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# Young children's fast mapping and generalization of words, facts, and pictograms

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ABSTRACT

To test general and specific processes of symbol learning, 4- and 5-year-old children learned three kinds of abstract associates for novel objects: words, facts, and pictograms. To test fast mapping (i.e., one-trial learning) and subsequent learning, comprehension was tested after each of four exposures. Production was also tested, as was children's tendency to generalize learned items to new objects in the same taxon. To test for a bias toward mutually exclusive associations, children learned either one-to-one or many-to-many mappings. In Experiment 1, children learned words, facts (with or without incidental novel words), or pictograms. In Experiment 2, children learned words or pictograms. In both of these experiments, children learned words slower than facts and pictograms. Pictograms and facts were generalized more systematically than words, but only in Experiment 1. Children learned one-to-one mappings faster only in Experiment 2, when cognitive load was increased. In Experiment 3, 3- and 4-year-olds were taught facts (with novel words), words, and pictograms. Children learned facts faster than words; however, they remembered all items equally well a week later. The results suggest that word learning follows non-specialized memory and associative learning processes.



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## Introduction

There is an ongoing debate concerning whether children have specialized mechanisms for word learning (Deák, 2000; Golinkoff, Mervis, & Hirsh-Pasek, 1994; Markman, 1994). Arguments for specialization point to the efficiency of word learning. For example, "the adolescent vocabulary of ... up to 60,000 words is achieved with little effort" and "children build the lexicon so [fast] that [it implies] an independently evolved mechanism" (Hauser, Chomsky, & Fitch, 2002, pp. 1575–1576). Some researchers argue that such speed would be impossible without specialized mechanisms (Gleitman, Cassidy, Nappa, Papafragou, & Trueswell, 2005).

Claims of selective mechanisms for word learning are associated with *fast mapping*, that is, inferring a word's meaning from very few exposures (Carey & Bartlett, 1978). Children's ability to fast map words (Dollaghan, 1987; Heibeck & Markman, 1987) is treated as proof of the extraordinary efficiency of early word learning. Yet fast mapping can be considered as the outcome of a complex word-learning system. Within this system, other specialized mechanisms have been proposed to facilitate word learning (Markman, 1994). Some are *learning biases* or probabilistic assumptions that learners make about unfamiliar words. Two biases have garnered considerable attention: the *mutual exclusivity* bias, a disinclination to assign two words to one referent (Markman & Wachtel, 1988; Merriman & Bowman, 1989), and the *taxonomic bias*, an assumption that a new word refers to a category of referents rather than the named exemplar (Golinkoff et al., 1994; Markman, 1994).

We investigated whether and how fast mapping, and the mutual exclusivity and taxonomic biases, are specialized for word learning. To avoid complicating questions about when and how these skills or biases emerge (Liittschwager & Markman, 1994; Namy & Waxman, 1998; Smith, 1999), we tested 3- to 5-year-old children. By this age children know, and are learning, many hundreds of words (Anglin, 1993) and show fast mapping as well as behaviors consistent with both biases (Markman, 1989).

With respect to specialization, several questions remain unaddressed. With respect to fast mapping, Markson and Bloom (1997) found that children can fast map a word *or* a fact equally well. Yet other studies suggest that children do not fast map facts just the same as words but rather tend to generalize them differently (Waxman & Booth, 2000). In addition, it is unclear whether children fast map nonverbal stimuli. Similarly, although the mutual exclusivity bias might not be specific to word learning (Markman, 1994), little research has tested how children apply it to words and nonwords. The question of specialization remains unanswered. Finally, although a "taxonomic bias" certainly goes beyond words—children generalize various properties to categories (e.g., Deák & Bauer, 1996)—it is possible that children tend to treat words as special "markers" of categories. For example, Waxman, Philippe, and Branning (1999) claimed, "Children [have a] specific expectation that count nouns ... refer to object categories" (p. 65), yet empirical support for this claim remains equivocal.

These questions, although unanswered, have been addressed in previous work on fast mapping and on the mutual exclusivity and taxonomic biases.

Fast mapping appears to be specialized because it seems to diverge from common incremental learning trajectories (Thorndike, 1911). However, this assumption is questionable; models of incremental learning would predict that in a sample of to-be-learned associations, learning times should be normally distributed (McMurray, 2007). That is, children will learn most associations after an intermediate number of exposures, but a small proportion will be learned after one or two exposures (i.e., fast mapping) and a few will be learned very slowly. Notably, Deák and Wagner (2003) found that 4- and 5-year-olds learned some words quite slowly, requiring many exposures. Thus, fast mapping might simply describe the short tail of a normal distribution of learning times. Consequently, we cannot assume that fast mapping is even a robust word-learning phenomenon. However, if fast mapping *is* specialized for words, children should fast map a higher proportion of words than other items. To test this, we taught children one of several kinds of stimuli, either words or nonwords, and tested learning after each exposure. If more words than nonwords are learned after one or two exposures, it will indicate specialization. Otherwise, it will support the simpler hypothesis that fast mapping is a by-product of general learning processes (Sloutsky, Lo, & Fisher, 2001; Xu & Tenenbaum, 2007; Yu, 2008).



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Several studies have used this logic to investigate whether fast mapping is specialized. Behrend, Scofield, and Kleinknecht (2001) and Waxman and Booth (2001) confirmed Markson and Bloom's (1997) finding that preschoolers can fast map new facts as well as words. Several other studies reported that toddlers learn symbolic gestures or melodic sequences as readily as words (Campbell & Namy, 2003; Childers & Tomasello, 2002, 2003). However, Namy and Waxman (1998) reported that by their second year of life, children map words faster than nonverbal gestures. This implies that fast mapping might become more efficient for words. Alternately, however, fast mapping might become moderately specialized for verbal items such as facts but not for nonverbal items such as visual events. For example, Markson and Bloom (1997) found that children fast mapped a word or fact but not a nonverbal action (i.e., placing a sticker). Thus, fast mapping might become specialized for verbal information.

However, fast mapping might not be equivalent for facts and words. Not all words are equally easy to learn, nor are all facts equally easy to learn. One factor that affects the difficulty of learning a fact is whether the words within the fact are themselves difficult or unfamiliar. Novel words within a fact should tax working memory and impede learning. However, in previous studies, facts and words were unmatched for the presence of a novel phonological string. That is, Behrend and colleagues (2001), Markson and Bloom (1997) and Waxman and Booth (2000) used facts consisting of entirely familiar words (e.g., "My uncle gave me this"). Perhaps, then, facts are *not* fast mapped as easily as words if phonological novelty is controlled. To resolve this question, we compared words with two types of facts: facts with all familiar words ("familiar facts") and facts with an incidental novel word ("novel facts"). If novelty impedes learning, novel facts and novel words should be learned slower than familiar facts. If fast mapping is specialized for words, there should be an advantage of words over novel facts. This would suggest that previous studies (e.g., Markson & Bloom, 1997) obscured a fast-mapping advantage for words.

Another variable that affects fact learning is content familiarity or prior conceptual knowledge (Johnson & Mervis, 1994). Content knowledge increases throughout childhood, and memory for facts improves correspondingly (Siegel & Ryan, 1989). To indirectly assess the effect of content knowledge on fast mapping, we tested 3- to 5-year-olds, using age as a proxy for increasing content knowledge. However, all specific facts were arbitrary and not previously known by any child.

In addition to these questions, we asked whether fast mapping extends beyond words and facts to nonverbal items. Toddlers fast map abstract nonverbal information (e.g., gestures; Childers & Tomasello, 2002; Namy, 2001), but they might become faster at word learning (Namy & Waxman, 1998), presumably due to experience (Smith, 1999). From 3 to 5 years of age, preschoolers accrue more experience learning words than a parallel nonverbal type of information: pictorial symbols or pictograms. Pictograms (e.g., red circle crossed by a diagonal line meaning "prohibited") are symbolic if they are abstract (i.e., refer to a category), non-iconic, and conventional (see Deacon, 1997). We know very little about how children learn pictograms. For example, it is unknown whether preschoolers will fast map symbols that are less common and less functionally important than words—for example, pictograms. By comparing pictograms with facts and words, we tested whether children who are expert word learners can fast map nonverbal symbols as well as verbal symbols (words) and elaborated/predicated symbols (facts). The results will indicate whether and how fast mapping is specialized for symbolic and/or verbal information.

Another question was whether the mutual exclusivity bias is stronger for words than for nonverbal items (Ellis, 2006; Frank & Poulin-Dubois, 2002). Anderson (1976) found adults have more trouble learning and remembering facts if subjects and predicates are paired in many-to-many relations—that is, if each subject occurs with multiple predicates and vice versa. This *fan effect*—essentially a mutual exclusivity effect for facts—suggests that facts might elicit mutual exclusivity effects in children. Markman (1994) speculated that mutual exclusivity effects stem from a general associative bias that is reflected in other learning effects (Kamin, 1969). Piccin and Blewitt (2007) reported mutual exclusivity effects in children learning color associations. This implies that mutual exclusivity is a general bias, but there is little additional evidence to confirm the claim. Therefore, we taught children associations in one of two patterns. In a *one-to-one mapping* condition, each novel item (word, fact, or pictogram) referred to one object and each object had a unique item. In a *many-to-many mapping* condition, one item referred to two objects and another object was associated with two items. If

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mutual exclusivity is stronger for words than for facts or pictograms, word learning should be slowed more by many-to-many mappings with objects compared with fact or pictogram mappings. If the effects of a many-to-many pattern do not differ for words, facts, or pictograms, it would fail to support claims of specialization.

