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A Comprehensive Examination of Prediction-Based Error as a Mechanism for Syntactic Development: Evidence From Syntactic Priming

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Abstract

Prediction-based accounts of language acquisition have the potential to explain several different effects in child language acquisition and adult language processing. However, evidence regarding the developmental predictions of such accounts is mixed. Here, we consider several predictions of these accounts in two large-scale developmental studies of syntactic priming of the English dative alternation. Study 1 was a cross-sectional study ($N = 140$) of children aged 3–9 years, in which we found strong evidence of abstract priming and the lexical boost, but little evidence that either effect was moderated by age. We found weak evidence for a prime surprisal effect; however, exploratory analyses revealed a protracted developmental trajectory for verb-structure biases, providing an explanation as for why prime surprisal effects are more elusive in developmental populations. In a longitudinal study ($N = 102$) of children in tightly controlled age bands at 42, 48, and 54 months, we found priming effects emerged on trials with verb overlap early but did not observe clear evidence of priming on trials without verb overlap until 54 months. There was no evidence of a prime surprisal effect at any time point and none of the effects were moderated by age. The results relating to the emergence of the abstract priming and lexical boost effects are consistent with prediction-based models, while the absence of

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age-related effects appears to reflect the structure-specific challenges the dative presents to English-acquiring children. Overall, our complex pattern of findings demonstrates the value of developmental data sets in testing psycholinguistic theory.

Keywords: Prediction-based learning; Syntactic priming; Syntax; Language acquisition

A core aim of psycholinguistics is to explain diverse phenomena from child language acquisition and adult language processing by invoking common principles that enable learning and implement processing routines. One such mechanism is the minimization of prediction error (Chang, Dell, & Bock, 2006; Dell & Chang, 2014; Fine & Jaeger, 2013; Ramscar, Dye, & McCauley, 2013). Prediction-based accounts of learning and processing assume that, while comprehending language, listeners implicitly predict upcoming linguistic material (e.g., syllables, words, morphemes, and syntactic elements). In the domain of syntax, our focus here, these implicit predictions are compared to the observed linguistic structure, generating a prediction error, and listeners' syntactic expectations are updated according to the magnitude of this error. For example, a child who hears *the boy passed...* may implicitly predict that the verb will be followed by a noun phrase (NP) denoting a theme (e.g., *the ball*), and a prepositional phrase denoting a recipient (e.g., *to the girl*), thus resulting in a prepositional-object dative structure (POD). If the verb is instead followed by two NPs, as in *the boy passed the girl the ball* (i.e., a double object dative, DOD), a prediction error will be generated and the child's syntactic knowledge will be updated accordingly, increasing the probability that DODs are used more frequently.

Prediction-based learning accounts of syntactic structure are appealing for several reasons. First, they integrate seemingly diverse empirical phenomena within psycholinguistics, such as adult syntactic priming (Chang et al., 2006), child language acquisition (Chang et al., 2006; Ramscar et al., 2013), sensitive period effects in bilinguals (Janciauskas & Chang, 2018), and brain activity (Clark, 2013; Fitz & Chang, 2019; Friston, 2009). Second, the concept of prediction error provides a clearly articulated example of how to learn from indirect negative evidence—as the absence of a predicted structure is informative about the grammatical rules—weakening the relevance of classical poverty of stimulus arguments that (partially) rely on the assumption of the absence of negative evidence to support arguments for innate linguistic knowledge (Fitz & Chang, 2017; Ramscar et al., 2013). However, some of the theory's most important predictions, specifically those regarding the developmental trajectories of various effects in the syntactic priming paradigm, have mixed empirical support. This may be due to difficulties in specifying the dynamics of how a prediction-based mechanism interacts with evolving knowledge states in acquisition, a problem that is related to methodological limitations concerning how development has been operationalized in past studies, which have binned together children of wide age ranges (Messenger, Branigan, Buckle, & Lindsay, 2022). Here, we aim to overcome many of these methodological limitations and provide one of the most comprehensive examinations of the developmental predictions of prediction-based learning accounts. In particular, we present both cross-sectional and longitudinal studies of syntactic priming aimed at tracking the emergence and trajectories of several key syntactic priming effects in the English dative alternation.

Syntactic priming

Syntactic priming, the tendency for speakers to persist in the use of a particular syntactic structure after having recently processed it, is an ideal method for examining the acquisition, representation, and processing of grammar (Bock, 1986; Branigan & Pickering, 1998, 2017). Priming is observed in both naturalistic speech (Gries, 2005; Jaeger & Snider, 2013; Travis, Cacoullos, & Kidd, 2017) and in experimental contexts across a wide array of participant populations (Bock, 1986; Savage, Lieven, Theakston, & Tomasello, 2003, for meta-analytic review of adult literature, see Mahowald, James, Futrell, & Gibson, 2016). In a typical experiment, participants are asked to describe a scene that can be construed using either of two grammatical constructions after processing a prime sentence using one of those constructions. A participant is primed if they produce, for example, a DOD (e.g., *The boy gives the girl the ball*) more often following an unrelated DOD prime (e.g., *The monkey sends the cat a package*) than after a similarly unrelated POD prime (e.g., *The monkey sends a package to the cat*). Priming effects are considered to indicate shared linguistic representations between the prime and target sentence (Branigan & Pickering, 2017). They can be reliably observed even when the prime and target sentence share no open-class lexical items (Bock, 1986; Mahowald et al., 2016), suggesting that the effect is due to shared abstract representations between the prime and target sentence, whether these be syntactic (Bock, 1989; Bock & Loebell, 1990; Branigan & Pickering, 2017) or semantic in nature (Chang, Bock, & Goldberg, 2003; Ziegler & Snedeker, 2018; Ziegler, Snedeker, & Wittenberg, 2018).

While some theories attribute priming effects to residual activation of the prime structure (Pickering & Branigan, 1998), others attribute priming effects to implicit learning of syntactic structure via prediction-based learning (Bock & Griffin, 2000; Chang et al., 2006; Jaeger & Snider, 2013). As an example of how such accounts work, consider the most fully computationally implemented model of such accounts, the Dual-path model (Chang et al., 2006). The model learns to produce the target language's syntactic constructions via incremental, error-based learning. As it comprehends each word in a sentence, it predicts the subsequent word, and when the subsequent word is encountered, the model calculates the prediction error between its expectations and the encountered word and updates its weights (and, therefore, future expectations) accordingly. This learning mechanism naturally produces syntactic priming effects—comprehending a given structure measurably increases the probability that the structure will be used. This illustrates an important claim of prediction-based learning accounts: syntactic priming effects reflect implicit learning via the same mechanisms involved in ordinary language acquisition. Examining the trajectories of syntactic priming effects, therefore, has the potential to shed light on the mechanisms underlying language development more broadly.

While abstract priming effects are observed independent of open-class lexical items, priming effects can be moderated by lexical variables in two prominent ways, the so-called *lexical boost* and *prime surprisal* effects. The lexical boost refers to larger priming effects when the prime and target sentence share open-class lexical content such as verbs. While it was initially argued to reflect the same residual activation mechanism that was used to explain abstract priming (Pickering & Branigan, 1998), some researchers have suggested that abstract

priming and the lexical boost could be due to different mechanisms (Bock & Griffin, 2000; Reitter et al., 2011). For example, due to the difficulty in explaining the boost with prediction error-based learning, Chang, Janciauskas, and Fitz (2012) proposed an *explicit memory* account of the lexical boost, where temporary verb-structure bindings are created and stored separately from long-term linguistic knowledge. Since these bindings are temporary, this account can explain why the lexical boost dissipates quickly in adults relative to the abstract priming effect (Branigan & McLean, 2016; Hartsuiker, Bernolet, Schoonbaert, Speybroeck, & Vanderelst, 2008; Mahowald et al., 2016; though see Bernolet, Collina, & Hartsuiker, 2016). Moreover, since these bindings are not stored as long-term syntactic representations, they can involve ungrammatical or novel pairings of verbs and structures, explaining the observed lexical boost for such structures (Ivanova, Pickering, McLean, Costa, & Branigan, 2012). Further evidence comes from sentence-recall studies showing that verb overlap increases recall of the target sentence and that the magnitude of this effect is statistically indistinguishable from the lexical boost in a priming task (Zhang, Bernolet, & Hartsuiker, 2020). While the explicit memory account of the lexical boost is logically separate from prediction-based accounts of priming, its veracity is relevant to prediction-based accounts, which otherwise struggle to explain the lexical boost.

The prime surprisal effect refers to the finding that priming effects are larger when the prime verb is paired with a structure that it occurs with infrequently (Bernolet & Hartsuiker, 2010; Jaeger & Snider, 2013). For example, while the verb *bring* can occur in both the POD (e.g., *she brought the ball to the boy*) and the DOD (e.g., *she brought the boy a ball*), it is POD biased, occurring in the POD structure more often. In general, a DOD prime will be stronger (i.e., it will increase the likelihood that another DOD will be used) when it contains the verb *bring* than when it contains a verb that is biased to the DOD, such as *give*. Prediction-based accounts naturally explain the prime surprisal effect (Bernolet & Hartsuiker, 2010; Jaeger & Snider, 2013) since uncommon verb-structure pairings should elicit greater prediction error. For example, Jaeger and Snider (2013) explain this by arguing that priming is influenced by the mismatch between the structural expectations due to the verb and the actual prime structure that is experienced. The Chang et al. (2006) model also predicts such surprisal effects, but it differs in an important respect. Since the model learns its structural representations from prediction error about the next word in the sequence, its syntactic representations have lexical associations that can modulate priming effects.

Predicting to learn: The development of syntactic priming effects

While prediction-based learning accounts (and the associated explicit memory account of the lexical boost) provide plausible explanations of adult syntactic priming effects, their most impressive feature is that they integrate an adult language processing phenomenon with child language acquisition. As such, they make novel predictions about the emergence and developmental trajectories of the abstract priming, lexical boost, and prime surprisal effects, which have the potential to shed light on the mechanisms underlying both child language acquisition and adult language processing. We discuss the predictions and developmental evidence for each effect below.

The abstract priming effect

Because priming effects are assumed to reflect implicit learning of syntactic structure, abstract priming should be observed as soon as a child has acquired an abstract representation of the relevant construction. There is clear evidence for the early emergence of abstract priming effects, although this appears to differ depending on the alternation under investigation. Children as young as 3 years show abstract priming effects for the passive (Bencini & Valian, 2008; Brooks & Tomasello, 1999; Kumarage, Donnelly, & Kidd, 2022). Abstract priming of the dative alternation seems to appear later: under some conditions, it has been observed as young as 3 years (Shimpi, Gámez, Huttenlocher, & Vasilyeva, 2007), but more robust evidence has been reported in slightly older children, aged, on average, 3;8–4;0 years (Rowland, Chang, Ambridge, Pine, & Lieven, 2012; Peter, Chang, Pine, Blything, & Rowland, 2015). However, given that the age ranges in past studies investigating the dative have been large (i.e., an age group will typically span more than a whole year, see Messenger et al., 2022), it is difficult to determine the time point of its emergence.

Predictions about the developmental trajectory of such priming effects are subtler. Because prediction-based learning assumes that the magnitude of the priming effect is moderated by the degree of prediction error, less skilled language users should exhibit larger priming effects, a prediction supported by studies showing larger priming effects in groups of aphasiacs and language-delayed children compared to typical controls (Hartsuiker & Kolk, 1998; Leonard et al., 2000). As a result, several developmental studies have predicted that the abstract priming effect should decrease with age (Kumarage et al., 2022; Rowland et al., 2012; Messenger et al., 2022). However, most studies have not observed this effect. For example, the majority of studies with passives find no difference in the magnitude of priming effects between children and adults (Branigan & McLean, 2016; Messenger, Branigan, McLean, & Sorace, 2012). In one cross-sectional study of the dative alternation, Rowland et al. (2012) found that effect sizes for their younger participants (mean age = 3;8) were substantially larger than those for older children and adults although the interaction between age and priming was not significant. However, in a similar study, Peter et al. (2015) observed an *increase* in the priming effect with age.

