Reduced, Reused and Recycled: The Life of a Dataset in Machine Learning Research

Bernard Koch  
University of California, Los Angeles  
bernardkoch@ucla.edu

Emily Denton  
Google Research, New York  
dentone@google.com

Alex Hanna  
Google Research, San Francisco  
alexhanna@google.com

Jacob G. Foster  
University of California, Los Angeles  
foster@soc.ucla.edu

Abstract

Benchmark datasets play a central role in the organization of machine learning research. They coordinate researchers around shared research problems and serve as a measure of progress towards shared goals. Despite the foundational role of benchmarking practices in this field, relatively little attention has been paid to the dynamics of benchmark dataset use and reuse, within or across machine learning subcommunities. In this paper, we dig into these dynamics. We study how dataset usage patterns differ across machine learning subcommunities and across time from 2015-2020. We find increasing concentration on fewer and fewer datasets within task communities, significant adoption of datasets from other tasks, and concentration across the field on datasets that have been introduced by researchers situated within a small number of elite institutions. Our results have implications for scientific evaluation, AI ethics, and equity/access within the field.

1 Introduction

Datasets form the backbone of machine learning research (MLR). They are deeply integrated into work practices of machine learning researchers, serving as resources for training and testing machine learning models. Datasets also play a central role in the organization of MLR as a scientific field. Benchmark datasets provide stable points of comparison and coordinate scientists around shared research problems. Improved performance on benchmarks is considered a key signal for collective progress. Such performance is thus an important form of scientific capital, sought after by individual researchers and used to evaluate and rank their contributions.

Datasets exemplify machine learning tasks, typically through a collection of input and output pairs [1]. When they institutionalize benchmark datasets, task communities implicitly endorse these data as meaningful abstractions of a task or problem domain. The institutionalization of benchmarks influences the behavior of both researchers and end-users [2]. Because advancement on established benchmarks is viewed as an indicator of progress, researchers are encouraged to make design choices that maximize performance on benchmarks, as this increases the legitimacy of their work. Institutionalization signals to industry adopters that models can be expected to perform in the real world as they do on the benchmark datasets. Close alignment of benchmark datasets with “real world” tasks is thus critical to accurate measurement of collective scientific progress and to safe, ethical, and effective deployment of models in the wild.

Given their central role in the social and scientific organization of MLR, benchmark datasets have also become a central object of critical inquiry in recent years [3]. Dataset audits have revealed concerning biases that have direct implications for algorithmic bias and harms [4][5][6][7]. Problematic categorical
schemas have been identified in popular image datasets, including poorly-formulated categories and the inclusion of derogatory and offensive labels [8, 9]. Research into the disciplinary norms of dataset development has revealed troubling practices around dataset development and dissemination, like unstandardized documentation and maintenance practices [10, 11, 12]. There is also growing concern about the limitations of existing datasets and standard metrics for evaluating model behavior in real-world settings and assessing scientific progress in a problem domain [13, 14].

Despite the increase in critical attention to benchmark datasets, surprisingly little attention has been paid to patterns of dataset use and reuse across the field as a whole. In this paper, we dig into these dynamics. We study how dataset usage patterns differ across machine learning subcommunities and across time (from 2015-2020) in the Papers With Code (PWC) corpus.[1][4] More specifically, we study machine learning subcommunities that have formed around different machine learning tasks (e.g., Sentiment Analysis and Facial Recognition) and examine: (i) the extent to which research within task communities is concentrated or distributed across different benchmark datasets; (ii) patterns of dataset creation and adoption between different task communities; and (iii) the institutional origins of the most dominant datasets.

We find increasing concentration on fewer and fewer datasets within most task communities. Consistent with this finding, the majority of papers within most tasks use datasets that were originally created for other tasks, instead of ones explicitly created for their own task—even though most tasks have created more datasets than they have imported. Lastly, we find that these dominant datasets have been introduced by researchers at just a handful of elite institutions.

The remainder of this paper is organized as follows. First, we motivate our research questions by underscoring the critical importance of benchmarks in coordinating machine learning research. Second, we describe our analyses on the PWC corpus, a catalog of datasets and their usage jointly curated by the machine learning community (manually) and by Facebook AI Research (algorithmically). We then present our findings and discuss their implications for scientific validity, the ethical usage of MLR, and inequity within the field. We close by offering recommendations for possible reform efforts for the field.

2 Related Work: Scientific, Social, and Ethical Importance of Datasets

Following Schlangen [1], we understand machine learning benchmarks as community resources against which models are evaluated and compared. Benchmarks typically formalize a particular task through a dataset and an associated quantitative metric of evaluation. The practice was originally introduced to MLR after the “AI Winter” of the 1980s by government funders, who sought to more accurately assess the value received on grants [15, 16]. Today, benchmarking is the dominant paradigm for scientific evaluation in MLR, and the field collectively views upward trends on benchmarks as noisy but meaningful indicators of scientific progress [1, 2, 17]. Over time, MLR has evolved strong norms to facilitate widespread benchmarking, including the development of open-access datasets, formal competitions and challenges, and accompanying “black-box” software that allows researchers to test their algorithms on benchmark datasets with minimal effort.

The establishment of benchmark datasets as shared evaluative resources across the MLR community has unique advantages for coordinating scientists around common goals. First, barriers to participation in MLR are reduced, since well-resourced institutions can shoulder the costs of dataset curation and annotation.[2] Second, by reducing otherwise complex comparisons to a single agreed-upon measure, the scientific community can easily align on the value of research contributions and assess whether progress is being made on a particular task [19, 20]. Finally, a complete commitment to benchmarking has allowed MLR to relax reliance on slower institutions for evaluating progress like peer-review, qualitative or heuristic evaluation, or theoretical integration. Together, these advantages have contributed to MLR’s unprecedented transformation into a “rapid discovery science” in the past decade [21].

While there are clear advantages to benchmarking as a methodology for comparing algorithms and measuring progress, there are growing concerns about benchmarking cultures in MLR that tend
to valorize state-of-the-art (SOTA) results on established benchmark datasets over other forms of quantitative or qualitative analysis. The necessity of SOTA results on well-established benchmarks for publication has been identified as a barrier to the development of new ideas [22]. There have been growing calls for more rigorous and comprehensive empirical analysis of models beyond standard top-line metrics: reporting model size, energy consumption, fairness metrics, and more [23, 24, 25, 26]. The standard benchmarking paradigm also contributes to issues with underspecification in ML pipelines: a given level of performance on a held-out benchmark test set doesn’t guarantee that a model has learned the appropriate causal structure of a problem [14]. In short, while community alignment on benchmarks and metrics can enable rapid algorithmic advancement, excessive focus on single metrics at the expense of more comprehensive forms of rigorous evaluation can lead the community astray and risk the development of models that generalize poorly to the real world.

