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Implicit learning of structure across time: A longitudinal investigation of syntactic priming in young English-acquiring children

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ABSTRACT

Theories of language acquisition vary significantly in their assumptions regarding the content of children's early syntactic representations and how they subsequently develop towards the adult state. An important methodological tool in tapping syntactic knowledge is priming. In the current paper, we report the first longitudinal investigation of syntactic priming in children, to test the competing predictions of three different theoretical accounts. A sample of 106 children completed a syntactic priming task testing the English active/passive alternation every six months from 36 months to 54 months of age. We tracked both the emergence and development of the abstract priming effect and lexical boost effect. The lexical boost effect emerged late and increased in magnitude over development, whilst the abstract priming effect emerged early and, in a subsample of participants who produced at least one passive at 36 months, decreased in magnitude over time. In addition, there was substantial variation in the emergence of abstract priming amongst our sample, which was significantly predicted by language proficiency measured six months prior. We conclude that children's representation of the passive is abstracted early, with lexically dependent priming coming online only later in development. The results are best explained by an implicit learning account of acquisition (Chang, F., Dell, G., S., & Bock, K. 2006. Becoming Syntactic. Psychological Review, 113, 234–272), which induces dynamic syntactic representations from the input that continue to change across developmental time.

Introduction

The core aim of psycholinguistics is to explain the architecture and processes underlying the human capacity for language. This includes both how linguistic representations are used during the business of language production and comprehension, but also how those representations emerge and change across ontogeny. One method that is particularly useful in investigating the nature of linguistic representations is syntactic priming, the process whereby processing a specific syntactic structure increases the frequency of its use in subsequent discourse (Bock, 1986). For example, in Bock's (1986) seminal study, adult participants were more likely to produce a passive description like '*the church is being struck by lightning*' after saying a passive prime (*the referee was punched by one of the fans*) than after saying an active prime (*one of the fans punched the referee*). In the current study, we present the first longitudinal study of syntactic priming of the English active–passive alternation in monolingual children aged 3–4.5 years. In doing so, we address several methodological problems present in the developmental literature, enabling us to test competing theoretical possibilities concerning the emergence and development of syntactic knowledge during a period of rapid developmental change.

Syntactic priming is an ideal method to investigate syntactic development because priming effects are assumed to reflect common representations across prime and target. Since syntactic priming is observed in the absence of shared open- or closed-class lexical content, semantic content, or sentence prosody (Bock, 1989; Bock & Loebell, 1990), the nature of that shared representation is in many circumstances argued to be *abstract* syntactic knowledge, or syntactic procedures that operate

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over abstract categories (Branigan & Pickering, 2017). The effect is robust, at least in adults: a recent meta-analysis of production priming studies concluded that there is strong evidence for syntactic priming without influence from publication bias (Mahowald et al., 2016).

Priming effects are larger when the prime and target share the same main verb, the so-called *lexical boost effect* (e.g., *the bird was hugged by the dog* primes *the mouse was hugged by the chicken* to a greater extent than *the cat was pushed by the boy* does). Although the lexical boost effect is robustly larger in magnitude than abstract priming (Mahowald et al., 2016), it appears to behave slightly differently, suggesting it may have a different source. Notably, in comparison to abstract priming it is short-lived (Bock & Griffin, 2000; Hartsuiker et al., 2008), and is more difficult to observe in young children (Peter et al., 2015; Rowland et al., 2012; c.f. Branigan & McLean, 2016). This has led to suggestions that it might reflect a different mechanism to that responsible for abstract priming, one which develops with age (Chang et al., 2006, Chang et al., 2012). We next discuss mechanistic explanations of priming and the lexical boost and how they relate to the key conceptual divisions in language acquisition.

Mechanisms of syntactic priming effects

Broadly speaking, there are two competing mechanistic explanations for syntactic priming phenomena. The first attributes priming to *residual activation* of shared linguistic representations between prime and target, which underlies both abstract priming and the lexical boost (Pickering & Branigan, 1998). The second account attributes abstract priming to error-based (implicit) learning (Chang, 2002; Chang et al., 2006) and assumes the lexical boost is due to separate, potentially more explicit, memory processes (Chang et al., 2012). We discuss each in turn.

Priming as residual activation

On the residual activation approach, processing a prime sentence activates a mental representation of its syntactic structure, thereby increasing the structure's short term accessibility (Pickering & Branigan, 1998). The model assumes that individual verbs are linked to structural 'nodes' denoting the syntactic frames in which they can occur. For example, the verb *chase* can occur in either an active or passive transitive sentence. If a speaker hears *the cat was chased by the dog*, the passive node becomes activated, increasing the likelihood that the structure will be used again. As both syntactic nodes and the connections between verbs and syntactic nodes are subject to residual activation, the model explains both the abstract priming effect and lexical boost using the same mechanism. This type of mechanism cannot fully explain priming effects, since findings suggest that priming can endure across time periods longer than residual activation is presumed to persist (Bock & Griffin, 2000; Hartsuiker et al., 2008; though even abstract priming effects decay, Bernolet et al., 2016). However, the theory continues to be influential. In particular, the assumption that priming is due to the activation of interconnected nodes of grammatical representations, and thus reflects the presence of structural knowledge, still holds (e.g. Branigan & Pickering, 2017; Hartsuiker & Bernolet, 2017). It is in this spirit that we adopt the theory and connect it to models of acquisition.

While the residual activation model makes clear predictions about the adult data, predictions about syntactic priming in children are less clear because the model assumes fully abstract representations of syntactic structure. However, whether and when young children possess such representations is a matter of ongoing debate (Ambridge & Lieven, 2011). As we are aware of no activation-based priming account which makes strong assumptions about early syntactic knowledge, we, therefore, consider how this model would behave when assuming each of two accounts of early syntactic representations: early abstractionist (Gertner et al., 2006; Lidz et al., 2003; Valian, 2014; Yang, 2018) and usage-based accounts (Ambridge & Lieven, 2015; Tomasello, 2003). As these two accounts represent opposite sides of the theoretical spectrum, we are able to map out a broad space of predictions that are plausibly consistent with the residual activation model. We derive our predictions by combining accounts, since developmental theories lack detail regarding their architectural assumptions and how these influence and change in response to processing structure (see also Branigan & Mclean, 2016), and adult theories typically assume a competent speaker and thus do not integrate developmental constraints.

On the one hand, *early abstractionist* accounts of acquisition assume that children acquire language guided by innately conferred or constrained processes that enable them to deduce a language-specific and abstract grammatical system from very early in development, a process that depends on input for configuring but not constructing abstract linguistic categories (Bencini & Valian, 2008; Gertner et al., 2006; Lidz et al., 2003; Messenger & Fisher, 2018; Valian, 2014; Yang, 2018). Thus, a prediction derived from the early abstractionist approach is that abstract (i.e., verb-independent) priming will be observed early in development, demonstrating that children possess abstract knowledge. For a language-specific structure like the passive, which is our focus here, the assumption is that children have sufficiently abstract representations of syntactic categories such as *subject* and *object* and can flexibly map them to thematic roles like *agent* and *patient*, independently of lexically-specific knowledge, once they have acquired the syntactic frame (Bencini & Valian, 2008; Messenger & Fisher, 2018). While the account does not make any specific predictions about the lexical boost, assuming that the early emerging abstract knowledge is processed within an architecture that links specific verbs to abstract structure (as is assumed in the residual activation account), we can deduce that having adult-like abstract syntactic knowledge would result in an adult-like pattern of priming across all condition types. That is, the lexical boost should emerge at the same time as the abstract priming effect and should be higher in magnitude. Finally, since abstract knowledge is present early, the early abstraction account does not predict significant changes in priming across development. For the sake of clarity and convenience, we call the early abstraction account the *Early Syntax* instantiation of the residual activation account (or RA-Early Syntax).

On the other hand, emergentist and usage-based theories of development differ from early abstractionist accounts in that they do not assume children possess early or innate syntactic representations (Ambridge & Lieven, 2015; Savage et al., 2003; Tomasello, 2003). Instead, this approach argues that children gradually abstract over item-based instances to induce an adult-like grammar using general-cognitive learning mechanisms. For instance, a child's early use of the passive in *the cat was chased by the dog* may only be indicative of children knowing the passive structure as contingently linked to the verb *chase*, and thus they could not generalise the structure beyond the verb. On these functional accounts, abstraction takes developmental time, since children must induce generalised syntactic representations from the evidence available in the input (Ambridge & Lieven, 2015, 2011; Tomasello, 2003), which gradually become more abstract with experience. Thus, the approach does not predict early abstract syntactic priming effects; since early syntactic knowledge is lexicalised, the approach predicts the initial emergence of lexically-based priming effects in the absence of abstract priming.[1] For the sake of clarity and convenience, throughout this paper we call this the *Late Syntax* instantiation of the residual activation account (or RA-Late Syntax).

Taken together, depending on the posited nature of children's early syntactic knowledge, that residual activation mechanisms operate upon, accounts of syntactic acquisition make different predictions about the development of priming effects. If one assumes children's earliest syntactic knowledge is fully abstract, the prediction is that both syntactic priming and the lexical boost should be observable once children begin

[1] We use the term lexically-based priming for priming of syntactic structures which relies solely on lexicalised syntactic knowledge and the term lexical boost for an increase in syntactic priming when there is lexical overlap between prime and target relative to when there is not.

producing the relevant grammatical construction (in this case, the passive). However, if one assumes children gradually construct abstract representations in an item-specific manner, the prediction that the earliest priming should be fully lexicalized, such that children exhibit a lexically-based priming effect prior to exhibiting abstract syntactic priming.[2]

Priming as error-based (implicit) learning

An alternative explanation of priming is that it reflects *Implicit Learning* of grammatical structure (Bock & Griffin, 2000; Chang et al., 2006; Fine & Jaeger, 2013). The most explicit articulation of this account comes from Chang et al.'s (2006) Dual-path model, which constitutes a theory of both the acquisition of grammatical structure and adult sentence production. A key feature of the model is that it acquires input-driven, language-specific syntactic categories via error-based learning. That is, using a Simple Recurrent Network (Elman, 1990) for the sequencing (syntactic) system and a separate meaning system for semantic information, the model makes next-word predictions based on its previous experience with the language. When those predictions are incorrect, such as when the model fails to predict a passive past participle after *the cat was* …, the network weights are adjusted via error-based learning, such that the likelihood that a passive will be predicted is higher. In this respect, the model acquires structure via priming, and each new experience with a given structure alters the likelihood that the structure will be subsequently used again. Thus, hearing a passive increases the probability that a passive will be used to describe a transitive event, relative to an active.

The model explains a diverse range of phenomena in language acquisition and adult language processing, including: the décalage between the comprehension and production of transitive sentences in acquisition (Chang et al., 2006), cross-linguistic differences in language production (Chang, 2009), structure dependence in the acquisition of subject-auxiliary inversion (Fitz & Chang, 2017), and the underlying nature of N400 and P600 event-related potential effects (Fitz & Chang, 2019). Naturally, the model explains many syntactic priming phenomena pertaining to abstract priming, including the fact that abstract priming persists over long periods, which the model explains as a signature of implicit learning of structure involving changes in representations rather than their activation (Kaschak et al., 2011). However, it crucially does not simulate the lexical boost effect, which is consistent with the observation that abstract priming and the lexical boost endure across different time frames (though Reitter et al., 2011 simulate the effect in one architecture by assuming two distinct mechanisms; see also Zhang et al., 2020). Chang et al. (2006) argued that this suggests the lexical boost is attributable to a different mechanism - specifically, explicit memory processes that may be more vulnerable to rapid decay (Chang et al., 2012).

The Chang et al. (2006) model makes predictions for priming in acquisition that differ from Early and Late Syntax accounts. Most broadly, the model predicts different developmental profiles for abstract priming and the lexical boost/lexically-based priming. In contrast to Early Syntax accounts, which predict stable abstract representations of syntax over development, the Implicit Learning model predicts that less experienced speakers will have syntactic representations that are based on fewer instances and are more susceptible to input (Rowland et al., 2012). Therefore, immature systems will be subject to greater error

Table 1
Summary of predictions for different priming models for developmental data across time.

	Abstract Priming		Lexical Boost/Lexically-based	
	Emergence	Development	Emergence	Development
RA - Early Syntax	Early	No change	Early	No change
RA - Late Syntax	Late	Increase	Early	Inverse U-shape
Implicit learning	Early	Decrease	Late	Increase

prediction, which predicts larger abstract priming effects in less experienced speakers. There is mixed evidence for this claim: grammatically less-skilled aphasic participants show larger abstract priming effects (Hartsuiker & Kolk, 1998), but adults do not show greater abstract priming in their second language than in their native language (Mahowald et al., 2016). In children, we would expect the abstract priming effect to *decrease* over the course of syntax acquisition (Rowland et al., 2012; however, this is contingent on children having first acquired the structure, see Messenger et al., in press). This prediction also differs to the Late Syntax account, which involves generalisation across lexically-specific representations rather than error-based learning, and therefore predicts an increasing abstract priming effect. In contrast, if the lexical boost is dependent on explicit memory, we should expect the opposite relation: the lexical boost effect should increase across development (Chang et al., 2012; Rowland et al., 2012). This is based on well-accepted findings demonstrating that implicit and explicit memory processes have different developmental schedules, with explicit/declarative processes continuing to develop throughout childhood and beyond (e.g., Finn et al., 2016; Lum et al., 2010).

Overall, then, we distinguish between three models. The different predictions regarding priming in acquisition over development derived from each model are summarised in Table 1.

There are a few things to note from Table 1. The first is that the three models all make different predictions regarding abstract priming. On our reading, they also make differing predictions regarding the lexical boost/lexically-based priming, although the specifics for the latter are less clear. Thus, the RA-Early Syntax approach should show this adult-like pattern early in development. The RA-Late Syntax approach predicts an early increase in lexically-based priming: since children's early grammatical knowledge is lexically-restricted, priming in conditions of lexical overlap should increase as children accumulate lexically-based representations and develop towards fully abstract knowledge. However, once abstract knowledge emerges there should be an increase in abstract priming relative to lexically-driven priming, and therefore a decrease in the "boost" provided by overlapping lexical content, which suggests an inverted U-shaped pattern.

Finally, we note an additional parameter that we bring to bear upon the nature and emergence of priming, and which can act as an additional source of evidence in constraining theory – individual differences (Kidd, Donnelly, et al., 2018; Kidd & Donnelly, 2020). Accounts of acquisition and processing that rely heavily on learning from the input, such as the RA-Late Syntax and Implicit Learning accounts, make straightforward predictions regarding individual variability. Notably, since children experience differences in their language exposure and have different learning rates, the emergence of the passive structure will be varied and predicted by their prior developmental states in a systematic matter. The predictions of the RA-Early Syntax approach are less clear. A straightforward reading of the approach as instantiating a traditional nativist approach that assumes continuity between the child and adults state leads to the prediction that there will be no systematic individual differences, only variability due to experimental noise (Crain et al., 2017; Crain & Thornton, 1998). However, RA-Early Syntax approaches differ on the continuity assumption, with more moderate approaches arguing that, for low frequency and language-specific structures like the passive, children may systematically vary in their knowledge and this will be linked to variables such as experience and processing ability (Messenger

[2] There is an alternative account (Reitter et al. 2011) of syntactic priming that is, in some ways, broadly consistent with the residual activation account. The model includes a short-term spreading activation mechanism, which we believe would combine with developmental accounts similarly to the residual activation model. However, this account also assumes a base-level activation mechanism to explain long-term priming effects. Given the model's complexity, it is not clear how it would behave when assuming non-adult syntactic representations and therefore difficult to speculate about its developmental predictions.