One explanation for this mixed pattern of results is between-participant variability in syntactic knowledge, which is common in studies of language acquisition (Kidd & Donnelly, 2020). According to Messenger et al. (2022), the average age-range within age groups in developmental studies is 20 months, which treats children at 3;6 and 5;2 years as having the same levels of language proficiency. Grouping together children with such different levels of language proficiency may obscure changes in priming effects for at least two reasons. First, in order to be primed, children need to be able to produce the target construction (Kumarage et al., 2022). Given individual differences in children's productive grammatical knowledge, decreases in priming effects in some children may be offset by increases in the number of children capable of producing the target structure, thereby eliminating the priming effect. Consistent with this, Kumarage et al. (2022) found stronger evidence of a decrease in abstract priming of passives with age in a subsample of participants who were producing the passive at 36 months. Second, variability in the format of early syntactic knowledge across children could also obscure such effects. For example, in the early stages of learning, the Dual-path

model learns representations of varying degrees of abstractness (Chang et al., 2006). As a result, a large error signal on the prime might only lead to a weak priming effect, because the syntactic structures for the prime and target only partially overlap due to variability in lexicalized grammatical knowledge. If young children's representations are lexicalized, it is possible that decreases in prediction error in some children will be offset by increases in abstractness of syntactic representations in other children (Savage et al., 2003; Tomasello, 2003).

Given these concerns, while we predict the abstract priming effect will decrease with age (and, presumably, prediction error; Kumarage et al., 2022; Rowland et al., 2012, Messenger et al., 2022), we add two caveats. First, it is possible that such an effect will only be observed among participants who are producing the relevant structure (as in Kumarage et al., 2022). Second, we do not expect to see such a decrease until there is clear evidence of abstract priming (i.e., priming without a shared verb between prime and target sentences). These concerns point to the need for more developmentally inspired designs, particularly longitudinal designs with tightly controlled age groups, which would allow us to distinguish between within and between participant variability.

The lexical boost

The explicit memory account of the lexical boost assumes that it is governed by separate mechanisms that have temporary effects and thus its emergence and trajectory should be decoupled from the abstract priming effect. There is strong evidence in support of this prediction. For example, both Rowland et al. (2012) and Peter et al. (2015) found a lexical boost that increased with age and was not significant in the youngest groups tested, despite the fact that those groups exhibited abstract priming. Kumarage et al. (2022) longitudinal study of priming of the active-passive alternation reported a similar developmental asynchrony. This pattern of a later lexical boost that increases with age fits nicely with the explicit memory account of the lexical boost (Chang et al., 2012), as there is evidence that explicit memory develops slowly over childhood (Finn et al., 2016; Lum, Kidd, Davis, & Conti-Ramsden, 2010). We, therefore, predict that the lexical boost follows a separate developmental trajectory from the abstract priming effect and, assuming this effect reflects explicit memory, that it emerges after the abstract priming effect and increases with age (and, therefore, explicit memory, Kumarage et al., 2022; Peter et al., 2015; Rowland et al., 2012).

However, an important caveat is that, in developmental samples, verb overlap could be a necessary precondition for priming if children's earliest syntactic representations are highly lexicalized, a possibility we call *lexically restricted priming* (Savage et al., 2003). If this is the case, we would expect to observe priming with verb overlap but not without verb overlap at the earliest ages. This would lead to an interaction between prime and overlap that is different in kind from the lexical boost with adults. Consistent with this possibility, Savage et al. (2003) found that 3- and 4-year olds were only primed when the prime sentence contained two pronouns, which could be reused on the target sentence, while 6-year olds were primed without any open-class lexical overlap (though see Bencini & Valian, 2008; Kumarage et al., 2022; Peter et al., 2015; Rowland et al., 2012). If we observe lexically restricted priming, we would not expect this interaction to increase with age (or episodic memory capacity), since developing explicit memory abilities may be offset by the emergence of an abstract priming

effect. As a result, we would only expect to see an increasing lexical boost with age if the lexical boost emerges simultaneously with, or after, the abstract priming effect.

The prime surprisal effect

The emergence and development of the prime surprisal effect is less studied. Because the prime surprisal effect and abstract priming effect are hypothesized to reflect the same mechanism (prediction error), they should emerge in parallel. However, few studies have tested this prediction and their results are mixed: while Peter et al. (2015) found evidence of a prime surprisal effect among groups of younger (average age 4;0) and older (average age 5;11) children, Fazekas et al. (2020) did not observe a prime surprisal effect in a group of 5- to 6-year olds (average age 6;4). One problem with this research has been the potentially erroneous assumption that verb biases are stable across children at different developmental levels. There is reason to believe that this is not the case. Computational models which learn verb biases from variable input (such as the Dual-path model) initially acquire non-adult-like variable verb bias associations and only gradually converge on the adult pattern (see Twomey, Chang, & Ambridge, 2014, for an exploration of this in the locative alternation). Consistent with this, Peter et al. (2015) found that adults showed stronger evidence of verb-structure biases than children. These results raise the possibility that corpus-based estimates of verb-structure biases from adults may not match those of children at different moments in development, thereby masking prime surprisal effects in children.

Once verb biases have stabilized to adult levels, there is some evidence that the prime surprisal effect may decrease with age. Peter et al. (2015) found a prime surprisal effect that emerged with the abstract priming effect at 4 years of age and decreased with age, with only a marginally significant effect in adults. This was consistent with the predictions of Chang et al. (2006) Dual-path model, where a combination of knowledge acquisition and decreases in learning rate across time lead to smaller priming effects. However, Fazekas et al. (2020) found no significant prime surprisal effect in either children or adults (though the effect was numerically larger in adults). Although the available evidence is equivocal, we expect that after verb biases have stabilized to adult levels, the prime surprisal effect will decrease with age.

Priming of the dative

While priming has been studied for several syntactic alternations in adults (see Branigan & Pickering, 2017 for an overview), the majority of developmental studies have focused on the active/passive alternation (Kumarage et al., in revision). As a result, there is a need for more studies examining the trajectories of priming effects for other alternations (Messenger et al., 2022). The dative is a particularly interesting comparison as its developmental trajectory differs in important ways from the passive. First, English-speaking children hear fewer passives than actives, and consistent with this, comprehension studies with novel verbs reveal evidence of abstract knowledge of the active (2 years; Gertner, Fisher, & Eisengart, 2006) before the passive (typically no younger than 3 years; Messenger & Fisher, 2018). However, while children hear more DODs than PODs (Rowland & Noble, 2010), 3-year olds appear to

Table 1

Predictions about emergence and developmental trajectory for each effect from a prediction-based learning account

Effect	Emergence	Trajectory
Abstract priming	Early	Decrease
Lexical boost	Later	Increase
Prime surprisal	With abstract priming	Likely decrease

have more robust abstract representations of PODs than DODs, in both novel-verb comprehension (Rowland & Noble, 2010) and production tasks (Conwell & Demuth, 2007). There is some evidence that even older children have trouble flexibly using the DOD. For example, Gropen, Pinker, Hollander, Goldberg, and Wilson (1989) employed a long block of training and priming trials to elicit DODs in a group of older children (average age 7;4) as a prior pilot study by Wilson, Pinker, Zaenen, and Lebeaux (1981) found the children “virtually never uttered” the structure (p. 226). Second, the dative is much more strongly verb biased than the passive, with verbs exhibiting strong preferences for either the DOD or POD (Goldberg, Casenhiser, & Sethuraman, 2004; Gropen et al., 1989). These two considerations indicate that dative priming may (a) emerge later than passive priming (which is observed reliably by 36 months (Bencini & Valian, 2008; Kumarage et al., 2022) and (b) exhibit a more protracted developmental trajectory, and (c) be differentially affected by verb overlap.

Present study

Developmental studies of syntactic priming effects have the potential to offer a powerful test of theories of prediction-based learning and the explicit memory account of the lexical boost. However, prior studies have suffered methodological limitations that make it difficult to evaluate these theories. Here, we present research overcoming those limitations, providing the most comprehensive developmental investigation of priming of the English dative alternation to date. In particular, we present two studies testing predictions about the abstract priming effect, lexical boost and prime surprisal effect (see Table 1). The studies are complementary in design. Study 1 is a large-scale cross-sectional study in children aged 3–9 years, where we investigate developmental effects across children of different ages, treating age as a continuous variable. This crucially differs from past studies, which have binned children from wide age ranges into discrete age groups, allowing us to more accurately test our developmental predictions. Moreover, this age range allows us to consider changes in these priming effects from shortly after the earliest ages at which children demonstrate abstract knowledge of the POD to the school years. Following Kumarage et al. (2022), Study 2 is longitudinal, following a large group of children, who were tested three times across a 1-year time frame, beginning when they were 42 months (3;6 years). This sample allows us to zoom in and study the trajectory of priming effects during the earliest age where there is the clearest evidence for abstract priming of dative structures (3;8 years in Rowland et al., 2012; 4;0 in Peter et al., 2015). Moreover, by testing the children at three time points within 1 year, we are able to study changes in the trajectories of priming effects that may have been masked in prior

cross-sectional studies, where the age-range within group has been much larger (Messenger et al., 2022). Both studies use estimates of verb-structure biases based on child production data.

1. Study 1

1.1. Method

1.1.1. Participants

One hundred and forty-five monolingual English-speaking children aged between 3;3 and 8;9 years were recruited from Canberra, a medium-sized Australian city. The majority were recruited from kindergartens and primary schools, with the youngest children recruited from a university child development lab. Five participants were excluded because of diagnosed hearing loss (one child) or an inability/refusal to complete the priming task (four children), resulting in a final sample of 140 participants. The mean age was 5.96 years old with 64 females (46% of the sample) and 75 males (54% of the sample).¹ Of the 140 children, 22 were younger than 4;6, 34 were between 4;6 and 5;6, 28 were between 5;6 and 6;6, 37 were between 6;6 and 7;6, and 19 were older than 7;6. All participants were acquiring English as their first language. Ethnicity was not recorded; however, the sample was representative of the local population, which is predominantly of White, Anglo-Celtic origin (approx. 90%) with a range of other ethnicities due to different waves of migration since the mid-20th century (Australian Bureau of Statistics, 2016).

1.1.2. Materials

1.1.2.1. Syntactic priming task: The syntactic priming task was an adapted version of Rowland et al. (2012) task. Participants watched a set of videos with the experimenter and the two took turns describing the scenes. The experimenter always described the prime trials and the participant described the target trials. The critical trials comprised 24 animated scenes that could be described by dative sentences (e.g., *Dora threw Boots a fish, or Dora threw a fish to Boots*). Scenes were created using Anime Studio Pro (see Fig. 1). Each scene depicted one of six actions (*brought, gave, passed, sent, showed, and threw*), undertaken by one of six pairs of animate characters (three sets of proper nouns: *Dora and Boots, Wendy and Bob, Tigger and Piglet*; and three sets of common nouns: *the boy and the girl, the king and the queen, the prince and the princess*) using one of five animate objects (*a baby, a cat, a fish, a rabbit, a puppy*). See Table 2 for a full description of all 24 scenes. In addition to these critical scenes, we created a further 30 scenes that depicted transitive and intransitive events with the same characters, which served as filler (22 scenes) and practice (8 scenes) trials.

