Picking on the Same Person: Does Algorithmic Monoculture lead to Outcome Homogenization?

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Abstract

As the scope of machine learning broadens, we observe a recurring theme of *algorithmic monoculture*: the same systems, or systems that share components (e.g., datasets, models), are deployed by multiple decision-makers. While sharing offers advantages like amortizing effort, it also has risks. We introduce and formalize one such risk, *outcome homogenization*: the extent to which particular individuals or groups experience the same outcomes across different deployments. If the same individuals or groups exclusively experience undesirable outcomes, this may institutionalize systemic exclusion and reinscribe social hierarchy. We relate algorithmic monoculture and outcome homogenization by proposing the *component sharing hypothesis*: if algorithmic systems are increasingly built on the same data or models, then they will increasingly homogenize outcomes. We test this hypothesis on algorithmic fairness benchmarks based on the US Census, demonstrating that increased data-sharing exacerbates homogenization, especially for small datasets. Further, given the current regime in AI of foundation models, i.e., pretrained models that can be adapted to myriad downstream tasks, we test whether model-sharing homogenizes outcomes across tasks. We observe mixed results: we find that for both vision and language settings, the specific methods for adapting a foundation model significantly influence the degree of outcome homogenization. We also identify societal challenges that inhibit the measurement, diagnosis, and rectification of outcome homogenization in deployed machine learning systems.

1 Introduction

Machine learning is built on strong traditions of sharing: we share datasets (e.g., ImageNet), models (e.g., BERT), libraries (e.g., PyTorch), optimizers (e.g., Adam), evaluations (e.g., SuperGLUE) and much more. This ethos of sharing serves the field well: we are able to repeatedly capitalize on the effort required to build high-quality assets (e.g., ImageNet has supported thousands of researchers in computer vision), and improvements to these assets have sweeping benefits (e.g., BERT raised all boats in NLP). Yet does sharing also have risks? Could this central tenet of the field lead to undesirable outcomes?

We observe that certain forms of sharing can be reinterpreted as monoculture: Kleinberg and Raghavan [2021] define *algorithmic monoculture* as the state "in which many decision-makers all rely on the same algorithm." In parts of society where algorithmic systems are ubiquitous, we see trends towards such monoculture [Moore and Tambini, 2018, Engler, 2021]. Monocultures often pose serious risks: Kleinberg and Raghavan [2021] show monoculture is suboptimal for decision-makers when their decisions are interconnected, as when they compete to hire job candidates. Should we think of our practices of sharing assets in ML as monoculture and, if so, what harms should we worry about?

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We investigate this question by proposing one potential risk we call *outcome homogenization*, i.e., the phenomenon of individuals (or groups) exclusively receiving negative outcomes from *all* ML models they interact with. We view outcome homogenization as an important class of *systemic* harms that arise when we study social *systems*, i.e., harms that require observing how individuals are treated by many decision-makers. In §2, we conceptually motivate outcome homogenization in the context of algorithmic hiring. In §3, we introduce the first mathematical formalism for outcome homogenization: we measure homogenization as the observed probability of systemic failure normalized by the base rate.

To link the practice of sharing in ML with the proposed harm of homogenization, we pose and test the *component sharing hypothesis*: algorithmic systems built using the same underlying components, such as training data and machine learning models, will systematically exclude the same individuals or groups. We see component sharing as a specific form of algorithmic monoculture, which generalizes the definition in Kleinberg and Raghavan [2021] from decision-makers deploying the *same* system to deploying *similar* systems in terms of how they are constructed. We investigate how two types of shared components — training data and foundation models — contribute to homogeneous outcomes.

In §4, we demonstrate that data-sharing reliably homogenizes outcomes for individuals and for racial groups, especially for small training datasets involving US census data. In §5, we discuss how the rise of foundation models [Bommasani et al., 2021], i.e., pretrained models that can be adapted to myriad downstream tasks, could yield unprecedented homogenization. Based on experiments with foundation models for vision (CLIP) and language (RoBERTa), to our surprise, we find the use of foundation models does not always exacerbate outcome homogenization. Instead, we find the specific mechanism for adapting the foundation model to the downstream task significantly influences homogenization: for example, linear probing consistently leads to more homogeneous outcomes than finetuning for both modalities. Through these experiments, it is clear that the relationship between sharing and homogenization is not fully explained by our hypothesis, but that there is some evidence that sharing homogenizes outcomes. To advance the study of homogenization in practice, where systemic harms are most consequential, we conclude by identifying key challenges for diagnosing, measuring, and rectifying homogenization in society (§6).

