Language Models Learn Rare Phenomena from Less Rare Phenomena: The Case of the Missing AANNs

Kanishka Misra **Kyle Mahowald**
Department of Linguistics
The University of Texas at Austin
{kmisra,kyle}@utexas.edu

Abstract

Language models learn rare syntactic phenomena, but the extent to which this is attributable to generalization vs. memorization is a major open question. To that end, we iteratively trained transformer language models on systematically manipulated corpora which were human-scale in size, and then evaluated their learning of a rare grammatical phenomenon: the English Article+Adjective+Numeral+Noun (AANN) construction ("a beautiful five days"). We compared how well this construction was learned on the default corpus relative to a counterfactual corpus in which AANN sentences were removed. We found that AANNs were still learned better than systematically perturbed variants of the construction. Using additional counterfactual corpora, we suggest that this learning occurs through generalization from related constructions (e.g., "a few days"). An additional experiment showed that this learning is enhanced when there is more variability in the input. Taken together, our results provide an existence proof that LMs can learn rare grammatical phenomena by generalization from less rare phenomena. Data and code: https://github.com/kanishkamisra/aannalysis.

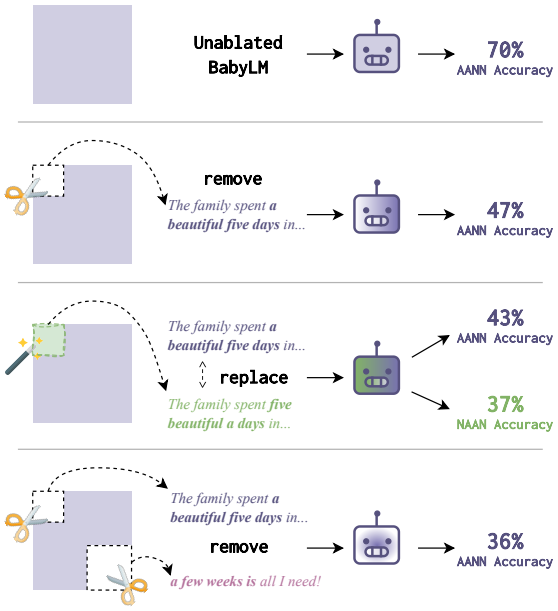

Figure 1: We train LMs on varied input corpora and measure learning of the AANN ("a beautiful five days"), comparing across systematically manipulated corpora. E.g. we train on a control corpus, a corpus in which we remove all AANNs, a corpus in which we replace all AANNs with a corrupted version ("beautiful a five days"), and a corpus in which we remove AANNs and remove related constructions like "a few weeks is". We measure learning of AANNs and corrupted variants.

1 Introduction

1.1 Motivation and Prior Work

Humans come to learn and use rare grammatical structures, even if they have encountered those structures only rarely or even not at all (Pullum and Scholz, 2002; Pearl, 2022). For instance, humans accept the grammaticality of the PiPP construction ("surprising though it may be...") even where the preposed element crosses a finite close boundary ("surprising though I know it may be that...") (Pullum, 2017) and even though they may plausibly have *never* encountered such a sentence in their linguistic experience (see Potts, 2023, for corpus estimate). How people come to know an utterance is grammatical has occupied a central place in linguistics. Specifically, mastery of never-before-encountered grammatical structures has been taken to mean that people are endowed with innate linguistic knowledge (Chomsky, 1986, 1957, 1965).

Recent evidence, though, suggests that Large Language Models (LLMs) can learn complex grammar (Wilcox et al., 2018; Futrell et al., 2019; Linzen et al., 2016; Mahowald et al., 2024) even from human-scale amounts of input (Warstadt et al., 2023; Eldan and Li, 2023; Huebner et al., 2021). This raises the possibility that input data, along with an appropriately sophisticated or weakly bi-

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ased statistical learning mechanism, is sufficient for learning rare constructions by allowing for models to emergently learn appropriate grammatical abstraction (Baroni, 2022; Misra and Kim, 2023). But modern LLMs often have access to much more training input than people do and thus might memorize in a way that humans cannot (Linzen, 2020; Warstadt, 2022; Warstadt et al., 2023). The possibility that LLMs are "stochastic parrots" (Bender et al., 2021), heavily reliant on memorization, is a common criticism of using LLMs to study human language (e.g., Chomsky et al., 2023).

There are different levels of memorization, though, requiring different levels of abstraction. Consider the AANN construction: "a beautiful five days in Texas" (Solt, 2007; Keenan, 2013; Dalrymple and King, 2019), which is rarer than the default "five beautiful days in Texas". A model that strictly memorizes this phrase might come to know that "a beautiful five days in Texas" is grammatical but has no idea that "a beautiful four days in Texas" is grammatical if it never appeared in its training. A model that generalizes just a bit more might know that "a beautiful five days in New York" is also grammatical by generalizing that any U.S. state can fill the slot. Knowing that "an astonishing 200 pages" is acceptable requires generalization beyond mere lexical items. And knowing that "a blue five pencils" is *not* acceptable (because colors are "stubbornly distributive", Schwarzschild 2011) requires yet more knowledge. Even for an idealized learner, it is difficult to precisely formulate how these kinds of generalizations emerge.

There is increasing evidence that LLMs can generate novel linguistic utterances (McCoy et al., 2023), and also make subtle judgments on relatively rare constructions like these (Weissweiler et al., 2022; Potts, 2023), including the AANN (Mahowald, 2023). If they do so by memorizing examples *verbatim* from an unrealistically large training corpus, that would not be particularly informative for human processing. But, if they do learn rare grammatical phenomena from smaller amounts of data and can generalize beyond just those verbatim instances, that would raise the question of how they do it and if it can inform theorizing about humans. For instance, in the context of the PiPP construction, Potts (2023) speculates that the comparative construction (e.g., "They are happier than we are.") "may be the key to all of this [i.e., learning the PiPP]" because such constructions are "incredibly common" yet share abstract structure with the PiPP. If LLMs learn rare grammatical structures in part by learning and generalizing structures from much more common constructions, that would be powerful evidence for abstraction in LLMs and raise the possibility that even quite general learners could learn very rare phenomena without strong innate priors, drawing in part on the long-posited linguistic hypothesis that apparently distinct grammatical phenomena often share underlying structure.

To that end, our goal in this paper is to study a relatively rare grammatical phenomenon in LMs trained on controlled input corpora that are (a) of human realistic scale, and (b) systematically manipulated with respect to the target constructions as well as key related constructions. Our hypothesis is that **generalization abilities of LMs on such rare phenomena come from abstractions and structures learned from more frequent related phenomena**—and that knowledge of more frequent phenomena *is* the "key to all of this."

As a case study, we focus on the aforementioned AANN construction, although we highlight how the methods used here could serve as a blueprint for work on other phenomena. Our method is to train different instantiations of a transformer model on the 100M-word BabyLM corpus, which we systematically manipulate—via removal and replacement—to explore how frequent and related phenomena encountered during training facilitate generalization behavior in LMs. To test for generalization, we subjected our LMs to a series of acceptability tests on sentences which do not appear in the training corpus and which specifically target the special properties of the AANN.

