

Effect of Ambiguity and Lexical Availability on Syntactic and Lexical Production

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Speakers only sometimes include the *that* in sentence complement structures like *The coach knew (that) you missed practice*. Six experiments tested the predictions concerning optional word mention of two general approaches to language production. One approach claims that language production processes choose syntactic structures that ease the task of creating sentences, so that words are spoken opportunistically, as they are selected for production. The second approach claims that a syntactic structure is chosen that is easiest to comprehend, so that optional words like *that* are used to avoid temporarily ambiguous, difficult-to-comprehend sentences. In all experiments, speakers did not consistently include optional words to circumvent a temporary ambiguity, but they did omit optional words (the complementizer *that*) when subsequent material was either repeated (within a sentence) or prompted with a recall cue. The results suggest that speakers choose syntactic structures to permit early mention of available material and not to circumvent disruptive temporary ambiguities. © 2000 Academic Press

With each use of a linguistic expression, two acts are accomplished. First, the creator of the linguistic expression—the speaker—must find the right

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words and order them in accordance with an intended thought. Second, the recipient of the linguistic expression—the listener—must understand those words to recover the original thought that the speaker intended to convey. To be timely, speakers must create their utterances as efficiently as possible. But an utterance is only effective if it is understood at least as rapidly as it is created. Thus, the system that creates linguistic expressions is subject to two simultaneous pressures: It must produce well-formed linguistic expressions as efficiently as possible, but it also must produce utterances that can be easily comprehended.

One way to evaluate the impact of these pressures is to examine the decisions that are made when the language production system builds a sentence and to determine whether those decisions aid the efficiency of production or of subsequent comprehension. One specific decision that the language production system must make is whether to include optional function words in certain sentences: complementizers like *that* in sentence complement structures, as in *The coach knew (that) you missed practice*, and relative complementizers and auxiliary verbs like *who were* in sentences with passive relative clauses, as in *The astronauts (who were) selected for the mission made history*. We present six experiments that test the degree to which such optional word mention is influenced by two separate mechanisms, one of which leads production to operate more efficiently, while the other leads production to create utterances that are more easily understood.

SYNTACTIC FLEXIBILITY AND OPTIONAL WORD MENTION

A language's *syntax* is a description of the allowable configurations of words in that language in terms of categories like *noun*, *verb*, and so forth. Languages offer some flexibility within their syntactic systems, so that a particular idea can be communicated with distinct configurations of words. In English, such flexibility commonly occurs with the alternations that occur with the active (*The tortoise defeated the hare*) versus the passive (*The hare was defeated by the tortoise*) form of a sentence. However, a more subtle form of flexibility occurs with *optional word mention*, where a speaker can grammatically include or omit certain function words, as in sentence complement structures like *The coach knew (that) you missed practice* mentioned above.

From the perspective of the information processing system that underlies language, such flexibility is a valuable resource that can be exploited to achieve different goals. We mention three of those goals, and our experiments address two of them.

One possibility is that syntactic flexibility is used to communicate subtle nuances of meaning, so that actives and passives are meaningfully different ("Mistakes were made"), as are sentence complement structures with or without the *that* (see Thompson & Mulac, 1991). Most language users have

the intuition that the syntactic variation that comes with syntactic flexibility primarily caters to such communicative needs, and research has shown that syntactic alternatives are not fully interchangeable (McKoon & Ratcliff, 1997) and communicate subtle differences in meaning (e.g., Thompson & Mulac, 1991, argue that omission of *that* involves weakening the distinctions between the main and embedded clauses, so that high epistemicity of the main clause or topicality of the complement clause lead to *that* being dropped).

However, psycholinguistic research has revealed that syntactic flexibility can also be exploited to address processing-related challenges faced by language users. Next, we describe two ways that syntactic decisions can alleviate processing difficulties that arise during language use. With the first strategy, the production system uses syntactic flexibility to more easily create fluent utterances. With the second strategy, the production system uses syntactic flexibility to present utterances that are easier for a potential listener to comprehend. These two processing functions of flexibility—easing the burden of the speaker and that of the listener—are not mutually exclusive; both could be at work, even at the same time. However, as we show, there are circumstances under which the two approaches make different predictions and hence the experiments presented ahead allow for an investigation not only of whether the strategies apply but also of their relative influence in affecting speaker choices.