2 Outcome Homogenization in Resume Screening

To illustrate outcome homogenization and its potential causes, we will use the example of algorithmic resume screening. Companies use resumes to screen job applicants, choosing which candidates to interview and which to reject. Maximum homogenization occurs when every company makes the same decision about each candidate, such that each lucky candidate is interviewed by all companies and each unlucky candidate by no companies. We say that the unlucky candidates who receive no interviews experience a *systemic failure*.

What factors might homogenize outcomes? Historically, hiring managers at each company decided who to interview and often agreeed in their decisions. This agreement can be attributed to multiple sources: first, if the needs of each company were identical, then managers at different companies may be incentivized to interview the same candidates, thereby homogenizing outcomes. Second, if hiring managers' choices are influenced by the same social biases, they will mistakenly reject the same people, thereby homogenizing their errors. Bias in resume screening is well-documented and remains significant [Jowell and Prescott-Clarke, 1970, Bertrand and Mullainathan, 2004, Kline et al., 2021, *inter alia*].

However, neither explanation implies that systemic failures are inevitable. Since companies have different needs and resumes are imperfect predictors of success in role, the "best" candidates will likely differ across companies. Further, bias is not uniform across companies: Kline et al. [2021] find that 21% of firms were responsible for 46% of the racial contact gap. Even if decisions are influenced by the same group-level biases, different companies may choose different individual members of the advantaged and disadvantaged groups. Variance in company needs, in prevalence of bias, and in individual hiring manager preferences all make it more likely that different resumes survive the screening stage at different companies, ensuring some diversity in resume screening outcomes.

How do these dynamics change with the introduction of automated decision-making? Most large companies now use automated resume screening software to parse resumes and decide which applicants advance. As a stylized example, if every company deploys the *same* deterministic system and has the same hiring criteria, then outcomes will be necessarily homogeneous: individuals will either receive interviews at every company or be rejected by all of them (i.e., systemic failure). This

example is not far from reality: a few major vendors dominate the marketplace for algorithmic resume screening with 700 companies, including over 30% of Fortune 100 companies, relying on Hirevue [Hirevue, 2021]. This practice of different companies deploying the same system is defined as *algorithmic monoculture* by Kleinberg and Raghavan [2021].

More generally, different companies may instead deploy *similar*, but non-identical, systems. We expand the definition of algorithmic monoculture to encapsulate this broader setting, which is also alluded to in Kleinberg and Raghavan [2021]. Engler [2021] describe this as the reality for college enrollment management algorithms, writing "there are a relatively small number (between five and 10) of prominent vendors in the enrollment management algorithm market, . . . their process and analytics are markedly similar. Since their processes seem relatively consistent, the outcomes might be as well — potentially leading to consistently good results for students who match the historical expectations of colleges, and consistently poor results for students who don't".

Component Sharing Hypothesis. In this work, we study systems that are related in how they are constructed, akin to what is described by Engler [2021]. We pose the **component sharing hypothesis** that relates such algorithmic monoculture with outcome homogenization: *If deployed algorithmic systems share components, outcome homogenization will increase (i.e., there will be more systemic failures)*. In this work, we empirically test this hypothesis for two prominent forms of component sharing: (i) the sharing of training data in training all deployed systems (§4) and (ii) the sharing of the same foundation model for building all deployed systems (§5).

3 Formalizing Outcome Homogenization

While prior work [Kleinberg and Raghavan, 2021, Creel and Hellman, 2021, Bommasani et al., 2021] alludes to outcome homogenization, here we provide the first mathematical formalism of outcome homogenization.[1] In line with our running example of resume screening, we formalize outcome homogenization for individuals in terms of *systemic failures* (i.e., every algorithmic system fails for an individual). We then generalize to the group setting, where groups are systemically excluded rather than individuals, with a discussion of how these metrics relate to established fairness, robustness, and accuracy metrics (§3.4).

3.1 Formalizing Outcome Homogenization for Individuals

Notation. Since we define outcome homogenization as a systemic phenomenon, we consider a social *system* $\{h^i\}_{i=1}^k$ where every individual j interacts with the k deployed models $h^1,\ldots,h^k$. Specifically, an individual $j \in [N]$ will submit features x_i^j (e.g., their resume) as input to company $i \in [k]$ and receive an outcome $h^i(x_j^i) = \hat{y}_j^i$ (e.g., an interview). Let D^i be the empirical distribution of inputs x^i for company i.[2]

To define a notion of failure, let $I^i(x_j^i)$ indicate if $\hat{y}_j^i$ is a negative outcome, i.e., individual j experiences a negative outcome from model h^i. The failure rate for model h^i is

$$\texttt{fail}(h^i) \triangleq \mathop{\mathbb{E}}_{x^i \sim D^i} I^i(x^i) = \Pr_{x^i \sim D^i}\big[I^i(x^i) = 1\big]. \tag{1}$$

Experimentally, we consider classification errors as failures (i.e., $I^i(x^i) \triangleq \mathbb{I}\big[h^i(x^i) \neq y^i\big]$), but other negative outcomes (e.g., rejections from hiring or educational opportunities; $I^i(x^i) \triangleq \mathbb{I}\big[h^i(x^i) = -1\big]$) also can be studied under our framework.

