Full Language Modeling pretraining objective ("CD-FLM"), which permits generation without finetuning, followed by a Prefix Language Modeling objective (PLM) for finetuning, where the causal attention mask is removed from the original prompt.

C.1 Pretraining Details

Our two pretraining datasets are C4 (Raffel et al., 2020) and the Pile (Gao et al., 2021). We use the same vocabulary for both as used in the original T5 from Raffel et al. (2020). All training is conducted using T5X (Roberts et al., 2022) and Tensorflow (Abadi et al., 2016) on TPUs. Specific hyperparameters for LM-XL and LM-Small pretraining are detailed in Table 7.

C.2 Finetuning Details

Unless otherwise noted, evaluation was performed by finetuning on the train set for each benchmark task, and then evaluating on either the validation or test set (specified in each section). We used the standard prompts accompanying each downstream training dataset and performed standard finetuning without any adapters. Finetuning hyperparameters are given in Table 8.

D Evaluation Details

We compare the general utility of the different models, as well as their performance on tasks we expect to be influenced by the dataset characteristics being ablated. Since we are comparing the performance of different pretrained models, we evaluate the performance of each pretrained model on downstream tasks by finetuning the model on the relevant dataset for each task and evaluated on the same testing data (using the default splits for each task unless otherwise noted). As a result, any *systematic* differences between finetuned results can only be attributable to differences in pretraining. For all tasks we report mean performance relative to a baseline, usually the performance of models trained on an unfiltered dataset.

Evaluating Domain Generalization We evaluate on the union of two question-answering benchmarks: Machine Reading for Question Answering (MRQA) (Fisch et al., 2019) and UnifiedQA (Khashabi et al., 2020), which together consist of 30 unique QA datasets. These QA datasets span a range of domains, allowing us to measure the impact of topic alignment (see Table 9).

Evaluating Temporal Misalignment Prior work has shown that a dataset's collection time can affect the downstream model's abilities (Lazaridou et al., 2021; Agarwal and Nenkova, 2022). Luu et al. (2022) release several datasets in which increasing temporal distance between *finetuning* and evaluation time decreases test performance. We choose 5 of these datasets from varying domains to evaluate whether a similar phenomenon exists between *pretraining* and evaluation time: PubCLS, NewSum, PoliAffs, TwiERC, and AIC.

Evaluating Toxic Generation Generateing profane, sexually explicit, insulting, or obscene text or

CATEGORY	DATASETS	DESCRIPTION
WIKI	AmbigQA, DROP, HotpotQA, NaturalQuestions, Quoref, RelationExtraction, ROPES, SearchQA, SQuAD-1, SQuAD-2, TriviaQA	Datasets with Wikipedia text.
WEB	AmbigQA, CommonsenseQA, DuoRC, NaturalQuestions, NewsQA, SearchQA, TriviaQA	Datasets partially sourced or collected from the web, including user logs and news.
BOOKS	NarrativeQA	A dataset sourced from books.
BIOMED	ARC-Easy, ARC-Hard, BioASQ, TextbookQA	Datasets with high-school or graduate level scientific or medical content.
ACADEMIC	AI2-Elementary-Science, ARC-Easy, ARC-Hard, RACE, ROPES, TextbookQA	General academic data and exams.
COMMON SENSE	CommonsenseQA, PhysicalIQA, SocialIQA	Datasets which test common sense reasoning.
CONTRAST SETS	Contrast-Set-DROP, Contrast-Set-Quoref, Contrast-Set-ROPES	Datasets re-configured as Contrast Sets (Gardner et al., 2020), which are manual perturbations to make examples more challenging.

Table 9: **Partitions of Question Answering evaluation datasets from the UnifiedQA (Khashabi et al., 2020) and MRQA (Fisch et al., 2019) collections.** To evaluate the performance of pretraining strategies on different text domains, we assign datasets into categories corresponding to their source material:web-based, wikipedia, academic, biomedical, or and/books). Certain datasets are also designed specifically to test advanced common sense reasoning, or decision boundaries using contrast sets (Gardner et al., 2020). Datasets can belong to multiple categories.

text that attacks identity groups or targets protected human attributes limits the applications LMs may be used for (Gehman et al., 2020). We evaluate this behavior with language model prompts designed to elicit biased or toxic outputs related to gender, race, and religion (Chowdhery et al., 2022), and then measuring the fraction of generated continuations which are assigned a high toxicity score by the Perspective API (see Appendix D.1 for details). We also use the RealToxicityPrompts dataset (Gehman et al., 2020), which consists of text excerpts from the OpenWebText dataset (Gokaslan* et al., 2019) that were labeled as toxic by the Perspective API.

Evaluating Toxicity Identification While some applications require LMs not to generate toxic text, in other applications it is important for LMs to *recognize* such language. *Toxicity Identification* has become particularly critical as a step in content moderation for major communication platforms (NYT, 2020; Singh, 2019). Definitions vary by setting, targeting hate speech, stereotypes, social bias, or some definition of toxicity. We evaluate this ability with a variety of toxicity interpretations, using train and test sets from Social Bias Frames (SBF, Sap et al., 2020), DynaHate (DH, Vidgen et al., 2021), and Toxigen (Hartvigsen et al., 2022).[7]

[7]We use the offensiveness detection task from Social Bias Frames. DynaHate releases 4 rounds of adversarial datasets, for which we use the test sets for Round 3 (R3) and Round 4 (R4).

D.1 Toxicity Evaluation Details

In this section we describe the evaluation details for the results presented in Section 5.

Toxicity Identification For toxicity identification evaluation, we finetune separately on each of the following datasets: Social Bias Frames (SBF, Sap et al., 2020), DynaHate (DH, Vidgen et al., 2021), and Toxigen (Hartvigsen et al., 2022). We then evaluate on their respective test sets, using AUC-ROC, which computes the error rates over different classification thresholds. Note that each of these datasets has a slightly different version of toxicity they are evaluating for: biases and offensive stereotypes implied in language for Social Bias Frames, types of hateful speech like animosity, dehumanization, and derogation for DynaHate, and adversarial, subtle and implicit forms of toxicity towards minority groups (e.g., stereotyping, microaggressions), for Toxigen. We average over them to understand a model's capabilities in identifying toxicity broadly.