The MLR community has begun to reflect on the utility of established benchmarks and their suitability for evaluative purposes. For example, the Fashion-MNIST dataset was introduced because the original MNIST dataset came to be perceived as over-utilized and too easy [27]; the utility of ImageNet — one of the most influential ML benchmarks in existence — as a meaningful measure of progress has been a focus of critical examination in the past few years [28, 29]. SOTA-chasing concerns are also compounded by the great capacity ML algorithms have to be “right for the wrong reason” [30], enabling SOTA results that rely on “shortcuts” rather than learning the causal structure dictated by the task [13]. Bender et al. suggest the NLP community may have been “led down the garden path” by over-focusing on “beating” benchmark tasks with models that can easily manipulate linguistic form without any real capacity for language understanding [31]. Recent dataset audits have also revealed that established benchmark datasets tend to reflect very narrow — typically white, male, Western — slices of the world [4, 5, 6, 7, 9]. Thus, over-concentration of research on a small number of datasets and metrics can distort perceptions of progress within the field and have serious ethical implications for communities impacted by deployed models. Despite these discussions, little empirical work has considered whether over-concentration of research on a small number of datasets is a systemic issue across MLR. This prompts our first research question:

**RQ1:** How concentrated are machine learning task communities on specific datasets, and has this changed over time?

There are also growing concerns regarding the gap between benchmark datasets and the problem domains in which they are used to evaluate progress. For example, Scheuerman et al. found that computer vision datasets tend to be developed in a manner that is decontextualized from a particular task or application area [12]. Supposedly “general purpose” benchmarks are often valued within the field, though the precise bounds of what makes a dataset suitable for general evaluative purposes remains unclear [17]. These observations prompt our second research question:

**RQ2:** How frequently do machine learning researchers borrow datasets from other tasks instead of using ones created explicitly for that task?

Despite widespread recognition that datasets are critical to the advancement of the field, careful dataset development is often undervalued and disincentivized, especially relative to algorithmic contributions [12, 32]. Given the high value the MLR community places on SOTA performance on established benchmarks, researchers are also incentivized to reuse recognizable benchmarks to legitimize their contributions. Dataset development is time- and labor-intensive, making large-scale dataset development potentially inaccessible to lower-resourced institutions. These observations prompt our final research question:

**RQ3:** What institutions are responsible for the major ML benchmarks in circulation?

Our paper makes two distinct contributions to the literature. First, it provides a concise, multi-dimensional discussion of the pros and cons of benchmarking as an evaluation paradigm in MLR, drawing on earlier work as well as insights from the sociology of science. Second, and more substantially, it provides the first field-level, quantitative analysis of benchmarking practice in MLR.

### 3 Data

Our primary data source is Papers With Code (PWC), an open source repository for machine learning papers, datasets, and evaluation tables created by researchers at Facebook AI Research. PWC is
largely community-contributed — anyone can add a benchmarking result or a task, provided the benchmarking result is publicly available in a pre-print repository, conference proceeding, or journal. Once tasks and datasets are introduced by humans, PWC scrapes arXiv using keyword searches to find other examples of the task or uses of the dataset.

We downloaded the complete PWC dataset on 06/16/2021 (licensed under CC BY-SA 4.0). In this study, we focus primarily on the “Datasets” archive, as well as papers utilizing those datasets. Each dataset in the archive is associated with metadata such as the modality of the dataset (e.g., texts, images, video, graphs), the date the dataset was introduced, and the paper title that introduced the dataset (if relevant). We found 4,384 datasets on the site and scraped 60,647 papers that PWC associates with those datasets using a PWC internal API (see Figure A2 for a truncated histogram of usage across datasets).

In PWC papers, benchmarks and datasets are associated with tasks (e.g., Object Recognition, Machine Translation). Because we are interested in the dynamics of dataset usage (both within and across task communities), our first two analyses are restricted to dataset usages published in papers annotated with tasks. We call the task for which the dataset was originally designed the “origin task.” We call the task of the paper using the dataset the “destination task.” For example, ImageNet [33] was introduced as a benchmark for Object Recognition and Object Localization (origin tasks), but is now regularly utilized as a benchmark for Image Generation (destination task) among many others.

PWC includes a taxonomy of tasks and subtasks. The graph is cyclic, making it hard to disentangle dataset transfer between broad tasks and finer-grained tasks. For each dataset transfer, we record the transfer between the origin task and the destination task. We also record the transfer between the origin’s parents and the destination’s parents. This approach allows us to accurately capture transfer dynamics between larger tasks (e.g., Image Classification and Image Generation), and between finer-grained tasks (e.g., Image-to-Image Translation and Image Inpainting, which are both children of Image Generation).

We took three additional steps to pre-process the data. First, we only consider datasets that are used by others at least once. Second, because we found dataset usages in PWC to be noisy (i.e., a paper would be associated with a dataset if the corresponding dataset name appeared multiple times in the paper), we dropped dataset usages where the dataset-using paper shared no tasks in common with the dataset itself. Second, we found 640 papers that introduced a dataset but were not associated with a task. Two authors manually annotated the top 90 most widely-used dataset papers with origin tasks (see GitHub for justifications and appendix for details). We dropped the remaining 550 dataset papers (accounting for only 10.2% of total usages).

Datasets for Analysis 1 and 2 (RQ1, RQ2): To minimize double-counting of dataset usages across parent tasks and child subtasks, we chose to focus exclusively on parent tasks in PWC. The outcome measures we use in these analyses (Gini, Adoption Proportion, and Creation Proportion) are biased in small samples, so we used only parent tasks above the median size of 34 papers (see GitHub for the list of tasks). Because these tasks were larger, we also felt that parent tasks tended to be more widely-recognized as coherent task communities. Table 1 presents descriptive statistics for the data used in each analysis. Analysis 1 explores dataset usage within tasks, so it includes datasets that are introduced in papers as well as those that are not (e.g., introduced on a website or competition). Analysis 2 explores the transfer of datasets between origin and destination tasks. This dataset is smaller because we can only determine the origin task for a dataset if it is introduced in a paper (Table 1). In the appendix, we describe robustness checks that remove some of these cleaning steps; these choices minimally affect our results.

Dataset for Analysis 3 (RQ3): To study the distribution of widely-utilized datasets across institutions, we linked all dataset-introducing papers to the Microsoft Academic Graph (MAG) [34]. Analyses were performed on dataset usages for which the dataset’s last author affiliation was annotated in MAG (Table 1). We again imposed the restriction that usages must share a labeled task with the dataset, but again found it had minimal effects on the results (see appendix).