Systemic failures for individuals. If an individual exclusively experiences failure, we say they experience *systemic failure*. The *observed rate of systemic failure* S is

$$S \triangleq \mathop{\mathbb{E}}_{j}\left[\prod_i I^i(x_j^i)\right] = \Pr_j\big[I^1(x_j^1) = 1 \wedge \cdots \wedge I^k(x_j^k) = 1\big]. \tag{2}$$

[1] The formal model of Kleinberg and Raghavan [2021] is related, but substantially distinct. Concretely, their formalism considers harms experienced by decision-makers, whereas we center decision-subjects.

[2] Note that our framework is general: we permit the deployed models to be for different tasks and for the individual's inputs to not be the same, though in our resume screening example all the models perform the same task and applicants often submit the same resume to different companies.

In other words, it is the fraction of individuals who are failed by every model.

Homogenization metric for individuals. S quantifies homogeneous outcomes but is difficult to compare across real-world systems with different underlying accuracies: S will in general be higher for less accurate systems independent of a *specific* tendency to pick on the same person. While we may sometimes want to combine accuracy and outcome homogenization into an overall measure of utility or social welfare, which S implicitly does, we focus on a *relative* measure of homogenization that disentangles accuracy from homogenization. In particular, we are interested in outcome homogenization even, and perhaps especially, in systems that are highly accurate.

As a result, we measure individual-level outcome homogenization for a social system $\{h^i\}_{i=1}^k$ by normalizing the *observed* rate of systemic failure by the *expected* rate of systemic failure.

$$H^{\textbf{individual}}(h^1,\ldots,h^k) \triangleq \frac{S}{\prod_{i=1}^{k} \texttt{fail}(h^i)} = \frac{\mathop{\mathbb{E}}_{j}\left[\prod_i I^i(x_j^i)\right]}{\prod_i \left[\mathop{\mathbb{E}}_{j} I^i(x_j^i)\right]} \tag{3}$$

This measure can be interpreted as the ratio between (i) the probability that an individual experiences systemic failure and (ii) the probability that randomly sampled outputs for each model are all misclassified. That is, the measure captures how the rate of systemic failure changes when we attend to the structure of individuals.

3.2 Formalizing Outcome Homogenization for Groups

In addition to our individual-level metric, we also define a group-level metric for outcome homogenization. While our individual-level metric individualizes harm, complementing work on group-level harms like bias and inequity, we may also want to identify when broader social groups (e.g., Black women) are systemically excluded. This is especially relevant when we do not have access to individual-level information (e.g., due to privacy concerns; see §6) or when individuals are not shared across data distributions (e.g., hiring in different states).

Notation. For each input x^i, we denote the associated group as $G(x^i) \in \mathcal{G}$. Group identity may correspond with the data producer (e.g., the age of a user querying a search engine) or the data subject (e.g., the race of an individual subject to face recognition). Let D_g^i indicate restricting the data distribution to inputs to group g, i.e., $\forall x^i \in D_g^i, G(x^i) = g$. The *group failure rate* $\texttt{fail}_g(h^i)$ is

$$\texttt{fail}_g(h^i) \triangleq \mathop{\mathbb{E}}_{x^i \sim D_g^i} I^i(x^i). \tag{4}$$

Homogenization metric for groups. To measure group-level outcome homogenization for a social system $\{h^i\}_{i=1}^k$, we modify our individual-level metric to compute a weighted average over groups in place of a simple average over individuals.

$$H_G^{\textbf{group}}(h^1,\ldots,h^k) = \frac{\frac{1}{\sum_g W(g)} \sum_g \left[W(g) \prod_i \texttt{fail}_g(h^i)\right]}{\prod_{i=1}^{k} \texttt{fail}(h^i)} \tag{5}$$

Weight functions. We consider three weight functions $W : \mathcal{G} \to \mathbb{R}$ (full definitions in §A.1):

Average ($H_{\textbf{avg}}$) Each group g receives a weight proportional to its *frequency* across all deployments.

Uniform ($H_{\textbf{unif}}$) Each group g receives equal weight.

Worst ($H_{\textbf{worst}}$) The group with the highest rate of systemic failure receives a weight of 1 and all other groups receive a weight of 0.

We introduce these weight functions to clarify that, much like having both individual-level metrics and group-level metrics, we may want to weight groups differently in different circumstances. For example, weighting by frequency may provide a useful overall measurement of homogenization but obscure systemic exclusion experienced by minority groups or specifically the worst-off group.

