

# Linking Rule Acquisition in Novel Phrasal Constructions

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All natural languages rely on sentence-level form-meaning associations (i.e., linking rules) to encode propositional content about who did what to whom. Although these associations are recognized as foundational in many different theoretical frameworks (Goldberg, 1995, 2006; Lidz, Gleitman, & Gleitman, 2003; Pinker, 1984, 1989) and are—at least in principle—learnable (Allen, 1997; Morris, Cottrell, & Elman, 2000), very little empirical work has been done to establish that human participants are able to acquire them from the input. In the present work, we provided adult participants with 3 min worth of exposure to a novel syntactic construction and then tested to see what was learned. Experiment 1 established that participants are able to accurately deploy newly acquired linking rules in a forced-choice comprehension task, and that constructional knowledge largely persists over a 1-week period. In Experiment 2, participants were exposed to the linking rules immanent in one of two novel constructions and were asked to describe novel events using their exposure construction. The data indicate that participants were successful in using their exposure construction's linking rules in production, and that performance was equally good regardless of the specifics of the target linking pattern. These results indicate that linking rules can be learned relatively easily by adults, which, in turn, suggests that children may also be capable of learning them directly from the input.

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## Introduction

Grammar allows speakers to encode and decode semantic relationships in sentences—to identify who did what to whom—in a number of ways. For a transitive event in which an agent acts on a patient, for example, English links the agent to the subject position of an active sentence and links the patient to the object. Likewise, in Latin, the same agent-patient relationship is signaled through the use of nominative case marking on the agent and accusative case marking on the patient. The grammatical knowledge that underlies these sorts of syntax-semantics mappings goes by various names: linking rules (Pinker, 1984, 1989), mapping principles (Lidz, Gleitman, & Gleitman, 2003), or argument structure constructions (Goldberg, 1995, 2006). And, although there is widespread agreement that mapping knowledge is deployed in online sentence production (Ferreira & Slevc, 2007; Levelt, 1989) and comprehension (McRae, Spivey-Knowlton, & Tanenhaus, 1998), there is substantial disagreement concerning its origins.

According to *nativist* mapping theories, syntax-semantics associations exist as part of an inborn linguistic endowment known as Universal Grammar (UG; Baker, 1988; Lidz et al., 2003; Pinker, 1984, 1989); that is, speakers' linguistic competence includes knowledge of linking rules because they are *born* with knowledge of linking rules. In contrast, *constructionist* theories propose that mapping generalizations are *learned* from the input, with learning constrained by pragmatics, categorization principles, attentional biases, and other domain-general factors (Casenhiser & Goldberg, 2005; Goldberg, 2004; Morris, Cottrell, & Elman, 2000; Perfors, Kemp, Tenenbaum, & Wonnacott, 2007; Tomasello, 2003). Both of these approaches agree that syntax-semantics mappings are subject to constraints and biases. They disagree, however, concerning the locus of these constraints—whether they are innate and specific to language or whether they emerge from domain-general cognitive processes.

Nativist mapping theories are attractive in that they provide a straightforward way to account for certain crosslinguistic tendencies in argument realization. For example, prominent semantic arguments (e.g., agents) tend to be expressed in prominent syntactic positions (e.g., subject position). Moreover, nativist mapping approaches have played a large role in the literature on semantic and syntactic bootstrapping. Both of these theories assume preexisting mapping knowledge that children use to guide grammatical and word learning (Landau & Gleitman, 1985; Pinker, 1984, 1989). In Pinker's work, for instance, learners bootstrap into the grammar by associating semantic categories like agent and patient with syntactic categories like subject and object. These

associations are specified in innate linking rules, which are predicted to have a uniform effect across all normally developing children learning any of the world's languages. Unfortunately, however, Pinker provides no evidence to indicate that an innate mapping account should be preferred to an account in which mappings are constrained by domain-general factors (e.g., Goldberg, 2004, 2006). Instead, the hypothesis that linking rules are innately specified in UG is treated as an assumption that is required to get grammar learning off the ground.

Naigles, Gleitman, and Gleitman (1993) called this sort of postulate the "weakest link" in semantic and syntactic bootstrapping proposals and write that because

some of these correspondence [linking] rules vary cross-linguistically . . . it is not possible to say that the learner is provided with all of them by nature—as part of the language faculty . . . . Clearly the topic of how correspondence [linking] rules develop cries out for investigation, and has not been resolved or even addressed by us. (pp. 136–137)

Along the same lines, Bowerman (1990) offers a critique of nativist mapping theories in which she notes that because the syntax-semantics associations that are present in many of the world's constructions violate putative innate linking rules, nativist theories must posit a learning mechanism to acquire noncanonical linkings. Because the learning mechanism would compete against a UG bias that favors the innate linking patterns, this predicts that noncanonical constructions should be acquired later by children. As Bowerman noted, however, naturalistic corpus data fail to support this prediction.

Although this sort of finding tends to suggest that nativist mapping theories are less than empirically adequate, it is not the case that learning theories are able to offer a perfectly convincing story about the development of mapping generalizations either. The emphasis from constructionists has been on the conservative nature of children's early learning, with demonstrations focusing on children's failure to generalize beyond the input until they have been exposed to a vast amount of data at age 3.5 or beyond (Akhtar & Tomasello, 1997; Braine, 1976; Ingram & Thompson, 1996; Lieven, Pine, & Baldwin, 1997; for reviews, see Tomasello, 2000, 2003). The implication of this work is that constructions must be learned, because they are acquired so late and in such a piecemeal fashion.

Likewise, a number of computational models have demonstrated that constructions and the mapping generalizations that they specify are, *in principle*, learnable from the input without the need for specifically linguistic constraints

(Allen, 1997; Morris et al., 2000). However, although these models suggest that children can learn mapping generalizations—as does the data on children's early conservative behavior—they do not conclusively prove that this is the case. Such a conclusion would be bolstered by studies that demonstrate actual construction learning in a controlled experimental setting. There has, however, been precious little work along these lines, and the studies that have addressed this issue have not definitively demonstrated that participants learn to associate specific syntactic positions with specific semantic arguments.