AVAILABILITY-BASED SENTENCE PRODUCTION

Most models of language production assume that the information-processing heart of sentence production occurs with *grammatical encoding*. Grammatical encoding begins with a *message* — the representation of the concepts and their interrelations that a speaker wishes to express—and ends with the selection of *word forms*—representations of the phonological content of the words of a sentence. Between these two stages, the production system accesses *lemmas*: representations of the syntactic properties of the to-be-produced words.

The stage of lemma access is important for many reasons, one of which is that sentence production is often characterized as *lemma driven* (e.g., Levelt, 1989; but see Garrett, 1975). That is, lemmas are taken to encode the information that is used to construct the syntactic structure of a sentence. (This emphasis on the role of lexically specific information has a long history in linguistics, as in Bresnan, 1978, 1982, and is now becoming important in psycholinguistic theory as well; MacDonald, Pearlmutter, & Seidenberg, 1994). Lemma-driven production has desirable properties, especially that it supports *incremental* production (Ferreira, 1996; Levelt, 1989)—the construction of a sentence piecemeal, from beginning to end. This approach permits selected words to be produced in compatible sentences, so that “wh-

words'' such as *what* and *who* can trigger the use of an interrogative structure or so particular verbs can call on appropriate intransitive, transitive, or ditransitive structures.

However, not only do the syntactic privileges of the to-be-produced lemmas affect syntactic structure, but so too can the *timing* of lemma selection have important effects on the syntactic structure of a sentence. This point can be illustrated with passive versus active production. Assume a speaker wishes to describe the outcome of the race between the tortoise and the hare in Aesop's fable with a verb such as *defeat*. Furthermore, assume that the lemma for the word *hare* is quickly activated and selected. Given the early selection of the *hare* lemma, the most efficient strategy is for the speaker to produce the passive, *The hare was defeated by the tortoise*, rather than the active, *The tortoise defeated the hare*, since only with the passive can the already-selected *hare* lemma be immediately mentioned. If the speaker produces the active, then one of two inefficient processing strategies must be adopted: Either the already-selected *hare* lemma must remain active in a buffered state until the sentence-final position arrives for production (while other words are selected and produced in earlier sentence positions), or the already-selected *hare* lemma must be deactivated and subsequently reactivated. From this, a general principle can be induced: Production proceeds more efficiently if syntactic structures are used that permit quickly selected lemmas to be mentioned as soon as possible. We call this *the principle of immediate mention*.

The principle of immediate mention makes a straightforward prediction for sentence complement structures with optional complementizers, like *The coach knew (that) you missed practice*. Assume a speaker has already selected the lemmas for *coach* and *know*, so that the next word in the sentence will be the complementizer *that* or the embedded subject *you*. If the *you* lemma becomes available quickly, then according to the principle of immediate mention, a sentence complement structure without a *that* should be used, since only such a structure permits immediate mention of *you*. If the *you* lemma becomes available more slowly, then a sentence complement structure with a *that* can be used, perhaps to maintain the impression of fluency despite the relatively greater difficulty (i.e., the *that* operates as a grammatical 'um'). More generally, if the embedded subject of a sentence complement is selected quickly, then a *that*-less sentence complement structure should be used to accommodate immediate mention of that quickly selected embedded subject.

In general, such *availability-based effects* on sentence production link the availability of the to-be-produced lemmas to the processes responsible for selecting the sentence structures to be used. The operation of availability-based effects has been demonstrated in a wide range of structures involving order-of-mention effects (e.g., the choice between using an active or passive; see Bock, 1986a, 1987 for especially relevant demonstrations, and see Bock,

1982, for review of earlier work). However, the operation of availability-based effects on sentence choices involving optional-word mention, like the choice of using or omitting *that* in sentence complement structures, has not been demonstrated. Experiments 4, 5, and 6 here provide such tests.

AMBIGUITY-SENSITIVE SENTENCE PRODUCTION

Syntactic flexibility can be exploited not only to make production processing proceed more efficiently, but also to make a potential listener's comprehension processing proceed more efficiently. Given a choice among sentence alternatives, the most straightforward way to increase comprehension efficiency is to avoid sentences that are more difficult to comprehend. Here, we describe a kind of difficulty that arises especially with the sentence complement structures and passive relative clause structures under analysis here.