3.3 Understanding our metrics

As a ratio of probabilities, our metrics take values in $[0,\infty)$ where 0 indicates no systemic failures, 1 indicates the observed rate matches the expected rate, and values greater than 1 indicate some degree of outcome homogenization. In the individual setting, we assume each individual generates exactly one input per deployment, which may not hold in practice (e.g., people may submit multiple resumes or not apply to every company). We appropriately generalize our individual-level metric to address this in Appendix A. Further, in the group setting, we recover the individual-level metric using the **uniform** weighting (or the **average** weighting) if each individual's inputs are treated as belonging to their own group.

3.4 Relationship with related concepts

Given that we introduce (several) metrics, we want to be cognizant of how they relate to existing metrics for related concepts (e.g., accuracy, fairness, robustness). This speaks to the convergent and divergent validity of our metrics [Campbell and Fiske, 1959, Messick, 1987, Jacobs and Wallach, 2021], i.e., whether they are adequately correlated with metrics of similar constructs and adequately uncorrelated with metrics of dissimilar constructs. Here, we discuss theoretical relationships, whereas in §5.2 we look at the empirical correlations.

Accuracy. We design our metrics to minimize correlation with accuracy to separate homogenization effects explicable by the accuracy of the underlying models from those that go beyond what is predicted by accuracy. While not theoretically guaranteed, we demonstrate in Table 1 that our metrics are consistently uncorrelated, or very weakly correlated, with accuracy.

Fairness and Robustness. Beyond accuracy, outcome homogenization is closely related to fairness and robustness. However, we emphasize that outcome homogenization is fundamentally about correlated outcomes for social *systems*, whereas almost all robustness or fairness metrics are defined for a single model. Recent work [Zhao and Chen, 2019, D'Amour et al., 2020, Wang et al., 2021] has initiated the study of fairness in multi-task learning, however these works focus on favorable overall trade-offs across tasks as opposed to systemic modes of failure. Conversely, our metrics cease to be interesting (e.g., $H^{\textbf{individual}}$ is always 1) in the single-model setting as systemic failures are single-model failures.

At a more fine-grained level, algorithmic fairness metrics [e.g., Dwork et al., 2012, Hardt et al., 2016, Corbett-Davies and Goel, 2018], as well as fairness metrics from other fields like the Gini coefficient, emphasize *discrepancies between individuals/groups*. In contrast, our metrics do not (explicitly) center these differences: we are interested in the observed rate of systemic failures across all deployments (and whether this exceeds the expected rate). Performance differences across individuals/groups are not *sufficient* for outcome homogenization: if the performance disparities for each deployment do not align across deployments, then the observed rate of systemic failure may not exceed the expected rate. For robustness metrics, our metric $H_{\textbf{worst}}$ in the worst-case setting closely resembles the metrics studied in work on worst-group robustness [e.g., Sagawa* et al., 2020]. In particular, when there is only one deployment, our metric recovers the standard worst-group accuracy normalized by the overall accuracy.

3.5 Alternative metrics

In Appendix A, we more extensively discuss desiderata for our metric, alternatives we considered, and how we arrived at the metrics we present in the main paper. With that said, we also note conditions where we may instead favor alternatives, as well as connections to familiar quantities like the covariance, Pearson correlation, and (pointwise) mutual information in the binary setting ($k=2$).

4 Data-Sharing Experiments

Having stated our mathematical formalism and metrics for outcome homogenization, we test if sharing training data leads to outcome homogenization. We choose to look at the US Census, which has been well-studied in work on algorithmic fairness and ensures we have individual-level outcomes.

Data. We work with the **ACS PUMS** data[3] introduced by Ding et al. [2021], which contains US Census survey data recording 286 features (e.g., self-reported race and sex, occupation, average hours worked per week) for 3.6 million individuals. Ding et al. [2021] construct several classification tasks using

[3] Full reproducibility details are provided in §B.1.

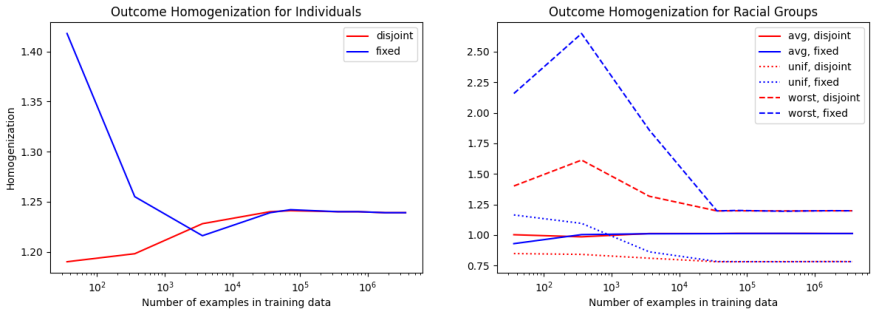

Figure 1: Results for data-sharing experiments showing homogenization (y) as a function of training dataset size (x). Training across tasks on the same data (**fixed**) yields more homogeneous outcomes than on non-identical but identically distributed data (**disjoint**), especially for small datasets.

