LEARNING WHAT NOT TO SAY: THE ROLE OF CATEGORIZATION,
STATISTICAL PREEMPTION, AND DISCOUNTING IN A-ADJECTIVES

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ABSTRACT

It has appeared mysterious that speakers are able to learn certain seemingly arbitrary
distributional restrictions. This paper investigates one such case: the fact that speakers resist
using certain adjectives prenominally (e.g., ?? the asleep man). Experiment 1 indicates that
speakers tentatively generalize or categorize the distributional restriction beyond their previous
experience. Experiment 2 demonstrates that speakers are sensitive to statistical preemption; i.e.,
speakers learn not to use a formulation if an alternative formulation with the same function is
consistently witnessed. Finally, Experiment 3 finds evidence that speakers discount a pseudo-
preemptive context, rationally ignoring it as uninformative.

1.1.1.1

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Gibson’s lab at MIT (2009), the linguistics department at Harvard (2009), the AFLiCO
conference in Paris (2009), and a workshop at Freie University in Berlin (2009).
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1. **INTRODUCTION.** How do speakers learn *not* to say certain things? For example, how do speakers learn to avoid using certain adjectives such as *asleep* in prenominal position (1), when the vast majority of English adjectives readily appear in this position (2)?

(1) ??the asleep boy

(2) the insightful/long/tedious/sketchy/award-winning/obscure proposal

The language that people hear does not come overtly marked with question marks or asterisks to indicate unacceptability, and we know that speakers are not generally corrected for producing ill-formed utterances (Baker 1979; Bowerman 1988; 1996; Braine 1971; Brown & Hanlon 1970; Marcus 1993; Pinker 1989). It is thus an open question how it is that learners are able to limit the overgeneralization of productive linguistic patterns. This paper investigates the role that three factors may play in constraining overgeneralization errors: categorization, statistical preemption and discounting. Each is discussed in turn below.

1.1. **CATEGORIZATION: A CLASS OF $A$-ADJECTIVES.** We are constantly encountering new entities and situations that deviate in large and small ways from our previous experience. The next door we encounter may differ from previous doors in being larger or smaller, wooden or windowed, and may require pushing, pulling, or sliding to open. And yet we have no trouble recognizing this new door as a door. Rats recognize new foods arranged in new ways as food to eat; dogs recognize new dogs as dogs to mate with. Categorization is a process whereby related entities are treated alike for certain purposes. It is clearly ubiquitous in humans and throughout the animal kingdom.

We undoubtedly categorize linguistic elements as well. For example, we project that the past tense of a nonsense word *spling* might be *spling* on the basis of *spling*’s phonological
similarity to *sing* and *ring* (Bybee & Moder 1983; Pinker 1999). What is less clear is whether 
people categorize potential *restrictions* on a new item—i.e., whether in the course of learning 
that *spling* inflects for the past tense as *spling* they also learn that competing patterns do not 
apply (e.g., *splung, *spling). We address this question by considering the distributional 
restriction on certain “*a*-adjectives” like *asleep*.

*Asleep* is one member of a class of adjectives beginning with a syllabic schwa (“a”) that 
resist appearing prenominally in attributive position. Other examples include the following:

(3) a. ??the/an afraid child 
   b. ??the/an afloat ship 
   c. ??the/an alive monster 
   d. ??the/an ablaze building 

The restriction is not a general semantic nor general phonological restriction, since near 
synonyms and words with closely related phonology readily appear prenominally. This point is 
discussed in some detail in other work (Goldberg & Boyd, forthcoming), but to get a sense of 
how the pattern is idiosyncratic, note first that the following near synonyms are fully acceptable 
prenominally.

(4) Semantic near-synonyms 
   a. the scared man 
   b. the sleeping child 
   c. the living monster 
   d. the burning building
Furthermore, the class of $a$-adjectives is defined beyond the initial syllabic schwa insofar as each member is morphologically segmentable into $a$- plus a semantically related stem, as in the following examples:

(5) Segmentable into $a + \text{stem}$

a. $a$float: $a + \text{float}$; cf. $\text{float}$

b. $a$live: $a + \text{live}$; cf. $\text{live}$

c. $a$blaze: $a + \text{blaze}$; cf. $\text{blaze}$

d. $a$raid: $a + \text{raid}$; cf. $\text{frighten, fraidy-cat}$

Other adjectives with similar phonology, but which are not segmentable fall outside the $a$-adjective category. For example, $\text{adult}$ is superficially similar to $a$-adjectives in that it consists of $a$- followed by other material, but the candidate stem $/dult/$ is not a recognizable English morpheme, much less one that is semantically related to $\text{adult}$. Along the same lines, while parsing $\text{acute}$ as $a$+cute actually does result in a recognizable English morpheme—cute—this “stem” has nothing whatsoever to do with the meaning of $\text{acute}$. Similarly, the syllables $/stut/$ and $/luf/$ are clearly not morphemes in $\text{astute}$ or $\text{aloof}$. Since these adjectives are not segmentable into $a$- plus a semantically related stem, they are not $a$-adjectives and are thus not subject to the prohibition on prenominal use:

(6) Non-segmentable into $a + \text{stem}$

a. the adult male

b. the acute sickness

c. the astute comment

d. the aloof woman
Additionally, the adjectives in (6) provide further evidence against the hypothesis that that phonology might somehow be solely responsible for *a*-adjectives’ dispreference for prenominal placement. Given that the adjectives in (6) are phonologically quite similar to those in (3), we can conclude that while *a*-adjectives are, in part, defined by the way that they sound, the category does not reduce to phonological characteristics.

We leave aside several interesting questions concerning this pattern, including how it came to be and the issue of segmentability (but see Goldberg & Boyd, in prep.). We focus instead on how speakers learn to avoid using the class of *a*-adjectives prenominally. Since there is no semantic or formal reason to treat the members of the class as anything other than adjectives, their unusual distribution poses a clear learnability challenge.

Experiment 1 aims to investigate the extent to which the *a*-adjective category is productive. Do speakers avoid using novel *a*-adjectives prenominally? If novel *a*-adjectives are treated like familiar *a*-adjectives, it would indicate that speakers have formed a productive category of *a*-adjectives. If not, it would indicate that each items’ restriction must be learned anew without reference to an implicit generalization.