The notion of *garden path* is well studied in the psycholinguistic literature examining sentence comprehension. Garden paths occur when sentences (a) contain temporary syntactic ambiguities and (b) are biased at the point of temporary ambiguity toward a syntactic analysis that is eventually inappropriate. These points are described in turn.

A sentence contains a temporary syntactic ambiguity when it momentarily permits more than one syntactic interpretation. If a comprehender gets part of a sentence like *The coach knew you*. . . , two syntactic analyses are possible: The *you* can be taken as what is known by the coach (a *direct object* interpretation) or as part of an upcoming statement of what the coach knows (an *embedded subject* interpretation). If the sentence ends with *The coach knew you well*, the direct object interpretation is correct; if it ends with *The coach knew you missed practice*, the embedded subject interpretation is correct. Similarly, a fragment like *The astronauts selected*. . . is temporarily ambiguous because *selected* can either be analyzed as the main verb of a simple sentence (*The astronauts selected their spacesuits*) or as a passive verb in a relative clause modifying the subject noun (*The astronauts selected for the Apollo missions made history*, where *made* is the main verb).

By itself, a temporary ambiguity is not sufficient to cause notable difficulties in comprehension (indeed, every sentence contains an indefinite number of temporary ambiguities as it unfolds). Difficulties specifically occur when biases lead comprehension processes to commit to a syntactic analysis for the temporarily ambiguous fragment that is incompatible with the analysis that the entire sentence will eventually require. For example, given *The coach knew you* . . . , comprehension processes are unable to determine whether *you* is a direct object or an embedded subject. However, processing biases (which can include syntactic simplicity, Frazier & Fodor, 1978, and frequency of occurrence, MacDonald et al., 1994), cause compre-

henders to take the postverbal noun phrase in such structures to be a direct object; here, they take *you* to be the direct object of the verb *know*. However, by definition, sentence complement structures continue with *you* as an embedded subject, as in *The coach knew you missed practice*. Thus, when a comprehender receives the second verb (*missed*), the misanalysis is discovered (i.e., *you* must be an embedded subject) and the initial direct object interpretation is discarded in favor of the correct embedded subject interpretation. Similarly, for passive relative clause structures, the verb *selected* in *The astronauts selected. . .* is dominantly interpreted as a main verb rather than a passive verb in a relative clause modifying the subject noun. However, in a passive relative clause sentence like *The astronauts selected for the Apollo missions made history*, the main verb analysis of *selected* is incorrect; as a comprehender receives *for the Apollo missions* and the main verb *made*, the error in the analysis of *selected* is revealed, and the initial analysis must be revised.

Thus, when sentences with sentence complements or passive relative clauses are produced in their *reduced* form (without the optional function words), they may constitute garden path sentences. However, if the same sentences are produced in *full* form (with the optional function words), the garden paths can be avoided. So, in the fragment of the sentence complement *The coach knew that you . . .*, the noun phrase *you* is nearly impossible to analyze as a direct object, since after the complementizer *that*, a direct object noun phrase is rare. Similarly, in the fragment of the relative clause sentence *The astronauts who were selected . . .*, the verb *selected* cannot be interpreted as a main verb. This implies that syntactic flexibility can be exploited by production to increase comprehension efficiency in a straightforward way: If the reduced form of a sentence includes a garden path, then produce that sentence in its full form. Any such tendency would have the effect of reducing the number of ambiguous sentences seen in spoken language, though it is unlikely that any such pressure would be so powerful as to eliminate temporary ambiguities completely.

One study (Elsness, 1984) examined whether there is any tendency in natural spoken and written text (the Brown corpus) for writers to produce the full forms of sentences that include garden paths in their reduced forms.¹ The study did not find compelling evidence for such a tendency, despite the fact that it has been shown that readers read the full forms of sentences more easily than their reduced forms (Rayner & Frazier, 1987). However, it is possible that a strong tendency to avoid ambiguity was not discovered for two reasons: First, the specific sentence choices made during writing may respond to a variety of demands which may have little to do with temporary

¹ The study included contrasts similar to those in Experiments 1 and 2, investigating whether complementizers are omitted more often when the subsequent embedded subject is unambiguously case marked.

ambiguity (an analogous concern may hold for a similar study of naturally spoken sentences—Craig, Nicol, & Barss, 1995). The present experiments, by contrasting ambiguous and unambiguous sentences under controlled circumstances, may be more sensitive to any effect. Second, writing is likely to be a more deliberative process than speaking, so that if a tendency to avoid ambiguity is implicit, it may be more apparent in a spoken task. We discuss possible ambiguity-avoidance mechanisms in light of the experimental outcomes in the General Discussion.