The datasets, a datasheet [10], and code for curation/analysis can be found at https://github.com/kochbj/Reduced_Reused_Reycled.
Table 1: Descriptive statistics for data used in the three analyses. Note that the number of dataset usages is larger than the number of papers because many papers use multiple datasets.

| Analysis | # Datasets | # Usages | # Tasks | # Papers |
|----------|------------|----------|---------|----------|
| 1        | 2,063      | 49,008   | 133     | 26,691   |
| 2        | 960        | 33,034   | 133     | 20,747   |
| 3        | 1,933      | 43,140   | N/A     | 26,535   |

4 Methods and Findings

4.1 Analysis 1 (RQ1): Concentration in Task Communities on Datasets

4.1.1 Methods

To measure how concentrated task communities are on certain datasets (RQ1), we calculated the Gini coefficient of the distribution of observed dataset usages within each task. Gini is a continuous measure of dispersion in frequency distributions. It is frequently used in social science to study inequality [35].

The Gini score varies between 0 and 1, with 0 indicating that the papers within a task use all datasets in equal proportions, and 1 indicating that only a single dataset is used across all dataset-using papers. Gini is calculated as the average absolute difference in the usage of all pairs of datasets used in the task, divided by the average usage of datasets. Formally, if \( x_i \) is the number of usages of dataset \( i \) out of all \( n \) datasets used in the task, then the Gini coefficient of dataset usage is,

\[
G = \frac{1}{n^2} \sum_{i=1}^{n} \sum_{j=1}^{n} |x_i - x_j| = \frac{1}{2n} \sum_{i=1}^{n} \sum_{j=1}^{n} |x_i - x_j| = \frac{1}{n} \sum_{i=1}^{n} \frac{1}{2n} \sum_{j=1}^{n} |x_i - x_j|
\]

Because Gini can be biased in small samples [36], we use the the sample-corrected Gini, \( G_s = \frac{n}{n-1} G \), and excluded tasks (or task-years when disaggregating by time) with fewer than 10 papers.

Regression Model 1: In addition to descriptive statistics, we built a regression model to assess the extent to which observed trends in Gini from year-to-year could be attributable to confounding variables like task size, task age, or other task-specific traits at that time. Our outcome is \( G_s \) in each task year from 2015-2020 (Figure A3 shows PWC coverage is limited for papers published before 2015). Our predictors of interest are:

1. **Year** (since we are interested in trends in concentration over time)
2. **CV, NLP, Method** (three dummy variables indicating whether the task belongs to the Computer Vision, Natural Language Processing, or Methodology categories in PWC).

To absorb additional variation, we also included the following control covariates:

1. **Task size** in number of dataset-using/introducing papers for that task in that year
2. **Task age** (because younger tasks may have higher Gini coefficients)
3. **Random intercepts for each task** (because we have repeated observations over time)

We use beta regression to model Gini because the beta distribution is very flexible, between 0 and 1, and commonly used for this purpose [35]. However, we apply the smoothing transformation in [37] to deal with the occasional task-year where the Gini is 0. We use a model with the following interactions:

\[
\text{Beta}(G_s) = \alpha + \beta_1 \text{Year} + \beta_2 \text{TaskSize} + \beta_3 \text{TaskAge} + \beta_4 \text{CV} + \beta_5 \text{NLP} + \beta_6 \text{Methods} + \beta_7 \text{Year} \times \text{TaskSize} + \beta_8 \text{CV} \times \text{Year} + \beta_9 \text{NLP} \times \text{Year} + \beta_{10} \text{Methods} \times \text{Year}
\]

4To give some indication of the range of Gini, the country with the lowest Gini for income inequality according to the World Bank [linked here] is Slovenia with a Gini of 24.6 (scaled 0 to 100). The country with the highest Gini inequality is South Africa at 63. The U.S. has a Gini of 41.4.

6Notation from Wikipedia which provides an excellent exposition.

6Example “Methods” tasks in PWC include Transfer Learning, Domain Adaptation, and AutoML.
This model was chosen among a set of nested models with two- and three-way interactions because it had the lowest Akaike information criterion (AIC) and Bayesian information criterion (BIC). See the appendix for model selection criteria and Table A1 for fit statistics.

4.1.2 Findings

Controlling for task size, task age, and task-specific effects, Model 1 finds significant evidence for increasing concentration in task communities for the full dataset over time, predicting a marginal increase in Gini of 0.113 from 2015-2020 (Figure 1 top green; Table A2). This trend is also visible in the overall distributions of Gini coefficients over this period (Figure 1 bottom). By 2020, the median Gini coefficient for a task was 0.60. There are no statistically significant differences between Computer Vision and Methodology tasks compared to the full sample (Figure 1 top, Figure A1), but Model 1 suggests that increases in concentration are attenuated for Natural Language Processing task communities (Figure 1 top orange). We note that this is the only result that varies somewhat with our model specification; while the rate of increasing concentration in NLP tasks is always significantly lower than the rest of the dataset, the sign and slope of this change does vary somewhat across models. We discuss this point in the appendix.

4.2 Analysis 2 (RQ2): Changes in Rates of Adoption and Creation of Datasets Over Time

4.2.1 Methods

We created two proportions to better understand patterns of dataset usage and creation within tasks as outcomes:

\[
\text{Adoption Proportion} = \frac{\# \text{ of Papers Using Datasets from Other Tasks}}{\# \text{ of Papers Using Datasets from Other Tasks} + \# \text{ of Papers Using Datasets from this Task}}
\]

\[
\text{Creation Proportion} = \frac{\# \text{ of Datasets Created Within this Task}}{\# \text{ of Datasets Created within this Task} + \# \text{ of Datasets Imported from Other Tasks}}
\]

**Aggregated Descriptive Analyses:** We first computed these proportions for each of the 133 parent tasks aggregated across all years, and subsetted these by the “Computer Vision,” “Natural Language Processing,” and “Methodology” categories.

**Regression Models 2A & 2B:** Because we chose to formulate our outcomes as fractions of discrete events, logistic regression is the most theoretically appropriate model for these data. We used a mixed effects logistic regression to model these outcomes with the same predictors as Model 1.

4.2.2 Findings

The top row of Figure 2 shows a wide variance in adoption proportions in both the full sample and the subcategories. Within the full sample, more than half of task communities use adopted datasets at least 57.8% of the time. However, this number varies dramatically across the three PWC subcategories. In more than half of Computer Vision communities, authors adopt at least 71.9% of their datasets from a different task. The equivalent statistic in Methodology tasks is 74.1%. Conversely, half of Natural Language Processing communities adopt datasets less than 27.4% of the time.

In the bottom row of Figure 2 we see a largely inverted trend. Of all unique datasets used in a task community, 62.5% are created specifically for that task in more than half of tasks. Within Computer Vision and Methods tasks, the median is lower at 53.3% and 52.6%, with similar distributions across tasks. Most strikingly, 76.0% of datasets are created specifically for the task in more than half of NLP communities, with a much tighter variance.

We were unable to recover convincing evidence for trends in adoption or creation proportions either way (Regression Models 2A & 2B) because of a lack of data (results not shown). Disaggregating tasks over time creates a significant number of task-years with no events, and these metrics are undefined in those circumstances.
4.3 Analysis 3 (RQ3): Concentration in Dataset-Introducing Institutions Over Time

4.3.1 Methods

To look at trends in Gini inequality across institutions and datasets over time for the larger set of dataset-using papers, we calculated the Gini coefficient $G_s$ in each year for dataset usages both by dataset and by institution. We regressed this Gini on year, as well as residuals capturing variance in the size of PWC that is not correlated with time (see appendix), using a standard beta regression. We also mapped dataset-introducing institutions using the longitude and latitude coordinates provided for the last author’s institution on Microsoft Academic.