Toxic Generation For toxic generation, we do not do any finetuning. Instead, we evaluate how often each model generates toxic continuations. To measure a model's propensity to generate sexual, profane, or toxic responses to a mix of benign and malicious inputs, we use prompts from the RealToxicityPrompts (Gehman et al., 2020) dataset, as well as the prompt set developed to explore representational bias in PaLM (Chowdhery et al., 2022).

For each model, we generate 25 responses per prompt, using top-k sampling ($k = 40$), with a temperature of 1.0. Following Chowdhery et al. (2022), we use the Perspective API to score responses, labeling those with a toxicity score $>= 0.5$ as toxic. We compute the probability one of these generations per prompt is toxic, for two evaluation splits, one with non-toxic inputs, and one with toxic inputs. The resulting evaluation metrics are denoted RPT-T (for Toxic inputs) and RPT-NT (for Non-Toxic inputs).

The Representational Bias benchmark was constructed from the identity terms in (Rae et al., 2021) and is described in more detail in Chowdhery et al. (2022). It uses templates constructed for several identity markers, for which we use the subset related to gender, race, and religion. Following the practice with RealToxicityPrompts, we sample 50 responses per prompt, use top-k sampling ($k = 40$, temperature $= 1.0$), and then classify a response as toxic if Perspective API evaluates its toxicity score $>= 0.5$. We average the toxicity rates per identity marker and per example to compute the overall RepBias score, where higher indicates more toxic responses were produced on average. We also compute the 95% confidence interval to show where changes in mean are significant.

D.2 Time Evaluation Details

This section describes the evaluation details for the results presented in Section 4. In applied settings, the available training data (either for pretraining or finetuning) may be from different years than the test-time data. To mimic these situations, Luu et al. (2022) construct several datasets segmented by the year they are collected from in order to measure the performance impact of differences in the time of collection of finetuning and evaluation splits. As described in Appendix D, we select 5 of the datasets that are shown to be quite sensitive to these temporal misalignments, and that cover different tasks and data sources. These tasks are summarization, named entity recognition, classifying political affiliation, classifying academic topic, and classifying the news source.

Due to the unique nature of each of these tasks in the temporal degradation experiments, we simply finetune on each task individually, before evaluating on their respective test sets. For each dataset, we finetune using 4x4 TPUs with a batch size of 64, a maximum sequence length of 128, and we validate every 500 training steps. We select the test set score with the highest validation accuracy across training. The best learning rate and the total number of steps required to reach convergence varied by model and model size, and are reported in Table 10. These hyperparameters are chosen based on initial experiments attempting to produce stable learning curves which peak near the values observed in Luu et al. (2022).

			LM-XL		LM-SMALL	
DOMAIN	TASK	METRIC	LR	STEPS	LR	STEPS
NEWS	PUBCLS	Acc	1e-4	30k	1e-3	30k
	NEWSUM	Rouge-L	5e-4	40k	1e-3	40k
TWITTER	POLIAFF	Acc	1e-4	15k	1e-4	15k
	TWIERC	Acc	1e-4	30k	1e-3	30k
SCIENCE	AIC	Acc	1e-4	30k	1e-3	60k

Table 10: **Time Dataset & Training Details**: For each of the five datasets used to evaluate the model's ability over different temporal periods, we report the learning rate and number of steps used in each model size. These hyperparameters were chosen to ensure consistent convergence and stability within our infrastructure settings.

We follow Luu et al. (2022)'s exact prescription in calculating Temporal Degradation (TD), as well as their reported Pearson correlation measurements (r). Temporal degradation can be interpreted as the average rate of deterioration in performance for a time period, measured in years. Since a temporal deterioration score is calculated per evaluation year, we average over all evaluation years to compute a final TD score for a dataset. Furthermore, each dataset has a different span of available training and evaluation years. To account for this, we follow Luu et al. (2022) in presenting the Pearson correlation coefficient, which presents the strenght of the relationship between time differences and performance deterioration. We also replicate the Wald test with null hypothesis that the slope is zero.

For evaluating the temporal degradation of pretraining, TD_p, we modify Luu et al. (2022)'s original formula to measure the different $D(t' \rightarrow t)$ where t' is now the pretraining year. However, in this setting, performance samples are represented with different finetuning years. To account for this, we only compare the relative performance changes of the pretraining year t_p, against models with the same finetuning t_f and evaluation years t_e. In other words, given $S_{t_p \rightarrow t_f \rightarrow t_e}$, we will only compare its performance to $S_{t'_p \rightarrow t^f \rightarrow t^e}$ where $t'_p \neq t_p$, but t^f

and t^e are fixed to their respective values.

$$D(t'_p \to t_e) = - (S_{t'_p \to t''_f \to t_e} - S_{t_p \to t''_f \to t_e}) \cdot \text{sign}(t'_p - t_e)$$

In some edge cases, there is no evaluation year equivalent to a pretraining year, $\forall t \in T, t_p \neq t_e$, and so the term $S_{t_p \to t''_f \to t_e}$ does not exist. In this case, we set this term to be the one where t_p and t_e are closest. And, as before, the precise term used will depend on which version of t_f is being calculated for.

D.3 Evaluating Domains with Question Answering Datasets

This section describes the evaluation details for the results presented in Section 6. These experiments involve pretraining models with different subsets of the corpora from the Pile (Gao et al., 2020) and seeing the effects on a variety of downstream evaluation domains, represented by question answering datasets. As such, we are able to map the effects of pretraining domains to evaluation domains.

