Filling the gap in gap-filling: long distance dependency formation in sentence production

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Abstract

In a sentence like *Who does the artist think chased the chef?*, the *who* at the beginning depends on the last bit of the sentence, *chased the chef*. This is an instance of a *long-distance dependency*. What is the nature of the cognitive process that allows speakers to produce sentences that include distant elements that form dependencies? In four experiments (total N = 176), speakers described drawings that elicited long-distance dependencies. Critically, speakers were sometimes primed to produce a *that* in sentences where *that* was ungrammatical due to a grammatical constraint known as the *that*-trace constraint (e.g., **Who does the artist think that chased the chef*). Results showed that, when primed to say an ungrammatical *that*, speakers were slower to *start* to speak. Because the *that*-trace constraint applies selectively to certain configurations of long-distance dependencies, this suggests that the grammatical details of the long-distance dependency are already planned before speakers start to speak the sentences involving long-distance dependencies. I propose a formal model that explains how speakers plan long-distance dependencies in advance of speaking them while also managing the cognitive pressure to speak sentences incrementally.

Keywords: Sentence production, Structural priming, Syntactic processing, Filler-gap dependency, Picture-word interference

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Introduction

One of the peculiar properties of human language is the existence of grammatical relationships between words and phrases that are arbitrarily far apart. These are known as long-distance dependencies in linguistics and have been studied extensively in both syntax (Chomsky, 1957, 1965, 1973, 1981, 1986, 1995; Frank, 2004; Kroch & Joshi, 1985; Lasnik & Saito, 1994; Pollard & Sag, 1994; Ross, 1967, among many others) and in how readers or listeners understand sentences (Aoshima, Phillips, & Weinberg, 2004; Clifton & Frazier, 1989; Fodor, 1978; Frazier, Clifton, & Randall, 1983; Frazier & d’Arcais, 1989; Garnsey, Tanenhaus, & Chapman, 1989; Omaki et al., 2015; Wanner & Maratsos, 1978, among many others), due to their implications about both the nature of core computational mechanisms underlying human language and about the nature of the cognitive mechanisms underlying language processing, such as working memory. However, in contrast to our knowledge of the syntax and the comprehension of long-distance dependencies, our knowledge of how speakers formulate long-distance dependencies in production is limited, and most models of language production do not describe the processes involved in forming long-distance dependencies in detail. Against this background, the current paper aims to examine how speakers plan sentences involving long-distance dependencies, specifically focusing on filler-gap dependencies.

In this article, I specifically focus on two types of constructions involving filler-gap dependencies: *wh*-questions and relative clauses in English. *Wh*-questions and relative clauses involve non-adjacent elements that form both semantic and syntactic dependencies. For example, in the sentence *Who do you think that the girl likes*, *Who* and *likes* form a (discontinuous) constituency and dependency relationship, specifically a head-complement relationship in syntax and an argument-predicate relationship in semantics. Similarly, in *the boy who the girl likes came to the lab*, the relative pronoun *who* under the head-external and the matching analysis *likes* form a dependency and constituency relationship (e.g., Jackendoff, 1977; Chomsky, 1965; Quine, 1960). For the expository purposes, here I adopt the terminology from psycholinguistic literature and henceforth refer to the extracted elements as *fillers* and the traces that are left behind as a result of movement as *gaps*.

The main argument in this paper is the following: speakers plan the grammatical details of filler-gap dependencies before starting to speak the filler, without necessarily planning words that intervene the filler and the gap. That is, the filler and the gap may be represented temporarily close together in speakers’ mind, despite the fact that they appear at a distance on the surface. Speakers are somehow capable of ‘extending’ the planned dependency by some operation that inserts material in between the head and the tail of dependency. I will provide evidence supporting these claims, and develop a formal model that captures these claims.

Filler-gap dependency in production: previous studies

Despite several decades of experimental studies on sentence production, little is known about filler-gap dependency formation processes in sentence production. Indeed, prominent overviews on language production models from last twenty years or so (Bock, 1995; Bock & Ferreira, 2013; Bock & Levelt, 1994; Ferreira, 2000; Ferreira & Slevc, 2007; Griffin & Ferreira, 2006) either do not discuss filler-gap dependencies entirely or include only brief notes

about them. Of course, there are occasional studies that investigate constructions involving filler-gap dependencies (Asudeh, 2012; Do & Kaiser, 2019; Fadlon, Morgan, Meltzer-Asscher, & Ferreira, 2019; Ferreira & Swets, 2005; Gennari, Mirković, & MacDonald, 2012; Scheepers, 2003; Scontras, Badecker, Shank, Lim, & Fedorenko, 2015; Zukowski, 2009), but many of these studies use filler-gap dependency constructions to study other aspects of production, such as incrementality (Ferreira & Swets, 2005), the relationship between word order and lexical competition (Gennari et al., 2012), the relationship between hierarchical and linear structures in production (Scheepers, 2003), factors guiding the initial message encoding (Do & Kaiser, 2019), and why resumptive pronouns are produced (Asudeh, 2012; Ferreira & Swets, 2005). The nature of long-distance dependency formation process itself is still poorly understood, as I review below.

It is known, however, that that speakers retain in memory *some* information about filler-gap dependencies as they produce sentences (Fadlon et al., 2019; Momma & Phillips, 2018). At the very least, when producing a sentence involving a filler-gap dependency, speakers must remember that there is a filler-gap dependency as they speak. If this were not the case, (a) speakers should frequently produce erroneous utterances like *The boy thinks that the girl likes*, where the argument is missing in absence of a filler gap-dependency and (b) speakers should frequently produce erroneous utterances like *Who does the boy likes who/someone?* or *The girl that the boy likes the girl*, where the fillers are repeated (Fadlon et al., 2019). But these kinds of errors, as far as I know, are not prevalent (though they are observed relatively frequently in children, at least in experimental settings, Grolla & Lidz, 2018).

There is also some evidence that holding filler-gap dependency representations in memory is costly. Fadlon et al. (2019) showed that speakers have some strategies to minimize the distance between the filler and the gap. Fadlon and colleagues showed that when producing a non-restrictive relative clause (e.g., *The day-worker that, as mentioned, the farmer praised/was praised by the farmer*), English speakers tended to use the passive voice more in the embedded clause compared to when producing a restrictive relative clause (e.g., *The day-worker that the farmer praised/was praised by the farmer*). Based on previous findings in comprehension (Dillon, Clifton, Sloggett, & Frazier, 2017), a non-restrictive relative clause was hypothesized to hamper filler retention, because non-restrictive relative clauses were assumed to be syntactically inaccessible, and the intervening inaccessible material was assumed to hamper filler retention (Note, however, that Dillon et al. found that long-distance dependency parsing with non-restrictive relative clauses are *easier* than with restrictive relative clauses, so the opposite prediction could have been made). Fadlon and colleagues suggested that speakers tend to minimize the distance between the filler and the gap by using the passive voice (in English) when filler retention is made difficult (by non-restrictive relative clauses), presumably to reduce the memory burden due to the filler retention. Combined with the observations discussed above, it is relatively clear that speakers must keep in working memory *at least* some information about filler-gap dependencies as they speak. Thus, it is reasonable to assume that speakers must retain in memory at least the presence of filler-gap dependencies until the dependency can be resolved at the gap position.

In an elicited production task, Scontras et al. (2015) reported that object relative clauses and object wh-questions (with full noun phrases inside the wh-phrase, as in *which boy*) are produced with longer latency and duration than subject relative clauses and subject wh-questions (see also Zukowski, 2009). For example, they found that speakers are faster to speak subject relative clauses, which they argue reflect the difference in structural complexity between object and subject extraction sentences. Their result is not easy to interpret because there are multiple potential explanations for the observed subject relative clause advantage. For example, speakers in their experiments needed to inhibit passive subject relative clause production selectively in the object relative clause production condition, which would explain the increased production time in the object relative clause production (MacDonald, Montag, & Gennari, 2016; but see Scontras, Badecker, & Fedorenko, 2017 for a reply). Also, the fourth word (the second content word) of the sentence is different in syntactic category between subject and object extraction sentences. In object relative clauses, the second noun phrase is closer to the first noun phrase, so speakers should experience lexical competition, leading object extraction sentences to be harder (cf. Gennari et al., 2012). Thus, the precise cause of their effect is not clear. Nevertheless, their results are compatible with the view that speakers retain filler-gap dependencies in memory until dependencies are resolved. Under this interpretation, object extraction sentences are harder to produce because filler and gap are separated further (in linear or structural distance) in object extraction sentences than in subject extraction sentences.

Previous studies show that filler-gap dependency production involves retaining some information about filler-gap dependencies over time. However, the nature of filler-gap dependency representations kept in memory remains unclear. On the one extreme, speakers may simply retain a syntactically under-specified filler representation in memory until immediately before they have to speak the gap position. Under this view, speakers may postpone decisions about the syntactic status of the gap until late in an utterance. This strategy may be beneficial for speakers because it allows flexible production. At the other extreme, speakers may fully commit early to the syntactic and semantic status of the filler, including its conceptual roles, grammatical function, case, and syntactic environment. This strategy may be beneficial for speakers because it allows speakers to avoid producing ungrammatical structures that violate various constraints on filler-gap dependencies. Both of these extreme possibilities remain viable, as they are compatible with the observations and previous studies discussed above.

The time-course of filler-gap dependency formation

To study the production of filler-gap dependencies in detail, it is necessary to first characterize the problems speakers must solve to produce well-formed filler-gap dependencies. I argue that filler-gap dependency formation in production can be usefully characterized as a series of grammatical decision-making problems. To produce filler-gap dependencies, speakers must eventually make decisions about at least the following properties of the filler: conceptual role, thematic role, case, grammatical function, and syntactic location (in hierarchical tree representations). Under this framework, one question is when these decisions are made in the course of production. This time-course question is useful in understanding how filler-gap representation is computed as speakers construct sentences.

First, when do speakers make a decision about the conceptual role of a filler? In the standard model of sentence production, speakers are assumed to know, roughly, ‘who-did-what-to-whom’ of the event that they are trying to convey before starting the linguistic formulation process (Ferreira & Slevc, 2007; Griffin & Bock, 2000), at least under some circumstances. Thus, according to the standard assumption, speakers should already commit to a specific conceptual role for a filler/gap before starting to formulate the linguistic features, and therefore, before starting the articulation of the filler. Supporting this claim, previous studies suggest that speakers can extract schematic information about events very early on in picture description tasks to guide their sentence formation processes (Griffin & Bock, 2000), at least when the event is easy to apprehend (Van de Velde, Meyer, & Konopka, 2014). This view is often referred to as *hierarchical incrementality*, and there is a general agreement that some version of this view is correct, though the definition of the term varies from study to study. There are studies that suggest that relational information may not strongly constrain the initial sentence formulation process (cf. Gleitman, January, Nappa, & Trueswell, 2007), but those studies do not suggest that relational representations are absent at the early stage of planning processes. Thus, along with many others, I assume that speakers at least know the conceptual role of the filler before it is spoken.

Second, when do speakers make a decision about the grammatical case, and relatedly, grammatical function of the filler? Here, for simplicity, I discuss case and grammatical function together, though they are demonstratively distinct (e.g., Japanese has nominative objects and genitive subjects). For current purposes, this distinction is not critical. On the one hand, at least in non-case-marking languages like English, it is possible that speakers postpone the decision about the case and grammatical function of the filler and the gap, until shortly before speakers reach the gap. Under this view, speakers may produce a functionally underspecified filler. On the other hand, it is possible that speakers immediately commit to the case and grammatical function for the filler and the gap. Under this view, speakers do not start speaking the filler until they specify the case and grammatical function of the filler and the gap. Consistent with this view, many languages (including, to some limited extent, English) morphologically mark the case of the filler (e.g., *who* is nominative and *whom* is accusative). Given this observation, it is plausible that speakers decide on the case and grammatical function of the filler before they choose an appropriate filler form (i.e., before the onset of the filler production). But this evidence is limited in two ways. First, English speakers rarely use *whom* in spoken language (as opposed to written language, where speakers can edit the output sentence as they wish - and they still may not use *whom* regularly), so it is not clear if English speakers commit to the case of the filler even when *who* is used. Second, the existence of many languages with case-marking does not necessarily mean that English speakers also need to specify the case of the filler, because speakers of a different language may develop different strategies for satisfying language-specific requirements. Nevertheless, these observations suggest that speakers at least must specify the grammatical function and case before uttering the filler, at least under some circumstances. However, it remains unclear when speakers commit to a specific case or grammatical function of a filler in non-case-marking languages like English.

Third, when do speakers make a decision about the syntactic environment of the gap? Making an early decision on the syntactic environment of the gap could be useful

in avoiding illicit filler-gap dependencies, due to the existence of syntactic islands and various other constraints on filler-gap dependencies (Boeckx, 2012; Ross, 1967). For example, the production system must ensure that the gap will not be immediately preceded by a complementizer (clause introducer) *that* (to avoid what is termed a *that*-trace violation). There are many other constraints on long-distance dependencies, so speakers must ensure that the gap is not located in the illicit syntactic environment. Thus, to reliably avoid illicit filler-gap dependencies, speakers may need to commit to the syntactic details of the filler-gap dependencies early. However, if speakers make decisions about the syntactic environment of the gap, speakers may lose flexibility in speaking, making the production process more difficult. Indeed, speakers in many languages can, and often do, use a pronoun instead of a gap (known as resumptive pronouns) in island contexts (Ferreira & Swets, 2005; Morgan & Wagers, 2018). Thus, it could still be the case that speakers do in fact frequently talk themselves into a corner, resorting to using a resumptive pronoun to seemingly save the utterance (but see Momma & Phillips, 2018 for a different view). Overall, it is unclear when speakers commit to the detailed properties of the syntactic environment surrounding the gap.

Finally, when do speakers commit to a specific predicate for a gap? Fillers, just like other arguments, need a predicate to obtain their relational meaning in a sentence. In order to reliably determine the case and the grammatical function of a filler, the argument structure information about the gap-hosting predicate may be indispensable, at least for object arguments, (which are sometimes thought to be the only argument of verbs, Kratzer, 1996, 2003). Furthermore, English and many other languages allow (and sometimes require) the pied-piping of a preposition in *wh*-questions. For example, in English, speakers can say sentences like *On what did the girl dine?*, but not *On what did the girl eat*. The very fact that speakers can correctly insert or omit the appropriate preposition before the filler suggests that the predicate and its argument structure information can already be planned before the filler. Furthermore, because the choice of prepositions is to some extent idiosyncratically determined by verbs, speakers may need to plan a specific verb that selects the preposition before articulating the pied-piped filler. Thus, speakers might need to select a predicate that in turn selects the gap-licensing preposition before saying a filler. But this evidence is limited in two respects. First, pied-piping is a relatively rare construction, especially in oral speech. Second, it is possible that, in speaking, the choice of prepositions may constrain the choice of verbs, rather than the other way around. It could be that speakers probabilistically choose a preposition early, without committing to a specific verb, and then choose a verb that is compatible with the chosen preposition later. For those reasons, it is still unclear when speakers commit to a specific predicate for the filler before the filler is produced.

To summarize, filler-gap dependency formation in production can be characterized as involving series of inter-related grammatical decision-making processes. But the time-course of these decision-making processes remains poorly understood. Characterizing the time-course of processes is critical in developing and constraining an algorithmic theory of filler-gap dependency formation processes, and so ultimately, of how the sentence-production process works. The current study thus aims to characterize the time-course of grammatical decision-making processes involved in the production of sentences involving filler-gap dependencies.

The early vs. the late commitment hypotheses

As an initial attempt, two extreme hypotheses were contrasted: the *early commitment* hypothesis and the *late commitment* hypothesis. The early commitment hypothesis posits that speakers commit to detailed syntactic features of the filler/gap early in the sentence production process. By *early*, I specifically mean before the filler is articulated. Note that the filler may not be at the beginning of the sentence (e.g., *I wonder who the dog likes*). For this reason, by *early* I do not necessarily mean before the speech onset, but before the production of the filler. Why plan the grammatical details of filler-gap dependency early? The potential motivation for the early commitment hypothesis is to avoid illicit filler-gap dependencies. If speakers know the grammatical details of filler-gap dependencies before the filler is spoken, they can ensure that the form of the filler will not lead to a violation of grammatical constraints on long-distance dependencies. Moreover, early commitment might be helpful in languages that have case-marking or obligatory pied-piping, because knowing the syntactic features of the filler is helpful in determining the appropriate phonological form of the filler. In contrast, the late commitment hypothesis posits that speakers commit to specific syntactic features of the filler right before the speakers produce the gap-hosting verb. Why plan the grammatical details of the filler-gap late? The potential motivation for this strategy is to maintain flexibility in production. For example, speakers may want to passivize the wh-question after they start speaking when they notice difficulties naming the grammatical subject. Indeed, other evidence suggests that speakers produce utterances so as to delay commitments as much as possible (Wasow, 1997). Thus, both the early and late commitment strategies are viable, as they both have potential functional benefits for speakers.

Of course, the truth may lie somewhere between the two extremes. For example, speakers may make an early commitment to the grammatical function of the gap but not the syntactic environment, or plan in different ways at different times. Also, it is possible that how speakers plan long-distance dependencies vary cross-linguistically. However, these two extreme hypotheses are both viable in light of the discussion above, at least in English. Thus, I consider them a useful starting point to contrast two extreme possibilities.

Current experiments

The main goal of the current experiments is to examine the time-course of the construction of filler-gap dependency representations in real-time production. To this end, I introduce two methodologies that allow us to probe the time-course of gap and gap-hosting verb planning processes.

Investigating the time-course of filler-gap dependency formation. As far as I am aware, there is no standard method that that allows for the investigation of the time-course of filler-gap dependency formation processes in production. Thus, I need to first establish a method that allows us to probe when speakers make grammatical decisions about filler-gap dependencies. To explain the method, two concepts need to be introduced first. First, I exploit a constraint known as the *that-trace constraint* (Perlmutter, 1968; Pesetsky, 2017). The *that-trace* constraint prohibits filler-gap dependencies, selectively when the gap is the subject of the embedded clause that is headed by the overt complementizer *that*. For instance, consider the following sentences:

1. Who do you think likes the boy?
2. * Who do you think that likes the boy?
3. Who do you think the boy likes?
4. Who do you think that the boy likes?

This pattern of acceptability shows that a subject gap immediately preceded by an overt complementizer *that* is prohibited in English (a similar constraint can be found in other languages, e.g., French and Russian, see Pesetsky, 2017 for an overview). The reliability of the *that*-trace effect is well-established via laboratory-based acceptability judgment studies (Ritchart, Goodall, & Garellek, 2016). The precise cause of this constraint is not clear, though it is unlikely to be reducible entirely to prosodic factors (Ritchart et al., 2016). For current purposes, the specific cause of this constraint is not the main topic of interest (although I will discuss it in more depth in General Discussion). What is relevant here is that this constraint bans the formation of a filler gap dependency specifically when the gap is in the subject position in an embedded clause headed by the overt complementizer *that*.

Second, I use *structural priming* (Bock, 1986 among many others). Structural priming refers to speakers' tendency to reuse syntactic structures that they have recently encountered. For example, when a speaker encounters a sentence like *the girl gave the boy the book*, the speaker is more likely to reuse the double-object dative construction when they speak a ditransitive sentence. More recently, Ferreira (2003) showed that speakers tend to use the overt complementizer *that* more often when the preceding prime sentence also contains the overt complementizer *that*, compared to when the preceding prime sentence contains a phonologically null complementizer. Phonologically null complementizer is believed to exist in the mental representations of sentences, partly because it is syntactically primable (indeed, Ferreira showed that phonologically null complementizer may be even *more* primable than the *that* complementizer), and partly because supposing its existence is helpful in explaining syntactic distribution of embedded clauses.