4.3.2 Findings

Overall, we find that widely-used datasets are introduced by only a handful of elite institutions (Figure 3 left). In fact, over 50% of dataset usages in PWC as of June 2021 can be attributed to just twelve institutions. Moreover, this concentration on elite institutions as measured through Gini has increased to over 0.80 in recent years (Figure 3 right red). This trend is also observed in Gini concentration on datasets in PWC more generally (Figure 3 right black).

Figure 3: Increases in concentration of dataset usages on institutions and datasets (non-task specific) over time. Left: Map of dataset usages per institution as of June 2021. Dot size indicates number of usages. Blue dots indicates for-profit institutions and orange dots indicate not-for-profit. Institutions accounting for 50%+ of usages labeled. Right: Gini coefficient for concentration of dataset usages across the whole PWC dataset over time for both institutions and datasets. Ribbons indicate 95% CI; dot size indicates number of usages that year.
In this paper, we find that task communities are heavily concentrated on a limited number of datasets, and that this concentration has been increasing over time (see Figure 1). Moreover, a significant portion of the datasets being used for benchmarking purposes within these communities were originally developed for a different task (see Figure 2). This result is striking given the fact that communities are creating new datasets — in most cases more than the unique number that have been imported from other tasks — but the newly created datasets are being used at lower rates. When examining PWC without disaggregating by task category, we find that there is increasing inequality in dataset usage globally, and that more than 50% of all dataset usages in our sample of 43,140 corresponded to datasets introduced by twelve elite, primarily Western, institutions.

NLP tasks differ somewhat from PWC as a whole: the broader trend of increasing concentration on a few datasets is moderated in NLP communities, new datasets are created at higher rates, and outside datasets are used at lower rates. One possible explanation for these findings is that NLP task communities in our dataset tend to be bigger than other task communities (median size of 76 dataset usages compared to 55). While we find very modest evidence of correlations between task size and adoption or creation proportions overall (Kendall’s $\tau = -0.08$, $p = 0.89$; $\tau = 0.14$, $p = 0.81$ respectively), these correlations are stronger within NLP tasks (Kendall’s $\tau = -0.10$, $p = 0.45$; $\tau = 0.09$, $p = 0.50$ respectively). It is possible that larger NLP communities are more coherent and thus generate and use their own datasets at higher rates than other task communities. Another possibility is that NLP datasets are easier to curate because the data are more accessible, easier to label, or smaller. The resolution of this puzzle is beyond the scope of this paper, but the distinct nature of NLP datasets provides an interesting direction for future work.

For our broader findings, there are valid reasons to expect widespread adoption and concentration on key datasets. First, a certain degree of research focus on a particular benchmark is both necessary and healthy to establish the validity and utility of the benchmark (or in some cases, to contest these properties) and to gain community alignment around the benchmark as a meaningful measure of progress. Second, the curation of large-scale datasets is not just costly in terms of resources, but may require unique or privileged data (e.g., anonymized medical records, self-driving car logs) accessible to only a few elite academic and corporate institutions. Nevertheless, the extent of concentration we observe poses questions relating to the scientific rigor and ecological validity of machine learning research and underscores benchmarking as a potential driver for inequality in the field. In the remainder of this section we discuss our findings in relation to these two broad themes and outline recommendations that can be enacted at an individual and institutional level. We close by discussing limitations of this analysis and outlining directions for future work.

5.1 Scientific Rigor and Ecological Validity of MLR

The heavy concentration of research on a small number of datasets for each task community is a fairly unsurprising result given the value placed on SOTA performance in established benchmark datasets —

![Figure 4: Top datasets used across Image Generation and Face Recognition task communities: (a) Origin task communities of top Image Generation datasets. Only 7.49% of Image Generation papers in PWC evaluate on datasets developed for Image Generation. (b) Names of top Image Generation datasets. Only one of the top datasets, FFHQ [38], was developed for the task. (c) The small number of datasets in usage within the high stakes domain of Face Recognition. Two of the datasets, MegaFace [39] and MS-Celeb-1M [40] (in white), have been recently retracted, the latter due to serious ethical violations [41].](image-url)
a valuation that incentivizes individual researchers to concentrate on maximizing performance gains on well-established benchmarks. However, as discussed in Section 2, over-concentration of research efforts on established benchmark datasets risks distorting measures of progress. Moreover, as the rate of technology transfer has accelerated, benchmarks have been increasingly used by industry practitioners to assess the suitability and robustness of different algorithms for live deployment in production settings. This transition has transformed epistemic concerns about overfitting datasets into ethical ones. For example, critical research on datasets for facial recognition, analysis, and classification has repeatedly highlighted the lack of diversity in standard benchmark datasets used to evaluate progress [4], even as the technologies are applied in law enforcement contexts that adversely affect underrepresented populations [12]. Figure 4c shows the top datasets in usage within the Face Recognition community. Here, we see a significant amount of high stakes research being concentrated on a small number of datasets, many of which contain significant racial and gender biases [4, 43]. An in-depth examination of bias within the top benchmarks datasets in use within different task communities is outside the scope of this work. However, the systemic nature of bias concerns in ML datasets compounds the epistemic concerns associated with highly concentrated research.

Our findings also indicate that datasets regularly transfer between different task communities. On the most extreme end, the majority of the benchmark datasets in circulation for some task communities were created for other tasks. For example, Figure 4 plots the dataset usages of Image Generation papers on PWC broken down by origin task (Figure 4a) and dataset name (Figure 4b). We observe that only one of the datasets heavily used in the Image Generation community was designed specifically for this task. The widespread practice of adopting established datasets to train and evaluate models in new problem domains isn’t inherently a problem. However, this practice does raise potential concerns regarding the extent to which datasets are appropriately aligned with a given problem space. Moreover, given the widespread prevalence of systematic biases in the most prominent ML datasets, adopting existing datasets, rather than investing in careful curation of new datasets, risks further entrenching existing biases.

Our findings on creation and adoption rates are quite nuanced. The extent to which high adoption rates raise significant concerns to ecological validity is yet to be determined. Furthermore, it is worth distinguishing between at least two forms of dataset adoption that seem to be conflated in the PWC data. On the one hand, we observe how datasets that have been developed for one task become adapted in some form for a new task through, for example, the addition of new annotations. On the other hand, we observe some datasets being imported whole cloth from one task community to another. Each of these forms of dataset adoption raises potentially unique concerns regarding the validity of the benchmark in a given context. That said, our results add empirical support to the growing body of scholarship calling for dataset development and use to be rooted in context [3, 12], which is particularly important for application-oriented tasks.