Another question was whether children are more biased to taxonomically generalize words than other items. Waxman and Booth (2000) and Behrend and colleagues (2001) found that 4-year-olds generalize words more than facts to same-category exemplars, and Waxman and Booth (2001) claimed that children expect words to represent categories. However, this might depend on the specific content of items. A fact like "... was recently cleaned" might not generalize to a cohesive category, whereas "... is available with side airbags" does. This distinction might explain some previous findings. For example, several of Behrend and colleagues (2001) facts described *accidental properties* (e.g., "My cat stepped on this," "This fell in the sink"). These properties are not category based and should not be generalized. By contrast, at least one kind of fact should be generalized; generic properties (e.g., "This is made from lye"; Deák & Bauer, 1996) strongly imply a category property (Gelman, Star, & Flukes, 2002), and children sometimes generalize them. Thus, content matters. Nonetheless, one study (Waxman & Booth, 2000) found less generalization of non-accidental facts than words. Perhaps, then, the taxonomic bias is stronger for words. Clearly, however, the claim requires verification. We compared children's generalization of novel words and facts using facts that neither encourage (i.e., generic) nor discourage (i.e., accidental) generalization but could be true of either a category or an exemplar. This makes for an unbiased test of taxonomic specialization.

We also examined taxonomic generalization of pictograms, which has not previously been tested in 3- to 5-year-old children. Although 4- and 5-year-olds recognize some commercial logos (Horner, 2005), it is unknown how quickly children learn and generalize such symbols. This study addressed this question.

Because learning goes beyond fast mapping and specialization of word learning might emerge not just after one exposure, children were tested after each of several exposures to an item and were tested using multiple measures. Comprehension (i.e., choosing the appropriate referent) was tested after each of four exposures. Generalization to novel items was tested after comprehension testing and, thus, after four exposures. Production, a fundamental metric of symbol learning, was also tested. In children, productive competence for fast-mapped words is much lower than comprehension (Dollaghan, 1987). This fits other evidence that children understand more than they will produce (Benedict, 1979; Clark & Hecht, 1983). However, it is unknown whether the comprehension advantage is different for words and facts. Facts are longer than words, so they should impose a larger verbal memory load. In addition, novel words within facts might impede production (Whitehurst, 1972). Thus, children might show a similar comprehension advantage for facts and words. However, facts also have internal organization and associations (e.g., words, phrases) that can serve as retrieval cues (Casenhiser & Goldberg, 2005). These rich associations might help children to retrieve facts in production as well as in comprehension.

Children were also prompted to produce novel pictograms. However, pictogram production—drawing—involves a different perceptual motor system than speaking and will be limited by children's drawing ability (Goodnow, 1977). Moreover, children's drawing has been studied almost entirely with familiar stimuli; we do not know how well children can visually reproduce fast-mapped pictograms. Thus, although we compared children's production of pictograms with words and facts, differences will be challenging to interpret. Therefore, this aspect of the study was exploratory.

In addition, in Experiment 3, we tested memory for words, facts, and pictograms after a week. Word-learning specialization is meaningful only if words are retained in the mental lexicon (Horst & Samuelson, 2008). To assess finer-grained differences in retention, we tested immediate retrieval and then tested relearning following a single additional exposure (i.e., reminder) to the item.

In sum, this study tested whether preschool children fast map words more than facts or pictograms. If so, word comprehension should be superior after one or two exposures. The study also tested whether a bias to learning one-to-one mappings (i.e., mutual exclusivity) was greater for words than for facts or pictograms. If so, many-to-many mappings should impede word learning more than fact or pictogram learning. Finally, the study tested whether children have a stronger taxonomic bias for words than for facts or pictograms. If so, children should generalize words more systematically to new variants of the referent category.

All of these questions are potentially independent; that is, there might be a fast-mapping advantage for words with no mutual exclusivity or taxonomic specialization. Alternately, there might be specialization of one bias or the other but not for fast mapping. This is because fast mapping is not presumed to rest solely and specifically on those two biases. Finally, there might be differences among words, facts, and pictograms in fast mapping (i.e., after one exposure) or after a few more exposures. Furthermore, differences might emerge in subsequent production or in retrieval following a delay.

## Experiment 1

Preschool children were taught several words, facts, or pictograms, shown in Fig. 1 and Table 1. In what follows, any such stimulus is called a *novel item*, and its referent is called a *novel object* or simply an *object*. Children heard or saw each specific item-to-word pairing (i.e., mapping) four times. To test and control phonological novelty effects in fact learning, one fact-learning group heard facts with all familiar words (called *familiar facts*) and another group heard facts with embedded novel words (*novel facts*) that did not label the object. A pictogram-learning group saw novel non-iconic figures paired with objects. Pictograms were similar in complexity to real objects (e.g., "do not enter" signs).

Referent objects were four distinctive novel artifacts, shown in Fig. 2. In the generalization test, three variants of each training object were added. A fourth object, identical to the training object, was added to test "minimum" generalization. If children choose only one object, it would suggest that they interpreted the item as a proper name or an exemplar associate. If they choose only the original and identical objects, it would suggest that they generalized the item very narrowly.

To test for a one-to-one bias, children were randomly assigned either to a one-to-one group that learned a different item for each of four objects (Fig. 3, top) or to a many-to-many group (Fig. 3, middle) that learned three items for three objects. The latter group learned two items for one of the objects, a third item that referred to two other objects, and fourth item for the last object. This yielded the same number of mappings (i.e., four) as the one-to-one condition. As a consequence, however, there was one fewer item and object. Because it is impossible to match both the number of items/objects and the number of mappings, and either of these might affect learning, Experiment 2 used another many-to-many scheme (Fig. 3, bottom) that controls for number of items/objects instead of

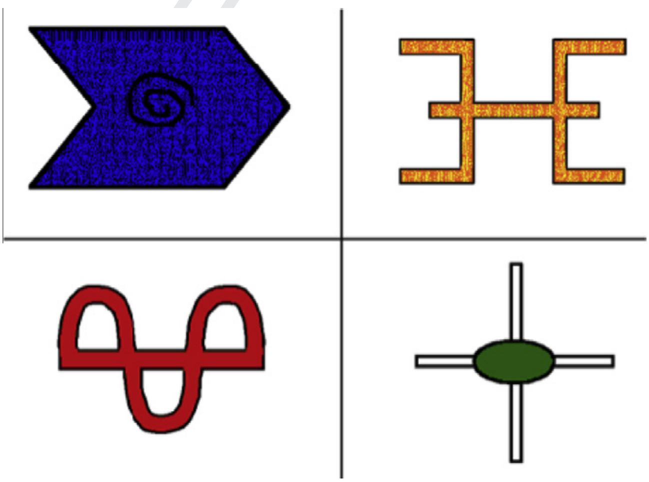

**Fig. 1.** Pictograms used in Experiments 1 and 2. Each is a different color. (For interpretation of the reference to color in this figure legend, the reader is referred to the Web version of this article.)

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**Table 1**
Novel words, familiar facts, and novel facts used in Experiment 1.

| Novel words | Familiar word facts | Novel word facts |
| --- | --- | --- |
| Tama | "My sister gave this to me" | "My tama gave this to me" |
| Oni | "My friend also has this" | "My oni also has this" |
| Saybu | "This is from Japan" | "This is from Saybu" |
| Kumo | "I keep this on my desk" | "I keep this on my kumo" |

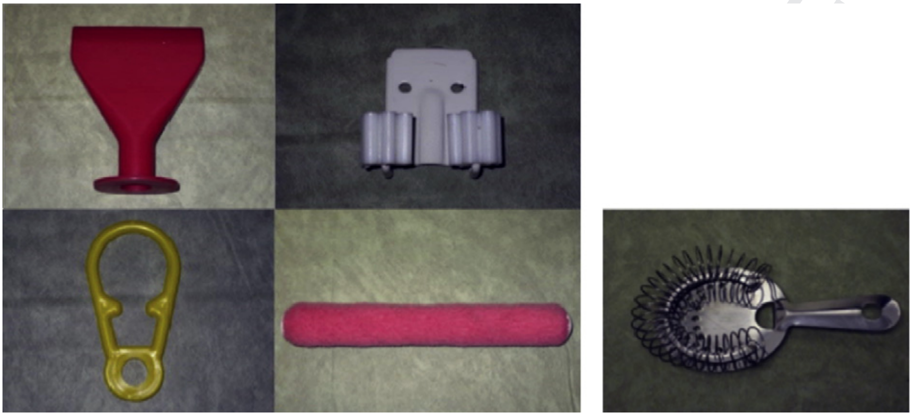

**Fig. 2.** Novel objects used in Experiments 1 and 2. In Experiment 3, the metal strainer (right) replaced the pink roller. (For interpretation of the reference to color in this figure legend, the reader is referred to the Web version of this article.)

mappings. In either case, however, we expected children to learn one-to-one mappings faster than many-to-many mappings.

## *Method*

### *Participants*

In total, 98 4- and 5-year-olds (48 girls and 50 boys, mean age = 4 years 11 months [4;11], range = 3;10–5;11) were tested in urban and suburban preschools in San Diego, California, on the U.S. southwest coast. All children were fluent in English (by teacher report, verified by playing with the children). Most children were middle socioeconomic status and European American. Another 13 children were excluded due to attrition or testing error.

### *Materials*

Thirteen familiar objects were used: two exemplars of drinking cups, apples, and candies and single exemplars of a car, a bicycle, a hammer, a toothbrush, a crayon, a book, and a hat. These were labeled (in the words condition) as *glass, cup, apple, fruit, chocolate, candy, car, bicycle, hammer, toothbrush, crayon*, *book*, and *hat.* The Facts group heard them described by simple familiar facts (e.g., cup = "to drink from," hat = "goes on your head," crayon = "use [it] to color"). The Pictogram group saw related cartoon-like stylized pictures. Thus, for familiar items, children did not need to learn new mappings.