& Fisher, 2018).[3] Consequently, the study of individual differences provides important boundary conditions on models of acquisition.

Thus, it is important to also examine individual differences in the emergence of the passive priming effect. Such an analysis also tells us something important about what priming means in acquisition and the conditions under which it arises. There is a general assumption in the literature that lexically-independent priming is a signal of the presence of abstract syntactic knowledge (Branigan & Pickering, 2017), but as we have seen, in acquisition the existence of abstract knowledge at various ages is hotly debated. If the emergence of the priming effect is predicted by variability in language proficiency, it tells us the conditions under which priming arises in development (Kidd, 2012), and in concert with longitudinal data, how priming changes within individuals across time.

Past empirical work

A significant literature on syntactic priming in children exists, although to date all of these studies have either reported on one age group or have compared age groups in cross-sectional designs. These data suggest that, consistent with the RA-Early Syntax and Implicit Learning accounts, the abstract priming effect emerges early, though whether and how it develops across development is unclear (Bencini & Valian, 2008; Hsu, 2019; Peter et al., 2015; Rowland et al., 2012). Most studies testing children at 3 years, the youngest age group for which syntactic priming tasks appear achievable, have found some evidence of abstract priming of multiple structures (for passives: Bencini & Valian, 2008; Shimpi et al., 2007, but not Savage et al., 2003; for datives: Shimpi et al., 2007; and for SVO-ba Hsu, 2019), and by 4 years, evidence for abstract priming is consistent (e.g., Huttenlocher et al., 2004; Messenger, Branigan, McLean, et al., 2012; Messenger et al., 2011). However, 3-year-olds appear to have difficulty with the task: Bencini and Valian (2008) reported that 35% (28/81) of 3-year-olds in their study could not complete the task, and Shimpi et al. (2007) only found abstract priming in 3-year-olds when they were asked to repeat, not just listen to, the prime sentence. These are both suggestive of variability in young children's ability to be primed and therefore, developmental change. Given the ambiguous evidence for abstract priming at 3 years compared to 4 years of age, the period of development between these age groups appears to be crucial for observing this change.

Evidence related to the developmental trajectories of abstract priming effects is less clear. In studies using the dative alternation, Rowland et al. (2012) found that children's abstract priming effect was larger than that of adults, whilst Peter et al. (2015) found the opposite. Hsu's (2019) investigation of the SVO-*ba* alternation in Mandarin found equivalent abstract priming across 3-, 4- and 6-year-olds. In studies using the passive alternation, Messenger, Branigan, McLean and Sorace (2012) found no difference between the size of children's and adult's priming effects but Messenger (2021) found a marginally significant decrease in abstract priming from children to adults, and while Messenger, Branigan and McLean (2012) found that 6- and 9-year-olds were equally likely to produce a passive after a passive prime, they also found that 6-year-olds produced more invalid responses with passive syntax but reversed thematic roles (e.g., producing *the chicken was hugged by the mouse* to describe a scene in which a chicken is hugging a mouse). A shortcoming of these past developmental studies is that they were cross-sectional in design; only longitudinal research can unambiguously determine the developmental trajectories of the effects.

The lexical boost effect is not consistently found in young children and appears to increase over development. Savage et al. (2003) found lexically-based priming when there was pronoun overlap in transitive clauses. Branigan et al. (2005) found evidence of a lexical boost in a study investigating noun overlap in the adjectival/relative clause alternation (e.g., *the red cat* vs *the cat that's red*), whilst Foltz et al. (2015) did not. In studies using verb rather than noun overlap, Branigan and McLean (2016) found a lexical boost effect for the active–passive alternation in 3–4-year-olds, while both Rowland et al. (2012) and Peter et al. (2015) did not find one for the dative alternation in children of the same age. Differences in power are unlikely to explain the divergent findings, since those studies with the largest samples (Peter et al., 2015; Rowland et al., 2012) did not find the effect, but it is possible that the lexical boost develops on structure-specific schedules. In terms of the development of the effect, two studies from the same lab have found increases in the magnitude of the lexical boost effect for the dative alternation from younger children to older children to adults (Peter et al., 2015; Rowland et al., 2012). No study has investigated developmental changes in the lexical boost for passive sentences, and, as is the case with the abstract priming effect, no study has ever studied the lexical boost using a longitudinal design. Therefore, whether the lexical boost also increases over development for the passive, and precisely when it may emerge is unknown.

There is little research on individual differences in priming effects in children, despite Bencini and Valian's (2008) high dropout rate and Shimpi et al.'s (2007) task adjustments for 3-year-olds suggesting variability in children's ability to be primed. Kidd (2012) found that children's nonverbal ability predicted their tendency to be primed, and that vocabulary size and grammatical knowledge predicted the magnitude of the priming effect for those children who were primed. Messenger (2021) found a marginal correlation between the magnitude of children's priming effect and their vocabulary size. Children's passive production in the priming task also predicted their passive production in a post-test phase. The findings of both studies point to the possibility that differences in developmental levels, which are imperfectly related to age, lead to individual differences in priming, but further corroborating research is required.

The need for longitudinal data

Many high-quality studies of syntactic priming in children have been conducted; however, the conclusions we can draw from the current literature are limited by the cross-sectional design of those studies. Messenger et al. (in press) point out that the average age range for samples of children in studies comparing age groups is 20 months. Therefore, there is likely substantial variation in children's stage of language development, which could contribute to variation in priming effects by including both children who are more likely to be primed and less likely to be primed in the same comparison group. This problem is particularly likely considering studies typically use samples between 3 and 5 years old, the period in which, in languages like English (and other European languages), knowledge of the passive develops. Existing studies cannot track changes in children's priming effects over critical periods of change. The lack of clarity over the development of the abstract priming effect or the precise timing of the emergence of the lexical boost indicates the shortcomings of cross-sectional designs. Developmental change is best investigated using designs which tightly control age and compare children to themselves over a period of time in which development occurs, that is, longitudinal designs.

The present study

The past empirical evidence suggests an early-emerging abstract priming effect (i.e., unambiguously by 4-years), in support of the RA-Early Syntax and Implicit Learning accounts. However, how this priming effect changes across time and whether it varies systematically across individuals are open questions. Moreover, evidence regarding the

[3] Note that we discount an older nativist explanation of the late emergence of the passive – maturation (Borer & Wexler, 1987), which argued that children do not produce passives until around 6 years because of the late maturation of components of Universal Grammar. There is now sufficient evidence to discount this as a likely explanation of development across languages (for which it was intended) or, indeed, just in English.

lexical boost effect is suggestive of a late-emerging developmental effect, as predicted by the Implicit Learning account, although the data on this topic are sparse and inconsistent. Even fewer studies have investigated individual variability in priming effects but those that have done so suggest systematic differences that are related to children's prior knowledge, in line with the RA-Late Syntax and Implicit Learning accounts. In order to better distinguish between these approaches, longitudinal evidence – the gold standard for developmental science – is required at key periods of development. In the current study we investigate the longitudinal development of both the abstract priming effect and the lexical boost effect in a large sample of children acquiring English as a first language. We followed them across four testing sessions, starting from 36 months, when the abstract priming effect does not appear to have emerged for all individuals, to 54 months, when it is well established. We examine the English active–passive alternation. As outlined in Table 1, we expect the following predictions from each account.

1) If the RA-Early Syntax account holds, we expect early abstract priming and lexical boost effects that remain stable across the 18-month period. Moreover, if the most strongly nativist of these perspectives hold (e.g., Crain et al., 2017), we expect minimal systematic individual variability between participants in the emergence of the passive structure.
2) If the RA-Late syntax account holds, we expect priming to be initially restricted to trials with lexical overlap, resulting in an increasing abstract priming effect across time and a progression from lexically-based priming to a decreasing lexical boost as abstract priming increases. Such accounts further predict systematic individual variability in the emergence of the passive structure.
3) If the Implicit Learning account holds, we expect to see early abstract priming that decreases across time and a late emerging lexical boost that increases across time. This account further predicts systematic individual variability in the emergence of the passive structure.

Data availability

Our sentences, pictures and experimental lists, data and scripts are accessible on the Open Science Framework (https://osf.io/35kzm/).

Methods

Participants

The participants were taking part in the *Canberra Longitudinal Child Language* project (Kidd, Junge, et al., 2018), a longitudinal study of children's language processing and language acquisition between the ages of 9 months and 5 years. The project initially recruited 124 children, based on the following criteria: (i) full-term (at least 37 weeks gestation) babies born with a typical birth weight (>2.5 kg), (ii) a predominantly monolingual English language environment (in all but two cases, no >20% exposure to a language other than English), 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 socio-economic status of the families was measured via parental education, measured on a 7-point scale: 0 = some high school, 6 = PhD. Consistent with the demographics of the city (Canberra, Australia), SES was high, with a median education of 4 (Bachelor degree) for caregiver 1 (SD = 1.12, Range = 0: 6) and 4 for caregiver 2 (SD = 1.12, Range = 0: 6). Ethnicity was not recorded.

Here we report on children's priming of the active–passive alternation, which was measured longitudinally when the children were aged 36 (35;29–37;6 months, mean 36;18), 42 (42;12–43;5 months, mean 42;19), 48 (48;11–51;27 months, mean 48;20), and 54 months (54;10–55;5 months, mean 54;20). By the 36-month time point, 19 families had dropped out of the study, six of whom withdrew because their child had been diagnosed with a developmental disorder or delay. A further two children were excluded due to suspected developmental delay, which was corroborated by their language proficiency, as measured by the Macarthur-Bates Communicative Development Inventory (Fenson et al., 2007) at 30-months, being >2 standard deviations below the mean for the sample (3.77 and 2.85 standard deviations, respectively). Of the 103 children remaining, 73 completed the priming task at all four timepoints, with a sample of over 80 at each timepoint ($N_{36} = 94$ (52 females), $N_{42} = 92$ (50 females), $N_{48} = 91$ (50 females), $N_{54} = 82$ (46 females)). Additional data loss was due to missed sessions ($N_{36} = 7$, $N_{42} = 6$, $N_{48} = 5$, $N_{54} = 14$), further withdrawals from the study ($N_{42} = 5$, $N_{48} = 7$, $N_{54} = 7$), or inattention/inability to complete the task ($N_{36} = 2$).

Design

We employed a 4 (Age: 36-, 42-, 48- & 54-months) × 2 (prime: active vs passive) × 2 (verb match: match vs unmatched) within-participants longitudinal design.

Materials

Syntactic priming task

The priming task was conducted using the *SNAP* (picture matching) game paradigm, a procedure which has successfully been used with young children (Branigan et al., 2005; Branigan & McLean, 2016; Branigan & Messenger, 2016). Twenty-four two-participant actions that can be described by transitive verbs were created from 6 transitive verbs (*chase, hug, kiss, poke, pull, push*) and 12 animate characters (*bird, cat, chicken, cow, dog, duck, horse, monkey, mouse, rabbit, sheep, tiger*) and then drawn by a professional artist. All verbs are highly transitive action verbs that are known to children of this age. Each has appeared in past studies of the passive and all but *poke* have been shown empirically to be understood by children in the passive construction by 3–4 years of age (see Nguyen & Pearl, 2021). The pictures were arranged into prime-target pairs across five experimental lists, each containing 12 prime-target pairs. Children therefore experienced three trials for each experimental condition (e.g. verb matched passive primes). This low number of trials was necessitated by time and attention constraints for children participating in a longer session. Across lists, each verb was counterbalanced to occur equally as often as prime and target, as passive and active prime, and in matched and unmatched verb trials. Prime descriptions for each list were present tense active and full passive sentences.

In addition, 14 one-participant actions that can be described by intransitive verbs were chosen to make up the 22 filler items, which occurred as prime-target pairs between each experimental item. For six of the filler pairs, prime and target were the same picture. Fig. 1 illustrates an example sequence of the structure of each trial in the task. The task began with two practice prime-target pairs using *pull* and *feed* and one practice filler item to familiarise children with the procedure. One practice prime was active and the other passive.

The same materials were used at each timepoint. Children were randomly assigned to lists at each timepoint with approximately even numbers of children per list. Table 2 shows the distribution of trials presented to children across each condition and timepoint. Since each list was balanced across conditions, trials are balanced across conditions except for two experimenter errors.

MacArthur-bates communicative development inventory

Across early stages of the longitudinal study the children's primary caregiver completed the MacArthur-Bates Communicative Development Inventories (Donnelly & Kidd, 2021). At 30 months caregivers completed the Words and Sentences form (Fenson et al., 2007), which measures expressive vocabulary, grammatical complexity and mean

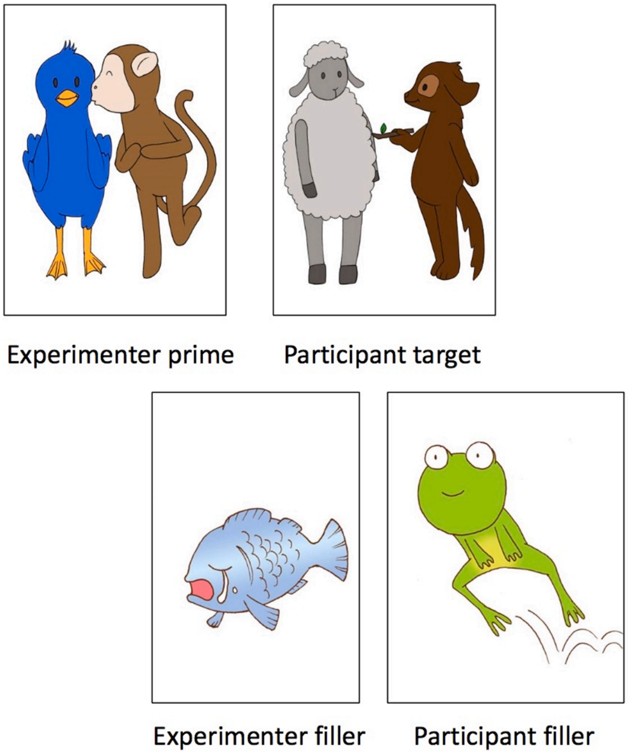

Fig. 1. Structure of a trial in the syntactic priming task: experimenter describes prime picture, participant describes their target picture, experimenter describes an intransitive filler, participant describes their intransitive filler.

Table 2
Number of trials presented per condition at each timepoint.

Timepoint	Active		Passive	
	Unmatched	Verb matched	Unmatched	Verb matched
36 m	283	281	282	282
42 m	276	277	276	275
48 m	273	273	273	273
54 m	246	246	246	246
Total	1078	1077	1077	1076

utterance length. In the form, caregivers are asked to indicate which words and phrases their child says, and to list their 3 longest sentences (from which MLU is calculated). Following Reilly et al. (2007), some minor changes were made to some of the words in the inventory and two were removed to better reflect the Australian dialect, but otherwise the instrument was used as per the standard instructions. The instrument has excellent psychometric properties (see Fenson et al., 2007).

Procedure

At each time point participants completed the priming task as part of an hour-long lab visit for the larger longitudinal study. The task was audio recorded using a Zoom H2n audio recorder. Children were seated at a child-sized table and chair set with the experimenter and were told they would be playing a game of *SNAP*, a popular card game for Australian children. The experimenter and child both had a pre-ordered stack of cards. The game started with two practice trials, in which the child was familiarised with the procedure of the game.