First, we discuss the construction of the pretraining domains. We partition the Pile's source datasets into categories representing thematically similar sources of data, as seen in Table 5. We refer to these categories as Domains. These domain partitions are subjective and cannot perfectly separate out text into these categories. For instance, Wikipedia, Books, and Common Crawl data inevitably contain some Academic information, but overall these partitions represent distinct features (see Section 3) that we have attempted to delineate by areas of interest to practitioners and researchers. Prior work has attempted to measure, emphasize, or target (either for inclusion or exclusion) the particular categories of data we've used in our partitions, such as more books and structured data (Brown et al., 2020; Chowdhery et al., 2022), code data (Chen et al., 2021), and legal data (Dodge et al., 2021), among others.

The Domains of the Pile were then each separately ablated from pretraining to understand the effect of their absence. To evaluate their absence on the performance of downstream domains, we chose to use the question answering task expressly because there is a wide variety of similarly formatted evaluation datasets available. For these question answering datasets we train only on Natural Questions (Kwiatkowski et al., 2019), a popular QA dataset, to teach the model the general task. For evaluation, as described in Section 2.3, we use UnifiedQA (Khashabi et al., 2020) and MRQA (Fisch et al., 2019)'s collection of datasets to evaluate how each pretrained model performs on a given "domain", or set of datasets with similar source characteristics. We partition the question answering datasets from UnifiedQA and MRQA into five categories. Datasets with Wikipedia documents represented in their collection are assigned to the WIKI category, datasets with scraped web documents or news are assigned to the WEB category, and so on. Datasets may belong to multiple categories, depending on how they were constructed. The question answering evaluation partitions are shown in Table 9. Finally, we evaluate on each question answering dataset and report the average F1 score for each category.

E Impact of Data Curation on Data Composition: Further Analysis

Feature Definitions As discussed in Section 3, we calculated a set of features across all datapoints to better understand the distribution shifts for each ablation. The full list of features is as follows:

- **Profanity, Toxicity, and Sexually Explicit** The Perspective API classifies text as violating or passing each of these categories, as described in Appendix B.1.
- **Text Quality** The same bag-of-words-based linear classifier as used in PaLM (Chowdhery et al., 2022) and GLaM (Du et al., 2022), is used to distinguish between text that looks like Wikipedia and books from other text, as described in Appendix B.1.
- **Personally Identifiable Information (PII)** A basic classifier, similar to Google Cloud NLP (2023b), detects the presence of four categories of personally identifiable information: **names**, **phone numbers**, **addresses**, and **emails**.
- **Readability** The Flesch–Kincaid readability test (Kincaid et al., 1975) is applied to each document, assigning documents a grade level based on the number of words per sentence and number of syllables per word.
- **Average Word Length** Measured in characters.
- **Document Length** Measured in characters.

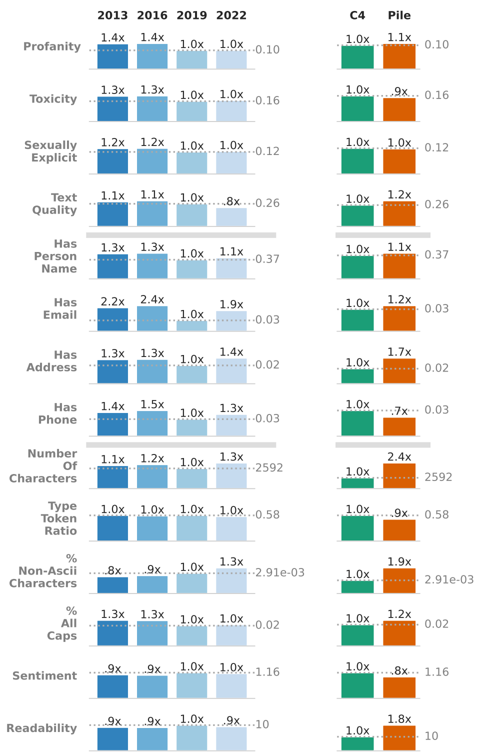

Figure 6: **Time snapshots of C4 (left) and feature differences across C4 and the Pile (right).** Bar height indicates average feature value of each dataset, except for the PII categories which show the fraction of datapoints containing that PII type. The numbers are the fraction difference between the dataset and the baseline, which in this case is C4. The gray dashed line and gray number show the actual value for the baseline.

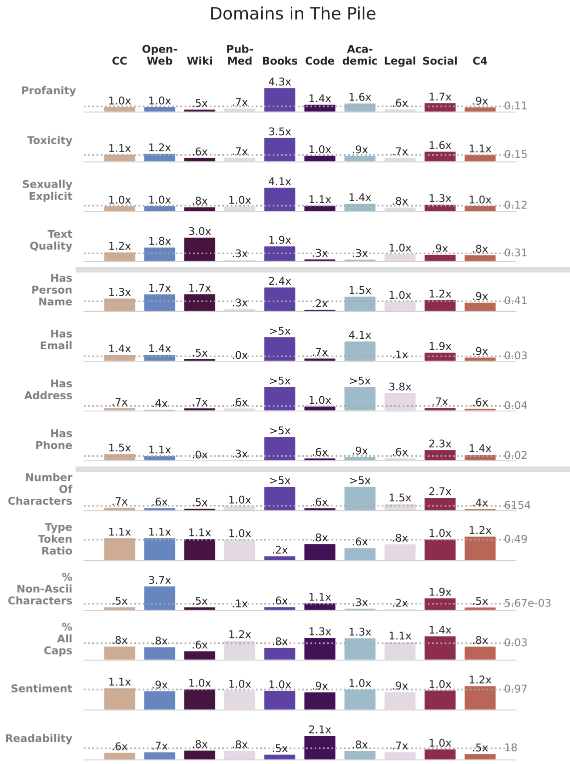

Figure 7: **Feature differences across domains of the Pile.** Bars show the ratio between the mean feature value for the domain and the mean value for the Pile, which is indicated by a horizontal gray line. For example, Wiki text has half the profanity and three times the quality values as the average for the Pile.