Importantly, the *that*-trace effect and the *that*-priming effect can conflict with each other. On the one hand, the *that*-trace constraint prevents speakers from using optional *that* when there is an embedded subject gap. On the other hand, the *that*-priming effect can encourage speakers to use optional *that*. Due to this conflict, it is possible that speakers will be slower to produce an embedded subject-gap sentence when a prime sentence includes a *that*. I call this hypothesized slow-down effect the *adverse that-priming* effect.

Given that the time-course of filler-gap dependency formation is our main focus, the critical question is not just *whether*, but *when* the adverse *that*-priming effect can be observed (though the *whether* question is still important in its own light). In order for a speaker to experience an adverse *that*-priming effect, they must have already committed to placing the gap in the subject position of the embedded clause headed by the overt complementizer *that*. Thus, if speakers experience an adverse *that*-priming effect before the production of the filler, the early commitment hypothesis is supported. On the other hand, if speakers experience the adverse *that*-priming effect late in the utterance before they need to produce the gap, the late commitment hypothesis is supported. Thus, by measuring when speakers experience the adverse *that*-priming effect, it is possible to infer when speakers commit to the grammatical function, the syntactic position, and the syntactic environment of the gap.

Investigating the timing of verb planning

In addition to probing the time-course of the adverse *that* priming effect, it is also useful to study when speakers plan gap-hosting verbs, to understand what pieces of the sentences are needed to construct filler-gap dependencies. However, as I discussed above, the prediction about when speakers plan verbs according to the early and late commitment hypothesis is subtle. To explain why, I need to first introduce the method for probing when speakers plan verbs, and previous research on the timing of verb planning in sentence production.

An experimental task that has proven useful in examining the timing of verb planning is the *extended picture-word interference* task (Meyer, 1996; Momma, Slevc, & Phillips, 2016; Momma et al., 2018; Schnur, 2011; Schriefers, Teruel, & Meinshausen, 1998; Wagner, Jescheniak, & Schriefers, 2010), which is an extension of the picture-word interference (PWI) task (Lupker, 1979; Schriefers, Meyer, & Levelt, 1990). In a stereotypical PWI task, speakers simply name a picture while ignoring auditory or visual distractors that are presented temporally close to the picture. In the ePWI task, instead of naming simple objects or actions, speakers describe a picture in phrasal or sentence format, while ignoring distractors. Just like in a stereotypical PWI task, when a distractor is conceptually related to the target word (which is embedded in a phrase/sentence context in the ePWI task), speakers are slower to say the target utterance. This slow down effect is known as the *semantic interference effect*, which is argued to reflect the process of accessing abstract lexical representations known as *lemmas* (see Schriefers et al., 1990; Kempen & Huijbers, 1983; Levelt, Roelofs, & Meyer, 1999). The key idea is that the semantic interference effect on verbs can be used as a diagnosis of when speakers plan the verbs' lemma. For example, if speakers show a semantic interference effect from the distractor *run* before starting to speak *the girl is swimming*, that would suggest that speakers have already planned the lemma of the verb *swim* before starting to speak *the girl*. Thus, by examining whether speakers show the semantic interference effect on verbs before starting to speak the initial word of a sentence, it is possible to investigate if speakers plan the verbs' lemma before uttering the initial element of a sentence.

Using this logic, in a series of ePWI experiments in German, Schriefers et al. (1998) found no evidence for verb interference before the sentence onset, when the initial constituent is the subject, like in *the girl swims* (in German). This led some researchers to claim that speakers do not need verbs to grammatically encode their arguments, at least in verb-final languages (Allum & Wheeldon, 2007; Iwasaki, 2010). However, a more recent series of ePWI experiments suggest a different conclusion. Following up on Schriefers and colleagues' results, Momma et al., also using the ePWI paradigm, showed that the verb interference effect was observed before the speech onset, but selectively when the initial constituent was a patient or theme argument. They showed that, both in Japanese (Momma et al., 2016) and English (Momma & Ferreira, 2019; Momma et al., 2018), speakers are slower to start speaking given related verb distractors in ePWI experiments, but only when the initial noun phrase was a patient or theme argument of the target verb. These results accord well with some modern theories of syntax. Since Kratzer (1996), it is generally agreed that *internal arguments* (roughly, non-agentive arguments) are the only arguments of verb roots in the strict sense. *External arguments* (roughly agentive arguments) are not arguments

of verbs, but instead, they are arguments of a functional projection known as *little v* or *Voice*. Linguistic arguments for or against this view are technical and so beyond the scope of the present work (see Kratzer, 1996, 2003; Champollion, 2010; Williams, 2009, 2015). But in general, under this view, speakers do plan verbs before their arguments, but external ‘arguments’ are simply not the arguments of verbs. Combined with the view that speakers need predicates to grammatically encode arguments (including extracted arguments), it can be predicted that speakers plan predicates selectively before articulating their internal arguments, and not before articulating their external arguments. This prediction has been borne out at least in three independent studies, across three constructions in two languages (see Momma & Ferreira, 2019 for a summary).

Thus, given prior research, the early commitment and late commitment hypotheses do not necessarily make differing predictions with respect to when speakers plan gap-hosting verbs. However, the information about when speakers plan gap-hosting verbs is still useful in probing our central question: what representational pieces are used in formulating filler-gap dependencies? Moreover, examining when speakers plan verbs is useful for testing the possibility that speakers may simply plan sentences fully before starting to speak. If speakers are simply planning entire sentences before starting to speak, it would not be possible to distinguish between the early and late commitment hypotheses. To the extent I can show that the verb interference effect is observed on a just-in-time basis, this possibility can be excluded. For these reasons, I incorporated ePWI in two of our experiments, which I will report below.

Experiment 1

The goal of Experiment 1 was to examine when speakers plan the structural details of the gap and gap-hosting verbs by investigating the time-course of the adverse *that*-priming effect and the timing of the embedded verb interference effect in embedded wh-question production. The early commitment hypothesis predicts that the adverse *that*-priming effect should occur before the production of the filler (i.e., in the onset latency measure) only in embedded subject wh-questions. In contrast, the late commitment hypothesis predicts that the adverse *that*-priming effect should not occur before the production of the filler, but instead on a just-in-time basis. Both hypotheses predict that the verb interference effect should be observed on a just-in-time basis at least in the embedded subject wh-questions, so the timing of the verb interference effect does not distinguish between the two hypotheses. But examining the timing of the verb interference effect is useful for assessing the possibility that speakers simply plan the utterance entirely before the speech onset. If speakers simply plan the entire utterance before the speech onset, both the adverse *that*-priming effect and the verb interference effect should be observed in the speech onset latency measure.

Methods

Participants. Eighty-one undergraduate students at the University of California, San Diego participated in Experiment 1. All reported to be native speakers of English. Written consent was obtained from all participants. Twenty-nine participants were excluded because their error rates were too high (less than three error-free trials in any condition, twenty-two participants), because they did not follow instructions or because the experimenter

did not provide proper instructions (five participants), or because of the recording failures (two participants). The data from the remaining fifty-one participants were analyzed.

Material. Twenty-eight line-drawn picture frames like Figure 1 were prepared. For each picture frame, three versions corresponding to the subject question, object question, and filler conditions were created by replacing relevant event participants with stick figures. This resulted in the total of 56 experimental pictures and 28 filler pictures. Figure 1 shows an example picture used for eliciting subject and object questions in the critical condition, and matrix subject questions in the filler trials.

Figure 1. Example pictures for eliciting embedded subject question (*Who does the artist think is chasing the ballerina*), embedded object question (*Who does the artist think (that) the ballerina is chasing*), and matrix subject question (*Who thinks (that) the chef is chasing the ballerina*). Note that in all three grammatical positions, the set of characters used across trials was identical.

For prime sentences, I constructed a total of 112 two-clause sentences involving sentential complements (see Appendix for more details). Each experimental picture is paired with two prime sentences. Those 56 picture-sentence pairs were distributed across 8 conditions, according to a Latin square design. Related distractors for the verbs were chosen initially based on intuitive judgment and the relatedness judgments were verified using Latent Semantic Analysis (Landauer & Dumais, 1997). All distractor verbs were chosen from the set of target verbs used, to increase the chance of obtaining semantic interference effect (Roelofs, 1993). Those related distractors were re-paired with other pictures to create unrelated distractors. This means that no first-order properties of distractors can explain the difference between related and unrelated conditions. See the appendix for the full list of related and unrelated distractors.

Participants were told that the experiment involves two tasks: sentence recall and picture description. They were shown a sample trial that was identical in structure to actual trials, and they were told that their task was to do two things. First, they were told to memorize two sentences and recall one of them based on a prompt that matches the initial portion of one of the two memorized sentences. They were instructed to recall the entire sentence (including the initial part used as the prompt) aloud, as accurately as possible. Second, they were told to produce a question when they saw a picture. The relationship between the thought bubble and what participants were to produce was explained. They were also explained that each picture contained one unknown character (represented by a stick figure with a question mark on its face). Participants were told to formulate questions by asking the identity of the unknown character, while naming all the characters in the picture. They were instructed to use the character names and verbs they studied in the preceding familiarization phrases. Those instructions were provided alongside sample pictures

and sample sentences spoken by the experimenter. After participants were given those instructions, they were then asked to do four practice trials. Participants were observed by the experimenter for their understanding of the task, and if they did not follow instructions, their errors were corrected by the experimenter, and they repeated practice trials until they did the practice trials as prescribed.

Procedure. Each participant was tested individually. Each participant first underwent two familiarization phases. In the first familiarization phase, participants saw all fourteen characters involved in the event pictures used in the experimental stimuli. In the second familiarization phase, participants studied the entire set of event pictures (thirty pictures) with corresponding declarative sentences, two of which were used for the subsequent practice session. These familiarization phases were included to increase the consistency and production time and are a common practice in production research especially when timing is measured. After the familiarization phase, the experimenters explained the task structure with the practice pictures.

Each trial consisted of the following events. First, participants were presented with two written sentences, one at a time. Each sentence was displayed on the screen for 5 seconds, and after each prime sentence, the screen remained blank until the participant pressed the space bar. Following the memorization of the two sentences, the initial fragment of one of the two sentences appeared on the screen in red font. When the sentence contained *that*, the fragment also contained *that*. Participants recalled aloud one of the two memorized sentences that start with the sentence fragment (including the initial fragment). The rationale for requiring this memory task based on previous studies (Lombardi & Potter, 1992; Potter & Lombardi, 1990, 1998; Sachs, 1974) suggesting that speakers, even in a short-term recall, use message-level representations to regenerate memorized sentences. This message-based (re-)generation of sentences may strengthen the magnitude of syntactic priming effects. After they finished recalling the sentences aloud, they pressed the space bar. After their key press or after 10 seconds after the recall cue onset, whichever was earlier, the fixation cross appeared on the screen for 300 milliseconds. Following the fixation cross, the picture stimulus appeared on the screen for 12 seconds, or until they pressed the space bar after finishing their sentence, whichever was earlier. Participants spoke the appropriate sentence describing the picture as accurately as possible. A schematic illustration of the trial structure is shown in Figure 2.

Figure 2. Schematic illustration of the task structure in Experiment 1 and 2.

Scoring and Analysis. First, the analysis of the effect of *that*-priming on *that* production frequency was conducted. For this analysis, all trials where speakers produced sentences with the intended type of long-distance dependency were considered scorable. In case speakers repaired their utterances, the final sentence that they produced in that trial

was considered. Incomplete utterances were not considered scorable.

In all statistical models reported below, I initially constructed models with maximum random effects structure (Barr, Levy, Scheepers, & Tily, 2013). In case of convergence failure, I removed random slopes that explained the least amount of variance step-by-step, until the model converged. The final model structures are reported along with relevant results below.

For filler trials (in which speakers produced sentences with matrix subject extraction, (e.g., *who thinks (that) the chef is chasing the ballerina*), I built a logistic mixed-effects model with Prime Type as a fixed effect and with by-participant and by-item random intercepts and slopes for Prime Type. For critical trials (in which speakers produced sentences with the embedded subject or object extraction, e.g., *who does the artist think is chasing the ballerina* or *who does the artist think (that) the chef is chasing*), I conducted another logistic mixed-effects analysis with Prime Type, Extraction Type, their interactions, and Relatedness as fixed effects and with by-participant and by-item random slopes for Prime Type and Extraction Type. In this model, I did not include interactions involving Relatedness as fixed effects or random effects, because the interaction effects involving Relatedness are not our main theoretical interest, and because the full model involving all three factors and their interactions were too complex to ensure model convergence.

For the analyses of production time, a trial was identified as erroneous if it involved (a) a deviation from target sentences (with an exception described below); (b) an overt filler (e.g., *uh*, *um*); (c) a non-speech physiological noise, such as coughing, sneezing, clearing throat, sniffing, audible yawning and (d) a spill-over of recall sentences. Only the trials that were identified as error-free were submitted to production time analyses. However, I did not exclude trials where speakers used near-synonymous names for characters (e.g., *waiter* for *waitress* and *robber* for *burglar*), to maximize the number of data points.

To test if speakers plan gap structures before the filler or just in time, I defined two regions of interest: the speech onset latency (the onset region) and the total production time of the two words immediately preceding the gap (e.g., *artist think*, henceforth the *before-gap* region). There are other ways to define the second region of interest, but I defined these regions based on a prior study that asked a similar question (Momma & Ferreira, 2019). Briefly, the rationale is that, if a single process of computing the gap structure takes somewhere between 200-500ms, the effect should appear 1-3 syllables before the gap position, under the hypothesis that it is planned on a just-in-time basis. The two words immediately preceding the gap covers this time range. These two regions were first analyzed separately, using a mixed effects model with Prime Type, Extraction Type, Relatedness, and their interaction terms as fixed effects. Subsequently, to assess the time-course of the adverse *that*-priming effect, I compared the magnitude of the effect across the two regions by the following procedure. I first subsetting the data to the subject extraction conditions (because the adverse *that*-priming effect should only be observed in the subject extraction condition). Then, I constructed a mixed effects model involving Relatedness, Prime Type, Region, and their interactions as fixed effects based on the subsetting data. The random effects structure of the initial models for the time-course analysis included by-subject, by-item, and by-trial random intercept, as well as by-subject and by-item random slopes for Prime Type, Region and their interaction.

To test if speakers plan verbs before the utterance onset or just in time, I defined two regions of interest: the onset region (identical to the region defined for examining the adverse *that*-priming effect) and the total production time of the two words immediately preceding the embedded verb (the before-verb region). The second region of interest was chosen based on the same rationale as above. The time-course of the verb interference effect was then assessed by comparing the magnitude of the effect in this region and the onset region. For this analysis, I constructed a mixed effects model involving Prime Type, Extraction Type, Relatedness, Region, and their interactions as fixed effects, and by-subject, by-item and by-trial random intercepts, and by-subject and by-item random slopes for Relatedness, Region and their interaction.

Before the analysis of production time, clear outliers (production times of more than 5000 milliseconds) were removed. Additionally, any data points more than 3 median absolute deviations (MAD) away within each participant within each region were considered outliers and were removed (Leys, Ley, Klein, Bernard, & Licata, 2013). I chose the conservative threshold of 3 instead of the threshold of 2.5 recommended by Leys et al. (2013), because onset latency and production time are right-skewed and the semantic interference effect in sentence-level production task tends to reside in the right tail of the distribution (Momma & Ferreira, 2019).

Results

One participant failed to complete the experiment, so that participant only had 75 trials (instead of 84 trials). Thus, the total number of trials was 4359, instead of the expected 4368 (84 trials x 52 participants).

Production of *that*. Table 1 summarizes the frequency of *that* production across conditions. In filler trials, 1409 of 1452 (97.04%) of the filler trials were scorable. Among the 704 scorable filler responses following prime sentences with *that*, speakers produced *that* 381 times (54.12%). In comparison, among the 705 scorable filler responses following the prime sentences without *that*, speakers produced *that* 313 times (44.40%). Thus, speakers were 9.72% more likely to produce *that* when primed with *that* ($\hat{\beta} = 0.75$, $SE = 0.21$, $z = 3.52$, $p < .001$), replicating Ferreira (2003). The random effects structure of the final model included by-subject and by-item random intercepts, and a by-subject random slope for Prime Type.

In critical trials, 2778 of 2907 (95.56%) were scorable. Among the 1392 scorable responses in the *that* prime conditions, speakers produced *that* 73 times (5.24%). In comparison, among the 1386 scorable responses in the null prime conditions, speakers produced *that* 47 times (3.39%). That is, speakers were 1.85% more likely to produce *that* given a prime sentence with *that* than a prime sentence without *that*. The main effect of Prime Type did not reach significance ($\hat{\beta} = 1.32$, $SE = 0.76$, $z = 1.73$, $p = .084$), nor the interaction between Prime Type and Extraction Type ($\hat{\beta} = 0.16$, $SE = 0.60$, $z = 0.27$, $p = .789$). The main effect of Extraction Type was highly significant ($\hat{\beta} = -2.31$, $SE = 0.31$, $z = -7.49$, $p < .001$), suggesting that speakers were highly sensitive to the *that*-trace constraint. The random effect structures of the final model included by-subject and by-item random intercepts, and a by-subject random slope for Prime Type.

In the object extraction conditions, speakers produced *that* 58 times out of 687 trials

Table 1

The frequency of exitit{that} production in each condition in Experiment 1.

ExtractionType	Relatedness	PrimeType	Null frequency	That frequency	% that
Matrix (filler)	N/A	Null	392	313	44.40
Matrix (filler)	N/A	That	323	381	54.12
Object	Related	Null	326	22	6.32
Object	Related	That	312	29	8.50
Object	Unrelated	Null	322	17	5.01
Object	Unrelated	That	317	29	8.38
Subject	Related	Null	343	4	1.15
Subject	Related	That	344	9	2.55
Subject	Unrelated	Null	348	4	1.14
Subject	Unrelated	That	346	6	1.70

(8.44%) in the *that* prime condition and 39 times out of 687 trials (5.68%) in the null prime condition. That is, speakers were 2.77% more likely to produce *that* given the prime sentences with *that* than given the prime sentences with a null complementizer in the object extraction condition, but a planned comparison revealed that this difference was only marginally significant ($\hat{\beta} = -1.24$, $SE = 0.68$, $z = -1.81$, $p = .071$), likely because the rate of *that* production was near floor.

In the subject extraction conditions, speakers produced *that* 15 times out of 705 trials (2.13%) in the *that* prime condition and 8 times out of 699 trials (1.14%) in the null prime condition. That is, speakers were 0.98% more likely to produce *that* given the prime sentences with *that* than given the prime sentences with a null complementizer in the object extraction condition, but a planned comparison revealed that this difference was not significant ($\hat{\beta} = -1.40$, $SE = 0.94$, $z = -1.49$, $p = .136$), again, likely because the rate of *that* production was near floor.

Errors. Overall, 1972 of 2778 scorable critical trials (70.99%) were identified as error-free, and thus they were included the subsequent analyses of onset latency and production time. The analysis of errors using logistic mixed effects regression revealed that there was a marginally significant main effect of Relatedness ($\hat{\beta} = -0.18$, $SE = 0.11$, $z = -1.62$, $p = .105$) and Extraction Type ($\hat{\beta} = -0.20$, $SE = 0.09$, $z = -2.11$, $p = .035$). There was also a significant two-way interaction between Relatedness and Prime Type ($\hat{\beta} = 0.55$, $SE = 0.19$, $z = 2.91$, $p = .004$). This interaction effect suggests that speakers were more likely to produce erroneous utterances given related distractors, but only given the prime sentence with a null complementizer. It is unclear why the two way interaction is found; I had no a priori reason to predict this pattern. Post-hoc pairwise comparisons suggest that speakers made more errors given related distractors in the null prime condition ($\hat{\beta} = 0.46$, $SE = 0.15$, $z = 3.14$, $p = .002$), but not in the *that* prime condition ($\hat{\beta} = -0.09$, $SE = 0.15$, $z = -0.62$, $p = .535$). Given that this effect was not predicted, I will not interpret this effect further. No other factors were significant in this error analysis. The random effect structures of the final model included by-subject and by-item random intercepts, and a by-item random slope for Prime Type and Relatedness.