this data, where each task uses some of an individual's features as inputs and one of their features as the label for the prediction task (e.g., predicting if an individual's income exceeds \$50000). We work with three such tasks: **ACSEmployment** (predict if an individual is employed), **ACSIncomePovertyRatio** (predict an individual's income normalized by the poverty threshold), and **ACSHealthInsurance** (predict if an individual has health insurance).

Individuals and Groups. For each individual, we have their true label for each of the three tasks, enabling us to measure individual-level homogenization. In the group setting, we measure how outcomes homogenize when grouping by the 9 self-identified racial categories (e.g., American Indian, Asian, Black/African American, White, two or more races).

Experimental Design. To test if data-sharing influences outcome homogenization, we formulate a controlled comparison by specifying two settings for the training data: **fixed** and **disjoint**. In the fixed setting, we sample n points without replacement from the entire **ACS PUMS** training dataset and set the training data for every task to this set. In the disjoint setting, we sample $3n$ points without replacement and randomly partition them across the three tasks. In other words, in the **fixed** setting, the three models are trained on data corresponding to exact same individuals, whereas in the **disjoint** setting, no pair of models is trained on data corresponding to the same individual. Having specified the training data, we train logistic regression models for each of three tasks following Ding et al. [2021]. To account for randomness, we report results averaged over 25 trials of the experiment (i.e., 5 samples of training data and 5 training runs per sample for every value of n we consider).

Results and Analysis. In Figure 1, we present our results for this controlled comparison. Across all of our homogenization metrics, we see clear trends (for both individuals and racial groups) that the **fixed** setting yields more homogeneous outcomes than the **disjoint** setting (i.e., blue line is above the corresponding red line). This provides clear evidence for our hypothesis: the use of the same training data leads to greater outcome homogenization than the use of different (but identically distributed) training data. Interestingly, our results also suggest that homogenization can be improved by randomly subsampling a dataset. Given a fixed dataset D, Figure 1 (**left**) suggests that homogenization is better (lower) if each of the three applications randomly subsamples a third of D rather than using the entire dataset D. However, subsampling can reduce accuracy, which poses a tradeoff we discuss in §6. Further, we see that these effects wane as the amount of training data increases (i.e., gap between blue and red lines from left to right). This comes as no surprise: the two distributions converge due to concentration of measure, hence the distinction between the training data in the **fixed** and in the **disjoint** settings is increasingly small for larger amounts of data.

Picking on the same person. Further, we contrast the degree of homogenization in the individual and group settings. (Recall the **average** and **uniform** metrics are the group-level analogues of our individual-level metric.) Outcomes are consistently more homogeneous at the individual-group than for racial groups. This has significant ramifications for many works on algorithmic fairness, which only consider social groups (e.g., race): these works may miss systemic failures for particular individuals that are obscured at the group-level [cf. Kearns et al., 2018, Hashimoto et al., 2018]. Even intersectional approaches may not suffice to surface these systemic failures, unless each intersectional group comprises a single individual.

5 Model-Sharing Experiments

Having found that data-sharing significantly exacerbates outcome homogenization, especially for small datasets, we now turn to model-sharing. Specifically, we test how sharing pretrained models, or *foundation models* [Bommasani et al., 2021], affects outcome homogenization. Bommasani et al. [2021] define foundation models as "models trained on broad data (generally using self-supervision at scale) that can be adapted to a wide range of downstream tasks". These models have had a sweeping impact on the AI research community, most notably in NLP, and are increasingly central to deploying ML at both startups (e.g., Hugging Face, Anthropic, Inflection) and established technology companies (e.g., Google, Microsoft, Meta). Sharing is endemic to foundation models: to justify their immense resource requirements, the models must be used repeatedly for these costs to amortize favorably. In the extreme, if an entire domain like NLP comes to build almost all downstream systems on one or a few foundation models, then any biases or idiosyncrasies of these models that pervasively manifest downstream could potentially yield unprecedented systemic failures and outcome homogenization [Bommasani et al., 2021, Fishman and Hancox-Li, 2022]. We see initial evidence for such algorithmic monoculture: BERT was downloaded 10 million times in the past month alone and GPT-3 enables hundreds of deployed apps.[4] Consequently, we believe it is especially timely to understand if, and to what extent, outcomes get homogenized as these models become entrenched as infrastructure.