This approach of training on a systematically manipulated corpus has been used to debias models (Maudslay et al., 2019; Lu et al., 2020), explore the effect of permuting words on pretrained models (Sinha et al., 2021), and test whether LMs can learn languages judged to be hard for humans (Kallini et al., 2024). It is also becoming a fruitful method for giving *causal* answers to questions about syntactic learning in language models, including hypotheses about learning subject-verb agreement (Wei et al., 2021), the acquisition of negative polarity items (Jumelet et al., 2021; Weber et al., 2021), subject-auxiliary inversion (Warstadt, 2022), and the English passive alternation (Leong and Linzen, 2024). Using this "filtered pretraining" method, Patil et al. (2024) find evidence of syntactic generalization underlying models' success on syntactic benchmarks. While this related work has largely fo-

cused on ubiquitous linguistic structures (e.g., passives, subject-verb agreement, etc.), we specifically zero in on a rare construction to explore learning in the linguistic "long tail", where there is relatively little evidence available in the input.

1.2 Summary of findings

First, we find BabyLM-trained LMs to successfully generalize to novel instances of the AANN construction. Performance unsurprisingly drops for LMs that were trained *without* being exposed to even a single AANN during training, but perhaps surprisingly, not by all that much—they are substantially above chance. This suggests that certain items present in the training data might give rise to LMs' non-trivial performance in judging acceptability of AANNs. This finding is further strengthened by the fact that LMs trained on counterfactual variants of the AANN—e.g., ANAN and NAAN, obtained by shuffling word order and are far less likely to share structure with training data items—are unable to generalize to those constructions as well as they do to AANNs (one which they have not seen at all).

Next, we investigated what might enable LMs' learning of the AANN, by further systematically manipulating their training data to hold out utterances conforming to specific linguistic and statistical phenomena. Through our manipulations, we find LMs become worse at predicting novel instances of the AANN as more frequent, non-AANN-but-AANN-related phenomena are held out. For example, phenomena such as the treatment of measure noun phrases as singular (e.g., ***a few days is*** *all we need*)—similar to how AANNs treat a plural NP as singular—end up making unseen AANNs less likely by 36.5% on average. Importantly, these results could not be explained simply by loss of data—LMs that were trained with these phenomena left in but without an equivalently large chunk of the training data removed were almost as good as LMs that never saw AANNs. This further strengthens the conclusion that the hypothesized linguistic phenomena did indeed affect generalization of the targeted construction. Notably, LMs are largely affected by these manipulations when they do not see *any* AANNs during training, highlighting the expected non-trivial role of encountering some instances of AANNs to show stronger generalization.

Finally, we characterized the aforementioned interplay between the properties of the encountered AANNs and the LMs generalizations on novel instances. Here we found LMs that observed AANNs with more variability on the adjective, numeral, and noun slots to show better generalization than did LMs that saw more restricted-but-repeating instances of AANNs. This importantly mimicked analogous findings of inductive inference in humans across disciplines (Osherson et al., 1990; Goldberg, 2005; Xu and Tenenbaum, 2007; Baayen, 2009; Suttle and Goldberg, 2011; O'Donnell, 2015).

Taken together, these results provide an existence proof that a weakly biased but sophisticated general-purpose statistical learner can learn rare and complex linguistic phenomena, in part because of key related phenomena seen during training. While our analyses suggest potential links between "constructions" (Goldberg, 1995), our findings are also compatible with theories that think of rare phenomena as derivationally related (Chomsky, 1965) to more frequent and well-attested structures (much as Potts, 2023, posits shared *syntactic* structure as the key to the PiPP).

2 General Methods

2.1 Corpus

Throughout, we use the BabyLM-strict corpus (Warstadt et al., 2023) as our base training set. We use BabyLM-strict because of its human-realistic scale and tractable size (100M tokens), which allows us to (1) detect and control the instances of the target construction as well as related linguistic phenomena; and (2) train a large number of LMs in a reasonable timeframe.

2.2 Language Model

Our LMs are instances of OPT LM (Zhang et al., 2022), an autoregressive transformer architecture. Our LMs have 12 layers and attention heads, use a vocabulary size of 16,384, and are trained for a maximum of 20 epochs using the `transformers` library (Wolf et al., 2020). The results we report for a given LM are averaged over three different runs (with different random seeds). We list other hyperparameters and architectural details in App. B.

2.3 Construction Detection

To detect AANNs, we used a regex over a part-of-speech tagged version of BabyLM. Specifically, we started with a regex for detecting AANNs and then measured its recall by hand-annotating examples (with annotations performed by the authors) found by an extremely permissive regex that looked for any "a" or "an" that appeared sequentially prior

to a numeral and a plural noun in a sentence (thus likely capturing almost all AANNs, albeit with very low precision). We used the hand annotations to iteratively refine our regex and handle special cases. We continued this process until, on the final set of hand annotations, we detected 17/18 instances (missing only an instance where "pound" was used instead of "pounds" due to an apparent typo—but since this violates the key plural-noun requirement of AANNs, it is unclear if it counts as a genuine missed instance). Ultimately, our final regex detected 2,448 AANNs in the BabyLM corpus (about 0.02% of the total 11.5M utterances). See App. C for our detailed pipeline and our recall analysis.

Even with the refined regex, we cannot guarantee perfect recall—a potential issue for claims about learning in the absence of *any* occurrences. To address this issue, we include controls in which we assume that we missed 300 AANNs (a conservatively high number, given our recall estimate) and artificially "pollute" the data to drown out the effect of any remaining AANNs. As described below, our conclusions were unchanged in this robustness analysis, suggesting our results were not driven by undetected AANNs.

2.4 Acceptability data

To test our LMs on their knowledge of the AANN, we use data from Mahowald (2023), which consists of 12,960 templatically generated sentences that contain AANNs. Out of these, 3,420 contain acceptability ratings provided by 190 human participants, ranging from 1 (unacceptable) to 10 (acceptable). We use 7 as the threshold for clear acceptability, in that we only keep instances where human participants rated the acceptability of the construction in context to be greater than 7. We additionally discarded instances where the AANNs appear in the BabyLM training set ($n = 4$), as testing on these would not shed light on the LMs' generalization behavior. This leaves us with 2,027 items.

For each AANN instance in the dataset, Mahowald (2023) has also made available its corresponding corrupted versions, which focus on (1) adjective-numeral order; (2) presence of the article; (3) presence of the adjective; and (4) presence of the numeral. A hypothetical example of these corruptions is shown in Table 1 under the "AANN" column. A model that has knowledge of the AANN should find the well-formed instance to be more likely than each of its corrupted versions. Below we describe methods to measure likelihood and assess accuracy on these tests.