1.2. **Statistical Preemption.** A number of theorists have suggested that a process of preemption plays a role in speakers learning to avoid syntactic overgeneralizations (Foraker et al. 2007; Goldberg 1993; 1995; 2006; Pinker 1981). Preemption is a particular type of indirect negative evidence. It is an implicit inference speakers make from repeatedly hearing a formulation, B, in a context where one might have expected to hear a semantically and pragmatically related alternative formulation, A. The result is that speakers implicitly recognize that B is the appropriate formulation in such a context, and that A is not appropriate.
Morphological preemption (or blocking) is already familiar from morphology: *went* preempts *goed, and* *feet* preempts *foots* (Kiparsky 1982). But preemption between two phrasal forms requires explanation, since expressions formed from distinct phrasal constructions are virtually never semantically and pragmatically identical, and thus it is not clear that an instance of one phrasal pattern could preempt the use of another (Bowerman 1996; Pinker 1989). For example, the ditransitive construction in (7a) is distinct, at least in terms of its information structure, from the prepositional paraphrase (7b) (Goldberg 1995; Hovav & Levin 2005). Thus knowledge that the prepositional paraphrase is licensed as in (7b) should not in any simple way preempt the use of the ditransitive (7a). And in fact, a large number of verbs do freely appear in both constructions (e.g., *tell* as in 8a-b).

(7) a. *She explained me the story.*

   b. She explained the story to me.

(8) a. She told me the story.

   b. She told the story to me.

Still, preemption could play an important role in learning to avoid expressions such as (7a), once a speaker’s expectations are taken into account in the following way. Learners may witness repeated situations in which the ditransitive might be expected because the relevant information structure suits the ditransitive at least as well as the prepositional paraphrase. If, in these situations, the prepositional alternative is systematically witnessed instead, the learner can infer that the ditransitive is not after all appropriate (Goldberg 1993; 1995; 2006; Marcotte 2005).

As Goldberg (2006) emphasizes, the process is necessarily statistical, because a single use of the alternative formulation could of course be due to some subtle difference in the
functions of the two formulations that favors formulation B—such differences almost always exist. Or a single use may simply be due to an error by the speaker. But if an alternative formulation is consistently heard, a process of statistical preemption predicts that speakers will learn to use the alternative.

Statistical preemption has not received a great deal of attention in the experimental literature, except in notable work by Brooks and colleagues. Brooks has demonstrated that seeing novel intransitive verbs in periphrastic causative constructions significantly preempts children’s use of them in simple transitives (Brooks & Zizak 2002; Brooks & Tomasello 1999). For example, Brooks and Tomasello (1999) found that children aged six and seven were less than half as likely to productively use a novel verb causatively when the verb had been modeled in both the intransitive and in a periphrastic causative, than when it was only modeled in the intransitive form. Thus if the child had heard both *The cow is chamming* (intransitive), and *Ernie’s making the cow cham* (periphrastic causative), they were less likely to respond to “what did Elmo do to the cow?” with *Ernie chammed the cow* (causative), than they were if only the intransitive construction had been witnessed. Hearing the novel verb used in the periphrastic causative construction provided a readily available alternative to the causative construction, statistically preempting the use of the latter.

Experiment 2 investigates statistical preemption in the case of *a*-adjectives and additionally asks whether the preemptive context is itself generalized. That is, if a certain novel *a*-adjective is heard used in a preemptive context, do speakers implicitly extend the restriction to other novel *a*-adjectives witnessed in the same experimental setting? We consider this question by exposing participants to novel *a*-adjectives in relative clause (RC) structures as in (9), on the assumption that this will preempt their usage in attributive structures like (10):
(9) The pig that was ablim moved to the star.

(10) The ablim pig moved to the star.

1.3. DISCOUNTING. Recognizing a role for statistical preemption raises an important question. How savvy are speakers in determining whether a given utterance should qualify as a preemptive context? If the particular formulation used can be attributed to some other factor, do speakers readily discount that context, essentially recognizing that the formulation is irrelevant to the potential alternative?

In Experiment 3, we expose participants to a pseudo-preemptive context: that is, a context in which the novel a-adjective appears in an alternative construction (an RC), but the alternative is attributable to some other aspect of the utterance. In particular, subjects witnessed the novel a-adjective conjoined with a complex adjective in an RC as in (11). Notice that complex adjectives such as proud of itself never appear prenominally, so that a relative clause formulation is the only option. It does not depend on the other conjoined adjective (cf. 12 and 13). Are speakers sensitive to the fact that (11) therefore represents a pseudo-preemptive context?

(11) The fox that was ablim and proud of itself…

(12) *The proud of itself fox…

(13) *The red and proud of itself fox…

1.4. EXPERIMENTS. We report three studies with adult native English speakers that were designed to elicit adjectival productions in a naturalistic context. Participants were given the opportunity to use different adjectives either prenominally in an attributive construction, or post-nominally in an RC construction. The basic protocol asked
participants to view scenes in which one of two animals moved across a computer screen, then produce a description of what they saw. All animals were labeled onscreen with a word; on critical trials the words were always adjectives. This means, for example, that if a critical trial showed two cows (see Figure 1a)—one labeled with *vigilant* and the other labeled with *asleep*—and the cow with the *asleep* label moved towards an onscreen star, then participants should produce either an attributive construction like *The asleep cow moved to the star*, or an RC construction like *The cow that’s asleep moved to the star*. Crucially, the choice of which construction to use was entirely up to the participants.

2. **METHODS AND MATERIALS FOR EXPERIMENTS 1, 2, AND 3.** All of the three experiments made use of similar materials and methods, and had the same structure. Participants began by watching the experimenter work through a number of trials in an exposure block, then completed a series of trials themselves in a production block. The production block was identical across experiments; we manipulated the contents of the exposure block, however, as a means of controlling participants’ experience with novel *a*-adjectives like *ablim* and *adax*. In Experiment 1, the exposure block was designed to provide no experience with novel *a*-adjectives. Speakers’ behavior in the production block thus reflected their baseline production preferences. In Experiment 2, the exposure block contained examples like (9), with novel *a*-adjectives in RC structures. We hypothesized that speakers would treat these sorts of sentences as implicit negative evidence and would therefore reduce their use *a*-adjectives in attributive position later in the production block. In Experiment 3, the exposure block contained sentences like (11) that were potentially pseudo-preemptive. If speakers rationally consider the nature of the evidence available to them, these sentences should fail to cause attributive avoidance for novel *a*-adjectives.
2.1. MATERIALS. Figure 1 gives examples of four different critical trials from the production block used in Experiments 1-3. Participants viewed slides in which one animal from a pair moved towards an onscreen star. The type of animal was identified in a sentence at the bottom of each slide, and the individuals in each pair were differentiated through the use of descriptive labels. For example, Figure 1A is a slide that depicts an event involving two cows in which the cow associated with the label vigilant (the foil label) remains stationary, while the cow associated with the label asleep (the target label) moves towards the star. After viewing an event of this sort, participants would read a centrally-presented question on the following slide that prompted them to describe what they had just seen—e.g., Which cow moved to the star?. Participants typically responded with the target label embedded in either an attributive structure or an RC structure.