5.1 Experiments

Data. To test how foundation models influence homogenization, we run experiments for both vision and language data.[5] On the vision side, we work with the **CelebA** dataset [Liu et al., 2015] of celebrity faces paired with annotations for facial attributes. For each face image, given the associated attributes, we define two tasks (**Earrings**, **Necklace**) that involve predicting whether the individual is wearing the specific apparel item. Attribute prediction in CelebA has been studied previously in work on fairness and robustness [Sagawa* et al., 2020, Khani and Liang, 2021, Wang et al., 2021]. On the language side, we use four standard English text classification datasets following Gururangan et al. [2019]: **IMDB** [Maas et al., 2011], **AGNews** [Zhang et al., 2015], **Yahoo** [Chang et al., 2008], and **HateSpeech18** [de Gibert et al., 2018].

Individuals and Groups. Since the vision tasks are all based on CelebA, we have individual-level information. However, since the language tasks involve entirely different data (e.g., movie reviews vs. news articles), there is no (shared) individual-level information. At the group-level, for vision we use annotations for *hair color* and for whether the individual has a *beard*, whereas for language we automatically group inputs by *binary gender*.

Experimental Design. To test if model-sharing influences outcome homogenization, we contrast setting with differing degrees of model-sharing. In the vision experiments, we produce task-specific models for each task by either (i) training from **scratch** on CelebA data, (ii) linearly **probing** by fitting a linear classifier on frozen CLIP [Radford et al., 2021] features, or (iii) **finetuning** CLIP. To ensure meaningful comparisons, the models trained from scratch shared the same ViT architecture [Dosovitskiy et al., 2021] used in CLIP but with weights initialized randomly.

In the language experiments, we further hone in on the specific *adaptation method* used to adapt the foundation model (specifically RoBERTa-base [Liu et al., 2019]) to each task. We consider (i) linear **probing**, (ii) **finetuning**, and (iii) **BitFiT** [Ben Zaken et al., 2022], which is a recent *lightweight finetuning* method in NLP that involves freezing all the RoBERTa weights except the bias parameters which are updated as in finetuning. Consequently, BitFit is an intermediary between probing and finetuning, which has been shown to achieve similar accuracy as finetuning while updating very few of the pretrained parameters. For both vision and for language, all models are trained for the same number of epochs and we repeat each experiment for 5 random seeds per adaptation method.

Hypotheses. Much like data-sharing, model-sharing is graded and is not binary: different downstream systems can share varying degrees of underlying models. By design, our experimental design suggests a continuum in sharing: first, downstream system either can share a foundation model or not (**scratch**). Second, among methods that involve foundation models, all methods initialize the weights using the pretrained weights but differ in which parameters remain the same *after* adaptation

[4] `https://huggingface.co/bert-base-uncased` as of May 2022.

[5] Full reproducibility details for vision are in §B.2; for language are in §B.3.

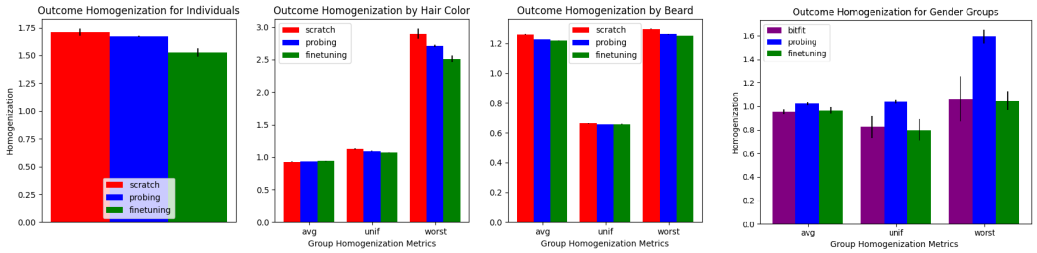

Figure 2: Results for model-sharing experiments showing homogenization as a function of training method for vision (**left three**) and language (**rightmost**).
Vision: **scratch** is the most homogeneous, then **probing**, then **finetuning**.
Language: **probing** is the most homogeneous; **finetuning** and **BitFit** are similarly homogeneous.

is completed: **finetuning** changes all the parameters, **BitFit** only changes the bias parameters, and **probing** changes none of the parameters. As a result, overall, we can rank methods from most to least sharing as (i) **probing**, (ii) **BitFit**, (iii) **finetuning**, (iv) **scratch**, which leads us to predict the degree of homogenization will also follow this ranking under our component-sharing hypothesis.

