Familiarity Plays a Small Role in Noun Comprehension at 12-18 months

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Abstract

12

13 Infants amass thousands of hours of experience with *particular* items, each of which is
14 representative of a broader category that often shares perceptual features. Robust word
15 comprehension requires generalizing known labels to new category members. While young
16 infants have been found to look at common nouns when they are named aloud, the role of
17 *item familiarity* has not been well-examined. This study compares 12-18-month-olds' word
18 comprehension in the context of pairs of their own items (e.g. photos of their own shoe and
19 ball) versus new tokens from the same category (e.g. a new shoe and ball). Our results
20 replicate previous work showing that noun comprehension improves rapidly over the second
21 year, while also suggesting that item familiarity appears to play a far smaller role in
22 comprehension in this age-range. This in turn suggests that even before age two, ready
23 generalization beyond particular experiences is an intrinsic component of lexical development.

24

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Introduction

While young infants do not produce words until around the first birthday (Fenson et al., 1994), they begin to understand words months earlier, particularly for common nouns and salient individuals (Bergelson & Swingley, 2012; Parise & Csibra, 2012; Tincoff & Jusczyk, 1999). One challenge in this process is that word learning is necessarily both specific and general. That is, infants must be able to link their experiences with *particular* referents in the world and the words used to label them, but also properly generalize their knowledge to *new* instances of the word. It's not enough for a learner to realize that their favorite toy is called "ball" when a helpful caretaker unambiguously labels it this way. First of all, the world rarely provides such clear-cut data. But even if it did, learners would still require some sense of the general concept or category to enable proper extension to new instances. For instance, if a baby can pick a ball out of a basket of toys they've never encountered upon hearing "ball", or recognize a ball from a 2-dimensional drawing, this provides evidence that they know "ball" refers to a category of round, rollable toys.

Indeed, behavioral lab studies of word comprehension capitalize on this logic, testing whether young infants look more at a named image when presented with multiple options, in what is often dubbed the "looking-while-listening" task. (Fernald, Pinto, Swingley, Weinberg, & McRoberts, 1998; Fernald, Zangl, Portillo, & Marchman, 2008). Given infants' success in this task from six months onward with both novel images and videos of familiar categories or people (Bergelson & Aslin, 2017a; Bergelson & Swingley, 2012; Tincoff & Jusczyk, 1999, 2012), they must already have some rudimentary skills to conceptualize and categorize word-forms and referents. In the current work we investigate how broad of a category infants have represented, and how their own experience with particular tokens comes into play.¹

¹We focus on meanings, but *mutatis mutandis*, the same issues apply to early phonetic representations.

Relevantly, categorization skills at both a conceptual and perceptual level improve alongside growing linguistic skills over infants' first year of life (Oakes, n.d.). While infant categorization results are strongly influenced by methods and stimuli (Rakison & Yermolayeva, 2010), a growing literature shows that infants' processing of specific tokens can shape aspects of their discrimination, attention, memory, and learning of categories, within and across animate and inanimate classes like people, monkeys, pets, and vehicles (Oakes, n.d.). For instance, by six months, infants treat previously unseen instances of a visual category that are the average of previously seen tokens as familiar (Haan, Johnson, Maurer, & Perrett, 2001), and also remember individual instances (Oakes & Kovack-Lesh, 2013). Furthermore, both longer-term home experience and brief lab-experience influence new category formation (Bornstein & Arterberry, 2010). With common categories like animals and furniture, previous research has shown that over 12-30 months, infants' categorization shifts from more to less inclusive, with a large role for perceptual contrast in explaining behavior (Bornstein & Arterberry, 2010). Finally, while many aspects of categorization are perceptually guided, by age one infants can group objects by unseen conceptual features like function as well (Träuble & Pauen, 2007). On some accounts, words are one such grouping feature (Deng & Sloutsky, 2015). Indeed, whether words are a privileged type of feature is under (at times vociferous) debate (Althaus & Westermann, 2016; Ferguson & Waxman, 2017).

Views on how infants come to properly extend words to new instances largely fall in two camps: narrow-to-broad and broad-to-narrow (Hirsh-Pasek, Golinkoff, Hennon, & McGuire, 2004). On the narrow-to-broad view (Hirsh-Pasek et al., 2004; Smith, 2000), children are believed to first learn that a word is used to label individual objects and then, over time, begin to generalize the word to a category of objects (on either perceptual or other bases). On this view, children construct categories inductively—they begin to group objects together based on similarities they notice over time, eventually having the second order insight that X's are X-shaped for many concrete nouns X (Smith, Jones, Landau,

Gershkoff-Stowe, & Samuelson, 2002). Conversely, the broad-to-narrow view of word learning suggests that infants are biased to understand a noun as referring to a category of like objects, before later understanding more fine-tuned, specific references (Csibra & Shamsudheen, 2015; Waxman & Markow, 1995). However, much of this work has used novel words and objects, leaving a bit unclear how these processes unfold at a real-life scale, i.e. during infants' daily learning experiences with common words and referents.