Experiments 1–3 test the ambiguity-avoidance claim, while Experiments 4–6 test both the ambiguity-avoidance and availability-based claims. All experiments use variants of a *sentence recall task* to test these issues, which is described next.

TASK AND TASK MODEL

In the sentence recall task, speakers received critical sentences embedded among one or two filler sentences and then recalled each sentence based on a small number of cue words taken from each sentence. Use of the sentence recall task permits assessment of production preferences under well controlled circumstances, since many features of speakers' productions can be constrained. However, by removing the speaker from a conversational setting and using the cued-recall situation to plant a to-be-expressed message in the speaker's mind, the task compromises naturalness in pursuit of that experimental control. Nevertheless, based on existing research, a task model of the sentence recall task that implicates natural sentence production processes can be specified.

It is well known that memory for the syntactic structure of a sentence is quite poor, relative to memory for the meaning of the sentence (e.g., Bates, Masling, & Kintsch, 1978; Sachs, 1967). Nevertheless, memory for entire sentences is often quite accurate, at least for short retention intervals. These two claims can be reconciled by considering the results of experiments by Potter and Lombardi (1990, 1998; Lombardi & Potter, 1992). Speakers were given sentences to remember, like *The rich widow is going to give the university a million dollars*. Then, as a purported distraction task, speakers were given individual words to name. Sometimes, a named word like *donate* was (roughly) synonymous with the verb of the sentence (*give*), leading it to sometimes replace its counterpart when the speaker tried to recall the original sentence (but only when that synonymous word was consistent with the overall meaning of the sentence; Potter & Lombardi, 1990). Furthermore, if the replaced word did not permit the same structure to be used (i.e., one cannot say *The rich widow is going to donate the university a million dollars*), speakers modified the syntax of the sentence to permit the replacement. Thus, speakers replaced *give* with *donate*, modified the syntax, and said *The rich widow is going to donate a million dollars to the university*, even though the

syntactic change required the sentence to depart further from the originally encoded sentence (Lombardi & Potter, 1992). Thus, the mostly accurate recall of sentence structure may be linked to memory for the sentence's meaning and memory for words in the sentence, particularly words such as verbs that trigger the production of particular structures. An additional factor that may contribute to recall of structure is syntactic priming (e.g., Bock, 1986b; Potter & Lombardi, 1998), a tendency for speakers to implicitly reuse the structure of a previously produced or heard sentence.

Based on these findings, a task model emerges for the sentence recall task: First, the sentence is stored in a conceptual format based largely on its propositional content (e.g., Ratcliff & McKoon, 1978). Individual lexical items and processes responsible for the creation of specific syntactic structures may remain activated because of their recent use, but are not necessarily linked to the memory trace for the conceptual content of the sentence. At the time of recall, contextual cues, plus experimenter-presented cues (like the prompt words used here) make contact with the stored representation of the sentence meaning (e.g., as in Gillund & Shiffrin, 1984; or McClelland, 1981), and through this retrieval process, a conceptual message is made available to sentence production processes. The sentence production processes then take over: Entities and actions within the message are mapped onto content words, and relations among those entities and actions are mapped onto grammatical relations. As in natural production, these mapping processes are subject to priming, so that recently used words and syntactic structures are likely to be used again. Given this task model, performance in natural production differs from the sentence recall task largely in that for the sentence recall task, the content of the message to be expressed is based on memory of the meaning of the originally encoded sentence, whereas in natural production, the content of the message is determined by the speaker's communicative goals.

The validity of this task model for the present issues can be verified by determining how much the inclusion or omission of the relevant optional function words is determined by memory for the encoded sentence rather than by other production factors. Note that these optional words are function words—short words like *that* which carry little meaning and instead communicate information about the structure of a produced sentence. If speakers recall sentences by regenerating the sentence from its meaning, then memory for the *that* is unlikely to be instrumental, since the semantic weakness of *that* renders it unlikely to be part of a meaning representation in the first place. If so, *memory accuracy*—including the *that* when it was present in the encoded sentence and omitting it when not—should be poor. The experiments will show that memory accuracy was quite low (usually around 65%, where chance is 50%), suggesting that optional word mention in the sentence recall task can be sensitive to production factors, such as the availability-based and ambiguity-avoidance strategies tested here.