- **Non-ASCII Characters** Measured as a percentage of all characters in the document.
- **All-caps Words** Measured as a percentage of all words in the document.
- **Type-Token Ratio** A measure of the lexical diversity, or the ratio of unique tokens to total tokens (Bender, 2013).
- **Sentiment** The score assigned by a classifier similar to Google Cloud NLP (2023a), evaluating the overall sentiment of the text along a spectrum from positive to negative.

C4 vs the Pile Figure 6 shows the differences between the two source datasets. Documents in the Pile are on average longer (2.4x), have more non-ASCII characters (1.9x) indicating greater linguistic range, and are also measured as higher quality (1.2x) and more readable (1.8x). Pile documents also contain more PII, in particular personal names, addresses, and emails.

PII However, Pile documents with high toxicity are 1.4-1.9 times more likely to have PII of various kinds, while in C4 this is not true. Documents classified as high quality in C4 were longer (1.3x and 1.2x), and had more names (1.6x and 1.8x), but fewer emails, addresses, and phone numbers.

Time in C4 Figure 6 shows that the percentage of non-ASCII characters increased steadily in more recent years while the measured text quality declines. This growth may be due to increasing non-English content, but could also correspond to rising use of emojis and non-ASCII punctuation. Toxicity scores also decrease slightly in later years, while sentiment increases.

Domains in the Pile Figure 7 compares domains in the Pile, as discussed in Section 3.

F Experimental Results

In this section, we lay out the raw results for our toxicity, quality, and temporal degradation evaluations, spanning several evaluation datasets.

F.1 Extended Temporal Degradation Results

Luu et al. (2022) measure the temporal degradation due to finetuning and evaluation misalignment. First we replicate the findings of Luu et al. (2022), which demonstrate a finetuning and evaluation misalignment causes temporal degradation in performance (Figure 8). Next, we extend this framework to measure the effect specifically of *pretraining* and evaluation misalignment, and find a similar though slightly less prominent pattern (Figure 9).

Next we share the original evaluation results from which we computed the temporal degradation values for both finetuning and pretraining. These contain a cross-section of the scores produced using a given pretraining year (y-axis), finetuning year(s) (y-axis), for an evaluation year (x-axis). These results, Tables 11 to 14, are provided for both LM-XL and LM-SMALL, for comparison.

F.2 Extended Toxicity & Quality Filtering Results

We also provide full results for our experiments with toxicity and quality filters, presented in Section 5. The evaluation results of the models with *toxicity* filters applied to their data are visualized in Figure 4 (top), with full details in Appendix F.2. The evaluation results of the models with *quality* filters applied to their data are visualized in Figure 4 (bottom) and detailed in Table 16.

We also show full results for the effects of quality and toxicity filters on the QA domains that we evaluate on. See Figure 10 for the effects of quality filters, and Figure 11 for the effects of toxicity filters.

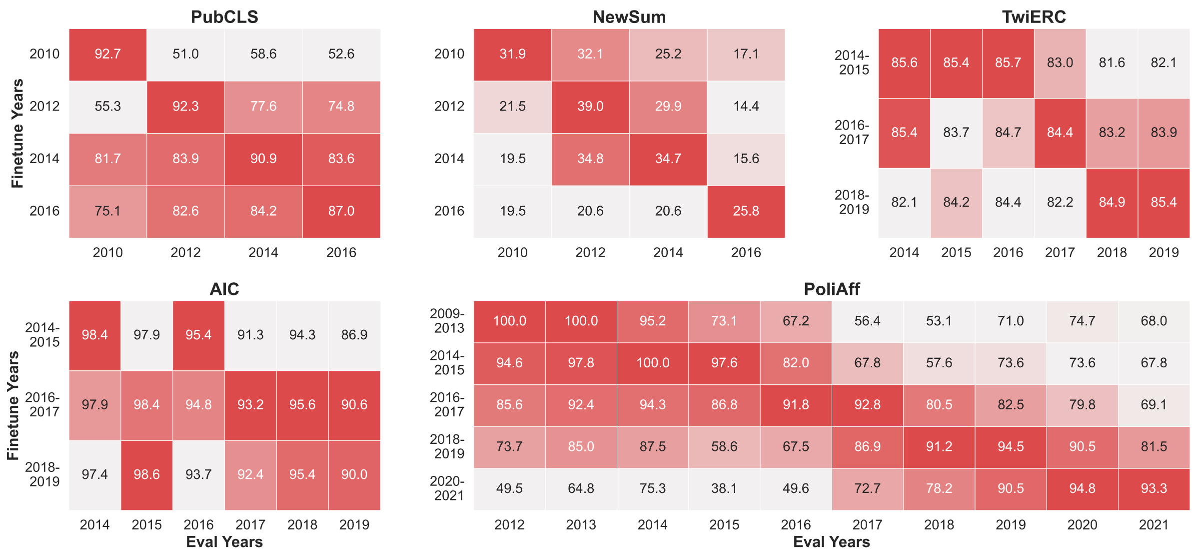

Figure 8: **A replication of how temporal misalignment in finetuning affects task performance (Luu et al., 2022). In contrast to Figure 9, which shows the effects of pretraining misalignment, this figure focuses on the more well established effect of finetuning misalignment.**