2.5 Scoring and Accuracy

We use the Syntactic Log-odds Ratio (SLOR) (Pauls and Klein, 2012; Lau et al., 2017) to score sentences in our tests. Given a sentence containing a prefix followed by our target construction $\mathcal{C}$ and an optional suffix, SLOR is computed as the log of the ratio between the probability of the construction given the prefix as estimated by the LM, and that estimated by a unigram model, normalized by the length of the construction. Given a language model m and a unigram estimator u:

$$\text{SLOR}_{\text{prefix}}(\mathcal{C}) = \frac{1}{|\,\mathcal{C}\,|} \log \frac{p_m(\mathcal{C} \mid \text{prefix})}{p_u(\mathcal{C})} \quad (1)$$

Importantly, we train the unigram estimator for a given corpus using the same tokenizer used to train our autoregressive LMs on that corpus. We use SLOR in lieu of the usual normalized log-probability measure, ensuring that the comparison between two models cannot be explained simply by the difference in unigram frequencies due to our manipulations. Log-probabilities were computed using `minicons` (Misra, 2022). An instance within our test set is considered to be correct *iff* the SLOR values of the well-formed construction is greater than that for *all* four corrupted instances. The accuracy, then, is the proportion of correct instances within the test set. Since this involves four pairwise comparisons, chance performance is 6.25%.

2.6 Ablations

Common to subsequent experiments (§4 and §5) is the fact that we hold out certain parts of the BabyLM corpus—parts that conform to a certain linguistic or statistical hypothesis. This creates a gap between the experience of LMs trained on these ablated versions of the corpus, and that of the LM trained on the full BabyLM data. To circumvent this issue, we up-sample non-hypothesis-conforming utterances in BabyLM after performing our ablations, in a manner such that the LM still encounters the exact same number of tokens.

3 Experiment 1: LMs learn about AANNs without having seen a single instance

LMs learn about AANNs... To investigate the extent to which LMs trained on BabyLM learn the AANN construction, we measure their accuracy on our tests described in §2.4. From Fig. 2, we observe

Context	AANN	ANAN	NAAN
WELL-FORMED	a whopping ninety LMs	a ninety whopping LMs	ninety whopping a LMs
		Corruptions	
ORDER-SWAP	a ninety whopping LMs	a whopping ninety LMs	whopping ninety a LMs
NO ARTICLE	whopping ninety LMs	ninety whopping LMs	ninety whopping LMs
NO MODIFIER	a ninety LMs	a ninety LMs	ninety a LMs
NO NUMERAL	a whopping LMs	a whopping LMs	whopping a LMs

Table 1: Well-formed and corrupted examples of the AANN construction and its counterfactual versions (ANAN and NAAN). Corruption types are taken from Mahowald (2023).

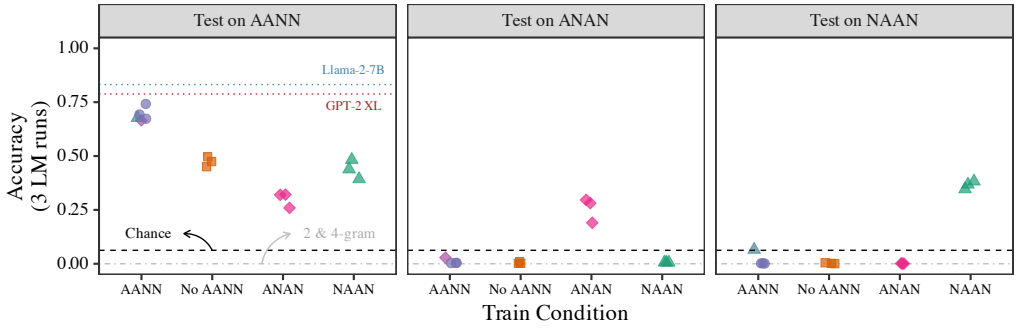

Figure 2: Accuracies on tests for AANN and its counterfactuals (ANAN and NAAN), achieved by LMs trained on BabyLM with various AANN-manipulations (AANN as is, NO AANN, ANAN, NAAN). ◆ and ▲ under the AANN training condition are cases where training data was polluted by randomly replacing 300 AANNs with ANANs and NAANs, respectively, in order to assess the impact of an imperfect AANN detection system. The dashed line represents chance performance (6.25%) and the dot-dashed line represents accuracies for 2- and 4-gram LMs trained on BabyLM. Accuracies for GPT-2-XL (Radford et al., 2019) and Llama-2-7B (Touvron et al., 2023) are computed using **log-probabilities**, since unigram frequencies were unavailable for these LMs' corpora.

that the BabyLM-trained LMs obtain accuracies around 70%, which is substantially above chance. **This suggests that LMs can reasonably acquire knowledge of AANNs, even though they make up only 0.02% of training utterances.**

For comparison to larger, state-of-the-art LMs, we test Llama-2-7B (Touvron et al., 2023) and GPT-2 XL (Radford et al., 2019) on the AANNs. They got 83% and 78%, respectively. As a comparison to shallower LMs, we tested on 2-and 4-gram LMs trained on BabyLM and found both got 0% accuracy, strongly suggesting that the observed results are not due to n-gram statistics.

...without having seen a single instance... Given that BabyLM-trained LMs learn the AANN construction, how well would an LM generalize to AANNs *without having seen a single positive instance*? To this end, we compare accuracies in the previous experiment to that obtained by LMs trained on BabyLM with our **2,448 detected AANNs removed** (i.e., NO AANN). From Fig. 2, we find LMs trained with the NO AANN condition to achieve an average accuracy of 47%, which is a noticeable drop compared to the 70% obtained by the LMs trained on the complete BabyLM corpus, but importantly 40.75 points above chance performance (and, as we show below, well above comparable baselines with other constructions). This is a non-trivial result, since it suggests that LMs can learn the acceptability of a construction without having seen a single positive occurrence, which indicates that there exist systematic patterns in the corpus driving this generalization behavior.

...more strongly than they learn counterfactual AANN variants... To further contextualize the above results, we consider two counterfactual cases, where we replaced AANNs in BabyLMs with instances involving the same lexical items, but in a word order that violates English grammar: (1) ANAN (e.g., *a 90 whopping LMs*); and (2) NAAN (e.g., *90 whopping a pages*). This allows us to test if the results before are genuinely because LMs recognize the nuances of the AANN construction. If LMs are able to learn these counterfactual constructions just as well as the LMs in the previous experiments learned AANNs, then the generalization claims from the previous experiments would be weakened. To test for such possibilities, we

create counterfactual versions of the Mahowald (2023) stimuli, where we apply analogous corruptions to the counterfactual variants of AANN, such that they are violated in a similar manner as in the AANN tests. Examples for the three types of instances in our tests can be found in Table 1. We then evaluate the previous two LMs (trained on BabyLM with and without seeing any AANNs) with LMs trained on BabyLM with these counterfactual variants on judging the acceptability of AANNs, ANANs, and NAANs. Fig. 2 shows these results, from which we make two high-level observations. First, and most importantly, **LMs that see ANANs and NAANs do not learn those constructions as well as they learn AANNs**—especially the LM that saw no AANNs (47% AANN accuracy compared to 37% NAAN accuracy obtained by the NAAN-trained LM). Second, these LMs end up learning AANNs better than they learn counterfactual constructions that they observed in lieu of the AANN—e.g., NAAN trained LM achieves around 43% accuracy on AANNs *even though* NAANs *appeared frequently in the data and no* AANNs *did*. This, combined with the results of the previous two sub-experiments suggests strongly that LMs pick up on cues from other—likely related—constructions encountered during training in order to assign non-trivial probability to unseen instances of AANNs.