Each trial proceeded as follows: the experimenter turned over her card and described it according to a script using either an active or passive sentence for primes, or intransitive for fillers. Then the child turned over their card and was asked to describe it (‘*What’s happening on your card?*’). If the child did not use a complete sentence to describe the card, they were prompted to provide more detail (e.g. ‘*Tell me the whole thing*’). If children used a verb different to the one intended, they were provided feedback such as ‘*Yes they are running, maybe they’re chasing*’. This was to reduce the number of future trials containing a ‘verb error’. On six of the filler trials, the child’s and experimenter’s card were the same, in which case the first participant (i.e., either experimenter or child) to say ‘*snap*’ and place their hand on the cards won. Children were always allowed to win, which aimed to increase their motivation for the game. All sessions were audio and video recorded. The task lasted approximately 10 min.

Coding

Children’s responses were transcribed from the audio recording. Children’s first complete and intended response was scored. If children corrected themselves, their corrected response was coded. If children’s initial response was not a complete sentence (e.g. ‘*cuddles*’ or ‘*sheep and the duck*’) but they produced a full sentence after a prompt, this second sentence was scored. Sentences were coded as active, passive, or other. Sentences were coded as active if they contained an agent as the subject, a transitive verb and a patient as the object (e.g. ‘*the sheep is pulling the chicken*’).

We chose to use a *lax* as well as *strict* coding scheme in order to capture those responses where children produced passives that did not fully match the primed model. Bencini and Valian (2008) previously used lax and strict coding schemes to allow comparison with both more generously coded child priming studies and with adult priming studies. Under our strict coding scheme, a response was coded as passive if it contained a patient in the subject position, an auxiliary, a correctly inflected transitive verb and a *by*-phrase containing the agent. A passive under the lax coding scheme required correctly assigned thematic roles, a transitive verb, and either an auxiliary or a prepositional phrase containing the agent. Therefore, truncated passives (e.g. ‘*he’s being carried*’), omission of the auxiliary verb (e.g. ‘*monkey pushed by a cat*’), errors in inflection (e.g. ‘*a sheep is being chasing by a horse*’) and errors in the prepositional phrase (e.g. ‘*a cow was being cuddled from a chicken*’) were permitted (children also made similar mistakes in active sentences). Children sometimes produced sentences which could either be simply an error in inflecting the verb or the omission of an auxiliary and an inflection error (e.g. ‘*a rabbit is cuddling by a monkey*’ for monkey cuddles rabbit). These were coded as other in the strict coding but passive in the lax coding.

All other sentences, including incomplete sentences, intransitives, datives, and infinitives were coded as ‘Other’. There were some notable errors in this category that may indicate early attempts at passive production. Most commonly at earlier timepoints, children occasionally inserted “by” or “being” into otherwise active sentences (e.g. ‘*a tiger is carrying by a bird*’ for tiger carries bird, and ‘*a cat is being chase a dog*’ for cat chases dog). These could indicate priming of at least surface level features of the passive, in the same way that corpus studies of adult language have shown that priming need not involve phrasal heads (e.g., Snider, 2009; see Reitter et al., 2011). We also excluded both reversal errors where the agent was the subject (e.g. ‘*a dog is being kissed by a chicken*’ for dog kisses chicken), and duplication errors where children used the same noun for both agent and patient (e.g. ‘*a monkey is being kissed by a monkey*’). Interestingly, children made reversal and duplication errors in active, intransitive, dative and infinitive sentences too. Table 3 presents the number of each of these errors made by children at each timepoint.

As the study investigated the lexical boost, we also coded whether children used the verb intended by our materials or not. If the use of an incorrect verb changed the verb match condition, the trial was excluded. In verb matched trials the use of any other verb in the target sentence resulted in it no longer matching the prime verb. In contrast, children rarely produced the same verb as the prime verb in their target sentence in the unmatched verb condition.

Table 3
Number of notable errors in children's sentences at each timepoint.

	Insertion error		Reversal error			Duplication error		
	Active	Other	Active	Passive	Other	Active	Passive	Other
36 m	14		14	26 (1)	2	3	0	
42 m	9		9	40 (6)	5	8	9	1
48 m	6	1	3	19 (1)	1	2	8	
54 m	1		2	23 (3)	1	2	4	

Note: Numbers in brackets represent the number of passives in that error category which were truncated passives.

Table 4
Categories of children's responses at each timepoint.

Timepoint	Coding	Excluded		Other		Excluded verb error		Included verb error		Remaining trials	
		N	%	*N*	%	*N*	%	*N*	%	*N*	%
36 m	strict	24	2.1	250	22.2	90	8.0	143	12.7	621	55.1
	lax	24	2.1	225	20.0	94	8.3	146	12.9	639	56.7
42 m	strict	16	1.5	213	19.3	47	4.3	112	10.2	716	64.9
	lax	16	1.5	166	15.0	49	4.4	118	10.7	755	68.4
48 m	strict	5	0.5	130	11.9	42	3.9	108	9.9	807	73.9
	lax	5	0.5	101	9.3	43	3.9	110	10.1	833	76.3
54 m	strict	2	0.2	90	9.2	20	2.0	64	6.5	808	82.1
	lax	2	0.2	78	7.9	20	2.0	68	6.9	816	82.9

Note: Trials in both the 'Included verb error' and 'Remaining trials' columns were included in analyses.

Table 5
Number of included responses per participant.

Timepoint	Coding	Transitive responses			Included trials		
		mean	median	range	mean	median	range
36 m	strict	9.09	9	2 – 12	8.06	8	2 – 12
	lax	9.35	10	3 – 12	8.28	8	2 – 12
42 m	strict	9.51	10	5 – 12	8.96	9	4 – 12
	lax	10.02	11	5 – 12	9.45	10	4 – 12
48 m	strict	10.52	11	4 – 12	10.02	10	3 – 12
	lax	10.84	11	6 – 12	10.33	10	5 – 12
54 m	strict	10.88	11	6 – 12	10.62	11	6 – 12
	lax	11.02	11	6 – 12	10.77	11	6 – 12

Note: The *Transitive responses* category includes truncated passives for the lax coding scheme. Since verb errors which changed the verb-match condition were excluded, fewer trials were included in analyses than were transitive.

Results

Data loss

Trials were excluded if children did not produce a response ($N_{36} = 23$, $N_{42} = 12$, $N_{48} = 3$), their response was inaudible ($N_{36} = 1$, $N_{42} = 1$, $N_{48} = 2$), or there was an error in administering the trial ($N_{42} = 3$, $N_{54} = 2$). Trials coded *as Other* and trials where the child used a different verb to the one intended, thus changing the verb match condition, were also excluded. The distribution of trials across each time point is presented in Table 4 and the number of trials included for each participant is presented in Table 5. Whilst a few children produced mostly *Other* responses at each timepoint, the vast majority produced a high number of transitive responses with a median and mean of at least 9 transitive responses out of 12 trials per participant.

Fig. 2 presents the distribution of trials in Table 4 split by verb match condition and excluding administration errors and inaudible trials. In verb matched trials children made fewer verb errors than in unmatched trials and produced fewer *Other* and non-responses. Children's use of an unintended verb in verb matched trials always results in unmatched verbs, therefore changing the verb match condition and resulting in the trial being excluded. In unmatched trials, if children used an unintended verb, it was rarely the same as the prime verb and so trials rarely needed to be excluded. Therefore, at earlier timepoints, slightly more data were lost in matched than unmatched trials despite there being fewer *Other* responses in this condition. It is important to note that this data loss is non-random in a way for which our modelling does not account. The matched condition containing more transitive responses, or more successful passive productions vs incorrect attempts (captured under *Other* responses), could suggest a kind of lexical boost not captured simply by investigating the proportion of passive responses out of active and passive responses. This is important to note as we found stronger evidence for a lexical boost at the latter two timepoints, where the least data were lost. Fig. 2 graphs data loss under the strict coding scheme; the pattern of data loss is identical under the lax coding scheme with only slight numerical differences.

Abstract priming and lexical boost effects

Table 6 and Fig. 3 summarise the proportion of active and passive responses in each experimental condition. There appears to be a consistent abstract priming effect, with more passives produced following passive primes than active primes. In addition, there appears to be a lexical boost effect that increases over time: at 48 and 54 months, children exhibited larger priming effects for passive primes containing the same verb as the target picture.

We also plotted the priming effect by target verb to check for consistency in the priming effects by item. Since our data contains only 6 levels of item, mixed models including random slopes by item are unlikely to converge, or, under Bayesian statistics, the random effects may be estimated with particularly large credible intervals. This means our models may not adequately control for differences in the priming effects

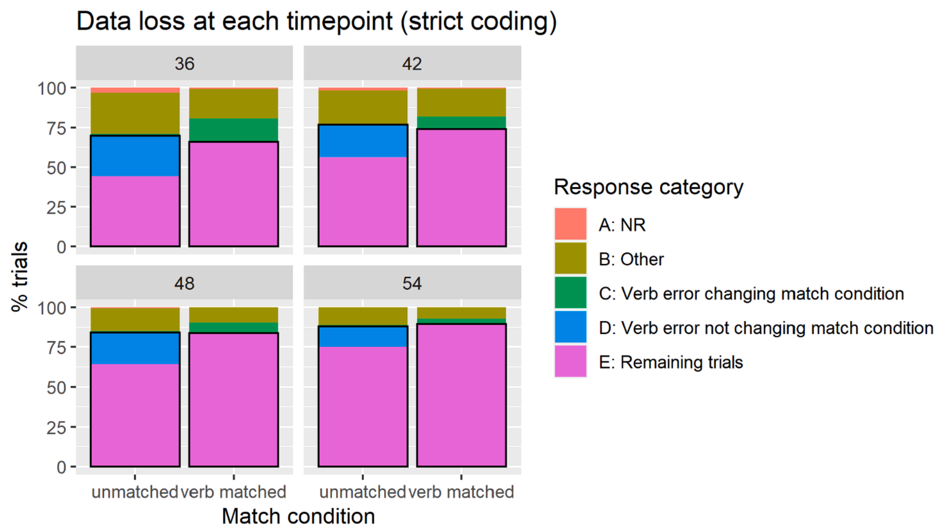

Fig. 2. Categories of children's responses at each timepoint. *Note:* Black outline indicates included trials; trials in both the 'Included verb error' and 'Remaining trials' categories were included in analyses.

Table 6
Number and proportion of passives and actives in each experimental condition.

Timepoint	Prime	Verb match	Active		Passive - strict		Passive - lax	
			N	%	N	%	N	%
36 m	Active	Unmatched	184	92.5 / 90.2	15	7.5	20	9.8
		Matched	197	95.2 / 95.2	10	4.8	10	4.8
	Passive	Unmatched	163	85.8 / 82.7	27	14.2	34	17.3
		Matched	124	76.5 / 72.9	38	23.5	46	27.1
42 m	Active	Unmatched	201	90.5 / 88.6	21	9.5	26	11.5
		Matched	211	94.2 / 92.1	13	5.8	18	7.9
	Passive	Unmatched	146	74.9 / 68.6	49	25.1	67	31.5
		Matched	110	60.1 / 55.0	73	39.9	90	45.0
48 m	Active	Unmatched	185	79.1 / 78.4	49	20.9	51	21.6
		Matched	220	91.3 / 90.2	21	8.7	24	9.8
	Passive	Unmatched	148	66.4 / 63.0	75	33.6	87	37.0
		Matched	82	38.3 / 36.4	132	61.7	143	63.6
54 m	Active	Unmatched	170	79.8 / 79.4	43	20.2	44	20.6
		Matched	210	92.1 / 91.7	18	7.9	19	8.3
	Passive	Unmatched	140	63.9 / 61.7	79	36.1	87	38.3
		Matched	72	34.1 / 33.8	139	65.9	141	66.2

Note: The two percentages presented for Active responses are for the strict and lax coding schemes respectively.

by item. This is common in acquisition studies, where the choice of items is limited to what children know and can be easily depicted, and is thus not unique to our study. Fig. 4 shows that the priming effects were not consistent across verbs at 36 months. Notably, *push* behaved very differently to the other verbs. When it was the target verb, children showed the opposite pattern of results in unmatched verb trials and a reversal of this pattern in matched verb trials. We see an *anti-priming effect* in the unmatched verb condition, but a typical priming effect in matched verb trials. At later timepoints, *push* continues to behave in a similar manner, with no priming effect in the unmatched verb condition, but a typical priming effect in matched verb trials.

To explain this idiosyncrasy, we first checked whether data loss was biased by verb, perhaps skewing results for *push* due to low trial numbers (Appendix A). However, *push* trials were the most numerous, suggesting low trial numbers did not contribute to the result. We then analysed a corpus of Australian child-directed English (Kidd & Bavin, 2007) and the larger Manchester corpus (i.e., a corpus of British English; Theakston et al., 2001) to check whether *push* is passivised more frequently than our other verbs, under the assumption that *push* may prefer a passive frame (see Appendix B for details). Whilst *push* was by far the most frequent verb children heard and made up nearly half of the passives identified, as a proportion of total utterances *push* was as likely as other verbs to occur in the passive. Although the reason for this item-based effect was unclear, in light of the consistent and large discrepancy in the pattern of results, we decided to analyse the data both including *push* trials, as originally intended, and excluding them. Table 7 and Fig. 5 summarise the proportion of active and passive responses in each experimental condition, excluding *push* trials. Graphically, the effects appear similar to the full set of trials, with a consistent abstract priming effect and the effect of matched verbs becoming larger over time.

Cross-sectional models

The data were first analysed cross-sectionally, to understand the pattern of results at each timepoint. For example, whilst a longitudinal analysis may reveal that an effect increases over time, it does not differentiate between the effect emerging at a particular timepoint and the effect being present at all timepoints but becoming larger in magnitude. For the former situation, cross-sectional analyses also allow us to pinpoint when the effect emerged. In addition, the presence of an abstract priming effect at 3 years of age (our earliest timepoint) is of particular relevance to distinguishing between theories.