Production time analyses. The mean onset latency of the sentence, word-by-word production time, and accuracy rate in each condition is summarized in Table 2. The difference in production time in each region due to the adverse *that*-priming effect and due to the semantic interference effect are shown in Figure 4.

Table 2

The by-subject mean production time in milliseconds for each word in Experiment 1.

ExtractionType	Relatedness	PrimeType	Onset	Who	does	the	artist	think	is/the	chasing/chef	the/is	accuracy
Object	Related	Null	1778	299	289	251	565	659	284	483	307	70.98
Object	Related	That	1784	331	283	198	557	610	294	494	274	76.54
Object	Unrelated	Null	1757	310	294	222	591	646	270	498	276	77.88
Object	Unrelated	That	1685	305	275	206	586	615	286	504	263	71.97
Subject	Related	Null	1657	307	263	211	571	573	309	615	210	70.89
Subject	Related	That	1778	314	280	206	580	604	347	673	259	79.32
Subject	Unrelated	Null	1691	304	261	189	572	558	289	621	213	79.55
Subject	Unrelated	That	1808	319	260	189	570	586	312	627	224	80.11

Table 3

The summary of the statistical results in the onset region in Experiment 1.

	Estimate	Std. Error	t value	Pr(> t)
Intercept	1781	76	23.21	< .001
Prime type	42	26	1.60	.109
Extraction type	-18	26	-0.71	.477
Relatedness	-10	27	-0.38	.706
Prime type x Extraction type	124	52	2.37	.018
Prime type x Relatedness	-43	53	-0.82	.412
Extraction type x Relatedness	58	55	1.06	.291
Prime type x Extraction type x Relatedness	60	105	0.57	.569

Figure 3. By-subject mean onset latency in the subject extraction conditions (left panel) and the object extraction condition (right panel) in Experiment 1. Error bars represent within-subject standard error of the means. For the graphing purpose only, for each region, any data points with the production time of more than 2000ms were excluded.

The adverse that-priming effect. As explained in the analysis section above, there were two regions of interest for examining the adverse *that*-priming effect: the onset region and the before-gap region (e.g., *artist think*). In the onset region, speakers were on average 121 ms slower to start speaking subject extraction sentences in the *that*-prime condition than in the null prime condition. In comparison, speakers were 27 ms *faster* to start speaking object extraction sentences in the *that*-prime condition than in the null prime condition. As can be seen in Table 3, a mixed effects analysis revealed a significant two-way interaction between Prime Type and Extraction Type. Subsequent planned comparisons revealed that there was a significant effect of Prime Type in the subject extraction condition ($\hat{\beta} = -104.92$, $SE = 36.75$, $t = -2.86$, $p = .004$), but not in the object extraction condition ($\hat{\beta} = 20.05$, $SE = 38.00$, $t = 0.53$, $p = .598$). The random effect structures of the final model involved by-subject and by-item random intercepts only.

Figure 4. By-subject mean onset latency in the subject extraction conditions (left panel) and the object extraction condition (right panel) in Experiment 1. Error bars represent within-subject standard error of the means.

Table 4

The summary of the statistical results in the before-gap region in Experiment 1.

	Estimate	Std. Error	t value	Pr(> t)
Intercept	1165	42	27.18	< .001
Prime type	-19	17	-1.10	.271
Extraction type	-53	17	-3.00	.003
Relatedness	-21	18	-1.17	.242
Prime type x Extraction type	13	35	0.39	.700
Prime type x Relatedness	22	35	0.64	.525
Extraction type x Relatedness	-87	37	-2.36	.018
Prime type x Extraction type x Relatedness	-6	71	-0.08	.932

In the before-gap region, speakers were on average -9 ms slower to start speaking subject extraction sentences in the *that*-prime condition than in the null prime condition. In comparison, speakers were 25 ms faster to start speaking object extraction sentences in the *that*-prime condition than in the null prime condition. However, as can be seen in Table 4, there was no significant adverse *that*-priming effect: no effect involving Prime Type was significant. There was a significant effect of Extraction Type, suggesting that speakers took more time to say the region immediately preceding the embedded clause, perhaps because object extraction sentences are harder to produce (Scontras et al., 2015). The random effect structures of the final model involved the by-subject and by-item random intercepts only.

To characterize the time-course of the adverse *that*-priming effect, I compared the magnitude of the adverse *that*-priming effect between the onset region and the before gap region (see the analysis section above for more details). This analysis revealed a significant

Figure 5. By-subject mean production time in the before-gap region in the subject extraction conditions (left panel) and the object extraction condition (right panel) in Experiment 1. Error bars represent within-subject standard error of the means.

two-way interaction between Prime Type and Region, ($\hat{\beta} = 63$, $SE = 31$, $t = 2.02$, $p = .043$). The effect of Region was significant ($\hat{\beta} = 622$, $SE = 88$, $t = 7.08$, $p < .001$). But this simply means that speakers spent more time preparing their utterance than saying the before-gap region. The main effect of Prime Type was not significant ($\hat{\beta} = 13$, $SE = 16$, $t = 0.79$, $p = .430$) The random effect structures of the final model involved by-subject, by-item, and by-trial random intercepts, and by-subject and by-item random slopes for Region.

The verb interference effect. There were two regions of interest for examining the verb interference effect: the onset region and the before-verb region (e.g., *think is* in the subject extraction condition or *chef is* in the object extraction condition). In the onset region, speakers were 31 ms *faster* to start speaking subject extraction sentences given related distractors than given unrelated distractors. In comparison, speakers were 55 ms slower to start speaking object extraction sentences given related distractors than given unrelated distractors. But the interaction between Extraction Type and Relatedness was not significant, as can be seen in Table 3.

In the before-verb region, speakers were 47 ms slower to speak the pre-verb words in the subject extraction condition given related distractors than unrelated distractors. Meanwhile, speakers were 9 ms slower to speak pre-verbal words in the object extraction condition given related distractors. As can be seen in the Table 5, the main effect of Relatedness was marginally significant, and the main effect of extraction was highly significant. No other effects were significant. The random effects structure of the final model involved by-subject, by-item random intercepts, and the by-subject and by-item random slope for Relatedness. A subsequent planned comparison revealed that the effect of Relatedness was significant in the subject extraction condition ($\hat{\beta} = 55$, $SE = 25$, $t = 2.16$, $p = .036$), but not in the object extraction condition ($\hat{\beta} = 14$, $SE = 26$, $t = 0.54$, $p = .591$). This pattern suggests that the marginally significant main effect of relatedness was mainly driven by the subject extraction

Table 5

The summary of the statistical results in the before-verb region in Experiment 1.

	Estimate	Std. Error	t value	Pr(> t)
Intercept	786	28	27.82	< .001
Prime type	18	15	1.21	.225
Extraction type	132	15	8.37	< .001
Relatedness	-34	19	-1.74	.097
Prime type x Extraction type	10	30	0.35	.730
Prime type x Relatedness	6	31	0.21	.834
Extraction type x Relatedness	-40	32	-1.25	.212
Prime type x Extraction type x Relatedness	13	62	0.21	.834

sentences.

To characterize the time-course of the verb interference effect, I compared the magnitude of the verb interference effect between the onset region and the before verb region (see the analysis section above for more details). This analysis revealed a significant three-way interaction between Relatedness, Extraction Type and Region, ($\hat{\beta} = -139$, $SE = 62$, $t = -2.26$, $p = .024$). The random effects structure of the final model involved by-subject, by-item and by-trial random intercepts, and the by-subject random slope for Region. This pattern suggests that speakers experienced a greater verb interference effect in the before-verb region in the subject condition than in the object extraction condition, but the opposite pattern was found in the onset region. This three-way interaction is indeed consistent with the hypothesis that speakers plan verbs early in the object-initial sentences but late in the subject-initial sentences, but given that the two-way interaction between Relatedness and Extraction Type was not significant in each individual region of interest, I avoid giving a strong interpretation of this result.

Discussion

There were two main findings in Experiment 1. First, speakers were slower to start speaking embedded subject extraction sentences (e.g., *who does the artist thinks is chasing the ballerina*) in the *that*-prime condition than in the null-prime condition. This adverse *that*-priming effect was not found in *wh*-sentences with embedded object extraction (e.g., *who does the artist thinks is chasing the ballerina*), suggesting that the current pattern cannot be explained by any simple differences between the *that*-prime condition and the null-prime condition (e.g., the length of the prime sentences). Additionally, the adverse *that*-priming effect was larger in the onset region than in the before-gap region. Thus, the current result suggests that speakers plan the gap structure before starting to speak the filler, at least consistently enough to result in the robust adverse *that*-priming effect. This supports the early commitment hypothesis, and is not compatible with the late commitment hypothesis.

Second, when speaking the subject extraction sentences, speakers showed a verb interference effect in the production time of the before-verb region but not in the onset latency. This result suggests that speakers planned verbs primarily on a just-in-time basis, at least in the subject extraction sentences. The timing of verb planning in object extraction

Figure 6. By-subject mean production time in the before-verb region in the subject extraction conditions (left panel) and the object extraction condition (right panel) in Experiment 1. Error bars represent within-subject standard error of the means.

sentences is less clear, because the statistical results were not definitive. As explained in introduction, this just-in-time verb planning does not necessarily distinguish between the early and the late commitment hypothesis, because subjects may not be an argument of the verb at all (Kratzer, 1996, 2003). Thus, verbs may not be involved in the structural processes of forming subject filler-gap dependencies. However, the late timing of verb interference effect in the subject extraction condition is not easily compatible with the alternative interpretation of the early timing of adverse-*that* priming effect: that speakers simply planned sentences entirely before beginning articulation.

Thus, the results of Experiment 1 support the early commitment hypothesis. The adverse *that*-priming effect observed in the speech onset latency suggests that speakers already made detailed grammatical decisions about the gap before the filler is spoken. They are not easily compatible with the late commitment hypothesis, as the late commitment hypothesis predicts that speakers plan the gap structure immediately before starting to speak the embedded clause. Additionally, it is unlikely that speakers simply planned the entire utterances before articulating the filler. If speakers simply planned the entire utterances before articulating the filler, it is unclear why they predominantly planned the gap-adjacent verb on a just-in-time basis, as suggested by the late timing of verb interference effect in the subject extraction sentences. Thus, the most plausible interpretation of the current results is that speakers constructed the detailed grammatical representations of the filler-gap dependency well in advance, before the filler is spoken.

Finally, there was an unexpected significant interaction between Prime Type and Relatedness in the error analysis. Although this could be a spurious effect given that it was not predicted, further experiments are necessary to determine if this effect deserves interpretation. I continue to investigate these issues in subsequent experiments.

Experiment 2

Experiment 2 aimed to extend Experiment 1 by examining the production of sentences with embedded relative clauses. Many linguistic theories posit that relative clauses and wh-questions share essentially the same filler-gap dependency relations, because the constraints on the formation of wh-questions and relative clauses are similar. If speakers form filler-gap dependencies with wh-questions and relative clauses in the same fashion, the adverse-*that* priming effect observed in Experiment 1 should also be observed when speakers produce relative clauses in which the subject gap is located in a complement clause. Because the *that*-trace constraint also applies to relative clauses, the experimental logic used in Experiment 1 can be directly applied to the production of sentences involving relative clauses. Thus, participants in Experiment 2 produced the sentences containing subject relative clauses, like *The chef who the artist thinks is chasing the ballerina*, and sentences containing object relative clauses, like *The ballerina who the artist thinks the chef is chasing*, given the pictures likes in Figure 7. Just like in Experiment 1, in Experiment 2 I examined when speakers plan filler-gap dependencies by investigating when speakers experience the adverse *that*-priming effect when producing relative clauses.

Figure 7. Example pictures for eliciting embedded subject relative clause (*The chef who the artist thinks is chasing the ballerina is red.*), embedded object question (*The ballerina who the artist thinks (that) the chef is chasing is red.*), and matrix subject question (*The artist who thinks (that) the chef is chasing the ballerina is red.*). Note that the set of characters used for the embedded subject, embedded object, and matrix subject was identical across conditions.

Method

Participants. Ninety-eight undergraduate students at the University of California, San Diego participated in Experiment 2. All reported to be native speakers of English. Written consent was obtained from all participants. None participated in Experiment 1. Forty-eight participants were excluded because their error rates were too high (less than three, i.e., < 50% error-free trials in any condition, twenty-four participants), because they did not follow instructions or because the experimenter did not provide proper instructions (twenty-one participants), or because of the recording failures (three participants). The data from the remaining fifty participants were analyzed.

Materials. The same set of pictures, prime sentences, and verb distractors as in Experiment 1 were used, except that in Experiment 2, instead of replacing one of the event participants with a stick figure, one of the event participants was colored with translucent red, as in Figure 7.

Procedures. The same procedures as in Experiment 1 were used, except that in Experiment 2, the instruction for the picture description part was different. In Experiment

2, participants were asked to produce a relative clause that describes the character in the picture that was colored in. Participants were instructed to create the answer to the question, *who is red?* for each picture presented to them. Experimenters trained participants to produce a specific relative clause using example pictures.

Scoring and analysis. The scoring and analysis procedures were the same as in Experiment 1, except for the following. For the analysis concerning the verb interference effect, one more region of interest was added. In addition to analyzing the same two regions as before (the onset region and the before-verb region), I analyzed the total production time of the two words preceding the relative pronoun (*who*). This is because relative pronouns, not head nouns, are sometimes considered to be arguments of the embedded verb, under the non-raising analyses (e.g., Jackendoff, 1977; Quine, 1960). If this is the case, simply looking at the onset latency of an utterance is not a good test of the hypothesis that speakers plan verbs before starting to speak their object arguments. Hereafter, I refer to this region of interest *before-relative pronoun region*. The same analysis procedure as in the other regions was applied to this region.

Results

Production of *that*. Table 6 summarizes the frequency of *that* production across conditions. In filler trials, 1349 of 1397 (96.56%) of the filler trials were scorable. Among the 678 scorable filler responses following prime sentences with *that*, speakers produced *that* 183 times (26.99%). In comparison, among the 671 scorable filler responses following the prime sentences without *that*, speakers produced *that* 116 times (17.29%). Thus, speakers were 9.70 % more likely to produce *that* when primed with *that* ($\hat{\beta} = 0.96$, $SE = 0.28$, $z = 3.43$, $p < .001$), replicating Ferreira (2003). The random effects structure of the final model included by-subject and by-item random intercepts, and the by-subject random slope for Prime Type.

In critical trials, 2702 of 2798 (96.57%) were scorable. Among the 1358 scorable responses in the *that* prime conditions, speakers produce *that* 25 times (1.84%). In comparison, among the 1344 scorable responses in the null prime conditions, speakers produced *that* 19 times (1.41%). That is, speakers were not more or less likely to produce *that* given the prime sentence with *that* than the prime sentence without *that*. The main effect of Prime Type was not significant ($\hat{\beta} = -0.55$, $SE = 1.17$, $z = -0.47$, $p = .636$), nor the interaction between Prime Type and Extraction Type ($\hat{\beta} = -0.96$, $SE = 1.62$, $z = -0.60$, $p = .552$). The main effect of Extraction Type was significant ($\hat{\beta} = -3.92$, $SE = 0.82$, $z = -4.77$, $p < .001$), suggesting that speakers were highly sensitive to the *that*-trace constraint. The random effects structure of the final model included by-subject and by-item random intercepts, and the by-subject random slope for Prime Type.

In the object extraction conditions, speakers produced *that* 24 times out of 674 trials (3.56%) in the *that* prime condition and 18 times out of 667 trials (2.70%) in the null prime condition. That is, speakers were 0.86% more likely to produce *that* given the prime sentences with *that* than given the prime sentences with a null complementizer in the object extraction condition, but a planned comparison revealed that this difference was not significant ($\hat{\beta} = 0.07$, $SE = 0.73$, $z = 0.10$, $p = .921$), likely because the rate of *that* production was near floor.

Table 6

The frequency of that production in each condition in Experiment 2.

ExtractionType	Relatedness	PrimeType	Null frequency	That frequency	% that
Matrix (filler)	N/A	Null	555	116	17.29
Matrix (filler)	N/A	That	495	183	26.99
Object	Related	Null	326	8	2.40
Object	Related	That	322	13	3.88
Object	Unrelated	Null	323	10	3.00
Object	Unrelated	That	328	11	3.24
Subject	Related	Null	340	0	0.00
Subject	Related	That	340	1	0.29
Subject	Unrelated	Null	336	1	0.30
Subject	Unrelated	That	343	0	0.00

In the subject extraction conditions, speakers produced *that* 1 time out of 684 trials (0.15%) in the *that* prime condition and 1 time out of 677 trials (0.15%) in the null prime condition. That is, speakers were 0.00% more likely to produce *that* given the prime sentences with *that* than given the prime sentences with a null complementizer in the object extraction condition. This (non)difference was not significant ($\hat{\beta} = 1.04$, $SE = 1.87$, $z = 0.55$, $p = .581$), again, likely because the rate of *that* production was near floor.

Errors. Overall, 1974 of 2702 scorable critical trials (73.06%) were identified as error-free, and thus were included the subsequent analyses of onset latency and production time. The analysis of errors using logistic mixed effects regression revealed no significant main effect of Relatedness ($\hat{\beta} = -0.16$, $SE = 0.14$, $z = -1.14$, $p = .256$) and a significant effect of Extraction Type ($\hat{\beta} = -0.20$, $SE = 0.10$, $z = -2.03$, $p = .043$). Unlike in Experiment 1, the two-way interaction between Relatedness and Prime Type was not significant ($\hat{\beta} = 0.30$, $SE = 0.19$, $z = 1.58$, $p = .115$). But the pattern was similar as in Experiment 1. The random effects structure of the final model included by-subject and by-item random intercepts, and by-item random slopes for Prime Type and Relatedness.

Production time analyses. The mean onset latency of the sentence, word-by-word production time, and accuracy rate in each condition is summarized in Table 7. The difference in production time in each region due to the adverse *that*-priming effect and due to the semantic interference effect are shown in Figure 9.

Table 7

The by-subject mean raw production time for each word in Experiment 2.

ExtractionType	Relatedness	PrimeType	Onset	The	ballerina	who	the	artist	thinks	is/the	chasing/chef	the/is	accuracy
Object	Related	Null	1948	296	721	295	208	590	698	252	505	237	73.65
Object	Related	That	1976	258	732	297	220	597	688	275	506	251	76.42
Object	Unrelated	Null	1935	240	697	290	229	628	661	251	495	231	77.48
Object	Unrelated	That	1936	240	660	279	199	636	670	260	503	245	76.11
Subject	Related	Null	1957	241	706	291	200	558	583	317	608	211	76.47
Subject	Related	That	1966	288	711	300	217	578	582	313	633	203	77.71
Subject	Unrelated	Null	1880	269	742	287	215	595	591	289	624	202	83.09
Subject	Unrelated	That	2012	265	745	299	229	577	560	295	634	192	79.01

Figure 8. By-subject mean onset latency by condition in Experiment 2. Error bars represent within-subject standard error of the means. For the graphing purpose only, for each region, any data points with the production time of more than 2000ms were excluded.

The adverse that-priming effect. Again, there were two regions of interest for examining the adverse *that*-priming effect: the onset region and the before-gap region (e.g., *artist think*). In the onset region, speakers were on average 72 ms slower to start speaking subject extraction sentences in the *that*-prime condition than in the null prime condition. In comparison, speakers were 13 ms slower to start speaking object extraction sentences in the *that*-prime condition than in the null prime condition. Though this pattern is consistent with the results of Experiment 1, as can be seen in Table 3, the two-way interaction between Prime Type and Extraction Type was not significant, though the numerical pattern was similar to that in Experiment 1. The random effects structure of the final model included by-subject and by-item random intercepts only. Subsequent planned comparisons revealed that there was a marginally significant effect of Prime Type in the subject extraction condition ($\hat{\beta} = -65.56$, $SE = 36.84$, $t = -1.78$, $p = .075$), and no significant effect in the object extraction condition ($\hat{\beta} = -3.85$, $SE = 38.24$, $t = -0.10$, $p = .920$).