Muddying the waters further, language can of course label individuals or categories, often leaving a grammatical signature of this distinction. For instance, English syntax generally separates proper nouns (for which there is no broader category) and common nouns by adding articles for the latter ("here's Sarah" vs. "here's *a* ball"). However, early on, caretaker labels like "mommy" and "daddy" are proper-noun-like in their extension ("Here's Mommy!"). Indeed, for six-month-olds, these words refer to their mother and father and not other women and men (Tincoff & Jusczyk, 1999). Moreover, infants' early productive vocabulary contains both nouns naming individuals (e.g. "mommy") alongside count nouns like "dog" (Fenson et al., 1994). Building on this, exciting recent evidence suggests infants know both individual and category-level nouns from the onset of word comprehension around six months (Campbell, 2018), supporting a "narrow-*and*-broad" view.

The current investigation extends these lines of research to further explore the effect of infants' linguistic and physical environments on their noun and object knowledge, and how this may change across development. We ask whether infants understand words better when tested with images of the concrete nouns they see in their own daily lives, as opposed to generic images of these same words. We look at this between 12 and 18 months, a particularly rich developmental period when the productive vocabulary is beginning to grow, comprehension rapidly improves, and infants' ability to discriminate instances of common categories is well-established (Bergelson & Swingley, 2015; Fenson et al., 1994; Oakes, n.d.).

Using the looking-while-listening spoken word comprehension task (Fernald et al.,

2008), we measure whether infants’ performance varies as a function of whether the images they see are of their own objects from home (*specific* items), or generic (i.e. typical but novel) exemplars of the same category (*generic* items). While visual-processing and categorization have been shown to vary as a function of home experience with objects or pets by 5-6 months (Bornstein & Arterberry, 2010; Hurley, Kovack-Lesh, & Oakes, 2010), this has not, to our knowledge, been tested over a set of common nouns during spoken word comprehension (cf. Campbell, 2018, which we return to in the discussion).

Given that previous word comprehension studies using this method with generic images find that word comprehension is relatively robust at 12-18 months (Bergelson & Swingley, 2012, 2015; Swingley, Pinto, & Fernald, 1999), we predict infants will readily understand words in the generic trial-type, looking to the named image significantly more than the distractor. Whether they understand words equivalently or even better when shown familiar images (i.e. in the specific trial-type) is the open question we test here; we have no reason to think performance would be *worse* in the specific trial-type based on the previous literature. If understanding a word—as measured in this task—is tightly linked to infants’ own experiences with referents in their daily life, we’d predict better performance on specific items over generic ones. In contrast, if understanding a word entails recognizing any reasonable instance, whether new or familiar, we’d predict equivalent and above-chance performance across both generic and specific trial-types.

Equivalent performance on both trial-types would be more consistent with either a broad-to-narrow view of word learning, or a narrow-*and*-broad view (cf. Campbell, 2018), while better performance on the specific images than the generic images would better align with the narrow-to-broad view, suggesting that infants’ (initial) understanding of the referential nature of words is tied to their specific learning instances. Finally, performance across and/or within trial-types may vary with age; we test a cross-sectional sample to address this. Given that word comprehension improves over infancy, with particular boosts

in comprehension using the present method just after the first birthday (Bergelson & Swingley, 2012, 2013, 2015), here we investigate whether this improvement may be due to better categorization and generalization skills interacting with linguistic development.

Thus, the present study asks two questions: 1) do infants understand words better when tested with images of their own items versus generic ones and 2) does comprehension in the context of such specific or generic images change as a function of age over 12-18 months.

Methods

All audio and visual stimuli, data, and code are on OSF for blinded peer review.
https://osf.io/pb2g6/?view_only=eaa80f8c114f47df9cbaf82b71e60680.

Participants

Our final sample included 30 12-20-month-olds (Range: 11.56-18.23 months, $M = 14.78$, 13 girls). An additional 5 infants were excluded due to fussiness, poor calibration, or parental interference resulting in useable data for <50% of trials (see trial exclusion criteria below). Infants were recruited from a metropolitan area in the southeast United States by mail, phone, e-mail, and in person, in a protocol approved by University IRB that is in accordance with the provisions of the Declaration of Helsinki. Written consent was provided by parents or guardians on behalf of every child. All of the participants were healthy, full-term, had no history of vision or hearing loss, and heard English at least 75% of the time. Based on demographic forms completed by families, the current sample primarily came from upper-middle class homes; 86.67% of mothers completed a bachelor's degree or higher, 43.33% of mothers worked 40 hours/week or more, 50% had a family income of >\$100k/year, and the majority were White.

Materials

	book	doggy	ball	shoe	blocks
Specific Items
Generic Items

Figure 1. Examples of specific and generic items tested. Parents provided photos based on a list of 26 ‘suggested objects’. Samples of the five most frequently received and tested object are shown above. Images of all items used can be found on OSF, see text for link.