The production of passives was analysed as the frequency of passives out of active and passive responses (i.e., *Other* responses are excluded from the analyses). Results were analysed using mixed logistic models, which are suited to analysing binary outcome data and allow random

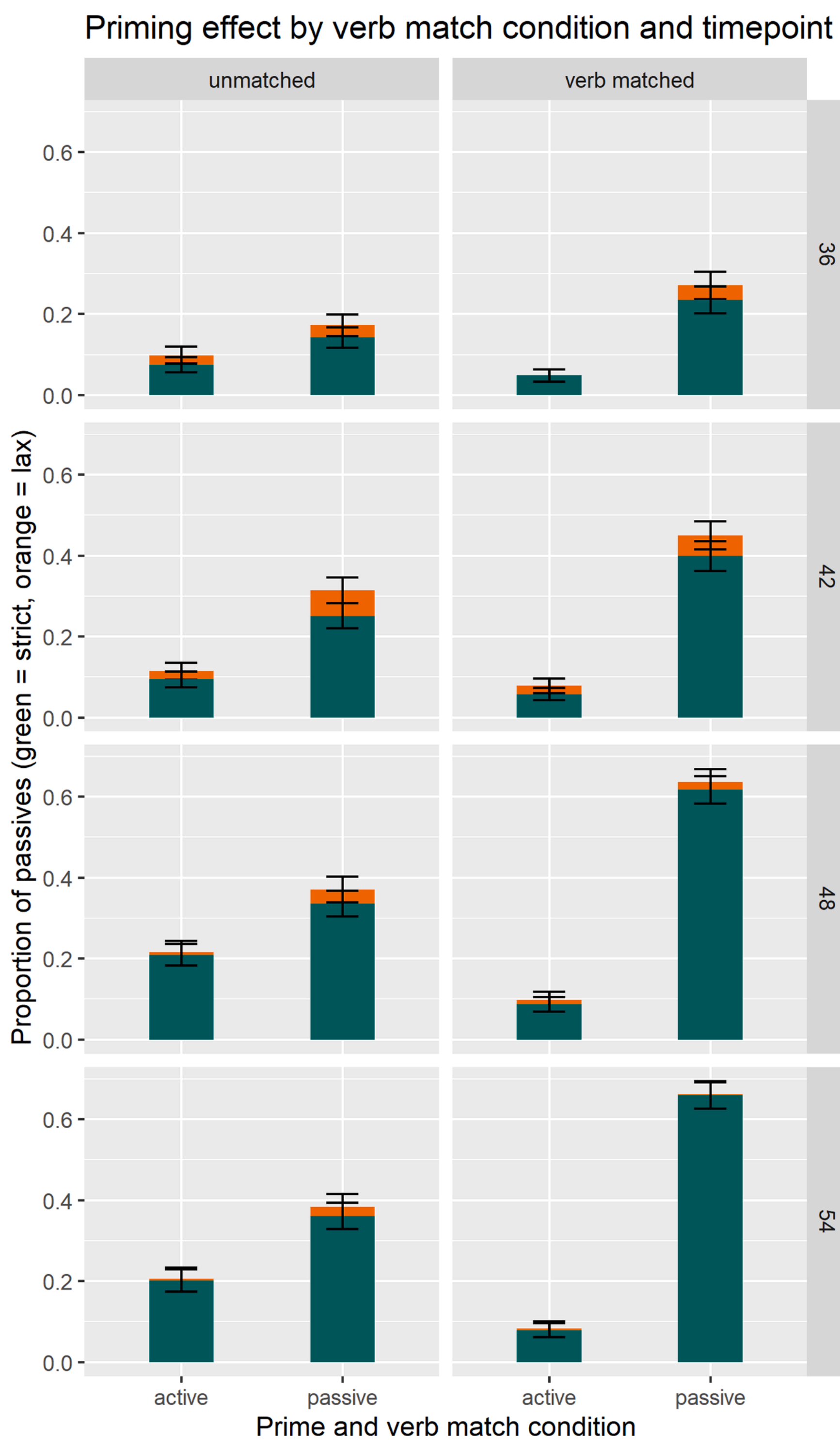

Fig. 3. Proportion of passive responses in each experimental condition at each timepoint. Abstract priming is indicated by a larger proportion of passives in the passive than prime condition for unmatched verb trials. The lexical boost effect is indicated by a larger difference between active and passive prime conditions for verb matched vs unmatched trials.

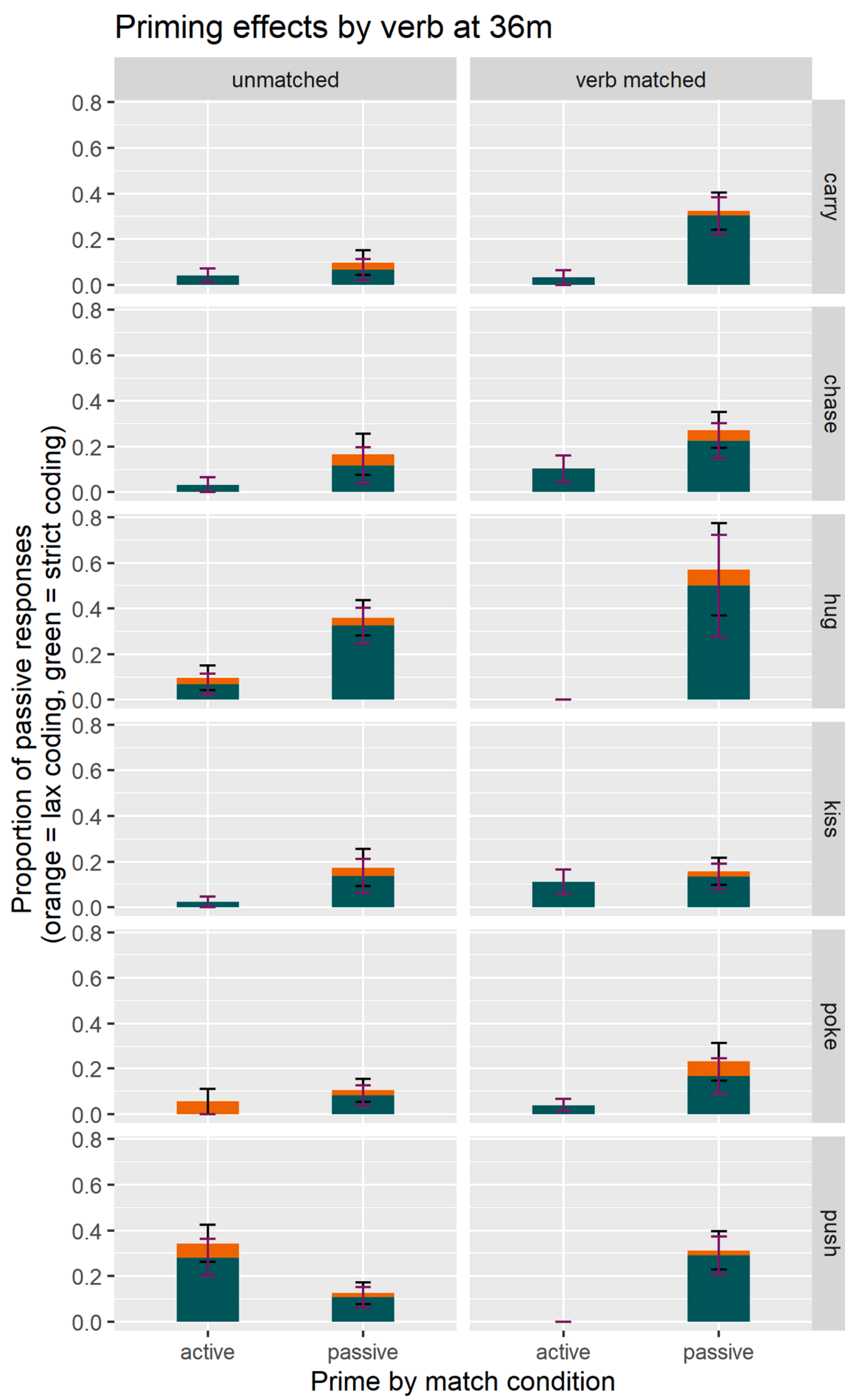

Fig. 4. Proportion of passive responses in each experimental condition for each verb at 36 months. All verbs except *push* appear to show abstract priming, a greater proportion of passives in the passive than active priming condition for unmatched verb trials.

Table 7
Number and proportion of passives and actives in each experimental condition excluding push trials.

Timepoint	Prime	Verb match	Active		Passive – strict		Passive – lax	
			N	%	N	%	N	%
36 m	Active	Unmatched	161	96.4/95.3	6	3.6	8	4.7
		Matched	152	93.8/93.8	10	6.2	10	6.2
	Passive	Unmatched	121	84.6/81.2	22	15.4	28	18.8
		Matched	102	77.9/73.9	29	22.1	36	26.1
42 m	Active	Unmatched	177	91.7/89.9	16	8.3	20	10.2
		Matched	161	94.2/92.5	10	5.8	13	7.5
	Passive	Unmatched	107	72.8/65.2	40	27.2	57	34.8
		Matched	96	60.0/55.2	64	40.0	78	44.8
48 m	Active	Unmatched	166	83.4/83.0	33	16.6	34	17.0
		Matched	174	91.6/90.2	16	8.4	19	9.9
	Passive	Unmatched	116	65.9/62.4	60	34.1	70	37.6
		Matched	72	40.2/37.9	107	59.8	118	62.1
54 m	Active	Unmatched	149	81.4/81.0	34	18.6	35	19.0
		Matched	165	91.7/91.2	15	8.3	16	8.8
	Passive	Unmatched	109	63.0/60.9	64	37.0	70	39.1
		Matched	64	36.0/35.6	114	64.1	116	64.4

Note: The two percentages presented for Active responses are for the strict and lax coding schemes respectively.

effects for subjects and items to be accounted for in the same model (Jaeger, 2008). We used Bayesian rather than frequentist estimation due to the complexity of our models and the lower likelihood of convergence issues with Bayesian statistics (Eager & Roy, 2017). The R statistical environment was used for data analysis (verion 3.6.1.; R Core Team, 2014). The tidyverse packages (version 1.3.0.; Wickham et al., 2019) were used for data processing and visualisation and the brms package (version 2.13.0.; Bürkner, 2017) was used for statistical modelling.

We included random effects by item (target verb) and by participant. We were able to include the maximal random effects structure with random slopes for prime, verb match, and their interaction as well as correlations between random effects. The prime variable was effects coded (active: −0.5, passive: 0.5), and the verb match variable base coded (unmatched: 0, matched: 1). This allowed for an intuitive interpretation of the results with the prime effect being a simple effect for unmatched verb trials (therefore the abstract priming effect) and the match effect being a main effect across active and passive prime trials.[4] The interaction effect represents the lexical boost effect. Each model was run with 3000 iterations, 500 of them warm-up, and 4 chains. The default brms priors were used (uninformative priors). Across all the cross-sectional models for each parameter the maximum Rhat was 1.01, the minimum bulk effective sample size was 1037, and the minimum tail effective sample size was 998. The value of adapt_delta, which decreases the step sizes taken by the model, was increased from the default 0.8 closer to 1 as required to prevent divergent transitions (minimum 0.95, maximum 0.98 across all models).

Note that Bayesian statistics handle hypothesis testing differently to frequentist statistics and can in fact be interpreted more intuitively. A 95% credible interval is provided for each effect, which indicates the range of values within which the effect has a 95% chance of falling given the data. For effects of interest we provide the posterior probability for a one-sided hypothesis. The posterior probability represents the proportion of the parameter's posterior distribution that is above or below 0. A posterior probability of .95 indicates a 95% chance that the effect falls above or below 0 given the data. Rather than a binary decision about the presence or absence of an effect, we use the approach taken by Engelmann et al. (2019):

- If the posterior probability is >.95, we interpret this as strong evidence for an effect given the data.
- If the posterior probability is >.85, we interpret this as weak evidence for an effect.
- If the posterior probability is close to.5 we conclude there is no evidence for an effect.

Table 8 reports the results of the four models run at the 36-month timepoint. Despite maximal random effects being included in the model, the influence of trials where the target verb was *push* is strong enough that we observe different results depending on their inclusion. When *push* is included, there is weak evidence for the simple main effect of prime (unmatched trials only), or the abstract priming effect, whilst there is strong evidence for the interaction between prime and verb match, or the lexical boost effect, under lax coding and weak evidence under strict coding. This suggests priming is driven by the verb-match condition. We see the opposite pattern of results when push is excluded, with strong evidence for abstract priming, but no evidence for the lexical boost effect.

Table 9 reports the results from the four models run at the 42-month timepoint. The abstract priming effect now has strong evidence regardless of whether *push* trials are included, though it is numerically larger when *push* trials are excluded. There is strong evidence for the lexical boost effect when *push* trials are included but weak evidence when *push* trials are excluded.

Table 10 reports the results at the 48-month timepoint. Like at 42 months, there is strong evidence for the abstract priming effect regardless of the inclusion of *push* trials, and the same pattern is now evident for the lexical boost. The priming effect is numerically larger, and lexical boost effect smaller when *push* trials are excluded.

Table 11 presents results from the models run at 54 months. Again, the lexical boost effect has strong evidence across all the models and the abstract priming effect has a consistent magnitude across the models but has weak rather than strong evidence in the strict coding model excluding *push* trials. When *push* trials were excluded, the numerically larger priming effect and smaller lexical boost effect were evident, but this pattern is far less pronounced than at the earlier time points.

Fig. 6 plots the coefficients and credible intervals of the abstract priming and lexical boost effects at each timepoint. There appears to be less certainty in the size of both effects at earlier timepoints, with larger credible intervals and greater divergence depending on the inclusion of *push* trials. Even accounting for this, there are some visible trends. In particular, when *push* trials are excluded, the size of the lexical boost effect increases over time. In contrast, the abstract priming effect appears to either slightly decrease or stay constant in magnitude over time.

[4] The intercept of an effect is determined by its coding. For both prime and match, the intercept is set at 0. However, 0 represents only the unmatched verb condition for the match effect, but the average of active and passive primes for the prime effect. Therefore, the effect of prime is a simple effect – at only one level of the other independent variable, whilst the effect of match is a main effect – averaging across both levels of the other independent variable.

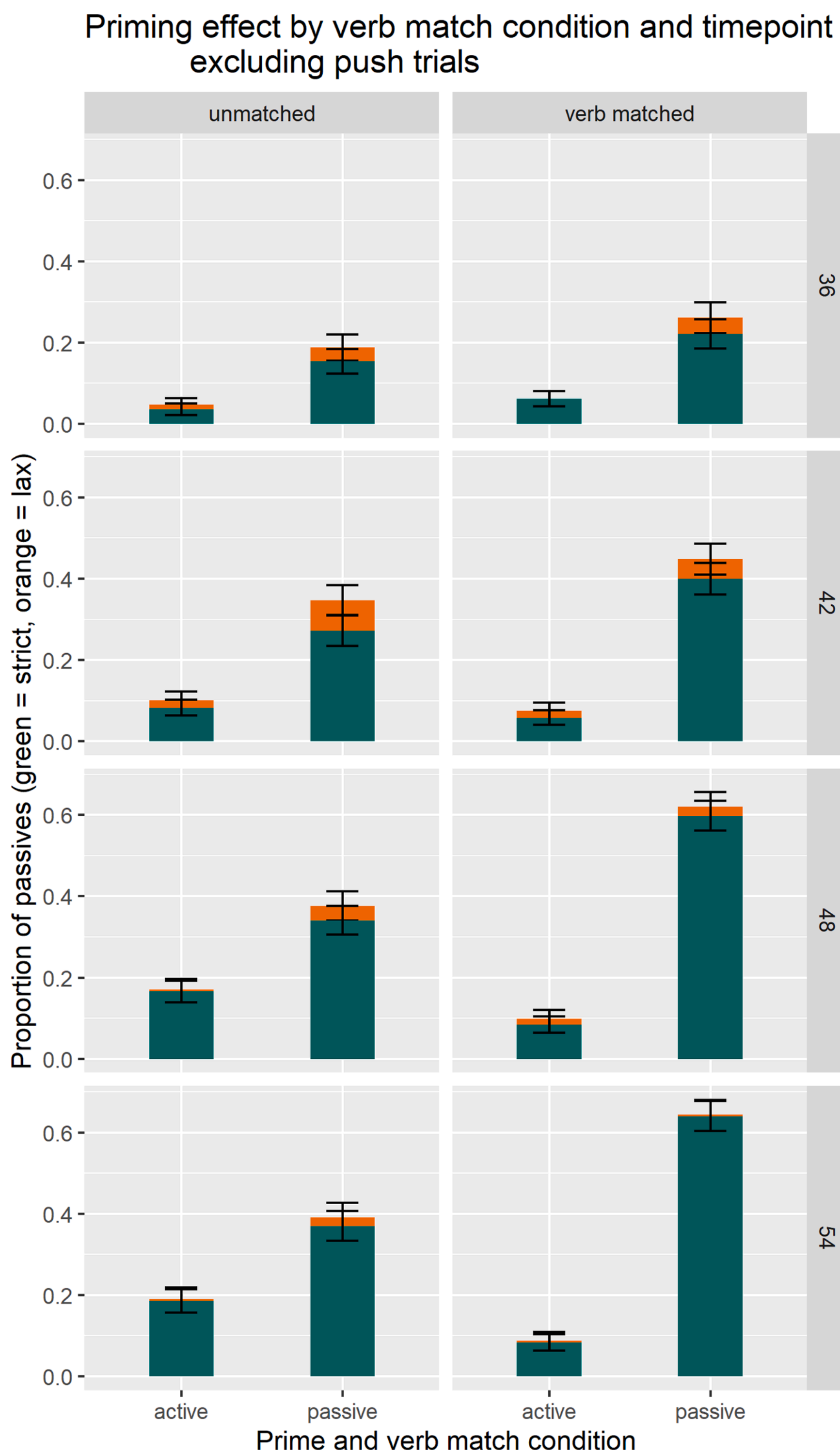

Fig. 5. Proportion of passive responses in each experimental condition at each timepoint, excluding *push* trials. Abstract priming is indicated by a larger proportion of passives in the passive than prime condition for unmatched verb trials. The lexical boost effect is indicated by a larger difference between active and passive prime conditions for verb matched vs unmatched trials.