EXPERIMENTS 1 AND 2

To assess whether syntactic flexibility is exploited in an ambiguity-sensitive manner, speakers must produce sentences that are either ambiguous or unambiguous in their reduced form. Evidence for sensitivity to ambiguity would come if speakers include more *thats* in sentences that would otherwise be ambiguous, compared to sentences that would not.

As described above, a reduced sentence complement with *you* as its embedded subject is temporarily ambiguous because the syntactic role of the noun phrase *you* is underspecified. One reason the ambiguity arises is because the particular noun phrase used (*you*) is compatible both with a direct object and an embedded subject interpretation. Other pronouns, however, like *I*, *she*, and *he* (among others) occur in complementary distribution with respect to subject versus object roles (e.g., *I* vs. *me*); thus, reduced sentence complements with these pronouns as their embedded subjects are not temporarily ambiguous and cause less disruption during comprehension (Traxler & Pickering, 1996; Trueswell, Tanenhaus, & Kello, 1993). So, a fragment like *The coach knew I . . .* alleviates or eliminates the garden path because the pronoun *I* is unambiguously case marked as a subject pronoun, whereas the pronoun *you* does not have such unambiguous case marking. For present purposes, we call pronouns like *I*, *she*, and *he* *unambiguous* pronouns and pronouns like *you* *ambiguous* pronouns.

Experiments 1 and 2 contrasted the production of sentence complement structures with unambiguous and ambiguous pronouns (see Nicol, Barss, & Craig, 1996, for a similar contrast). Both experiments used *you* as the ambiguous pronoun, but differed in the unambiguous pronouns that were used: Experiment 1 used *I*, whereas Experiment 2 used *she* and *he*. The prediction of both experiments is the same: If speakers exploit syntactic flexibility to avoid ambiguity, then speakers should include more *thats* with sentences that contain ambiguous pronoun embedded subjects (*you*), compared to sentences that contain unambiguous pronoun embedded subjects (*I*, *she*, and *he*).

Experiment 1

Method

Speakers. In all experiments, speakers were college-aged members of the University of Illinois community who participated for class credit or cash payment. All reported that they were native speakers of English. The number of speakers in each experiment is shown in Table 1 below. In all experiments, speakers were excluded if at least 50% of their critical utterances were not scorable (defined below); the number excluded is reported in Table 1.

Materials. Sentence complement structures were created based on the set of 100 sentence complement-taking verbs normed by Garnsey, Lotocky, and McConkie (1992). Of the 100 verbs, 50 were eliminated because they were

TABLE I
Results for Critical Conditions in All Experiments

E	Ss	Is	Condition examples	Misc (%)	Mem Acc (%)	Scored (%)	Full sentences produced		
							Encoded reduced	Encoded full	M
1	96(0)	48	The coach knew (that) I missed practice. <i>The coach knew (that) you missed practice.</i>	3.3 2.6	63.2 63.2	78.2 67.6	61.2 63.4	87.2 89.6	74.3 76.6
2	48(0)	48	The coach knew (that) she/he missed practice. <i>The coach knew (that) you missed practice.</i>	5.3 6.3	66.8 67.1	71.4 77.6	51.2 53.2	84.2 86.9	68.0 70.2
3	48(1)	24	The astronauts (who were) chosen . . . made history. <i>The astronauts (who were) selected . . . made history.</i>	0.5 0.5	66.7 63.0	90.6 90.1	12.3 15.3	46.0 41.9	29.2 28.9
4	48(2)	48	I knew (that) I had booked a flight for tomorrow. You knew (that) I had booked a flight for tomorrow. <i>I knew (that) you had booked a flight for tomorrow.</i> You knew (that) you had booked a flight for tomorrow.	1.2 2.4 1.9 1.7	67.1 65.1 68.8 70.4	86.1 83.0 86.1 82.8	49.0 58.9 53.8 42.5	82.6 88.4 91.2 83.5	66.1 73.8 72.6 62.9
5	32(5)	32	You suspected (that) I felt uncomfortable. <i>I suspected (that) you felt uncomfortable.</i> You suspected (that) I felt uncomfortable. <i>I suspected (that) you felt uncomfortable.</i>	2.0 0.4 2.0 1.6	79.1 82.6 80.3 80.8	73.0 69.5 71.5 75.6	33.7 29.2 26.3 20.6	93.3 94.4 87.5 82.3	62.0 61.8 55.7 51.3
6	24(0)	48	Addressee Absent I knew (that) I had . . . You knew (that) I had . . . <i>I knew (that) you had . . .</i> You knew (that) you had . . .	— — — —	61.1 60.3 61.1 70.2	81.3 83.0 83.0 79.2	57.3 62.7 62.8 42.7	79.5 85.8 85.6 82.2	68.4 73.6 74.1 63.2
	24,24	(0)	Addressee Present I knew (that) I had . . . You knew (that) I had . . . <i>I knew (that) you had . . .</i> You knew (that) you had . . .	— — — —	64.9 64.2 63.3 64.8	83.0 75.7 75.7 78.8	56.4 70.1 67.0 59.5	85.2 97.3 95.3 87.9	71.1 83.9 80.7 74.0