This paper complements and supports the growing calls to include forms of qualitative and quan-
titative evaluations beyond top-line benchmark metrics [23, 24, 25, 26]. Given the observed high concentration of research on a small number of benchmark datasets, we believe diversifying forms of evaluation is especially important to avoid overfitting to existing datasets and misrepresenting progress in the field.

5.2 Social Stratification in MLR

The extent of concentration we observe underscores that benchmarking is also a vehicle for inequality in science. The prima facie scientific validity granted by SOTA benchmarking is generically confounded with the social credibility researchers obtain by showing they can compete on a widely recognized dataset, even if a more context-specific benchmark might be more technically appropriate. We posit that these dynamics creates a “Matthew Effect” (i.e. “the rich get richer and the poor get poorer”) where successful benchmarks, and the elite institutions that introduce them, gain outsized stature within the field [44].

Insofar as benchmarks shape the types of questions that get asked and the algorithms that get produced, current benchmarking practices offer a mechanism through which a small number of elite corporate, government, and academic institutions shape the research agenda and values of the field (Figure 3 left). Empirical support for this claim is beyond the scope of this paper, but there is work within the sociology of science and technology showing that government and corporate institutions tend to support research that serves (at least in part) their own interests, e.g., [45].
There is nothing \textit{a priori} scientifically invalid about powerful institutions being interested in datasets or research agendas that would benefit them. However, issues arise when the values of these institutions are not aligned with those of other ML stakeholders (i.e., academics, civil society). For example, Dotan and Milli argue that deep learning’s reliance on large datasets has forced MLR to confront decisions about the extent to which it is willing to violate privacy to acquire/curate data \cite{2}. Corporate and government institutions have objectives that may come into conflict with privacy (e.g., surveillance), and their weighting of these priorities is likely to be different from those held by academics or AI’s broader societal stakeholders. Returning to the Facial Recognition example in Figure 4c, four of the eight datasets (33.69\% of total usages) were exclusively funded by corporations, the US military, or the Chinese government (MS-Celeb-1M, CASIA-Webface, IJB-A, VggFace2). MS-Celeb-1M was ultimately withdrawn because of controversy surrounding the value of privacy for different stakeholders \cite{41}.

The recently introduced NeurIPS Dataset and Benchmark Track is a clear example of an intervention that shifts incentive structures within the MLR community by rewarding dataset development and other forms of data work. We believe these sorts of interventions can play a critical role in incentivizing careful dataset development that is meaningfully aligned with problem domains. However, our finding that a small number of well-resourced institutions are responsible for most benchmarks in circulation today has implications for data-oriented interventions in the field. Our research suggests that simply calling for ML researchers to develop more datasets, and shifting incentive structures so that dataset development is valued and rewarded, may not be enough to diversify dataset usage and the perspectives that are ultimately shaping and setting MLR research agendas. In addition to incentivizing dataset development, we advocate for equity-oriented policy interventions that prioritize significant funding for people in less-resourced institutions to create high-quality datasets. This would diversify— from a social and cultural perspective— the benchmark datasets being used to evaluate modern ML methods.

5.3 Limitations and Future Work

Because our findings rely on a unique community-curated resource, our results are contingent on the structure and coverage of PWC. Sensitivity analyses suggest that while PWC’s coverage of ML publications is not perfect and exhibits some recency bias, the omitted papers tend to be low impact. Moreover, the crowdsourced taxonomy of parent-child task relations in PWC may be subjective and/or noisy, especially for small or new tasks.\footnote{To increase our confidence in task annotations, we focused our analyses on larger, higher-level task communities and considered dataset usages invalid if they did not share a task label with the dataset. Lastly, we find that the concentration trends in Regression 1 are largely robust to model specification and our choice of Gini as an outcome. See the appendix for details on design choices and sensitivity analyses.}

Finally, we emphasize that our findings are highly nuanced. We report trends that our analyses revealed, but refrain from imposing normative judgements on many of these trends. For example, the high rates of adoption raise potential concerns and point to an important future area of examination. The mere fact that datasets travel between task communities is not necessarily problematic, and indeed the widespread sharing of datasets has been central to methodological advancements in the field. We hope this work will offer a foundation for future empirical work examining the details of dataset transfer and the context-specific implications of our findings.

6 Conclusion

Benchmark datasets play a powerful role in the social organization of the field of machine learning. In this work, we empirically examine patterns of creation, adoption, and usage within and across MLR task communities. We find that benchmarking practices are heavily concentrated on a small number of datasets for each task community and heavily concentrated on datasets originating from a small number of well-resourced institutions across the field as a whole. We also find that many benchmark datasets flow between multiple task communities and are leveraged to evaluate progress on tasks for which the data was not explicitly designed. We hope this analysis will inform community-wide initiatives to shift patterns of dataset development and use so as to enable more rigorous, ethical, and socially informed research.

\footnote{The full list of parent tasks and parent/child relations is available in the GitHub.}
Acknowledgments and Disclosure of Funding

We thank the reviewers for their helpful comments. The authors have no competing interests to disclose. BK was supported by DDRIG and GRFP grants from the US National Science Foundation. JGF was supported by an Infosys Membership in the School of Social Science at the Institute for Advanced Study.