Table 8
Results from models run at 36 months.

Effect	Strict	Lax	Strict – no push	Lax – no push
Intercept	−3.50	−2.87	−3.74	−3.18
	−4.88 | −2.38	−3.97 | −1.89	−5.37 | −2.41	−4.50 | −2.03
Prime	1.52	1.23	2.41	2.27
	−0.43 | 3.83	−0.50 | 3.12	0.49 | 4.96	0.56 | 4.40
	.942†	.932†	.990*	.993*
Match	−1.31	−1.27	−0.45	−0.55
	−4.05 | 0.74	−3.47 | 0.44	−3.01 | 1.48	−2.67 | 1.12
Prime*Match	2.22	2.88	0.79	1.44
	−1.23 | 5.95	−0.20 | 6.37	−2.31 | 4.04	−1.24 | 4.47
	.904†	.969*	0.698	.858†

Note: For all effects we report the coefficient of the effect and a non-directional, 95% credible interval around this estimate. For effects of interest we report the posterior probability that the effect is > 0 in a directional hypothesis test. * indicates strong evidence that the effect is > 0 in this test. †indicates weak evidence that the effect is > 0.

Table 9
Results from models run at 42 months.

Effect	Strict	Lax	Strict – no push	Lax – no push
Intercept	−2.73	−2.14	−2.83	−2.22
	−3.62 | −1.98	−2.92 | −1.47	−3.90 | −1.90	−3.18 | −1.34
Prime	1.74	1.83	2.19	2.33
	0.70 | 2.90	0.53 | 3.22	0.91 | 3.64	0.82 | 4.00
	.998*	.993*	.997*	.994*
Match	−0.21	−0.14	−0.15	−0.12
	−1.33 | 0.76	−1.25 | 0.84	−1.70 | 1.25	−1.66 | 1.35
Prime*Match	1.83	1.69	1.36	1.41
	−0.12 | 4.03	0.15 | 3.44	−1.07 | 4.04	−0.59 | 3.68
	.968*	.982*	.869†	.925†

Note: For all effects we report the coefficient of the effect and a non-directional, 95% credible interval around this estimate. For effects of interest we report the posterior probability that the effect is > 0 in a directional hypothesis test. * indicates strong evidence that the effect is > 0 in this test. †indicates weak evidence that the effect is > 0.

Table 10
Results from models run at 48 months.

Effect	Strict	Lax	Strict – no push	Lax – no push
Intercept	−1.45	−1.25	−1.58	−1.35
	−2.09 | −0.86	−1.85 | −0.70	−2.19 | −0.99	−1.94 | −0.81
Prime	0.85	0.99	1.18	1.38
	−0.04 | 1.79	0.02 | 2.01	0.22 | 2.15	0.47 | 2.31
	.971*	.977*	.986*	.993*
Match	0.24	0.25	0.27	0.30
	−0.45 | 0.90	−0.36 | 0.86	−0.58 | 1.09	−0.55 | 1.08
Prime*Match	2.90	2.66	2.58	2.29
	1.30 | 4.59	1.06 | 4.30	0.87 | 4.46	0.55 | 4.24
	.999*	.997*	.995*	.989*

Note: For all effects we report the coefficient of the effect and a non-directional, 95% credible interval around this estimate. For effects of interest we report the posterior probability that the effect is > 0 in a directional hypothesis test. * indicates strong evidence that the effect is > 0 in this test. †indicates weak evidence that the effect is > 0.

Table 11
Results from models run at 54 months.

Effect	Strict	Lax	Strict – no push	Lax – no push
Intercept	−1.45	−1.38	−1.47	−1.39
	−2.02 | −0.89	−1.97 | −0.83	−2.14 | −0.79	−2.05 | −0.75
Prime	0.96	1.10	1.09	1.20
	−0.14 | 2.13	0.03 | 2.18	−0.39 | 2.61	−0.18 | 2.62
	.961*	.977*	.941†	.962*
Match	0.44	0.33	0.52	0.41
	−0.33 | 1.12	−0.49 | 1.05	−0.42 | 1.43	−0.57 | 1.32
Prime*Match	3.66	3.64	3.44	3.42
	2.05 | 5.41	2.09 | 5.51	1.53 | 5.51	1.51 | 5.62
	>.999*	>.999*	.998*	.998*

Note: For all effects we report the coefficient of the effect and a non-directional, 95% credible interval around this estimate. For effects of interest we report the posterior probability that the effect is > 0 in a directional hypothesis test. * indicates strong evidence that the effect is > 0 in this test. †indicates weak evidence that the effect is > 0.

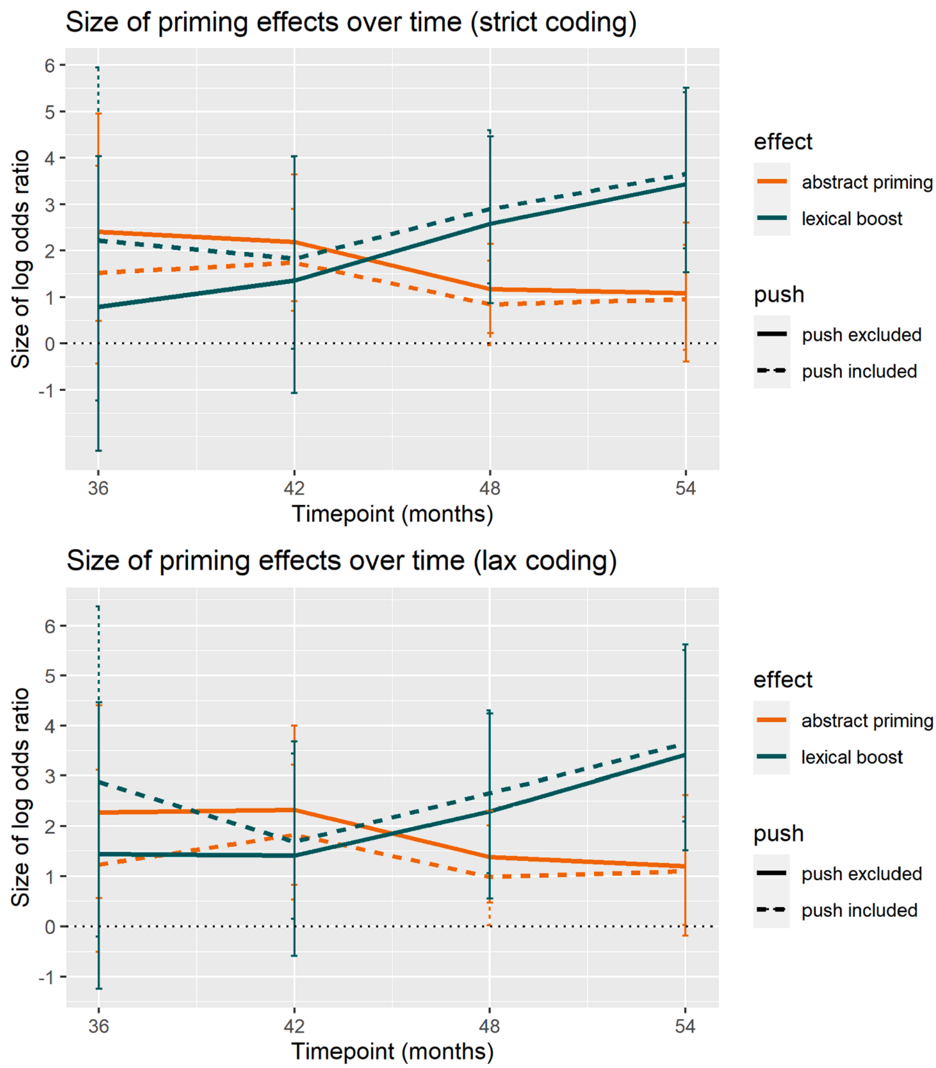

Fig. 6. Size of the abstract priming effect and lexical boost effect coefficients from cross-sectional models. Error bars span the 95% credible interval for the coefficient.

In order to investigate these trends over time, we combined data from all timepoints into our models.

Longitudinal models

In the longitudinal models, there were two sorts of dependence within participants: dependence within participants across the four timepoints and dependence within participant for trials within a particular timepoint. As such, we included maximal random effects by participants and by participants nested within session. Note that because timepoint varies within participants, but not within sessions, random effects for time, and all its interactions were included by participant but not by session. Whilst there were also two types of dependence by items, we chose not to include more random effects by item than there were levels of the item (5 for no push models, and 6 for all verb models), since reasonable estimates would not be reached. Therefore, the by-item random effects were as for the cross-sectional models, with random intercept by item (target verb) and random slopes for prime, verb match, and their interaction. We also included correlations between random effects.

Again, the prime variable was effects-coded (−0.5, 0.5), and the verb match variable base-coded (0, 1). Time was coded as 1, 2, 3, 4 for each timepoint and then centred. We can therefore interpret our main effects as before, at the mid-point of the timepoints. However, since prime is effects coded (−0.5, 0.5) whilst verb match is base coded (0, 1), we must interpret the main effect of time for both prime conditions but only unmatched verb trials. Each model was run with 5000 iterations, 500 of them warm-up, and 4 chains. The default brms priors were used (uninformative priors). Across all longitudinal models for each parameter the maximum Rhat was 1.00, the minimum bulk effective sample size was 1319, and the minimum tail effective sample size was 1471. The value of adapt_delta, which decreases the step sizes taken by the model, was increased from the default 0.8 closer to 1 as required to prevent divergent transitions (minimum 0.99, maximum 0.995 across models).

Table 12 presents the results of the longitudinal models. Across both coding schemes and when *push* trials are excluded we see the same patterns of results. There is strong evidence for abstract priming and lexical boost effects at the midpoint of our timepoints. This is consistent with our cross-sectional models, where abstract priming reliably received strong evidence across most models and evidence for the lexical boost effect was always strong at the latter two timepoints and strong when *push* trials were included in the first two timepoints. There is strong evidence for the effect of time across all models. Due to the coding of our variables, this can be interpreted as follows: in all unmatched verb trials, the production of passives increases over time. That is, children's overall passive production independent of prime condition increased over time. There was no support for a decrease in the abstract priming effect in models including all verbs but there was weak evidence for the

Table 12
Results from longitudinal models.

Effect	Strict	Lax	Strict – no push	Lax – no push
Intercept	−2.21	−1.87	−2.31	−1.97
	−2.77 | −1.65	−2.37 | −1.35	−2.90 | −1.70	−2.50 | −1.41
Prime	1.11	1.21	1.52	1.68
	0.22 | 2.06	0.20 | 2.19	0.73 | 2.32	0.92 | 2.46
	.989*	.986*	.998*	.999*
Match	0.16	0.08	0.28	0.21
	−0.40 | 0.71	−0.44 | 0.57	−0.42 | 1.01	−0.42 | 0.79
Time	0.69	0.53	0.75	0.60
	0.44 | 0.94	0.31 | 0.76	0.49 | 1.03	0.36 | 0.85
	>.999*	>.999*	>.999*	>.999*
Prime*Match	2.23	2.16	1.57	1.54
	0.96 | 3.45	0.96 | 3.38	0.54 | 2.62	0.59 | 2.53
	.998*	.998*	.996*	.997*
Prime*Time	−0.04	−0.00	−0.26	−0.26
	−0.39 | 0.32	−0.33 | 0.32	−0.67 | 0.15	−0.63 | 0.10
	.580	.505	.893†	.920†
Match*Time	0.19	0.20	0.10	0.11
	−0.09 | 0.48	−0.06 | 0.47	−0.22 | 0.43	−0.18 | 0.41
Prime*Match *Time	0.98	0.81	1.35	1.14
	0.41 | 1.57	0.29 | 1.35	0.70 | 2.03	0.55 | 1.75
	>.999*	>.999*	>.999*	>.999*

Note: For all effects we report the coefficient of the effect and a non-directional, 95% credible interval around this estimate. For effects of interest we report the posterior probability that the effect is > 0 or < 0 in a directional hypothesis test. * indicates strong evidence that the effect is > 0 or < 0 in this test. †indicates weak evidence that the effect is > 0 or < 0.

Table 13
Percentage of children who produced a passive and were primed at each timepoint.

		36 months		42 months		48 months		54 months	
		N	%	*N*	%	*N*	%	*N*	%
Strict	Produced Passive	41	43.6	54	58.7	76	83.5	71	86.6
	Primed	39	41.5	51	55.4	76	83.5	68	82.9
Lax	Produced Passive	50	53.2	64	69.6	79	86.8	71	86.6
	Primed	47	50.0	63	68.5	77	84.6	68	82.9

decrease in models without *push* trials. There was strong evidence for an increasing lexical boost effect.

Individual variability in priming

We next examined variability in priming and passive production. Only 40 of 92 children produced a passive at 36 months, in comparison to 71 of 82 children at 54 months. Table 13 presents the percentage of children who produced a passive and were primed (produced a passive after a passive prime) under each coding scheme at each timepoint. In the vast majority of cases, children who produced passives were also primed. Fig. 7 graphs the percentage of children, who completed all four timepoints and who were primed at each timepoint. Consistent with past studies (Bencini & Valian, 2008; Shimpi et al., 2007), it reveals substantial variability in priming in the youngest age group.

We investigated whether this variability was systematically linked to children's linguistic knowledge. The children's primary caregiver completed the Macarthur-Bates Communicative Development Inventory at 30 months, which included measures of vocabulary, grammatical complexity and mean length of utterance. These measures were intercorrelated, and so we ran a principal component analysis with promax rotation in SPSS to extract a single language proficiency component (see Appendix C for details). Table 14 presents the factor loadings of that component. It explains >70% of the variance of each measure individually and 65% of variance overall. Therefore, we used the extracted component as our measure of children's prior linguistic knowledge.

We first analysed whether children's component scores predicted whether or not they were primed at 36 months. We ran Bayesian logistic regressions to predict children's membership category. Table 15 presents the results of these models. There was strong evidence that children's language proficiency component score predicted their tendency to produce passives and be primed (produce a passive after a passive prime) under both coding schemes.

Table 14
Results of principal components analysis.