The current work builds on a series of earlier studies in the area of novel construction learning (Casenhiser & Goldberg, 2005; Goldberg, Casenhiser, & Sethuraman, 2004; Goldberg, Casenhiser, & White, 2007). In these experiments, adults and 6-year-olds were exposed to a phrasal construction that paired a novel *form* with a novel *meaning* (cf. studies in which only the form is novel: Akhtar, 1999; Ambridge, Theakston, Lieven, & Tomasello, 2006; Wonnacott, Newport, & Tanenhaus, 2008). In a subsequent forced-choice comprehension task, both groups proved able to distinguish new instances of the novel construction from known construction types. Learning was very fast—participants received only 3 min of exposure, which is reminiscent of fast mapping in word learning (Carey & Bartlett, 1978)—and, interestingly, acquisition was facilitated when participants were exposed to a low-variance input sample centered around a particular novel verb. This effect has been observed in nonlinguistic learning (Elio & Anderson, 1984; Posner, Goldsmith, & Welton, 1967), is consistent with an underlying general-purpose learning mechanism (Borovsky & Elman, 2006; Perfors et al., 2007), and is potentially quite useful in language development, as many constructions are represented by a handful of high-frequency exemplars in the input, which presumably form a low-variance nucleus that seeds category development (Ellis & Ferreira-Junior, 2009; Goldberg et al., 2004; Zipf, 1935).

One important limitation of this earlier work was that it did not explore whether learners acquired mapping knowledge that links specific semantic arguments to specific syntactic positions: The results are consistent with participants having learned the novel construction as a global gestalt, without having attached any particular significance to the semantic roles being played by its different nominals. The current work addresses this shortcoming in two experiments with adult learners. The experiments rely on a new comprehension measure (Experiment 1) and a production task (Experiment 2) to assess whether the specifics of constructional linking rules have been acquired. They additionally explore three other issues: whether novel constructions—like

natural language constructions—are stored in long-term memory (Experiment 1); whether the learning process is biased such that some constructions are easier to learn than others (Experiment 2); and whether novel constructions count as real language (Experiments 1 and 2).

## Experiment 1

The primary goal of Experiment 1 was to investigate whether syntax-semantics mappings can be learned when participants are given brief exposure to a novel syntactic construction. After exposure, participants were asked to listen to brand new exemplars of the construction and choose which of two movies depicted its meaning. Crucially, the movies demonstrated reversible actions, and so specific mapping knowledge was required for above-chance performance.

A secondary goal of Experiment 1 was to determine whether the knowledge that participants obtain from exposure to a novel construction can be maintained over time. If the constructional knowledge that is acquired is short-lived, then this would seem to go against a category-based learning account (e.g., Goldberg et al., 2007), because memory traces of experience with constructional exemplars need to be stored to form the basis of an abstract constructional category. In order to determine whether memory for novel constructions is maintained over time, we tested participants' comprehension immediately following exposure, and at a 1-week lag.

## Participants

Thirty-two undergraduate native speakers of English were recruited from the Princeton University Department of Psychology subject pool and took part in Experiment 1 in exchange for course credit. Each participant was randomly assigned to either an experimental condition or a control condition.

## Novel Construction

The construction that participants in the experimental condition were exposed to describes events in which objects appear at a location, and takes the form  $NP_1 NP_2 V$ , where  $NP_1$  is the appearing object (the *theme*),  $NP_2$  is the location at which  $NP_1$  appears (the *locative*), and  $V$  is a nonce verb that describes a manner of appearance. The sentence *The bird the flower moopos*, for example, signifies an event in which a bird (the theme) magically fades into view on top of a flower (the locative).

All instantiations of the novel construction that were used in the present experiment paired two English definite NPs (e.g., *the bird* and *the flower*) with

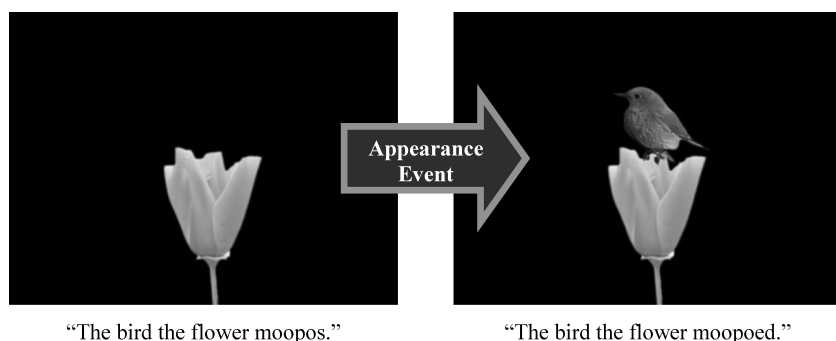
a verb ending in *-o* (e.g., *moopo*). Previous work using this paradigm has indicated that constructional acquisition is facilitated when learners are given morphological cues to construction meaning on the verb (Casenhiser & Goldberg, 2005). Note, however, that whereas the *-o* suffix may help participants identify the sentences that they hear as belonging to the class of constructions signifying appearance, it carries no information about which NP is the theme and which is the locative. In order to learn about these features of the novel construction, participants must instead attend to word order cues.

Twenty-eight exemplars of the novel construction were generated for use in Experiment 1. Participants were familiarized with 16 of these in an initial *exposure* block. In the following *test* block, their comprehension ability was tested on 12 new exemplars of the construction. Crucially, there was no lexical overlap between the constructional exemplars in the two blocks—that is, an entirely new set of nouns and verbs was used in the test block items. This means that in order for participants to do well at test, they could not rely on lexically specific details of the novel construction, but instead had to depend on its more abstract features.

## Exposure

Participants in the experimental condition were exposed to the novel construction in the context of 16 short, computer-animated movies. Each movie began by showing a construction's locative argument at the center of the screen. The participant then heard a present tense exemplar of the novel construction (e.g., *The bird the flower moopos*). Following this, the construction's theme argument magically appeared at the location. The movie then ended with a final past tense exemplar of the construction (e.g., *The bird the flower moopoed*). Figure 1 provides a storyboard for an exposure movie. All exposure movies were approximately 12 s in duration.