In the before-gap region (e.g., *artist thinks*), speakers were on average 2 ms faster to start speaking subject extraction sentences in the *that*-prime condition than in the null prime condition. Meanwhile, speakers were 9 ms faster to start speaking object extraction sentences in the *that*-prime condition than in the null prime condition. As can be seen in Table 4, there was a marginally significant main effect of Prime Type. But this marginally significant effect was in the opposite direction than the adverse *that*-priming effect, and this effect was not found in Experiment 1. Thus, I consider this marginal significant effect as spurious. There was again a highly significant effect of Extraction Type, suggesting that speakers took more time to say the region immediately preceding the embedded clause, perhaps because object extraction sentences are harder to plan. The random effects structure of the final model included by-subject and by-item random intercepts only.

To characterize the time-course of the adverse *that*-priming effect, I again compared

Table 8

The summary of the statistical results in the onset region in Experiment 2.

	Estimate	Std. Error	t value	Pr(> t)
Intercept	1961	97	20.09	< .001
Prime type	34	26	1.31	.191
Extraction type	2	26	0.11	.915
Relatedness	-28	27	-1.06	.290
Prime type x Extraction type	61	53	1.16	.245
Prime type x Relatedness	25	53	0.48	.631
Extraction type x Relatedness	-3	54	-0.06	.951
Prime type x Extraction type x Relatedness	123	106	1.17	.244

Figure 9. By-subject mean onset latency in the subject extraction conditions (left panel) and the object extraction condition (right panel) in Experiment 2. Error bars represent within-subject standard error of the means.

Table 9

The summary of the statistical results in the before-gap region in Experiment 2.

	Estimate	Std. Error	t value	Pr(> t)
Intercept	1186	35	33.28	< .001
Prime type	-4	17	-0.25	.802
Extraction type	-129	17	-7.31	< .001
Relatedness	18	18	1.04	.301
Prime type x Extraction type	11	35	0.33	.745
Prime type x Relatedness	-36	35	-1.03	.305
Extraction type x Relatedness	-4	36	-0.13	.897
Prime type x Extraction type x Relatedness	-49	70	-0.70	.485

Figure 10. By-subject mean production time in the before-gap region in the subject extraction conditions (left panel) and the object extraction condition (right panel) in Experiment 2. Error bars represent within-subject standard error of the means.

the magnitude of the adverse *that*-priming effect between the onset region and the before-gap region (e.g., *artist thinks*). This analysis failed to reveal a significant two-way interaction between Prime Type and Region, ($\hat{\beta} = 70$, $SE = 43$, $t = 1.61$, $p = .107$), though the trend was in the same direction as predicted. Like in Experiment 1, the main effect of Prime Type was not significant ($\hat{\beta} = 34$, $SE = 22$, $t = 1.53$, $p = .126$). The effect of Region was again significant ($\hat{\beta} = 23$, $SE = 102$, $t = 0.23$, $p = .822$). But this simply means that speakers spent more time preparing their utterance than saying the before-gap region. The random effects structure of the final model included by-subject, by-item, and by-trial random intercepts, and a by-subject random slope for Region.

The verb interference effect. There were three regions of interest for examining the verb interference effect: the onset region, the before-relative pronoun region (i.e., the first two words), and the before-verb region (e.g., *thinks is* for the subject extraction condition, and *chef is* in the object extraction condition). In the onset region, speakers were 14 ms *faster* to start speaking subject extraction sentences given related distractors than given unrelated distractors. In comparison, speakers were 8 ms slower to start speaking object extraction sentences given related distractors than given unrelated distractors. But the interaction between Extraction Type and Relatedness was not significant, as can be seen in Table 8.

In the before-verb region (i.e., *think is* / *chef is* region), speakers were 32 ms slower in the subject extraction condition given related distractors than unrelated distractors. In comparison, speakers were 4 ms slower to speak pre-verbal words in the object extraction condition given related distractors. As can be seen in the Table 11, the only significant effect was the main effect of Extraction Type. No other effects were significant. The random effects structure of the final model included by-subject and by-item random intercepts, and a by-item random slope for Relatedness. Unlike in Experiment 1, subsequent planned

Table 10

The results of the mixed effect model analysis on the production time in the before-relative pronoun region in Experiment 2

	Estimate	Std. Error	t value	Pr(> t)
Intercept	932	43	21.40	< .001
Prime type	5	15	0.36	.721
Extraction type	43	15	2.75	.006
Relatedness	-22	26	-0.85	.407
Prime type x Extraction type	-17	30	-0.58	.559
Prime type x Relatedness	-9	30	-0.32	.749
Extraction type x Relatedness	48	31	1.54	.124
Prime type x Extraction type x Relatedness	-34	60	-0.56	.573

comparisons revealed only a marginally significant effect of Relatedness in the subject extraction condition ($\hat{\beta} = 41$, $SE = 21$, $t = 1.98$, $p = .053$). There was no hint of the effect of Relatedness in the object extraction condition ($\hat{\beta} = -5$, $SE = 22$, $t = -0.23$, $p = .817$). This only marginally significant effect may be due to the fact that speakers often made prolonged pauses in the *think* region (because of the clausal boundary) both in Experiment 1 and 2. In Experiment 1, the verb interference effect in the verb region was primarily driven by the production time in the *is* region not the *think* region. Given this post-hoc result, I again analyzed the *is* region using the data from the subject extraction conditions. This post-hoc analysis suggests that speakers indeed showed a significant verb interference effect in the *is* region in the subject extraction condition ($\hat{\beta} = 14.13$, $SE = 4.62$, $t = 3.05$, $p = .002$). Thus, although the analysis on the before-verb region did not provide reliable evidence for the just-in-time verb planning, it is likely that the absence of the verb interference effect in the before-verb region in Experiment 2 was because the increased variability due to the inclusion of the *think* region, which corresponded to a clause boundary. However, given the marginal significance in the initial analysis and the exploratory nature of the follow-up analysis, this result should be interpreted cautiously.

In the before-relative pronoun region, speakers were 13 ms faster to speak the pre-relative pronoun words in the subject extraction condition given related distractors than unrelated distractors. In comparison, speakers were 45 ms slower to speak pre-relative pronoun words in the object extraction condition given related distractors. As can be seen in the Table 10, there was no significant two-way interaction between Extraction Type and Relatedness. Subsequent planned comparisons revealed no significant effect of Relatedness in the subject extraction condition ($\hat{\beta} = -2$, $SE = 31$, $t = -0.05$, $p = .957$) or in the object extraction condition ($\hat{\beta} = 47$, $SE = 32$, $t = 1.48$, $p = .145$).

Because there was no clear verb interference effect in the onset region, the before-relative pronoun region, or the before-verb region, I did not conduct the time-course analysis on those regions.

Figure 11. By-subject mean production time in the before-relative pronoun region in the subject extraction conditions (left panel) and the object extraction condition (right panel) in Experiment 2. Error bars represent within-subject standard error of the means.

Discussion

The results of Experiment 2 suggest that speakers experienced the adverse-*that* priming effect before speech onset when producing sentences with an embedded subject relative clause. This pattern is congruent with the hypothesis that speakers plan the structures of the gap before the filler in embedded relative clause production, just like in embedded wh-question production. However, unlike in Experiment 1, I failed to find the evidence for the selectivity of the adverse-*that* priming effect. That is, there was no significant interaction between Prime Type and Extraction Type. There are several potential reasons for this difference between Experiment 1 and Experiment 2.

First, it may be that speakers actually experienced some amount of the adverse-*that*

Table 11

The summary of the statistical results in the before-verb region in Experiment 2.

	Estimate	Std. Error	t value	Pr(> t)
Intercept	767	26	29.35	< .001
Prime type	4	12	0.35	.730
Extraction type	114	13	8.72	< .001
Relatedness	-17	15	-1.12	.278
Prime type x Extraction type	-16	25	-0.65	.518
Prime type x Relatedness	-18	25	-0.71	.479
Extraction type x Relatedness	-45	27	-1.69	.091
Prime type x Extraction type x Relatedness	-65	51	-1.27	.205

Figure 12. By-subject mean production time in the before-verb region in the subject extraction conditions (left panel) and the object extraction condition (right panel) in Experiment 2. Error bars represent within-subject standard error of the means.

priming effect in the object relative clause sentences as well, making it harder to reliably observe the difference between the subject and the object extraction conditions. Unlike in *wh*-questions, speakers must select a complementizer that introduces relative clauses, in addition to the complementizer introduced by the embedding verb *think*). Thus, speakers must choose between *that* and null complementizer in both subject relative clauses and object relative clauses. Critically, however, the presence of *who* prevents speakers from using the *that* complementizer (e.g., * *The chef who that the ballerina...*). This constraint, sometimes referred to as the *doubly-filled comp filter* (Chomsky & Lasnik, 1977), might have conflicted with the *that*-priming effect, resulting in a slow-down effect. This potential effect due to the conflict between the doubly-filled comp filter and the *that*-priming may have obscured the potential difference between the subject and the object relative clause conditions.

Second, it is possible that the predicted interaction effect between PrimeType and ExtractionType was not observed simply due to the lack of statistical power. Because the current task was very difficult, I needed to exclude many participants and consequently, only fifty participants were included in the data analysis. Although the sample size of fifty is not particularly small for a psycholinguistic study, the predicted effect of interest here was a non-cross-over interaction effect, which is not easy to detect statistically. In addition, the task in Experiment 2 was likely more difficult than in Experiment 1, probably because speakers needed to retrieve three character names for each target sentence in Experiment 2. This increased task difficulty might have increased the variability in speech onset latency, making it harder to reliably detect the interaction effect in question. Given this uncertainty, and given that the adverse *that*-priming effect has not been reported before, it is important to verify this effect with more experiments.

The verb interference effect was not significant in the before-verb region in Experiment 2, but this is likely because the production time of the before-verb region was not optimal. Speakers in both Experiments 1 and 2 showed a relatively long time in the region corresponding to the word *think*, probably because this region corresponds to a clausal boundary. Clausal boundaries increase the probability of pauses (e.g., Ford & Holmes, 1978). A long pause increases the variability in production time. The increased variability in production time may make the effect of distractor relatedness hard to see clearly, at least with the sample size I adopted here. Supporting this claim, the post-hoc analysis revealed the effect of distractor relatedness in the *is* region. Thus, it is likely that speakers in the current experiments planned embedded verbs mostly on a just-in-time basis in the subject extraction condition. On the other hand, there was no evidence that speakers planned the verbs on a just-in-time basis in the object extraction sentences. There was no statistically reliable verb interference effect in the object extraction condition in any regions I investigated, but looking at the numerical pattern in the before-relative pronoun region, speakers may have planned embedded verbs predominantly before starting to speak the relative pronoun (*who*) in the object extraction sentences, consistent with the claim that speakers plan verbs before starting to speak its internal arguments (Momma & Ferreira, 2019; Momma et al., 2016, 2018). Further experiments specifically designed to address this issue for relative clause production is needed to make any strong claims about when speakers plan verbs in long-distance object relative clauses.

The results of Experiment 1, and to some extent Experiment 2, are not easily compatible with the possibility that speakers simply planned the sentences from “beginning to the end” before the speech onset, extensively enough to include the gap. This possibility is unlikely because there was some evidence in Experiment 1, and to some extent Experiment 2, that speakers planned embedded verbs on a just-in-time basis, at least consistently enough for the just-in-time verb interference effect to be observed. In addition, there was no evidence that speakers showed any sign of verb planning before the sentence onset in either Experiment 1 or 2. Thus, to argue for the possibility that speakers simply planned the sentence extensively, one would have to argue that speakers in the current experiment planned sentences in advance, up to the gap to the exclusion of embedded verbs. I deem this possibility rather unlikely because gaps and embedded verbs are right next to each other in the current studies. However, gaps still linearly precedes verbs in subject extraction sentences, both in Experiment 1 and Experiment 2, and the statistical pattern in Experiment 2 was not very clear. Thus, I do not entirely disregard the hypothesis that speakers plan the sentence up to the gap but not the gap-hosting verb. Stronger evidence against this possibility would be to show that some word that linearly precedes the gap is planned later than the gap structure. I will come back to this issue below.

Finally, the presentation of distractor verbs may have altered the normal course of sentence planning. For example, it is in principle possible that speakers planned the gap structure in advance by anchoring the gap to the distractor verb presented to them. If this is the case, the results obtained so far can only be generalized to the current experiments where verb distractors were visually presented.

In sum, at least three issues remain. First, the selectivity of the adverse *that*-priming

effect has not been firmly established in relative clause production, due to the failure to obtain the significant interaction effect between Prime Type and Extraction Type in Experiment 2. Second, given that the verb interference effect was not robust, it is still possible that speakers planned the sentence extensively, both in *wh*-sentences and relative clauses. Third, the presence of verb distractors might have altered the time-course of normal sentence planning. To address these concerns, I conducted Experiment 3, where I aimed to test the selectivity of the adverse *that*-priming effect in relative clause production without the use of verb distractors.

Experiment 3

Experiment 2 failed to provide the strong evidence for the selectivity of the adverse *that* priming effect in embedded relative clause production. In Experiment 3, I aimed to retest whether speakers plan the gap structures prior to speaking the filler in embedded relative clause production. However, instead of simply repeating the same experiment, I decided to make number of design changes. First, Experiment 2 had relatively high error rates, and consequently, high exclusion rate of participants, probably because the experimental task was too difficult for some participants. Second, because the task was difficult, the variability in the speech onset latency was high, making effects unstable across participants and across items. Thus, in Experiment 3, I opted to design a simpler experiment, where speakers immediately repeat back one prime sentence per trial and describe target pictures using the same embedded verbs across the board (the verb used was *love*, see below for more details). This means that, in Experiment 3 (unlike in Experiment 2), speakers did not have to encode two prime sentences in memory for recall, and they also did not have to use a diverse set of verbs to speak target sentences. These simplifications were meant to alleviate the problem of high error rates and high exclusion rates found in Experiment 2.

In addition, in Experiment 3, I did not use verb distractors because I wanted to rule out the possibility that the early timing of adverse-*that* priming effect was due to distractor verbs. To the extent that I observe the similar results in Experiment 1, 2 and 3 in terms of the adverse-*that* priming effect, the early planning of gap structures is not the result of verb distractor presentation.

Method

Participants. Fifty-six undergraduate students at the University of California, San Diego participated in Experiment 3. All were reported to be native speakers of English. Written consent was obtained from all participants. None participated in Experiment 1 or 2. Seven participants were excluded because their error rates were too high (less than three error-free trials in any condition, following the same criterion as in Experiments 1 and 2). Seven more participants were excluded because they failed to follow instructions. The data from the remaining forty-two participants were analyzed.

Materials. Twenty-four picture frames similar to the ones used in Experiments 1 and 2 were used for Experiment 3. Unlike in Experiments 1 and 2, all pictures expressed the *love* relation between characters inside thought bubbles. Two versions for each picture frame were created: one that elicited embedded subject relative clauses and another that elicited embedded object relative clauses. Thus, a total of forty-eight different pictures were

created. These forty-eight pictures were distributed across four experimental lists, according to a Latin square design. Twenty-four additional pictures were created for filler trials, and they elicited embedded object extraction sentences. Note that the set of prime sentences were not the same for the subject and the object extraction conditions only in Experiment 3. This was because, in Experiment 3, the same picture frame was used twice for each participant, once for the subject extraction condition and the other for the object extraction condition. To avoid repeating the same prime sentence for the same participant, the same set of prime sentences could not have been used within a single experimental list. I could have counter-balanced the prime sentences exactly by doubling the number of experimental lists. However, the random difference in prime sentences between the subject and object conditions is unlikely to create the predicted interaction between Prime Type and Extraction Type. Thus, I decided to keep the simple Latin Square distribution with four experimental lists. Figure 13 shows the example picture of each type. New prime sentences of various types were also created. See Appendix for the list of prime sentences.

Figure 13. Example pictures for eliciting embedded subject relative clause (*The ballerina who the nun thinks loves the artist is red.*), embedded object relative clause (*The artist who the nun thinks (that) the ballerina loves is red.*), and matrix subject clause (*The nun who thinks (that) the ballerina loves the artist is red*) in Experiment 3. Note that the set of characters used for the embedded subject, embedded object, and matrix subject was identical across conditions.

Procedures. The procedures were similar to those in Experiment 1 and 2, but with following changes. First, Experiment 3 did not use distractor words unlike in Experiment 1 and 2. Second, speakers did not memorize two prime sentences, but instead simply read aloud one prime sentence. This change was made to make the task easier. Third, the target embedded verb was always *love*. Fourth, the prime sentence was displayed indefinitely, until participants pressed the space key. Finally, participants simply read aloud the prime sentence. The schematic illustration of the trial structure is shown in Figure 14.

Scoring and analysis. The same scoring criteria as in Experiments 1 and 2 were used in Experiment 3. The analysis procedures were also similar to Experiments 1 and 2, but this time, the analysis of the production time at the before-verb region was not conducted, because when speakers plan embedded verbs was not probed in the current experiment. Thus, the two regions of interest were investigated: the onset region and the before-gap region.

Results

Production of *that*. Table 12 summarizes the frequency of *that* production across conditions. In filler trials, 983 of 1008 (97.52%) were scorable. Among the 489 scorable filler responses following prime sentences with *that*, speakers produced *that* 79 times (16.16%). In

Figure 14. Schematic illustration of the task structure in Experiment 3.

comparison, among the 494 scorable filler responses following the prime sentences without *that*, speakers produced *that* 72 times (14.57%). Thus, speakers were 1.58% more likely to produce *that* when primed with *that*. This effect was marginally significant ($\hat{\beta} = 0.91$, $SE = 0.55$, $z = 1.66$, $p = .098$). The marginal significance was likely because the base rate of *that* production is low in Experiment 3. The random effects structure of the final model included by-subject and by-item random intercepts, and a by-subject random slope for Prime Type.

In critical trials, 1988 of 2016 (98.61%) trials were scorable. Among the 997 scorable responses in the *that* prime conditions, speakers produce *that* 56 times (5.62%). In comparison, among the 991 scorable responses in the null prime conditions, speakers produced *that* 48 times (4.84%). That is, speakers were 0.77% more likely to produce *that* given the prime sentence with *that* than the prime sentence without *that*. The main effect of Prime Type did not reach significance ($\hat{\beta} = 0.13$, $SE = 0.29$, $z = 0.45$, $p = .650$), nor the interaction between Prime Type and Extraction Type ($\hat{\beta} = -0.45$, $SE = 0.58$, $z = -0.79$, $p = .431$). The main effect of Extraction Type was highly significant ($\hat{\beta} = -2.10$, $SE = 0.30$, $z = -6.98$, $p < .001$), suggesting that speakers were highly sensitive to the *that*-trace constraint. The random effects structure of the final model included by-subject and by-item random intercepts only.

In the object extraction conditions, speakers produced *that* 47 times out of 495 trials (9.49%) in the *that* prime condition and 38 times out of 495 trials (7.68%) in the null prime condition. That is, speakers were 1.82% more likely to produce *that* given the prime sentences with *that* than given the prime sentences with a null complementizer in the object extraction condition, but a planned comparison revealed that this difference was not significant ($\hat{\beta} = -0.36$, $SE = 0.29$, $z = -1.23$, $p = .218$), likely because the rate of *that* production was near floor.

In the subject extraction conditions, speakers produced *that* 9 times out of 502 trials (1.79%) in the *that* prime condition and 10 times out of 496 trials (2.02%) in the null prime condition. That is, speakers were -0.22% more likely to produce *that* given the prime sentences with *that* than given the prime sentences with a null complementizer in the object extraction condition, but a planned comparison revealed that this difference was not

Table 12

The frequency of that production in each condition in Experiment 3.