References

[1] David Schlangen. Targeting the benchmark: On methodology in current natural language processing research. arXiv preprint, 2007.04792, 2020.
[2] Ravit Dotan and Smitha Milli. Value-laden disciplinary shifts in machine learning. In Proceedings of the 2020 Conference on Fairness, Accountability, and Transparency, FAT* ’20, page 294, New York, NY, USA, 2020. Association for Computing Machinery.
[3] Amandalynne Paullada, Inioluwa Deborah Raji, Emily M. Bender, Emily Denton, and Alex Hanna. Data and its (dis)contents: A survey of dataset development and use in machine learning research. NeurIPS Workshop on Machine Learning Retrospectives, Surveys, and Meta-analyses, 2020.
[4] Joy Buolamwini and Timnit Gebru. Gender shades: Intersectional accuracy disparities in commercial gender classification. In Sorelle A. Friedler and Christo Wilson, editors, Proceedings of Machine Learning Research, volume 81, pages 77–91, New York, NY, USA, 23–24 Feb 2018. PMLR.
[5] S. Shankar, Yoni Halpern, Eric Breck, J. Atwood, Jimbo Wilson, and D. Sculley. No classification without representation: Assessing geodiversity issues in open data sets for the developing world. arXiv preprint, 1711.08536, 2017.
[6] Jieyu Zhao, Tianlu Wang, Mark Yatskar, Vicente Ordonez, and Kai-Wei Chang. Gender bias in coreference resolution: Evaluation and debiasing methods. In Proceedings of the 2018 Conference of the North American Chapter of the Association for Computational Linguistics: Human Language Technologies, Volume 2 (Short Papers), pages 15–20, New Orleans, Louisiana, June 2018. Association for Computational Linguistics.
[7] Lucas Dixon, John Li, Jeffrey Sorensen, Nithum Thain, and Lucy Vasserman. Measuring and mitigating unintended bias in text classification. In Proceedings of the 2018 AAAI/ACM Conference on AI, Ethics, and Society, pages 67–73, 2018.
[8] Kate Crawford and Trevor Paglen. Excavating AI: The politics of images in machine learning training sets. https://www.excavating.ai/ 2019.
[9] Abeba Birhane and Vinay Uday Prabhu. Large image datasets: A pyrrhic win for computer vision? In Proceedings of the IEEE/CVF Winter Conference on Applications of Computer Vision, pages 1537–1547, 2021.
[10] Timnit Gebru, Jamie Morgenstern, Briana Vecchione, Jennifer Wortman Vaughan, Hanna Wallach, Hal Daumé III, and Kate Crawford. Datasheets for datasets. arXiv preprint arXiv:1803.09010, 2018.
[11] R. Stuart Geiger, Kevin Yu, Yanlai Yang, Mindy Dai, Jie Qiu, Rebekah Tang, and Jenny Huang. Garbage in, garbage out? do machine learning application papers in social computing report where human-labeled training data comes from? In Proceedings of the 2020 Conference on Fairness, Accountability, and Transparency, FAT* ’20, page 325–336, New York, NY, USA, 2020. Association for Computing Machinery.
[12] Morgan Klaus Scheuerman, Emily Denton, and Alex Hanna. Do datasets have politics? Disciplinary values in computer vision dataset development. In The 24th ACM Conference on Computer-Supported Cooperative Work and Social Computing (CSCW), 2021.
[13] Robert Geirhos, Jörn-Henrik Jacobsen, Claudio Michaelis, Richard Zemel, Wieland Brendel, Matthias Bethge, and Felix A Wichmann. Shortcut learning in deep neural networks. arXiv preprint, 2004.07780, 2020.
Underspecification presents challenges for credibility in modern machine learning.

arXiv preprint, 2011.03395, 2020.

Kenneth Ward Church. Emerging trends: A tribute to Charles Wayne. 
Natural Language Engineering, 24(1):155–160, 2018.

Kenneth Ward Church and Joel Hestness. A survey of 25 years of evaluation. 
Natural Language Engineering, 25(6):753–767, 2019.

Deborah I Raji, Emily M. Bender, Amandalynne Paullada, Emily Denton, and Alex Hanna. 
AI and the everything in the whole wide world benchmark. 
NeurIPS Workshop on Machine Learning Retrospectives, Surveys, and Meta-analyses, 2020.

Dario Amodei, Danny Hernandez, Girish Sastry, Jack Clark, Greg Brockman, and Ilya Sutskever. 
AI and compute. https://openai.com/blog/ai-and-compute/, 2018.

Susan Elliott Sim. A theory of benchmarking with applications to software reverse engineering. 
University of Toronto, 2003.

S.E. Sim, S. Easterbrook, and R.C. Holt. Using benchmarking to advance research: a challenge to software engineering. 
In 25th International Conference on Software Engineering, 2003. Proceedings., pages 74–83, 2003.

Randall Collins. Why the social sciences won’t become high-consensus, rapid-discovery science. 
Sociological Forum, 9(2):155–177, 1994.

Tom Simonite. Google’s AI guru wants computers to think more like brains. 
Wired, 2018.

D. Sculley, Jasper Snoek, Alexander B. Wiltschko, and A. Rahimi. Winner’s curse? On pace, progress, and empirical rigor. 
In The Sixth International Conference on Learning Representations (ICLR), 2018.

Roy Schwartz, Jesse Dodge, Noah A. Smith, and Oren Etzioni. Green AI. 
arXiv preprint, 1907.10597, 2019.

Jesse Dodge, Suchin Gururangan, Dallas Card, Roy Schwartz, and Noah A. Smith. Show your work: Improved reporting of experimental results. 
In Proceedings of the 2019 Conference on Empirical Methods in Natural Language Processing and the 9th International Joint Conference on Natural Language Processing (EMNLP-IJCNLP), pages 2185–2194, Hong Kong, China, November 2019. Association for Computational Linguistics.

Kawin Ethayarajh and Dan Jurafsky. Utility is in the eye of the user: A critique of nlp leaderboards. 
arXiv preprint, 2009.13888, 2020.

Han Xiao, Kashif Rasul, and Roland Vollgraf. Fashion-MNIST: a novel image dataset for benchmarking machine learning algorithms. 
arXiv preprint, 1708.07747, 2017.

L. Beyer, Olivier J. H’enaff, Alexander Kolesnikov, Xiaohua Zhai, and Aäron van den Oord. Are we done with ImageNet? 
arXiv preprint, 2006.07159, 2020.

Dimitris Tsigras, Shibani Santurkar, Logan Engstrom, Andrew Ilyas, and Aleksander Madry. From ImageNet to image classification: Contextualizing progress on benchmarks. 
International Conference on Machine Learning (ICML), 2020.

Benjamin Heinzerling. NLP’s Clever Hans moment has arrived. The Gradient, 2019.

Emily M. Bender, Timnit Gebru, Angelina McMillan-Major, and Shmargaret Shmitchell. On the dangers of stochastic parrots: Can language models be too big? . 
In Proceedings of FAccT 2021, 2021.

Nithya Sambasivan, Shivani Kapania, Hannah Highhill, Diana Akrong, Praveen Kumar Paritosh, and Lora Mois Aroyo. "Everyone wants to do the model work, not the data work": Data cascades in high-stakes AI". In Proceedings of SIGCHI 2021, 2021.
[33] J. Deng, W. Dong, R. Socher, L.-J. Li, K. Li, and L. Fei-Fei. ImageNet: A Large-Scale Hierarchical Image Database. In Proceedings of the IEEE/CVF Conference on Computer Vision and Pattern Recognition (CVPR), 2009.

[34] Kuansan Wang, Zhihong Shen, Chiyuan Huang, Chieh-Han Wu, Yuxiao Dong, and Anshul Kanakia. Microsoft academic graph: When experts are not enough. Quantitative Science Studies, 1(1):396–413, 2020.

[35] James B. McDonald and Michael Ransom. The Generalized Beta Distribution as a Model for the Distribution of Income: Estimation of Related Measures of Inequality, pages 147–166. Springer New York, New York, NY, 2008.

[36] George Deltas. The small-sample bias of the gini coefficient: results and implications for empirical research. Review of economics and statistics, 85(1):226–234, 2003.

[37] Michael Smithson and Jay Verkuilen. A better lemon squeezer? maximum-likelihood regression with beta-distributed dependent variables. Psychological methods, 11(1):54, 2006.

[38] Tero Karras, Samuli Laine, and Timo Aila. A style-based generator architecture for generative adversarial networks. In 2019 IEEE/CVF Conference on Computer Vision and Pattern Recognition (CVPR), pages 4396–4405, 2019.