Previous work on the learning of both novel and attested constructions has shown that acquisition is facilitated when the overall similarity of the constructional exemplars used during exposure is increased (i.e., when variance in the input sample is decreased; Casenhiser & Goldberg, 2005; Goldberg et al., 2004, 2007; Maguire, Hirsh-Pasek, Golinkoff, & Brandone, 2008). In order to take advantage of this feature of the learning mechanism in the present experiment, the overall similarity of the items in the exposure block was increased in the following way. Five different novel verbs (*moopo*, *feigo*, *suuto*, *vako*, and *keybo*) were used in the constructions that occurred in exposure movies, but their token frequencies were skewed so that *moopo* was used in half of the exposure movies (i.e., eight) and the remaining four verbs were evenly divided among



**Figure 1** In the exposure block, participants viewed short movies in which present and past tense exemplars of the novel construction were paired with on-screen appearance events (e.g., a bird magically fading into view on top of a flower, as in the before-and-after frames shown here).

the remaining eight exposure movies. This type of input is ecologically valid, as tokens of argument structure constructions are typically overrepresented by examples involving a particular verb (Ellis & Ferreira-Junior, 2009; Goldberg, 2006; Goldberg et al., 2004; Zipf, 1935).

Each verb in the exposure movies was associated with a distinct manner of appearance. The different manners all had a magical quality, in that there was never an obvious causal agent associated with the appearance act. *Moopo*, for example, described appearance events in which the theme argument faded into view, whereas *keybo* described events in which the theme appeared from behind a cloud of sparkles.

A control condition was included in Experiment 1 to verify that good performance at test in the experimental condition was due to learning that occurred as a result of exposure to the novel construction. The control condition featured the same movies used in the experimental condition, but with the novel construction replaced by voiceovers in which the theme and locative arguments were named, in turn, by nouns. Each control participant viewed half of the 16 exposure movies paired with theme-locative utterances (e.g., *bird . . . flower*) and the other half paired with locative-theme utterances (e.g., *flower . . . bird*). This ensured that the control condition's voiceovers were related to what was shown in the movies, but that there was no consistent mapping pattern available to be learned. The order of argument presentation was additionally counterbalanced across participants so that, for example, the movie shown in Figure 1 occurred with *bird . . . flower* for half of the control participants and with *flower . . . bird* for the other half.

## Testing

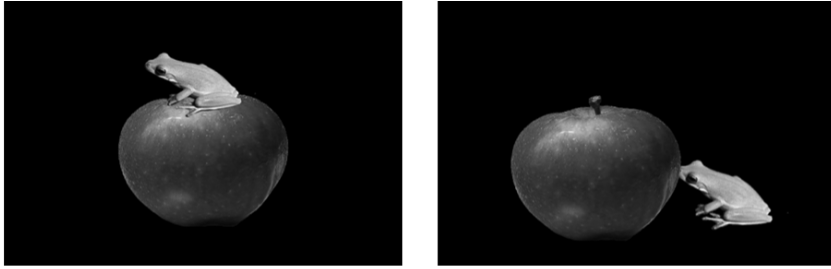
Test trials contained three elements: a voiceover consisting of a new sentence that had not been heard during exposure, and two movies played simultaneously side-by-side—a target movie and a distractor. The target movie depicted the event described by the voiceover sentence, and the distractor movie depicted an alternative event. For each test trial, participants were instructed to listen to the voiceover sentence and then point to the movie that matched it. Test movies looped indefinitely, and participants were instructed to watch them as many times as necessary before responding. Correct responses were points to the target movie; incorrect responses were points to the distractor movie.

The test block contained three different trial types: appearance, transitive, and mapping. In *appearance* trials, the voiceover was an exemplar of the novel construction (e.g., *The frog the apple zoopos*). This sentence occurred with a target movie depicting an appearance event (e.g., a frog appearing on an apple) and a distractor movie depicting a transitive event (e.g., a frog pushing an apple). *Transitive* trials were structurally identical to appearance trials, except that now the voiceover featured a transitive sentence with a novel verb (e.g., *The dog zats the chair*). The target movie showed a transitive event (e.g., a dog pushing a chair), and the distractor movie showed an appearance event (e.g., a dog appearing on a chair). Transitive trials served as a control to ensure that good performance on appearance trials did not occur because participants had a general preference for appearance movies. Figure 2 gives examples of appearance and transitive trials.

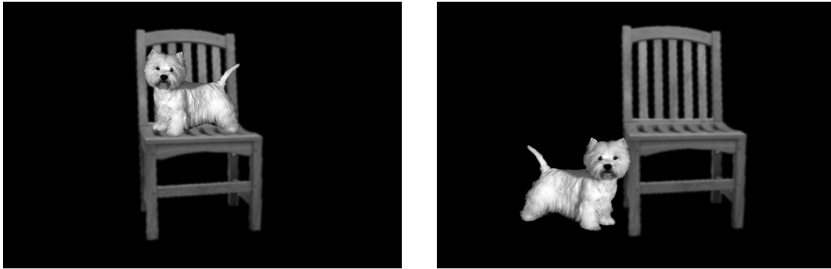
Although above-chance performance on both appearance and transitive trials would indicate that participants who received brief exposure to the novel construction are able to distinguish it, formally and semantically, from a known construction type, this would not guarantee that mappings from specific syntactic positions to specific semantic arguments (i.e., linking rules) had been learned. In order to get at this question, Experiment 1 also featured *mapping* trials. These had the same tripartite structure as transitive and appearance trials, but utilized reversible events that made above-chance performance impossible in the absence of linking rules. Figure 3 shows an example mapping trial.

Over the course of the test block, each participant saw six appearance trials, six mapping trials, and six transitive trials, with nine target movies appearing on the left and nine appearing on the right. For both appearance and mapping trials, the exemplars of the novel construction that participants heard contained no nouns or verbs that had been seen during the exposure block. This ensured that correct responding could only occur on the basis of constructional





“The frog the apple moopos.”



“The dog zats the chair.”