	Factor 1
Vocabulary	.817
Complexity	.849
MLU	.747
Eigenvalue	1.95
% variance	64.91

We then analysed whether the magnitude of the priming effect for those who were primed could be predicted by children's 30-month language proficiency. Table 16 presents the Spearman's correlations between children's language proficiency component score and their 36-month passive production and priming for only the children who produced a passive at 36 months. We observed correlations between the number of passives produced after a passive prime and the proficiency component. Under lax coding, there was a significant medium-sized correlation between priming magnitude (proportion of passives after passive primes – proportion of passives after active primes) and the proficiency component. These correlations suggest that children's language proficiency at 30 months is associated with the magnitude of their abstract priming effect.

We next re-ran the 36-month cross-sectional models on the subset of participants who produced a passive, with language proficiency and its interactions included as additional predictors (subsetting the children by whether they were primed made no difference to the pattern of results). Across these models for each parameter the maximum Rhat was 1.00,

Table 15
Results from models predicting membership category at 36 months.

	Produced passive		Primed	
	Strict	Lax	Strict	Lax
Intercept	−0.54	−0.02	−0.58	−0.14
	−1.05 | −0.04	−0.52 | 0.46	−1.10 | −0.09	−0.65 | 0.36
Proficiency	0.95	1.16	0.87	1.14
	0.39 | 1.58	0.59 | 1.80	0.32 | 1.47	0.56 | 1.80
	>.999*	>.999*	.999*	>.999*

Note: For all effects we report the coefficient of the effect and a non-directional, 95% credible interval around this estimate. For effects of interest we report the posterior probability that the effect is > 0 in a directional hypothesis test. * indicates strong evidence that the effect is > 0 in this test. †indicates weak evidence that the effect is > 0.

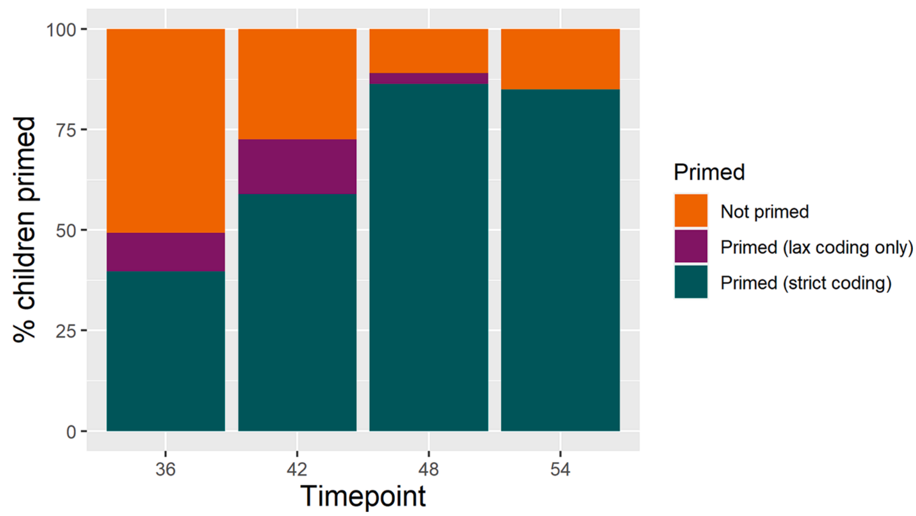

Fig. 7. Percentage of the 73 children who completed all timepoints who were primed at each timepoint.

Table 16
Spearman's correlations between proficiency and passive production and priming in subset of participants who produced passives or were primed.

		1	2	3	4	5	6	7
1. Proficiency		–						
2. *N* passives produced	strict	.33^	–					
3.	lax	.38*	.92***	–				
4. *N* passives primed	strict	.41*	.86***	.73***	–			
5.	lax	.44**	.83***	.89***	.83***	–		
6. Priming magnitude	strict	.25	.33*	.25	.68***	.51***	–	
7.	lax	.31*	.35*	.38**	.62***	.65***	.94***	–

^*p*-value between .05 and .1, *correlation is significant at the *p* <.05 level, **at the p <.01 level, ***at the.001 level.

the minimum bulk effective sample size was 1353, and the minimum tail effective sample size was 1625. The value of adapt_delta, which decreases the step sizes taken by the model, was increased from the default 0.8 to 0.97 to prevent divergent transitions. Table 17 presents the results of these models. The evidence for an abstract priming effect was strong in all models except for the lax coding model including *push* trials, where is it was weak. This is similar to the original models where there was only strong evidence for it when *push* was excluded. Interestingly, in this more linguistically advanced subset of participants, there is strong evidence for the lexical boost effect even with *push* trials excluded. Children's language proficiency does not interact with prime to predict the magnitude of priming, but there is weak evidence for it predicting the production of passives more generally in three of the four models.

Exploratory analyses

Contrary to the predictions of the Chang et al. (2006) model, in our longitudinal models, the abstract priming effect did not reduce in magnitude over time. However, in the analyses excluding the idiosyncratic *push* verb, there was weak evidence toward a negative effect. In addition, we found that the age at which children first produced a passive in the task was variable. It is possible that this variability in acquisition of the passive masked a decrease in priming.

In exploratory analyses, we repeated our longitudinal analyses on the subset of children who demonstrated knowledge of the passive at 36 months because they produced at least one passive during that testing session. Table 18 presents the results of these analyses. We used the same model specifications as for the longitudinal models. Across all models for each parameter the maximum Rhat was 1.00, the minimum bulk effective sample size was 1858, and the minimum tail effective sample size was 1361. The value of adapt_delta, which decreases the step sizes taken by the model, was increased from the default 0.8 closer to 1 as required to prevent divergent transitions (minimum 0.98, maximum 0.99 across models).

As in the full sample, the evidence for the abstract priming effect, the lexical boost effect and the increase in the lexical boost effect was

Table 17
Results from models predicting magnitude of priming at 36 months in children who produced a passive.

Effect	Strict	Lax	Strict – no push	Lax – no push
Intercept	−1.87	−1.74	−2.64	−2.53
	−3.13 | −0.83	−2.84 | −0.76	−4.59 | −1.14	−4.26 | −1.18
Prime	1.66	1.00	3.14	2.79
	−0.26 | 3.95	−0.71 | 3.00	0.60 | 6.77	0.68 | 5.91
	.958*	0.878†	.991*	.994*
Match	−1.28	−1.57	0.02	−0.60
	−4.71 | 1.29	−4.75 | 0.86	−3.11 | 2.69	−4.01 | 1.99
Proficiency	0.34	0.50	0.91	1.00
	−0.81 | 1.54	−0.43 | 1.46	−0.79 | 2.91	−0.38 | 2.56
	.742	.879†	.868†	.933†
Prime*Match	4.55	5.60	3.05	3.94
	0.21 | 10.67	1.45 | 11.71	−1.80 | 9.24	−0.59 | 10.02
	.981*	.997*	.884†	.956*
Prime*	0.12	0.48	−0.93	−0.70
Proficiency	−1.90 | 2.09	−1.13 | 2.06	−4.16 | 1.81	−3.21 | 1.42
	.551	.738	.738	.729
Match*	1.22	0.35	0.72	0.09
Proficiency	−1.21 | 4.20	−2.00 | 2.84	−2.06 | 3.88	−2.60 | 3.11
Prime*Match*Proficiency	0.48	−0.64	1.05	0.25
	−4.24 | 5.48	−4.99 | 3.65	−3.98 | 6.68	−4.36 | 5.03
	.578	.624	.659	.545

Note: *For all effects we report the coefficient of the effect and a non-directional, 95% credible interval around this estimate. For effects of interest we report the posterior probability that the effect is > 0 or < 0 in a directional hypothesis test. * indicates strong evidence that the effect is > 0 or < 0 in this test. †indicates weak evidence that the effect is > 0 or < 0.

Table 18
Results of longitudinal models on children who produced a passive during the first session.

Effect	Strict	Lax	Strict – no push	Lax – no push
Intercept	−1.39	−1.23	−1.63	−1.40
	−1.95 | −0.85	−1.69 | −0.79	−2.18 | −1.10	−1.88 | −0.93
Prime	1.28	1.24	1.71	1.68
	0.39 | 2.21	0.47 | 2.04	0.75 | 2.65	0.95 | 2.47
	.993*	.996*	.996*	.999*
Match	0.21	0.14	0.42	0.28
	−0.39 | 0.79	−0.30 | 0.57	−0.21 | 1.04	−0.25 | 0.79
Time	0.22	0.19	0.32	0.29
	−0.08 | 0.51	−0.06 | 0.45	−0.01 | 0.66	0.00 | 0.58
	.922†	.933†	.972*	.977*
Prime*Match	2.20	2.18	1.82	1.75
	1.15 | 3.23	1.26 | 3.08	0.57 | 3.14	0.80 | 2.80
	>.999*	>.999*	.994*	.998*
Prime*Time	−0.13	0.01	−0.48	−0.35
	−0.62 | 0.35	−0.40 | 0.41	−1.03 | 0.07	−0.79 | 0.08
	.699	.480	.958*	.941†
Match*Time	−0.01	0.09	−0.11	−0.03
	−0.35 | 0.34	−0.21 | 0.41	−0.51 | 0.30	−0.38 | 0.32
Prime*Match	0.64	0.51	1.14	0.92
*Time	−0.06 | 1.34	−0.09 | 1.11	0.34 | 1.98	0.23 | 1.63
	.965*	.953*	.997*	.996*

Note: For all effects we report the coefficient of the effect and a non-directional, 95% credible interval around this estimate. For effects of interest we report the posterior probability that the effect is > 0 or < 0 in a directional hypothesis test. * indicates strong evidence that the effect is > 0 or < 0 in this test. †indicates weak evidence that the effect is > 0 or < 0.

consistently strong. However, effects involving time were more evident once *push* is excluded from the analyses. In these analyses, there is strong rather than weak evidence for an increase in passive production over time, and strong evidence for a decreasing priming effect under strict coding, and weak evidence under lax coding. Fig. 8 presents the condition means of the raw data and those predicted by the strict, no *push* model. It shows that the decrease in abstract priming but increase in passive production arises because children increase their passive production only following active primes (slope estimate = 0.56, CI_{95} = 0.10 | 1.05), whilst passives following passive primes remain stable (slope estimate = 0.09, CI_{95} = -0.29 | 0.47).

Discussion

In this paper we have presented the first longitudinal study of syntactic priming in development. A key feature of our study is that we are, therefore, able to track both the emergence and the developmental trajectory of abstract priming and lexically-based priming/the lexical boost, which enabled us to test the predictions of three different theoretical approaches that make different predictions concerning these priming effects. These predictions and our findings are summarised in Table 19. We found an early emerging abstract priming effect in support of the RA-Early Syntax and Implicit Learning accounts but counter to the RA-Late Syntax account. This effect remained stable in magnitude across development when including all lexical items, which is only predicted by the RA-Early Syntax account. However, counter to at least some instantiations of the RA-Early Syntax account and in support of the two input-driven models, there was large variation in the onset of abstract priming, with <50% of 3-year-olds exhibiting priming under the strict coding scheme. This variation was systematic and meaningful, with the

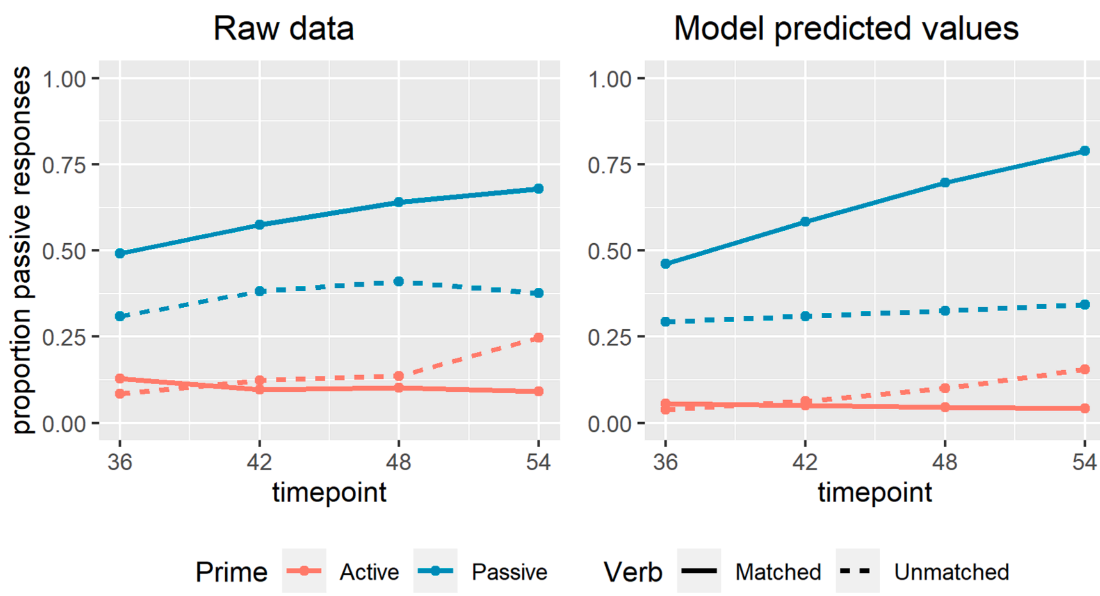

Fig. 8. Predicted and actual condition means for the strict, no *push* analysis of children who produced a passive at 36 months. *Note*: these are plotted on the probability scale (the scale of the raw data) rather than the logit scale, on which the model assumes linear relationships. Plots on the logit scale are available on https://osf.io/35kzm/.

Table 19
Predictions of structural priming accounts and study findings.

	Abstract Priming		Lexical Boost/Lexically-based	
	Emergence	Development	Emergence	Development
RA - Early Syntax	Early	No change	Early	No change
RA - Late Syntax	Late	Increase	Early	Inverse U-shape
Implicit learning	Early	Decrease	Late	Increase
Study findings	Early	No change/ decrease	Late	Increase

tendency to be primed predicted by children's language proficiency six months earlier, although the magnitude of priming was not (though the bivariate correlation between proficiency and priming magnitude was positive and significant). Moreover, when we excluded an atypical lexical item or considered only participants who exhibited a priming effect at 36 months, we found some evidence that the priming effect decreased over time. The lexical boost effect was idiosyncratic at earlier timepoints, with significance depending on a single lexical item. Excluding this item, the effect was late emerging and increased in magnitude over development, a pattern only explicitly predicted by the Implicit Learning account.

Interpretation of results

We first note that our pattern of results was largely consistent between models that included and excluded *push* items. On the occasions where they diverge, we took the models excluding *push* to be more representative of the true priming effects, since the presence or absence of an effect in those models is not driven by a single lexical item. For the lexical boost effect, the differences between the models appeared systematic in that models including *push* were developmentally ahead of time. That is, the lexical boost effect increased in both *push* and no *push* longitudinal models, but received strong evidence at an earlier timepoint in *push* rather than no *push* cross-sectional models. For the abstract priming effect, the anti-priming effect in *push* trials at 36 months masks the size of the effect for other verbs, in turn masking the decrease in abstract priming in the subset of children who produced a passive at 36 months. Differences between strict and lax coding models were fewer, usually with the size of an effect being comparable but with strong evidence in one model and weak evidence in the other.

Abstract priming effect

We found an abstract priming effect at all timepoints, and an increase in overall passive production over development. In analyses of the entire data set, although overall passive production increased, the difference between the number of passives following passive primes compared to active primes remained stable over development. However, when *push,* which displayed an anti-priming effect at the first timepoint, was excluded, we found weak evidence for a decreasing abstract priming effect alongside strong evidence for an increase in overall passive production. In a set of exploratory analyses, we further examined priming effects in a sub-group of participants who produced a passive at 36 months. Our motivation for these analyses was that variation in the emergence of priming amongst children in the full sample may have masked a true decrease in priming magnitude. Children in this sample exhibited a stable abstract priming effect in models with all verbs included but decreasing effect in models excluding *push,* which eliminated the masking effect of anti-priming in *push* trials at 36 months. The strength of the evidence for the decreasing effect was stronger than in the full sample. Children in this sub-sample also exhibited an overall increase in passive production over time, though with weaker evidence than in the full sample.