Each participant saw all 24 critical scenes over the course of the experiment. Scenes were presented in 12 prime-target pairs. Each action occurred four times within participants, twice in prime trials and twice in target trials. Half of the prime trials were described with a DOD sentence and half were described with a POD sentence. Half of the pairs of trials contained the same action (verb overlap trials) and half contained separate actions (no overlap trials); thus, participants completed three trials of each cell in the 2×2 design (prime structure by verb overlap). Each action occurred in one overlap trial (thus as both the prime and target



Fig. 1. Image from the video clip depicting a scene that could be described with the DOD or POD dative “*The king showed the queen a baby / a baby to the queen.*”

scenes) and two no-overlap trials (in the prime scene for one pair and the target scene for the other). Each pair of critical scenes was separated by a pair of filler scenes with intransitive actions (also to be described by the experimenter and participant).

Eight experimental lists were created to balance the presentation of verbs across each cell from the two factors above (POD-Prime, Overlap; POD-Prime, No Overlap; DOD-Prime Overlap; DOD-Prime, No Overlap). List 1 was created to meet the conditions in the paragraph above. List 2 was created so that the order of the scenes within each pair was reversed; that is, the prime scenes from List 1 were the target scenes in List 2 and vice versa. In List 3, the DOD primes from List 1 were POD primes and vice versa. In List 4, the DOD primes from List 2 were POD primes and vice versa. List 5 was created by reordering the pairs from List 1. Lists 6, 7, and 8 were created by applying the same steps as Lists 2, 3, and 4 to List 5. All experimental lists are presented in Appendix A.

1.1.2.2. Syntactic priming procedure: The experimenter and the participant sat together to watch the videos on a laptop computer. Prior to beginning the task, the experimenter ensured that the participant was familiar with all of the characters and actions. Following this, the experimenter told the participant that they were going to take turns with the experimenter describing what was happening in the video clips. The participant and the experimenter then completed eight practice trials, with intransitive actions. After the practice trials, the

Table 2
POD and DOD sentences for priming and responses

Verb	Sentences (POD/DOD)	Verb	Sentences (POD/DOD)
Gave	The king <i>gave</i> a baby to the queen/ the queen a baby.	Showed	Piglet <i>showed</i> a cat to Tigger/ Tigger a cat.
	Dora <i>gave</i> a rabbit to Boots/ Boots a rabbit.		The boy <i>showed</i> a puppy to the girl/ the girl a puppy.
	Wendy <i>gave</i> a fish to Bob/ Bob a fish.		The boy <i>showed</i> a rabbit to the girl/ the girl a rabbit.
Passed	The prince <i>gave</i> a puppy to the princess/ the princess a puppy.	Sent	Piglet <i>showed</i> a baby to Tigger/ Tigger a baby.
	The boy <i>passed</i> a fish to the girl/ the girl a fish.		The prince <i>sent</i> a cat to the princess/the princess a cat.
	Wendy <i>passed</i> a puppy to Bob/ Bob a puppy.		Piglet <i>sent</i> a baby to Tigger/ Tigger a baby.
	Piglet <i>passed</i> a cat to Tigger/ Tigger a cat.		Dora <i>sent</i> a puppy to Boots/ Boots a puppy.
Threw	The king <i>passed</i> a baby to the queen/ the queen a baby.	Brought	The boy <i>sent</i> a fish to the girl/ the girl a fish.
	Dora <i>threw</i> a fish to Boots/ Boots a fish.		Wendy <i>brought</i> a rabbit to Bob/ Bob a rabbit.
	The king <i>threw</i> a rabbit to the queen/ the queen a rabbit.		The prince <i>brought</i> a rabbit to the princess/the princess a rabbit.
	The prince <i>threw</i> a cat to the princess/ the princess a cat.		The king <i>brought</i> a puppy to the queen/ the queen a puppy.
	Wendy <i>threw</i> a puppy to Bob/ Bob a puppy.		Dora <i>brought</i> a fish to Boots/ Boots a fish.

participant and the experimenter began the 46 test trials (12 pairs of prime and target trials and 11 pairs of filler trials).

On prime trials, the experimenter said the prime sentence and asked the participant to repeat it.² If the participant repeated the sentence incorrectly, they were encouraged to say it again and repeat it exactly. Experimenters repeated the prime sentence if necessary. On the test trials, the experimenter prompted the child with the beginning of the sentence, for example, “*the boy sent,*” thereby ensuring the child used the correct verb without biasing them toward either form of the dative. If the child did not produce a full dative, they were encouraged to do so as follows:

Experimenter: “The girl threw...”

Participant: “The girl threw a puppy...”

E: “Can you tell me a bit more? Who else is in the movie?”

P: “The girl threw a puppy to the boy.”

Trials were excluded if the participant produced a nontarget response (a nondative structure), or they produced a verb that either (a) was not one of the six verbs in the study³ or (b) affected the overlap between the target and prime verb.⁴ Trials were also removed if the child failed to attend to the experimenter, or if there was an error administering the trial or prompt. All other trials were included even if the participant produced an incorrect NP (e.g., misnaming the character).

1.1.3. Analytic procedures

We fit a series of Bayesian mixed effects logistic regressions using brms version 2.17.0 (Bürkner, 2017) in R (4.1.0). Inferences about parameters of interest were based on the proportion of the posterior distribution in the hypothesized direction for each parameter (similar to a one-tailed test in frequentist statistics). Because hypothesized age-related changes in the magnitude of the priming effect could be offset by age-related increases in the number of children who are able to produce the DOD, we ran all models twice, once including all participants and once including only participants who produced at least one DOD (for a similar approach, see Kumarage et al., 2022). The latter set of analyses allowed us to examine potential developmental effects in children who had unambiguously acquired the DOD. As these two approaches (including all participants vs. only those who produced at least one DOD) yielded similar results, we report on this latter set of analyses in Appendices B and C. We used brms's default priors for all parameters except random effect standard deviations. For these parameters, we chose slightly more conservative priors (a truncated normal distribution with a standard deviation of 2 and mean of 0⁵), which effectively ruled out implausibly large (given the logit link function) random effects standard deviations (> 5).⁶ We did this to better estimate random effects given the relatively low number of observations within participants (12 trials). When models produced divergent iterations, the model was refit, increasing the adapt_delta control parameter to .9 or .95 as necessary.

For all models, we examined the distributions of residuals using the DHARMA package (Hartig, 2022), which uses simulations to approximate the cumulative distribution function of residuals. QQ plots for all models were excellent. However, all models were underdispersed, which may reflect our conservative random effects structures, and had statistically significant, though weak, relationships between residuals and fitted values. As these patterns were weak, and since the DHARMA documentation notes that residuals from multilevel models can create artifactual patterns that become significant with relatively large sample sizes, we do not believe these reflect a major problem with our models. All diagnostic plots are available in the accompanying html files for review.

Unlike frequentist statistical methods which estimate a point estimate for each coefficient (and a confidence interval and p value derived from its sampling distribution), Bayesian methods estimate a distribution of plausible values for each coefficient, that is, a posterior distribution. For each coefficient in our models, we report on the mean of its posterior distribution (b), a 90% credible interval (CI ; indicating the 5th and 95th percentile of the posterior distribution) and the percentage of the posterior distribution in the a priori hypothesized direction (above or below 0, which we denote as $P(b) < 0$ and $P(b) > 0$ below). Following Engelmann et al. (2019) and Kumarage et al. (2022), we interpreted this percentage value in a graded way.

In particular, we treated 95% (or greater) of the posterior mass in the predicted direction as strong evidence of an effect, 85% (or greater) of the posterior mass in the predicted direction as weak evidence of an effect, and less than 85% of the posterior mass as no evidence. However, we treat these specific values as heuristics and interpret these probabilities in a graded way. We report 90% credible intervals as they exclude the smallest 5% of parameter values and the largest 5% of parameter values, thereby paralleling the directional hypothesis tests used above.

All results from Studies 1 and 2 are linked to html formatted output. Additionally, all data and scripts are available on the Open Science Framework repository: <https://osf.io/ymj5w/>. The scripts on the OSF page contain additional sensitivity analyses which are referenced but not described in detail in the text.

1.2. Results

Full results for the analyses of the cross-sectional data are available in html format (<https://rpubs.com/sdonnelly85/DativeXSec>).

1.2.1. Missing data

Of the original 1680 trials (140 participants with 12 trials each), 22 (1.31%) were excluded, for the following reasons: 10 administration errors, 6 verb errors, 4 nontarget responses, 1 nonallowable prompt, and 1 failure to attend to the experimenter. Of the total 140 participants, 13 had at least one nonvalid trial (missing 1 = 6 participants, missing 2 = 6, missing 4 = 1).

1.2.2. Analysis of abstract priming effect and lexical boost

To examine the developmental trajectory of the lexical boost and abstract priming effect, we estimated a Bayesian mixed effects logistic regression with all relevant random intercepts, slopes, and correlations by participant and target verb.⁷ We included prime structure, verb overlap, age, and their interactions as fixed effects. We sum coded prime structure (−.5 for POD and .5 for DOD), dummy coded verb overlap (0 for no overlap and 1 for overlap), and centered age (Brehm & Alday, 2022; Schad, Vasishth, Hohenstein, & Kliegl, 2020). Coding the variables this way allowed us to interpret our coefficients as follows: The coefficient for the abstract priming effect quantifies the difference in the likelihood of producing a DOD after a DOD prime versus POD prime (on the logit scale), hypothesized to be positive, without verb overlap (since overlap was coded as 0 for no overlap and 1 for overlap) at the average age (since age was centered) (hypothesized to be positive); the coefficient for the interaction between prime structure and age quantifies how much the abstract priming effect (on the logit scale) varies as a linear function of age (hypothesized to be negative); the interaction between prime structure and verb overlap quantifies how much larger the priming effect was with versus without overlap (on the logit scale), that is, *the lexical boost*, at the average age in our sample (hypothesized to be positive), and the three-way interaction between prime-structure, verb overlap, and age quantifies how the lexical boost (on the logit scale) varies as a linear function of age (hypothesized to be positive).

Table 3
Coefficients from cross-sectional models

Predictors	Model 1 Log-odds	Model 2 Log-odds
Intercept	−1.61 (−2.59: −0.59)	−1.96 (−2.95: −0.94)
Prime (DOD)	1.05* (0.45: 1.72)	0.48 (−0.35: 1.34)
Overlap	−0.66 (−1.22: −0.10)	
Age.c	0.45* (0.09: 0.82)	0.54 (0.11: 1.00)
Prime * Overlap	1.64* (0.73: 2.69)	
Prime * Age.c	−0.19 (−0.54: 0.16)	0.01 (−0.59: 0.60)
Overlap * Age.c	0.06 (−0.47: 0.61)	
Prime * Overlap * Age.c	0.08 (−0.48: 0.64)	
Bias Mismatch (POD)		0.45 (−0.36: 1.21)
Prime * Bias Mismatch		0.97 ^a (−0.30: 2.36)
Bias Mismatch * Age.c		−0.14 (−0.59: 0.33)
Prime * Bias Mismatch * Age.c		−0.38 (−1.29: 0.54)
<i>N</i>	140 Case_ID	140 Case_ID
Observations	6 Trial_verb 1658	6 Trial_verb 827

Note. Mean of posterior distribution 90% credible intervals in parentheses. Posterior probabilities were calculated for (a) Prime, (b) Prime * Overlap, (c) Prime * Age.c, (d) Prime*Overlap*Age.c, (e) Prime * Biasmismatch, and (f) Prime * Biasmismatch * Age.c. We did not calculate posterior probabilities for other parameters because they were not of substantive interest and there was, therefore, no directional hypothesis to test.

* $p > .95$ in hypothesized direction.

^a $p > .85$ in hypothesized direction.