Note. E reports experiment number; Ss reports number of speakers, with 24 additional participants as listeners in Experiment 6 (number of excluded speakers in parentheses); Is reports number of items. Misc reports percentage of trials misencoded. Mem Acc reports memory accuracy for the full or reduced form (compared to encoding) for the condition. For Experiment 5, underlined material in Condition corresponds to the recall cue material. Temporarily ambiguous sentences indicated with italics.

biased too strongly toward continuing with a direct object (so that sentence complements sounded odd) or too strongly toward continuing with a sentence complement (which might undermine the ambiguity). Verbs that accepted either structure in less than 10% of the normed cases were not used. Sentence frames were made with the 50 remaining verbs, and the two sentences that intuitively sounded most unnatural were eliminated, leaving 48 sentences. Based on the Garnsey et al. (1992) norms, the used verbs were associated with sentence complement structures an average of 43.5% of the time, with a standard deviation of 21.6% and a range of 12.5 to 85.6%.

All sentence frames began with an 'NP-V-(that)-NP' structure, as in *The coach knew (that) you missed practice*. Sentences were designed to be sensible with both the unambiguous pronoun (*I*) and the ambiguous pronoun (*you*). All sentences are reported in the Appendix.

Procedure. In the sentence recall paradigm, triads of sentences were created by combining one critical sentence with two filler sentences. Fillers represented various syntactic structures, sometimes including pronouns. (Of the 96 filler sentences, 13 included sentence complement-type verbs, of which 3 were used with direct objects, 3 were used with sentence complements, while the others used different postverbal constituents.) At encoding, speakers were visually presented with each sentence individually, left justified on successive lines on a computer screen, in standard sentence case. Each sentence remained for 6 s. The critical sentence was always presented second. Speakers were asked to read each sentence aloud (at least) once (to ensure proper encoding). Half of the critical sentences were presented to speakers with the *that* and half without, counterbalanced across speakers and sentences.

After all three sentences were presented, speakers recalled each sentence individually with a visually presented cue consisting of two to four words from each sentence. The cue was centered horizontally and vertically on the screen, entirely in lowercase letters (except the personal pronoun *I*, which appeared only in cues for fillers). Speakers were asked to recall aloud the entire sentence that included the cue words, or to remember as much of the sentence as they could. Speakers were then prompted for the other sentences, until they recalled all encoded sentences.

The order of cued sentences at recall was determined quasirandomly on each trial, so that the critical cue words could occur first, second, or third (speakers saw an equal number of critical trials in the three recall positions, and every sentence appeared an equal number of times in all three recall positions, though, due to a programming error, this factor was not counterbalanced across the other experimental conditions).² For the critical sentences,

² The first 48 speakers saw only unambiguous pronoun trials in the second recall position and only ambiguous pronoun trials in the third recall position. To balance the experiment, the last 48 speakers saw the opposite confounding. An analysis by speakers revealed that this variable (first 48 vs. last 48 speakers) did not significantly affect the dependent measure, nor did it interact with any other variables (all $ps > .2$).

the cue words were always the main subject noun (without the determiner) and the main verb, so that for *The coach knew (that) you missed practice*, speakers received the cue words *coach knew*.