Novel objects were (Fig. 2, clockwise from upper left) a red sprinkler fixture, a white broom holder, a pink paint roller, and a yellow plastic hanger. Four more exemplars of each were used in the generalization test: one identical exemplar and three exemplars that differed in color (blue sprinkler, yellow holder, yellow roller, green hanger), texture and color (mottled black sprinkler, speckled gray holder, matted light blue roller, nubby rough white hanger), and shape and color (silver sprinkler with flange removed, red holder with base reduced, shortened purple roller, orange hanger with hook removed).

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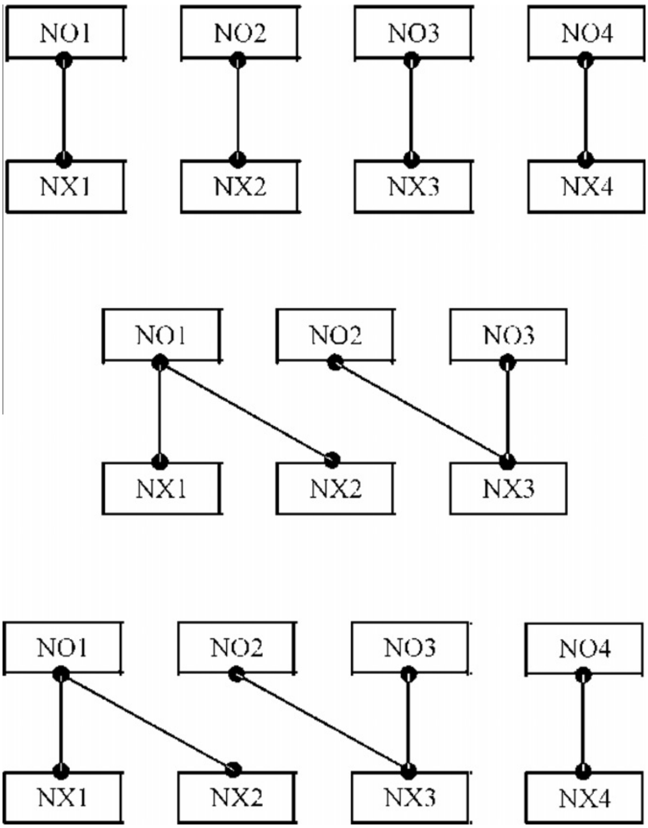


**Fig. 3.** Top: One-to-one mapping scheme (Experiments 1 and 2). Middle: Many-to-many mapping scheme controlling number of mappings (Experiment 1). Bottom: Many-to-many mapping scheme controlling number of items (Experiment 2). NO, novel object; NX, novel item (word, fact, or pictogram). Lines indicate mappings.

Novel words were chosen to be short, easy to pronounce, and distinctive. Familiar facts were brief sentences with all familiar words (see Table 1). Novel facts each included one of the novel words, but each word was embedded in the sentence so that it was not interpretable as a label for the object. Facts were unrelated to any distinctive object property.

Pictograms had distinctive shapes and colors that were dissimilar to any object property. Pictograms and familiar object cartoons were printed on 10-cm$^2$ cards.

*Design and procedure*

Children were randomly assigned to learn novel words, familiar word facts, novel word facts, or pictograms. Within each of these groups, children were randomly assigned to learn either one-to-one mappings or many-to-many mappings (refer to Fig. 3).

Every group completed a comprehension test with four blocks of trials, then a production test, and finally a generalization test. For each child, item order was randomized except that in the many-to-many condition both mappings for an object were presented in succession. Items were randomly assigned to objects.

*Pretest.* Children named objects until 13 familiar and 4 novel stimuli were identified. Children were told to say "I don't know" if they could not name an object. If a child recognized a novel object or could not name a familiar one, it was replaced. This occurred very rarely.

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*Training.* To train the word group, the experimenter held a novel object and said, "This is a _____." For fact learners, she said, for example, "My _____ gave me this." For pictogram learners, she pointed to the card and said, "This one goes with this [object]." Children were asked to repeat the word or fact to ensure that they could produce it or to point to the pictogram to ensure that they had been attentive. The proportion of correct repetitions or points averaged .79 ($SD$ = .37) for words, .68 ($SD$ = .45) for familiar facts, .49 ($SD$ = .42) for novel facts, and .99 (correct pointing) for pictograms. The difference is significant, $F(3, 88) = 14.3$, $p < .001$. Post hoc Scheffé's tests revealed that words and pictograms were repeated or pointed to, respectively, more than either type of fact. This confirms that working memory demands were greater for facts. This effect underscores the learning results reported below. Object order was randomized for each trial, and this order was repeated in each block or test to control intertrial intervals across items.

*Comprehension test.* Children were asked to identify the referent(s) of each word/fact/pictogram. All 17 novel and familiar objects were arranged haphazardly on a tray in front of children. In each of four test blocks, children were questioned about each novel item and several familiar items, starting with familiar items. Familiar item questions were based on children's condition (e.g., children in the word, fact, and pictogram conditions were asked, "Can you find a cup?", "Can you find one that you drink from?", and "Can you find one that goes with this [picture of cup]?", respectively. Familiar item trials ensured that children were compliant, attentive, and able to understand the task. The number of familiar items decreased from four to two across blocks to minimize boredom.

In each block, one familiar item had two correct referents (e.g., two apples) and two items referred to one object (e.g., "glass" and "cup"). On these trials, after children answered the initial comprehension question (e.g., "Is there a hat here?"), they were asked an "another one" question (e.g., "Is there another hat here, or is that the only one?"). These questions established the permissibility of choosing two referents, particularly for many-to-many children (who needed to choose both referents of one object to get full credit). However, to control for this procedure, children in the one-to-one condition also were asked "another one" questions in some trials (randomized). Thus, for both groups, the correct answer to *most* "another one" questions was "no" but was occasionally "yes."

In every item comprehension trial, for each block children were asked, in the word condition, "Can you find [e.g., an *oni*]?" or, in the fact condition, for example, "Can you find [one my (sister/*oni*) gave me]?" In the pictogram condition, the experimenter pointed to a card and asked, "Which one goes with this picture?" Feedback was provided after each trial (e.g., "That's right, that is an *oni*" or "Actually, *this* one [pointing] goes with this"). This feedback served as the next exposure to the item–object mapping. Thus, children were tested after the first exposure to each item and then after each of the next three exposures, one per item per trial block.

*Production test.* Immediately after the comprehension test, children were asked to produce the word or fact for each object ("What is this called?" or "What did I tell you about this one?") or to draw each pictogram. No feedback was given. For pictogram production, the cards were removed and crayons and paper were provided. Children were shown each object, in random order, and asked, "Do you remember the picture that goes with this? Can you draw that picture?" All children were asked some "another one" questions (e.g., "Do you remember another word for this?") to test production of many-to-many mappings. To limit the test duration, children did not draw familiar items.

*Generalization test.* After the production test, children were shown an array of four familiar objects and 20 novel objects: the four original novel items and four same-category variants of each (identical, different color, different texture and color, and different shape and color exemplars). Objects were presented in haphazard positions on a tray. Children were prompted to look at all objects for approximately 30 s. They were then asked to find any exemplars of the item. Word learners were first asked, for instance, "Are there any _____s or not? Can you find all the _____s?" After choosing an object, children were asked, "Are there any more _____s, or is that all?" Thus, children could say "no" at any time; there were minimal pragmatic demands to select multiple exemplars. Fact learners were asked, for example, "Are there any things here that [fact]? Can you find anything that [fact]?" and, subsequently, "Are there any more here that [fact], or is that all?" Pictogram learners were asked, for example, "Are there any [card raised]? Can you find any [card raised]?" and, subsequently, "Are there any more [card raised], or is that all?" After each trial, the tray was removed and objects were scrambled, and the next item was queried. Items were queried in the order they were taught. Children received no feedback.

*Coding*

All responses were recorded online by the experimenter. Another coder independently rescored all sessions from video recordings. Intercoder agreement about children's responses averaged 98%. Correct comprehension entailed choosing the trained object (expected accuracy if responding at random = 6%). Pictogram drawings were coded independently by two researchers, one of whom was blind to the hypotheses. Agreement was 97%. Disagreements were resolved by discussion. To receive credit for production, a child must have spoken or drawn every major element (phoneme, word, shape, and color) in the right order or configuration. Changes in anaphoric elements (e.g., "my sister" vs. "your sister") were ignored, as were minor changes in pronunciation and distortions of drawn shapes (e.g., relative sizes, alignment). Children received half credit for productions that lacked one element but unambiguously represented a specific item (i.e., words with one phoneme altered; e.g., "toda" for "toma," drawings with two of three specific shape features, facts with similar meaning but a rewording, e.g., "You got it from your desk" rather than "You keep it on your desk"). Two researchers independently scored all productions. There were fewer than 5% disagreements; these were resolved by discussion. Only 8% of responses received half credit; results do not differ if they are counted as incorrect. In the generalization test, a child's score was the proportion of same-category objects for each item. Other-category choices determined an *overgeneralization ratio* that was a correction weight for children's generalization scores (see below).

*Results*

Gender differences in every measure were assessed by $t$ tests (two-tailed). No significant or marginal differences were found, so girls and boys were combined in all analyses.

*Comprehension, familiar items*

Children accurately chose familiar objects for words ($M = .92$, $SD = .08$), familiar facts ($M = .81$, $SD = .18$), novel facts ($M = .83$, $SD = .23$), and pictograms ($M = .89$. $SD = .07$). Words elicited the highest accuracy, strengthening the results below. One-to-one and many-to-many groups averaged .88 ($SD = .18$) and .84 ($SD = .14$), respectively. Scores were compared in a 3 (Item Type) × 2 (Mapping Scheme) analysis of variance (ANOVA), with age covaried. All effects and interactions were nonsignificant, and the full model accounted for only $R^2 = .07$ (all $R^2$ values are adjusted). Thus, children performed uniformly well on familiar items, indicating that they were attentive and compliant.