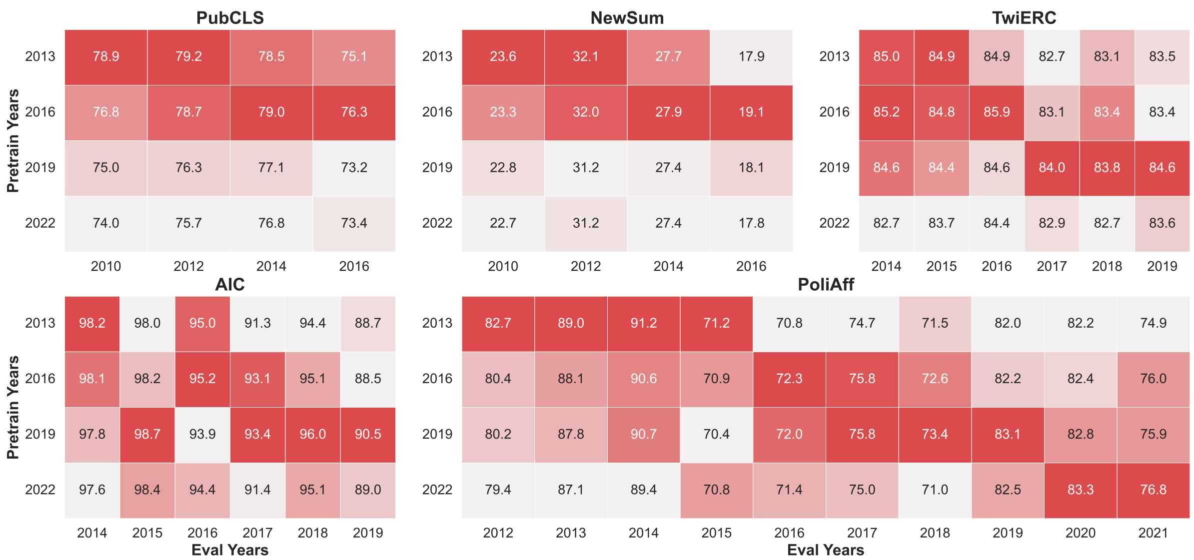

Figure 9: **Temporal Misalignment between Pretraining and Evaluation causes performance degradation**. Four LM-XL's, each pretained on a different C4 time split, are evaluated on each time split across five datasets. Heatmap colors are normalized by column, following Luu et al. (2022) to show the best pretraining year for each evaluation year.

Pretrain Time	Finetune Time	Eval Time 2010	2012	2014	2016
LM-XL					
2013	2010	93.7	51.9	58.4	52.5
	2012	60.2	94.6	78.4	75.6
	2014	83.1	85.6	90.8	84.8
	2016	78.7	84.7	86.2	87.6
2016	2010	93.8	51.6	59.2	53.2
	2012	55.2	93.9	79.5	77.0
	2014	81.5	86.2	92.8	85.6
	2016	76.9	82.9	84.3	89.6
2019	2010	92.9	50.6	58.6	52.2
	2012	53.4	90.5	75.9	72.9
	2014	81.3	83.2	90.6	82.8
	2016	72.3	81.1	83.4	84.8
2022	2010	90.5	49.9	58.4	52.4
	2012	52.4	90.4	76.4	73.9
	2014	80.9	80.7	89.3	81.1
	2016	72.3	81.7	83.0	86.1
LM-Small					
2013	2010	92.9	51.9	60.2	54.1
	2012	55.4	93.3	75.7	75.9
	2014	78.2	81.9	89.9	82.5
	2016	70.5	80.0	80.7	87.4
2016	2010	93.0	51.8	58.8	53.2
	2012	56.7	92.9	77.7	75.5
	2014	77.3	80.2	89.6	81.4
	2016	69.9	80.1	82.1	87.7
2019	2010	92.9	51.3	59.2	53.0
	2012	58.9	93.3	76.4	75.6
	2014	78.4	82.1	90.2	82.7
	2016	69.8	81.4	80.8	87.7
2022	2010	93.3	51.6	59.1	53.2
	2012	56.2	93.2	75.6	75.1
	2014	76.4	81.0	90.1	81.7
	2016	67.8	80.4	80.1	86.8

Pretrain Time	Finetune Time	Eval Time 2010	2012	2014	2016
LM-XL					
2013	2010	33.3	32.8	24.6	16.8
	2012	21.4	39.5	30.0	14.1
	2014	19.9	35.0	35.1	14.9
	2016	19.9	21.2	21.1	25.7
2016	2010	31.9	33.3	27.1	17.8
	2012	21.4	39.0	30.1	15.3
	2014	20.2	35.0	34.5	17.2
	2016	19.6	20.8	20.0	26.1
2019	2010	31.8	31.6	24.8	16.7
	2012	21.4	39.1	29.3	13.6
	2014	18.6	33.8	34.0	15.7
	2016	19.5	20.1	21.4	26.2
2022	2010	30.7	30.8	24.4	17.2
	2012	21.6	38.2	30.1	14.3
	2014	19.5	35.5	35.0	14.7
	2016	19.1	20.4	19.9	25.2
LM-Small					
2013	2010	22.7	25.0	20.1	13.5
	2012	14.0	24.5	19.5	9.9
	2014	13.1	21.8	21.3	9.6
	2016	14.1	17.8	17.5	18.4
2016	2010	22.1	25.5	20.7	14.0
	2012	14.0	23.8	19.7	9.6
	2014	13.5	22.8	21.5	10.0
	2016	14.1	19.5	19.1	18.5
2019	2010	23.5	26.4	21.4	14.3
	2012	14.5	25.4	20.6	10.1
	2014	14.0	23.6	22.5	10.5
	2016	15.1	20.1	19.2	18.5
2022	2010	23.4	26.2	21.1	14.1
	2012	13.9	24.4	19.4	9.5
	2014	13.6	23.2	21.7	9.7
	2016	14.3	19.3	18.3	18.2

Table 11: *Left:* Full results on the **PubCLS** temporal task splits from (Luu et al., 2022). This task evaluates news article source classification, measured with Accuracy. *Right:* Full results on the **NewSum** summarization task temporal splits from (Luu et al., 2022), evaluated in Rouge-L.