2.1.1. CRITICAL TRIALS. The labels printed under animals were always adjectives in critical trials. We manipulated two characteristics of target adjectives by factorially crossing adjective class (a- vs. non-a), and adjective novelty (familiar vs. novel). This resulted in the four experimental conditions represented in the different panels of Figure 1. We additionally controlled for semantic differences between familiar a- and non-a adjectives by choosing non-a adjectives that were near synonyms of the a-adjectives. Asleep (A) and sleepy (B) in Figure 1 illustrate this relationship. There were four target labels in each of the four conditions, resulting in a total of 16 critical trials. Table 1 lists all target labels used in critical trials.³

³ Note that all novel non-a adjectives ended in -y. This was done in order to facilitate the recognition of these novel labels as adjectives (rather than verbs), since -y is a common adjectival ending in English. The same technique could not be applied to novel a-adjectives, however. We wanted this set to be phonologically similar to existing a-adjectives, and since -y does not occur on existing a-adjectives, its use on items in the novel a-adjective condition was
precluded. This difference—the fact that all novel non-\(a\) adjectives ended in -\(y\) while no novel \(a\)-adjectives did—could be viewed as a potential confound if it turned out that novel \(a\) and non-\(a\) adjectives patterned differently with respect to their likelihood of being used attributively. If, for example, novel \(a\)-adjectives appeared in attributive position less often, then one could say that this had nothing to do with their \(a\)-adjective status, but instead resulted from their lack of a -\(y\) ending, which made their lexical category status less transparent and consequently increased the likelihood of them being used as verbs.

There are two sources of evidence that rule out this possibility though. First, in pilot work that included novel non-\(a\) adjectives that did not end in -\(y\) (e.g., breem) as well as those that did, we found no difference in rates of attributive usage based on the presence of a -\(y\) ending, \(t(31) = 1.49, p = 0.15\). Second, if participants really had been more likely to think of novel \(a\)-adjectives as verbs and thus to use them as verbs in an RC structure, then they should have added the verbal third-person singular inflection, as in *The cow that ablimes moved to the star*. That this never occurred—either in pilot data, or in any of the data reported below for Experiments 1-3—suggests that participants failed to respond to the presence or absence of -\(y\) endings. This cleared the way for the adjective class manipulation to proceed without fear of confounding.
Figure 1. Critical trials involved describing scenes like those above, where the label associated with the moving animal (the target label) was either a familiar $a$-adjective (A), a familiar non-$a$ adjective (B), a novel $a$-adjective (C), or a novel non-$a$ adjective (D).

Table 1

<table>
<thead>
<tr>
<th>Critical target adjective labels</th>
<th>Familiar</th>
<th>Novel</th>
</tr>
</thead>
<tbody>
<tr>
<td>$A$</td>
<td>Non-$A$</td>
<td>$A$</td>
</tr>
<tr>
<td>afloat</td>
<td>floating</td>
<td>ablim</td>
</tr>
<tr>
<td>afraid</td>
<td>frightened</td>
<td>adax</td>
</tr>
<tr>
<td>alive</td>
<td>living</td>
<td>afraz</td>
</tr>
<tr>
<td>asleep</td>
<td>sleepy</td>
<td>agask</td>
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</table>

2.1.2. Filler Trials. An additional 16 filler trials were interleaved with critical trials. The only difference between the two trial types is that whereas critical trials were designed to leave the choice of which structure to produce up to participants, filler trials were designed to promote the production of either RC structures, or attributive structures. Half of all fillers encouraged RC responses by using third-person singular verbs as target labels—e.g., *smokes, runs, snowboards* (see Figure 2A). Since participants were required to use exact labels in formulating their sentences (see Section 2.3, Procedure), filler trials with verb labels invariably resulted in the production of RC structures. The remaining filler trials encouraged the production of attributive structures by using high frequency adjectives that commonly occur in attributive position as target labels—e.g., *fast, old, strong* (see Figure 2B).

Figure 2. Filler trials used either third-person singular verb labels to elicit an RC response (A), or high frequency adjective labels to elicit an attributive response (B).
Filler trials served to encourage response variability and to guard against strategic responding. Given, for example, that half of the filler trials favored RC responses and the other half favored attributive responses, it was unlikely that a participant would be able to respond using one structure or the other throughout the course of the experiment. Instead, fillers served to increase the probability that participants would flexibly choose between the two structures on a trial-by-trial basis.

The type of filler that preceded a critical trial was counterbalanced across each condition to ensure that fillers did not unduly influence participants’ responses—i.e., half of all critical trials in any condition were preceded by fillers that were likely to elicit attributive responses, while the other half were preceded by fillers likely to elicit RC responses.

The common production block of Experiments 1-3 was formed by interleaving 16 filler trials with 16 critical trials.

2.1.3 Exposure Trials. Immediately prior to the production block, each participant paid attention while the experimenter worked through 12 exposure trials. These trials had the same structure as critical and filler trials—i.e., one of two labeled animals performed an action—and were designed to give participants different kinds of exposure to \(a\)-adjectives under the guise of having the experimenter demonstrate the experimental procedure to them.

These trials differed systematically in the three experiments as described below. As a means of ensuring that the exposure trials did not unduly influence participants to favor the use of one type of event description over another, the experimenter modeled attributive structures in half of the trials, and RC structures in the other half.
2.2. SLIDESHOWS. As a means of guarding against order effects, participants were randomly assigned to view one of four slideshows in which critical, filler, and exposure trials were presented in different pseudo-random orders. Each slideshow consisted of a block of 12 exposure trials (the exposure block) followed by a block of 16 critical trials plus 16 fillers (the production block). Exposure blocks had no more than two trials in a row in which \( a\)-adjectives were targets. Production blocks began with a filler trial, and alternated between critical and filler trials thereafter. Additionally, there were no more than two fillers of the same type (i.e., adjective labels vs. verb labels) in a row, there were no more than two critical trials from the same condition in a row, and there were no more than two trials featuring \( a\)-adjectives in a row. Each slideshow was balanced so that half of the target labels appeared on the left, and half on the right.