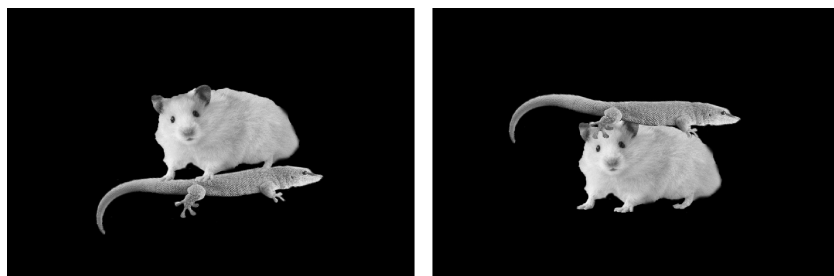
**Figure 2** In appearance trials (top panel), participants heard the novel construction and viewed simultaneously displayed movies showing an appearance target (e.g., a frog magically appearing on an apple, left) and a transitive distractor (e.g., a frog pushing an apple, right). In transitive trials (bottom panel), participants heard a transitive construction and viewed simultaneously displayed movies showing a transitive target (e.g., a dog pushing a chair, right) and an appearance distractor (e.g., a dog magically appearing on a chair, left).

representations that abstracted over the item-specific details of the exposure exemplars.

### Procedure

Participants were tested individually. At the beginning of the exposure block, each participant was instructed to pay attention to the exposure movies. They then viewed all 16 movies in different random orders.

At the beginning of the test block, participants were instructed to listen to the voiceover sentences in the test trials and to point to the onscreen movie that depicted the event described in the sentence. They were additionally encouraged to view each test trial as many times as necessary before responding (i.e., before pointing to one movie or the other).



**Figure 3** In mapping trials, participants heard the novel construction and viewed simultaneously displayed movies showing reversible appearance events. Given the sentence *The hamster the lizard maytos*, for example, participants would choose between a movie in which a hamster magically appeared on top of a lizard (the target, left) and a movie in which a lizard magically appeared on top of a hamster (the distracter, right).

The 18 test trials were divided into two halves, with each half balanced in terms of the number of appearance, transitive, and mapping trials it contained. Each half started with a set of randomly interleaved transitive and appearance trials (three of each) and ended with three randomly ordered mapping trials. For each test item, the half in which it appeared was counterbalanced across participants. Participants in the control condition were administered both halves back-to-back, immediately after the exposure block. For experimental participants however, one half was administered immediately after exposure and the other was administered after a 1-week interval. Test instructions were repeated before testing in the 1-week session.

## Results

We performed two analyses over the data. The first aimed to determine whether exposure to the novel construction in the experimental condition led to better performance at test relative to controls. Because participants in the experimental group were tested at two lags—immediately after exposure and at a 1-week delay—whereas participants in the control group were tested at the immediate lag only, we excluded all of the experimental group's 1-week data from the comparison between the two groups. This eliminated test lag as a potential confound. Mean percent correct scores were calculated on a participant-by-participant basis from the remaining data, and are summarized by condition and trial type in Table 1.

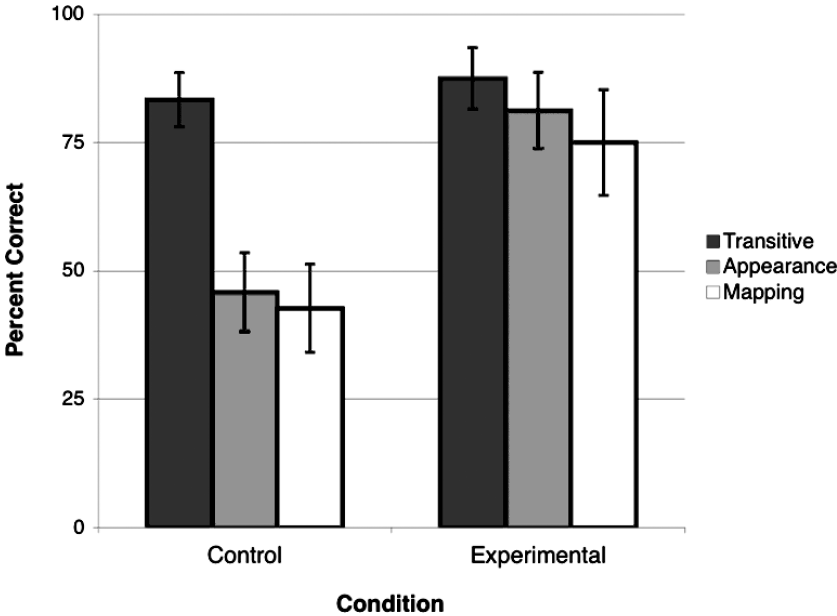
The Table 1 means were submitted to a  $2 \times 3$  ANOVA with condition (control vs. experimental) as a between-participants variable and trial type

**Table 1** Mean percent correct by condition and trial type

Trial type	Condition	
	Control	Experimental
Transitive	83.33	87.50
Appearance	45.83	81.25
Mapping	42.71	75.00

(transitive vs. appearance vs. mapping) as a within-participants variable. The results show a main effect of condition,  $F(1, 30) = 15.87, p < .001$ , demonstrating that experimental participants did learn on the basis of brief exposure. There was, additionally, a main effect of trial type,  $F(2, 60) = 6.43, p < .01$ , and a marginal interaction of condition and trial type,  $F(2, 60) = 2.37, p = .10$ , suggesting that the effect of condition may not have been equivalent across the different types of test trials. To investigate this possibility, a series of two-tailed Welch  $t$  tests—which correct for unequal group variances by adjusting the degrees of freedom—was conducted. As expected, participants in the control and experimental groups performed similarly when tested on the already familiar transitive construction,  $t(29.52) = -0.52, p = .61$ , but the experimental group outperformed controls on both appearance trials,  $t(29.97) = -3.31, p < .01$ , and mapping trials,  $t(29.06) = -2.40, p = .02$ . This pattern of results is depicted graphically in Figure 4. Equivalent performance on transitive trials presumably reflects similar levels of experience with the transitive construction. The experimental group's significantly better performance on appearance and mapping trials, however, suggests a positive effect of exposure to the novel construction (i.e., learning).