ExtractionType	PrimeType	Null frequency	That frequency	% that
Matrix (filler)	Null	422	72	14.57
Matrix (filler)	That	410	79	16.16
Object	Null	457	38	7.68
Object	That	448	47	9.49
Subject	Null	486	10	2.02
Subject	That	493	9	1.79

significant ($\hat{\beta} = 0.10$, $SE = 0.50$, $z = 0.19$, $p = .847$), again, likely because the rate of *that* production was near floor.

Errors. Overall, 1611 of 1988 scorable critical trials (81.04%) were identified as error-free, and thus they were included in the subsequent analyses of onset latency and production time. The analysis of errors using logistic mixed effects regression revealed that there was no significant main effect of PrimeType ($\hat{\beta} = 0.12$, $SE = 0.18$, $z = 0.66$, $p = 0.51$) or interaction between PrimeType and ExtractionType ($\hat{\beta} = 0.04$, $SE = 0.26$, $z = 0.15$, $p = 0.88$). The main effect of ExtractionType was marginally significant ($\hat{\beta} = -0.22$, $SE = 0.13$, $z = -1.69$, $p = 0.09$), potentially suggesting that speakers made more errors in the object extraction condition. The random effects structure of the final model included by-subject and by-item random intercepts, and by-subject random slope for Prime Type.

Production time analyses. The mean onset latency of the sentence, word-by-word production time, and accuracy rate in each condition is summarized in Table 13. The differences in production time in each region due to the adverse *that*-priming effect are shown in Figure 16.

Table 13
The by-subject mean raw production time for each word in Experiment 3.

ExtractionType	PrimeType	Onset	The	ballerina	who	the	nun	thinks	loves/the	the/artist	accuracy
Object	Null	1360	230	808	293	238	669	560	242	488	84.85
Object	That	1379	210	840	302	215	656	542	233	495	83.43
Subject	Null	1249	221	814	311	230	605	523	468	257	87.50
Subject	That	1347	212	799	297	233	623	540	483	270	85.86

Table 14

The summary of the statistical results in the onset region in Experiment 3.

	Estimate	Std. Error	t value	Pr(> t)
Intercept	1341	53	24.98	< .001
Prime type	60	20	2.87	.004
Extraction type	-58	42	-1.37	.185
Prime type x Extraction type	83	41	2.00	.046

Figure 15. By-subject mean onset latency by condition in Experiment 3. Error bars represent within-subject standard error of the means.

The adverse *that*-priming effect. In the onset region, speakers were on average 98 ms slower to start speaking subject extraction sentences in the *that*-prime condition than in the null prime condition. In comparison, speakers were 18 ms slower to start speaking object extraction sentences in the *that*-prime condition than in the null prime condition. As can be seen in Table 14, the two-way interaction between Prime Type and Extraction Type was significant. Subsequent planned comparisons revealed that there was a significant effect of Prime Type in the subject extraction condition ($\hat{\beta} = -101.72$, $SE = 29.12$, $t = -3.49$, $p = < .001$), and no significant effect in the object extraction condition ($\hat{\beta} = -18.36$, $SE = 29.91$, $t = -0.61$, $p = .539$). The random effects structure of the final model included by-subject and a by-item random intercepts, and by-item random intercept for Extraction Type.

In the before-gap region, speakers were on average 25 ms slower to start speaking subject extraction sentences in the *that*-prime condition than in the null prime condition. In comparison, speakers were 4 ms slower to start speaking object extraction sentences in the *that*-prime condition than in the null prime condition. As can be seen in Table 4, there was a marginally significant effect of Prime Type, but the pairwise comparison revealed no significant effect of Prime Type in the subject extraction condition or in the object extraction condition. Given that this effect in this region was not found in Experiment 1 or 2, this

Figure 16. By-subject mean onset latency by condition in Experiment 3. Error bars represent within-subject standard error of the means.

Table 15

The summary of the statistical results in the before-gap region in Experiment 3.

	Estimate	Std. Error	t value	Pr(> t)
Intercept	1129	38	29.01	< .001
Prime type	11	14	0.76	.446
Extraction type	-35	14	-2.41	.016
Prime type x Extraction type	27	29	0.93	.353

effect is likely to be spurious. The main effect of Extraction Type or the interaction between Prime Type and Extraction Type was not significant. The random effects structure of the final model included by-subject and by-item random intercepts only.

To characterize the time-course of the adverse *that*-priming effect, I again compared the magnitude of the adverse *that*-priming effect between the onset region and the before gap region (see the analysis section above for more details). Like in Experiments 1 and 2, this analysis revealed a significant two-way interaction between Prime Type and Region, ($\hat{\beta} = 78$, $SE = 34$, $t = 2.31$, $p = .021$). Like in Experiment 1 but unlike in Experiment 2, the main effect of Prime Type was highly significant ($\hat{\beta} = 64$, $SE = 18$, $t = 3.53$, $p < .001$). The effect of Region was again significant ($\hat{\beta} = 201$, $SE = 64$, $t = 3.16$, $p = .003$). The random effects structure of the final model included by-subject, by-item and by-trial random intercepts, and by-subject and by-item random slopes for Region.

Discussion

Experiment 3 (re-)tested if the adverse *that*-priming effect is selective to embedded subject relative clause production. The results showed that it is indeed selective to embedded

Figure 17. By-subject mean production time in the before-gap region by condition in Experiment 3. Error bars represent within-subject standard error of the means.

subject relative clause production, as supported by the significant interaction between PrimeType and ExtractionType. Thus, Experiments 1, 2, and 3 all suggest that speakers are slower to start speaking the filler when primed with *that*, and Experiment 1 and 3 both suggest that the adverse *that* priming effect is selectively observed when speakers produce subject extraction sentences. This conclusion holds for both wh-question (Experiment 1) and relative clause production (Experiments 2 and 3).

One unexpected result in Experiment 3 is the simple *that* priming effect in the filler sentences was only marginally significant. This weak *that* priming effect could be due to the low base rate of producing *that*. In Experiment 3, the embedded verb was uniformly *loves*, unlike in Experiment 1 and 2 so it should be easier to plan the embedded clause, both conceptually and linguistically. Given that one of the functions of *that* is to provide a buffer region for planning upcoming materials, it is possible that speakers in Experiment 3 did not have to use *that* as frequently as in Experiments 1 and 2. Ferreira (2003) showed that the *that* priming is to a large extent the priming of null complementizer rather than the priming of *that* complementizer. If speakers produce the null complementizer frequently to begin with, they may not be further primed to produce the null complementizer, just like it is not easy to prime active sentences as opposed to passive sentences (e.g., Bock, 1986; Bock & Loebell, 1990; Hartsuiker & Kolk, 1998), a phenomenon known as the *inverse-preference* effect (Pickering & Ferreira, 2008; Reitter, Keller, & Moore, 2011).

In Experiment 3, speakers showed longer onset latencies in the object relative clause than in the subject relative clause conditions, unlike in Experiment 2. This result is more consistent with Scontras et al. (2015), who showed that the speakers took longer to start speaking object relative clauses than subject relative clauses. This difference between Experiment 2 and Experiment 3 might be due to the difference in the position of the character corresponding to the relative clause head in the pictures. In Experiment 2, patient

entities (i.e., the head noun in the object relative clause condition) appeared both to the left or the right of the agent entities in the picture stimuli. In comparison, in Experiment 3, theme entities always appeared to the right of the experiencer entities. The theme entities were not placed to the left of experiencer entities in Experiment 3 to avoid potentially confusing participants (in Experiment 3, the only visual cue to tell which character corresponds to the experiencer entity was the directionality of the arrow). Given the left-to-right bias in English speakers, it may be easier to speak subject relative clauses than object relative clauses. Thus, it is unclear if the difference in the onset latency between subject and object relative clause production reflects the difference in structure building processes involved in subject vs. object relative clauses. Given the inconsistency in the subject-object relative clause differences between Experiments 2 and 3, it remains unclear if object relative clauses are indeed more difficult to produce.

So far, I have been assuming that the adverse-*that* priming effect is due to the conflict between the *that* priming effect and the *that*-trace constraint. This interpretation is plausible and it naturally explains why the effect is selective to the sentences involving the extraction of embedded subjects. It is possible that the adverse-*that* priming effect is actually due to the *facilitation* effect from priming the phonologically null complementizer. That is, the production of embedded subject extraction may be facilitated by the priming of null complementizer. This interpretation is plausible given the results from Ferreira (2003) that suggests that the null complementizer primes the null complementizer more than *that* primes *that*. This possibility does not undermine the main argument that speakers plan the complementizer structure before starting to speak the filler. However, the effect reported here has not been reported before. Thus, to understand the nature of the effect more precisely and to establish the effect more clearly, I aim to compare the competition and facilitation accounts directly in Experiment 4. Experiment 4 also serves as a conceptual replication of Experiment 1-3.

Experiment 4

Experiments 1-3 all showed an adverse *that*-priming effect. However, it remains unclear if adverse-*that* priming is a slow-down effect caused by *that* priming or a speed-up effect caused by null complementizer priming. As discussed in the introduction, our hypothesis of the effect was that *that* priming causes the slow-down effect. Experiment 4 aims to test this prediction, using a neutral priming condition as the baseline. For prime sentences, I constructed sentences like the following:

1. *The professor thought that the student got an A on the final exam.* [that prime]
2. *The professor thought the student got an A on the final exam.* [null prime]
3. *The professor thought about the student who got an A on the final exam.* [neutral prime]

The *that* prime sentences like (1) and the null prime sentences like (2) are similar to the prime sentences used in previous experiments. Sentences like (3) do not contain a complement clause, so it is expected to prime neither the *that* nor the null complementizer, and thus it can be considered a neutral prime. The neutral primes serve as a baseline for identifying whether the adverse *that* prime effect is a slow-down effect due to the *that* prime,

or a speed-up effect due to the null prime.

Method

Participants. Thirty six undergraduate students at the University of Massachusetts, Amherst participated in Experiment 4.¹ All were reported to be native speakers of English. A written consent was obtained from all participants. None participated in Experiment 1, 2, or 3. Three participants were excluded because their error rates were too high (less than three error-free trials in any condition, following the same criterion as in Experiments 1, 2 and 3), and one participant was excluded because of a technical issue. The data from the remaining thirty-two participants were analyzed.

Materials. Forty-eight picture frames similar to the ones used in Experiment 3 were used for Experiment 4. Unlike in Experiment 3, where the target embedded verb was always *love*, there were three different target embedded verbs in this experiment: *love*, *like* and *be angry at*. I created forty-eight picture frames, and two versions for each picture frame were created: one that elicited matrix subject relative clauses and the other that elicited embedded subject relative clauses. Ninety-six prime sentences like (1)-(3) were created and paired with the target pictures. Those ninety-six picture-sentence pairs were distributed across six experimental lists, according to a Latin square design. In addition, twenty-four pictures that elicited embedded object extraction sentences were created, and they were used for filler trials.

Procedure. Experimental procedures were the same as in Experiment 3, except for the following changes. First, participants in Experiment 4 were instructed to produce wh-questions with extraction from embedded clauses, like in Experiment 1. Second, instead of simply reading aloud the prime sentence, participants recalled the prime sentence aloud, immediately after the prime sentence disappeared from the screen (upon participants' button press). Participants had an indefinite amount of time to recall the prime sentence, before they pressed the space bar for the target picture. The immediate recall component was added to encourage participants to process prime sentences deeply.

Scoring and Analysis. The same scoring criteria as in Experiments 1, 2 and 3 were used. The analysis procedures were mostly similar to those in Experiments 1, 2 and 3, except for the following differences. In Experiment 4, the factor Prime Type was a three-level factor so the coding scheme was according to the question I aimed to answer statistically. In particular, for the analysis on *that* frequency and error frequency, Prime Type was dummy-coded so that the neutral prime condition is compared against the *that* prime condition and against the null prime condition, to test whether *that* and the null complementizer both prime, as suggested by Ferreira (2003). For the analysis of production time, Prime Type was helmert-coded, so that the *that* condition was compared against the average of the neutral and the null conditions, and the neutral condition was compared against the null condition. This coding corresponded to our hypothesis that the adverse *that* priming is a slow-down effect due to *that* priming. Also, in Experiment 4, speakers were on average much (~350ms) faster to speak matrix subject extraction sentences than to speak embedded subject extraction sentences. Thus, the MAD trimming procedure would

¹I originally aimed to recruit 50 participants (according to the sample size in Experiment 1-3), but due to the COVID-2019 pandemic, it was not possible to collect more than 36 participants.

Table 16

The frequency of that production in each condition in Experiment 4.

ExtractionType	PrimeType	Null frequency	That frequency	% that
Embedded Object (filler)	NA	616	134	17.87
Embedded Subject	Neutral	244	6	2.40
Embedded Subject	Null	246	4	1.60
Embedded Subject	That	250	3	1.19
Matrix Subject	Neutral	121	132	52.17
Matrix Subject	Null	133	120	47.43
Matrix Subject	That	102	152	59.84

disproportionately remove the embedded subject extraction sentences, especially in the *that* prime condition given the results of Experiment 1-3. Furthermore, it was not possible to define the same before-gap region in the matrix subject extraction sentences, because the gap in the matrix subject extraction sentences was located right after *who*. For those reasons, the MAD trimming procedure was not applied in this experiment. However, note that I also conducted analyses on log-transformed production time to make sure that a small number of inferential data points were not driving any effect. The significance pattern from the analyses on raw production time and log-transformed production time were identical, so I report only the analyses on raw reaction time below, to be consistent with the reporting style in previous experiments.

Results

Production of *that*. Table 16 summarizes the frequency of *that* production across conditions. Because I did not manipulate Prime Type in the filler sentences (i.e., object extraction sentences) in this experiment, there is no statistical analysis of the effect of *that* priming in the filler sentences. Also, because the production of *that* was too infrequent in the embedded subject extraction conditions just like in Experiments 1-3, I analyzed the matrix subject extraction condition only.

Overall, 1513 of 1536 (98.50%) of the experimental trials were scorable. In the matrix extraction conditions, speakers produced *that* 152 times out of 254 trials (59.84%) in the *that* prime condition, 120 times out of 253 trials (47.43%) in the null prime condition, and 132 times out of 253 trials (52.17%) in the neutral prime condition. A mixed effects analysis using this coding revealed that speakers produced *that* in the *that* prime condition more than in the neutral prime condition ($\hat{\beta} = 0.84$, $SE = 0.29$, $z = 2.87$, $p = .004$), and that speakers produced *that* in the neutral prime condition marginally more than in the null prime condition ($\hat{\beta} = -0.55$, $SE = 0.29$, $z = -1.89$, $p = .059$). The random effect structure of the final model included the by-subject and by-item random intercepts only.

As can be seen, speakers rarely produced *that* in embedded subject conditions. Because there are too few instances of *that* production, I did not conduct any statistical analyses for the embedded subject extraction condition.

Errors. Overall, 1303 trials of 1513 trials (86.12%) were identified as error-free, and thus they were included in the subsequent analyses of onset latency and production time. The analysis of errors using logistic mixed effects regression revealed that there was no significant main effect of PrimeType, ExtractionType, or the interaction between the two (all $ps > .24$). Note that only the intercept-only models converged in this analysis.

Production time analyses. The mean onset latency of the sentence, word-by-word production time, and accuracy rate in each condition is summarized in Table 17. The differences in production time in each region due to the adverse *that*-priming effect are shown in Figure 19.

Table 17

The by-subject mean raw production time for each word in Experiment 4.

ExtractionType	PrimeType	Onset	Who	thinks/does	the	num	loves/think	the/loves	artist/the	accuracy
Matrix	Neutral	1650	260	437	162	578	354	266	406	88.14
Matrix	That	1611	269	437	157	601	342	237	425	85.83
Matrix	Null	1616	256	416	156	585	346	244	415	87.75
Subject	Neutral	1913	238	195	136	604	424	386	293	86.80
Subject	That	2043	249	222	162	627	453	416	287	83.40
Subject	Null	1897	236	206	143	628	431	399	302	87.20

Figure 18. By-subject mean onset latency by condition in Experiment 4. Error bars represent within-subject standard error of the means.

The adverse that-priming effect. In the onset region, speakers were on average 146 ms slower to start speaking embedded subject extraction sentences in the *that*-prime condition than in the null prime condition. In comparison, speakers were 4 ms faster to start speaking matrix subject extraction sentences in the *that*-prime condition than in the null prime condition. Likewise, speakers were on average 130 ms slower to start speaking subject extraction sentences in the *that*-prime condition than in the neutral prime condition, but they were 39 ms faster to start speaking matrix extraction sentences in the *that*-prime condition than in the neutral prime condition. Supporting this pattern, there was a two-way interaction between the first contrast of Prime Type (*that* vs. the average of null and neutral) and Extraction Type, as can be seen in Table 18. Subsequent planned comparisons revealed that, in the embedded subject extraction condition, there were a significant effect of Prime Type comparing the *that* and the null prime conditions ($\hat{\beta} = 150.02$, $SE = 59.18$, $t = 2.54$, $p = .011$) and a significant effect of Prime Type comparing the *that* and the neutral conditions ($\hat{\beta} = 128.30$, $SE = 59.47$, $t = 2.16$, $p = .031$). In comparison, there was no significant difference between the null and the neutral prime conditions in the matrix subject extraction condition. No difference was found in the object extraction conditions (all $ps > .55$). The random effects structure of the final model included by-subject and by-item intercepts only.

Table 18

The summary of the statistical results in the onset region in Experiment 4.

	Estimate	Std. Error	t value	Pr(> t)
Intercept	1789	79	22.41	< .001
Prime type (that vs. neutral + null)	58	36	1.61	.108
Prime type (neutral vs. null)	22	41	0.55	.581
Extraction type	338	34	9.93	< .001
Prime type (that vs. neutral + null) x Extraction type	161	72	2.24	.025
Prime type (neutral vs. null) x Extraction type	-2	82	-0.03	.977

Figure 19. By-subject mean onset latency by condition in Experiment 4. Error bars represent within-subject standard error of the means.

As described above, there is no before-gap region in the matrix subject condition. Thus, in the before-gap region, I analyzed only the embedded subject extraction condition for the analysis examining the potential adverse *that*-priming effect. As such, the statistical model did not include ExtractionType as a fixed or random effect, unlike in Experiment 3. This analysis revealed no significant effect of Prime Type (all $ps > .24$). The random effects structure of the final model included by-subject and by-item random intercepts only.

To characterize the time-course of the adverse *that*-priming effect, I again compared the magnitude of the adverse *that*-priming effect between the onset region and the before gap region (see the analysis section above for more details) using the data from the embedded subject extraction condition only. Like in Experiments 1, 2 and 3, this analysis revealed a significant two-way interaction between the first contrast of Prime Type and Region, ($\hat{\beta} = 122$, $SE = 59$, $t = 2.07$, $p = .039$). Like in Experiment 1 and 3 but unlike in Experiment 2, the effect of the first contrast of Prime Type was significant ($\hat{\beta} = 84$, $SE = 34$, $t = 2.46$, $p = .018$). The effect of Region was again significant ($\hat{\beta} = -8$, $SE = 37$, $t = -0.22$, $p = .831$). No other effects (the second contrast of Prime Type, the interaction between the second contrast of Prime Type and Region) were significant (all $ps > .03$). The random effects structure of the final model included by-subject and by-item random intercepts, by-subject random slope for Region, and by-item random slopes for Region and Prime Type.

Discussion

Experiment 4 tested whether the adverse *that* priming effect is a slow-down effect due to *that* priming or a speed-up effect due to null complementizer priming. The results show that speakers were slower to start speaking the filler given the *that* priming than given the null prime or given the neutral prime. This pattern supports the claim that the adverse *that* priming effect is a slow-down effect arising from the conflict between the *that* priming effect and the *that*-trace constraint. I will come back to the cognitive underpinning of the

Figure 20. By-subject mean production time for each Prime Type condition in the before-gap region in Experiment 4. Error bars represent within-subject standard error of the means.

adverse-*that* priming effect in General Discussion, when I discuss a step-by-step model of how speakers formulate filler-gap dependencies in sentence production.