...even with artificially polluted data... As discussed in §2.3, our AANN detection pipeline could miss AANNs in the training corpus. This limitation could impact the conclusions of this experiment if LMs' preference for assigning greater probabilities to AANN instances in the test set could be explained by the presence of undetected AANNs, even in the 'No AANN' condition. We controlled for this confound by artificially polluting the training corpus, such that a small percentage of the detected AANNs are replaced by NAANs/ANANs. This simulates a scenario analogous to the issue at hand: our target is now a counterfactual variant of the AANN, and our 'imperfect' pipeline has missed out on a handful of instances in the training set. If there is a genuine impact of such a setting, then we should observe greater accuracies on the counterfactual instances and at the same time, a drop in performance on AANNs. We ran two experiments to test this, where we replaced 300 AANNs (about 12%) of the detected AANNs with ANANs in one experiment, and NAANs in the other. We then tested the two resulting LMs—pretrained on corpora reflecting these ablations—on both AANNs and the respective counterfactual constructions. As seen in Fig. 2, we observe almost no differences in the results obtained from this artificial pollution experiment and those from our original experiments (see ◆ for ANAN, and ▲ for NAAN). Because 300 is a conservative upper bound on undetected AANNs, we do not think imperfect recall drives our results.

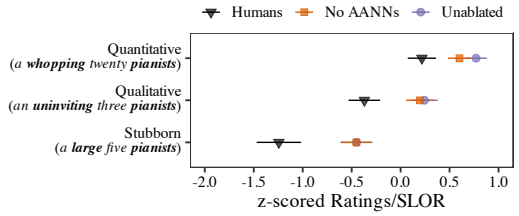

Figure 3: *z*-scored AANN acceptability ratings from humans and LMs trained on corpora with (1) AANNs removed (i.e., NO AANN); and (2) left unablated for AANNs with 'Human' nouns in Mahowald (2023)'s dataset. Even with ablated models, we observe the predicted dispreference for stubbornly distributive adjectives in the AANN. Full results in Fig. 7.

...in a way that extends to lexical constraints. While we focused on overall structural properties of AANNs, there are also idiosyncrasies to the construction that arise from lexical semantic constraints. For instance, in many AANN sentences, people prefer quantitative adjectives such as *mere* and *hefty* to qualitative ones such as *beautiful* (Mahowald, 2023; Solt, 2007) and find "stubbornly distributive" adjectives ("*a *blue* five pencils") completely unacceptable (Schwarzschild, 2011). Insofar as our models learn AANNs, we also should expect them to learn these lexical constraints. To test this, we compared LMs' SLORs to human acceptability judgments on all 3.4k instances in Mahowald's data across different adjective and noun classes. We found LMs trained on the original, unmodified BabyLM corpus to pattern similarly to humans in their preference for lexical constraints affecting AANNs. Interestingly, these patterns were *unchanged* for LMs trained with the NO AANN condition, conforming to our predictions. For instance, as seen in Fig. 3, both our models share the human preference for quantitative and qualitative adjectives in the AANN, compared to stubbornly distributive adjectives. More detailed results on lexical constraints can be found in App. E and we hypothesize that our broader set of results extends to include learning of lexical constraints on the construction.

4 Experiment 2: Keys to Learning AANNs

Experiment 1 reveals that, keeping everything else the same, LMs learn the AANN construction more accurately than they do its counterfactual variants. Furthermore, we also see strong AANN acceptability judgments in LMs that have (almost) never encountered a single instance. This suggests that there could be instances in the training data that contribute to the learning of the construction.

What might these be? Below we enumerate four hypotheses, each of which tackles subtly different aspects of the AANN construction, and then measure the effect of these phenomena by separately holding them out during training and computing the average SLOR of the well-formed instances of the AANN tests. The effect of a particular phenomenon on the acceptability of AANNs can therefore be measured by comparing SLORs before and after holding out instances of that phenomenon. Methods for detecting the hypothesized phenomena can be found in App. C. As control, we additionally also hold out a random set of utterances, which do not conform to the target phenomena of interest. Table 2 lists the hypotheses we consider, along with an example of their utterance and frequency of occurrence, in the BabyLM corpus.

The presence of "the ANN" Phrases like "*the beautiful five days*" are common in corpora, which are not as unusual because "the" regularly takes plural nouns. We hypothesize that the acceptability of these structures affects the acceptability of AANNs, since an LM might analogize from the general observation that 'a' or 'an' can substitute 'the' (e.g., a ball vs. the ball). Therefore, we consider all cases where a determiner precedes the contiguous sequence of article, numeral, plural noun.

A few/couple/dozen/etc. NNS Another related phenomenon that is more common relative to the AANN construction involves phrases such as "*a few days*" or "*a couple bottles*". To an LM learner, they might provide evidence that measure noun phrases with plural nouns can be attached to an indefinite article (a/an; Solt, 2007), as is the case in AANNs.

Measure NNS treated as singular We consider yet another phenomenon involving phrases that treat measure nouns as singular, this time in terms of agreement, e.g., "*Five miles **is** a long way to go*", and "*1,000 pages **is** a lot for a dissertation.*" These cases might provide further evidence to the model that measure noun phrases with plural nouns can be treated as a singular unit (Solt, 2007), thereby affecting the acceptability of the AANN. These are less prevalent compared to the cases involving **a few/couple/dozen NNS**, but still far more frequent than the AANN, therefore, we combine the two as a general case of treating measure NPs as singular.

Phenomenon/Manipulation	Example/Desc.	Freq.
AANN	**a fine eighteen months**	2,448
DT ANN	**the usual forty dollars fine**	15,781
A few/couple/dozen/etc. NNS	**a few plums**	55,373
Measure NNS with SG verbs and/or indef. articles	**6 months is a long time**	62,744
A/An + ADJ/NUM balancing	`enforce freq. balance`	571,874
Random removal (**control**)	`randomized ablation`	571,874

Table 2: Manipulated Phenomena, their examples/descriptions, and their frequency in the BabyLM corpus.

Balancing the frequencies of A/An + ADJ/NUM A more surface-level reason why "a beautiful five days" might be more natural to LMs than is "a five beautiful days", could be that adjectives are more likely to follow indefinite articles than are numerals. For instance, adjectives are ≈14.6 times more likely to follow indefinite articles in the BabyLM corpus than are numerals. To measure this effect, we hold out instances such that adjectives and numerals are equally likely to follow an indefinite article. This ends up being the largest portion of the data that we hold out.