We additionally evaluated each of the cells in the design relative to chance. The six means specified in Table 1 were compared to a hypothesized mean of 50% using a series of two-tailed  $t$  tests. The results show that transitive trials were statistically above chance in both the control group,  $t(15) = 6.32, p < .0001$ , and the experimental group,  $t(15) = 6.26, p < .0001$ . In contrast, controls were at chance on both types of trials that required knowledge of the novel construction [appearance:  $t(15) = -0.54, p = .60$ ; mapping:  $t(15) = -0.85, p = .41$ ], whereas experimental participants were above chance on the same trials [appearance:  $t(15) = 4.20, p < .001$ ; mapping:  $t(15) = 2.42, p = .02$ ]. Together with the paired  $t$  test results reported here, this outcome demonstrates learning in the experimental condition that is sufficient to distinguish the experimental group both from controls, and from chance.



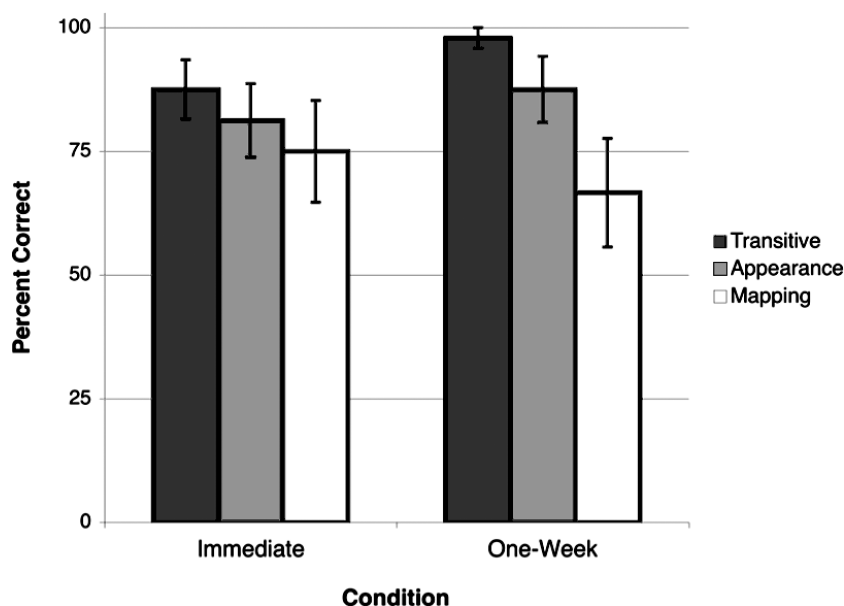
**Figure 4** Participant performance on transitive, appearance, and mapping trials in the control and experimental conditions. Error bars show the standard error of the mean.

Our second analysis of the Experiment 1 data aimed to evaluate whether participants’ knowledge of the novel construction persisted significantly beyond exposure. To answer this question, we calculated mean percent correct scores for each of the experimental participants according to trial type and test lag. A summary of the relevant means is given in Table 2.

Participants were submitted to a 2 × 3 ANOVA, with test lag (immediate vs. 1 week) and trial type (transitive vs. appearance vs. mapping) as within-participants factors. The results show a main effect of trial type,

**Table 2** Mean percent correct by test lag and trial type

Trial type	Test lag	
	Immediate	1-Week
Transitive	87.50	97.92
Appearance	81.25	87.50
Mapping	75.00	66.67



**Figure 5** Participant performance on transitive, appearance, and mapping trials immediately after exposure and at a 1-week delay. Error bars show the standard error of the mean.

$F(2, 30) = 3.44, p = .045$ , with participants performing best on transitive trials, then appearance trials, then mapping trials, and a null effect of test lag,  $F(1, 15) = 0.15, p = 0.71$ , suggesting that there was no overall decrement in performance from immediate testing to testing 1 week later. Additionally, there was a null interaction of test lag and trial type,  $F(2, 30) = 1.29, p = 0.29$ , which is consistent with the notion that there was no decrement in performance from the immediate to 1-week lags, even when considering the different trial types individually. Figure 5 summarizes these results.

Although the null effect of test lag combined with the null interaction suggests that constructional knowledge did not decay over the 1-week period, even when broken down by trial type, we felt that it was important to evaluate performance in each of the cells of the design according to chance. To this end, the Table 2 means were compared to a hypothesized mean of 50% using a series of two-tailed  $t$  tests. As noted previously, this analysis showed above-chance performance on all trial types when testing was conducted immediately after exposure. When testing was conducted at a 1-week lag, however, transitive and appearance trials were still above chance [transitive:  $t(15) = 23$ ,

$p < .0001$ ; appearance:  $t(15) = 5.58, p < .0001$ ], but mapping performance was not statistically above chance,  $t(15) = 1.52, p = .15$ . This suggests that the specifics of syntax-semantics mappings may in fact decay if not reinforced.

### Experiment 1 Discussion

The experimental group in Experiment 1 performed reliably better than the control group on exactly those trials that tested knowledge of the novel construction. This outcome replicates the finding from previous studies that even brief exposure to a novel construction can have a facilitatory effect (Casenhiser & Goldberg, 2005; Goldberg et al., 2004). It significantly extends this line of work, however, by documenting above-chance performance on mapping trials—a trial type that had not been used in previous work. Crucially, these trials required participants to associate an exemplar of the novel construction to one of two reversible appearance events. That participants were able to correctly do so at above-chance levels is most straightforwardly explained by their ability to rapidly acquire linking rules that map the first NP of the novel construction to its theme argument, and the second NP to its locative argument. The present study thus provides the first demonstration that the linking rules associated with a novel construction can be quickly and accurately learned from the input.

The present results additionally bear on the question of how long-lasting the knowledge acquired through brief exposure to a novel construction is. We find that the *general* association of NP<sub>1</sub>NP<sub>2</sub>V forms with appearance events is robust after 1 week, both when compared to performance at the immediate lag and when compared to chance. At the same time, although direct comparison between mapping trial performance at the immediate and 1-week lags showed no significant difference, we found that only performance at the immediate lag was statistically above chance. Note that specific constructional knowledge in the form of linking rules is presumably more difficult to learn than the general association between NP<sub>1</sub>NP<sub>2</sub>V forms and appearance events. Linking rules are not mastered immediately in naturalistic first language learning (Tomasello, 2000), and they are likely difficult to learn robustly in the present experiment, in which exposure to the novel construction was, by design, quite limited. Having to acquire more detailed knowledge over a very brief time period may have led to incomplete learning or to the acquisition of complete representations that were less entrenched and, therefore, not as stable over time. These less robust representations may have been sufficient to support above-chance performance on mapping trials at the immediate lag but not at the 1-week lag. Further testing

is required to determine what kinds of exposure conditions are necessary to support the development of linking rules that are more persistent over time.