2.3. PROCEDURE. Each experiment started with the same instructions. Participants were briefed on the experimental procedure, and were told that in formulating their descriptions they must make use of the exact label associated with the target animal. This ensured that filler trials that were intended to result in RC responses actually did. For example, given the stimuli in Figure 2A, the most straightforward description that obeyed the exact-label constraint would be *The owl that smokes moved to the star*; changing the label was disallowed (e.g., *The smoking owl moved to the star*). The use of exact labels was emphasized in two practice trials with feedback: one in which an RC structure was produced, and one in which an attributive structure was produced.

Participants were also instructed that some trials might make use of words that they had never seen before. On these trials they were encouraged to avoid awkward utterances, and to instead formulate a description that sounded like something that a native speaker of English might say. As a means of demonstrating this principle, the experimenter then worked through the exposure block while the participant observed. This involved the experimenter reading the labels
on each slide aloud, then watching to see which animal moved, then formulating an utterance that described what happened.

At the end of the exposure block the experimenter asked participants if they had any questions about the procedure, then started the production block. As in the exposure block, the experimenter read the labels on each trial aloud. This ensured that participants would know the correct pronunciation for novel adjectives, which was especially important for novel *a*-adjectives, since class membership is in part contingent on phonological form (see Section 1.1). In contrast to the exposure block, however, participants formulated their own descriptions on each trial.

Each participant was debriefed at the end of their test session in order to identify any individuals who may have guessed the purpose of the experiment. Participants were asked whether they had noticed that certain types of adjectives in the experiment seemed to work well with some sentence types but not others. Participants whose answers revealed explicit knowledge of the constraint against *a*-adjective use in attributive structures had their data excluded from consideration.

Audio recordings were made of each session in order to facilitate later coding and analysis. Utterances in all critical trials were coded as either attributive responses (e.g., *The asleep cow moved to the star*), or RC responses (e.g., *The cow that’s asleep moved to the star*). If participants initially responded with one structure but then changed to another—which happened on only a handful of trials—their final response was coded. There were no responses that did not fit clearly into one coding category or the other.
The dependent measure for all analyses reported below was the proportion of attributive responses. This was calculated for each condition on a by-participants basis by dividing the number of attributive responses by the total number of responses.

3. Experiment 1: Categorization. Experiment 1 investigated speakers’ use of familiar and novel a-adjectives without prior exposure to any adjectives occurring within the experimental setting. We asked two questions. First, do speakers avoid using familiar a-adjectives (e.g., asleep) attributively? Although experimental evidence from speakers in an online production task has never before been brought to bear on this issue, intuitive judgments and corpus data suggest that speakers should, in fact, resist using familiar a-adjectives in attributive position (Goldberg & Boyd, in prep.). We therefore anticipated answering this first question in the affirmative. The second question—and the one that we were primarily interested in—asks whether speakers might also show a dispreference for using novel a-adjectives (e.g., ablim) attributively. If novel a-adjectives avoid attributive use in the same way that real a-adjectives are predicted to, then this would constitute evidence of generalization, which would suggest that speakers’ grammars include an abstract a-adjective category that disallows the attributive use of any category member. If, on the other hand, novel a-adjectives fail to pattern like familiar a-adjectives, then this would indicate that speakers’ representations of a-adjectives are predominantly item-based. That is, it would suggest that the prohibition on attributive a-adjective use is learned on an adjective-by-adjective basis, and does not generalize to new items.
3.1. PARTICIPANTS. Twenty adult native speakers of English were recruited from the Princeton Department of Psychology subject pool, and participated in Experiment 1 in exchange for course credit.

3.2. EXPERIMENT 1 EXPOSURE. The exposure block in Experiment 1 consisted of 12 trials, all with target labels that were novel non-\(a\) adjectives that did not occur later in the production block. The lack of overlap between the two blocks was by design. Since no adjectives from the production block were represented in the exposure block, speakers’ responses in the production block could be said to represent a baseline level of performance that could be compared to the results of Experiments 2 and 3, where more active manipulations of the exposure block were planned.

Participants watched as the experimenter used two adjectives three times each in attributive structures, and two different adjectives three times each in RC structures. Since attributive and RC structures were evenly represented, there was nothing in the block overall to suggest that one kind of structure was privileged over the other. Participants could thus reasonably be expected to anticipate that either an attributive or an RC response would be legitimate later on in the production block.

3.3. RESULTS. Responses were registered for 313 critical trials—a recording error led to the loss of seven trials from one participant. Additionally, all of the data from another participant were excluded from analysis because his answers to the debriefing questions revealed explicit knowledge of the prohibition against attributive \(a\)-adjective use. This left us with 297 analyzable datapoints, from which the proportion of attributive responses was calculated according to the
procedure outlined above. Table 2 gives summary means for Experiment 1, as well as for Experiments 2 and 3, which will be discussed in detail in Sections 4 and 5, respectively.

### Table 2

<table>
<thead>
<tr>
<th>Condition</th>
<th>Experiment 1</th>
<th>Experiment 2</th>
<th>Experiment 3</th>
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<tr>
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<tr>
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<td>61.84</td>
<td>23.75</td>
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<tr>
<td>Non-$A$</td>
<td>88.16</td>
<td>93.75</td>
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</table>

An ANOVA model was fit to the Experiment 1 data using adjective class ($a$-adjective vs. non-$a$ adjective), and novelty (familiar vs. novel) as within-participants variables and the proportion of attributive responses as the dependent. The results show a main effect of adjective class, $F(1, 18) = 58.42, p < .0001$, with $a$-adjectives being used significantly less often in attributive position than non-$a$ adjectives (41% vs. 83%), and a main effect of novelty, $F(1, 18) = 24.53, p < .001$, with familiar adjectives being used significantly less often in attributive position than novel adjectives (49% vs. 75%). Additionally, there was a significant interaction of adjective class and novelty, $F(1, 18) = 6.85, p = .017$, indicating that the effect of adjective class differed depending on whether the adjective was familiar or novel.

As a means of following up on the interaction effect, we conducted a series of pairwise comparisons among the four experimental conditions. These demonstrated three important characteristics of the dataset. First, as expected based on intuition and corpus results reported elsewhere (Goldberg & Boyd, in prep.), familiar $a$-adjectives are used in attributive position significantly less often than familiar non-$a$ adjectives, $t(18) = 7.13, p < .0001$. Second, familiar $a$-adjectives are also used in attributive position significantly less often than novel $a$-adjectives,
\( t(18) = 4.92, p < .001 \). This difference is what drives the interaction effect, and would seem to suggest that novel \( a \)-adjectives do not pattern like real \( a \)-adjectives. But such a conclusion is mitigated by the third finding from the pairwise comparisons: novel \( a \)-adjectives are significantly less likely to appear attributively than both novel non-\( a \) adjectives, \( t(18) = 3.20, p < .01 \), and familiar non-\( a \) adjectives, one-tailed \( t(18) = 1.94, p < .05 \). This outcome suggests that the novel \( a \)-adjectives of Experiment 1 did pattern like real \( a \)-adjectives to some extent: regardless of novelty, all \( a \)-adjectives resisted attributive use relative to their non-\( a \) counterparts. This resistance was, however, more pronounced among familiar \( a \)-adjectives than among novel \( a \)-adjectives.