Results. In Figure 2 (**left**), across all vision settings, we surprisingly find that **scratch** is the most homogeneous, i.e., more homogeneous than either approach involving shared foundation models. This is the opposite of what we hypothesized: we posit that this may indicate model sharing is not the key explanatory variable for outcome homogenization here, but instead it is a more complex form of data sharing. Specifically, we conjecture that since the **scratch** models are only trained on **CelebA** data, whereas the others also are trained on the much larger WebImageText via the CLIP foundation model, this may mean that the models based on CLIP are effectively regularized from learning idiosyncrasies of **CelebA** that the **scratch** models acquire. This may more generally suggest that a more correct hypothesis around data sharing should factor in the relationship (e.g., distribution shift) between the training data and the evaluation data for each model. Additionally, we find **probing** is consistently more homogeneous than **finetuning**, which aligns with our hypothesis. Finally, akin to the census results (§4), we once again find that outcome homogenization is significantly higher for individuals than for groups (comparing to $H_{\textbf{avg}}$ and $H_{\textbf{unif}}$).

In Figure 2 (**right**), across all language settings, we find the ordering of homogenization matches what our hypothesis predicts. Specifically, we find **BitFit** and **finetuning** achieve similar levels of homogeneity, even though **BitFit** updates 0.08% of the parameters full finetuning does (i.e., the number of shared parameters for BitFit is more like probing than finetuning), suggesting the number of shared parameters is not the right lens for understanding model sharing. More broadly, these results do suggest parameter-sharing effects may contribute to outcome homogenization within the foundation model regime, but comparisons between foundation models and no foundation models may be more complex to explain.

5.2 Correlations between Metrics

Since we introduce several metrics, we measure the correlations between our metrics. Further, we measure correlations with accuracy (specifically, the expected rate of systemic failure) to test if homogenization is disentangled from accuracy. Since outcome homogenization is related to fairness, we also measure the correlation between our metrics and a standard group fairness metric. Fairness metrics are generally defined for a single model h, whereas we study entire systems $\{h^i\}_{i=1}^k$. We extend the fairness definition used by Khani et al. [2019] as the variance in the systemic failure rates across groups.

$$\text{Fairness}_G(h^1,\ldots,h^k) \triangleq \operatorname*{Var}_g\left[\prod_i \texttt{fail}_g(h^i)\right] \tag{6}$$

Results. In Table 1, we report the pairwise correlation between metric pairs, based on the models we trained in §5.1. These correlations are for 45 systems (3 methods $\times$ 3 groupings $\times$ 5 random seeds) of 2 models for vision and 15 systems of 4 models for language. For vision, our metrics are highly correlated with each other, whereas for language, $H_{\textbf{unif}}$ patterns quite differently (columns 1-3, 6-8). This is to be expected in that the vision groups (e.g., hair colors) all share similar frequencies, whereas the female group is significantly rarer in the language datasets. For both language and vision, we find that our metrics are generally not correlated, or perhaps weakly correlated, with accuracy as we intended (columns

Table 1: The correlation between our metrics and other metrics. Correlations are (Pearson R^2, Spearman ρ) with * indicating significance at $p = 0.05$ and *italics* indicating $p = 0.001$.

	Vision					Language				
	$H_{\mathbf{avg}}$	$H_{\mathbf{unif}}$	$H_{\mathbf{worst}}$	Accuracy	Fairness	$H_{\mathbf{avg}}$	$H_{\mathbf{unif}}$	$H_{\mathbf{worst}}$	Accuracy	Fairness
$H_{\mathbf{avg}}$	-	(*0.87*, *0.93*)	(0.0, *0.96*)	(0.0, 0.09*)	(0.0, *0.8*)	-	(*0.22*, *-0.47*)	(*0.11*, *0.56*)	(0.06*, -0.22*)	(0.02, 0.09)
$H_{\mathbf{unif}}$	(*0.87*, *0.93*)	-	(0.0, *0.96*)	(0.0, -0.02)	(0.0, *0.74*)	(*0.22*, *-0.47*)	-	(*0.63*, *-0.53*)	(0.0, 0.19*)	(0.0, -0.01)
$H_{\mathbf{worst}}$	(0.0, *0.96*)	(0.0, *0.96*)	-	(*0.05*, 0.1*)	(*1.0*, *0.82*)	(*0.11*, *0.56*)	(*0.63*, *-0.53*)	-	(0.02, 0.13)	(*0.13*, *0.47*)

4, 9). With respect to fairness, our worst-case metric $H_{\mathbf{worst}}$ is strongly correlated for both modalities, but for the other two metrics we see no linear correlations and only monotone correlations for the vision experiments (columns 5, 10). This is in line with our broader expectations that fairness and outcome homogenization are indeed related (especially for the worst-performing group), but that given they are distinct theoretical constructs, they should not always be correlated [Campbell and Fiske, 1959].

6 Societal Considerations and Challenges

To situate our work in a broader social context, we identify and discuss core challenges in **diagnosing**, **measuring**, and **rectifying** outcome homogenization.