## Experiment 2

The question of whether linking rules are learned can also be addressed using a production measure. In Experiment 2, participants were given brief exposure to one of two novel appearance constructions and were then asked to describe entirely new appearance events using their exposure construction. If participants are able to quickly associate specific semantic arguments with specific syntactic positions, then they should produce their exposure construction's two arguments in a fixed, nonrandom order that mirrors the input that they received. Such behavior would constitute important corroborating evidence in favor of the argument that linking rules can be quickly learned.

Experiment 2 additionally provides data bearing on *flexibility* in the acquisition of linking rules, in that the two novel constructions that participants were exposed to had identical meanings but different linking patterns. If participants perform similarly on both constructions at test, then this would suggest that the learning mechanism is able to flexibly acquire different sorts of linking rules in a very short time. Alternatively, if one construction outperforms the other, then this may be evidence that some sort of bias is operative in linking rule development.

Finally, the presence of good production abilities in Experiment 2 would place on much firmer footing the argument that novel constructions count as real language, and that the type of learning that participants are undertaking in novel construction learning paradigms is specifically linguistic. If Experiment 2's participants can use a novel construction to describe new events, then this—along with the comprehension data from Experiment 1—would demonstrate that novel constructions are functionally equivalent to natural language constructions.

## Participants

Seventy-two adult native speakers of English were recruited from the Princeton University Department of Psychology subject pool and participated in Experiment 2 in exchange for course credit. They were randomly assigned in equal numbers to control, theme-locative-verb, or locative-theme-verb conditions.

## Novel Constructions

Experiment 2 utilized two novel constructions. In the theme-locative-verb (TLV) condition, participants received brief exposure to the same NP<sub>1</sub>NP<sub>2</sub>V

construction used in Experiment 1, in which NP<sub>1</sub> mapped to the construction's theme argument and NP<sub>2</sub> mapped to its locative argument. In the locative-theme-verb (LTV) condition, participants received exposure to roughly the same construction, but with the linking pattern reversed so that NP<sub>1</sub> now mapped to the locative argument and NP<sub>2</sub> mapped to the theme.

The experiment made use of eight exemplars of the TLV construction and eight exemplars of the LTV construction, which were created by reversing the order of the two NPs in the TLV exemplars.

### Exposure Movies

The exposure movies used in Experiment 2 followed the same format shown in Figure 1—a present tense exemplar of a novel construction, followed by an onscreen appearance event, followed by the same exemplar in the past tense. The Experiment 2 movies, however, differed from those used in Experiment 1 in that they were live-action (not computer animated) and were fewer in number. In Experiment 1, participants had 16 exposure trials and saw different movies on each trial. Experiment 2 also had 16 trials, but now each participant saw eight different movies shown twice each in random order. These changes did not result in noticeably different learning outcomes and were implemented solely in order to make the results of Experiment 2 more directly comparable with the results of other studies using the same paradigm (e.g., Casenhiser & Goldberg, 2005; Goldberg et al., 2004).

Additionally, the same frequency structure from Experiment 1 was adopted in Experiment 2: Half of the constructional tokens seen during exposure featured the nonce verb *moopo*, whereas the remaining tokens were evenly divided among four other verbs (*feigo*, *suuto*, *vako*, and *keybo*).

Control participants in Experiment 2 watched the same movies that participants in the TLV and LTV conditions did, but with noun-noun voiceovers that consisted of a theme argument followed by a locative argument in half of the trials, and the opposite order in the other half. As in Experiment 1, this ensured that no consistent linking patterns were extractable from the input in the control condition.

### Production Trials

Immediately after the exposure block, participants took part in a production block in which they were shown a series of novel appearance events in the context of magic tricks performed by the experimenter, and were asked to describe the tricks using the “same kinds of sentences” heard during exposure. For each trick, the experimenter first introduced the items used in the trick—a theme item



and a locative item. The theme item was then hidden—out of the participant's view—inside the locative item. Finally, the experimenter uttered *Abracadabra!*, and the theme item “magically” appeared from (or in) the locative item.

Three different tricks were used in the experiment, always in the same order: a handkerchief appeared from a cloth bag, a quarter appeared in a wooden box, and then a lollipop appeared from a different bag. The order in which the items used in each trick were introduced was counterbalanced across participants as a means of eliminating possible effects of introduction order.

### Procedure

Participants were tested individually and took part in an exposure block followed by a production block. The exposure block began with instructions to pay attention to the exposure movies; participants then viewed 16 exposure movies in different random orders. Total exposure time was roughly 3 min.

For the production block, participants were instructed to provide descriptions for the magic tricks noted above using the “same kinds of sentences” seen during exposure. Participant utterances were audio-recorded for later coding and analysis.

### Analysis and Results

For utterances produced by participants in the TLV and LTV conditions, our primary interest was in whether the linking rules associated with each construction had been learned. To this end, we coded the relative order in which the two NP arguments were produced. Utterances that consisted of a theme argument followed by a locative argument followed by a verb were coded as theme-before-locative (TL). Similarly, utterances that consisted of a locative argument followed by a theme argument followed by a verb were coded as locative-before-theme (LT). For a minority of utterances, participants either failed to produce both arguments, incorrectly realized arguments as PPs, or produced transitive SVO orders. All of these utterance types were coded as other (O).

The control group provides a baseline measure of participants' syntax-semantics mapping tendencies. Reliable deviations from this baseline in the TLV and LTV groups would indicate that significant learning of linking patterns had occurred. Because exposure in the control condition consisted of hearing the exposure movies described by noninformative noun-noun utterances, we treated all noun-noun combinations produced at test as potentially following the example set during exposure. Noun-noun combinations consisting of a theme followed by a locative were coded as TL. Noun-noun combinations consisting

**Table 3** Frequency distribution of participants by condition and response type

Condition	Response type		
	Theme-before-locative (TL)	Locative-before-theme (LT)	Other (O)
TLV	20	3	1
LTV	3	12	9
Control	11	9	4

of a locative followed by a theme were coded as LT. Utterances that followed the noun-noun pattern but failed to mention both the theme and the locative arguments shown in a magic trick were coded as O. Likewise, all utterances that failed to follow the noun-noun pattern were coded as O. For the most part, these consisted either of normal English descriptions of the magic tricks (e.g., “The lollipop came out of the bag”) or of descriptions that failed to indicate a preferred argument order (e.g., “Handkerchief bag . . . bag handkerchief”).