Visual Stimuli. For the *specific* trial-type, parents provided 12 photographs of items their child commonly sees, based on a suggested list of 26 items. Suggested items included toys, clothing, pets, and food-related objects. Corresponding *generic* images were selected from a lab database that is part of the SEEDLingS Corpus (Bergelson, 2016), after ensuring that the lab’s generic image depicted a different object than parents provided. For instance, if the “book” picture from the family and the lab database depicted the same book, a different book image was used. Four warm-up trial images were also displayed, including apple, bottle, spoon, bear, and cat; warm-up items were chosen so that they did not overlap with experimental stimuli. All images were edited onto a plain grey background. See Figure 1.

Item Pairing. We tailored test items individually. Specifically, of the twelve images parents provided, eight items (corresponding to eight target words) were combined into four yoked item-pairs for each infant. Items were chosen based on photo clarity, and two pairing criteria: (1) avoid pairing items with the same initial sound, and (2) avoid creating mixed animacy pairs (e.g. duck-pacifier) given that infants are highly drawn to faces. In two cases it was not possible to avoid overlapping onset (bottle/blanket and block/ball; 1.67% of item-pairs). In 6 cases (5% of item-pairs)) it was necessary to pair animates with inanimates.

Audio Stimuli. Sentences containing each target word were recorded by a female using infant-directed speech. Each sentence was 1.1-1.8 s long and normalized to 72 db using Praat. The sentences were in one of four constructions: “Can you find the X?”, “Do you see the X?”, “Look at the X!”, and “Where is the X?” (X being the target object word). One carrier phrase was used per item-pair. During warm-up trials, sentences played over computer speakers. During test trials, only the parent heard these pre-recorded sentences; infants heard them from their parent (as detailed in Procedure).

Questionnaires. Parents completed the MacArthur-Bates Communicative Development Inventory (CDI) Words & Gestures Form (Fenson et al., 1993) and a Word Exposure Survey, which asked parents to report how often they believed their child heard the eight words tested in the experiment on a five-point scale (1 = “Never”, 5 = “Several times a day”). Parents were also asked to describe infants’ vocalizations and to give examples of words their child could say, if their child had begun to talk. Researchers selected “Saying Words” if parents named five or more words that their child could produce. Finally, parents were asked if their child was breast-fed, bottle-fed, both, or neither, in order to establish whether “milk” would be an appropriate item to display in an image.

Procedure

Infants' word comprehension was tested using an adapted looking-while-listening procedure (Bergelson & Aslin, 2017a; Bergelson & Swingley, 2012). After paperwork was complete, infants and caretakers were escorted to a dimly lit testing room, where infants were seated on their parent's lap in front of a computer monitor (33.7×26.9 cm screen, 1280×1024 resolution). An Eyelink 1000+ Eyetracker camera (SR Research), mounted on a moveable arm and set to "remote" mode, collected infants' visual fixation data, sampling monocularly at 500 Hz (<50° average accuracy), aided by a small, high-contrast sticker on infants' forehead. After calibration, infants were shown four warm-up trials, during which a single image appeared on the screen, while a pre-recorded sentence directed their attention to the object.

After this warm-up, infants would see 32 test trials, half in each trial-type (generic, specific). In each test trial infants saw photos of two objects on a gray background, one on each side of the screen, displayed at 500x500 pixels. Each of the 8 target items were tested on four separate trials, 2 per trial-type (e.g. if ball was a test item, it was the target twice in a generic trial and twice in a specific trial). Parents, who wore headphones and a visor to block their view of the screen, heard test sentences over headphones, which they repeated aloud to the infant when prompted by a beep. The target word occurred ~3-3.5s after the trial began. An experimenter pressed a button to indicate when the parent said the target word, after which the images were shown for 5s. See Figure 2.

Trial order was pseudo-randomized such that neither the same item-pair nor the same carrier phrase appeared back-to-back, and target images did not occur more than twice in a row on the same side of the screen. Additionally, whether an image appeared on the left or right was controlled within item-pairs across trials. As needed, a 2s video including colorful shapes, smiley faces, and a bird-chirping sound, was invoked to maintain infant interest.

Figure 2. Sequence of one test trial: Parents heard test sentences over headphones, followed by a beep. After the beep, they repeated them aloud to their infant. The images were displayed from trial start until 5s after the beginning of the target word (as said by the parent). Representative item-pairs for specific and generic trial images for a single item-pair are also shown below the timeline.