### 3.4. Experiment 1 Discussion.

The hypothesis that adjectives like \emph{afloat}, \emph{asleep} and \emph{afraid} resist attributive placement based solely only item-specific knowledge predicts that speakers should avoid using these adjectives attributively, but that they should show no such dispreference for \( a \)-adjectives that they have never heard before. The fact, however, that participants in Experiment 1 were significantly less likely to use both familiar \emph{and} novel \( a \)-adjectives attributively relative to their non-\( a \) counterparts suggests generalization and categorization.

While a dispreference for the attributive use of novel \( a \)-adjectives suggests that they were treated as members of an abstract \( a \)-adjective category, such categorization appears to have been tentative. That is, novel \( a \)-adjectives patterned differently than familiar \( a \)-adjectives with respect to the degree of dispreference shown. Speakers only subtly dispreferred the attributive use of novel \( a \)-adjectives, whereas they overwhelmingly dispreferred using familiar \( a \)-adjectives attributively. What accounts for this difference in behavior? One possibility is that membership in the \( a \)-adjective category is gradient, and the degree of dispreference that an adjective shows
for attributive use is directly proportional to the degree to which it is viewed as an $a$-adjective. On this analysis, novel $a$-adjectives in Experiment 1 may have been viewed as $a$-adjectives on the basis of phonology—they all consisted of a schwa followed by a stressed syllable—but not necessarily on the basis of morphology, where the segmentation suggested in (5) would have resulted in unrecognizable “stems” like blim and fraz (cf. ablim and afraz). This hypothesis predicts that making these stems recognizable—arranging for prior exposure that assigns them fixed meanings that are semantically related to those of ablim and afraz—should make them less likely to be used attributively relative to a control condition where no exposure is given. This prediction has thus far been borne out in other experimental work (Goldberg & Boyd, in prep.).

Determining what sorts of criteria speakers use to judge $a$-adjective category membership is, however, orthogonal to our main goal. Regardless of how the category is defined in the minds of adult speakers, it is still unclear how children might learn to avoid using alive, asleep and afraid attributively. In general, children do not receive explicit negative feedback in a consistent manner, so it is highly unlikely that they are overtly corrected in this case with “Don’t say the asleep man; please say, the man who was asleep!” (Baker 1979; Pinker 1989). Given the lack of explicit negative evidence telling children what not to do, how is the distributional restriction learned for familiar $a$-adjectives in the first place?

As discussed in the introduction, we hypothesize that speakers learn the restricted distribution through a process of statistical preemption. This predicts that giving speakers experience with novel $a$-adjectives—experience that demonstrates their consistent predicative use in RC structures and not in attributive structures—will cause them to avoid using novel $a$-adjectives in attributively. In particular, participants who see words like adax and ablim modeled in RC structures but not attributive structures should begin to treat them like existing $a$-
adjectives, since now the input suggests that *adax, ablim, aghast* and *asleep* all disprefer attributive use.

**4. Experiment 2: Statistical Preemption.** In order to determine whether seeing novel *a*-adjectives used in RC structures causes them to pattern more similarly to existing *a*-adjectives, a new set of participants was run on a slightly modified version of Experiment 1. As before, participants described slides that showed one of two animals moving towards a star. This time however, during the exposure block, half of the experiment’s novel *a*-adjectives were modeled for participants in RC structures. According to the hypothesis outlined above, this should lead to reduced rates of attributive production for novel *a*-adjectives, since speakers would have indirect negative evidence against using them attributively.

Experiment 2 additionally provides an alternative method for determining whether the prohibition against attributive *a*-adjective use is item- or category-based. Since only half of the experiment’s novel *a*-adjectives are experienced in a preemptive RC context, this opens up the possibility of differential patterning based on exposure: novel *a*-adjectives that are represented in the exposure block (the *exposure* condition) might show greater attributive resistance than those that are not (the *no exposure* condition). This outcome would be consistent with the hypothesis that *a*-adjective distributions are learned on an item-by-item basis. On the other hand, if all novel *a*-adjectives show increased attributive avoidance and there is no effect of exposure, then this would constitute evidence in favor of an *a*-adjective category. The only way, for instance, that exposure to *ablim* might affect how one uses *adax* is if the two items have shared representations at a more abstract level. This sort of outcome would corroborate the interpretation from Experiment 1 that reduced attributive use for novel *a*-adjectives relative to novel non-*a* adjectives signals the presence of an *a*-adjective category.
4.1. PARTICIPANTS. A new set of 20 adult native speakers of English was recruited from the same source as Experiment 1. All participants took part in exchange for course credit.

4.2. EXPERIMENT 2 EXPOSURE. Recall that the exposure block in Experiment 1 featured four novel non-\textit{a} adjectives that did not appear later in the production block. Two of these were modeled by the experimenter three times each in attributive structures, and two were modeled three times each in RC structures. The innovation in Experiment 2 was to replace the last two items with two novel \textit{a}-adjectives that would crucially appear later in the production block as target labels in critical trials. This provided participants with brief, potentially preemptive RC exposure to some novel \textit{a}-adjectives under the guise of illustrating how the task was to be performed.

All other aspects of the materials and procedure were identical to Experiment 1.

4.3. RESULTS. Responses were recorded for 320 critical trials. Since no participant indicated any explicit knowledge of the correlation between membership in the \textit{a}-adjective class and the avoidance of attributive placement, all 320 trials were analyzed below. Summary means for the four main experimental conditions are given in Table 2.