Control: Random removal A potential confound in the above could be that the SLOR values of the AANNs go down merely due to loss of content, even though we add back additional tokens from BabyLM (such that all LMs see the exact same amount of tokens). To account for this, we additionally consider a control where we remove as many tokens as in the largest ablation (i.e., the **A/An + ADJ/NUM** case) such that none of the above phenomena are taken out.

4.1 Analysis and Results

In our experiments, we individually ablate out each of the aforementioned phenomena under two settings: (1) AANNs are removed during training in addition to the target phenomena; and when possible, (2) AANNs are seen during training. (1) is a stricter setting, since here the LMs see neither the target phenomenon nor a single instance of the AANN. Comparing average SLORs obtained in this condition to that obtained for the NO AANN can shed light on the extent to which the target

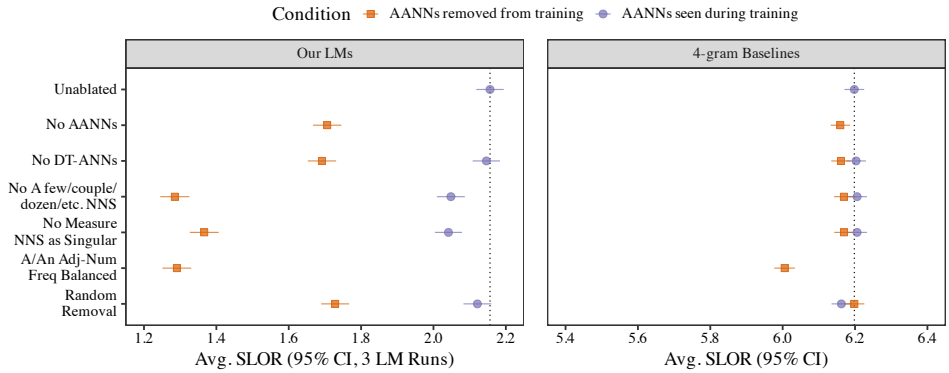

Figure 4: SLORs on AANNs from Mahowald (2023) for our LMs (left) and a 4-gram baseline (right) trained on BabyLM and ablated versions. Our LMs show a range of hypothesized effects, suggesting they contribute to AANN learning. In contrast, the 4-gram LMs show mostly null results (except for the adjective/numeral balanced condition, which is highly sensitive to n-gram frequencies). The dotted line is SLOR for an **unablated BabyLM-trained LM**.

phenomenon is critical in allowing LMs to assign non-trivial probabilities on unseen AANNs, *zero-shot*. On the other hand, (2) still allows for LMs to perform lexical generalization (Kim et al., 2022), where they may exhibit strong probabilities on the test AANNs by performing slot filling, without necessarily relying on the hypothesized phenomena.

Fig. 4 shows the average SLORs obtained across various ablations under the two settings. As a baseline, we compare our results to 4-gram LMs, trained using KenLM (Heafield, 2011), on corresponding ablations of the BabyLM corpus. We observe that holding out most of our hypothesized phenomena has non-trivial effects on our LMs' ratings of unseen AANNs, and that these effects are different for when AANNs are seen during training, or are held out. When AANNs are held out along with the phenomena, we see substantial drops in the average SLOR values assigned by LMs on unseen AANNs relative to that assigned by LMs in the NO AANN condition. Specifically, balancing the frequency of adjectives and numerals following an article, along with the two cases where measure nouns are treated as singular, have the greatest effect. This suggests that, in the absence of even a single AANN during training, these phenomena are critical for LMs to assign probability to AANNs. Interestingly, holding out cases that involve any determiner + adjective + numeral + noun sequence has almost no impact relative to LMs trained on a corpus without *only* the AANNs removed. Simply ablating large amounts of data cannot explain these results, since LMs trained on our controlled condition show higher SLOR values than in our hypothesis-informed ablations. These patterns are absent in 4-gram LMs, suggesting that they do not arise as a result of shallow statistics—with the exception of differences observed for the article+adjective/numeral ablation. Overall, this finding indicates that **LMs can demonstrate a novel phenomenon (AANN) by relying on other related—and more frequent—phenomena.**

When AANNs *are* seen during training, however, we observe LMs' results on unseen AANNs to show more similar SLOR values with respect to the LMs trained on the unablated BabyLM corpus, although they are still significantly reduced in some cases (e.g., singular measure nouns). We conclude that direct evidence of observing instances of AANN construction substantially enables generalization to unseen instances. At the same time, the presence of some key related phenomena in addition to direct evidence has an additive effect on this generalization behavior.

5 Experiment 3: The Role of Variability

Results from Experiment 2 highlight the importance of seen AANNs in order for LMs to generalize to unseen instances. What properties of these seen instances facilitate LMs generalization behavior? This broadly relates to a longstanding question as to how the nature of the instances of a construction provided during learning affect its (partial) productivity (Goldberg, 2005, 2019). In the context of AANNs, we consider the role of *variability* on the open slots of the construction as a factor that might play a role in LMs' productivity on unseen instances. Encountering a slot with a wide variety of lexical items could serve as a cue that the slot is flexible. The idea that instance-variability could affect learnability is motivated by theoretical claims

in usage-based linguistics (Bybee, 1995), as well as existing research on novel constructions (Suttle and Goldberg, 2011), morphological productivity (Baayen, 2009; O'Donnell, 2015), and inductive inferences in cognitive psychology (Osherson et al., 1990; Xu and Tenenbaum, 2007).

We hypothesize that instances of AANNs that provide natural evidence of greater open-slot variability—i.e. evidence that many different adjectives, numerals, and nouns can go in their respective positions in the AANN construction—would lead LMs to assign greater likelihood to unseen AANNs. On the other hand, LMs that encounter only a restricted set of instances might be more conservative in extending the coverage of possible AANNs to novel combinations of the slot-fillers. To test this, we divided our set of 2,448 AANN-containing utterances in the BabyLM corpus into two roughly equal subsets—one that contained AANNs which were restricted in which tokens occur in a particular slot (low variability), and the other where the AANNs showed more variability in those slots. We obtain these subsets by performing a median split based on the number of unique occurrences in a target slot(s), which resulted in a set of 1224 low and high variability instances. We repeated this for all three open slots (adjective/numeral/noun) jointly as well as those slots individually—i.e., 4 different types of target slots and 2 conditions each (low vs. high variability). Details about the slot fillers and examples from each set are provided in App. F. We then trained LMs on the BabyLM corpus containing utterances involving either of these two cases. Fig. 5 shows the resulting average SLORs obtained from this experiment, along with those obtained by LMs trained on unablated BabyLM and the NO AANN conditions.