210 Data analysis

211 We used R (Version 3.6.1; R Core Team, 2019)²,

²We, furthermore, used the R-packages *AICcmodavg* (Version 2.2.2; Mazerolle, 2019), *broom* (Version 0.5.2.9002; Robinson & Hayes, 2019), *citr* (Version 0.3.2; Aust, 2019), *dplyr* (Version 0.8.3; Wickham et al., 2019), *forcats* (Version 0.4.0; Wickham, 2019a), *ggplot2* (Version 3.2.1; Wickham, 2016), *kableExtra* (Version 1.1.0; Zhu, 2019), *knitr* (Version 1.25; Xie, 2015), *lme4* (Version 1.1.21; Bates, Mächler, Bolker, & Walker, 2015), *lubridate* (Version 1.7.4; Grolemund & Wickham, 2011), *Matrix* (Version 1.2.17; Bates & Maechler, 2019), *papaja* (Version 0.1.0.9842; Aust & Barth, 2018), *polypoly* (Version 0.0.2; Mahr, 2017), *purrr* (Version 0.3.2; Henry & Wickham, 2019), *readr* (Version 1.3.1; Wickham, Hester, & Francois, 2018), *readxl* (Version 1.3.1; Wickham & Bryan, 2019), *stargazer* (Version 5.2.2; Hlavac, 2018), *stringr* (Version 1.4.0; Wickham,

for all our analyses and will share all code on GitHub upon publication; all scripts and data used to generate this paper are currently on OSF for blinded review (see link at start of Methods section).

Results

We present three sets of analyses below, after providing descriptive statistics from the questionnaire data. First, we examine the proportion of target looking, i.e. our proxy of comprehension, considering the influence of trial-type and age, within a window of interest. Next we compare the target-looking trajectories directly using Growth Curve Analyses. Finally, to explore the possibility that our results are influenced by the similarity between generic and specific images, we incorporate similarity ratings from adults into our eyetracking analysis.

Questionnaire Results

For the CDI, parents reported that infants understood 157.60 words on average (SD=89.96, range=25–335), and produced 25.20 words (SD=35.64, range = 0–137). 66.67% of infants were reported to have said at least five words. For the Word Exposure Survey, where we asked how often parents believed their child heard each of the target words, parents reported that these words were quite common: 82.08% of responses were “Once a Day” or “Several Times a Day”, i.e. a 4 or 5 on a 5 point scale. See Table 1. For the query about word production, convergent with the CDI production results, 70% of infants were reported to have started saying words. 26.67 % of infants were exclusively breastfed.³

2019b), *tibble* (Version 2.1.3; Müller & Wickham, 2019), *tidyr* (Version 1.0.0; Wickham & Henry, 2019), and *tidyverse* (Version 1.2.1; Wickham, 2017).

³In practice, parents sometimes indicated “breast-fed” when infants received breastmilk by bottle. We thus retained the item “milk” regardless of feeding status in the case (n=1) that parents sent us a photo of a bottle of milk as one of the child’s items.

Table 1

Mean word exposure (Range:1-5) and CDI scores by age-half and over all infants

age_half	M_exposure	CDI_U	CDI_Test_U	CDI_P	CDI_Test_P	Age
older	4.52	218.93	4.20	44.27	3.00	497.53(32.11)
younger	4.27	96.27	4.80	6.13	0.67	401.93(32.49)
all Ss	4.39	157.60	4.50	25.20	1.83	449.73(58.06)

Note. U=understood; P=produced; Test=tested items from study (8 per child); Age is in days. Note that 19/87 items we tested across infants were not on the CDI (which has 396 vocabulary items), e.g. giraffe.

Eyetracking Data Preparation and Exclusion

After exporting the eyetracking data from Eyelink, we converted the fixation data into 20ms bins, aligned such that time 0 reflected the time of target word onset (e.g. “ball” in “Can you find the ball?”). For each trial we then computed the proportion of target looking over a 367-2000ms target window of analysis, i.e. from the first eye-movements that may be relevant to the target word (Fernald et al., 1998) to 2s later (as in Swingley & Aslin, 2002).⁴

Our final dataset included 831 trials from 30 infants ($M = 27.70(4.36/32)$, Range:16-32). To arrive at this final dataset, we excluded trials in which parents failed to say the target word (n=4), or in which infants failed to look at either the target or distractor for more than 1/3 of the 367-2000ms window of interest (n=210 low-data trials). This latter

⁴While this proportion is sometimes subtracted from a pre-target baseline to account for saliency differences among images, here such a correction was unnecessary as the proportion of target looking in the pre-target baseline from -2000 to 0ms did not differ from chance (.5) by Wilcoxon Test (pseudomedian= 0.50, p= 0.38; a Wilcoxon Test was used because a Shapiro Test and qqplot indicated that the distribution was highly divergent from a normal one, p= 0.00)

type of trial exclusion generally occurred if infants failed to look at the screen during a given trial, and/or because the experiment was ended early due to infant fussiness/crying. The experiment ended early for 4 infants; no experiment ended before reaching 27/32 trials. Following this trial-level exclusion, we then excluded participants who contributed data to <50% of trials (n=5); these participants accounted for 98 of the 210 low-data trials.