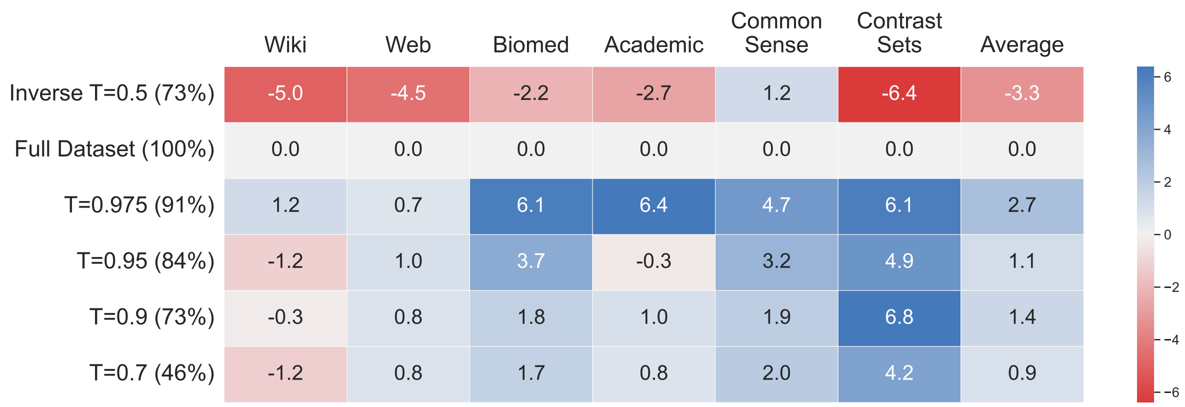

Figure 10: **Quality filtering C4 increases LM-XL's downstream performance on all QA task domains, except for Books.** The quality filter threshold is on the x-axis, with percentage of training data remaining in parenthesis. Each column represents a set of QA evaluations from a domain. The 'Full Dataset' is unfiltered, and the 'Inverse' filter removes the highest quality data instead.

PRETRAIN TIME	FINETUNE TIME	2014	2015	2016	2017	2018	2019	2014	2015	2016	2017	2018	2019
		EVAL TIME											
		LM-XL						LM-SMALL					
2013	2014-2015	98.0	97.7	94.6	88.0	93.1	83.4	86.1	85.5	85.7	83.2	80.5	81.9
	2016-2017	98.2	96.6	94.4	91.6	94.0	88.2	86.1	84.0	84.7	83.9	84.0	83.7
	2018-2019	97.4	97.6	94.0	91.5	95.4	87.9	82.9	85.2	84.2	81.2	84.6	85.0
2016	2014-2015	98.4	98.3	95.1	87.5	92.5	82.7	86.2	85.7	86.2	82.7	81.5	81.7
	2016-2017	97.8	97.5	94.6	91.9	93.3	86.7	86.7	84.1	86.0	85.1	83.2	83.4
	2018-2019	96.7	98.0	94.1	91.3	95.7	87.6	82.7	84.6	85.5	81.5	85.5	85.0
2019	2014-2015	98.3	97.7	94.4	88.4	93.7	82.1	85.6	85.4	85.3	83.1	82.2	83.2
	2016-2017	97.7	97.5	93.5	89.6	94.3	88.6	85.7	83.8	83.8	85.4	83.5	84.8
	2018-2019	96.4	97.9	93.5	90.3	95.9	88.1	82.4	83.9	84.7	83.5	85.6	86.0
2022	2014-2015	98.4	98.1	95.1	88.1	94.1	84.6	84.4	84.8	85.6	83.0	82.0	81.7
	2016-2017	97.9	97.2	93.8	89.4	94.6	88.3	83.2	83.1	84.5	83.1	82.2	83.6
	2018-2019	96.5	97.6	93.9	90.7	96.3	87.9	80.5	83.1	83.2	82.6	84.0	85.7

Table 12: Full results on the **TwiERC** temporal task splits from Luu et al. (2022). This task evaluates Twitter Named Entity Classification with Accuracy.

PRETRAIN TIME	FINETUNE TIME	2014	2015	2016	2017	2018	2019	2014	2015	2016	2017	2018	2019
		EVAL TIME											
		LM-XL						LM-SMALL					
2013	2014-2015	98.7	97.5	95.6	89.0	94.0	86.0	74.5	75.3	80.4	74.0	71.9	69.5
	2016-2017	98.2	98.0	95.0	93.1	95.2	90.2	74.3	74.0	77.0	75.4	74.7	70.9
	2018-2019	97.7	98.5	94.4	91.8	94.0	89.9	68.1	70.2	76.2	71.2	75.4	75.0
2016	2014-2015	98.5	98.4	95.6	92.0	94.3	86.3	74.9	75.9	81.7	74.4	71.0	70.7
	2016-2017	98.0	98.1	95.4	94.0	95.1	89.7	74.1	72.9	78.9	74.0	74.1	70.0
	2018-2019	97.6	98.2	94.6	93.4	95.8	89.4	69.5	70.3	76.7	72.1	75.3	75.3
2019	2014-2015	98.2	98.5	95.0	93.6	94.8	88.0	74.9	75.9	79.4	76.8	70.3	69.7
	2016-2017	97.9	98.8	94.0	94.0	96.4	91.4	73.9	74.5	78.4	75.0	74.9	69.7
	2018-2019	97.3	98.9	92.7	92.5	96.7	92.0	67.8	69.8	77.5	73.9	75.4	76.2
2022	2014-2015	98.2	97.4	95.3	90.6	94.2	87.3	72.8	78.6	78.3	72.6	70.7	69.5
	2016-2017	97.5	98.9	94.7	91.7	95.8	90.9	71.9	73.4	77.6	74.4	72.6	69.0
	2018-2019	97.0	98.8	93.1	91.9	95.1	88.7	66.8	71.6	74.6	73.9	74.7	72.7

Table 13: Full results on the **AIC** temporal task splits from (Luu et al., 2022). This task evaluates the classification of science articles from Semantic Scholar into those published at ICML or AAAI, measured with Accuracy.