[39] Ira Kemelmacher-Shlizerman, Steven M Seitz, Daniel Miller, and Evan Brossard. The megaf ace benchmark: 1 million faces for recognition at scale. In Proceedings of the IEEE Conference on Computer Vision and Pattern Recognition, pages 4873–4882, 2016.

[40] Yandong Guo, Lei Zhang, Yuxiao Hu, Xiaodong He, and Jianfeng Gao. Ms-celeb-1m: A dataset and benchmark for large-scale face recognition. In Proceedings of the 14th European Conference on Computer Vision (ECCV), volume 9907, pages 87–102, 2016.

[41] Adam Harvey and Jules LaPlace. Exposing AI. https://exposing.ai 2021.

[42] Clare Garvie, Alvaro Bedoya, and Jonathan Frankle. The perpetual line-up: Unregulated police face recognition in America. Technical report, Georgetown Law, Center on Privacy & Technology, 2016.

[43] Mei Wang, Weihong Deng, Jiani Hu, Xunqiang Tao, and Yaohai Huang. Racial faces in the wild: Reducing racial bias by information maximization adaptation network. In 2019 IEEE/CVF International Conference on Computer Vision (ICCV), pages 692–702, 2019.

[44] R.K. Merton and N.W. Storer. The Sociology of Science: Theoretical and Empirical Investigations. Phoenix books. University of Chicago Press, 1973.

[45] Naomi Oreskes. Science on a Mission: How Military Funding Shaped What We Do and Don’t Know about the Ocean. University of Chicago Press, 2021.

[46] Evelyn C Pielou. The measurement of diversity in different types of biological collections. Journal of theoretical biology, 13:131–144, 1966.
Reduced, Reused and Recycled: The Life of a Dataset in Machine Learning Research

A     Appendix

A.1     Sensitivity Analyses

We conducted three types of sensitivity analyses to gauge the extent of biases stemming from the Papers with Code corpus, our own curatorial decisions, and model selection/robustness. We describe each of these analyses below, as well as potential limitations still unaddressed.

Coverage biases in PWC

The validity of our findings is contingent on PWC representing the field of MLR more broadly. To estimate the extent of coverage bias and recency bias in PWC, we searched PWC for all papers published in ten top ML conferences (NeurIPS, ICML, ICLR, ACL, AAAI, AISTATS, KDD, CVPR, SIGIR, IJCAI) between 2015 and 2020 (46,774 papers) according to Microsoft Academic. We found that 58.9% of these papers appeared in PWC. However these 58.9% of papers accounted for 89.3% of collective citations received by the 46,774 papers, suggesting that the missing papers are primarily lower impact papers. When we disaggregated this analysis by year, we find that coverage ranges from 38.9% of 2015 papers to 68.8% of 2020 papers, but the proportion of citations covered in each year from 2015 on never drops below 86%. While these numbers suggest that omitted papers may be less cited and thus unlikely to propose widely used datasets, we note that they do not address possible under-annotation of dataset creations or usages within included PWC papers.

Robustness to Cleaning Decisions

Focus on larger, higher-level tasks

Another set of possible biases has to do with subjective, unreliable, and/or spurious task annotations in PWC. To our understanding, the task ontology in PWC is purely crowd-sourced. We chose to focus on higher-level tasks in PWC that were parents to others because we wanted to minimize the double counting of dataset transfers, and were concerned that some of the smaller tasks might represent idiosyncratic labels by individuals rather than stable problem communities. We also chose to focus on the top 50% of parent communities with the most dataset usages (133 task communities with more than 34 usages, the median number of usages). This choice allowed us to reduce sample-size-related issues in our regression analyses and further sharpened our focus on the largest, most popular tasks. Below we describe how these choices affect Analyses 1 and 2 (Analysis 3 is task-independent).

For Analysis 1, when we focus on all 269 parent tasks instead of the 133 largest, there is still a significant increase in Gini over time for all but the smallest tasks. However, the interaction with NLP becomes statistically insignificant. When we do not disaggregate by time, the median Gini for the whole dataset drops from 0.72 to 0.65 (still quite large), with the smaller task communities tending to have lower Gini coefficients. If we include both children and parent tasks, this creates a different dataset by adding 1,164 additional tasks with a median task size of 7 aggregated across all years, and 3 when disaggregated across years. The significant increase in concentration over time is preserved, as is the significant interaction with NLP. Median Gini drops to 0.48 in this dataset. Nevertheless, we discourage interpretation of these findings because Gini is known to be biased in small samples.

For Analysis 2, when we focus on all 269 parent tasks instead of the 133 largest, the median creation proportion is stable at 62.5%. The median adoption proportion increases from 57.7% to 69.7%. Again we see similar patterns for the NLP community, where the median creation proportion is higher (75%) than the full dataset and the median adoption proportion is lower at 40%. When we include transfers between all 1,025 tasks, the proportions become increasingly biased as the median task size dips to 7, with a median creation proportion of 33% and a median adoption proportion of 100% (because community sizes are so small).

Cleaning of spurious annotations

Our analyses on the cross-task transfer of datasets required that the dataset-introducing paper be labeled with origin tasks and the dataset-using papers be labeled with destination tasks. However,
we found that some of the most widely-used datasets in PWC had no task annotations (e.g., MNIST, CIFAR-10, CelebA, ImagenNet). To include these datasets in our analyses, two authors went through each dataset-introducing paper and extracted evidence for specific PWC task labels. These annotations and justifications can be found in the GitHub. Starting with the raw data, we scraped 92,874 dataset usages from 46,697 dataset-using PWC papers labeled with tasks. Only 49,589 (53%) of those were to datasets already labeled with tasks in PWC. We first labeled the 45 highest-used datasets with their original tasks, skipping datasets that did not seem designed for MLR or where origin tasks were unidentifiable from language used in the paper or website. By manually labeling the 90 largest datasets with tasks, we recovered 33,739 usages, leaving just 10.2% of total dataset usages unlabeled with tasks across 550 datasets. It was too time-intensive for us to label these last 550 datasets. However, our results do not change when we include only 45 manually annotated datasets versus 90.

We can also relax the requirement that both the dataset and the focal dataset-using paper be labeled with an overlapping task in Analysis 3. For this analysis, this relaxation allows us to consider 78,289 usages (primarily algorithmically-labeled) of 2,174 datasets in 46,842 papers. Including these potentially noisy usages does not affect our findings: Gini still increases from 0.31 to 0.86 from 2011-2020 across datasets and from 0.38 to 0.80 across institutions. The number of institutions accounting for 50%+ of usages shrinks to 9 (from most usages to least): Princeton, Stanford, Microsoft, AT&T, Max Planck, CUHK, Google, NYU, Toyota Technical Institute at Chicago.