We first considered whether there was an effect of exposure on the use of novel \textit{a}-adjectives: Did novel \textit{a}-adjectives that were modeled for participants pattern differently than those that were not? Condition means (exposure vs. no exposure) were calculated on a by-participant basis, and were compared using a \textit{t}-test. The results show that while items in the exposure condition were produced attributively at numerically lower rates than items in the no exposure condition, the difference between condition means (20\% vs. 28\%) was not significant, \( t(19) = 1.00, p = 0.33 \).
Since novel *a*-adjectives appeared to be used in attributive structures at similar rates, regardless of whether participants had prior preemptive exposure to them, we combined all novel *a*-adjectives into one group for the subsequent analysis, which replicated Experiment 1’s adjective class-by-novelty ANOVA. As in Experiment 1, the results show a significant main effect of adjective class, $F(1, 19) = 279.37, p < .0001$, with *a*-adjectives roundly dispreferring attributive placement compared to non-*a* adjectives (19% vs. 92%). In contrast to Experiment 1, however, there was no effect of novelty, $F(1, 19) = 1.89, p = .19$, nor was there a significant interaction of adjective class and novelty, $F(1, 19) = 0.52, p = .48$. Instead, novel *a*-adjectives patterned very similarly to familiar *a*-adjectives: both groups uniformly avoided attributive use.

Pairwise comparisons among the Experiment 2 condition means give further credence to this conclusion. As suggested by the main effect of adjective class and lack of interaction, both familiar and novel *a*-adjectives showed significant avoidance of attributive placement relative to their non-*a* counterparts (familiar *a*-adjectives vs. familiar non-*a* adjectives: $t(19) = 13.66, p < .0001$; novel *a*-adjectives vs. novel non-*a* adjectives: $t(19) = 10.47, p < .0001$). And in a direct comparison between the behavior of novel *a*-adjectives and familiar *a*-adjectives, no significant difference was detected, $t(19) = 1.25, p = .23$. Surprisingly, this outcome held even when only items from the no exposure condition were compared to familiar *a*-adjectives, $t(19) = 1.53, p = .14$. These results suggests that the experience that participants gained with just two novel *a*-adjectives in the exposure block generalized to all novel *a*-adjectives in the production block.

While the overall data pattern indicates that novel *a*-adjectives in Experiment 2 were just as likely to avoid attributive use as familiar *a*-adjectives, it does not tell us whether this resulted from manipulation of the contents of the exposure block. In other words, did giving participants preemptive RC exposure to some novel *a*-adjectives cause them to increase avoidance of
attributive use beyond what was seen in Experiment 1? To answer this question, a planned comparison was conducted using a mixed ANOVA with experiment (Experiment 1 vs. Experiment 2) modeled as a between-participants factor, adjective class and novelty as within-participants factors, and the proportion of attributive responses as the dependent variable. The results show main effects of adjective class, $F(1, 37) = 275.12, p < .0001$, and novelty, $F(1, 37) = 20.10, p < .0001$, as well as significant interactions between adjective class and experiment, $F(1, 37) = 19.65, p < .0001$, and novelty and experiment, $F(1, 37) = 7.10, p < .05$. Additionally—as expected if there were a targeted reduction in attributive use for novel $a$-adjectives from Experiment 1 to Experiment 2—there was a marginal three-way interaction between experiment, adjective class, and novelty, $F(1, 37) = 2.95, p = .094$.

![Graphical summary of results for Experiment 1 (A), Experiment 2 (B), and Experiment 3 (C). Error bars indicate SEM.](image)

**Figure 3.** Graphical summary of results for Experiment 1 (A), Experiment 2 (B), and Experiment 3 (C). Error bars indicate SEM.

Visual inspection of the data (compare panels A and B in Figure 3) suggests that the three-way interaction was indeed driven primarily by a decrease in attributive responding in Experiment 2’s novel $a$-adjective condition. This result was verified using a series of Welch (1947) two-sample $t$-tests that compared each of the Experiment 2 condition means to its Experiment 1 counterpart. These confirmed that while there were no significant cross-experiment differences for familiar $a$-adjectives, $t(37) = 0.80, p = .43$, familiar non-$a$ adjectives, $t(27) = 1.67$, ...
$p = .11$, or novel non-$a$ adjectives, $t(27) = 0.85$, $p = .40$, novel $a$-adjectives showed significantly greater resistance to attributive placement in Experiment 2 than Experiment 1, $t(34) = 3.72$, $p < .001$. This strongly suggests that the preemptive RC exposure that was provided to participants in Experiment 2’s exposure block is what drove the reduction in novel $a$-adjective attributive use.

4.4. EXPERIMENT 2 DISCUSSION. The results of Experiment 2 confirm that speakers can learn to avoid using a particular formulation if an alternative formulation is witnessed instead. Moreover, the data indicate that the preemptive context itself need not be witnessed for every relevant item; speakers’ treatment of the two novel $a$-adjectives that they did not witness in a preemptive context was indistinguishable from their treatment of the two novel $a$-adjectives that they had witnessed. This outcome bolsters the argument that speakers avoid attributive $a$-adjective use based on more than just item-specific distributional knowledge. Instead, the fact that they disprefer the attributive use of $a$-adjectives that they have never seen before (Experiment 1), and that exposure to some novel $a$-adjectives is generalized to the rest of the class (Experiment 2), suggests the existence of an $a$-adjective category.

One unexpected outcome from Experiment 2 was that a very small amount of preemptive exposure—seeing just two novel $a$-adjectives in RC structures three times apiece—had very large effects on speakers’ tendency to use novel $a$-adjectives attributively. Presumably when children initially learn the restriction against using alive and asleep attributively it takes more input than this. But children who are learning about the distributions of alive and asleep for the first time differ from the adult speakers of Experiments 1 and 2 in that they are in the process of learning an $a$-adjective category, whereas adults likely already have the category in place. For adults then, learning the distribution of new $a$-adjectives can be very fast: all that is required is that they implicitly recognize words like ablim and adax as $a$-adjectives. Once this is
accomplished, *ablím* and *adax* can inherit the distributional statistics associated with the
category, which leads to rates of attributive use that are indistinguishable from those of more
familiar *a*-adjectives like *alive* and *asleep*. In contrast, children’s learning proceeds more slowly,
in a more piecemeal fashion. They must begin by collecting enough item-based statistics for each
individual *a*-adjective that the common distributional pattern becomes apparent. According to
this account, it is only after an *a*-adjective category has formed that preemptive exposure would
be able to work quickly, through the category. Prior to this, preemption would only occur at an
item-specific level. That is, seeing *ablím* used in an RC would count as evidence against using
*ablím* attributively, but not necessarily as evidence against using *adax* attributively.

It thus seems that statistical preemption—especially when combined with a preexisting
category—is a powerful process. Indeed, one might be concerned that it appears to be *too*
powerful. As noted in the initial discussion, words can typically appear in more than one phrasal
construction. Witnessing a word repeatedly in one construction should not necessarily preempt
its use in an alternative construction. In order for statistical preemption to work effectively then,
speakers should not be too quick to judge a context as preemptive. A preempting context would
need to be at least equally consistent with the preempted formulation as with the alternative. But
*are* speakers good at identifying truly preemptive contexts and ignoring or *discounting* pseudo-
preemptive contexts? We set out to address this question in Experiment 3.