PRETRAIN TIME	FINETUNE TIME	2012	2013	2014	2015	2016	2017	2018	2019	2020	2021
						EVAL TIME					
					LM-XL						
2013	2009-2013	100.0	100.0	95.5	73.5	65.4	56.5	51.1	70.1	74.2	67.2
	2014-2015	95.1	97.9	100.0	97.4	81.9	65.2	56.7	72.6	73.0	66.4
	2016-2017	88.2	92.8	95.0	87.2	92.3	92.8	80.4	82.6	79.0	69.3
	2018-2019	76.8	86.0	88.4	58.9	66.2	87.0	91.3	94.5	90.2	79.9
	2020-2021	53.6	68.4	77.0	39.2	48.1	72.0	77.9	90.2	94.7	91.9
2016	2009-2013	100.0	100.0	94.9	72.9	67.8	55.8	53.7	70.3	73.7	67.5
	2014-2015	94.9	98.2	100.0	97.3	82.5	68.4	58.7	73.0	73.4	67.9
	2016-2017	85.0	92.6	94.6	87.8	91.8	93.1	80.7	83.0	79.9	69.2
	2018-2019	73.1	85.2	87.9	58.3	68.5	88.1	91.3	94.4	90.4	81.5
	2020-2021	49.0	64.3	75.5	38.0	50.8	73.4	78.6	90.5	94.6	93.7
2019	2009-2013	100.0	100.0	95.5	73.3	68.0	57.9	55.2	71.8	74.3	68.4
	2014-2015	93.8	97.4	100.0	97.7	82.5	69.7	59.1	74.6	73.9	67.9
	2016-2017	85.0	92.7	94.6	87.1	92.0	93.1	82.0	83.4	80.4	68.3
	2018-2019	73.8	84.8	87.6	58.4	68.9	86.7	91.9	94.8	90.3	81.4
	2020-2021	48.4	64.2	75.6	35.7	48.6	71.7	78.6	90.7	95.0	93.7
2022	2009-2013	100.0	100.0	94.9	72.6	67.5	55.6	52.3	72.0	76.7	69.0
	2014-2015	94.4	97.9	100.0	97.9	81.0	68.0	56.1	74.3	73.9	68.8
	2016-2017	84.1	91.5	93.2	85.2	90.9	92.2	78.7	80.9	79.7	69.6
	2018-2019	71.1	83.9	86.0	58.9	66.6	85.8	90.3	94.5	91.1	83.0
	2020-2021	47.2	62.4	73.0	39.6	50.8	73.6	77.5	90.6	94.9	93.8
					LM-SMALL						
2013	2009-2013	89.1	87.5	80.2	48.5	42.3	38.9	42.4	57.0	62.9	56.4
	2014-2015	77.8	88.5	89.5	64.7	50.4	46.3	42.0	60.3	63.3	55.7
	2016-2017	40.9	43.4	58.2	36.1	40.0	54.7	47.4	61.2	61.2	54.4
	2018-2019	41.2	39.3	44.0	21.7	23.0	42.3	49.8	63.1	67.2	56.9
	2020-2021	40.8	37.9	42.6	20.5	22.5	37.2	45.4	64.6	71.9	65.6
2016	2009-2013	89.9	89.2	80.5	51.7	45.7	39.9	42.6	57.7	62.6	55.4
	2014-2015	78.2	87.8	87.4	63.9	49.6	45.6	41.8	59.7	61.9	54.3
	2016-2017	51.3	49.3	57.9	37.4	38.1	51.1	46.3	60.2	60.2	53.6
	2018-2019	49.8	43.1	46.5	24.4	26.8	42.6	48.3	62.9	66.3	56.2
	2020-2021	51.7	43.0	42.5	22.7	24.8	36.3	40.8	61.5	70.1	63.3
2019	2009-2013	89.2	87.0	77.9	48.5	39.8	38.7	41.7	57.8	64.6	55.6
	2014-2015	73.3	87.7	87.9	63.8	48.7	42.8	39.5	57.4	61.8	53.8
	2016-2017	34.8	45.7	55.6	36.6	36.2	50.1	44.5	59.8	60.4	53.1
	2018-2019	32.6	36.4	43.6	21.6	21.7	41.2	48.7	62.8	66.6	55.7
	2020-2021	34.8	37.6	43.7	21.3	21.3	36.0	42.4	62.7	70.9	62.0
2022	2009-2013	90.3	88.8	79.0	47.9	41.0	37.6	40.9	57.9	64.7	56.6
	2014-2015	76.9	89.7	90.3	67.2	54.6	45.2	41.0	60.5	63.4	56.5
	2016-2017	41.5	48.8	56.9	37.0	38.6	53.7	47.7	62.0	60.7	53.2
	2018-2019	33.0	34.3	39.2	19.9	20.5	43.2	50.9	65.5	68.8	56.4
	2020-2021	39.5	37.0	38.5	19.4	19.6	33.6	41.8	65.2	72.8	66.1

Table 14: Full results on the **PoliAff** temporal task splits from Luu et al. (2022). This task evaluates classification of political affiliation from tweets, measured in Accuracy.

Filter	% Data	Toxicity Identification (↑)					Toxicity Generation (↓)			
		SBF	Toxigen	DH R3	DH R4	Score	RTP-T	RPT-NT	RepBias	Score
The Pile										
Full Dataset	100.0	90.7	90.8	88.7	84.1	0.0	88.9	44.4	4.6±0.7	0.0
T=0.95	99.1	90.6	90.9	87.8	83.5	-0.5	85.6	43.9	4.6±0.8	-1.9
T=0.9	97.4	90.2	90.8	86.4	83.7	-0.9	80.4	41.9	4.0±0.6	-9.2
T=0.7	90.8	89.9	90.9	87.4	82.7	-1.0	83.3	39.9	2.9±0.5	-18.1
T=0.5	80.7	89.4	90.4	86.0	82.8	-1.6	83.3	35	2.2±0.4	-26.7
T=0.3	60.1	88.4	89.9	85.3	81.3	-2.7	78.5	31.4	2.2±0.5	-31.1
Ngrams	70.7	89.7	90.4	86.3	82.4	-1.6	76.1	33.6	2.5±0.6	-28.0
C4										
Inverse T=0.06	92.2	93.2	91.4	90.0	85.7	1.4	87.8	49.6	4.8±0.8	15.6
Full Dataset	100.0	91.2	91.1	89.0	84.2	0.0	84.6	41.8	3.9±0.7	0.0
T=0.95	97.7	90.7	91.3	87.7	83.4	-0.7	84.3	41.9	3.9±0.7	0.0
T=0.9	94.9	90.4	90.6	87.5	83.9	-0.9	81.1	40.3	3.1±0.6	-9.0
T=0.7	85.8	90.5	90.5	86.1	82.8	-1.6	71.3	34.8	2.4±0.5	-23.8
T=0.5	75.8	89.8	90.5	86.9	81.9	-1.8	65.2	30.0	1.8±0.4	-35.0
T=0.3	60.8	89.4	90.2	82.1	75.6	-5.2	55.0	19.8	1.2±0.3	-52.1
Ngrams	78.6	89.8	90.7	87.0	81.8	-1.8	74.7	31.8	2.3±0.5	-25.6