Influence of Age and Image Familiarity on Overall Comprehension

Table 2

Multi-Level Regression Results

<i>Dependent variable:</i>			
Mean Proportion of Target Looking			
	(1)	(2)	(3)
Trial-Type		0.018 (0.023)	0.018 (0.023)
Centered Age	0.029*** (0.010)	0.029*** (0.010)	0.033*** (0.011)
Trial-Type: Age			−0.008 (0.012)
Intercept	0.613*** (0.018)	0.604*** (0.021)	0.604*** (0.022)
Observations	60	60	60
Log Likelihood	40.903	38.359	35.078
Akaike Inf. Crit.	−73.807	−66.717	−58.155
Bayesian Inf. Crit.	−65.430	−56.246	−45.589

Note:

*p<0.1; **p<0.05; ***p<0.01

Random Effects for all models are (1|Subject)

We first conducted a multi-level linear regression testing whether (centered) age and trial-type (specific/generic) accounted for significant variance in the mean proportion of target looking, with a random effect for subject (i.e. mean proportion of target looking $\sim$ age-centered + trial-type + (1|subject)); model residuals were normal by a Shapiro Test ($p=0.82$). We found an age effect, but no effect of trial-type. Comparing with models without trial-type or with an age*trial-type interaction, we found that trial-type did not significantly interact with age ($p = 0.51$) or improve fit above a model with age alone ($p = 0.42$; see Table 3 for model summaries).

Given the highly robust age effect in our data, we were curious to explore whether trial-type differences might emerge in younger vs. older infants. We thus conducted a median-age split (14.69 months), which created two age-groups ($n=15/\text{group}$). All patterns reported above with continuous age hold for age-group (i.e. the older half showed stronger comprehension than the younger half, with no effects of trial-type). See Figures 3 and 4.

Table 3

Proportion of Target Looking by Age-Group & Trial-Type. (comp.=Comparison; chance=.5)

Age Group	Trial-Type	M(SD)	comp. to chance	t-test p-val	# subjs > chance	binomial test p-val
older	Generic	0.664(0.143)	t(14)=4.442	0.001	12/15	0.035
older	Specific	0.661(0.1)	t(14)=6.234	0.000	13/15	0.007
younger	Generic	0.544(0.11)	t(14)=1.545	0.145	9/15	0.607
younger	Specific	0.584(0.122)	t(14)=2.665	0.018	12/15	0.035

Performance on generic vs. specific trials did not vary overall by a paired two-tailed T-test (mean difference = -0.02, $t(29) = -0.80$, $p = 0.43$), or within either age-group (older: mean difference = 0.00, $t(14) = 0.12$, $p = 0.90$); younger: mean difference = -0.04, $t(14) = -1.09$, $p = 0.30$)⁵. Thus, our age-half analysis also finds no statistical support for a trial-type

⁵The distribution of mean proportion of target looking differed slightly from a normal distribution ($p=0.04$); for simplicity we report paired T-tests rather than Wilcoxon Tests but all patterns hold for paired Wilcoxon Tests

Figure 3. Mean proportion of target-looking over the 367-2000ms window of analysis, by age and trial-type (generic vs. specific). Individual infants' trial-type means are shown by each point (older=triangles, younger=circles). Chance looking (.5) is indicated by a dashed horizontal line, median age is repressed by a dotted vertical line. The curve and 95%CI are from a loess fit (i.e. local estimator); since trial-type did not predict performance in any analysis, a single line is fit, though trial-type means for each infant are depicted for data transparency (generic in blue, specific in red)

265 difference (though we note that younger infants in the generic trial-type showed slightly
 266 weaker performance; see Table 3). Binomial tests rendered a consistent pattern: most infants'
 267 means were above chance in both age-groups in the specific trial-type, as were older infants'
 268 in the generic trial-type (see Table 3 for Ns, means, and binomial test results).

269 Growth Curve Analysis

270 While the subject-level aggregated data showed clear age effects but no trial-type
 271 effects, the timecourse data (Fig. 4) highlighted diverging gaze patterns across trial-type
 272 over time, especially in the younger infants. To further explore the online dynamics of
 273 spoken word comprehension in this study, we used growth curve analysis over our 367ms to

Figure 4. Timecourse of mean proportion of target looking (aggregated by infant), split by median-age (14.7m) and trial-type (generic vs. specific). Time 0 is target word onset (e.g. “bottle” in “Where’s the bottle?”). The window of analysis (367-2000ms, as in Swingley & Aslin 2000) is indicated by vertical green lines. Chance looking (.5) is indicated by a dashed horizontal line.

2000ms analysis window. Following previous research, we transformed the data from its raw categorical, bounded state (i.e. a 1 or 0 for target-looking vs. not in any given timebin) to continuous unbounded values using the empirical logit of target looking per subject per trial-type per timebin (cf. Mahr, McMillan, Saffran, Weismer, & Edwards, 2015). This in turn made the standard linear statistics available to us (Barr, 2008).