*Comprehension, novel items*

Preliminary analysis of accuracy showed that for all item types, the first and second blocks of trials did not differ and the third and fourth blocks of trials did not differ. Thus, learning was not significant from the first to the second repetition of each item or from the third to the fourth repetition of each item. Therefore, we combined Blocks 1 and 2 into "early" blocks and combined Blocks 3 and 4 into "later" blocks. A paired-samples $t$ test revealed significant learning from early to later blocks, $t(97) = 7.9$, $p < .001$. Means and standard deviations for comprehension and all other measures, in all three experiments, are summarized in Table 2.

Comprehension means were compared in a 2 × 4 × 2 multivariate analysis of variance (MANOVA), with early blocks (1 and 2) versus later blocks (3 and 4) within participants and with item type (words, familiar facts, novel facts, or pictograms) and mapping (one-to-one or many-to-many) between participants. Age (in months) was a covariate.

Fig. 4 shows the proportions of correct early and later responses by item type. There was a significant item type multivariate effect, $F(6, 174) = 7.8$ (Hotelling's trace), $p < .001$, $\eta^2 = .21$ (all effect sizes are adjusted). The item type effect was significant in early blocks, $F(3, 89) = 8.6$, $p < .001$, $\eta^2 = .22$, and in later blocks, $F(3, 89) = 11.0$, $p < .001$, $\eta^2 = .27$. There was no word-learning advantage; rather, there

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**Table 2**
Mean proportions (and *SDs*) of correct responses to dependent measures in Experiments 1, 2, and 3 by item type.

| | Comprehension | | Production | Generalization |
|---|---|---|---|---|
| Experiment 1 | Early | Late | | |
| Words | .45 (.24) | .59 (.19) | .14 (.18) | .26 (.24) |
| Familiar facts | .74 (.23) | .93 (.15) | .72 (.33) | .83 (.21) |
| Novel word facts | .44 (.27) | .78 (.27) | .27 (.31) | .68 (.28) |
| Pictograms | .57 (.23) | .76 (.21) | .23 (.34) | .63 (.38) |
| Experiment 2 | Early | Late | | |
| Words | .48 (.20) | .50 (.23) | .19 (.21) | .61 (.41) |
| Pictograms | .66 (.19) | .88 (.16) | .45 (.43) | .80 (.25) |
| Experiment 3 | Session 1 | Session 2 | | |
| Words | .40 (.16) | .69 (.33) | .22 (.21) | .91 (.17) |
| Novel word facts | .64 (.24) | .66 (.25) | .20 (.14) | .90 (.38) |
| Pictograms | .43 (.24) | .60 (.24) | .12 (.18) | .87 (.37) |

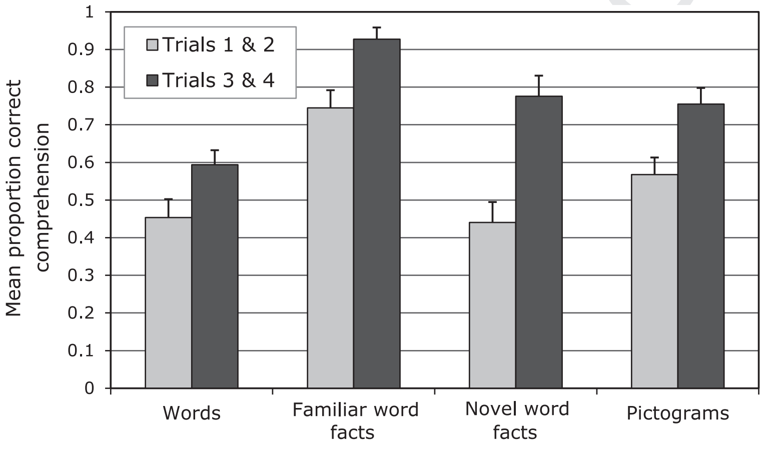


**Fig. 4.** Mean (± *SD*) proportions correct earlier and later comprehension blocks by item type: Experiment 1.

was a fact advantage. Scheffé's tests showed that in early blocks, familiar fact learners outperformed word learners ($p$ = .001) and novel fact learners ($p$ < .001). No other differences were significant. In later blocks, familiar fact learners outperformed word learners ($p$ < .001), as did novel fact learners ($p$ = .036); pictogram learners were marginally more accurate than word learners ($p$ = .051). A follow-up analysis indicated that the interaction between blocks (early vs. later) and item types was not significant ($p$ = .092).

Mapping effects were nonsignificant, although one-to-one groups trended toward greater accuracy both in early blocks (*Ms* = .60 vs. .52, *SD* = .27, $p$ = .152) and in later blocks (*Ms* = .79 vs. .74, *SD* = .21, $p$ = .263). The interaction of mapping and item type was not significant ($F$ < 1). Thus, mutual exclusivity effects were no larger for words than for facts or pictograms. Age was marginally related to early accuracy, $F(1, 89) = 4.4$, $p = .039$, $\eta^2 = .05$, but not to later accuracy ($p$ = .125).

The full model captured $R^2_{adjusted} = .21$ in early blocks and $R^2_{adjusted} = .22$ in later blocks.

*Production*

Production was compared in a 4 (Item Type) × 2 (Mapping) ANOVA, with age covaried. There was a significant item type effect, $F(3, 85) = 20.3$, $p < .001$, $\eta^2 = .42$; familiar facts were produced more accurately ($M$ = .72, $SD$ = .33) than words ($M$ = .14, $SD$ = .18), novel facts ($M$ = .27, $SD$ = .31), and pictograms ($M$ = .22, $SD$ = .34), all *ps* < .001. Notably, 11 of 25 children (44%) in the pictogram condition sometimes drew objects instead of pictograms, suggesting that they did not understand the task. With those children excluded, the remaining pictogram learners averaged .46 correct ($SD$ = .37).

Neither the mapping effect nor the item by mapping interaction was significant ($F < 1$). However, the age covariate was significant, $F(1, 85) = 4.4$, $p = .039$, $\eta^2 = .05$. The full model accounted for $R^2 = .38$.

*Generalization*

Because we cannot interpret generalization choices for items that children never learned, we analyzed *learned items*, defined as any items that received at least three correct responses in the five previous trials (binomial $p = .051$). Some children did not meet this criterion for any item and were excluded from the analysis (8 word learners, 1 familiar fact learner, 9 novel fact learners, and 2 pictogram learners). The remaining children were compared on their proportion of same-category objects chosen (out of five). Accuracy was as follows: words ($M$ = .32, $SD$ = .26), familiar facts ($M$ = .87, $SD$ = .19), novel facts ($M$ = .75, $SD$ = .23), and pictograms ($M$ = .67, $SD$ = .34).

Generalization scores were entered into a 4 × 2 ANOVA (Item Type × Mapping), with age covaried. However, scores were adjusted to account for individual children's false alarm rate—that is, their tendency to overgeneralize to out-of-category objects. Although overgeneralization was infrequent ($Ms$ = 2%–10% across groups), it could potentially bias the generalization rates. Adjusted correct generalization means are shown in Table 2. There was a significant item type effect, $F(3, 69) = 11.9$, $p < .001$, $\eta^2 = .34$. Post hoc Scheffé's tests showed that words were generalized less than all other types ($ps \leqslant .006$); no other differences were significant. The mapping effect was not significant (one-to-one proportion = .65, many-to-many proportion = .59, $SD$ = .35, $F < 1$). The Item Type × Mapping interaction was nonsignificant ($p = .164$), and the age covariate was nonsignificant ($F < 1$). The results do not differ if all items are included or if uncorrected scores are used. The full model accounted for $R^2 = .33$.

Children chose the different within-category variants (identical, different color, different shape and color, and different texture and color) equally often, and this was true of each item type. Thus, children were not biased to generalize words or other item types according to the specific properties of same-category exemplars.

Comprehension, production, and generalization all were significantly correlated, with age partialled out: $r_{comprehension–production} = .55$, $r_{comprehension–generalization} = .53$, $r_{production–generalization} = .47$. Thus, the measures provide converging, but not redundant, measures of children's learning.

*Discussion*

Children learned facts faster and more accurately than words, as measured by comprehension, production, and generalization. By the third repetition, surprisingly, children had learned facts with embedded novel words *better* than those words alone. The unpredicted finding of slower word learning cannot be attributable to group differences in attentiveness, fatigue, motivation, or task understanding because word learners performed better than fact learners in the repetition check and in familiar item trials. In addition, items were randomly paired with objects, so item-specific effects can be ruled out.

Why was fact learning easier than word learning? One factor was word novelty; facts with embedded novel words were harder to learn than facts with only familiar words. However, that does not explain the difference between novel facts and words in later blocks. One hypothesis is that the cue context provided by facts can facilitate learning. Facts have internal structure and content dependencies that might facilitate recall (Potter & Lombardi, 1998). Retrieving any element of a fact might help children to recognize the associated referent. This might explain why familiar fact comprehension was good after a single exposure. Novel fact learning could have been slower because the added demand of learning a novel word reduced the initial probability of forming an association. However, by the third or fourth repetition, children had formed strong enough associations to select the correct referent.