Diagnosis. To diagnose homogenization as a byproduct of monoculture requires knowing which companies rely on the same vendor, dataset, or foundation model. How specific algorithmic systems are constructed is often so opaque that determining the responsibility of a shared component for outcome homogenization is nigh impossible. However, if a high outcome homogenization score was measured across deployments, the measurement itself could justify a request for increased transparency and access. We believe this could be a useful practical mechanism for balancing tensions in auditing to provide conditional access to auditors.

Measurement. Measuring homogenization only requires black box access to algorithmic systems, which is often achievable in practice [see Buolamwini and Gebru, 2018, Raji and Buolamwini, 2019, Metaxa et al., 2021]. However, identifying individual-level effects requires observing outcomes for the same individual across deployments. Due to privacy constraints, correlating individuals across datasets from different companies might be challenging or impossible. Such constraints incentivize the study of homogenization for groups as a more accessible concept, as in §3.2.

Rectification. Even once outcome homogenization is identified, organizations deploying systems responsible for homogeneous outcomes may not be incentivized to reduce it. When outcome homogenization does not align aligns with each organization's interests (e.g., maximizing accuracy), it may be necessary to introduce regulation, policy, or other compliance mechanisms to encourage organizations to alter their outcomes. Navigating this trade-off is further complicated if the harms of homogeneous outcomes take time to observe or accrue. More optimistically, Kleinberg and Raghavan [2021] show that in some instances, there may not be a trade-off between policies optimal for each organization and those that diversify outcomes for societal benefit.

7 Limitations and Conclusion

We have introduced, formalized, and measured outcome homogenization as a systemic harm that may arise from practices of sharing in ML. Our work has an important limitation: our measure cannot accommodate every setting and may be brittle given that systemic failures are often rare. Furthermore, direct optimization of our measure may not lead to desirable outcomes, potentially contributing to ethics-washing. The interpretation of our measure must be **contextual**: the implications and severity of homogeneous outcomes must be situated in a broader societal context.

Outcome homogenization is essential to comprehensively characterizing algorithmic harm at scale, especially given the trend towards algorithmic monoculture. Without scrutiny, the harms of homogenization may become insidiously entrenched. Consequently, we believe further study and early intervention is necessary to mitigate and prevent such harms in society.

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Checklist

The checklist follows the references. Please read the checklist guidelines carefully for information on how to answer these questions. For each question, change the default **[TODO]** to [Yes] , [No] , or [N/A] . You are strongly encouraged to include a **justification to your answer**, either by referencing the appropriate section of your paper or providing a brief inline description. For example:

- Did you include the license to the code and datasets? [Yes] See Section **??**.
- Did you include the license to the code and datasets? [No] The code and the data are proprietary.
- Did you include the license to the code and datasets? [N/A]

Please do not modify the questions and only use the provided macros for your answers. Note that the Checklist section does not count towards the page limit. In your paper, please delete this instructions block and only keep the Checklist section heading above along with the questions/answers below.

1. For all authors...

(a) Do the main claims made in the abstract and introduction accurately reflect the paper's contributions and scope? [Yes]

(b) Did you describe the limitations of your work? [Yes]

(c) Did you discuss any potential negative societal impacts of your work? [Yes]

(d) Have you read the ethics review guidelines and ensured that your paper conforms to them? [Yes]

2. If you are including theoretical results...

(a) Did you state the full set of assumptions of all theoretical results? [N/A]

(b) Did you include complete proofs of all theoretical results? [N/A]

3. If you ran experiments...

(a) Did you include the code, data, and instructions needed to reproduce the main experimental results (either in the supplemental material or as a URL)? [Yes]

(b) Did you specify all the training details (e.g., data splits, hyperparameters, how they were chosen)? [Yes]

(c) Did you report error bars (e.g., with respect to the random seed after running experiments multiple times)? [Yes]

(d) Did you include the total amount of compute and the type of resources used (e.g., type of GPUs, internal cluster, or cloud provider)? [Yes]

4. If you are using existing assets (e.g., code, data, models) or curating/releasing new assets...

(a) If your work uses existing assets, did you cite the creators? [Yes]

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(d) Did you discuss whether and how consent was obtained from people whose data you're using/curating? [Yes]

(e) Did you discuss whether the data you are using/curating contains personally identifiable information or offensive content? [Yes]

5. If you used crowdsourcing or conducted research with human subjects...

(a) Did you include the full text of instructions given to participants and screenshots, if applicable? [N/A]

(b) Did you describe any potential participant risks, with links to Institutional Review Board (IRB) approvals, if applicable? [N/A]

(c) Did you include the estimated hourly wage paid to participants and the total amount spent on participant compensation? [N/A]