To enable the use of nonparametric statistics, we then categorized all participants as either TL-responders, LT-responders, or O-responders based on the type of utterance produced on the majority of their test trials (i.e., on at least two out of their three trials). If, for example, a participant described the first magic trick that they saw with an LT utterance and the second and third magic tricks with TL utterances, they were coded as a TL-responder. This method of categorization was sufficient to classify 71 of the 72 participants in Experiment 2. The single remaining participant who produced one TL utterance, one LT utterance, and one O utterance was classified as an O-responder. The result of this categorization process was a frequency distribution of participants, organized according to condition and response type, as in Table 3.

In the analyses that follow, we employed Pearson’s chi-squared test and Fisher’s exact test to determine what kinds of patterns are discernable in the Table 3 data. The chi-squared test was used whenever all of the expected values in a contingency table analysis were at least 5. For all other cases, we relied on Fisher’s exact test.

### *O-Responders*

Before assessing whether participants in the TLV and LTV conditions were able to, with minimal input, learn the linking rules associated with their respective constructions, we first excluded O-responders from consideration. This was motivated by the possibility that O-responders may have based their utterances on an incorrect message-level interpretation of events. Transitive and

intransitive productions, for instance, may reflect transitive and intransitive interpretations of the magic tricks shown at test, rather than the intended appearance interpretation (although see the General Discussion section for more discussion on transitive interpretations). This sort of analysis—the exclusion of data points that fail to follow intended utterance patterns—is relatively common in production experiments (e.g., Bock, 1986; Bock & Loebell, 1990).

Post hoc analysis of the distribution of O-responders by condition showed that there were significantly more O-responders in the LTV than TLV condition, Fisher's exact test, two-tailed  $p = .01$ . The significance of this outcome is discussed in the Experiment 2 Discussion section.

### *Learning*

We found that the distributions of TL and LT responders in the TLV and LTV conditions were reliably different,  $\chi^2(1) = 17.04$ ,  $p < .0001$ , with more TL-responders in the TLV condition and more LT-responders in the LTV condition. This reflects the fact that participants tended to follow the argument order exemplified by the construction to which they were exposed. To determine whether it was statistically more likely than chance to have mostly TL-responders in the TLV condition and mostly LT-responders in the LTV condition, the distributions of TL-responders and LT-responders in these conditions were compared against the baseline provided by control participants. The results show that there were statistically more TL-responders in the TLV condition than the control condition (TLV: 87%; control: 55%),  $\chi^2(1) = 5.43$ ,  $p = .02$ . Likewise, there were more LT-responders in the LTV condition than in the control condition (LTV: 80%; control: 45%),  $\chi^2(1) = 4.38$ ,  $p = .04$ .

These results indicate that the order in which nominal arguments were produced *with* minimal exposure to stable linking patterns was different than the order in which they were produced *without* exposure to stable linking patterns. Participants very quickly adopted the linking rules featured in their exposure construction and produced utterances that were congruent with these rules at test. Further, when considering only TL and LT productions, it made no difference which linking pattern served as a learning template: Equivalent increases above baseline were evident in both conditions and the distributions of responders who followed the target order for their condition were statistically identical, Fisher's exact test, two-tailed  $p = .66$ .

### **Experiment 2 Discussion**

The outcome of Experiment 2 reinforces the finding in Experiment 1 that linking rules can be learned from the input. Given brief exposure to a novel

construction, participants viewed completely new appearance events in the form of magic tricks performed by the experimenter and were able to correctly map specific semantic arguments to specific syntactic constituents. This result holds additional significance in that it was obtained using a production measure. Combined with the comprehension results from Experiment 1, this suggests that the novel constructions that participants were exposed to are functionally equivalent to natural language constructions: Both novel and known construction types are used to map back and forth from semantic to syntactic representations. This strengthens the argument that the representations being learned in these experiments have a specifically linguistic character. We return to this point in more detail in the General Discussion section.

The present results are also difficult for some nativist mapping theories to accommodate. The ease with which the TLV and LTV linking patterns were acquired goes against at least one well-known nativist mapping proposal. Pinker (1989) claimed that locatives are universally mapped to oblique arguments. This generalization is approximately true in English, which encodes most obliques as PPs, not NPs.<sup>1</sup> However, the fact that participants in Experiment 2 readily encoded locatives as NPs indicates that they had no qualms about violating both Pinker's putative universal *and* the dominant encoding method used in English. Their behavior is thus fully consistent with an attempt to learn from and replicate regularities present in the input.

At the same time, however, the data suggest that there may have been a learning bias. Post hoc analysis revealed that the distribution of TL-responders to LT-responders in the control condition was not different than 50-50,  $\chi^2(1) = 0.10, p = .75$ . Whereas this outcome fails to suggest a bias toward one type of linking pattern over another, we also determined that there were significantly more O-responders in the LTV condition than the TLV condition (see Table 3 and the O-Responders subsection), intimating that some participants who were exposed to the LTV pattern resisted learning or producing it.

The possible bias in favor of TL orders is reminiscent of a possible agent-before-patient bias found in Wonnacott et al. (2008). In that study, adult native speakers of English who were trained to map novel forms to transitive events showed better production and comprehension performance when the form was consistent with an agent-before-patient argument order. Both this result and the TL preference in Experiment 2 can be interpreted in a number of ways. Proponents of innate mapping theories might, for example, claim that they show the operation of innate linking rules that specify the mapping of themes and agents to more prominent syntactic positions than locatives and patients. Although there is nothing in the present data to rule out

this possibility, there are at least two alternative explanations that also merit consideration.