The rich cue structure within facts might also have helped children to progressively retrieve elements of the fact, thereby facilitating correct fact production. However, this does not explain why facts were generalized more than words. Children spontaneously extended facts to same-category objects even though facts were ambiguous (they were not generic properties and might have applied only to

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the training exemplar). One interpretation is that unless a fact clearly describes an accidental or unique property, children tend to generalize it to similar objects. An alternative explanation, that there were strong task demands to select many objects, is implausible; on any trial, only 5 of 24 objects (21%) were same-category objects, yet children overgeneralized to fewer than 8% objects and averaged only 65% correct objects. Children did not liberally overselect objects; rather, they tended to assume that novel factual properties applied to categories, not individual objects.

A parsimonious explanation for the generalization data is that generalization reflects previous learning; if children had not learned an item well, they tended to generalize conservatively. Comprehension and production data show that word learning was slower and the generalization test simply confirmed the incompleteness of prior learning. If true, this hypothesis suggests two predictions. The first prediction is that weakly learned items are more likely to be forgotten between the last exposure and the generalization test. Thus, children should have forgotten more words than facts or pictograms. In fact, children forgot (i.e., generalized to the wrong category) a total of 14 words versus only 1 familiar fact, 6 novel facts, and 6 pictograms. Thus, words were more often forgotten or confused in the generalization test. The second prediction is that comprehension and generalization accuracy should be correlated within-child regardless of what item type a child learned. This was confirmed; for the entire sample, $r = .54$, $p < .001$. Thus, success of prior learning, not item type, predicted how children generalized.

How can this result be reconciled with reports that 4-year-olds generalize words more than facts, even facts that are ambiguous with respect to generalizability, as in Waxman and Booth (2000)? There are several procedural differences between that study and the current study, including wording of the generalization questions (current study: "Are there any more ______, or was that the [only/last] one?"; Waxman & Booth: "Are there any others that ______?") and details of the stimulus materials. However, no procedural difference can obviously account for the discrepancy. Thus, in Experiment 3, we compared facts and words using different procedural details to replicate one or the other result.

The pictogram condition sheds light on children's learning processes. Pictograms were learned as well as or better than words by later comprehension trials. The fact that children then generalized pictograms quite systematically extends prior evidence indicating that fast mapping might extend to visual items (Namy, 2001), at least in toddlers. The current results show fast mapping of nonlexical symbols even among experienced word learners. One interpretation is that preschoolers' experience with pictograms (Horner, 2005), however limited, allows them to interpret novel pictures as abstract symbols. An alternative interpretation is that children did not interpret pictograms as symbols. For example, a few children sometimes drew objects when asked to draw pictograms. Perhaps, then, they did not infer the abstract representational mapping of pictograms to objects. However, there are other possible reasons for the object-drawing responses. First, children who drew at least one object were no less accurate than their peers in comprehension or generalization scores, and accurate responses in those tasks imply a symbolic understanding of the items.[1] Second, object drawing might have been a fallback strategy when children could not remember enough to draw a pictogram. Alternately, some children might have misinterpreted the test instructions. For example, the phrases used to teach the pictograms and to elicit production used the predicate "goes with," which does not imply taxonomic relations (Deák & Bauer, 1995; Namy & Waxman, 1997). To address this possibility, the instructions in the pictogram condition were modified in Experiments 2 and 3.

The results did not show a stronger mutual exclusivity bias for words than for facts or pictograms, consistent with previous studies (Diesendruck & Markson, 2001; Piccin & Blewitt, 2007). However, the finding is equivocal because the one-to-one advantage was not statistically reliable. Thus, Experiment 2 used another many-to-many scheme that controlled the number of items but added one mapping. Specifically, many-to-many children learned one additional mapping to match the number of items and objects learned by one-to-one children. If there is a one-to-one learning advantage, and it is stronger for words than for facts or pictograms, it will imply specialization of the mutual exclusivity bias.

Q8 [1] By prevailing definitions of "symbol" (e.g., Peirce's "sign"; see Deacon, 1997; Wittgenstein, 1953), pictograms' non-iconicity and conventionality make them legitimate symbols.

Because so little is known about children's pictogram learning, Experiment 2 focused on further exploring the similarities and differences in children's learning of words and pictograms. A secondary purpose was to more thoroughly search for specialization of a mutual exclusivity bias.

## Experiment 2

To further compare how children learn verbal and nonverbal symbols in many-to-many or one-to-one mappings, 4- and 5-year-olds were taught either words or pictograms. Half of the children learned a many-to-many scheme that controlled the number of items and objects (Fig. 2, bottom). Children learned one item for two objects, two more items for a third object, and a fourth item for a fourth object. In this scheme, the many-to-many task should impose a higher cognitive load than the scheme in Experiment 1. If mutual exclusivity is modulated by cognitive load, as suggested by Liittschwager and Markman (1994), this scheme should amplify the bias. This should make it easier to detect any difference in children's mutual exclusivity bias toward words and pictograms.

### *Method*

#### *Participants*

In total, 49 English-speaking 4- and 5-year olds (23 girls and 26 boys, mean age = 4;9, range = 3;11–5;11) were recruited and tested in preschools in San Diego. All children were fluent in English (by teacher report, verified by the experimenter). Most children were European American and middle class.

#### *Materials*

All objects, symbols, and other materials were the same as in Experiment 1.

#### *Procedure*

Children were randomly assigned to learn either words or pictograms in either one-to-one or many-to-many relations. Testing and coding procedures were the same as in Experiment 1. Agreement on pictogram drawing scores was 97%.

### *Results*

Boys and girls did not differ on any measure and were combined in all analyses.

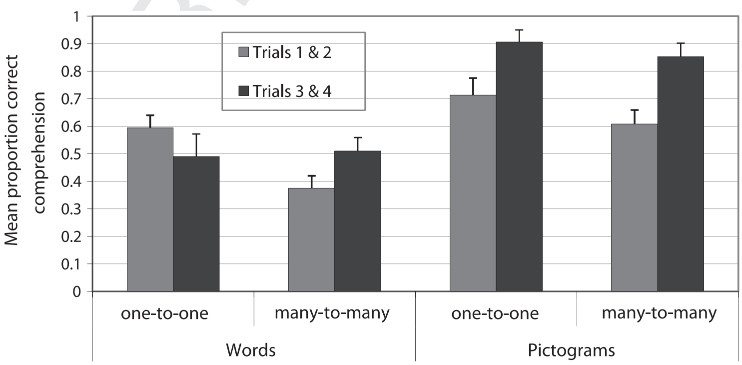


**Fig. 5.** Mean (± *SD*) proportions correct earlier and later comprehension blocks by item type: Experiment 2.

*Comprehension*

Children's proportion of correctly identified familiar word and pictogram referents averaged .93 and .92, respectively ($SD$ = .07). There was no difference between one-to-one (.91) and many-to-many (.93) groups.

As in Experiment 1, there were no differences between Blocks 1 and 2 or between Blocks 3 and 4. Thus, early blocks (1 and 2) were combined and later blocks (3 and 4) were combined. Comprehension accuracy was compared in a 2 (Item Type) × 2 (Mapping) MANOVA (see Fig. 5), with age as a covariate and with early blocks (1 and 2) versus later blocks (3 and 4) as within-participants dependent factors. Children correctly identified significantly more pictograms than words in early blocks ($M$s = .63 vs. .48, $SD$ = .22), $F(1, 44) = 7.3$, $p = .010$, $\eta^2 = .14$, and in later blocks ($M$s = .85 vs. .48, $SD$ = .28), $F(1, 44) = 38.3$, $p < .001$, $\eta^2 = .46$. A follow-up repeated-measures test revealed a significant interaction, with pictogram learners gaining more than word learners from early blocks to later blocks, $F(1, 44) = 14.1$, $p = .001$.

There was a significant mapping effect, $F(2, 43) = 7.3$, $p = .002$, $\eta^2 = .25$; accuracy averaged .68 and .54 in the one-to-one and many-to-many groups, respectively ($SD$ = .21). However, the difference was significant only in early blocks ($M$s = .65 vs. .46, $SD$ = .22), $F(1, 44) = 14.8$, $p < .001$, $\eta^2 = .25$, not in later blocks ($M$s = .70 vs. .62, $SD$ = .28), $F = 1.6$. The interaction was not significant ($F < 1$); one-to-one facilitation was not stronger for words than for pictograms. The age effect was nonsignificant ($F < 1$). The complete model accounted for $R^2 = .30$ in early blocks and $R^2 = .45$ in later blocks.

*Production*

Production was compared in a 2 (Item Type) × 2 (Mapping) ANOVA, with age covaried. Proportion correct averaged .40 and .19 for pictograms and words, respectively ($SD$s = .42 and .21), but the difference was not significant ($p = .073$). The mapping effect was nonsignificant ($F < 1$), as was the interaction ($F < 1$). Age was a significant factor, $F(1, 43) = 19.0$, $p < .001$, likely reflecting improvement of drawing skill. Word production did not improve with age. Of 24 children, 4 attempted to draw at least one object; the results do not change if these children are excluded.

*Generalization*

Only previously learned items (i.e., at least three of five correct responses) were considered; data from 6 word learners and 2 pictogram learners who learned no items were excluded. Children's mean proportion of same-category choices (out of five objects), corrected for individual children's overgeneralization rate, was entered into a 2 × 2 ANOVA, with age covaried. The item type effect was not significant ($M$s = .77 vs. .57, $SD$ = .35), $p = .097$. The mapping effect was nonsignificant (one-to-one $M$ = .75, many-to-many $M$ = .59), $p = .213$, and the mapping by item type interaction was nonsignificant ($F < 1$). The age covariate was nonsignificant ($F < 1$). The complete model explained only $R^2 = .03$, confirming that none of the variables had an effect. Uncorrected scores yield similar results. Children generalized equally to all same-category variants (e.g., different color, different shape).