Table 15: **Toxicity filtering the pretraining dataset decreases the ability of LM-XLto identify toxicity and to generate toxic text.** These results are visualized in Figure 4.

Filter	% Data	Toxicity Identification (↑)					Toxicity Generation (↓)			
		SBF	Toxigen	DH R3	DH R4	Score	RTP-T	RPT-NT	RepBias	Score
C4										
Inverse T=0.5	73.3	91.8	90.1	86.8	82.9	-0.9	86.3	44.3	4.1±0.6	+9.7
Full Dataset	100.0	93.1	91.0	87.4	83.5	0.0	84.1	41.8	3.4±0.6	0.0
T=0.975	90.6	93.1	91.3	87.8	82.7	-0.1	85.4	46.0	3.8±0.7	+7.3
T=0.95	83.9	93.2	91.3	89.4	85.0	+1.1	86.3	44.0	4.2±0.6	+10.4
T=0.9	73.3	93.3	91.2	88.6	85.9	+1.2	85.2	44.8	4.3±0.7	+11.1
T=0.7	45.6	93.3	91.4	89.9	86.6	+1.8	86.5	44.7	4.0±0.8	+9.6

Table 16: **Quality filtering the pretraining dataset decreases the ability of LM-XLto identify toxicity but surprisingly increases toxicity generation.** These results are visualized in Figure 4.

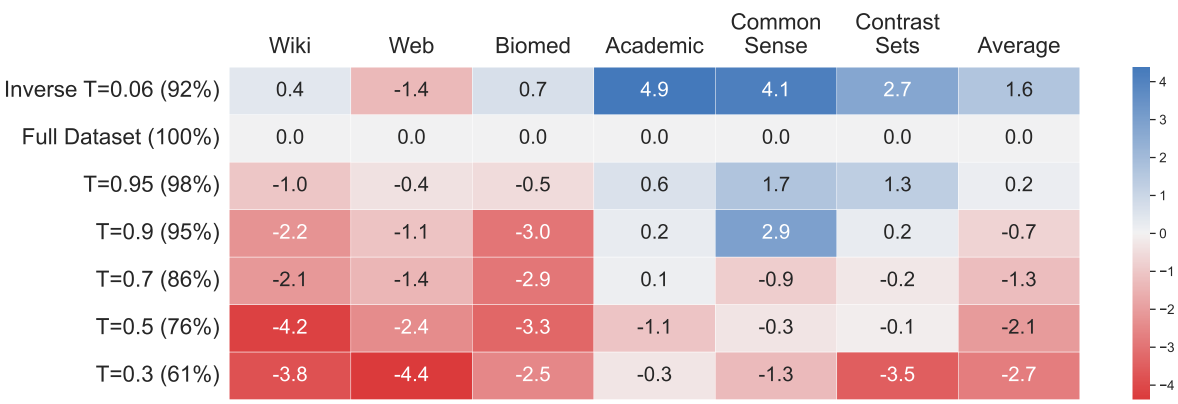

Figure 11: **Toxicity filtering C4 reduces LM-XL's downstream performance on most QA task domains.** The toxicity filter threshold is on the x-axis, with percentage of training data remaining in parentheses. Each column represents a set of QA evaluations from a domain. The 'Full Dataset' is unfiltered, and the 'Inverse' filter removes the lowest toxicity data instead.

Filter	% Data	Toxicity Identification (↑)					Toxic Generation (↓)			
		SBF	Toxigen	DH R3	DH R4	Score	RTP-T	RTP-NT	RepBias	Score
Full Dataset	100.0	90.7	90.8	88.7	84.1	0.0	88.9	45.4	4.6±0.7	0.0
No Social	98.8	90.9	91.0	87.8	84.9	+0.1	85.4	47.2	4.7±0.8	+0.4
No Wiki	97.9	90.6	90.8	88.1	83.6	-0.4	89.0	49.4	4.8±0.6	+4.2
No Books	93.1	89.9	90.3	87.1	82.6	-1.3	87.4	43.5	4.0±0.8	-6.2
No OpenWeb	93.1	89.9	90.3	86.4	82.5	-1.5	88.0	42.1	4.3±0.6	-5.2
No Legal	91.0	90.9	90.8	88.1	83.0	-0.4	88.2	46.1	4.7±0.8	+0.8
No Academic	87.1	90.7	91.0	88.2	84.5	+0.0	86.5	46.4	4.5±0.7	-1.2
No PubMed	85.1	90.6	90.8	88.0	84.3	-0.2	87.6	46.3	4.6±0.7	-0.2
No Code	80.9	91.0	91.2	88.5	84.5	+0.2	87.6	46.5	4.7±0.7	+0.6
No CC	73.1	89.9	90.0	85.3	82.4	-1.9	87.8	46.2	4.3±0.6	-2.1

Table 17: Effect of the Pile's domain composition on toxicity identification and generation. **Removing Books, CommonCrawl and OpenWeb lead to the greatest decrease in toxicity metrics. Removing Wikipedia had a strong increase in toxicity generation.**