The timing of matrix subject noun planning

In all four experiments (though not as clearly in Experiment 2), the results suggest that speakers planned the structural details of the filler-gap dependencies before starting to speak, despite the fact that the gap is located far downstream in their utterances. It is unlikely that speakers merely planned entire sentences, because the results from Experiment 1, and to some extent Experiment 2, suggest that speakers planned the gap-hosting verbs on a just-in-time basis. However, gap-hosting verbs linearly follow the gap position, and the verb interference effects in Experiment 2 were not clear. Thus, it remains possible that speakers in the current experiments planned their utterances from beginning to end, and extensively enough to plan the gap structure before utterance onset.

A strong test for this possibility is to examine when speakers plan the sentence-medial words *preceding* the gap position. If speakers plan sentence-medial words preceding the gap on a just-in-time basis, that would suggest that speakers planned the gap structure before linearly preceding words, contradicting the possibility that speakers planned the sentence linearly up to the gap. Here I examine when speakers plan the matrix subject nouns, which linearly precedes the gap position, using an effect known as the *codability* effect. Codability usually refers to the degree of naming agreement across speakers, as defined by the following formula:

$$H = \sum_{i=1}^k p_i \log_2(1/p_i)$$

where p_i is the proportion of the i -th name, and k is the total number of distinct names for a picture. A large H -value means that there is more variability in naming responses,

that is, a picture with higher H-values is less codable. For the ease of exposition, I use the sign-flipped version of H value ($-H$) for the measure of codability. Codability is usually cognitively interpreted as primarily affecting the ease of lemma selection (Griffin, 2001; Lachman, Shaffer, & Hennrikus, 1974; Lee, Brown-Schmidt, & Watson, 2013). Thus, by investigating the time-course of codability effect, it is possible to make inference about when speakers plan words at the lemma level.

Codability is usually calculated based on free single-word naming responses, that is, naming responses in the absence of prescribed names for pictures. However, it is not clear if that is the most suitable approach here. In the current experiments, speakers were pre-trained to name each character using names prescribed to them. Thus, the traditional codability measure may not capture the lemma selection difficulty in experiments with pre-training. In fact, in our separate studies using a subset of the character pictures used in the current experiments, people often used different names for both the *monk* picture and *artist* picture (many of them used names like *priest* for *monk* and *painter* for *artist*). But in the current experiments, speakers almost always named the *monk* picture as *monk* once they were trained to say so. In comparison, many speakers substituted *artist* with *painter*, even after the training. Thus, the adequate measures of codability with and without pre-training are likely to be different. To circumvent this issue, I calculated the codability of each character based on the current experimental data, rather than based on responses from separate norming studies. Because many speakers occasionally substituted names of various characters in the current experiments, despite the fact that they were pre-trained, there was fair amount of variability in naming response across different character pictures. Thus, it was possible to calculate codability of each character while taking into account the effect of pre-training. I used this measure to estimate when speakers experience difficulty retrieving the target word for a character picture after training. I contend that this (post-training) codability measure reflects the difficulty of retrieving the lemma of words in question. It must be noted that this analysis is exploratory, and thus needs to be interpreted cautiously. Nevertheless, if the same pattern can be observed across multiple experiments, we can be confident about the reliability of this approach.

I defined a region corresponding to the total production time of the two words preceding the matrix subject noun (i.e., *does the* in Experiments 1 and 4, and *who the* in Experiments 2 and 3). This decision was based on the assumption that this before-matrix subject noun region roughly corresponds to when speakers retrieve the matrix subject nouns, under the hypothesis that speakers retrieve them on a just-in-time basis. The magnitude of the codability effect in this region was compared to that in the onset region, to test the hypothesis that speakers plan the matrix subject noun predominantly in the before-matrix subject noun region rather than before the speech onset.

For each experimental data set, I built a mixed effects model where previous experimental factors (Prime Type and Experiment Type in all experiments, and Relatedness in Experiments 1 and 2) were included as control predictors, and centered Codability and Region as factors of interest. Because I suspected that the codability effect may differ in different structures, the model also included a two-way interaction between Extraction Type and Codability, as well as a three-way interaction between Extraction Type, Region and

Codability. In each model, because each trial contributed to two observations (one for each region), *trial* was included in the random effect structure, in addition to subject and item. The random effects structure was initially maximal with respect to the effect of interest (i.e., there were by-subject random slopes for Codability, Region and their interaction, as well as by-item random slope for Region), but they were simplified according to the same procedure as in the previous analyses. Because the production time in the before matrix subject noun region was radically different (around a 1500 millisecond difference) in scale from the onset latency, production times were z-score transformed within each region before the analyses. The results show that in all experiments, the codability (-H score) reliably negatively predicted the production time in the before-subject noun region, but not the onset latency. The results are summarized in Figure 21.

Figure 21. Regression plots showing the relationship between the matrix subject noun codability and production time in two regions: the onset region (left panel) and the before-matrix subject noun region (right panel). Each dot represent a distinct target noun.

In Experiment 1, The random effects structure of the final model included by-subject, by-item, and by-trial random intercepts, as well as a by-subject random slope for Region and the interaction between Codability and Region. There was a significant interaction between Codability and Region ($\hat{\beta} = 0.60$, $SE = 0.26$, $t = 2.31$, $p = .025$), with no significant three-way interaction between Codability, Region and Extraction Type ($\hat{\beta} = -0.07$, $SE = 0.44$, $t = -0.16$, $p = .874$). There was also a significant interaction between Extraction and Codability ($\hat{\beta} = -0.63$, $SE = 0.23$, $t = -2.77$, $p = .006$), potentially suggesting that the effect of codability was overall larger in the subject extraction condition. No other effects involving Codability were significant ($ps > .14$).

In Experiment 2, the random effects structure of the final model included by-subject, by-item, by-trial random intercepts and by-subject random intercept for Region. Again, there was a significant interaction between Codability and Region ($\hat{\beta} = 0.89$, $SE = 0.19$, $t = 4.82$, $p < .001$), with no significant three-way interaction between Codability, Region and Extraction Type ($\hat{\beta} = -0.27$, $SE = 0.38$, $t = -0.72$, $p = .474$). The main effect of Codability

was also significant ($\hat{\beta} = -0.56$, $SE = 0.13$, $t = -4.35$, $p < .001$), though I avoid interpreting this effect due to the presence of the significant two-way interaction between Codability and Region. No other effects of involving Codability were significant ($ps > .28$).

In Experiment 3, the random effects structure of the final model included by-subject, by-item, and by-trial random intercepts and a by-subject random intercept for Region. Again, there was a significant interaction between Codability and Region ($\hat{\beta} = 0.61$, $SE = 0.10$, $t = 5.83$, $p < .001$), with no significant three-way interaction between Codability, Region and Extraction Type ($\hat{\beta} = 0.02$, $SE = 0.21$, $t = 0.07$, $p = .942$). The main effect of Codability was also significant ($\hat{\beta} = -0.22$, $SE = 0.10$, $t = -2.30$, $p = .032$), though I avoid interpreting this effect due to the presence of the significant two-way interaction between Codability and Region. No other effects of involving Codability were significant ($ps > .30$).

In Experiment 4, the random effects structure of the final model included by-subject, by-item, and by-trial random intercepts, as well as by-subject random slope for Region, Codability and their interaction, and by-item random slope for Region. Again, there was a significant interaction between Codability and Region ($\hat{\beta} = 0.46$, $SE = 0.18$, $t = 2.54$, $p = .016$), with no significant three-way interaction between Codability, Region and Extraction Type ($\hat{\beta} = -0.29$, $SE = 0.25$, $t = -1.15$, $p = .252$). The main effect of Codability was also significant ($\hat{\beta} = -0.20$, $SE = 0.10$, $t = -2.08$, $p = .044$), though I avoid interpreting this effect due to the presence of the significant two-way interaction between Codability and Region. No other effects of involving Codability were significant ($ps > .30$).

One potential concern is that the onset latency measure may not be sensitive to the effect of matrix subject noun codability, because other processes can take place concurrently with matrix subject noun planning, masking the codability effect. To rule out this possibility, I re-analyzed the data from Experiments 2 and 3 (relative clause experiments), and tested whether the codability of the sentence-initial noun (i.e., the extracted nouns) can predict the onset latency. I limited our analysis to Experiments 2 and 3 because, in Experiment 1 and 3, the sentence initial noun was just *who*. Using the data from Experiment 2 and 3, I built a mixed effects model predicting the onset latency based on the codability of the sentence-initial noun. This model included previous experimental factors (i.e., Prime Type, Extraction Type and Relatedness) as control predictors, and the codability of the initial nouns (defined in the same way as the codability of the matrix subject nouns) as a critical predictor. The model also included the factor Experiment (Experiment 2 vs. 3), and the interaction between Codability and Experiment. The final model included the random intercept for subject and item, and the by-item random-slope for Codability. This analysis suggests that the codability of the extracted noun reliably predicted the onset latency. The main effect of codability was highly significant ($\hat{\beta} = -313$, $SE = 96$, $t = -3.24$, $p = .004$), with no interaction with Experiment ($\hat{\beta} = 55$, $SE = 193$, $t = 0.28$, $p = .780$). This result shows that the onset latency measure is indeed sensitive to the codability effect, when the relevant noun is sentence initial, that is, when the relevant nouns must be in the initial scope of planning. This result is not compatible with the possibility that the onset latency is simply insensitive to the effect of codability.

In sum, across four experiments, the two-way interaction between Codability and Region was consistently observed, with no evidence for the three-way interaction between

Codability, Region and Extraction Type. In all four experiments, the matrix subject noun codability effect was observed in the before matrix subject noun region but not in the onset region. Although the evidence from each experiment should be treated cautiously due to the exploratory nature of the current analysis, this consistent pattern provides consistent evidence that speakers in the current study planned the lemma of matrix subject noun predominantly on a just-in-time basis. This of course does not mean that speakers never plan the lemma of matrix subject noun before speech onset in long-distance wh-question and relative clause production. However, given the time-course of the adverse-*that* priming effect, the current results suggest that speakers predominantly planned the gap structure before speech onset, and predominantly planned the linearly preceding noun after they started speaking. This observation cannot easily be reconciled with the view that speakers simply planned entire utterances before speech onset in the current series of experiments.

General Discussion

The main findings are as follows. In Experiment 1 the adverse *that*-priming effect was observed before the onset of filler production, suggesting that speakers planned, minimally, the grammatical function of the gap and the type of the gap-hosting clause before starting to speak the filler. Experiment 1 also showed that speakers experienced the verb interference effect in the region immediately preceding the verb, suggesting that speakers planned the verbs predominantly on a just in time basis. Experiment 2 showed the analogous effects in relative clause production, though the effect was not as robust as in Experiment 1. Experiment 3 re-investigated relative clause production with a simpler design than Experiment 2, and showed that speakers did experience the adverse *that* priming effect before speech onset in embedded relative clause production. Experiment 4 again tested the production of the embedded wh-questions and showed that the adverse *that* priming effect is an interference effect caused by *that* priming, rather than a facilitation effect caused by null complementizer priming. The adverse *that*-priming effect across four experiments in the onset vs. the before-gap regions is shown in Figure 22. Overall, this pattern of results supports the early commitment hypothesis, which posits that speakers make detailed grammatical decisions about the filler-gap dependency before starting to speak the filler. Furthermore, the timing of verb interference effects in Experiment 1 and to some extent in Experiment 2 suggests that, at least in the embedded subject extraction sentences, speakers planned verbs predominantly on a just-in-time basis. This timing of the verb-interference effect is not easily compatible with the possibility that speakers simply planned entire sentences predominantly before starting to speak. Finally, post-hoc analyses of all experiments assessed the time-course of the matrix subject noun's codability effect. These analyses suggest that speakers plan matrix subject nouns predominantly on a just-in-time basis. This finding is incompatible with the hypothesis that speakers simply planned entire sentences sequentially before speech onset. Taken together, the current study suggests that speakers are capable of planning detailed structural properties of filler-gap dependencies before starting to speak the filler, without necessarily planning words preceding the gap position. This time-course of planning supports the early commitment hypothesis but without contradicting the accepted view that sentence production is incremental (see below for a more detailed discussion on incrementality in production).

Figure 22. The adverse-that priming effect across four experiments (a difference plot).

The time-course of filler-gap dependency formation in production

The current results suggest that speakers plan the grammatical function as well as the type of gap-hosting clause before starting to speak the sentence-initial filler. This *structure-first* sentence planning strategy suggests that speakers are capable of building sentence structures before and separately from words. This conclusion is broadly consistent with Garrett’s early model (Garrett, 1975), some modern models of sentence production that assume the separation between structure and content (Bock & Ferreira, 2013), as well as a specific version of the hierarchically incremental view of sentence production, where the skeleton of syntactic representations are planned before words are selected (note however that not all hierarchically incremental views assume that *syntactic* structures are planned in advance of words). In that sense, the current argument can be viewed as the elaboration of the view that structural representations guide sentence planning. However, importantly, there is one important modification of those views. We, following Momma and Ferreira (2019), claim that speakers can represent non-adjacent representations (the filler and the gap) without necessarily having already planned the linearly intervening words. That is, filler-gap dependencies are formulated initially *locally*, within a single unit of planning, but are later expanded by some structure building operations that increase the distance between the filler and the gap. Previous structure-first views do not explicitly permit this way of building structures (or at least, they do not consider such cases). If our view is correct, speakers may be able to form long-distance dependencies without compromising the *incremental* nature of sentence production as I discuss more in the next section.

Incrementality and filler-gap dependency formation

It may at first seem that the early commitment view is in conflict with the widely accepted view that sentence production is incremental. However, this depends on what it means for sentence production to be incremental. As discussed in Momma and Ferreira

(2019), there are at least three distinct definitions of incrementality in the literature:

1. Speakers can interleave the planning and articulation of a single sentence.
2. The retrieved words and planned structures are immediately integrated with each other into the overall representation of a sentence.
3. The planning and articulation of a particular element of an utterance synchronize as much as possible.

The first notion of incrementality is not controversial and is supported by the current results. The second notion of incrementality is also in line with the current data, as speakers can integrate the filler-gap representations immediately into the developing representation of the sentences. The only notion of incrementality that is incompatible with the current results is the third one. The current results suggest that planning and articulation are systematically desynchronized, and that surface word-order does not transparently reflect the order of planning at structural levels. The current results thus add to the recent series of studies questioning the assumption that the order of planning mirrors the surface order of sentences (Do & Kaiser, 2019; Momma & Ferreira, 2019; Momma et al., 2016, 2018), the view whose roots can be found in Garrett (1975).

Related to the third notion of incrementality is the notion of the ‘scope of planning’ in production (Allum & Wheeldon, 2007; Brown-Schmidt & Konopka, 2008; Brown-Schmidt & Tanenhaus, 2006; Griffin, 2001; Konopka, 2012; Konopka & Meyer, 2014; Meyer, 1996; Schriefers et al., 1998; Van de Velde et al., 2014; Wagner et al., 2010; Wheeldon, Ohlson, Ashby, & Gator, 2013; Wheeldon & Smith, 2003). The scope of planning refers to the extent to which speakers plan multiple parts of sentences simultaneously at a given moment, normally before speech onset. This issue is related to the third notion of incrementality in the sense that, if speakers show a large scope of planning, the degree of synchronization between planning and articulation is decreased. The general take away from the studies examining the scope of planning is that the scope of *lexical* planning is flexible and variable across different tasks (Ferreira & Swets, 2002), recent experiences (Konopka, 2012) and resource availability (Wagner et al., 2010), among potentially numerous other factors. This flexibility claim implies that, when it comes to the lexical (but not necessarily structural) scope of planning, speakers do not have to plan words too far ahead to be able to speak, at least when the words downstream are not critically involved in semantic or syntactic dependencies with the word to be produced earlier (Momma & Ferreira, 2019; Momma et al., 2016, 2018). The current results suggest precisely that: speakers in the current experiments showed no consistent sign that they planned the matrix subject nouns far in advance, despite the fact that they are the first (in wh-questions) and the second (in relative clause sentences) content words in the sentence.

Thus, the current results and interpretations are easily compatible with the incremental view of sentence production, and relatedly with prior work on the scope of planning. The only potential conflict is with the strongest version of the view that the planning and articulation of sentence elements should synchronize as much as possible, which has been questioned on independent grounds (Momma & Ferreira, 2019).

Relationship to previous studies on filler-gap dependency production

It is worth discussing the current results in the context of previous studies investigating filler-gap dependency production. First, Scontras et al. (2015) showed that producing object relative clauses takes more time than subject relative clauses. This finding is generally consistent with the current results. In all experiments, embedded object extraction sentences generally took more time to start producing than embedded subject extraction sentences, though the pattern of statistical significance was inconsistent across experiments. But given the various concerns raised in the Introduction and by MacDonald et al. (2016), I refrain from making any strong interpretations here. Further experiments specifically designed to address this issue are needed to confirm or reject the hypothesis that object relative clauses (or object wh-questions) take more time to produce than subject relative clauses (or subject wh-questions).

Second, Fadlon et al. (2019) found that speakers use various (potentially language-specific) grammatical strategies to minimize the distance between the filler and the gap, which in turn suggests that speakers must keep the filler representation in memory (see also Gennari et al., 2012). This claim is also consistent with the current results and implications. The current results suggest that speakers already commit to the grammatical details of filler-gap dependencies, and presumably this information must be kept in the working memory until the gap is correctly produced. Thus, the current results can be seen as offering a specific hypothesis about the nature of representation kept in working memory during filler-gap dependency production (more explicit description of the nature of representation kept in memory is offered below).

Third, Do and Kaiser (2019) suggest that, when speakers utter object wh-sentences like *which nurses did the maids tickle*, they tend to gaze at event participants corresponding to the subject early (*maids* in this case) in visual-world eye-tracking-while speaking (Griffin & Bock, 2000). This was so despite the fact that the subject is not the first phrase speakers said. Their results suggest that speakers can rapidly identify the grammatical functions of event participants and use that information to guide their eye-movements. The current results and implications are consistent with this finding, in that both Do and Kaiser (2019) and the current results suggest that speakers are able to rapidly identify the grammatical structures of target wh-sentences.

Thus, previous studies investigating filler-gap dependency production I cited above are consistent with the current results and implications.

Speakers' sensitivity to the *that*-trace constraint

The current results also show that speakers are sensitive to the *that*-trace constraint. In all four experiments, speakers produced *that* in the embedded subject extraction sentences only around 1 percent of the time, even though speakers were primed with *that* half the time. This suggests that individual speakers consistently possess knowledge that somehow encodes the *that*-trace constraint, and they use that knowledge consistently. The current data thus support the idea that abstract structural representations that are detailed enough to encode the *that*-trace constraint are causally involved in controlling the sentence production process. In this sense, models of sentence production should incorporate representational theories that

capture constraints like the *that*-trace constraint, if they were meant to cover mechanisms for not just simple utterances but also grammatically complex utterances.

A model of filler-gap dependency formation in production

Finally, I propose a model of structure building in sentence production that aims to capture the current results, and the nature of filler-gap dependency formation processes more generally. Of course, some assumptions in this model necessarily go beyond the current results and previous studies, but given the general lack of models describing long-distance dependency formation processes in sentence production, I believe that it is useful to put forward a proposal that is as explicit as possible.

The main distinct property of the model I propose is the ability to capture the structure-building processes that construct the filler-gap dependencies initially locally (i.e., within single units of representation), which then get ‘expanded’ into long-distance dependencies. The model is able to insert materials in between the head and the tail of a dependency, after the dependency is formulated. This way of building sentence structures captures our hypothesis that speakers construct filler-gap dependencies before planning material in between the filler and the gap. The representational framework I adopt, known as Tree-Adjoining Grammar (henceforth TAG) (Frank, 2004; Joshi, Levy, & Takahashi, 1975; Joshi & Schabes, 1997), is uniquely well-suited to capture this method of structure building, as I see below.