*Discussion*

The 4- and 5-year-olds learned pictograms faster and more accurately than words, but only by some measures. Pictogram referents were recognized better in early and later blocks. This replicates Experiment 1 and confirms that children can fast map visual symbols at least as well as words. This extends Namy's (2001) findings by showing that extensive word-learning experience does not cause narrow specialization of fast mapping. Of course, children might fast map words and pictograms via different parallel representational mechanisms (e.g., visuo-spatial vs. phonological working memory; Baddeley, 1999). Yet children subsequently produced and generalized pictograms as well as or better than words. These results confirm that children readily fast map *and* impute robust symbolic representations of pictograms.

As in Experiment 1, the one-to-one advantage was no larger for words than for pictograms; thus, there is no evidence that mutual exclusivity is specialized for words. Importantly, however, there was an overall bias; many-to-many learning was slower, indicating that the manipulation was effective



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(possibly because the many-to-many scheme imposed a relatively high cognitive load). Nonetheless, any slowing due to many-to-many overlap was resolved within two exposures.

The results can reconcile some seemingly contradictory findings. For example, children prefer to map a novel word to a novel object (Markman & Wachtel, 1988), but they readily produce several familiar words for a novel object (Deák & Maratsos, 1998) and clearly do not believe that symbols are mutually exclusive (Au & Glusman, 2000; Deák & Maratsos, 1998; Mervis, Golinkoff, & Bertrand, 1994). One interpretation is that children learn many-to-many mappings somewhat slower but eventually fully understand and use these overlapping semantic mappings.

The results, therefore, confirm the finding that words have no fast-mapping advantage, and in some circumstances have a disadvantage, relative to other symbolic information. Two unremarkable explanations for the results can be eliminated. First, children were equally attentive to words; coders watched all videos to ensure that children attended to the tester and/or object during each exposure. Second, the task was not unintentionally biased against word learning; for example, words were optimally short, distinctive, and easy to articulate, as shown by the production check, and word learners performed best in familiar item control trials. Nonetheless, particular stimuli or procedural details might have driven the results. Thus, conceptual replication is crucial. Experiment 3 was designed to further test fast mapping and generalization of words, facts, and pictograms, using modified stimuli and procedures.

One modification concerns the instructions. The phrase "goes with," used when teaching pictograms, might have implied an *indexical* relation instead of a symbolic relation (see Deacon, 1997, for an explanation). Although the pictograms meet the conventional definition of a *symbol* (Ransdell, 1966), and the generalization results suggest that children treated them as symbols, in Experiment 3 the pictogram condition was modified so that the instructions did not imply an associative relation.

Another modification involved stimulus presentation. It is possible that pictograms were remembered better than words because pictograms were presented (i.e., held up) for a longer time (several seconds) than words (spoken for ~1 s). Perhaps children encoded pictograms faster because they were given more time to encode them. Thus, in Experiment 3, pictograms were visible for the same amount of time that words were spoken. Although we do not expect encoding rates for words and pictograms to be equivalent, this modification is nonetheless an improvement in experimental control.

Experiment 3 also tested children's memory for words, facts, and pictograms after a week. Markson and Bloom (1997) found better memory for verbal items than for a nonverbal action after 1 month. Perhaps children expect words and facts, but not nonverbal items (e.g., a sticker, a pictogram), to be *conventional* and *persistent*. If children do not remember pictograms as well as words after a delay, it would suggest that their fast-mapping abilities preferentially support word learning.

## Experiment 3

The results of Experiments 1 and 2 were extended by testing children's and adults' long-term retention of words, facts, and pictograms. Several experiments suggest that toddlers can retain fast-mapped words for only a few minutes (Childers & Tomasello, 2002; Horst & Samuelson, 2008; Spiegel & Halberda, 2011; Vlach & Sandhofer, 2012), although increasing repetition might extend recognition memory for 1 or 2 days (Friedrich & Friederici, 2008; Yuan & Fisher, 2009). However, no study has compared preschoolers' learning *and* memory of novel words, facts, and pictograms. To address this, 3- and 4-year-olds were tested twice, as in early blocks of Experiments 1 and 2, and then retested a week later: first before any reminder and then after a single exposure to test reminder-based relearning of fast-mapped items. Although such reminder effects are well documented in adults' verbal memory (Ebbinghaus, 1885/1964), little is known about reminder effects in young children's verbal learning.

Another goal was to replicate and extend the results of Experiments 1 and 2. Because Experiment 1 found less generalization of words than of facts, contrary to previous studies, a conceptual replication is desirable (Lykken, 1968). To ensure that the results are not due to methodological details, several procedures were modified and improved.

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First, novel words were selected. They are optimally easy to pronounce, phonologically distinctive, and dissimilar from any common words. In addition, new pictograms were created. In addition, new novel word facts that were more uniform were written; each described the object's origin and used double-dative structure (e.g., "I bought this at a *toma* near here"). As in Experiments 1 and 2, these facts could plausibly apply to either a category or a single object. Only novel facts were used because familiar facts were learned so fast in Experiment 1. Finally, one object was modified and another was replaced with a more complex object (see Fig. 3).

Second, presentation duration was altered for pictograms; on each trial, the pictogram was presented for approximately 1 s or the time needed to present (speak) a novel word. This equates with encoding time. In addition, the phrasing was changed to imply a symbolic relation: "The sign for this [object] is this [card]."

Third, because limited, modest mutual exclusivity-related effects were found only in Experiment 2, all children learned one-to-one mappings. Fourth, to ascertain whether our sample was representative of healthy, middle-class English-speaking children, children completed an age-normed receptive language test, the Peabody Picture Vocabulary Test–Revised (PPVT-III; Dunn & Dunn, 1981). Although we could not independently assess the language abilities of children in Experiments 1 and 2, we now sought to determine whether our sample was representative in terms of receptive language. In addition, to further contextualize our sample characteristics, parents provided information about family demographic variables (e.g., parents' education that predicts language skills) and children's personal history.

Fifth, to ensure that the task was not too difficult, groups of adults were taught words or pictograms (not facts because even children learned these quickly) and were tested in the same manner as children. If adults failed to learn the items, it would suggest that the task is too difficult for young children and, therefore, is an inappropriate measure of language learning.

## *Method*

### *Participants*

In total, 42 typically developing 3- and 4-year-olds (24 girls and 18 boys, mean age = 3;10, range = 3;0–4;6) were recruited in preschools in San Diego. An additional 12 children were excluded due to tester error ($n = 1$), noncompliance ($n = 2$), attrition ($n = 5$), or pretest errors ($n = 4$). All participants were fluent in English. Among children whose parents returned demographic information, 60% were exposed only to English and 40% had some exposure to another language. In this sample, 1 child

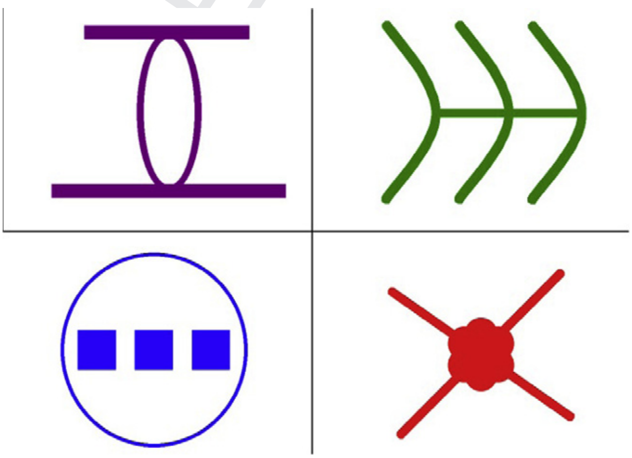

**Fig. 6.** Pictograms used in Experiment 3. Each is a different color. (For interpretation of the reference to color in this figure legend, the reader is referred to the Web version of this article.)

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was African American, 2 children were Asian, 31 were Caucasian, 3 were Hispanic, and 5 were multiethnic.

Children were quasi-randomly assigned to learn words, novel facts, or pictograms (gender was balanced among groups). Children's age and parents' age and education did not differ among groups.

In addition, 20 English-speaking college students (15 women and 5 men, mean age = 19 years) were randomly assigned to learn words or pictograms.

*Materials*

Children saw two familiar objects (watch and sunscreen) and two familiar distracters (cup and toy mailbox). Familiar objects were labeled (*watch* and *sunscreen*), described with a known fact (e.g., "This tells you what time it is"), or paired with an iconic but abstract picture (e.g., clip art clock face).

Novel objects were similar to those in Experiments 1 and 2, but one was replaced (Fig. 3). The generalization test consisted of 18 objects: the original objects and 2 same-category exemplars (different color and different shape variants), 2 entirely novel objects, and 4 familiar objects.

Novel words were *vep*, *toma*, *sabu*, and *koof*. These alternated CVC (consonant–vowel–consonant) and CVCV forms and had no duplicate phonemes. Adults learned the German words *brause*, *haken*, *sichel*, and *sieb*. Novel facts, each with an incidental novel word, were as follows: "A teacher can borrow this for her *koof*," "We keep this on the *sabu* in the office," "I bought this at a *toma* near here," and "This fits in the drawer of her *vep*." Novel pictograms were non-iconic and had several distinctive shape and color elements (Fig. 6). Pictograms were printed on laminated $21 \times 13$-cm cards.

*General procedure*

Children were tested in a quiet room at their preschool in two sessions a week apart. After the second session, children completed a standardized vocabulary test and a posttest of their ability to produce each item. Two experimenters alternated running the first and second sessions. Children received a gift for participating. Adults were tested in two sessions, also a week apart, for class credit.