Given issues with model convergence and singularity, and the unambiguous role of age in our results, we ran separate growth curve models for younger and older infants. This allowed us to focus in on the effect of trial-type in predicting (empirical logit transformed) proportion of target looking across the time-terms. We followed the approach outlined by Mirman (2014) and Mahr et al (2015), and used four orthogonal polynomial time terms,

Figure 5. Growth curve estimates of e(logit)-logit transformed mean proportion of target looking during analysis window, split by median-age (14.7m) and trial-type (generic vs. specific). Line represents model estimates; ribbon represents standard error. Points represent mean value over 20 ms. timebin, infant, and trial-type. N.B. e-logit value of 0 corresponds to a proportions of .5 in the preceding Figure.

examining their interaction with trial-type.⁶ We also included random slopes and intercepts for each subject, and the interaction of Trial-Type with each time term (with generic as the reference condition). Statistical significance for individual parameter estimates was assessed using the normal approximation (cf. Mirman, 2014). As seen in Figure 5, the models provided a reasonably good fit to the data.

For the younger infants, the key results of this analysis were a main effect of trial-type, an interaction of trial-type with the linear, quadratic, and quartic time terms. That is, younger infants performed better in the unique condition, and looking patterns showed divergent average curve height, slope, and inflectional steepness across conditions. More descriptively, infants appear to switch their gaze between target and distractor more for generic trials, leading to a more “wiggly” looking-time curve. For the older infants,

⁶cf. this very useful vignette http://www.eyetracking-r.com/vignettes/growth_curve_analysis

Table 4

Fixed Effects for Growth Curve Model, Younger Infants

term	estimate	std.error	T	df	p.value
(Intercept)	0.19	0.09	2.13	15.20	0.05
TrialTypeSpecific	0.11	0.02	7.01	2,358.54	0.00
time ¹	0.08	0.19	0.42	15.56	0.68
time ²	0.04	0.10	0.39	17.17	0.70
time ³	0.18	0.09	2.10	18.13	0.05
time ⁴	-0.12	0.07	-1.62	20.13	0.12
TrialTypeSpecific:time ¹	-0.14	0.05	-2.61	2,362.43	0.01
TrialTypeSpecific:time ²	-0.44	0.05	-8.33	2,365.87	0.00
TrialTypeSpecific:time ³	-0.01	0.05	-0.25	2,372.83	0.81
TrialTypeSpecific:time ⁴	0.18	0.05	3.53	2,367.53	0.00

Note. Random Effects for all models are (1 + time¹ + time² + time³ + time⁴ | SubjectNumber); see text for details

consistent with the previous analysis collapsing time, the growth curve analysis revealed no main effect of trial-type, though there was a significant trial-type interactions with the linear and cubic time terms, i.e. older infants' looking showed slope and curve shape differences across trial-types. More descriptively, older infants got to the target on unique trials a little faster, and but spent a bit less time there. Thus, considering how eye-movements unfolded over time in this growth curve analysis revealed a role for trial-type, particularly within the younger infants in this sample; the role of trial-type remained modest for the older infants. See Figure 5 and Tables 4 and 5 for model fits and summaries.

Table 5

Fixed Effects for Growth Curve Model, Older Infants

term	estimate	std.error	T	df	p.value
(Intercept)	0.66	0.12	5.66	15.18	0.00
TrialTypeSpecific	0.00	0.02	-0.26	2,362.12	0.79
time ¹	0.57	0.29	1.96	15.24	0.07
time ²	-0.87	0.19	-4.60	15.80	0.00
time ³	0.04	0.12	0.32	16.54	0.75
time ⁴	0.00	0.09	-0.01	18.90	0.99
TrialTypeSpecific:time ¹	-0.63	0.06	-11.01	2,364.02	0.00
TrialTypeSpecific:time ²	0.11	0.06	1.94	2,372.29	0.05
TrialTypeSpecific:time ³	0.31	0.06	5.36	2,373.14	0.00
TrialTypeSpecific:time ⁴	0.04	0.06	0.76	2,370.75	0.45

Note. Random Effects for all models are (1 + time¹ + time² + time³ + time⁴ | SubjectNumber); see text for details

Similarity Ratings

One possible contributor to infants' comprehension is the similarity between the specific and generic images they were tested on. That is, although in a given trial both images that infants saw were either specific or generic (*their* ball and shoe or our generic images for ball and shoe), it's possible that for some infants, the generic items were more similar to their items than for others. To directly test this, we had adult volunteers rate all images that the full sample of infants were tested on. Specifically, adult volunteers were shown pairs consisting of a specific image and a generic image of the same object (e.g. one infant's specific ball and the generic ball image they were tested on) displayed side-by-side on a gray background. For each pair, volunteers were asked to rate the items' similarity on a

7-point Likert scale (1=similar to 7=different). Ratings were obtained through a qualtrics survey. See Table 6.