Representational framework

I adopt TAG as a representational basis for our processing theory. TAG is a formal grammatical system that has been successfully adopted in psycholinguistic research, both in comprehension (Demberg, Keller, & Koller, 2013) and production (Ferreira, 2000; Frank, 2004). Thus, the current proposal can be seen as an extension of these previous works, and can also be viewed as the elaborated and extended version of a *hierarchically incremental* view of sentence production (Bock & Ferreira, 2013; Bock, Irwin, Davidson, & Levelt, 2003 among others; Griffin & Bock, 2000; Lee et al., 2013), which encompasses a broad class of sentence production models.

First, the basic building blocks of sentence representations must be defined. Unlike in other syntactic formalisms, such as Combinatorial Categorical Grammar (Steedman & Baldridge, 2011), Government and Binding (Chomsky, 1981), Head-Driven Phrase Structure Grammar (Pollard & Sag, 1994), Lexical Function Grammar (Bresnan, Asudeh, Toivonen, & Wechsler, 2016), or Minimalist Grammar (Chomsky, 1995), in TAG, *elementary trees* are the basic unit of syntactic structure building (but this does not mean that TAG and other formalisms are incompatible with each other, see Frank, 2004). Elementary trees are structured objects that have internal hierarchical organizations. Thus, TAG derivations involve combining already complex objects.

The exact shape of elementary trees depends on constraints on elementary tree formation a particular TAG-based theory imposes. Elementary trees can in principle be defined as having any arbitrary size. But for the theory to be restrictive, there needs to be constraints on how big or small elementary trees can be. In Frank (2004), it was hypothesized

that *all* syntactic dependencies, including long-distance dependencies, are expressible within a single elementary tree (Frank, 2004). Frank (2004) codified this as the fundamental TAG hypothesis:

- *The fundamental TAG hypothesis:* Every syntactic dependency is expressed locally within a single elementary tree.

This hypothesis imposes both the lower and the upper limit on the size of elementary trees. Elementary trees must be large enough to ‘contain’ all relevant syntactic dependencies. At the same time, elementary trees must be no larger than necessary to contain syntactic dependencies, to capture the bounded nature of syntactic dependencies. Thus, according to the fundamental TAG hypothesis, elementary trees can be defined as the minimal phrase that is necessary to contain relevant syntactic dependencies.

Elementary trees can be classified into two types: *initial trees* and *auxiliary trees* (Joshi & Schabes, 1997). Initial trees are elementary trees whose frontier nodes (the bottom nodes) are either terminal (i.e., lexical) or non-terminal (i.e., phrasal). Frontier non-terminal nodes are marked as *substitution* sites, conventionally indicated by a downward arrow. Substitution is one of the combinatorial operations in TAG that can replace a non-terminal frontier node with another elementary tree, whose root node is identical with the frontier non-terminal node being substituted. Figure 23 is an illustration of the derivation of a sentence *the dog chases the cat*, which involves three elementary trees, with two substitution operations.

Auxiliary trees are another type of elementary trees. The root and the foot node of any auxiliary tree must have an identical category. Auxiliary trees combine with another elementary tree via *adjoining*. Adjoining replaces a node with an auxiliary tree. Because auxiliary trees by definition have root and foot nodes belonging to the identical category, adjoining can replace a node of an elementary tree while preserving the structure above and below the adjoining node. As a result, adjoining expands the adjoined tree ‘inside-out,’ allowing the system to generate recursive structures. Figure 24 is an illustration of the derivation of a sentence *the dog often chases the cat*.

These assumptions are independently motivated to explain various phenomena in syntax (Frank, 2004, 2006; Frank, Kulick, & Vijay-Shanker, 2000), but there is one additional representational assumption I adopt, which deviates from Frank (2004). In Frank (2004), each elementary tree is hypothesized to correspond to an *extended projection*, in the sense of Grimshaw (1990). Roughly speaking, an extended projection is a projection of a lexical head plus functional heads associated with that head. For example, Complementizer phrases (CP) and Inflection phrases (IP) are extended projections of verbs, and Determiner phrases (DP) and Prepositional phrases (PP) are extended projections of nouns. However, if a TAG elementary tree is interpreted as defining the domain of planning in production, as I assume here, this assumption leads to the empirically unsupported prediction that speakers routinely plan the verb of a clause before they can start speaking that clause (Ferreira, 2000; see Momma et al., 2016, 2018; Momma & Ferreira, 2019; Schriefers et al., 1998 for the empirical data conflicting this prediction).

To resolve this problem, I import a theory of phrase structure that is widely accepted in the current syntax literature, known as the *vP hypothesis*, whose root can be found in

Figure 23. An example TAG derivation of a simple transitive sentence assuming simple VP structures.

Kratzer (1996, see also 2003; Larson, 1988; Marantz, 1981). The vP hypothesis posits that agents are not actually arguments of verb roots at all, and they do not form any direct syntactic dependency with verb roots. Instead, agent arguments are arguments of *voice* heads, also called little *v* (and hence the name vP hypothesis). Voice head is independent of the verb root and assigns agent role to the undervived subject. As a consequence, the initial elementary tree for an active sentence is headed by a little *v*, not by a verb root. Thus, even under the strong view that a TAG elementary tree defines the default minimal unit of syntactic planning, it is not necessary to predict that speakers need to plan verbs to start speaking the sentence-initial subject. More generally, the benefit of this assumption is that speakers do not have to extensively plan the entire clausal structures to start speaking; to speak the subject, they do not need plan the internal structure of verb phrases. Figure 25 is an illustration of the derivation of a sentence *the dog chases the cat*, adopting the vP hypothesis.²

²Admittedly, there is one problem resulting from adopting the vP hypothesis in the TAG framework. Namely, V-to-I movements and subject-verb agreement are no longer expressed within a single elementary tree. However, I should point out that the TAG theories discussed above face versions of the same problem, to account for head movement across elementary trees and cross-clausal agreement. This is an issue that needs to be evaluated further, not just in the current context but more generally in the context of studying

Figure 24. An example of TAG adjoining.

Finally, critically for the current purpose, how are filler-gap dependencies encoded in elementary trees? Because filler-gap dependencies must be confined to a single elementary tree according to the fundamental TAG hypothesis, the filler and the gap must both be present in a single elementary tree. Crucially, the clause that embeds the elementary tree for a filler-gap dependency is encoded separately from the elementary tree containing the filler and the gap. Embedding of the gap is accomplished via adjoining another elementary tree headed by clause-taking verbs (such as *think*, *believe*, etc). Adjoining expands the initial elementary tree ‘inside out,’ and it is the operation that captures the recursive nature of human language.

In sum, TAG is a grammatical formalism in which elementary trees are combined via two distinct combinatorial operations: substitution and adjoining. TAG encodes every syntactic dependency including filler-gap dependencies strictly locally (within a single elementary tree), but the filler and the gap can be separated indefinitely via adjoining. I combine TAG with the theory of argument structure that separates the agent role from verb roots using voice head. I take this representational framework as the foundation of our

the locality of syntactic phenomena.

Figure 25. An example TAG derivation assuming vP structures.

processing model, which I discuss next.

Real-time derivation of sentences involving embedded filler-gap dependencies

Following Ferreira (2000) and Frank (2004), I propose that the derivational step in TAG transparently reflects real-time structure building in production. Thus, elementary trees define the default unit of syntactic planning in production that can be sent to the phonological encoder, and substitution and adjoining are two combinatorial operations for structure building in production. Thus, structure building in sentence production is viewed as an interleaved chain of elementary tree constructions and compositions between two elementary trees.

How do speakers arrive at the representation of elementary trees? In principle, elementary trees can be stored in and retrieved from long-term memory or built by more elementary combinatorial operations on the fly. Here I assume that elementary trees are stored in and retrieved from content-addressable memory, the memory architecture widely adopted in the psycholinguistic literature (Lewis & Vasishth, 2005; Lewis, Vasishth, & Van Dyke, 2006; Martin & McElree, 2008; McElree, 2000; Reitter et al., 2011; Van Dyke & McElree, 2006). This is partly for the ease of exposition, and it is possible to translate

between memory-based and computational approaches.

When retrieving an elementary tree, I assume that a lexical item that matches with the elements of an elementary tree serves as a retrieval cue for that elementary tree. For example, a noun can be used as a retrieval cue for a DP elementary tree headed by that noun. Also, a non-terminal frontier node in a retrieved elementary can in turn be used as a primary retrieval cue for another elementary tree whose root node matches that non-terminal frontier node. For example, if an elementary tree contains a DP non-terminal node, it can serve as a retrieval cue for a DP elementary tree, potentially in conjunction with other cues. That is, I assume that retrieval of an elementary tree is based on content-addressable memory access using both lexical and phrasal representations as primary retrieval cues.

Once an elementary tree is retrieved, it can be sent to the phonological encoder, as long as the tree does not contain any features that are uninterpretable for the phonological encoder. Speakers do not have to plan the structure of an entire utterance to start speaking that utterance. Indeed, in our model elaborated below, speakers do not have to plan any overt lexical item other than the sentence-initial subject to start speaking a simple transitive sentence, although some non-lexical structures must be planned (for evidence that structural scope of planning may be more extensive than the lexical scope of planning, see Wheeldon et al., 2013). Thus, our model allows incremental production.

Having described the basic architecture of the model, now I discuss the step-by-step structure building process that generates structures involving long-distance filler-gap dependencies, using sentences involving embedded subject extraction as an example. All the assumptions about the shape of elementary trees, except for the introduction of vP, have already been fully justified based on syntactic analyses of various grammatical phenomena in Frank (2004). The goal here is not to provide a comprehensive model of structure building in production. Rather, our aim here is to provide an algorithmic description of structure building that involves the formation of filler-gap dependencies that can be explicit enough to be challenged, implemented, and improved. Note that I use discrete steps for describing the process, but some of the processes may occur in parallel.

Deriving sentences involving long-distance dependencies

Below is the description of how speakers build the structure of an embedded subject extraction sentence under the current model: *Who does the girl think the boy likes?* I chose to use this example because the process of generating embedded subject extraction sentences involves most of the processes I would like to highlight. Note that the similar derivational steps can derive embedded relative clauses.

Step 1. Step 1 is illustrated in Figure 26. Speakers first retrieve two elementary trees, T1 and T2, and combine them via substitution. Critically, the elementary tree T1 spans across the main and the embedded clauses. The layer above C' is a part of the main clause, and the lawyer below C' is a part of the embedded clause. To retrieve T1 and T2, speakers must first assemble a retrieval cue for each elementary tree. Here it is assumed that the lexical head of each elementary tree (the lemma of the null complementizer for T1 and *who* for T2) is a primary retrieval cue. Thus, for T1 retrieval, the accessibility of the null complementizer is a factor that influences the completion time of this step.

The *that* priming effect reduces the relative accessibility of the null complementizer, which consequently increases the time it takes to retrieve T1. This effect is selective to embedded subject extraction sentences, because the elementary tree for embedded object extraction sentences can be headed by either a null complementizer or *that* complementizer. Thus, reducing the accessibility of the null complementizer does not increase the processing time in object extraction questions. Therefore, under the current view, the reduced accessibility of the null complementizer at Step 1 is the source of the adverse *that* priming effect that is observed before speech onset.

The successful retrieval of T1 means that speakers already decided to produce an embedded subject wh-question with the null complementizer for the embedded clause, in accordance with the early commitment hypothesis discussed in previous sections. But because the embedded VP is not yet substituted, the planning of embedded verb (*likes* in this case) is not necessary, in accordance with the results that suggest that speakers predominantly plan gap-hosting verbs on a just-in-time basis in the embedded subject extraction sentences. At this stage, speakers can say *who* if they choose to, to start their utterance.

Figure 26. An illustration of processing step 1.

Figure 28. An illustration of processing step 3.

Speakers can now produce the full sentence *who does the girl think likes the boy*.

The proposed model captures the timing of critical effects I reported in the current experiments (as well as some past findings). Under the proposed model, the adverse *that*-priming effect occurs during Step 1. Given the *that*-prime, retrieving the elementary tree T1 (in Figure 26) is more difficult than when given the null prime. When speakers try to speak an embedded subject wh-questions (or relative clause), only the null complementizer can serve as a cue to retrieve an elementary tree encoding the appropriate filler-gap dependency type. Due to the *that*-trace constraint, the elementary tree that is headed by *that* and encodes embedded subject filler-gap dependencies does not exist (Frank, 2004; Kroch & Joshi, 1985). When speakers were primed for *that*, *that* is more likely to initially act as a retrieval cue for a tree that does not exist. That is, speakers are more likely to make a retrieval attempt that is guaranteed to fail, causing a slow-down.

The structure building process of object extraction sentences is similar to the above-described steps, so I am not going to discuss it in details here. However, the initial tree that contains the filler-gap dependency for object extraction sentences must also contain the embedded verb root, due to the requirement that filler-gap dependencies must be contained

Figure 29. An illustration of processing step 4.

in a single elementary tree, as in Figure 31. For this reason, the model predicts that the root of the embedded verb must be retrieved before the filler, that is, the model predicts that verb interference effects probed in Experiment 1 and 2 should be observed in early measures (onset latency in Experiment 1 and the before-relative pronoun region in Experiment 2). The pattern of verb interference effects in Experiment 1 supported this prediction, though in Experiment 2, the results were not statistically reliable so no strong conclusion is warranted. However, I should note that this prediction is naturally compatible with multiple past studies suggesting that speakers retrieve the root of verbs before starting to speak their underlying objects, but not their subjects (Momma & Ferreira, 2019; Momma et al., 2016, 2018).

The current model might at first seem to predict that speakers should be faster to retrieve the elementary tree with the null complementizer when the matching retrieval cue (null complementizer) is more readily available due to priming. This prediction was not supported by the current data. Speakers in the current experiments used the null complementizer more than 90% of the time in the embedded object extraction sentences and more than 98% of the time in the embedded subject extraction sentences. And yet, there was no evidence that the null complementizer primes facilitated the production of an embedded object or embedded subject extraction sentences. Why was this the case? One

Figure 30. An illustration of processing step 5.

possibility is that an elementary tree is not easy to prime when it is a dominant structure in a given context. Due to the *that*-trace constraint, there is no elementary tree that encodes an embedded subject extraction with *that*. That is, the conditional probability of the null complementizer given the embedded subject extraction is 100%. Also, due to some factor that is still not well understood (I will speculate why speakers produce only rarely *that* in the embedded object extraction conditions), the conditional probability of the null complementizer given embedded object extraction seems to be more than 90%, according to the current data. It has been shown independently that a structure that is dominant in a specific context is hard to prime further (Ferreira, 2003; Ferreira & Bock, 2006). For this reason, the absence of the facilitation effect from the null complementizer priming is not surprising.

Across all four experiments, speakers produced *that* less than 10% of the time in sentences with embedded object extraction, even though saying *that* does not result in ungrammatical utterances. In comparison, the same set of speakers used *that* around 50% of the time in the matrix subject extraction sentences. This pattern is consistent with the naturalistic corpus analysis reported by (Phillips, 2013), so it is unlikely to be just an artifact of the current experimental tasks. Why is this the case? This *that* avoidance

Figure 31. The initial tree for object wh-extraction sentences, headed by a verb extitlikes.

in the presence of an embedded object extraction may be due to some extra-grammatical factor that discourages the use of *that*. Namely, speakers may need to plan embedded verbs before starting to speak the filler (according to the prediction of the current model). When speakers plan embedded verbs in advance, it decreases the uncertainty associated with the embedded clause when speakers reach the potential *that* position. One of the functions of *that* is to create some buffering region for planning the embedded clause, especially when the embedded verb is difficult (Roland, Elman, & Ferreira, 2006). Thus, speakers may be less likely to use *that* in presence of the embedded object extraction, because producing embedded object extraction may encourage or even require speakers to be certain about the embedded verbs before the filler production, well before the potential *that* position. Furthermore, speakers may even want to produce the planned embedded verb as soon as possible, to minimize memory load. This pressure to produce the planned verb as soon as possible may further reduce the chance of *that* use. Those factors may conspire to reduce the use of *that*.

In sum, the current model identifies the source of the adverse *that*-priming effect to be the increased accessibility of *that*, which in turn increases the chance that speakers initially attempts to retrieve an elementary tree that does not exist. Because the retrieval

of the elementary tree for embedded subject extraction sentences is the very first step of the structure building process, the adverse *that* priming effect is expected to be observed early, before the onset of filler production. Moreover, the matrix subject noun and verb are expected to be retrieved later in the embedded subject extraction sentences, after the corresponding elementary trees are substituted in Step 4 and Step 5. Thus, at least in embedded subject extraction sentences, the relative timing of the verb interference effect and the matrix subject codability effect are expected to be late. Note that the current model posits that the minimal lexical scope of planning is small (not exceeding two words at any stage of processing), consistent with the standard assumption of many incremental production models such as Bock and Ferreira (2013), Ferreira and Slevc (2007), and Levelt (1989). In comparison, the scope of structural planning is larger, although, it is still smaller than a full clause (Wheeldon et al., 2013).

I foresee that many of the assumptions that are made explicit here will be proven incorrect, but the goal is to start building explicit models of long-distance dependency production that drive further research on this topic.

Conclusion

I reported four experiments that suggest that speakers plan the detailed structural representation of filler-gap dependencies, specifically the grammatical function of the gap and the type of clause that embeds the gap, before starting to speak the filler and before planning the words surrounding and linearly preceding the gap. I proposed a model of structure building in sentence production, focusing on how speakers establish long-distance dependencies. The core insight of the model is that speakers construct long-distance dependencies initially locally, and material that intervenes between the filler and the gap can be inserted (via adjunction) to make the dependency long-distance. Regardless of whether the model is correct, the current study suggests that sentence planning is guided by abstract grammatical structures that encode subtle constraints like the *that*-trace constraint. Adequate models of sentence production that aim to capture complex structures must incorporate theories of linguistic representations.

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Appendix

Stimuli and target sentences used in the current study (Table A1: Experiments 1 and 2; Table A2: Experiment 3; Table A3: Experiment 4). In Experiment 2, the prime sentences and verb distractors were the same as in Experiment 1. The target sentences in Experiment 2 can be reconstructed based on the target sentences in Experiment 1 by replacing “who” with the appropriate noun phrase, removing “does”, and adding “is red” at the end.