*Session 1. Training.* Children were told that they would learn about some objects and should try to remember what they were taught in order to tell the other experimenter. Children then heard each word or fact or saw each pictogram for 1 s (e.g., "The word for this is ______," "This fits in the drawer of her *vep*," "The sign for this is [card raised]"). Objects and pictograms were hidden at all other times.
*Comprehension.* Two blocks of trials each consisted of two familiar item trials and four novel item trials. Children saw a tray with 10 novel and familiar objects, arranged haphazardly, and were asked, "Can you find the ______?" (word learners), "Can you find the one that ______?" (novel fact learners), or "Can you find the [card raised]?" (pictogram learners). The second exposure was given as feedback (e.g., "Actually, *this* is a ______") at the end of the block.
*Production.* Children were asked to produce words, facts, or pictograms for the novel objects. For pictogram learners, the object was removed after 2 or 3 s to reduce the demand to draw the object. Productions were scored as in Experiments 1 and 2.
*Generalization.* For each of four items, children saw an array of 20 haphazardly arranged objects (4 original, 3 same-category variants per item, and 4 novel and familiar distracters). The procedure was the same as in Experiments 1 and 2.

*Session 2.* The second experimenter asked children to teach her what they had learned and administered two comprehension blocks: before any reminder (i.e., retrieval) and after feedback (i.e., reminder). Children then completed a production check and finally completed the PPVT-III vocabulary test.

All responses were rescored by a second coder for accuracy. Pictogram drawings were scored offline, with 33% recoded by a second coder who was blind to the hypotheses (agreement = 98%).

*Results*

*Children*

No gender differences were found in any learning measure or in the PPVT-III (boys' average = 108, girls' average = 109), so boys and girls were combined in further analyses. Because some children had

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significant exposure to a second language, we compared them with children who heard only English. The groups differed in PPVT-III scores ($M = 102.2$, $SD = 13.4$ vs. $M = 112.6$, $SD = 11.3$, respectively), $t(28) = 2.3$, $p = .03$. However, the groups did not differ in comprehension, production, or generalization of novel or familiar items. Thus, second-language exposure was not considered in subsequent analyses.

*Session 1 comprehension.* Proportion correct for familiar items averaged .93 ($SD = .18$) for words, .86 ($SD = .30$) for familiar facts, and .86 ($SD = .23$) for pictograms (*ns*). Comprehension of novel items was analyzed by MANOVA, with item type between participants, block (1 or 2) within participants, and age covaried. The item type effect was significant in Block 1, $F(2, 38) = 7.8$, $p = .001$, $\eta^2 = .29$ (Fig. 7). Follow-up Scheffé's tests showed that in Block 1, facts were learned better than words (*Ms* = .64 vs. .27, *SDs* = .32 vs. .25, $p = .002$) or pictograms ($M = .36$, $SD = .22$, $p = .021$), but words and pictograms did not differ ($p = .662$). The item type effect was not significant in Block 2 ($F < 1$). The age covariate was not significant in Block 1 ($F < 1$) but was significant in Block 2, $F(1, 38) = 5.8$, $p = .021$, $\eta^2 = .13$. The complete model accounted for $R^2_{adj} = .24$ in Block 1 and $R^2_{adj} = .11$ in Block 2.

*Production.* For familiar items, accuracy was higher for words ($M = 1.0$) than for facts ($M = .61$) or pictograms ($M = .43$), $F(2, 39) = 9.3$, $p < .001$. Similarly, the production check showed that words were easiest ($M = .96$, novel facts $M = .84$, pictograms $M = .60$). Thus, task demands favored word learners.

Production accuracy for novel items averaged .22 ($SD = .21$) for words, .20 ($SD = .14$) for novel facts, and .10 ($SD = .18$) for pictograms. Only 3 children ever drew an object. An ANOVA revealed no significant item type differences. The age covariate was nonsignificant.

*Generalization.* Same-category choices for learned items (at least two of three possible correct prior responses) were analyzed. In total, 9 children learned no items (4 word learners, 1 fact learner, and 4 pictogram learners) and were excluded. The remaining children averaged .92 ($SD = .18$), .91 ($SD = .21$), and .85 ($SD = .36$) same-category choices for words, facts, and pictograms, respectively. Most children chose all of the same-category exemplars. Means (corrected for overgeneralization) were compared by an ANOVA analogous to those above. The item type factor was nonsignificant, as was the age covariate. Generalization did not differ across object variants; children chose .90 ($SD = .43$) identical objects, .93 ($SD = .40$) different color objects, and .87 ($SD = .40$) different shape objects.

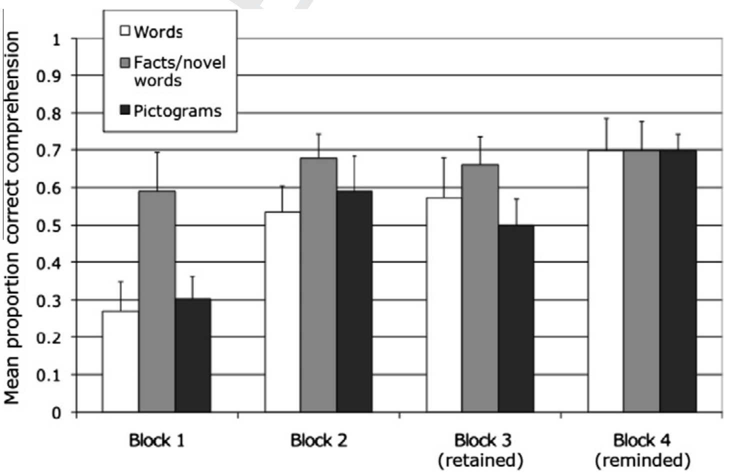


**Fig. 7.** Mean (± *SD*) comprehension accuracy in Experiment 3 by item type and block (Blocks 3 and 4 were in Session 2).

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*Session 2 comprehension.* Proportion correct after a week averaged .63 ($SD$ = .33), .68 ($SD$ = .25), and .60 ($SD$ = .24) for words, facts, and pictograms, respectively (Fig. 7). An ANOVA showed no significant item type or age effects and no interaction (all $F$s < 1) in either block. The reminder was effective; accuracy increased from .58 in Block 3 to .70 in Block 4, $t(41) = 2.5$, $p = .015$ (two-tailed). However, reminder-based gains did not differ by item type ($F < 1$). Session 2 accuracy was also analyzed in a stepwise regression with item type, age, parents' education, PPVT-III, and production check entered as predictors. Only vocabulary predicted significant variance, $\beta_{std} = .39$, $F(1, 28) = 5.0$, $p = .033$, $R^2_{adj} = .12$.

*Adults*

Adults were more accurate with novel pictograms than with words in Session 1, $t(18) = 2.4$, $p = .029$. However, by Session 2 they were at ceiling for both pictograms and words ($M$s = 1.0 and .94, respectively). Production accuracy was similar for words ($M = .78$) and pictograms ($M = .82$). Generalization means were high (.86) and similar for words and pictograms.

*Discussion*

The 3- and 4-year-olds showed few differences in learning words, novel facts, and pictograms. A notable exception was that children learned novel facts more than words from one exposure. This advantage disappeared, however, after a second exposure. The fast-mapping advantage is consistent with Experiment 1 (see Table 2).

Fact production was less accurate in Experiment 3 than in Experiment 1 ($M$s = 20% vs. 72%). This might be because the new facts' longer double-dative structure imposed a higher working memory load or because children had only two exposures to each item before the production test (compared with four exposures in Experiment 1). However, word production accuracy was fairly consistent, albeit low, across the three experiments (range = 14%–22%), suggesting that this measure was less sensitive to number of exposures. One interpretation is that fact production is particularly sensitive to factors including sentence content/structure and number of exposures.

Generalization did not differ across item types, further suggesting that children are not preferentially biased to generalize words, at least not compared with facts or pictograms. The result is somewhat difficult to interpret because of a ceiling effect. However, because the facts described plausibly object-specific properties—the location or origin of objects—the finding indicates a robust taxonomic bias for facts. Apparently, 3- and 4-year-olds assume that properties such as where an artifact came from and where it is kept are category-wide. This is reasonable; if told that a novel object "came from Taiwan," adults might infer that other similar objects came from Taiwan as well. Regardless, the results replicate Experiment 1 in disconfirming Waxman and Booth's (2000) claim that children generalize facts less than words.

The results further show that 3- and 4-year-olds consistently generalize pictograms to object categories. Thus, their tendency to generalize symbolic associates to categories is not restricted to lexical, or even verbal, items.

Receptive vocabulary was the only predictor of retention. This suggests that individual children's ability to learn new symbolic associations is correlated with their vocabulary growth. This is consistent with findings that children's vocabulary is correlated with overall verbal intelligence (Sattler, 1992).

## General discussion

To adult eyes, children's path to language seems to be traveled at a sprint. Children learn hundreds of words between 2 and 4 years of age, but it has remained unclear what specialized and/or general learning mechanisms are responsible for these gains. One question concerns fast mapping: What processes allow children to infer a word's meaning from very few exposures? Other questions concern learning biases: Is a mutual exclusivity bias stronger for words than for facts or pictograms? Are children especially predisposed to generalize novel words to taxonomically cohesive categories? The current study addressed these questions.



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### Fast mapping

The 3- to 5-year-olds learned words slower, by some measures, than facts or pictograms. In every experiment, early comprehension of facts was superior to that of words; in Experiments 1 and 2, pictograms were superior to words. This clarifies previous findings that 1-year-olds can fast map verbal or nonverbal symbols, whereas older toddlers more rapidly map lexical symbols (Namy, 2001). One possible interpretation is that fast mapping becomes specialized for words as children gain word-learning expertise. An alternative interpretation is that children become generally better at verbal learning as semantic knowledge, verbal memory, and sentence-processing skills improve. The current results favor the latter hypothesis, but neither one explains why preschoolers can fast map nonverbal pictograms as well as words in spite of limited pictogram-learning experience. It seems that fast mapping does not become specialized for verbal information (see also Moher, Feigenson, & Halberda, 2010).