We see that the SLOR patterns of LMs trained on corpora that differed in AANN slot-variability lie between the SLOR values elicited by LMs that never saw AANNs and ones that saw every single AANN in the original corpus. Among these, LMs that saw AANNs that were highly variable in their open-slots elicited SLORs that were greater than those elicited by LMs that saw AANNs with low open-slot variability. This was true for all cases except when "Numeral" was the target slot, where both variability conditions resulted in roughly similar SLORs. (We hypothesize that numerals may pattern differently since they may be inherently more fungible than other word classes.) Overall, these results suggest that LMs are sensitive to the nature of

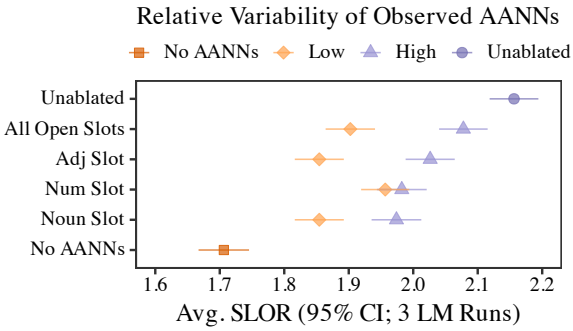

Figure 5: SLORs on AANNs from Mahowald (2023) for LMs trained on BabyLM with low and high variability in the open slots of the *observed* AANN instances. When models are presented with higher variability for a given slot, the construction is typically learned better.

range of fillers that go into the construction's open slots, showing relatively greater productivity when they observe evidence that the slots were highly variable. This is compatible with our hypothesis that slot-variability might affect the extent to which LMs "permit" productive uses of a construction.

6 Conclusion

Theoretically, there is, for good reason, considerable interest in how language models can handle what has been variously called the "long tail" of language (Prange et al., 2021), "extremely rare constructions" (Potts, 2023), "exceptions to syntactic rules" (Leong and Linzen, 2023), "rare linguistic phenomena" (Weissweiler et al., 2024), *inter alia*. Studies of such phenomena are important first because LMs (and statistical models in general) are sensitive to frequency and often perform far better in data-rich environments and, second, because the human ability to generalize to rare phenomena is central to linguistics.

Empirically, we found that LMs trained on modest amounts of data can learn a rare construction like the AANN. We found that this learning occurs even without veridical instances of the construction in the training data, and that it is mediated by occurrences of other related constructions in training. As such, these results join a body of work showing the ability of LLMs to learn rare phenomena (Tayyar Madabushi et al., 2020; Tseng et al., 2022; Li et al., 2022; Veenboer and Bloem, 2023) and to generalize from limited data in meaningful ways.

Methodologically, this work leave us optimistic that "controlled rearing" of LMs is a fecund method for understanding models, as well as for gleaning insight into human language more generally.

7 Limitations

In future work, it would be valuable to extend this method to a wider range of constructions. But scaling this approach up is not straightforward since it requires identifying and extracting idiosyncratic constructions, and—more onerously—developing testable hypotheses about what makes them learnable from limited amounts of data. Future work will likely benefit from synergistic collaborations between theoretical and computational linguists.

Another limitation is that our method requires repeated training of LMs from scratch which can be computationally expensive. Alternate methods could be to ablate knowledge of particular hypotheses using representational editing methods, though these may not guarantee perfect removal of the knowledge of targeted constructions.

Unlike Weissweiler et al. (2022), we do not test the ability to interpret these constructions for downstream tasks. Instead, our ablations target linguistic form alone, and preliminary experiments suggest that our ablations and manipulations leave the lexical semantic properties of the AANN unchanged (see App. E). Extending our ablation method to target these properties more directly would be quite informative.

Finally, this work only studies a rare construction in English, and on LMs that are trained on English text data. While this is a limitation of the paper, the paradigm introduced can be readily used in future work to study hypotheses and perform indirect evidence elicitation in multi-lingual LMs.

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A Dataset Access and Licensing

The AANN acceptability dataset by Mahowald (2023) is released using the MIT License and was accessed from the author's public github repo.[1] The BabyLM dataset[2] does not have a single license of its own but instead inherits the licenses of its constituents: CHILDES (MacWhinney, 2000), BNC Dialogue portion,[3] Children's Book Test (Hill et al., 2016), Children's Stories Text Corpus,[4] Standardized Project Gutenberg Corpus (Gerlach and Font-Clos, 2020), OpenSubtitles (Lison and Tiedemann, 2016), QCRI Educational Domain Corpus (Abdelali et al., 2014), Wikipedia,[5] Simple Wikipedia,[6] Switchboard Dialog Act Corpus (Stolcke et al., 2000). Since this dataset was specifically released to train LMs, we work under the assumption that our LMs do not violate its license policies. We will follow the inherited licenses' policies while making the trained LMs and ablated BabyLM data public, and refrain from releasing them if we find them to be in violation of the policies.

B LM training details

As mentioned in the main text (see §2), we use the OPT architecture (Zhang et al., 2022) to train our LMs on all versions of the BabyLM corpus. This was the best performing autoregressive LM in the BabyLM Competition (Warstadt et al., 2023). For each instance of the BabyLM (ablated or otherwise), we tune the learning rate[7] based on the validation set, and use the best learning rate as a result of the tuning to train an additional two language models using different seeds. As a result, for each ablation of the BabyLM corpus, we run 6 LM

[1] https://github.com/mahowak/aann-public

[2] accessed from https://babylm.github.io/

[3] http://www.natcorp.ox.ac.uk

[4] https://www.kaggle.com/datasets/edenbd/children-stories-text-corpus

[5] https://dumps.wikimedia.org/enwiki/20221220/

[6] https://dumps.wikimedia.org/simplewiki/20221201/

[7] We searched the following set: {1e-4, 3e-4, 1e-3, 3e-3}

training experiments, which amounts to a whopping 114 LMs for all our experiments. Table 3 contains further details of the training.

(Hyper)parameter	Value
Architecture	OPT (Zhang et al., 2022)
Embed size	768
FFN dimension	3,072
Num. layers	12
Attention heads	12
Vocab size	16,384
Max. seq. length	256
Batch size	32
Warmup steps	32,000
Epochs	20
Total parameters	97M
Training size	100M tokens
Compute	1x NVIDIA A40
Training time	21 hours

Table 3: LM Training details

C Detecting AANNs and related phenomena

In this section, we briefly describe our methods to extract constructions and phenomena relevant to this paper from the BabyLM corpus (Warstadt et al., 2023). Our methods primarily rely on: 1) the surface form of the sentences in the corpus; 2) their corresponding part-of-speech (POS) tag sequences; and in a few cases, 3) their dependency parses. For the latter two, we used `spacy` (Honnibal et al., 2020), specifically, its `en_web_trf` model, which is based on the RoBERTa-base LM (Liu et al., 2019). Next we describe how we used these artifacts to detect our target constructions:

C.1 AANNs

To detect AANNs, we constructed a regex-based pattern-matcher which operated over a POS-tagged version of the BabyLM corpus. We started with an initial regex pattern (`Regex v1`), as shown in Listing 1:

Listing 1: Regex v1.

```
pattern = r'\b(DT)(?:(?:\s(RB))*\s(JJ|JJR|JJS)
    (?:\s(CC))*)+(\s(CD|JJ|JJR|JJS|NN|CD\sCD)
    (?:\s(TO|CC)\s(CD))*)(\s(NNS|NNPS|(NN\sNNS)
    |((NN|NNS)_IN_NNS)))+'
```

here we restrict the determiner (`DT`) to be either 'a', 'an', or 'another'. This regex permits multiple adjectives (*an **exhilarating and marvelous** three months*) optional adverbs (*an **excruciatingly** painful two semesters*), multi-word noun phrases with plural head-nouns (*a refreshing two **glasses of aperol spritz***), numeral-expressions involving subordinate clauses (*a measly **three to five** days*), among other potential edge cases.