Table A1

Item	Prime sentence	Target sentence	Related distractor	Unrelated distractor
1	What does the shoemaker think the customers will like about the new shoe design?	Who does the monk think is biting the clown?	lick	stalk
2	Who does the student think the professor will reprimand?	Who does the monk think is biting the burglar?	lick	stalk
3	The prime minister thinks the new policy is good for the US economy.	Who does the monk think is biting the clown?	rob	pat
4	Who does the florist think the man will propose to?	Who does the pirate think is catching the sailor?	rob	pat
5	The drummer thinks the leader singer will say something.	Who does the pirate think is catching the sailor?	pull	watch
6	What does the sailor think his wife will do if he were to get injured?	Who does the pirate think the burglar is catching?	pull	watch
7	The shopkeeper thinks the tour guide is informing the tourists.	Who does the doctor think is dragging the clown?	splash	rob
8	What does the reporter think the witness will do?	Who does the doctor think the sailor is dragging?	splash	rob
9	Who does the senator think the president should appoint?	Who does the doctor think is dragging the clown?	watch	poke
10	The boy thinks his girlfriend will prefer to eat out.	Who does the doctor think the sailor is dragging?	watch	poke
11	The flight attendant thinks the captain will announce something.	Who does the waitress think is drying the pirate?	hug	push
12	The girl thinks her friend will invite someone to her birthday party.	Who does the waitress think the doctor is drying?	hug	push
13	The demon thinks the angel gave something to a human.	Who does the waitress think is drying the pirate?	push	tickle
14	The gardener thinks the owner will do something about the ant infestation.	Who does the swimmer think is inspecting the boxer?	push	tickle
15	The guitarist thinks the singer is singing about her ex-husband.	Who does the swimmer think the cowboy is inspecting?	tickle	catch
16	What does the chef think the critic will write about?	Who does the swimmer think is inspecting the boxer?	tickle	catch
17	The detective thinks the culprit was looking for someone who can help him.	Who does the cowboy think is kissing the monk?	kick	carry
18	Who does the boy think his sister is trying to impress?	Who does the cowboy think the boxer is kissing?	kick	carry
19	The apprentice thinks the blacksmith will make him learn how to temper steel.	Who does the cowboy think the boxer is kissing?	kick	carry
20	The concertmaster thinks the conductor wants something.	Who does the cowboy think the boxer is kissing?	catch	dry
21	The dog thinks the cat scratched a boy.	Who does the waitress think is lifting the doctor?	catch	dry
22	The counselor thinks the patient is obsessed with something.	Who does the waitress think the swimmer is lifting?	dry	kick
23	The employee thinks the complaining customer should talk to his boss.	Who does the waitress think the swimmer is lifting?	dry	kick
24	What does the man think the airport staff are trying to announce?	Who does the boxer think is patting the sailor?	chase	bite
25	The monkey thinks the gorilla punched a graffe.	Who does the boxer think the ballerina is patting?	chase	bite
26	What does the wedding planner think the bride wants the wedding invitations to look like?	Who does the boxer think the ballerina is patting?	spray	nudge
27	Who does the writer think the readers will want to die off in her next novel?	Who does the boxer think the ballerina is patting?	spray	nudge
28	What does the father think his son wants to eat?	Who does the chef think is punching the burglar?	drop	punch
29	The girl thinks her friend likes the quarterback.	Who does the chef think is punching the burglar?	drop	punch
30	What does the politician think the scientist discovered?	Who does the chef think is punching the burglar?	stalk	pull
31	The prisoner thinks the guard is looking for someone.	Who does the clown think is splashing the doctor?		
32	What does the actor think the audience will say about his performance?	Who does the clown think is splashing the doctor?		
33	Who does the artist think the model looks like?	Who does the clown think the monk is splashing?		
34	The passenger thinks the crew member will say something.	Who does the clown think is splashing the doctor?		
35	The passenger thinks the pilot is talking about him.	Who does the clown think the monk is splashing?		
36	What does the wife think the husband will order?	Who does the sailor think is stalking the clown?		
37	Who does the zookeeper think the lion attacked?	Who does the sailor think the ballerina is stalking?		
38	What does the child think his parents will buy for him?	Who does the sailor think is stalking the clown?		
39	Who does the detective think the murderer intended to kill?	Who does the pirate think the ballerina is stalking?		
40	What does the farmer think his wife will say about his ripped pants?	Who does the pirate think is washing the chef?		
41	Who does the girl think her classmates will nominate?	Who does the pirate think the monk is washing?		
42	Who does the dancer think the partner will dance with next?	Who does the pirate think the monk is washing?		
43	The carpenter thinks the customer will appreciate the discount.	Who does the pirate think the monk is washing?		
44	What does the officer think the thief will say?	Who does the pirate think the monk is washing?		
45	The novelist thinks readers will like the main character.	Who does the pirate think the monk is washing?		
46	The financial analyst thinks the market needs some change.	Who does the pirate think the monk is washing?		
47	The student thinks the dog is following the owner.	Who does the pirate think the monk is washing?		
48	The teacher thinks the college admissions will reject his student.	Who does the pirate think the monk is washing?		
49	The reporter thinks the convict believes something.	Who does the pirate think the monk is washing?		
50	The secret agent thinks the enemy is contacting someone.	Who does the pirate think the monk is washing?		
51	Who does the swimmer think his friend will marry?	Who does the pirate think the monk is washing?		
52	The speech pathologist thinks her client will have trouble saying something.	Who does the pirate think the monk is washing?		
53	Who does the financial analyst think the company should hire?	Who does the pirate think the monk is washing?		
54	The designer thinks the stylist dislikes the sofa.	Who does the pirate think the monk is washing?		

Table A3 continued

Item	Prime sentence	Target sentence	Related distractor	Unrelated distractor
30	The tourist thinks the police will do something about the pickpocket.	Who does the artist think is chasing the ballerina?	stalk	pull
31	The woman thinks the firemen are rescuing someone.	Who does the artist think the chef is chasing?	carry	sketch
32	What does the lifeguard think the surfer will do if he can't catch the next wave?	Who does the sailor think is dropping the swimmer?	carry	sketch
33	What does the bride think the groom will say?	Who does the sailor think the cowboy is dropping?	kiss	spray
34	What does the customer think the server ignored?	Who does the burglar think is hugging the gymnast?	kiss	spray
35	The author thinks the editor liked her book.	Who does the burglar think is hugging the gymnast?	punch	splash
36	What does the swimming teacher think the student is afraid of?	Who does the burglar think the artist is hugging?	punch	splash
37	Who does the teacher think the student is texting?	Who does the sailor think the gymnast is kicking?	bite	photograph
38	The grandmother thinks her grandson lost his phone.	Who does the sailor think the gymnast is kicking?	bite	photograph
39	The janitor thinks the manager will fire him.	Who does the sailor think the gymnast is kicking?	poke	wash
40	The politician thinks the constituents want to hear about his plan.	Who does the clown think the doctor is nudging?	poke	wash
41	The priest thinks the god loves people.	Who does the clown think the pirate is licking?	sketch	lick
42	What does the archeologist think the professor will analyze?	Who does the clown think the pirate is licking?	sketch	lick
43	Who does the teacher think the headmaster will ask her to tutor?	Who does the doctor think the swimmer is nudging?	drag	chase
44	The substitute teacher thinks the students are whispering about something bad.	Who does the doctor think the swimmer is nudging?	drag	chase
45	What does the nurse think the patient wants to visit?	Who does the doctor think the swimmer is nudging?	lift	kiss
46	What does the baker think the woman will say about his new helmet?	Who does the doctor think the swimmer is nudging?	lift	kiss
47	The child thinks the waiter looks like his mom.	Who does the swimmer think is photographing the monk?	photograph	drop
48	What does the astronomer think the star is made of?	Who does the swimmer think the boxer is photographing?	photograph	drop
49	The boy thinks the firefighter rescued someone.	Who does the swimmer think the boxer is photographing?	wash	hug
50	The actor thinks the audience liked the talk.	Who does the ballerina think is pulling the cowboy?	wash	hug
51	The archaeologist thinks the ancient people respected peacocks.	Who does the ballerina think the waitress is pulling?	pat	lift
52	The judge thinks the jury will decide the case in the next one.	Who does the ballerina think the waitress is pulling?	pat	lift
53	What does the farmer think the goat likes?	Who does the chef think the ballerina is pushing?	inspect	drag
54	The girl thinks her boyfriend is calling his mom.	Who does the chef think the ballerina is pushing?	inspect	drag
55	The therapist thinks the client should eat something.	Who does the chef think the ballerina is pushing?	nudge	inspect
56	The voice actor thinks the villain should sound like a witch.	Who does the chef think the ballerina is pushing?	nudge	inspect
	What does the cashier think the boy stole?	Who does the chef think the ballerina is pushing?		
	Who does the defendant think the prosecutor talked about?	Who does the boxer think is sketching the cowboy?		
	The manager thinks the customer will dislike the service.	Who does the boxer think is sketching the cowboy?		
	Who does the comedian think the audience hated?	Who does the boxer think is sketching the cowboy?		
	The audience thinks the magician will make a dove vanish.	Who does the monk think is spraying the gymnast?		
	Who does the woman think the conman is fooling?	Who does the monk think the pirate is spraying?		
	The thief thinks the resident put something expensive in the safe.	Who does the monk think the pirate is spraying?		
	What does the girl think her friend wants for Christmas?	Who does the ballerina think is tickling the pirate?		
	The illustrator thinks the author will consult a zoologist.	Who does the ballerina think the boxer is tickling?		
	What does the critic think this novel is lacking?	Who does the ballerina think the boxer is tickling?		
	Who does the engineer think his manager blames?	Who does the ballerina think the boxer is tickling?		
	What does the waiter think the customer will complain about?	Who does the ballerina think the boxer is tickling?		
	Who does the woman think the man is contacting?	Who does the artist think is watching the waitress?		
	What does the engineer think his boss will do if they cannot meet the deadline?	Who does the artist think the pirate is watching?		
	Who does the friend think the girl was talking to?	Who does the artist think the pirate is watching?		
	What does the teacher think the student learned?	Who does the artist think the pirate is watching?		
	Who does the tenant think the landlord will rent to next?	Who does the cowboy think is poking the artist?		
	What does the teacher think the child deserves?	Who does the cowboy think the doctor is poking?		
	The technician thinks the singer will fire the guitarist.	Who does the cowboy think the doctor is poking?		
	Who does the chaperone think the child will listen to?	Who does the cowboy think the doctor is poking?		
	The baker thinks the customer will order some cake.			

Table A2

Item	Prime sentence	Target sentence
1	The towel that the woman thinks her son washed is soft.	The artist who the nun thinks the ballerina loves is red.
2	The acorn that the girl thinks the squirrel bit is cracked.	The ballerina who the nun thinks loves the artist is red.
3	The medicine that the scholar thinks the ancient civilians used is effective.	The boxer who the musician thinks the fortune teller loves is red.
4	The table that the child thinks her mom bought is big.	The fortune teller who the musician thinks loves the boxer is red.
5	The piano that the teacher thinks the girl bought is big.	The burglar who the boxer thinks the astronaut loves is red.
6	The student that the teacher thinks the police officer found is tall.	The astronaut who the boxer thinks loves the burglar is red.
7	The algorithm that the professor thinks the computer scientist discovered is complicated.	The doctor who the ballerina thinks the bus driver loves is red.
8	The bird that the boy thinks the girl fed is cute.	The bus driver who the ballerina thinks loves the doctor is red.
9	The river that the scholar thinks the explorer discovered is dirty.	The sailor who the mechanic thinks the businessman loves is red.
10	The wallet that the woman thinks her husband will like is expensive.	The swimmer who the mechanic thinks loves the sailor is red.
11	The software that the customer thinks the engineer developed is useful.	The swimmer who the sailor thinks the custodian loves is red.
12	The chair that the old man thinks the nurse bought is brown.	The custodian who the sailor thinks loves the swimmer is red.
13	The room that the girl thinks that her boyfriend cleaned is still dirty.	The waitress who the clown thinks the detective loves is red.
14	The sandwich that the boy thinks that his mom made is big.	The detective who the clown thinks loves the waitress is red.
15	The poem that the woman thinks that the poet composed is short.	The clown who the astronaut thinks the firefighter loves is red.
16	The document that the government officials think that the president wrote is short.	The firefighter who the astronaut thinks loves the clown is red.
17	The math problem that the student thinks that the teacher wrote is hard.	The judge who the firefighter thinks the fisherman loves is red.
18	The villain that the boy thinks that the hero will defeat is tall.	The fisherman who the firefighter thinks loves the judge is red.
19	The t-shirt the woman thinks that the man always wears is dirty.	The king who the bus driver thinks the magician loves is red.
20	The guitar that the man thinks that the woman bought is well-made.	The magician who the bus driver thinks loves the king is red.
21	The teapot that the merchant thinks that the artisan made is beautifully designed.	The monk who the detective thinks the mechanic loves is red.
22	The orchestra that the man thinks that the musician organized is big.	The mechanic who the detective thinks loves the monk is red.
23	The door that the woman thinks that her husband fixed is old.	The nun who the businessman thinks the musician loves is red.
24	The phone that the bus driver thinks that the passenger left is shiny.	The musician who the businessman thinks loves the nun is red.
25	The laptop that the teacher thinks the student broke is expensive.	The ballerina who the king thinks the artist loves is red.
26	The church that the priest thinks the woman goes to is old.	The artist who the king thinks loves the ballerina is red.
27	The clock that the man thinks his grandpa will like is old.	The fortune teller who the custodian thinks the boxer loves is red.
28	The lamp that the woman thinks her husband bought is tall.	The boxer who the custodian thinks loves the fortune teller is red.
29	The song that the audience thinks the artist composed is long.	The astronaut who the judge thinks the burglar loves is red.
30	The carpet that the student thinks his roommate bought is cheap.	The burglar who the judge thinks loves the astronaut is red.
31	The watch that the man thinks his wife bought is small.	The bus driver who the fisherman thinks the doctor loves is red.
32	The rose that the woman thinks the gardener planted is beautiful.	The doctor who the fisherman thinks loves the bus driver is red.
33	The robot that the boy thinks the scientist invented is intelligent.	The businessman who the swimmer thinks the sailor loves is red.
34	The monster that the knight thinks the mad scientist made is ugly.	The sailor who the artist thinks loves the businessman is red.
35	The book cover that the critic thinks the artist designed is beautiful.	The custodian who the doctor thinks the swimmer loves is red.
36	The book that the librarian thinks the student lost is expensive.	The swimmer who the doctor thinks loves the custodian is red.
37	The letter that the woman thinks that her parents sent is long.	The detective who the painter thinks the waitress loves is red.
38	The salad that the farmer thinks that the chef made is delicious.	The waitress who the artist thinks loves the detective is red.
39	The iPod that the boy thinks that his dad bought is new.	The firefighter who the waitress thinks the clown loves is red.
40	The picture that the tour guide thinks that the bus driver took is beautiful.	The clown who the waitress thinks loves the firefighter is red.
41	The article that the critic thinks that the novelist wrote is well-written.	The fisherman who the fortune teller thinks the judge loves is red.
42	The building that the historians think that the king built is magnificent.	The judge who the fortune teller thinks loves the fisherman is red.
43	The story that the boy thinks that his grandma likes is scary.	The magician who the monk thinks the king loves is red.
44	The teddy bear that the man thinks that the girl likes is old.	The king who the monk thinks loves the magician is red.
45	The bag that the man thinks that his wife will like is expensive.	The mechanic who the magician thinks the monk loves is red.
46	The novel that the novelist thinks that the readers will like is long.	The monk who the magician thinks loves the mechanic is red.
47	The sword that the knight thinks that the blacksmith forged is expensive.	The nun who the burglar thinks loves the magician is red.
48	The jacket that the boy thinks that his dad wears is old.	The nun who the burglar thinks loves the magician is red.

Table A3

Item	Prime sentence	Target sentence
1	The wedding planner thought the bride wanted the wedding invitations to look like movie tickets.	Who thinks the ballerina loves the artist?
2	The writer thought about the readers who wanted the main character to disappear in her next novel.	Who does the nun think loves the artist?
3	The dancer thought the partner danced with the guest.	Who thinks the fortune teller loves the boxer?
4	The carpenter thought the customer appreciated the discount.	Who does the musician think loves the boxer?
5	The reporter thought the convict believed something unrealistic.	Who thinks the astronaut loves the burglar?
6	The senator thought the president appointed a new secretary.	Who does the boxer think loves the burglar?
7	The financial analyst thought the politician announced a new policy.	Who thinks the bus driver loves the doctor?
8	The girl thought her friend invited someone to her birthday party.	Who does the ballerina think loves the doctor?
9	The passenger thought the crew member said something inappropriate.	Who thinks the judge likes the fisherman?
10	The demon thought the angel gave something to a human.	Who does the fortune teller think likes the fisherman?
11	The entrepreneur thought about the sponsors who would support his new project.	Who thinks the custodian loves the boxer the swimmer?
12	The passenger thought the pilot was talking about him.	Who does the sailor think loves the swimmer?
13	The man thought the airport staff was trying to find a missing passenger.	Who thinks the detective loves the waitress?
14	The monkey thought about the zookeeper who fed a giraffe.	Who does the clown think loves the waitress?
15	The politician thought about the constituents who wanted to hear about his plan.	Who thinks the firefighter likes the judge?
16	The prisoner thought the guard was looking for someone.	Who does the clown think likes the judge?
17	The gardener thought about the owner who would do something about the ant infestation.	Who thinks the fisherman loves the judge?
18	The guitarist thought the singer was singing about her ex-husband.	Who does the firefighter think loves the judge?
19	The teacher thought the student learned a lot from her class.	Who thinks the magician loves the king?
20	The thief thought about the resident who put something expensive in the safe.	Who does the bus driver think loves the king?
21	The reporter thought about the witness who testified in the court.	Who thinks the mechanic loves the monk?
22	The programmer thought the customer might challenge his qualifications.	Who does the detective think loves the monk?
23	The shopkeeper thought about the tour guide who was informing the tourists.	Who thinks the musician loves the nun?
24	The secret agent thought his enemy was contacting someone.	Who does the businessman think loves the nun?
25	The boy thought the girl was trying to hide from her sister.	Who thinks the artist loves the ballerina?
26	The politician thought the scientist discovered a new species.	Who does the king think loves the ballerina?
27	The counselor thought the patient was obsessed with something.	Who thinks the boxer loves the fortune teller?
28	The employee thought the complaining customer wanted to talk to his boss.	Who does the custodian think loves the fortune teller?
29	The novelist thought about the reader who liked the main character.	Who thinks the builder likes the sailor?
		Who does the boxer think likes the sailor?
		Who thinks the doctor loves the bus driver?
		Who does the fisherman think loves the bus driver?
		Who thinks the sailor likes the businessman?
		Who does the swimmer think loves the businessman?
		Who thinks the swimmer likes the custodian?
		Who does the doctor think likes the custodian?
		Who thinks the waitress likes the detective?
		Who does the artist think likes the detective?
		Who thinks the clown likes the firefighter?
		Who does the waitress think likes the firefighter?
		Who thinks the businessman loves the sailor?
		Who does the builder think loves the sailor?
		Who thinks the king likes the musician?
		Who does the monk think likes the musician?
		Who thinks the monk likes the mechanic?
		Who does the magician think likes the builder?
		Who thinks the nun likes the musician?
		Who does the burglar think likes the musician?
		Who thinks the astronaut likes the doctor?
		Who does the musician think likes the doctor?
		Who thinks the burglar loves the astronaut?
		Who does the judge think loves the astronaut?
		Who thinks the businessman likes the swimmer?
		Who does the ballerina think likes the swimmer?
		Who thinks the custodian likes the waitress?
		Who does the mechanic think likes the waitress?
		Who thinks the detective likes the clown?
		Who does the sailor think likes the clown?

Table A3 continued

Item	Prime sentence	Target sentence
30	The girl thought about the trainer who loved the dog's owner.	Who thinks the firefighter loves the clown?
31	The boy thought the cheerleader liked the quarterback.	Who does the astronaut think loves the clown?
32	Bill thought about his girlfriend who always preferred to eat out.	Who thinks the fisherman likes the king?
33	The critics think the novelist wrote about the president.	Who does the astronaut think likes the monk?
34	The apprentice thought the blacksmith would make him learn how to temper steel.	Who does the firefighter think likes the monk?
35	The student thought about the teaching assistant who graded his essay.	Who thinks the builder is angry at the nun?
36	The police officer thought the culprit was looking for someone who can help him.	Who does the bus driver think is angry at the nun?
37	The therapist thought the client didn't eat anything this morning.	Who thinks the musician is angry at the ballerina?
38	The manager thought about the customer who disliked the service.	Who does the detective think is angry at the ballerina?
39	The designer thought the stylist wanted the sofa.	Who thinks the artist is angry at the fortune teller?
40	The plaintiff thought the jury will decide the case in the next one.	Who does the businessman think is angry at the fortune teller?
41	The manager thought about the CEO who wanted to hire a computer scientist.	Who thinks the boxer is angry at the astronaut?
42	The actor thought the audience liked the talk.	Who does the king think is angry at the astronaut?
43	The archaeologist thought the ancient people respected peacocks.	Who thinks the burglar is angry at the bus driver?
44	The speech pathologist thought about her client who had trouble speaking complex sentences.	Who does the custodian think is angry at the bus driver?
45	Julie thought her daughter wanted a watch for Christmas.	Who thinks the doctor is angry at the businessman?
46	The grandmother thought her grandson lost his phone.	Who does the judge think is angry at the businessman?
47	The receptionist thought about the manager who fired him.	Who thinks the sailor is angry at the custodian?
48	The detective thought the murderer intended to kill the policeman.	Who does the fisherman think is angry at the custodian?