The advantage of facts over words in comprehension was partly, but not entirely, related to lexical novelty. In Experiment 1, familiar facts showed an early block advantage over words, whereas novel facts did not show an advantage until later trials. In Experiment 3, however, the novel facts advantage over words was significant even in early blocks. However, there was no difference a week later. Thus, the magnitude of the fact advantage appears to depend on factors such as the content of the words or facts, number of exposures, and retrieval interval. Nevertheless, facts were generally amenable to fast mapping. One possible explanation is that facts include multiple meaningful words and phrases that can function as retrieval cues (Sadoski, Goetz, & Avila, 1995).

The results also show a sporadic and less marked advantage of pictograms over words. This cannot, however, be due to rich internal cue contexts. Another explanation is that the pictograms remained visible for a longer time than words were audible. Notably, when exposure time was controlled (Experiment 3), the pictogram advantage disappeared. Thus, duration of exposure might affect fast mapping. In sum, fast mapping might be modulated by several factors: richness of the associative cue context, number of exposures, and total encoding time.

Several less interesting explanations can be eliminated. First, video coding ascertained that children were attentive during each exposure. Second, word learners performed well on familiar items, so they understood the task. Third, production checks confirmed that words were easy to pronounce. Finally, the adult control group (Experiment 3) confirms that the word-learning task was not overly difficult.

### Taxonomic bias

The results disconfirm the claim that children are more biased to generalize words than facts to referent categories (Behrend et al., 2001; Waxman & Booth, 2000). No such result was obtained; to the contrary, in Experiment 1 children generalized words *less* than pictograms or facts. The simplest explanation is that words were weakly represented, and children are disinclined to generalize weakly represented items. Consistent with this, comprehension strongly predicted generalization accuracy independent of children's age or item types.

The results do not, however, indicate that children automatically generalize facts. Facts specify a functionally infinite range of relations among referents, and their generalizability will depend on content. Words also signify a vast range of referents, categories, and relations. Thus, it would be senseless to claim that facts per se, or words per se, differ in generalizability. Rather, any comparison must take into account the particular kinds of facts and words. We compared count nouns for objects with facts that indicated origins or locations and found that children generalized both of these types to similar objects. By contrast, for example, Behrend and colleagues (2001) found that children generalized facts less than words with some facts that described accidental properties. Thus, word or fact content, and the strength of the learned association, modulates generalizations. Whether an item is a word or fact per se does not.

This conclusion is at odds with Waxman and Booth's (2000) report that preschool children generalized novel facts less than words. The reason for the discrepancy is not clear because the methods differed in multiple ways. For example, our generalization array was larger, and the precise facts and stimuli differed. One notable procedural difference is that Waxman and Booth (p. B36) told children, "Look at this one. This one is SO special to me. And you know what? ["It is called a koba"]/["My uncle gave it to me"]." Perhaps emphasizing the phrase "*this* one" followed by the singular pronoun in "gave *it* to me" underscored the uniqueness of the object, whereas the indefinite noun phrase "*a* koba" implied a more general category. Thus, pragmatic cues might have compelled children to generalize words more than facts. More generally, the fact that different studies (Behrend et al., 2001; Namy, 2001; Waxman & Booth, 2000; current results) have shown every possible outcome (i.e., more word generalization, more fact generalization, and no difference) supports our claim that the specific content of words or facts, not whether an item is a word or fact per se, determines how children will generalize their mappings.

The pictogram generalization results provide additional insight. Although pictograms can function much like words, children have much less experience with pictograms. Most of the pictograms children see are public signage and brand symbols. Public signs (e.g., stop signs) seem to be no more specific than their corresponding labels ("stop signs"), although this can vary. Brand symbols (e.g., golden arches, Starbucks logo) are a bit more specific than the names because, for example, the word "McDonald's" can refer to the corporation, a specific location, products of the corporation, or possessions of people with the same names (e.g., Old McDonald's farm or his cow). The symbol, however, denotes only the corporation and its interests. Even if children do not appreciate this distinction, it seems that by 3 years of age they have inferred that pictograms are, in general, at least as likely as words to represent taxons. The taxonomic bias is not stronger toward words.

### *Mutual exclusivity bias*

In Experiment 2, children fast mapped items better if they mapped one-to-one onto objects. Critically, the one-to-one advantage did not differ in magnitude across item types. That is, there was no evidence that a mutual exclusivity bias was stronger for words than for facts or pictograms. Notably, the mapping scheme effect was not reliable in Experiment 1, and in Experiment 2 it was reliable only in early trials. Thus, there is an effect, albeit one that is small, context dependent, and rapidly resolved. This complements evidence of mutual exclusivity-like effects for various verbal and nonverbal materials (Diesendruck & Markson, 2001; Piccin & Blewitt, 2007). Apparently Anderson's (1976) "fan effect" extends to verbal *and* visual association learning in children, confirming that the so-called mutual exclusivity bias is not specialized for word learning (Markman, 1994). The sum of available evidence can be explained in terms of two general mechanisms. First is the tendency to associate phenomena based strictly on novel words (Merriman, Marazita, & Jarvis, 1995). Novelty detection can support the formation of associations (e.g., Randlich & Lolordo, 1979) and, thus, accounts for the effect most commonly attributed to a mutual exclusivity bias: the tendency to associate novel words with novel referents (Markman & Wachtel, 1988). Second, many-to-one associations might generate interference that triggers inhibitory processes. These processes take time and, therefore, impede learning. There is evidence of such inhibitory processes in basic animal associative learning (Kamin, 1969).

This latter mechanism implies a further prediction: Specifically, interference should increase as a function of cognitive load (Yonelinas, 1997), that is, the number of mappings to be learned. Additional interference, or entropy, should demand more inhibitory activity and increase error rates. This might explain why the mapping effect was significant in Experiment 2, where the many-to-many group learned four (not three) items and objects, but not in Experiment 1 (see also Liittschwager & Markman, 1994). In sum, novelty-based association, and inhibition of associative interference, can explain so-called mutual exclusivity effects in this study and previous studies.

### *Learning principles: General or specialized?*

The results suggest that word learning follows general phenomena and factors such as novelty detection, study time, and inhibitory processes. The results also suggest that symbol learning (Deacon, 1997) is not specialized for words even as children become more experienced word learners. The learning processes apply to a range of symbolic information.



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The results also add to our knowledge of repetition effects in children's word learning. Repetition enhances word learning in children and adults (Medina, Snedeker, Trueswell, & Gleitman, 2011). Our results show incremental learning but with non-uniform effects; for example, only in Experiment 3 were there reliable gains from the first to the second exposure. In addition, children in Experiments 1 and 2 benefited from a third and/or fourth exposure (i.e., early to later blocks), but in Experiment 2 these gains were smaller for words than for pictograms. Another repetition effect was obtained in delayed retrieval; providing one reminder exposure produced significant gains in comprehension accuracy. This finding supplements the scant research literature on reminding effects in preschoolers' learning.

It should be noted that children were tested repeatedly in every experiment, and learning might have been influenced by the demand to produce an overt response. This possibility requires further study. Notably, there is indirect evidence of performance effects in the production data; children produced more items after four exposures (Experiments 1 and 2) than after two exposures (Experiment 3). This fits prior reports that fast mapping does not support production (Dollaghan, 1987) and that productive accuracy increases with repetition (Cowan, 1988). It is also consistent with the advantage of comprehension over production in children's language (Clark & Hecht, 1983). Presumably, the comprehension task merely required an association between any distinctive property of the item (e.g., phoneme or content word) and the referent, whereas production required a more detailed representation of a number of properties. This representational strength account is the same as the explanation for reduced word generalization in Experiment 1 and for the correlation between comprehension and generalization.

This study also explored the differences between pictogram production and word production following fast mapping. Children produced more pictograms than words in Experiment 1 and produced marginally more pictograms than words in Experiment 2. Any comparison must be treated Q12 cautiously given the many differences of perceptual motor demands entailed by naming and drawing. Nonetheless, the results document that preschool-age children can learn novel pictorial symbols well enough from a few exposures to subsequently produce them, at least as they can Q13 reproduce words.

In sum, the results confirm significant learning of verbal and nonverbal symbolic items within a few exposures. The course of learning is affected by factors of repetition and delay. However, these factors were not systematically and consistently distinct for words, facts, and pictograms.

The results do not support the popular image of children as uncannily fast word learners. In fact, we do not know how many repetitions children *typically* need to learn words. In addition, there is no systematic documentation of prolific fast mapping of words by children in natural environments. To the contrary, Deák and Wagner (2003) found, in a rich play-like teaching session, that preschool children's word learning was slow and inaccurate. It is possible that researchers' overreliance on stripped-down experimental tests and a single simple measure of learning (i.e., one-trial forced-choice comprehension) has spawned an unfounded image of children as precocious word learners.

Although the results show some word-specific learning effects, words were learned slower than facts or pictograms. We speculate that it is not children's learning processes that are specialized rather the lexicon that is specialized as a unique sort of body of information to be learned. The lexicon is peculiar because (a) it is very large and deeply related to all conceptual knowledge, (b) it has a complex internal structure and patterns (e.g., morphology), (c) its referential mappings are confusable and underdetermined, and (d) it requires very diverse kinds of generalizations. These properties make fast mapping implausible as a normative mode of learning; it would yield too many incorrect word meanings. Children must eventually learn thousands of diverse meanings, and too often they will need to accrue a large number of input exemplars before converging on a word's meaning. It seems that fast mapping is a *general* learning capacity that occasionally—usually in very simple contexts—extends to word learning.

## Uncited references

Q14 Brown and Scott (1971), Deák et al. (2002) and Gray (2004).



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