Coefficients and 90% credible intervals are available in Table 3 (Model 1). Model predictions (on both the logit scale and probability scale) and raw data are depicted in Fig. 2. There was strong evidence for the abstract priming effect ($b = 1.05$, $CI = .45: 1.72$, $P(b) > 0 = 99%$) as well as a lexical boost ($b = 1.64$, $CI = .73: 2.69$, $P(b) > 0 = 99%$). There was no evidence that the abstract priming effect or lexical boost was moderated by age ($b = -.19$, $CI = -.54: .16$, $P(b) < 0 = 83%$; and $b = .08$, $CI = -.48: .64$, $P(b) > 0 = 59%$, respectively).⁸

We also ran an exploratory analysis with a simplified random effects structure. In our original model, we estimated the full random effects structure implied by the design (Barr, 2013;

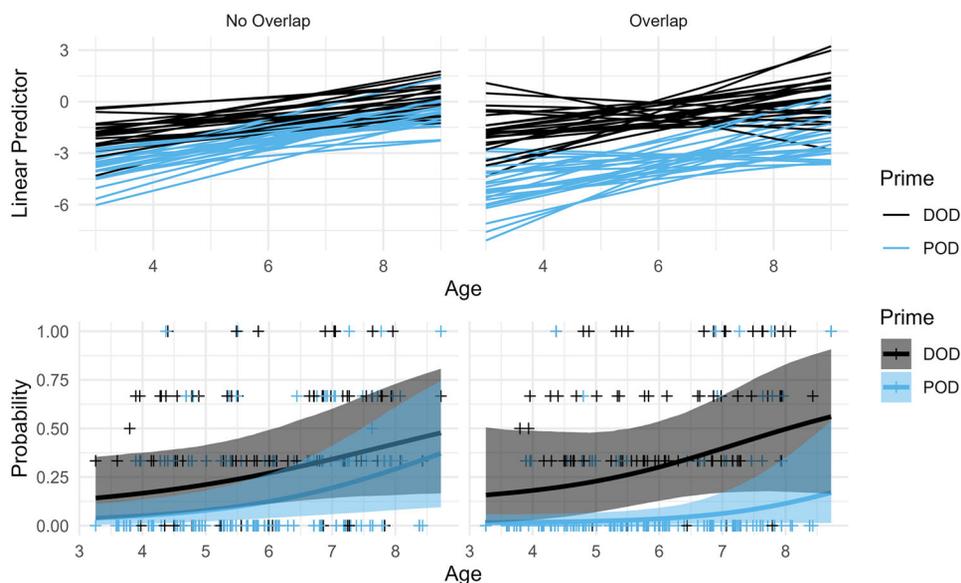


Fig. 2. Model implied proportions of DOD responses for the entire sample, on both logit (i.e., the linear predictor) and probability scale. Row 1 plots 25 posterior regression lines from Model 1 (fixed effects only), which shows the range of regression lines compatible with the data. The separation between the black (DOD prime) and blue (POD primes) in the no overlap condition illustrates the abstract priming effect from Model 1, and the comparatively larger separation of these lines in the overlap condition reflects the lexical boost. Row 2 plots mean predicted values and 90% credible interval (including uncertainty only in the fixed effects) on the probability scale. Crosses in row 2 are participant-level proportions of DOD responses for the relevant condition. Note that because of the nonlinear mapping between the probability and logit scales, main effects on the logit scale can appear to be interactions when transformed to probabilities (Jaeger, 2008). This is why there appears to be a three-way interaction between prime structure, verb overlap, and age in the bottom row, while there is clearly no effect on the top row (which is further indicated by the coefficients for this three-way interaction in Table 3).

Barr, Levy, Scheepers, & Tily, 2013). However, given that target verb only has six levels and including age and its interactions resulted in eight random-effect standard deviations, this model may have been overparameterized. We, therefore, fit an additional model removing the random effects for age (and its interactions). This simplified model revealed weak evidence for the predicted decrease of the abstract priming effect ($P(b) > 0 = .90$). However, model comparison via cross-validation revealed a modest preference for the original full model (elpd difference = -4.5 , $SE = 4.3$). Given that the full model fit (modestly) better, we conclude, conservatively, there was no evidence for the hypothesized decrease in abstract priming with age.

In sum, we found strong evidence for the abstract priming effect and the lexical boost in our cross-sectional sample, but the evidence that either of these effects were moderated by age was less clear. There was weak evidence for the hypothesis that abstract priming decreases with age, when tested with a simpler, but statistically dispreferred, random effects structure.

1.2.3. Analysis of prime surprisal

Following Peter et al. (2015), we tested for prime surprisal effects only on trials without verb overlap.⁹ Prime structure and age were coded the same as in Models 1 and 2, above. To determine each verb's preferred prime structure, we calculated the proportion of DOD responses for each verb and conducted a median split on this value. *Show*, *give*, and *bring* were more DOD biased (with 41%, 37%, and 25% DOD responses, respectively), and *pass*, *send*, and *throw* were relatively POD biased (with 19%, 14%, and 14% DOD responses, respectively). Then, for each prime verb/prime structure combination, we created a variable indicating whether the verb's bias and the prime structure matched (0) or mismatched (1). Calculating this variable on the basis of children's responses is somewhat problematic, as we are now controlling for a post-treatment variable. However, in Study 2, we used these verb bias estimates in a separate sample of participants from the same community. Note that given this coding scheme, the abstract priming effect refers to the priming effect when the prime structure matches the verb's bias (at the sample's mean age); the interaction between prime bias and prime structure reflects how much larger the prime effect was when the verb's bias mismatched the target structure (at the sample's mean age), that is, the prime surprisal effect; the interaction between age and prime structure reflects changes in the magnitude of the priming effect when the verb's bias matches the prime structure; and the three-way interaction reflects how the prime surprisal effect changes with age.

Coefficients and 90% credible intervals are presented in Table 3 (Model 3). Model predictions (on both the logit scale and probability scale) and raw data are depicted in Fig. 3. There was weak evidence for a prime surprisal effect ($b = .97$, $CI = -.30: 2.36$, $P(b) > 0 = 89\%$),¹⁰ and there was no evidence that this effect decreased with age ($b = -.38$, $CI = -1.29: .54$, $P(b) < 0 = 77\%$). To understand the weak evidence for a surprisal effect, we ran an exploratory analysis to examine the strength of verb bias as a function of age. In particular, we fit a fifth model predicting the probability of producing a DOD across all conditions, with random intercepts by participant and target verb and a random slope of age by target verb. We then calculated the verb-specific trajectories by combining using (a) the fixed effects and (b) the by-verb random effects. We plot these effects (with 90% credible intervals) and the proportion of DODs produced for each verb by each participant in Fig. 4. At the earliest ages, all verbs were POD biased and gradually became less so. Indeed, the average number of DODs produced did not exceed 50% until around 7 years of age for *gave* and *showed*, which were the two most clearly DOD-biased verbs.

In sum, we found weak evidence for the prime surprisal effect and no evidence that either of these effects were moderated by age. However, one possible explanation for this effect is the relatively protracted developmental trajectory for verb biases.

2. Discussion

In Study 1, we found strong evidence of an abstract priming effect and a lexical boost; however, contrary to our predictions, neither effect was strongly moderated by age. These results remained unchanged when children who produced no DODs were removed, indicating

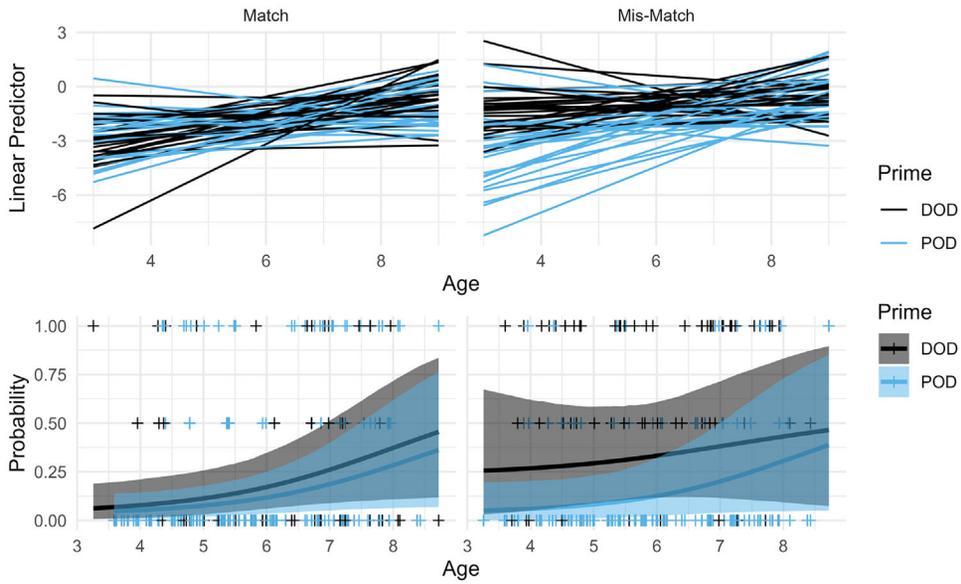


Fig. 3. Model implied proportions of DOD responses for the entire sample, on both logit and probability scale, for analyses of prime surprisal. Row 1 contains 25 posterior regression lines from Model 2. Row 2 plots mean predicted value and 90% credible interval on the probability scale. Crosses in row 2 are participant-level proportions of DOD responses for the relevant condition. Note that because this analysis did not contain verb overlap trials, it is based on less data (a total of six trials per participant) than Fig. 2.

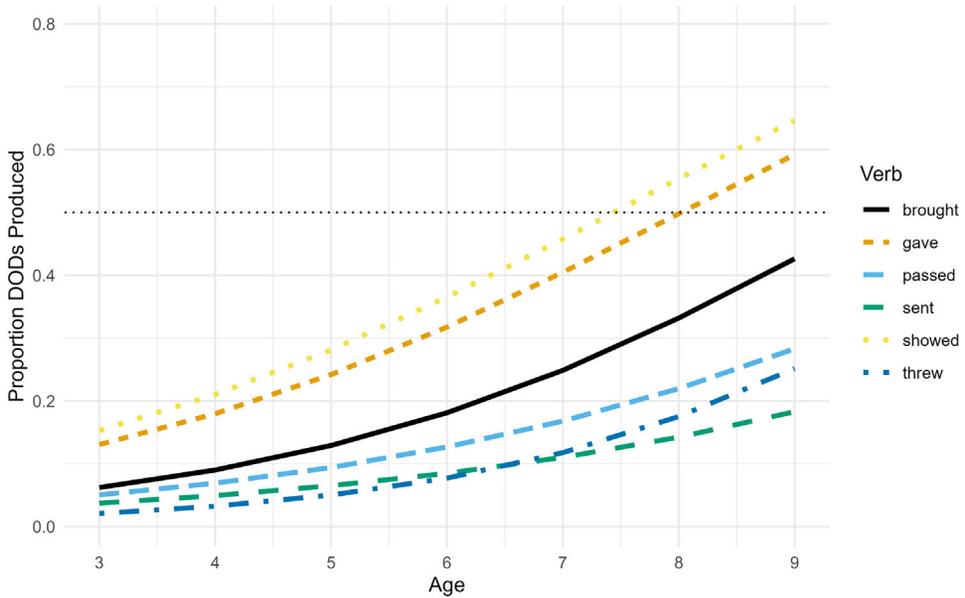


Fig. 4. Proportion of DODs produced for each verb, by age. Lines indicate posterior mean of DODs produced for each verb at each age (credible intervals have been removed for readability). Dotted line at .50 for reference.

that the lack of an interaction was not caused by including children who were not capable of being primed. We also found weak evidence of the prime surprisal effect, which was also not moderated by age.