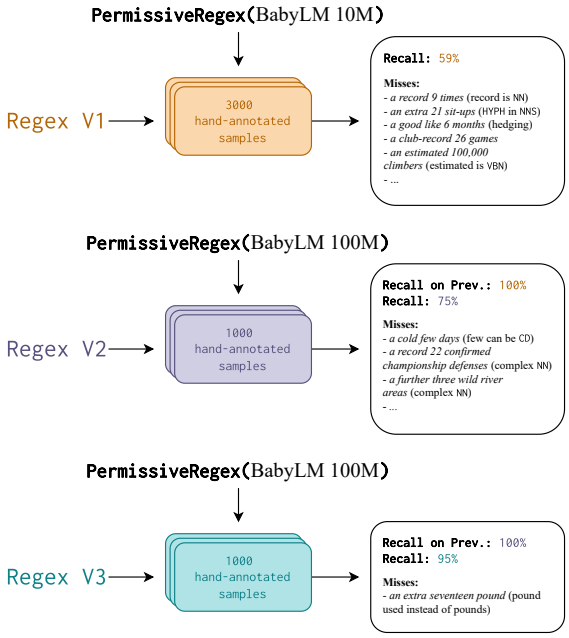


Figure 6: Pipeline to assess the recall of our AANN-detecting regex patterns, along with examples of cases missed by each regex. The recall for our final regex (`Regex v3`) is 95% (missing only one instance where there was a typo), and it is able to handle complex and sophisticated forms of the construction.

We then tested this regex pattern on a large sample of utterances which we extracted using a permissive regex applied to the 10M-token version of BabyLM (a subset of our 100M training set), which looked for any "a" or "an" or "another" that appeared sequentially prior to a numeral as well as a plural noun in a sentence. Importantly this regex filter did not rely on any POS tagging, to avoid issues attributable to tagging errors. We hand-annotated a sample of 3000 utterances from this set, and found 49 legitimate AANNs.[8] Our `Regex v1` only detected 29 of these, meaning its recall was around 59%.

We then developed a second version of the regex (`Regex v2`; see listing 2) to handle cases that the

[8] In reality, we found 50, but rejected one of them: "*a good 1-2" of snow...*, where '"' is inches. This would have never been caught unless we are to include '"' in our pipeline which would conflate other uses of quotes.

above regex pattern missed (e.g., using participle modifiers, occurrence of punctuation or extra spaces in between, accounting for hedging, a case where 'record' was used as a modifier, etc.).

Listing 2: Regex v2.

```
pattern = r'\bDT\s(((HYPH|,)\s))?((((RB|CC|IN)\s
    )+)?((JJ|JJR|JJS|VBN|((NN␣CC␣NN␣|NN␣HYPH␣)+(
    JJ|JJR|JJS|VBN)))((\s(HYPH|,))?)\s))+(((RB)\
    s)+)?(((HYPH|,)\s))?((UH)\s)?(((NN|CC)\s)+)
    ?((CD)(\s(TO|CC|(HYPH|,))(\s(HYPH|,))?)?\s)
    +(((HYPH|,)\s))?(JJR\s)?(DT\s)?((NNS|NNPS|(
    NN\sNNS)|((NN|NNS)␣IN␣NNS)))+'
```

To test Regex v2, we again used the permissive regex and extracted an additional 1000 samples from our training set. On hand-annotating them, we found 24 valid AANNs, out of which Regex v2 detected 18, bringing up the recall to 75%.

In both the previous cases, we were post-processing the detected AANNs to include certain adjectives (*few, dozen, couple, several, many, more*) as numerals, as per the guidelines of Kayne (2007) and Solt (2007). This allows the following to also be considered instances of the AANN:

(1) a. a beautiful **few** days.
    b. an amazing **dozen** eggs.
    c. a pictorial **several** pages.
    d. a great **many** days.

At the same time, this also ends up including cases such as:

(2) a. a few hundred dollars. (few modifies hundred but not dollars)
    b. an awful couple of days. (pseudo-partitive)

Similarly, we had to include NN within our adjective span of the regex pattern to accommodate 'record' when used as/as part of a modifier (e.g., *a* ***record-high*** *60 miles per hour*), but this exploded the number of "detected" AANNs, lowering our precision drastically, due to which we omitted it.

To address these issues, we decided to preprocess the POS-tagged corpora prior to using our regex, where we substituted articles of interest with the 'ARTICLE' tag, substituted *record* when preceeded by an article with the 'RECORD' tag, and numeral proxies with the 'FEW' tag, though ensuring that it appeared linearly after a known adjective which was not a numeral proxy. This led to the creation of Regex v3 (listing 3):

Listing 3: Regex v3 (final). Tags such as ARTICLE, RECORD, FEW are added after POS-tagging to include certain special tokens.

```
pattern = r'\bARTICLE\s(((HYPH|,)\s))?((((RB|CC|
    IN)\s)+)?((JJ|JJR|JJS|VBN|RECORD|((NN␣CC␣NN␣
    |NN␣HYPH␣)+(JJ|JJR|JJS|VBN|RECORD)))((\s(
    HYPH|,))?)\s))+(((RB)\s)+)?(((HYPH|,)\s))?((
    UH)\s)?(((NN|CC)\s)+)?((CD|FEW)(\s(TO|CC|(
    HYPH|,))(\s(HYPH|,))?)?\s)+(((HYPH|,)\s))?((
    JJR|JJ|VBN)\s)?(ARTICLE\s)?((NNS|NNPS|(NN\
    sNNS)|((NN|NNS)␣IN␣NNS)))+'
```

This was able to handle the idiosyncracies of all previously detected AANNs. We again extracted a further additional 1000 samples to hand-annotate and found 18 attested AANNs. Regex v3 was able to detect 17 out of these (recall of 95%), missing out on only one where an incorrect form was used in lieu of a plural noun (e.g., *pound* instead of *pounds*). We don't really consider this a meaningful missed example since the singular noun actually makes this a degenerate AANN, not a genuine one (but, to be conservative, count it as a miss for assessing a worst-case recall estimate). At this point, we stopped further refining our regex and **used Regex V3 as our final detector**, while also acknowledging that it is perhaps impossible to guarantee whether every single AANN instance is captured by the regex. Fig. 6 shows our recall analysis pipeline in a nutshell.