An increase in overall passive production suggests learning but its association with a decreasing priming effect is, at first glance, puzzling. Therefore, one might argue that these results reflect a test–retest effect or cumulative priming effects rather than implicit learning. Children's passive production in the task increased over and above baseline passive production in natural speech (0.1%; Xiao et al., 2006). Moreover, children increased in their proportion of passives produced after *active* primes from 5.6% of utterances at 36 months to 12.8% at 54 months (strict coding; lax coding: 10.6% to 15.04%).[5] There are several possible explanations for this increase in passive production. If the increase in

[5] In the full sample rather than the subset of children who produced a passive at 36 months, the proportion of passives produced after active primes increased from 4.5% to 12.4% under strict coding and 5.6% to 12.8% under lax coding from the 36- to 54-month timepoint.

passives reflects a test/re-test effect, children may have learned to produce more passives in the context of the SNAP task, without changes to their linguistic knowledge more generally. If the increase reflects an increase in cumulative priming effects across filler sentences (Branigan & McLean, 2016) children's tendency to be primed by earlier passive primes in subsequent trials would increase over development. Both of these scenarios would increase passive production after active primes as well as passive primes. If passive production after passive primes had reached ceiling, this could have led to a spurious observation of a decrease in priming. However, the data are not consistent with this interpretation because the probability of producing a passive after a passive prime is far below ceiling, and indeed lower than on verb overlap trials, at all time points (see Fig. 8). That is, it seems that what is changing is children's preference for producing passives *relative to actives*.

Such an effect suggests learning, and is consistent with the Chang et al. (2006) model. That is, as children's representation of the passive is tuned to occur more frequently, they produce more passives spontaneously after active primes and the prediction error caused by passive primes is smaller, resulting in an increase in passive production overall but a smaller effect of passive primes. Children hear few full BE passives outside of the lab, and their experience in the study across 18 months, where they hear a balanced number of actives and passives to describe transitive events, appeared to promote its use relative to the active across time, most prominently from the 48- to 54-month session. This is consistent with findings from training studies that increase children's exposure to passives. In an early study, Whitehurst, Ironside, and Goldfein (1974) modelled passive production to 4 – 5-year-old English-speaking children, which subsequently improved their production and comprehension relative to a control group. Similarly, Vasilyeva et al. (2006) increased passive sentences in 4-year-old English-speaking children's input via a two-week-long book reading intervention, which increased their production and comprehension of the passive relative to a group that heard active sentences. The difference between the current study and these past intervention studies is that we primed the active and passive within-participants, whereas the intervention studies did so between-participants. Thus, our data build upon the results of the intervention studies by showing that the relative weighting of structural options in the active–passive alternation is a property of the individual child.

Converging evidence for this interpretation comes from acquisition studies in languages that differentially weight the use of active and passive voice. Acquisition studies of languages such as Inuktitut (Allen & Crago, 1996), Ki'che' Mayan (Pye & Quixtan Poz, 1988), and Sesotho (Demuth, 1989; Kline & Demuth, 2010), where passives are structurally similar but are relatively more frequent than in European languages, show that children acquire the structure earlier and use it more frequently in their spontaneous speech.

We therefore conclude that, under conditions that take into account children's knowledge of the target structure at time 1, our results are consistent with the presence of a decrease in abstract priming over development. This is consistent with Rowland et al.'s (2012) study of the dative alternation. Additionally, Messenger (2021) found a very similar marginally significant effect in her passive priming study comparing children to adults: adults had a higher baseline rate of passive production than 3 – 4-year-olds, and their priming effect was marginally smaller than that of the children (some of whom were likely not yet primed, given our findings). Our finding is inconsistent with Peter et al. (2015), who found an increasing abstract priming effect, and those studies that have reported no developmental differences (e.g., Hsu, 2019; Messenger, Branigan, & McLean, 2012; Messenger, Branigan, McLean, et al., 2012). However, as we detail below, it is difficult to draw equivalence between longitudinal and cross-sectional designs.

The fact that there was less evidence for a decreasing abstract priming effect in the full sample than those primed at 3 years points to a lower bound on priming, such that there must be sufficient knowledge of the relevant structure in place prior to priming. This is supported by our finding that there is substantial variation in the emergence of priming, which is meaningfully linked to children's linguistic knowledge 6 months prior, and by Kidd's (2012) similar findings in older children. Thus, children need to have acquired the structure to a sufficient degree before priming can be observed. At that point priming is relatively large in magnitude but decreases across developmental time. The conflicting results from past studies may thus be attributable to variation in children's knowledge of the target structure, which is only partially related to age. We suspect the effect is small, requiring higher power than is often achieved in developmental studies, and is less easily observed in cross-sectional designs where children may be pooled with those at different developmental levels. This highlights the importance of longitudinal data, which compares children to their own past performance.

Lexically-based priming and the lexical boost effect

Turning to priming effects on trials with lexical overlap, we did not observe lexically-based priming in the absence of an abstract priming effect, suggesting that that there was no lexically-based priming prior to abstract priming, at least for the active/passive alternation. Moreover, we found strong evidence for a 3-way interaction between prime, time and verb match, suggesting that the lexical boost effect increases over development. In the more advanced subset of children who produced a passive at 3 years of age, the lexical boost effect was strong in three of the four cross-sectional models at 36 months, whilst for the full sample it was only strong under lax coding with *push* included. This finding suggests that the lexical boost effect emerges between 3 and 4 years of age, with its idiosyncratic nature the likely reason for past inconsistent results (Branigan & McLean, 2016 vs Rowland et al., 2012; Peter et al., 2015). Our study also confirms the dissociation between abstract priming and the lexical boost effect, with the former emerging earlier on in development, and with the two effects having different developmental trajectories. This result provides crucial developmental evidence in support of the suggestion that abstract priming and the lexical boost derive from separate mechanisms (Chang et al., 2006; Hartsuiker et al., 2008; Reitter et al., 2011).

One notable yet unexpected result concerning the lexical boost was the behaviour of one verb, *push*, which showed a lexical boost effect far earlier than the other verbs. The initially verb-dependent nature of the lexical boost could also explain inconsistent findings regarding its existence in young children. However, the reason for the result is unclear: all target verbs have an early age of acquisition, and they are all action verbs and so did not have semantic differences that may have influenced passivisation (Nguyen & Pearl, 2021). We checked data loss by verb and found that *push* trials were in fact the least likely to be excluded (see Appendix A), suggesting biased data loss was not the cause. A corpus analysis revealed that *push* was more frequent than the other verbs in child-directed speech, although it was not more likely to be passivized (Appendix B). Accordingly, children may be more familiar with *push* in the passive construction simply because it is a more frequent verb.

While infrequent structures are more syntactically prime-able than frequent ones (Bock, 1986; Jaeger & Snider, 2013), it may be that more frequent lexical items are more prime-able via lexical mechanisms. Specifically, frequent verbs may produce more enduring explicit memory traces linked to structure. How this occurs is still unclear. One possibility is that the verb-specific effect relates to how lexical entrenchment (i.e., verb frequency) establishes event representations of different strengths. If *push* is more frequent, then its event structure is likely more accessible (Elman, 2009). The early lexical boost effect for *push* suggests that the event construal is more flexible for this verb; that is, children can more flexibly alternate between the agent and the patient as starting points (MacWhinney, 1977), such that hearing a prime containing *push* in the active or the passive increases the likelihood that a target event containing *push* will be construed from the perspective of the topicalized NP. Sentence construal is prime-able in adults: using eye-tracking in a visual world paradigm, Sauppe and Flecken (2021) showed an active or passive prime significantly affected whether adult Dutch-speaking participants fixated on an agent or patient in a briefly-presented transitive event following the prime. Our suggestion is that in children such an effect may vary across individual verbs, and thus influence the likelihood that the lexical boost will be observed early in development. Investigating these processes using online methodologies like eye-tracking appears to be a promising avenue of future research.

Some further features of our data and past results support our claim of a nexus between event construal and priming effects. We found biased data loss such that transitive sentences were more likely in the verb overlap condition. We are not the first to report similar effects: Gámez and Vasilyeva (2015) analysed the likelihood that 5–6-year-old L2 English learners would produce complete sentences and found a significant effect of prime type (passive primes), and prime repetition. In addition, Shimpi et al. (2007) found in their second experiment that 3-year-olds were not primed but did produce more transitives following transitive primes. Studies of priming in languages that contain multiple structures allowing the speaker to emphasise the patient have shown that these structures prime each other (e.g., Spanish: Gámez et al., 2009; Russian: Vasilyeva & Waterfall, 2012), suggesting that the prime sentence primes children to construe an event from the perspective of the patient in general, from which point children select an appropriate syntactic structure. There was even some evidence in our English-speaking data of these processes. Specifically, we had difficulty categorising some errors where children appeared to be attempting to produce structures which topicalised the patient but had not fully acquired the appropriate passive structure to do so. For example, after hearing a passive prime, one 36-month-old child described *dog kisses chicken* with "A chicken by a dog… making the chicken be happy… maybe the dog is making the chicken be happy." Collectively, these findings show how prime sentences influence a broader spectrum of behaviour concerning scene perception and sentence construal.

Implications for accounts of priming and syntax acquisition

RA-late syntax account

Our results are least compatible with the RA-Late Syntax account (e.g., Savage et al., 2003; Tomasello, 2003). The account predicts the early emergence of lexically-based priming followed by a later emergence of the abstract priming effect, when in fact we observed the opposite pattern of results. There are two points worth considering in relation to the approach, despite its poor prediction of priming effects. Firstly, the theory is mostly concerned with explaining children's very early grammatical knowledge (e.g., Pine et al., 1998; Rowland, 2007; Tomasello, 1992, 2003), although its assumptions and mechanisms extend beyond the first sprouting of syntactic knowledge and have framed significant debates and theoretical development in the literature (e.g., Ambridge, 2019; Ambridge & Lieven, 2011; Fisher, 2002; McCauley & Christiansen, 2019; Özge et al., 2019; Thothathiri & Snedeker, 2008; Tomasello, 2000). While there is no doubt much idiosyncrasy in syntactic knowledge, such that constructional knowledge has different levels of abstraction (Goldberg, 1995, 2005), it appears that children move rapidly away from item-based syntactic knowledge of core argument relations quite early in development (Bannard et al., 2009). The challenge for the RA-Late Syntax approach is to develop a sufficiently detailed yet constrained account of syntactic development that distinguishes between item-based and abstract knowledge across different levels of developmental experience.

Secondly, we note that there are some features of our data that are broadly consistent with the RA-Late Syntax approach. The first is that there were meaningful individual differences in the emergence of children's knowledge of the passive, which was predicted by their linguistic proficiency 6 months prior. The second is that children's general production of the passive was more frequent across development, suggesting that children's overall knowledge of the passive strengthened across time (consistent with past research, see Marchman et al., 1991). As we will see, however, these are not unique features of the RA-Late Syntax account.

RA-Early Syntax account

The RA-Early Syntax approach predicts a large degree of continuity between adult and child priming effects (Bencini & Valian, 2008; Valian, 2014), such that there should be early emerging abstract priming and lexical boost effects, which remain stable across development. While we did find an early emerging abstract priming effect that appeared developmentally stable across the whole cohort, the individual variability in which children had acquired productive competence with the passive masked a negative developmental effect, whereby the abstract priming effect decreased with development. This effect constrains the RA-Early Syntax approach. In particular, contra to one version of the approach that explicitly assumes full continuity between the child and adult state (e.g., Crain et al., 2017; Crain & Thornton, 1998), our data strongly suggest developmental change in the system for both the abstract priming and lexical boost effects, which varies systematically across individuals. Thus, there exist observable and measurable learning effects for language-specific structures beyond 3 years, which presumably must be attributable to both children's variable input and variability in endogenous learning mechanisms (Kidd, Donnelly, et al., 2018; Kidd & Arciuli, 2016). Amongst RA-Early Syntax approaches, these data are more consistent with accounts that assume children necessarily acquire language-specific knowledge via the input, building upon less specified innate content (e.g., Fisher et al., 2020; Messenger & Fisher, 2018). We note that these accounts currently lack detail regarding how syntactic knowledge may change or be updated in response to further experience once abstract categories emerge. The models would need to be updated to reflect the specific developmental changes we observed in our data.

Implicit learning account

The Implicit Learning approach of Chang et al. (2006) provides the best fit to the data, accounting for both the early emergence of abstract priming and the late emergence and increase over development of the lexical boost effect. In addition, it explicitly predicts the decrease in the abstract priming effect found in a subset of children who produced a passive at 36 months. One additional advantage is that the account is computationally implemented, and thus provides an explicit account of the system's initial conditions and its learning mechanisms. There is a general consensus amongst computational models of priming that the abstract priming effect involves a form of implicit learning (Chang et al., 2000, 2006; Reitter et al., 2011); however, the Chang model, with which our data are most consistent, explains abstract priming via error-based learning, and correctly predicts the decreasing developmental effect (see also Dell & Chang, 2014). Additionally, the Chang model correctly predicts the asymmetry in the emergence and development of the lexical boost relative to abstract priming, although the model itself does not have a mechanistic account of the lexical boost. The Chang et al. (2006) model has a further advantage in being a model of language acquisition and therefore explains empirical phenomena beyond syntactic priming.

Conclusions

In this paper, we reported on the first longitudinal study of syntactic priming in development, tracking the priming of the active–passive alternation in a large sample of English-speaking children between the ages of 3;0 and 4;6. The longitudinal design allowed us to distinguish between several accounts of the acquisition of syntactic knowledge and, importantly, whether and how that knowledge changes across time. Our use of the syntactic priming method enabled us to make explicit connections between models of acquisition and mechanistic models of adult sentence production aimed at explaining priming effects (among other effects). Overall, we found evidence for the early emergence of abstract knowledge of the passive, which both varied across individual children and changed across developmental time. We also found evidence for asynchrony in the emergence and development of the lexical boost, supporting the suggestion that abstract priming and the lexical boost emerge via different mechanisms. These data are best accommodated within Chang et al.'s (2006) connectionist model, where knowledge of structure emerges via error-based learning relatively early in development, but continues to change with language use across developmental time.

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CRediT authorship contribution statement

Shanthi Kumarage: Data curation, Formal analysis, Visualization, Writing – original draft, Validation. **Seamus Donnelly:** Methodology, Formal analysis, Writing – review & editing. **Evan Kidd:** Conceptualization, Methodology, Resources, Writing – review & editing, Supervision, Project administration, Funding acquisition.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A

Data loss by verb

Figs. A1-A4 graph data loss at each timepoint by verb. They show that there was bias in trial numbers by verb. Especially at earlier timepoints, children often produced intransitive sentences using *run* for *chase* actions, or produced transitive sentences with *cuddle* for *hug* actions and *hold* for carry actions. However, *push* trials were least often excluded from analysis. This, and the consistency of the differing pattern of results for *push* across timepoints, suggests that this item-specific effect is reliable.