5. **Experiment 3: Discounting.** Participants in Experiment 2 witnessed certain novel *a*-
adjectives in a preemptive construction—a restrictive RC—during the exposure block of the
experiment. In the context of the experiment, it was clear that either the attributive construction
or the RC construction would serve as a good descriptor to identify one of the two animals in the
scene. Since the constructions’ functions in the context of the experiment were largely the same,
choosing one was essentially choosing not to use the other. This is exactly when statistical
preemption is supposed to apply.

But attributive and RC constructions are not always interchangeable—as noted at the
outset, different constructions almost always have subtly different functions. What if participants
were to witness novel a-adjecitives in an RC structure, but in a context in which the attributive
use would not be felicitous for some reason? In Experiment 3, we set out to investigate just this
situation.

5.1. PARTICIPANTS. A new set 21 adult native speakers of English took part in Experiment 3. As
for Experiments 1 and 2, these individuals were recruited from the Princeton Department of
Psychology subject pool, and participated in Experiment 3 in exchange for course credit.

5.2. EXPERIMENT 3 EXPOSURE. Recall that the exposure block in Experiment 2 contained six
trials in which two novel a-adjecitives appeared three times each in RC structures. Experiment 3
differed subtly from Experiment 2 in that these same trials were revised to involve labels in
which a novel a-adjecitive was conjoined with a complex modifier composed of an adjective
followed by a prepositional complement (e.g., ablim and proud of himself). The experimenter
used these longer labels in the same way that the novel a-adjecitive labels of Experiment 2’s
exposure block were used: they were incorporated into RC structures that described events of
animal motion, as in The hamster that’s ablim and proud of himself moved to the star. All other
aspects of the materials and procedure were identical to Experiments 1 and 2.

Note that participants who are exposed to novel a-adjecitives in this context might
respond in a number of ways in the production block. First, it is possible that simply seeing
words like ablim or adax in predicative position in an RC structure is enough to trigger
preemption and lead to attributive avoidance. On this account, learners would be insensitive to
other characteristics of the exposure context (e.g., the fact that *ablim* occurs in the conjoined modifier *ablim and proud of himself*), and the results of Experiment 3 should pattern like those of Experiment 2. On the other hand, it may be the case that learners will fail to learn anything about *a*-adjective distributions from this sort of input. Instead, if they attribute the appearance of *ablim* and *adax* in RC structures to constraints on the placement of complex modifiers like *proud of himself*—which cannot appear prenominally—then the presence of novel *a*-adjectives in RC structures should fail to preempt their later attributive use, and the results of Experiment 3 will pattern like those of Experiment 1.

5.3. RESULTS. Responses were collected for 336 critical trials. All of the data from one participant were excluded, however, based on her responses to debriefing questions, which revealed explicit knowledge of the constraint on the attributive use of *a*-adjectives. This left us with 320 analyzable datapoints, from which the proportion of attributive responses was calculated on a by-participants basis. Summary condition means are given in Table 2.

Application of the same adjective class-by-novelty ANOVA used for Experiments 1 and 2 to the Experiment 3 data revealed a significant main effect of adjective class, $F(1, 19) = 69.78$, $p < .0001$, which demonstrates that *a*-adjectives appeared less often in attributive position than non-*a* adjectives (60% vs. 96%), and a significant main effect of novelty, $F(1, 19) = 52.78$, $p < .0001$, where familiar items were less likely to appear attributively than novel items (63% vs. 94%). The analysis also showed a significant interaction, $F(1, 19) = 49.58$, $p < .0001$, which follow-up pairwise comparisons showed was very similar to the Experiment 1 interaction. There was significant attributive avoidance for familiar *a*-adjectives relative to familiar non-*a* adjectives, $t(19) = 9.75$, $p < .0001$, and marginal avoidance for novel *a*-adjectives relative to novel non-*a* adjectives, $t(19) = 2.03$, $p = .057$). As in Experiment 1, however, familiar *a*-
adjectives showed much stronger resistance to attributive placement than novel α-adjectives, $t(19) = 7.67, p < .0001$.

These results are consistent with visual inspection of the Experiment 1 and Experiment 3 data (compare panels A and C of Figure 3), which indicate similar interaction effects, but also perhaps an overall increase in attributive responding from Experiment 1 to Experiment 3. To investigate this possibility, we conducted a planned ANOVA along the same lines as the one reported above in Section 4.3. Experiment (Experiment 1 vs. Experiment 3) was modeled as a between-participants variable, adjective class and novelty were modeled as within-participants variables, and the dependent was the proportion of attributive responses. The results show—as expected based on the overall similar patterning between Experiments 1 and 3—significant effects of adjective class, $F(1, 37) = 125.83, p < .0001$, novelty, $F(1, 37) = 71.96, p < .0001$, and an adjective class by novelty interaction, $F(1, 37) = 36.42, p < .0001$. More importantly, there was also a main effect of experiment, $F(1, 37) = 10.08, p < .01$, and no significant interactions of experiment with other factors—experiment × adjective class: $F(1, 37) = .70, p = .41$; experiment × novelty: $F(1, 37) = .53, p = .47$; experiment × adjective class × novelty: $F(1, 37) = 2.22, p = .14$. This is consistent with a general increase in the rate of attributive use in Experiment 3.

Follow-up Welch (1947) $t$-tests between each condition in Experiment 3 and its Experiment 1 counterpart verified this conclusion. The main effect of experiment was driven by numerical increases in the dependent measure in all four conditions. Two of these increases led to significant cross-experiment differences (novel α-adjectives: $t(29) = 2.86, p < .01$; familiar non-α adjectives, $t(26) = 2.22, p < .05$), one led to a marginal difference (novel non-α adjectives: $t(20) = 1.77, p = .092$), and one led to a non-significant difference (familiar α-adjectives: $t(35) = 1.32, p = .20$).
5.4. **Experiment 3 Discussion.** Experiment 3 demonstrates that speakers apply statistical preemption rationally. They discount the witnessing of alternative formulations if they can attribute the alternative to some other factor. The reasoning involved may seem too complex for young children to apply when they are learning language. However, in a non-linguistic context, even fourteen-month old infants are capable of recognizing and discounting pseudo-preemptive contexts. Gergely et al. (2002) divided 14-month-old infants into two conditions. In one condition they watched an experimenter turn on a light in front of her by using her head instead of his hands. In the second condition, the experimenter also turned on the light with her head, but was simultaneously holding a blanket close to her chest with both hands, feigning chills. The researchers found that when the experimenter’s hands were free, 69% of infants mimicked her head action to turn on the light (a good example of preemption insofar as witnessing the light being turned on with the head preempted their turning it on with the hands). At the same time, only 21% of the infants turned the light on with their head when the experimenter’s hands were occupied. In this case, the infants’ reasoning appeared to be: “if the experimenter were able to use her hands she would have—but they were busy holding the blanket.” Thus, the infants essentially discounted the implication that the light is appropriately turned on with the head, because the use of the head was naturally attributed to an extraneous factor. It seems therefore, that the type of implicit reasoning involved is accessible even to very young children.