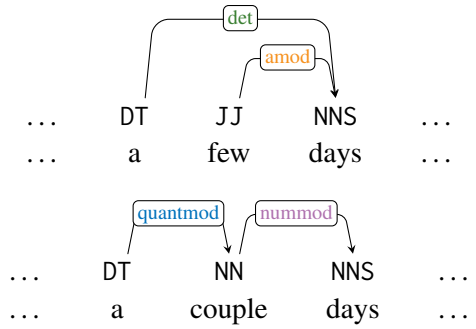
Once detected, we map the found constructions to their respective positions within the AANN format, which allows us to measure metrics such as slot variability, etc.

C.2 DT ANNs

We follow the exact same procedure as the one for AANNs, but no longer restrict the determiner position to only be an indefinite determiner.

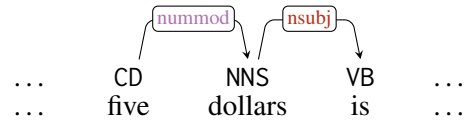
C.3 A few/couple/dozen NOUNs

An important phenomenon that we consider to be related to the AANN involves cases such as: "*that only lasted a few days*" and "*could you bring me a couple liters?*", etc., where the plural nouns are attached to an indefinite article. To detect such cases, we consider the following two dependency configurations, where we have an indefinite determiner (*a, an, another*) with either a det relation with the plural noun (NNS or NNPS) or a quantmod relation with a noun which has a nummod with the plural noun. In the former case, we usually have an amod relation between the noun and the adjective.



C.4 Measure NNS with Singular Verbs

Similar to the previous case, another phenomenon which might be related to the AANN constructions is when measure noun-phrases with plural nouns are treated as singular via their agreement with a verb—e.g., "*five dollars* ***is*** *plenty!*" To detect such cases, we again rely on the following dependency configuration, where we have a plural noun (NNS or NNPS) attached to a cardinal number (CD) via the nummod dependency relation, and at the same time also attached to singular verbs via the nsubj dependency relation (i.e., are subjects of singular verbs).



D A/An + ADJ/NUM frequency balancing

A corpus analysis of BabyLM, along with its POS-tagged version suggests that the sequence "a/an/another (JJ|JJR|JJS)" occurs 613,985 times while "a/an/another CD" occurs only 42,111 times – this suggests that adjectives are approximately 14.6 more likely to follow an indefinite article than are numerals. We therefore balance these values by removing 571,874 instances where adjectives follow an indefinite article. This constitutes the largest-sized ablation in this work.

E Lexical semantic constraints on AANN slots

Fig. 7 shows the breakdown of acceptability ratings from humans and LMs across various adjective and noun classes.

F Variability Analysis

In §5 we compared AANN-generalization of LMs trained on BabyLM versions which differed in the amount of variability that was present in the AANNs

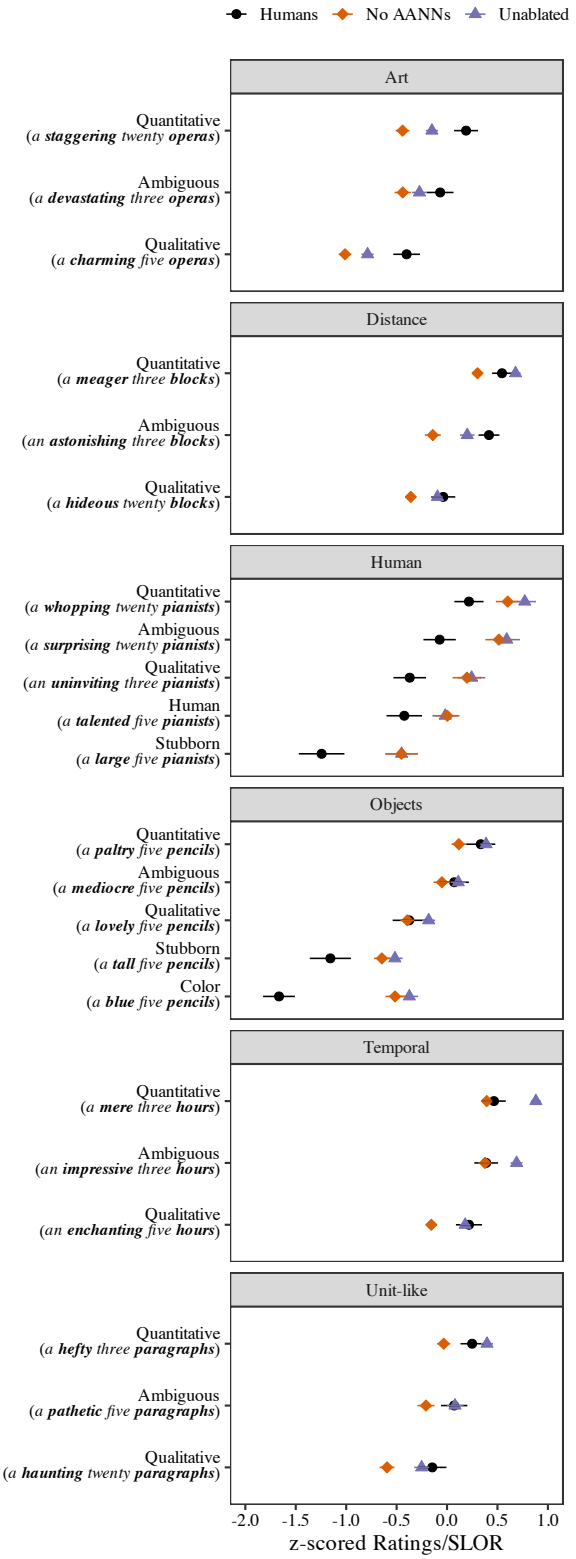


Figure 7: z-scored AANN acceptability ratings elicited from Humans (scale of 1-10) and LMs (SLORs) trained on corpora with (1) AANNs removed (i.e., NO AANN); and (2) left unablated. Ratings broken down based on adjective and noun classes. Ratings are computed for each system based on Mahowald (2023)'s entire dataset, which consists of human derived acceptability judgments on 3,420 different types of AANNs.

that the models were exposed to. In particular, we operationalized variability in terms of the slot-fillers of the adjective/numeral/noun slots, both together as well as individually. Table 4 shows three examples of high and low variability items (each) for the four different slot-filler based considerations in our experiments.

Slot	High Variability		Low Variability	
	Instance	Freq.	Instance	Freq.
All	impressive 30 appearances	1	great many things	42
	massive 108 years	1	good many years	21
	reported 14 million dolars	1	additional two years	4
Adj	career-high	38	great	355
	staggering	12	additional	111
	measly	7	mere	60
Num	20	32	two	174
	couple	17	five	67
	seven to eight	1	few	64
Noun	dollars	15	years	254
	students	8	miles	77
	kangaroos	1	hours	42

Table 4: Examples of slot fillers that were ablated as part of our variability experiments, along with their frequency in the training data, across all slots considered (All open slots, Adjective-only, Numeral-only, and Noun-only).