After removing false starts from the qualtrics data, 20 adults completed the rating task, providing ratings for 5912 pairs (out of 5920; there were 8 missing ratings.) We then removed ratings for items from the 5 infants who were excluded due to contributing insufficient data above, along with two further infants (due to an experimenter error adults also rated items for 2 infants for whom an experiment was prepared but who never came in.) In the final dataset of 5912 rated pairs, the grand mean across similarity ratings was 3.53 (SD = 1.95). Individuals raters' scores were normally distributed around this mean (Range = 1.84 to 5.47; $p=0.69$ by Shapiro Test). Ratings on individuals items (e.g. infant subject 1s generic vs. specific "ball" images) were more broadly and not normally distributed ($R = 1.40$ to 6.47, $p = 0.00$ by Shapiro Test).

We then incorporated these ratings into our eyetracking data analysis, calculating a similarity score for each infant. This score was the average similarity score from our adult volunteers for each infants' 8 specific and 8 generic items. For the window-of-interest analysis, there was no correlation between average similarity ratings and infants' overall subject means ($R=0.10$, $p = 0.58$), or between similarity ratings and their subject means on either trial-type (generic: $R= 0.13$, $p = 0.48$; specific: $R= 0.03$, $p = 0.87$). Moreover, a model adding similarity score as a predictor did not fit the data better than one that omitted this term ($p = 0.78$).⁷ Thus, this final analysis suggests that the visual similarity between generic and specific items did not drive infants' performance in our eyetracking study.

⁷Unfortunately, issues with model singularity (likely due to overfitting and data sparsity) prevented us from incorporating similarity scores into our Growth Curve Analyses

Table 6

Mean adult similarity scores(Range:1-7) and infant word exposure(Range:1-5) for common target words (>5 infants)

target	adult_mean_sim	mean_exposure	n_kids
book	2.52(1.51)	4.91(0.29)	22
ball	3.27(1.43)	4.65(0.7)	17
shoe	2.95(1.59)	4.4(0.7)	10
bottle	2.24(1.19)	4.62(0.52)	8
highchair	2.91(1.36)	4.71(0.49)	7
blocks	3.15(1.48)	4(1.1)	6
car	4.24(1.52)	4.5(0.84)	6
diaper	2.52(1.31)	5(0)	6
spoon	2.36(1.3)	3.83(1.17)	6

Note. Full data for each item tested available on OSF

Discussion

We set out to answer two questions: whether referent familiarity contributes to word comprehension, and how this may shift over the first half of the second year. Summarily, while we clearly see rapidly improving comprehension in year two (consistent with the literature, e.g. Fernald et al., 1998), we found only a modest role for referent familiarity. That is, overall, we found that word comprehension was robust when infants saw images of referents from their daily lives, and novel images of those same kinds of items.

There are several possible interpretations of this pattern of results. One possibility is that infants could not actually tell the familiar and novel images of our referents apart. Under this scenario, the lack of specific vs. generic effect would suggest not that infants generalize to new tokens, but that they have relatively poor representations of referents they

have hundreds of hours of experience with. We find this unlikely for several reasons.

First, our Growth Curve Analyses suggest that while there was no overall trial-type difference in our window-of-interest, our younger infants' fine-grained looking patterns varied between trial-types; it seems unlikely that a difference apparent to younger infants would be unapparent to older ones. Second, in the object categorization domain, even six-month-olds can detect subtle changes in a two-shape visual array, e.g. when shown a diamond and a circle on a grey background, they notice if the diamond changes color after a brief blank screen interlude (Cantrell, Kanjlia, Harrison, Luck, & Oakes, 2019). These are far subtler differences than those between our specific and generic versions of items, and our sample is 6-12 months older, suggesting it is quite unlikely they would not be able to tell an image of their dog or pacifier apart from a different token of these categories (cf. Feigenson, 2005). Relatedly, six-month-olds also readily recognize their own mother and father as compared to other women and men in a word comprehension context (Campbell, 2018; Tincoff & Jusczyk, 1999). Thus, previous work provides support for the idea that using similar methods, infants can tell when referents change, and "their" version of a referent apart from a different one.

Third, even more compelling evidence that the infants in our study could tell their items and the new items apart comes from a recent study on pets, proper nouns, and count nouns (Campbell, 2018). In their within-subjects study, 12-15-month-olds saw images of either their pet and another same-breed pet (e.g. their poodle Joey and a novel poodle), or their pet and a different-species pet (e.g. Joey and a novel cat) and heard "dog", "cat", or "Joey" on different trials. Infants looked at the correct referent across the board: on hearing e.g. "Joey" they looked at their own pet whether it was next to another same-breed dog, or a cat; when hearing "cat" or "dog" they looked at the cat or dog when only one of each was on the screen. When hearing "dog" while seeing their own pet and a novel same-breed dog, they looked at both equally. Thus, Campbell's findings provide evidence that infants in the age-range tested here can tell their own items apart from new ones (since e.g. "Joey" only

371 applying to the former), and moreover, understand that count nouns like “dog” apply to
372 both. More generally, this supports a *narrow-and-broad* account of word-learning, where
373 both individuation and categorical knowledge are part of early word learning.