The overall increase in attributive uses in Experiment 3 may also be a result of participants’ discounting the RC uses during the exposure block. If participants essentially ignored RC uses, the remaining attributive uses may have taken on correspondingly more salience, resulting in more overall attributive productions via a process of structural priming.
6. General Discussion. The combined results from Experiments 1-3 indicate that both categorization and statistical preemption play a role in restricting linguistic productivity. Participants extended the restriction on appearing in attributive position from familiar *a-*adjectives to novel *a-*adjectives, even without witnessing novel *a-*adjectives in a preemptive context (Experiment 1). When two novel *a-*adjectives *were* witnessed in a preemptive context—an RC construction—attributive uses of all four novel *a-*adjectives dropped to rates indistinguishable from familiar *a-*adjectives (Experiment 2). Finally, Experiment 3 demonstrated that speakers do not apply preemption indiscriminately: they *discounted* witnessing novel *a-*adjectives in an RC structure when RC use was attributable to some other factor—in this case, a conjoined complex modifier. Thus, people are adept at taking advantage of their input in order to categorize and generalize beyond their direct experience. In this way, they learn not only what they can say, but what they cannot say.

Remarkably, the debriefing procedure determined that only a tiny minority of participants were consciously aware of our manipulations. We dropped the few that indicated any awareness from the analysis, so the phenomena reported here indicate implicit learning processes.

A comparison of Experiments 2 and 3 sheds light on the relationship between preemption and a hypothesized role for entrenchment. Several theorists have argued that the process of hearing a pattern with sufficient token frequency—i.e., entrenchment—plays a key role in constraining overgeneralizations (Braine & Brooks 1995; Brooks & Tomasello 1999). For example, Braine and Brooks propose a “unique argument-structure preference” such that once an argument structure pattern has been learned for a particular verb, that argument structure pattern tends to block creative use of the verb in any other argument structure pattern, unless a second pattern is also witnessed in the input. Theakston (2004) demonstrated that children in an
experimental setting were more likely to overgeneralize verbs that did not occur frequently (e.g., to use *vanish* transitively), than to overgeneralize verbs that did occur frequently (e.g., to use *disappear* transitively). The difference in behavior was attributed to differences in token frequency.

The difference between entrenchment and preemption is that only the latter takes function into account. Entrenchment predicts that witnessing *any* alternative use of a particular word should discourage the word from being used in a target construction. Preemption predicts, on the other hand, that only alternative uses that have the *same function* as the target construction serve to discourage or preempt use of the target pattern.

In favor of preemption over entrenchment, Goldberg (2006) has pointed out that verbs that frequently appear in one argument structure pattern can readily be used creatively in new argument structure patterns, without any trace of ill-formedness as in examples (14-16).

(14) She sneezed the foam off the cappuccino.

(15) She danced her way to fame and fortune.

(16) The truck screeched down the street.

The preemptive process, unlike the notion of simple high token frequency, predicts that an expression like (14) would *not* be preempted by the overwhelmingly more frequent use of *sneeze* as a simple intransitive (as in 17) because the expressions would not be used in the same contexts. Their meanings are quite distinct.

(17) She sneezed.

The intriguing Theakston (2004) finding that high frequency verbs are less likely to be overgeneralized than low-frequency verbs is consistent with the idea that it is preemption that prevents overgeneralization, not the frequency of the verb per se. That is, the preemptive context
in which *disappear* might have been expected to occur transitively but instead is witnessed
intransitively (in a periphrastic causative construction) occurs more frequently than the same
preemptive context for *vanish*. That is, frequency plays a role in the process of statistical
preemption exactly because the preemption is statistical. Only upon repeated exposures to one
construction in lieu of another related construction can the learner infer that the second
construction is not conventional. As noted above, this requires that a given pattern occur with
sufficient frequency.

The results of Experiment 2 are equally well interpreted in terms of entrenchment or
preemption, but a comparison of Experiments 2 and 3 comes down decidedly in favor of
preemption as the operative mechanism. Note that two novel *a*-adjectives were witnessed in RCs
three times apiece in both Experiment 2 and Experiment 3. If simple entrenchment were the key
notion, we would expect that these items would continue to be used in RCs to the same extent in
both studies. And yet speakers were more likely to avoid using the novel *a*-adjectives
attributively in Experiment 2 than in Experiment 3. This is presumably because the attributive
form was only preempted in Experiment 2. Thus, Experiment 3 indicates that the relevant
process is statistical preemption and not simple entrenchment.

7. **Conclusion.** Experiment 1 demonstrated that speakers generalize their knowledge of *a-
adjectives in a tentative way. To a limited, but significant extent, they treated novel *a*-adjectives
like familiar *a*-adjectives in restricting their distribution. That is, novel *a*-adjectives (e.g., *adax*)
disprefer prenominal attributive use relative to non-*a* adjectives (e.g., *chammy*). This indicates
that participants tentatively assimilate never-before-seen *a*-adjectives to the category of familiar
*a*-adjectives. Experiment 2 tested to see whether learners were sensitive to statistical preemption,
and found that witnessing a novel *a*-adjective used in a preemptive RC context just three times
dramatically decreased incorrect prenominal uses so that novel uses were indistinguishable from familiar a-adjectives in avoiding prenominal distributions. Moreover, the restriction on novel a-adjectives was extended beyond the two novel items witnessed to other novel a-adjectives that were not witnessed. Finally, Experiment 3 showed that learners wisely disregard pseudo-preemptive input.

Collectively, these experiments go some way toward establishing how speakers are able to learn arbitrary distributional restrictions in their language—i.e., how they learn what not to say. Learners categorize their input. Familiar formulations can statistically preempt other formulations that have roughly the same function; speakers are additionally able to extend the information gleaned from preemptive contexts, applying it to other instances of the same category. At the same time, speakers are impressively adept at ignoring alternative formulations when they can be attributed to some irrelevant factor.
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TODO

1. need title for Goldberg & Boyd, in prep.