Overall, while we found evidence for abstract priming effect and lexical boost, the data were characterized by a distinct lack of developmental change. Accordingly, the results from Study 1 invite the preliminary conclusion that English-speaking children possess verb-general, abstract knowledge of the dative alternation from an early age. However, there is reason to doubt this lack of developmental change, as revealed by our exploratory analysis of changes in verb biases. Notably, we found a clear preference for the production of PODs in the young children, with verb biases changing according to verb-specific trajectories. These differences are likely to reflect both changes in structural knowledge and in verb-structure connections. Given this finding, it is unsurprising that the effect of prime surprisal was weak and has been inconsistently observed in past research (Fazekas et al., 2020; Peter et al., 2015); in developmental populations, it may be something of a moving target, with an initially strong POD-bias for all verbs. Thus, their initial knowledge of the DOD and thus their knowledge of the dative alternation may be more idiosyncratic and verb-specific. Our Study 2, which presents longitudinal data from children aged 3;6–4;6, sheds light on this possibility.

3. Study 2

3.1. Method

3.1.1. Participants

Participants came from a longitudinal study of language acquisition and processing from 9 to 60 months (Kidd, Junge, Spokes, Morrison, & Cutler, 2018; Kumara et al., 2022). The participants were recruited from the same population as the children in Study 1. Inclusion criteria for the longitudinal study were: (i) full-term (at least 37 weeks gestation) babies born with a typical birth weight (> 2.5 kg), (ii) a predominantly monolingual language environment; thus, the children were acquiring Australian English as a first language (mean percentage of language other than English = 2%, Range: [0, 40%], Mode = 0), and (iii) no history of medical conditions that would affect typical language development, such as repeated ear infections, visual or hearing impairment, or diagnosed developmental disabilities. The sample was drawn from families of high socioeconomic status with approximately 75% of the parents having completed a bachelor degree or higher. Ethnicity information was not recorded but reflects the same demographics as the children in Study 1.

Participants completed syntactic priming tasks at the ages of 42 (min = 41 months 27 days; max = 43 months 5 days), 48 (min = 48 months 11 days; max = 49 months 4 days), and 54 months (min = 54 months 10 days; max = 55 months, 5 days). Of the 124 participants who participated in the study, 104 were still participating in the study at 42 months and, therefore, completed at least one syntactic priming task. Of these, two were excluded because of later developmental diagnoses. As a result, 102 participants were included in the present analyses ($N = 102$). Of these 102, 90 completed the 42-month session, 90 completed the 48-month

session, and 88 completed the 54-month session. A total of 77 participants completed all three sessions, 12 completed two sessions and 11 completed one session. Because of COVID-19, 11 participants completed the 54-month session online via Zoom. As such, we analyzed 54-month-old sessions with and without these participants. As the results did not differ, we report on the full sample, but these models are available in our R Markdown files.

3.1.2. Materials

The syntactic priming task was administered during 3 hour long visits to the lab, during testing sessions at 42, 48, and 54 months.

3.1.2.1. Syntactic priming task: We used the same materials as in the cross-sectional study with two small modifications because the sample was so young. First, we used definite common nouns rather than proper nouns for all actors (e.g., the pairs of actors were referred to as *the girl* and *the monkey*, *the lady* and *the man*, *the tiger* and *the pig*, *the boy* and *the girl*, *the king* and *the queen*, *the prince* and *the princess*).¹¹ Second, we did not ask participants to repeat the prime sentence. We did this because children were young when they began the study and thus many were likely to have had difficulty repeating the prime sentences without error (though see Shimpi et al., 2007). Moreover, if priming effects reflect prediction-based implicit learning, as hypothesized in our introduction, then comprehension-to-production priming is the most appropriate test of this effect (Messenger et al., 2022). Two pieces of evidence suggest that prime repetition is not a necessary precondition for priming in this population, our unexpected results in Study 1 notwithstanding. First, these children also completed a passive priming task without prime repetition (reported in Kumarage et al., 2022) and showed an abstract priming effect at age 3;0 years. Second, a recent meta-analysis of the developmental priming literature (Kumarage et al., in revision) found that there was no reliable benefit of prime repetition.

3.1.3. Results

Because we were interested in both the emergence and trajectory of priming effects, we conducted two sets of analyses. We first ran separate analyses at each time point, to determine whether the abstract priming effect, lexical boost, and prime surprisal effect were present. We then estimated a longitudinal model, which included data points from all three time points. This allowed us to test the hypotheses that the abstract priming effect and prime surprisal effect decrease with age, while the lexical boost increases. Using this longitudinal model, we also calculated time point-specific effects (abstract priming effect, lexical boost, and prime surprisal effect), to compare with those estimates from the time point-specific models while controlling for each participant's tendency to produce DODs across the three sessions. We first describe the analyses from each time point separately and then report on the longitudinal model.

For all models, we ran several sensitivity analyses, including (a) an analysis with the problematic trial excluded (see Method), (b) an analysis with 54-month Zoom sessions excluded (where relevant), (c) an analysis without observations with Pareto's k values above .7 (which may suggest model misfit), and (d) an analysis with a simpler random effects struc-

Table 4
Parameter estimates from time point-specific models at 42, 48, and 54 months

Predictors	42 months		48 months		54 months	
	Model 1 Log-odds	Model 2 Log-odds	Model 3 Log-odds	Model 4 Log-odds	Model 5 Log-odds	Model 6 Log-odds
Intercept	-2.87 (-4.34: -1.38)	-2.40 (-3.89: -0.93)	-2.68 (-3.93: -1.36)	-2.91 (-4.48: -1.41)	-3.19 (-4.51: -1.78)	-3.03 (-4.66: -1.48)
Prime (DOD)	0.40 (-0.60: 1.46)	0.92 ^a (-0.43: 2.33)	0.40 (-0.31: 1.13)	-0.02 (-1.24: 1.16)	0.84 ^a (-0.06: 1.76)	0.87 (-0.92: 2.73)
Overlap	-0.99 (-2.49: 0.29)		-1.43 (-2.40: -0.62)		-0.56 (-1.62: 0.50)	
Prime * Overlap	1.29 ^a (-0.33: 2.96)		2.23* (0.86: 3.81)		0.72 (-0.97: 2.54)	
Bias Mismatch (POD)		-0.66 (-1.75: 0.38)		-1.22 (-2.98: 0.30)		-0.99 (-2.60: 0.33)
Prime * Bias Mismatch		-0.57 (-2.55: 1.51)		1.49 ^a (-0.70: 3.94)		0.55 (-1.79: 3.16)
N	84 Blind_ID 6 Target.verb	84 Blind_ID 6 Target.verb	90 Blind_ID 6 Target.verb	90 Blind_ID 6 Target.verb	87 Blind_ID 6 Target.verb	87 Blind_ID 6 Target.verb
Observations	835	385	1019	491	1011	479

Note. Mean of posterior distribution 90% credible intervals in parentheses. Posterior probabilities were calculated for (a) Prime, (b) Prime * Overlap, and (c) Prime * Biasmismatch. We did not calculate posterior probabilities for other parameters because they were not of substantive interest and there was, therefore, no directional hypothesis to test.

^a $p > .95$ in hypothesized direction.

^b $p > .85$ in hypothesized direction.

ture (removing the correlations between random effects). None of these models yielded different results from our main results; all are available in the R Markdown files for this paper. As with the cross-sectional models, we examined residuals for all models using the DHARMA package (Hartig, 2022). Similar to the models in Study 1, all models produced evidence of significant underdispersion and significant, though weak, relationships between residuals and fitted values. All diagnostic plots are available in accompanying html files.

3.1.4. 42 Months

3.1.4.1. Missing data: Of the 1080 trials (90 participants with 12 trials), 240 were missing. This was due to 216 nontarget responses (including nondative structures, incorrect prepositions [responses in which children omitted a preposition or used a preposition other than *to*, *for*, or *over to*], or reversal errors, i.e., producing *the pig brought the cat the tiger* when the tiger was the recipient and the cat was the theme), 16 verb errors, 7 administration errors, and 1 disallowed prompt. However, these missing data were concentrated among a subset of participants. In particular, three participants produced nontarget responses on all 12 trials and a further three produced nontarget responses on at least 10 trials. We removed these six participants from the data for this analysis.¹²

3.1.4.2. Analysis: All results from this section are available in html format: <https://rpubs.com/sdonnelly85/Dative42mo>. Table 4 (Models 1 and 2) presents all coefficients and credible intervals. There was no evidence of an abstract priming effect, though its coefficient was in the predicted direction ($b = .40$, $CI = -.60: 1.46$, $P(b) > 0 = 75\%$), and there was weak

evidence for an interaction between prime structure and verb overlap ($b = 1.29$, $CI = -.36: 2.98$, $P(b) > 0 = 91\%$). This interaction was driven by strong evidence for a priming effect when the target verb and prime verb overlapped ($b = 1.7$, $CI = .1: 3.41$, $P(b) > 0 = 96\%$). Model 2 revealed no evidence for the prime surprisal effect ($b = -.57$, $CI = -1.75: 2.55$, $P(b) > 0 = 31\%$).

3.1.5. 48 Months

3.1.5.1. Missing data: Of the 1080 trials (90 participants with 12 trials), 61 were missing. This was due to 54 nontarget responses (including nondative structures, incorrect prepositions, or reversal errors), 3 administration errors, 3 disallowed prompts, and 1 verb error. No participant was missing more than six trials.

3.1.5.2. Analysis: All results from this section are available in html format: <https://rpubs.com/sdonnelly85/Dative48mo>. Table 4 (Models 3 and 4) presents all model coefficients and credible intervals. There was no evidence of an abstract priming effect ($b = .40$, $CI = -.31: 1.13$, $P(b) > 0 = 82\%$), and there was strong evidence of an interaction between prime structure and verb overlap ($b = 2.23$, $CI = .86: 3.81$, $P(b) > 0 = 100\%$). This interaction was driven by strong evidence of a priming effect when there was overlap ($b = 2.63$, $CI = 1.29: 4.19$, $P(b) > 0 = 100\%$). Model 4 revealed weak evidence for the prime surprisal effect ($b = 1.49$, $CI = -.70: 3.94$, $P(b) > 0 = 87\%$).

3.1.6. 54 Months

3.1.6.1. Missing data: Of the 1056 trials (88 participants with 12 trials), 43 were missing. This was due to 33 nontarget responses (including nondative structures, incorrect prepositions, or reversal errors), and 10 verb errors (including trials on Zoom in which the audio cut out and the child did not hear the target verb). One participant produced 10 invalid trials and this session was removed.¹³

3.1.6.2. Analysis: All results from this section are available in html format: <https://rpubs.com/sdonnelly85/Dative54mo>. Table 4 (Models 5 and 6) presents all model coefficients and credible intervals. There was weak-to-strong evidence of an abstract priming effect ($b = .84$, $CI = -.06: 1.76$, $P(b) > 0 = 94\%$), and there was no evidence of an interaction between prime structure and verb overlap ($b = .72$, $CI = -.97: 3.28$, $P(b) > 0 = 77\%$). Consistent with the lack of evidence for the interaction, there was weak-to-strong evidence of a priming effect when there was lexical overlap ($b = 1.56$, $CI = -.08: 3.32$, $P(b) > 0 = 94\%$). Model 6 revealed no evidence for the prime surprisal effect ($b = .55$, $CI = -1.79: 3.16$, $P(b) > 0 = 64\%$).