First, a bias in favor of TL and agent-before-patient orders could be the result of transfer from English, as both biases involve the unmarked (more frequent) word order. For example, although the LT order does exist in English (e.g., *On the mat sat the cat*), the TL order is much more frequent (*The cat sat on the mat*). Both our results and Wonnacott et al.'s (2008) are thus consistent with a possible preference to utilize the dominant mapping patterns found in participants' first language. This hypothesis predicts that biases that favor TL or agent-before-patient orders should be attenuated in languages that prefer alternative orders. Similarly, biases that are due to interference from English should be diminished in children who are still in the process of acquiring English (see, for example, Akhtar, 1999).

Alternatively, an underlying preference to produce *cognitively accessible* material first may account for the TL and agent-before-patient biases, particularly insofar as these orders tend to dominate crosslinguistically. The appearance events used in the present experiments and the transitive events used by Wonnacott et al. (2008) have certain characteristics in common. Both event types feature one participant that is inherently more active than another: the theme argument in appearance events and the agent argument in transitive events. It may thus be that the smaller number of O-responders in the TLV condition of Experiment 2, and the preference for agent-before-patient orders in Wonnacott et al. (2008) both result from a tendency to name the most active and accessible event participant first. This interpretation is consistent with results from the language production literature, which indicate a preference to produce accessible material earlier (Bock & Irwin, 1980; Ferreira & Dell, 2000).

## General Discussion

We take the converging evidence from comprehension and production as a compelling indication that linking rules can be learned with very little exposure. Given an exemplar of a novel construction that has zero lexical overlap with exposure items (i.e., no shared nouns or verbs), participants are able to correctly map it to a target event, even in the presence of a distracter that differs only with respect to the linking pattern that is exemplified (Experiment 1). Similarly, given a completely novel appearance event, participants are able to correctly describe it using whatever linking pattern was present in their input (Experiment 2). The present results further suggest that the syntax-semantics mapping knowledge that participants acquire is quite robust: Even small amounts of

exposure were enough (a) to build representations that persisted significantly beyond the exposure event, and (b) to support production.

The production outcome is additionally significant because it strongly suggests that participants treated the constructions that they were exposed to as true linguistic objects. A number of studies have demonstrated that novel word and multiword expressions take on many of the processing characteristics associated with known linguistic expressions as speakers gain experience with them (Leach & Samuel, 2007; Wonnacott et al., 2008). Additionally, although the present experiments were not designed to address processing concerns, it is clear that our participants were able use the constructions that they learned to fulfill the two primary linguistic functions: comprehension (Experiment 1) and production (Experiment 2). It thus seems reasonable to conclude that although the constructions that we utilized were indeed new, they are nonetheless linguistic.

A possible alternative interpretation of the present results might claim that learning was fast in both experiments because the target construction was not truly novel, since English allows two NPs to appear before the verb in topicalized transitives, as in *Bagels, I cooked*. On this view, *The bird the flower moopos* would be interpreted such that the flower is an agent that somehow *causes* the bird to appear, whereas the bird is a patient that has been moved from postverbal object position to the front of the sentence, as in *The bird, the flower produces* (Lidz & Williams, 2009). If this were the case, then the quick constructional learning reported here and elsewhere (Casenhiser & Goldberg, 2005; Goldberg et al., 2004) might be considered less surprising, because participants would only need to learn to associate a particular meaning (caused appearance) with an already familiar topicalized transitive construction.

Although this account fails to explain why learning was also fast in Experiment 2's LTV condition, we were interested in finding out more about the interpretations that participants assigned to their exposure construction. To this end, we included a debriefing session immediately following Experiment 2's production block, and explicitly asked 21 of the 48 TLV and LTV participants what they thought the construction that they had been exposed to meant. Their answers were coded according to whether they imputed a semantically transitive relationship between the two NP arguments, or whether the relationship was consistent with the intended meaning involving a theme and a locative in a non-transitive relationship. The results show that 18 of the 21 participants surveyed (i.e., 86%) clearly interpreted the relationship as intended, not as semantically transitive. They described the novel constructions as referring to events in which an argument "somehow arrived" at a location, "magically appeared" at

a location, or “came onto” a location. In contrast, only 2 out of the 21 participants surveyed (i.e., 9.5%) interpreted the novel constructions transitively, and neither of these appeared to assign the agent role to the second NP, contra Lidz & Williams (2009). One final participant failed to provide a description of the construction’s meaning, and instead noted only that the sentences that she had heard were odd in that they contained novel verbs. Because individuals in the 21-person sample overwhelmingly interpreted the construction that they were exposed to in the intended manner,  $\chi^2(1) = 12.8, p < .0001$ , we chose not to debrief the remaining 27 participants.

## Conclusion

The present results suggest multiple avenues for future research. First and foremost, additional testing is needed to determine whether young children learn novel constructions with the same apparent ease and aptitude displayed by adults. Although fast construction learning by children might suggest that multiword phrasal patterns—like words themselves—can be learned quickly during first language acquisition (Carey & Bartlett, 1978), it may also be the case that novel constructions are learned on an item-by-item basis and only achieve abstract status over time. Evidence in favor of this last hypothesis comes from a recent study that applied Experiment 1’s methods to 5-year-olds and showed that—relative to an adult control group—the representations that were acquired were more strongly tied to specific constructional exemplars that had been seen in the past, and were less than fully abstract (Boyd & Goldberg, 2009). More research, however, is clearly needed in this area to elucidate the factors that account for this pattern of results.

Finally, our findings intimate that there may indeed be biases that affect learning, making some syntax-semantics mapping patterns more difficult to acquire than others. The data fail to mediate, however, between nativist proposals, which treat biases as the result of innate, domain-specific representations, and constructionist models, which argue that biases are the result of pragmatics and domain-general constraints on learning and processing. Be that as it may, to the extent that robust correlations exist between the input that learners are exposed to and the representations that they develop, the present data sit well with constructionist models, which allot a prominent role to learning. Regardless, however, of the theory that is used to interpret them, these results serve as a foundational step toward exploring the processes by which speakers acquire mappings between syntactic devices such as word order, and semantics.

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## Note

- 1 There are a few lexical items that encode goal arguments as NPs in English (e.g., *approach* and *reach*), but it is much more commonplace for locations to be expressed as PPs.

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