Appendix B

Corpus analysis

We ran a corpus analysis to check the frequency of each of our verbs in child directed speech, and whether *push* occurs more frequently as a passive. We searched for utterances containing the verb lemmas, and excluded any including noun or adjectival uses. We coded the utterances as active, passive, or other. Since natural speech is less systematic than that elicited in experimental contexts, we coded transitives more generously to include utterances with transitive thematic role order if not strictly transitive syntax. Therefore questions, imperatives with subjects, infinitival structures, existential structures, sentences with modal verbs, subordinate clauses, verb arguments, relative clauses, participle phrases, and subjectless structures where the subject was clear from the context were all included in the active category if they had a SVO thematic role order. Passives included truncated passives and some subjectless structures where the subject was clear from context. The *Other* category included structures without a clear SVO order including imperatives, intransitives, gerunds, some participle phrases, relative clauses and questions with non-transitive thematic role orders, structures without a subject where that subject was not clear from the context, and phrasal verbs.

We extracted utterances containing our verbs from an Australian corpus of child directed speech (Kidd & Bavin, 2007). However, we found too few instances of our verbs, and in fact no passive instances, to perform an analysis of their frequency as passives (see Table B1). Notably, *push*, which behaved differently in the priming experiment, is by far the most frequent verb children heard but mostly as a non-transitive imperative.

We ran the same analysis on the Manchester corpus from CHILDES (Theakston et al., 2001). The verbs were used similarly enough by parents to be comparable to Australian English and suitable for our purposes. Table B2 reports the outcome of this analysis and Table B3 details the 12 passive utterances found in our corpus analysis. Again, *push* was by far the most frequent verb. *Hug* was notably infrequent but occurred frequently as a noun in dative constructions which were excluded from our analyses. *Kiss* also occurred often as a noun in dative constructions but was additionally frequent as a verb.

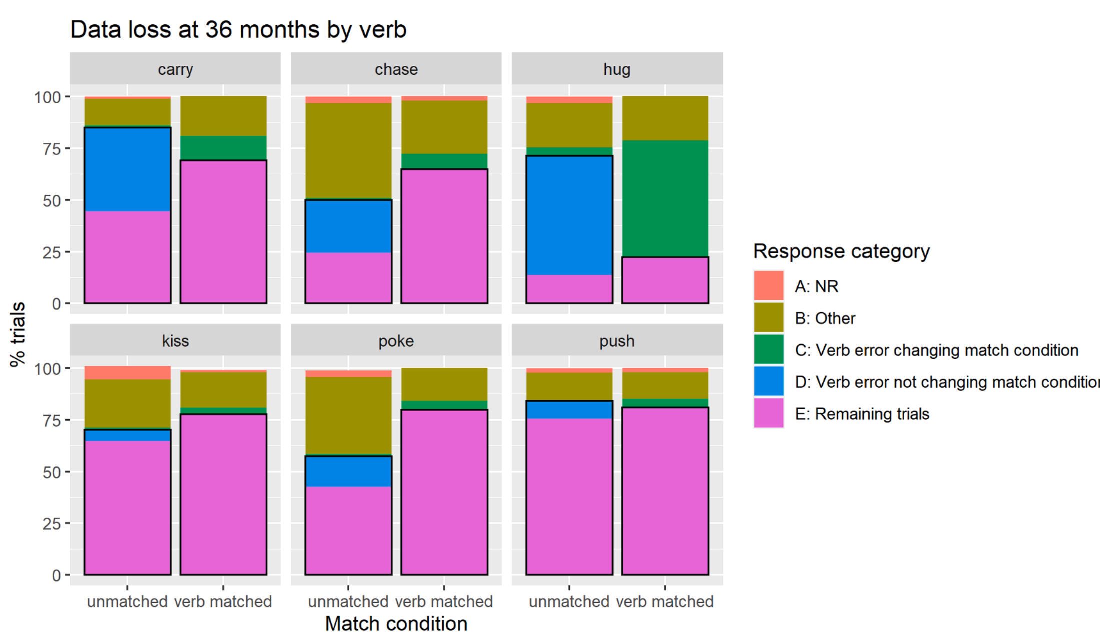

Fig. A1. Data loss by verb at the 36-month timepoint.

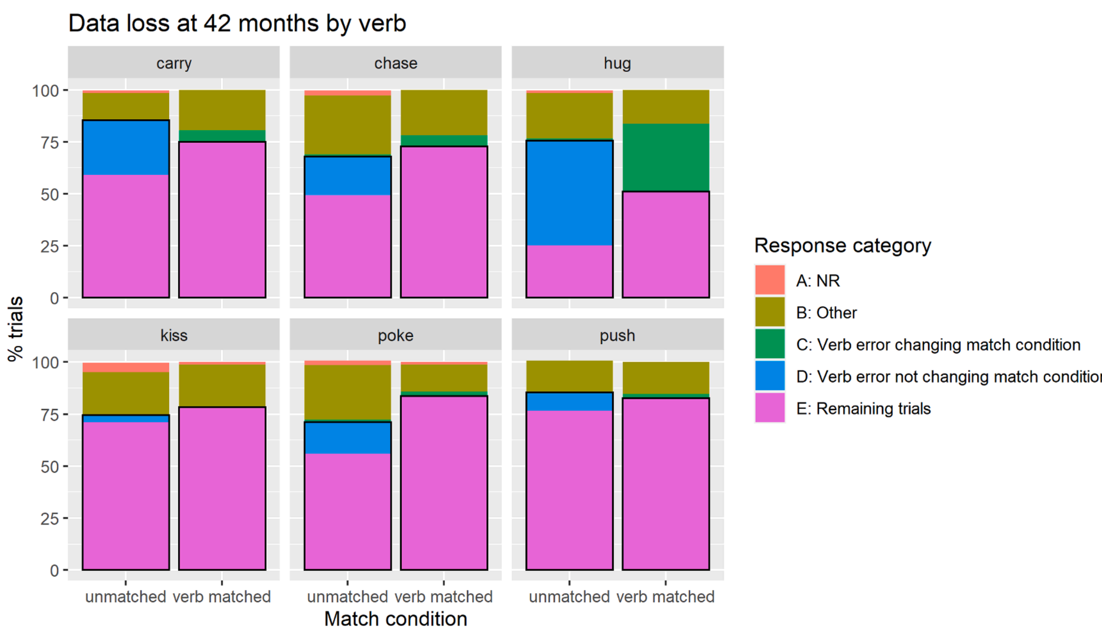

Fig. A2. Data loss by verb at the 42-month timepoint.

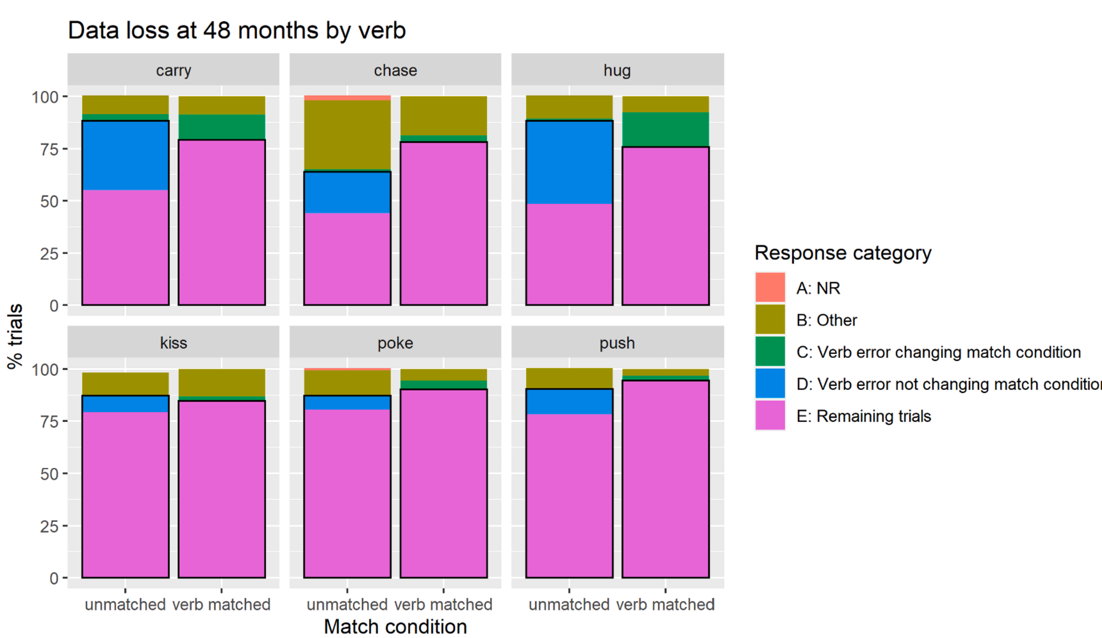

Fig. A3. Data loss by verb at the 48-month timepoint.

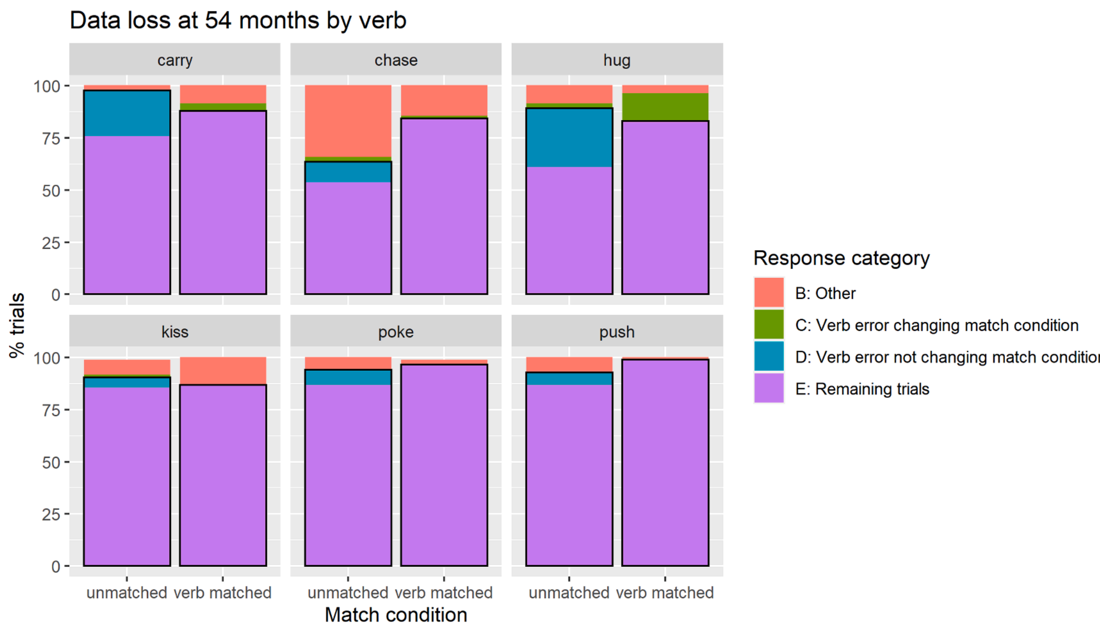

Fig. A4. Data loss by verb at the 54-month timepoint.

Table B1
Frequency of experimental items in Australian corpus of child directed speech.

Verb	*carry*	*chase*	*hug*	*kiss*	*poke*	*push*
Active	7	4	0	3	0	7
Passive	0	0	0	0	0	0
Other	1	5	0	0	1	26
Total	8	9	0	3	1	33

Table B2
Frequency of experimental items in Manchester corpus of child directed speech.

Verb	*carry*		*chase*		*hug*		*kiss*		*poke*		*Push*	
	N	%	*N*	%	*N*	%	*N*	%	*N*	%	*N*	%
Active	114	59.1	57	87.7	9	90.0	129	72.9	19	47.5	282	46.6
Passive	1	0.5	1	1.5	0	0.0	5	2.8	0	0.0	5	0.8
Other	78	40.4	7	10.8	1	10.0	43	24.3	21	52.5	318	52.6
Total	193		65		10		177		40		605	

Table B3
Passive utterances.

Verb	Utterance
carry	there's a baby being *carried* in a very special way.
chase	being *chased* again.*
kiss	**being *kissed* by a thing.***
	being *kissed* by all these creatures.*
	I'm not sure you deserve to be *kissed* better because you were being silly.
	doesn't the polar bear like being *kissed*?
	oh he's been *kissed* better by the vet, has he?
push	the poor cow'd be *pushed* out of the way, wouldn't it?
	I willn't be *pushed* down this time.
	I think if that's not *pushed* in you can't hear it.
	or was it *pushed*?
	yeah well that won't fit on now because it's not *pushed* down enough.

Note: Passives with a *by*-phrase in bold. The subject of * utterances was clear from context.

Push was not more likely to appear as a passive than our other verbs. *Kiss* was the verb most likely to appear as a passive with 2.8% of utterances occurring in the passive and all three *full* passives using *kiss*. However, of the 12 total passive utterances, *push* was as frequent as *kiss*, with 5 passives for each verb.

Appendix C

Principal components analysis

Children's primary caregiver completed the Macarthur-Bates Communicative Development Inventory at 30 months, which included measures of vocabulary, grammatical complexity and mean length of

Table C1
Spearman's correlations between predictors measured at 30 months.

	1	2	3
1. Vocabulary	–		
2. Complexity	.56***	–	
3. MLU	.42***	.49***	–

*** Correlation is significant at the.001 level (2-tailed).

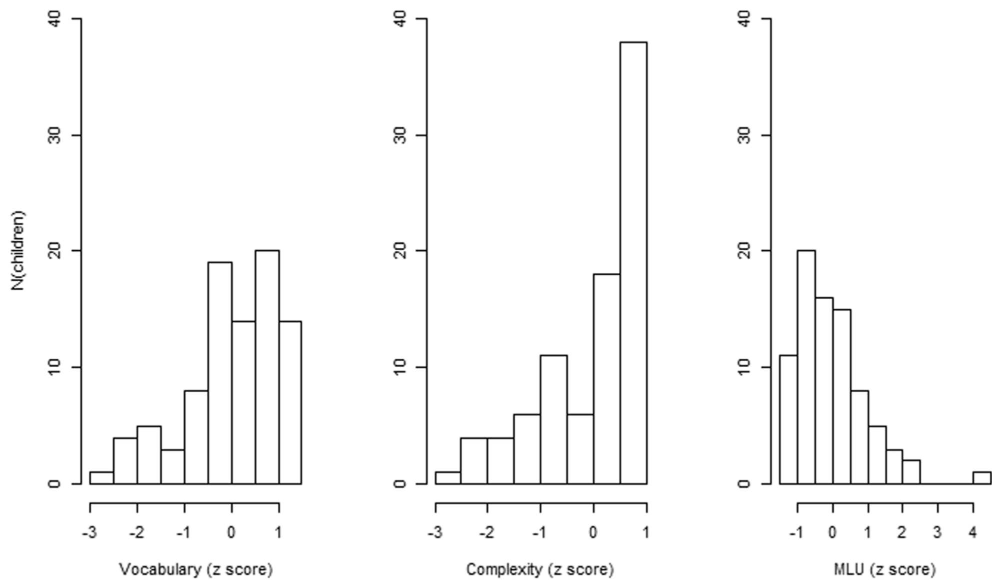

Fig. C1. Histograms for each predictive variable.

utterance (MLU). Table C1 presents the correlation matrix between the variables, which were all significantly correlated at medium to large correlation sizes.

Fig. C1 presents histograms for each of the three variables. All have skewed distributions, with left skew in the vocabulary and grammatical complexity measures and MLU being right skewed. Given their inter-correlation and skewed distributions, we decided to run a principal components analysis to check whether the three variables could be reduced to a single measure of language proficiency.

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