3.1.7. Longitudinal models

Specification for our longitudinal models was similar to Study 1, except that age was coded as a categorical variable using sum coding (42 months = -1, 48 months = 0, 54 months = 1 for one vector and 42 months = -1, 48 months = 1, and 54 months = 0 for the other vector) so that we could estimate the abstract priming effect and lexical boost at each time point. Using a categorical coding scheme allowed the model to estimate these effects at each

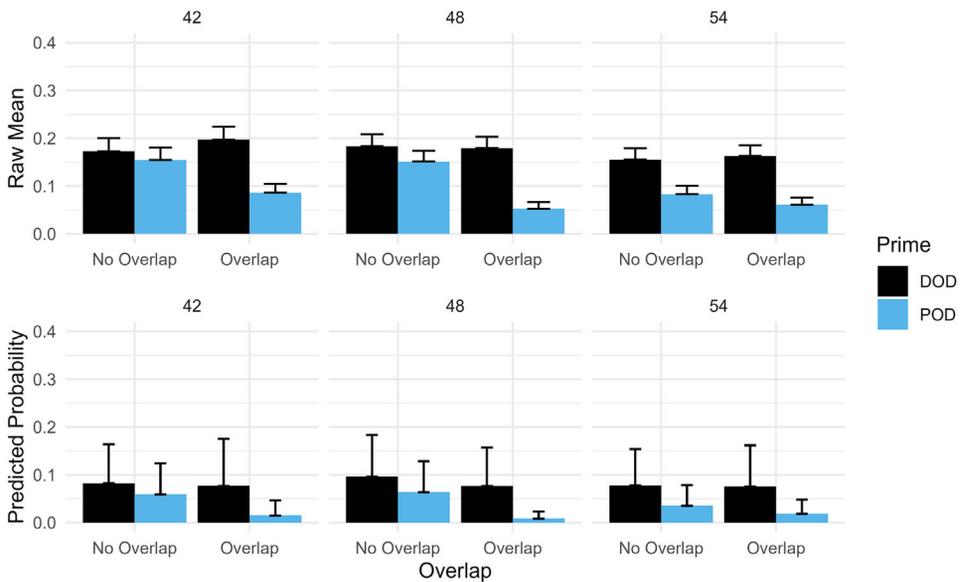


Fig. 5. Proportion of DODs produced for each priming condition, with and without lexical overlap, at each time point. Row 1 plots the raw data (the whisker indicates 1 standard error) and Row 2 plots the model's predicted values for each condition and each time point (whisker indicates one standard deviation of the condition's posterior distribution, roughly the Bayesian equivalent of a standard error).

time point separately, whereas using a continuous time variable would have forced estimated effects at 42 and 48 months to differ by the same amount as effects at 48 and 54 months. While it is conceptually possible to add a second level of participant-level random effects (by participant and by session within participant), including the second level of random effects would be redundant with the by-participant random slopes for time (and its interactions with other variables). We, therefore, included random effects by participant and by item.

3.1.7.1. Analysis of abstract priming effect and lexical boost: Output for the longitudinal model is available at <https://rpubs.com/sdonnelly85/DativeLongitudinal>. Coefficients and credible intervals are in Table 5. There was strong evidence for both the abstract priming effect and lexical boost ($b = .62$, $CI = .12: 1.15$, $P(b) > 0 = 98\%$; and $b = 1.48$, $CI = .51: 2.56$, $P(b) > 0 = 99\%$, respectively). However, we found no evidence for the predicted changes in the abstract priming effect or lexical boost with age (Time 54 * Prime Structure: $b = .31$, $CI = -.41: 1.01$, $P(b) < 0 = 22\%$; Time 48 * Prime Structure: $b = -.11$, $CI = -.72: .50$, $P(b) < 0 = 62\%$; Time 54 * Prime Structure * Overlap: $b = -.74$, $CI = -2.01: .51$, $P(b) > 0 = 15\%$; Time 48 * Prime Structure * Overlap: $b = .61$, $CI = -.39: 1.67$, $P(b) > 0 = 84\%$). Raw data and model-predicted values from this model are in Fig. 5.

We next derived time point-specific estimates of each effect from the posterior distribution of this model to compare to our time point-specific models. In particular, we derived the abstract priming effect, priming effect with lexical overlap, and lexical boost from each time

Table 5
Parameter estimates from longitudinal model

<i>Predictors</i>	Model 1 <i>Log-odds</i>	Model 3 <i>Log-odds</i>
Intercept	−3.03 (−4.37: −1.56)	−2.89 (−4.33: −1.34)
54 months	−0.26 (−0.80: 0.26)	−0.27 (−0.91: 0.40)
48 months	0.22 (−0.26: 0.70)	0.17 (−0.45: 0.79)
Prime (DOD)	0.62* (0.12: 1.15)	0.76 ^a (−0.18: 1.72)
Overlap	−1.00 (−1.65: −0.36)	
54 months * Prime	0.31 (−0.41: 1.01)	0.17 (−0.89: 1.19)
48 months * Prime	−0.11 (−0.72: 0.50)	−0.41 (−1.38: 0.55)
54 months * Overlap	0.49 (−0.32: 1.39)	
48 months * Overlap	−0.38 (−0.89: 0.15)	
Prime * Overlap	1.48* (0.51: 2.56)	
54 months * Prime * Overlap	−0.74 (−2.01: 0.51)	
48 months * Prime * Overlap	0.61 (−0.39: 1.67)	
Bias Mismatch		−1.13 (−2.04: −0.33)
54 months * Bias Mismatch		0.21 (−0.75: 1.19)
48 months * Bias Mismatch		−0.04 (−1.04: 0.94)
Prime * Bias Mismatch		0.47 (−0.93: 2.05)
54 months* Prime * Bias Mismatch		0.31 (−1.38: 1.96)
48 months* Prime * Bias Mismatch		0.59 (−0.96: 2.18)
<i>N</i>	100 <i>Blind_ID</i>	100 <i>Blind_ID</i>
Observations	6 <i>Target.verb</i> 2865	6 <i>Target.verb</i> 1355

Note. Posterior probabilities were calculated for (a) Prime, (b) Prime * Overlap, and (c) Prime * Biasmismatch and their interactions with age. We did not calculate posterior probabilities for other parameters because they were not of substantive interest and there was, therefore, no directional hypothesis to test.

* $p > .95$ in hypothesized direction.

* $p > .85$ in hypothesized direction.

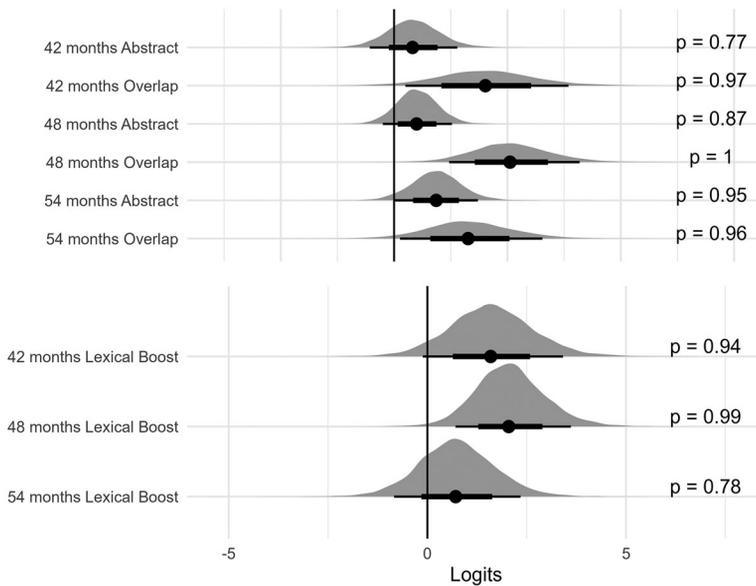


Fig. 6. Posterior distributions for the abstract priming effect and lexical boost at each time point on the logit scale (derived from longitudinal Model 1). The top panel depicts the posterior distributions (and associated 90% credible intervals) for the priming effects with and without overlap at each of the three time points. As can be seen, at 54 months, the majority of the posterior distributions for both effects ($> 95\%$ in each case) are positive, providing strong evidence for the existence of priming with and without overlap. However, at 42 and 48 months, the priming effect only differs from 0 when there is lexical overlap. The bottom panel contains the estimated lexical boost at each time point. As can be seen, there is evidence that the lexical boost was positive at 42 and 48 months, but not 54 months.

point (all on the logit scale). As all effects were hypothesized to be positive, we used the proportion of the posterior distribution greater than 0 to determine the strength of each effect (the more posterior probability greater than 0, the more certainty that the effect was positive). Parameter estimates of these effects are illustrated in Fig. 6. As can be seen, while there was no evidence of abstract priming effect at 42 months, there was weak-to-strong evidence of a lexical boost, driven by strong evidence of priming when there was verb overlap. A similar pattern was observed at 48 months, with weak evidence of priming and strong evidence of a lexical boost, driven by strong evidence of priming with verb overlap. At 54 months, there was strong evidence of abstract priming, and no evidence of a lexical boost.

3.1.7.2. Analysis of prime surprisal: We next examined the prime surprisal effect (Model 2). There was no evidence of a prime surprisal effect ($b = .47, CI = -.93: 2.05, P(b) > 0 = 70\%$), and no evidence for the predicted interaction between prime structure, bias mismatch, and time (Time 54 * Prime Structure * Mismatch: $b = .31, CI = -1.38: 1.96, P(b) < 0 = 37\%$; Time 48 * Prime Structure * Mismatch: $b = .59, CI = -.96: 2.18, P(b) < 0 = 26\%$). Fig. 7 plots the model's predictions against the raw data for each time point.¹⁴ Fig. 8 plots the prime surprisal effects, and priming effects for matching and mismatching prime/structure

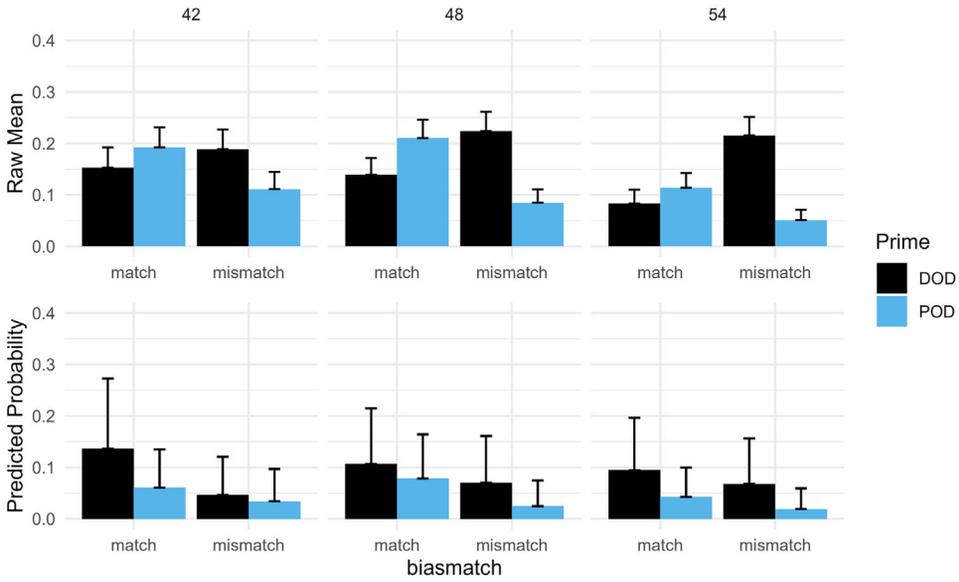


Fig. 7. Proportion of DODs produced for each priming condition, when a prime verb's bias matched or mismatched the prime structure, at each time point. Row 1 plots the raw data (the whisker indicates 1 standard error) and Row 2 plots the model's predicted values for each condition and each time point (the whisker indicates one standard deviation of the condition's posterior distribution, roughly the Bayesian equivalent of a standard error).