Robustness to Model Design Choices

Using Entropy-based metric instead of Gini

While we ultimately chose to use Gini inequality as the outcome for Model 1, there are alternative metrics for the evenness of categorical distributions. In addition to Gini, we performed all concentration analyses using the information-theoretic Pielou evenness \( J(y) \). Pielou evenness normalizes the observed Shannon entropy \( H(y) \) of dataset usages in each task-year by the maximum possible entropy in each task-year (i.e., the scenario where all datasets are used equally):

\[
J(y) = \frac{H(y)}{H_{\text{max}}(y)} = \frac{-\sum_{i=1}^{D} \frac{1}{D} \ln \frac{1}{D}}{\ln D}
\]

where \( D \) is the number of datasets used at least once in the task-year.

Like Gini, Pielou evenness is between 0 and 1, but interpretation runs in the opposite direction to Gini: higher numbers indicate high evenness (close to maximum entropy/maximum evenness), and low numbers indicate high concentration.

At 0.69, the median Pielou evenness across parent tasks is fairly high. However it behaves identically to Gini in regression analyses; the same model specification is selected, the same parameters are significant (Table A3), and the same trends are observed (Fig. A4).

Model Selection for Regression 1

The last possible source of bias that we consider in our findings comes from the specification of our models. We started with a fully restricted model presented below:

\[
\text{Beta}(G_s) = \alpha + \beta_1 \text{Year} + \beta_2 \text{TaskSize} + \beta_3 \text{TaskAge}
\]

\[
\beta_4 \text{CV} + \beta_5 \text{NLP} + \beta_6 \text{Methods} + \beta_7 \text{Year} * \text{TaskSize} + \beta_8 \text{Year} * \text{TaskAge}
\]

\[
\beta_9 \text{CV} * \text{Year} + \beta_{10} \text{NLP} * \text{Year} + \beta_{11} \text{Methods} * \text{Year} +
\]

\[
\beta_{12} \text{Year} * \text{TaskAge} * \text{TaskSize}
\]

We then compared various simpler models where we dropped three-way interactions, two-way interactions, and random intercepts to this fully restricted model using the Akaike information criterion (AIC) and Bayesian information criterion (BIC). Model fit metrics are provided in Table A1. Exponentiated coefficients for the best-performing model (presented in the main text) under both AIC and BIC are presented in Table A2. In general we found the coefficients are largely stable across the various models.

Model Specification for Regression in Analysis 3

Because we were concerned that the growth in PWC over time might have been a confounder for changes in Gini across institutions or datasets, we included residuals for the size of PWC after it was exponentially regressed on time as a covariate. Ultimately this coefficient was insignificant and did not affect our results.
### A.2 Supplemental Tables

Table A1: Fit statistics for Regression 1. AIC is the Akaike criterion. BIC is the Bayesian Information Criterion. ICC is the intraclass correlation coefficient for the different task communities.

| Model                                  | AIC      | BIC      | ICC  |
|-----------------------------------------|----------|----------|------|
| Fully Restricted                        | -725.050 | -656.208 | 1.027|
| No 3-Way Interaction                    | -724.400 | -664.163 | 1.026|
| No Year*Task Age                        | **-725.108** | **-669.174** | 1.027|
| No Year*Task Size                       | -696.567 | -640.633 | 1.025|
| No Year*Task Age, Year*Task Size, or Random Intercepts | -478.350 | -431.021 | N/A  |

Table A2: Exponentiated coefficients for fixed effects in Regression Model 1. Colons indicate interactions. Bolding highlight coefficients with $p < 0.05$.

| Term          | Exponentiated $\beta$ | Std Error | Z-Value | P-value |
|---------------|------------------------|-----------|---------|---------|
| Intercept     | 1.21                   | 1.22      | 0.95    | 0.34    |
| Year          | 1.10                   | 1.04      | 2.31    | **0.02**|
| Task Size     | 2.39                   | 1.15      | 6.14    | **0.00**|
| Task Age      | 0.94                   | 1.06      | -1.06   | 0.29    |
| CV            | 0.87                   | 1.21      | -0.72   | 0.47    |
| NLP           | 1.02                   | 1.23      | 0.10    | 0.92    |
| Methodology   | 0.74                   | 1.21      | -1.6    | 0.11    |
| Year:Task Size| 0.86                   | 1.03      | -5.59   | **0.00**|
| Year:CV       | 0.98                   | 1.04      | -0.63   | 0.53    |
| Year:NLP      | 0.92                   | 1.04      | -2.17   | **0.03**|
| Year:Methodology | 1.04              | 1.04      | 0.99    | 0.32    |

SD(Task Random Intercepts) 1.71

Table A3: Exponentiated coefficients for fixed effects in Regression Model 1 using Pielou evenness instead of Gini. Colons indicate interactions. Bolding highlight coefficients with $p < 0.05$.

| Term          | Exponentiated $\beta$ | Std Error | Z-Value | P-value |
|---------------|------------------------|-----------|---------|---------|
| Intercept     | 3.53                   | 1.26      | 250     | **0.00**|
| Year          | 0.89                   | 1.04      | 0.01    | **0.01**|
| Task Size     | .523                   | 1.14      | 0.00    | **0.00**|
| Task Age      | 1.06                   | 1.07      | 2.2     | 0.43    |
| CV            | 1.31                   | 1.25      | 3.35    | 0.23    |
| NLP           | 1.04                   | 1.28      | 1.19    | 0.85    |
| Methodology   | 1.57                   | 7.04      | -1.6    | 0.05    |
| Year:Task Size| 1.13                   | 107       | -5.59   | **0.00**|
| Year:CV       | 1.03                   | 1.04      | 2.21    | 0.42    |
| Year:NLP      | 1.12                   | 1.04      | 14.2    | **0.01**|
| Year:Methodology | 0.93              | 1.04      | .19     | 0.09    |

SD(Task Random Intercepts) 2.00
A.3 Supplemental Figures

Figure A1: Increases in concentration on datasets within task communities over time. Higher Gini coefficient indicates greater concentration on fewer datasets. We observe significant spread of Gini across different task communities, with the median trending upwards over time for all modalities. Green is the full dataset, other colors indicate subsets of the data by PWC task category.

Figure A2: Truncated distribution of usages per dataset in PWC. Usages measured conservatively by only allowing usages from tasks the dataset was labeled for. 3760 datasets with less than 5 papers and 8 dataset with over 500 uses dropped for clarity. These 8 dropped datasets are: Penn Treebank, CelebA, SQuAD, KITTI, MNIST, Cityscapes, ImageNet, COCO.
Figure A3: **Number of Papers in the Papers with Code Corpus.** Full set of "Papers with Abstracts" on Papers with Code as of June 2021. Total dataset size is 137,510 papers. Daily snapshots of this dataset are available at github.com/paperswithcode.

Figure A4: **Predicted Pielou evenness on datasets across task communities over time.** Pielou evenness predicted with same specification as Model 1 holding task size/age to means. Green plots show the estimated effects of the full dataset, other colors are fixed effects for categories. 95% confidence intervals shown.