374 If we had seen that infants only considered their familiar items as appropriate referents,
375 that would have provided evidence of overly narrow categories. That is not what we find. If
376 anything, previous studies suggest that rather than being overly specific in their
377 representations, in some contexts infants are overly accepting, i.e. include clearly
378 inappropriate referents as instances of a word. Indeed, previous work finds that infants’
379 representations for concrete nouns go through fine-tuning and competition depending on
380 perceptual and conceptual features (Arias-Trejo & Plunkett, 2010; Bergelson & Aslin, 2017b,
381 2017a). For instance, one study finds that around 12 months infants look at e.g. an image of
382 juice when hearing “juice” or “milk”; by around 18 months, they are less likely to do so
383 (Bergelson & Aslin, 2017b). This suggests that in some circumstances, even when a given
384 image is definitely not a category member (i.e. far more than simply being an unfamiliar but
385 plausible token), early in year two, infants treat it as a plausible referent. In this light, it is
386 perhaps not surprising that specific and generic images in the present study were both
387 considered plausible referents to infants.

388 Taken together, these previous studies leave it unlikely that infants simply couldn’t tell
389 familiar and novel instances of referents apart in the present study. To us, a more
390 parsimonious interpretation is that specific referent familiarity is both detectable and largely
391 irrelevant for this spoken word comprehension task. In this way, referent familiarity appears
392 to be analagous to voice familiarity. For instance, while in certain contexts young infants can
393 discriminate novel versus familiar voices (Johnson, Westrek, Nazzi, & Cutler, 2011), they do
394 not show a decrement in spoken word comprehension when hearing novel talkers (Bergelson
395 & Swingley, 2018), ostensibly because talker identity is not relevant to the task. Here too,
396 with common nouns and typical images of referents, spoken word comprehension appears to

397 proceed regardless of image familiarity.

398 To be clear, we are not claiming that referent familiarity could *never* play a large role
399 in spoken word comprehension. We are suggesting that using typical images of common
400 nouns, in a simple spoken word comprehension task with 12-18-month olds, it appears to
401 play a relatively minor one, especailly for infants approaching 18 months. Just as listeners
402 *can* take talker identity into account (e.g. showing surprise at hearing a young child
403 discussing cigarette smoking, Van Berkum, Van den Brink, Tesink, Kos, & Hagoort, 2008),
404 so too could familiarity play a big role in the right circumstances. For instance, typicality
405 appears to drive comprehension in 12-month-olds to a greater degree than in older infants
406 (Meints, Plunkett, & Harris, 1999). Given that typicality effects emerge from accrued
407 experiences, for categories that infants have less experience to draw on, familiarity may play
408 an detectable role. This awaits further work.

409 **What about younger infants?**

410 Our reasons for testing 12-18-month olds rather than younger infants were both
411 practical and theoretical. Older infants know more words, and know the words they do know
412 better (i.e. more robustly) than younger infants by various metrics (Bergelson & Swingley,
413 2012, 2015; Fernald et al., 1998; Frank, Braginsky, Marchman, & Yurovsky, n.d.). Thus, in
414 order to (a) be able to select a range of items for each child that we could pair while
415 mimizing animacy mismatch or onset overlap, (b) have reasonable confidence that infants
416 knew the labels for based on existing norms, results, and parental report, and (c) position
417 ourselves better to see trial-type differences without worrying about potential floor or ceiling
418 effects, i.e. to capture a range of development where knowledge is strong but improvements
419 are rapid, we opted for the first half of year two.

420 However, our chosen age range leaves open the possibility that infants under 12 months

would show a robust effect of item familiarity. Indeed, the younger infants in our sample did show weak evidence of word comprehension in the generic trial-type, though their performance did not differ on specific and generic trials overall (nor did older infants'). And although we found evidence of item familiarity (i.e. trial-type) contributing to performance in our growth curve analyses (particularly for younger infants), we did not see such an effect collapsing across our window of interest, in either age-group (or with age as a continuous variable). Our interpretation is that 12-15-month-olds' chance-level performance on generic trials is due to lower levels of performance in this age-group in this task.

That said, we cannot rule out the possibility that younger infants than those tested here would show stronger performance on specific vs. generic trials. However, here too we invoke data from Campbell (Campbell, 2018). That work finds that similar to the pet study described above, 6 and 9 month olds do not look more at images of their own hand or ball when seeing them alongside novel ones and hearing "ball" or "hand", but they do look more at the their own hand or ball when hearing those words and seeing a second item from a different category (e.g. foot.) We thus believe that we would fail to see substantive familiarity effects in younger infants, though this is an open empirical question.

Conclusion

Taken together, these results support a growing literature showing that spoken word comprehension improves robustly over infancy, as infants quickly shift their attention to new appropriate referents in mere seconds as utterances unfold. Intriguingly, this work provides some evidence that referent familiarity appears to play a small role in this process, particularly for infants approaching the midpoint of year two. In turn, this suggests that part of understanding words is robustly generalizing *beyond* thousands of hours of direct experience with specific referents, linking up categories and concepts with the words a given language uses to label them.

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