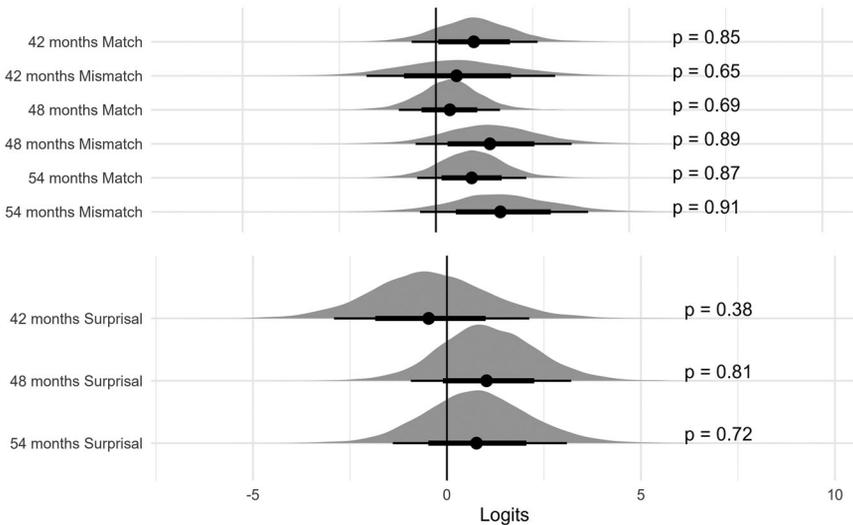


Fig. 8. Estimates of the prime surprisal effect at each time point on the logit scale (derived from longitudinal Model 2). As can be seen, less than 85% of the posterior distribution was positive at each of the three time points, indicating that there was no evidence for a prime surprisal effect.

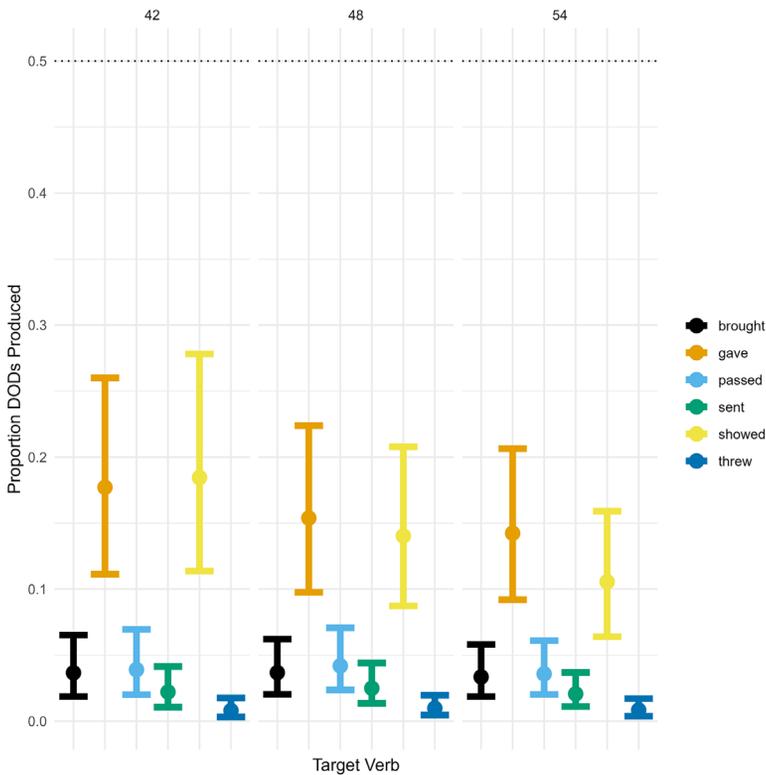


Fig. 9. Proportion of DODs produced for each verb at each of the three time points in Study 2. Circles represent the mean posterior probability of producing a DOD for a given verb at a given time point (derived from the model). Whiskers represent 90% credible intervals for the means.

pairings at each time point. There was no evidence for the prime surprisal effect at any of the three time points.

Following Study 1, we also examined the development of verb bias effects. In particular, we fit a model with fixed and random effects of time (by target verb and participant). We then plotted the posterior distribution for the proportion of DOD responses for each verb at each time point. Results are plotted in Fig. 9. Participants produced more PODs for all verbs, with slightly higher proportions of DODs for *gave* and *showed*, with no evidence of developmental changes in verb biases in this time window, which was relatively short compared to Study 1.

4. General discussion

In this paper, we tested several developmental hypotheses from prediction-based learning accounts of syntactic structure, focusing on three prominent syntactic priming effects: (i) abstract priming, (ii) the lexical boost, and (iii) prime surprisal, all using the English dative alternation. We examined these effects in large cross-sectional and longitudinal samples, which allowed us to overcome several methodological limitations present in previous studies. Our data are unprecedented in their scope in the developmental literature. Notably,

our combination of a large cross-sectional sample coupled with longitudinal data where children were sampled in narrow age bands across time affords us greater certainty concerning questions regarding both the presence and emergence of syntactic priming effects in children, and importantly, their developmental dynamics (or lack thereof). This in turn allows us to evaluate key developmental predictions of prediction-based accounts of the acquisition of syntax.

Before we evaluate our results in light of our hypotheses, we first summarize the pattern of results we observed across the two studies. In Study 1, our cross-sectional study, we found strong evidence for the presence of both abstract priming and the lexical boost, neither of which interacted with age. Notably, while there was some hint that the abstract priming effect decreased with age, this effect was weak and dependent on a statistically dispreferred random effects structure. We also found weak evidence for a prime surprisal effect, which did not interact with age. However, an exploratory analysis revealed a relatively protracted period of development for verb bias effects. In our longitudinal Study 2, we found early evidence for the existence of lexically restricted priming at both 42 (weak evidence) and 48 (strong evidence) months, but abstract priming appeared later, showing a clearer presence at 54 months. There was very little evidence of prime surprisal effects across the three time points. While we observed weak evidence of a prime surprisal effect in our time point-specific model at 48 months, this effect was not confirmed in our longitudinal model. Despite the fact that there were differences in the emergence and strength of the different priming effects across the longitudinal study, our overall analyses revealed none of the predicted interactions with age.

4.1. The emergence of the abstract priming and lexical boost effects

Overall, our results indicate that for the dative, verb-overlap appears to be a necessary precondition for priming before 54 months (Savage et al., 2003; Tomasello, 2003). At first pass, these results seem inconsistent with several studies of dative priming in young children (Peter et al., 2015; Rowland et al., 2012; Thothathiri & Snedeker, 2008). Both Rowland et al. (2012) and Peter et al. (2015), who used a similar task to ours, observed abstract priming effects without a lexical boost in their youngest samples. However, wide age ranges in their samples make their results difficult to compare with our own. In particular, the age ranges of the youngest groups in these two studies exceeded the range of ages across the three time points in our longitudinal study (21 and 23 months, respectively, vs. 12 months). Therefore, our longitudinal sample allowed us to test for changes that would have been undetectable in these studies (see Messenger et al., 2022 for a discussion).

While the results can be reconciled within the developmental literature on dative priming, they are intriguingly different from Kumarage et al. (2022), who observed abstract priming prior to the lexical boost with the active-passive alternation in the same children studied in Study 2 of this paper. In particular, Kumarage et al. (2022) observed an abstract priming effect from the earliest age point, when the children were 36 months, but did not observe a robust lexical boost until 12 months later. This difference is striking: the developmental ordering of these effects differed across two constructions within the same sample, pointing to alternation-specific developmental profiles in English-speaking children, with the dative showing a large degree of lexical-specificity and the passive revealing evidence for early abstract knowledge.

Under the assumption that syntactic priming represents a fairly direct method of assessing linguistic representations (Branigan & Pickering, 2017), one logical explanation for this difference is an asynchrony in the degree of abstractness in the dative versus the passive in children aged 3;0–4;0 years. There are several a priori reasons in support of this possibility. First, in comparison to the English passive, which can be used with a variety of verb types and tokens which in turn describe different between-participant relations between the subject and oblique arguments (e.g., agent-patient, theme-experiencer, experiencer-theme), the dative alternation is both restricted in the types of verbs that can be used in the structure and rigid in the mapping of syntactic arguments to thematic roles (Goldberg et al., 2004; Goldberg, 1995). Second, the dative's use in both child-directed and children's speech is fairly lexically specific, with a significant proportion of occurrences in one highly frequent verb—*give* (Goldberg et al., 2004; Gropen et al., 1989). These linguistic and distributional differences between the active-passive and dative alternations provide the conditions under which we might expect different patterns of abstraction within the same children. Across many domains, it is generally assumed that highly variable input challenges learners but quickly forces generalization, whereas less variable input is less challenging but resists generalization (for review, see Raviv, Lupyan, & Green, 2022). Some prediction-based models allow variation in the abstractness of representations for different constructions within the same model at the same point of development (Chang et al., 2006). For example, the Dual-path model exhibited developmentally sensitive verb-based bias for the transitive (fig. 25 in Chang et al., 2006) and showed sensitivity to thematic role overlap for priming with the locative, but not for the transitive (Chang et al., 2006; Chang et al., 2003). Thus, the greater lexical, conceptual, and functional variability of the passive could create early generalization patterns in comparison to the dative.

Another factor that may have contributed to differences in the abstract priming across the two studies is differences in the stimuli. Recall that we changed one feature of our primes from Study 1 to Study 2; notably, in Study 2, we used only generic definite NPs to refer to recipient arguments in order to remove any problems young children may have had with naming unfamiliar characters. However, this may have added some difficulty to the task. For example, Rowland and Noble (2010) showed that 3- and 4-year-old English-speaking children do not correctly interpret DO datives containing novel verbs where the recipient is a definite full lexical NP (e.g., *I'm meeping the duck the frog*), but they do when the recipient is named using a proper noun (e.g., *I'm meeping Donald the frog*). This pattern is consistent with the distributional properties of child-directed speech: Rowland and Noble reported that 94% of DODs in the speech of four English-speaking mothers occurred with a recipient that was expressed as either a proper noun or pronoun, whereas 76% of the theme arguments were expressed as full NPs. Thus, children's early knowledge of the DO dative may be particularly sensitive to statistical properties of the post-verbal arguments. If this is the case, the children may have had difficulty parsing the DO primes because distinguishing between two lexical NPs is more difficult than distinguishing between two NPs that differ in their discourse prominence (a kind of similarity-based interference that is compounded by fragile verb-general structural knowledge, Gennari, Mirković, & MacDonald, 2012). In these circumstances, the added value of verb overlap may have been the essential ingredient for children to be primed. It may also explain why we observed both abstract priming and

a lexical boost in Study 1: the materials were more aligned with the children's expectations about the realization of the post-verbal arguments and thus their syntactic representations. Thus, while the relatively late emergence of abstract priming effect is surprising given prior empirical work (Peter et al., 2015; Rowland et al., 2012), it is consistent with any theory that assumes structures must be abstracted gradually from lexicalized representations.

4.2. *The trajectories of the abstract priming effect and lexical boost*

Given the relatively late emergence of abstract priming (54 months), it is not surprising that we did not observe the predicted decrease in abstract priming with age in our longitudinal sample. However, we also did not observe a decrease in the abstract priming effect in our cross-sectional sample, where evidence of the abstract priming effect was clear.

One explanation for this is that experience-dependent changes in the magnitude of prediction error may not reliably lead to detectable changes in the magnitude of the priming effect in young children. Recall that in order to be primed, young children need to possess a sufficiently abstract representation of the target structure (Chang et al., 2006; Kumarage et al., 2022; Messenger et al., 2022). As children's knowledge of syntactic structures becomes more abstract with age, developmental changes in the magnitude of the priming effect may be very subtle and difficult to statistically detect. Consistent with this possibility, Kumarage et al. (2022) found the strongest evidence of a decrease in the magnitude of the priming effect when children who did not produce a passive in the first session and data from one problematic verb were removed. Likewise, in our cross-sectional study, we found weak evidence of a decrease in the magnitude of the priming effect when a simpler, though statistically dispreferred, random effects structure was specified.

Similarly, because we did not observe abstract priming prior to the lexical boost in our longitudinal study, it is not surprising that the lexical boost did not increase with age, as it likely reflected lexically restricted priming rather than the sort of lexical boost observed in adults. However, it was surprising that we did not observe such an effect in our cross-sectional sample, given how reliably previous developmental studies have observed increases in the lexical boost with age (Kumarage et al., 2022; Peter et al., 2015; Rowland et al., 2012) and the clear evidence that episodic memory capacity increases with age (Finn et al., 2016; Lum et al., 2010). One possibility is that the abstract priming effect and lexical boost reflect the same mechanism (Carminati, van Gompel, & Wakeford, 2019; Huang et al., 2023; van Gompel, Wakeford, & Kantola, 2023; Pickering & Branigan, 1998). However, this account is difficult to reconcile with the drastically different time courses of the two effects (Branigan & McLean, 2016; Hartsuiker et al., 2008; Mahowald et al., 2016, though see Malhotra, Pickering, Branigan, & Bednar, 2008), the similarity of verb overlap effects in both sentence-recall and priming tasks (Zhang et al., 2020), and the fact that in prior developmental studies the lexical boost has increased with age (Kumarage et al., 2022; Peter et al., 2015; Rowland et al., 2012).

An alternative explanation is the difference in materials between our study and prior developmental studies. Many prior studies have used experiments designed to be games, which may have encouraged children to explicitly attend to words in the prime sentence. For example, Kumarage et al. (2022) employed a "Snap" game, in which children won points if they

noticed their card matched that of the experimenter. While matching cards only occurred on intransitive filler trials, it is possible that this design encouraged children to more explicitly attend to the experimenter's cards on all trials. In contrast, in the present experiment, children and experimenters took turns describing animated clips. If the game-like design of prior studies encouraged more explicit processing of the prime and target sentences, those studies would be better suited to detect age-related changes in the ability to notice the overlap between the prime and target verb, which could create a lexical boost. In other words, the relatively low explicit memory demands of our task may have been sufficient for observing an experimental effect but not sufficient for reliably estimating between-participant age-related differences in the magnitude of this effect (Hedge, Powell, & Sumner, 2018). This is consistent with the adult literature, where the existence and magnitude of the lexical boost varies considerably across tasks (Huang et al., 2023; van Gompel et al., 2023), which presumably differ in their explicit memory demands (Chang et al., 2012).

4.3. *The emergence and trajectory of the prime surprisal effect*

We found little convincing evidence for prime surprisal effects in either sample. On balance, this is consistent with prior research on prime surprisal effects in children, which has reported mixed results: while Peter et al. (2015) found prime surprisal effects in two groups of children (mean ages = 4;0 and 5;11), Fazekas et al. (2020) did not observe a prime surprisal effect in a similarly aged group of children (mean age, 6;4). This stands in contrast to the adult literature, where the prime surprisal effect emerges more consistently (Benolet & Hartsuiker, 2010; Fazekas et al., 2020; Jaeger & Snider, 2013; Peter et al., 2015). The results in the present studies, therefore, are difficult to reconcile with theories that argue that priming is only due to structural prediction error (the likelihood of seeing the structure; Jaeger & Snider, 2013). However, from the perspective of theories that also posit that language acquisition involves a process of abstracting structures from lexicalized representations (Chang et al., 2006), then the lack of convincing evidence for the prime surprisal results could reflect developmental changes in lexical-structure links in our population.

To examine these development changes, we performed exploratory analyses of the trajectory of the verb bias. At the earliest time points, children were strongly POD biased for all verbs and gradually became less POD biased over time. In contrast to the Peter et al. (2015) 3- to 6-year-old children who used DOD more than 30% of the time with DOD-biased verbs, our cross-sectional children did not exhibit this level of DOD use until 7 years of age. The effect of this variability can be understood through the simulations by Twomey et al. (2014), who examined the development of verb bias for the locative alternation within the Dual-path model. They found that, initially, the model mapped alternating verbs to the most preferred structure (for the dative, POD is preferred and acquired earlier). Critically, the less preferred structure was initially a verb-specific structure, so a give-DOD structure might be different from a send-DOD structure, and only by abstracting across these separate verb-specific structures is the abstract DOD structure acquired. Therefore, even if a large amount of prediction error is generated early in development, the weight changes in the model will involve verb-specific structures and, therefore, resulting prime surprisal effects may be weak and variable.

Why age-related differences between children acquiring UK and Australian English exist is difficult to ascertain, although it is possible that there are dialect differences in structural preferences derived from usage patterns (as there are between Australian and U.S. English, Bresnan & Ford, 2010).

As our fixed, binary measure of verb preference did not capture the developmental changes during this period, it is perhaps not a surprise that we did not observe strong evidence for a prime surprisal effect. While it would have been conceptually possible to calculate child-specific verb biases for our analyses, we believe this would have been problematic for two reasons: First, such estimates would have been based on two trials per child, meaning that between-child differences would mostly reflect noise. Second, controlling for participants' responses could induce bias and undo randomization, making parameter estimates uninterpretable (McElreath, 2018). In the present paper, we tried to avoid this by using the verb bias estimates from Study 1 in Study 2 (since in Study 2, verb biases were calculated independently from the participants' responses). Therefore, future research should aim at estimating participant-specific verb biases in a separate task or block of trials and examine how such biases interact with prime structures (e.g., Fazekas et al., 2020).

4.4. Conclusion

In the current paper, we have presented the most comprehensive study of the development of syntactic priming to date, including large cross-sectional and longitudinal data sets. Our results provide mixed support for predictions for prediction-based models of language acquisition. We found clear evidence of both abstract priming and the lexical boost, and weak evidence for the presence of prime surprisal, which was likely affected by developmental changes in participants' verb-structure preferences. Developmental change, by and large, was less obvious in the data although it did emerge in Study 2, where lexically based priming preceded abstract priming. While the initially lexically restricted priming is inconsistent with prior studies (Rowland et al., 2012; Peter et al., 2015), the dissociation between our results and those of Kumarage et al. (2022) seems compatible with explicit memory and prediction-based approaches once the distributional properties of the different structures are considered. Additionally, the fact that developmental effects for both abstract priming and the lexical boost were found for the active-passive alternation in the same children in Study 2 by Kumarage et al. (2022) suggests that the absence of developmental effects is structure-specific in important ways. We argued that these alternation-specific distributional properties involving verb–structure relations also complicated the observation of the prime surprisal effect.

Overall, the results from both studies demonstrate both the challenge and value of acquisition data when evaluating psycholinguistic theory. Effects that are present in adults can be typically assumed to be stable, whereas this is not the case in children, whose linguistic systems are rapidly changing across childhood. While it is typically assumed that much of this development is complete by around 5 years, the data we have presented suggest that subtle fine-tuning of the system continues beyond that age. Future work that studies this fine-tuning in structures like the dative, particularly using longitudinal designs, would be a welcome addition to the literature.

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Notes

- 1 Gender was not reported for one child.
- 2 One of the two experimenters, who tested 40 of the 140 children, deviated from the procedure and did not ask children to repeat the prime sentence. Therefore, we ran a number of exploratory analyses testing the various priming effects for each experimenter separately. We have added footnotes to indicate when results differed across the two experimenters. However, because the prediction-based learning account assumes that priming reflects implicit learning resulting from prediction error during *language comprehension*, we do not think it would predict that including prime repetition would influence the relevant effects. Therefore, given the post hoc nature of such analyses and the lack of a candidate mechanism for explaining such discrepancies, we have avoided interpreting these results. However, we have posted full results for these analyses, as well as scripts to reproduce these analyses, to the osf page. We have also included these results in the Supplementary Materials for this paper.
- 3 This was done because we included random effects by target verb in our analyses.
- 4 In other words, all verb overlap trials in which the participant produced the incorrect verb were removed and nonoverlap trials in which the participant produced the same verb as the prime were removed.
- 5 A truncated normal distribution is a normal distribution that has been truncated at 0, which is sometimes used as a prior for standard deviations, which cannot be negative (Nicenboim, Schad, & Vasishth, 2023).
- 6 Brms's default priors for the other parameters are uninformative, including Student's t distribution with 3 degrees of freedom, a median of 0, and a scale parameter of 2.5 for the intercept, uniform distributions for the other fixed effects, and lkj distributions with a shape distribution of 1 for latent correlations.
- 7 Because participants saw each target verb twice, we considered a model with full random effects by participant, target verb, and participant \times target verb. However, when we compared a variance components model (a model with only intercepts and no predictors) with this structure to a variance components model with random factors by target verb and participant, the latter model fit better. We, therefore, opted for this simpler model. See Accompanying R Markdown file for full details.
- 8 When data from the two experimenters were considered separately, we found strong evidence for the abstract priming effect for both experimenters. We found weak evidence for the lexical boost for participants who were not asked to repeat the prime ($P(b) > 0 = .86$); however, this likely reflects a reduction in power for testing an

interaction in this much smaller sample ($N = 40$) as this effect was large with wide credible intervals ($b = 1.17$, $CI = -1.05: 3.91$). We found strong evidence for the predicted interaction between prime structure and age when participants were asked to repeat the prime ($P(b) < 0 = .96$). However, the prediction-based account of learning does not predict that such an effect would be specific to participants who were asked to repeat the prime structure. See osf files (and Supplementary Materials) for full details.

- 9 Fitting the model to the full data set would require us to estimate a four-way interaction between prime structure, lexical overlap, bias mismatch, and age. This model would be extremely difficult to interpret and we doubt we have enough data to reliably estimate such complex higher-order interactions.
- 10 We found strong evidence for the prime surprisal effect for participants who repeated the prime sentence ($P(b) > 0 = .99$). This was offset by a negative prime surprisal effect of similar magnitude for participants who did not repeat the prime sentence ($b = 2.12$ and $b = -1.86$, respectively). We see no reason why a prediction-based learning account would predict such a difference and believe these differences reflect the relatively small numbers of trials included in these analyses (with smaller numbers of trials than the analyses of the lexical boost and abstract priming effect and smaller numbers of participants than the analyses reported in the main text). See osf files (and Supplementary Materials) for full details.
- 11 There was an error in the counterbalancing lists for lists 6 and 8. In particular, for both lists, *gave* was administered as the target verb instead of *passed* on trial 7 (see Table 3). As a result, children who received these two sequences had seven lexical overlap trials and five nonoverlap trials, and three trials with *gave* as the target verb and one trial with *passed* as the target verb. As this only affects one (of 12) trial, on two (of eight) lists this is a very small deviation from a balanced design. However, we ran all analyses both with and without Trial 7 on these Lists (results did not differ but see R Markdown file for models).
- 12 One of these participants completed only the 42-month session. Dropping this participant reduced the sample for our longitudinal model by 1.
- 13 Since this participant only completed a 54-month session, doing so reduced our sample for our longitudinal model by 1.
- 14 Readers may notice that Fig. 7 appears to suggest a prime surprisal effect at 42 and 54 months. However, this is due to aggregation bias. In particular, the interaction between prime structure and bias was not perfectly balanced within each target verb and, as a result, the distribution of scores within each target verb looks different than the distribution of scores after summing over target verbs. This imbalance is accommodated by our by-verb random effects. See Appendix D for full details.
- 15 Residuals for this model revealed a significant, but weak, departure from normality.

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Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Table S1. Parameter estimates from models using data from Experimenter 1 and Experimenter 2 separately.

Table S2. Parameter estimates from models of prime surprisal using data from Experimenter 1 and Experimenter 2 separately.

Appendices