Speakers spoke into a microphone that was connected to both a cassette recorder and a voice key. So that speakers would not attempt to recall the sentence during articulation (which might affect optional word mention performance), dysfluent or slow production was discouraged with a production deadline: At voice onset, the cue words disappeared and were immediately replaced by a white bar 20 characters wide. The white bar turned red, character by character from left to right, simulating a progress bar, taking 3000 ms to fill completely. When the bar filled completely, a 250-Hz tone sounded for 250 ms. Speakers were asked to complete their sentences before the tone sounded, though performance was not scored relative to the deadline.

Speakers began each trial by pressing the space bar. After a 500-ms blank screen, each sentence was presented for study for 6 s. After each sentence, a blank screen was presented for 2 s. The cue words appeared immediately after the third and final sentence's 2-s delay and were replaced by the progress bar at voice onset. After the deadline tone, a blank screen was shown for 1000 ms, followed by the next recall cue, until all three sentences were tested.

The experiment consisted of the 48 critical trials (each consisting of three sentences), plus one dummy trial at the beginning and one at the end of the main experimental block. Five practice trials were given to speakers in a separate block at the beginning of the experiment. The entire experiment lasted 45–55 min.

Speakers were tested individually in a quiet room. The experiment was conducted on a Dell 386 microcomputer with a 13-inch SVGA monitor. The MEL software package implemented the experimental program, and voice onsets were detected with the MEL button box. The experiment was recorded on a Marantz PMD-201 cassette recorder for transcription. During the experiment, the experimenter sat next to the speaker to monitor performance.

Scoring. All recorded utterances in Experiments 1 and 2 were transcribed and coded. In all experiments, trials in which the speaker did not read the sentence exactly correctly during encoding were eliminated; the percentage of misencoded trials is reported in Table 1. For Experiments 1, 2, 4, and 6, recalled productions were analyzed provided that the speaker produced a sentence complement structure and accurately recalled the main subject, main verb (both of which speakers were highly accurate with, since they were identical to the recall cue), and embedded subject of the presented sentence (e.g., *The coach knew (that) you . . .*); any material the speaker said after the embedded subject was not evaluated. The percentage of scorable utterances is reported in the "scored" column in Table 1. Every sentence was coded as including a *that* or not.

Design. The main factor of interest is *embedded subject ambiguity*, as either unambiguous (*I*) or ambiguous (*you*). In all experiments, whether the originally encoded sentence was full or reduced was also counterbalanced

across speakers and items. Both factors varied within speakers and within sentences in a repeated measures design. The dependent variable in all experiments was the percentage of all scorable sentences in a particular condition that were produced as full sentences.

Results

In all experiments, the significance of the effects was assessed with repeated measures analyses of variance (ANOVAs), with both speakers ($F1$) and items ($F2$) as random factors. All effects are reported as significant when at or below the .05 level, unless otherwise noted, and 95% confidence interval halfwidths are reported. All reported means are calculated from raw scores, which can be different from means calculated from speaker or item means (because of missing values), so that a reported speaker or item difference can sometimes achieve significance even if the raw mean difference falls short of the corresponding confidence interval; in these cases, the corresponding mean calculated across speakers or items is reported.

The results of all experiments are shown in Table 1. Looking first at the results of Experiment 1, collapsing across whether a *that* was in the original utterance to be encoded, speakers mentioned *thats* 74.3% of the time with unambiguous sentences and 76.6% of the time with ambiguous sentences. Although this 2.3% difference is in the direction predicted by the ambiguity-avoidance theory, the effect is significant only across items [$F1(1, 95) = 2.08, p < .16, CI = \pm 3.1\%$; $F2(1, 47) = 4.70, CI = \pm 2.7\%$; difference across items = 2.9%].

Unsurprisingly, more *thats* were mentioned when presented in the to-be-encoded utterance [$F1(1, 95) = 214.9, CI = \pm 3.5\%$; $F2(1, 47) = 282.4, CI = \pm 3.1\%$]. The interaction between this factor and the ambiguity factor was not significant (both F s < 1 ; CIs = $\pm 3.7\%$, $\pm 4.6\%$ by speakers and items respectively).

Experiment 2

One reason why Experiment 1 may have failed to find a robust ambiguity effect is because the pronoun *I* may have privileged status in production, given its personal relevance. To circumvent this concern, Experiment 2 investigated the same effect with the unambiguous pronouns *she* and *he* instead.

Method

Materials. Roughly the same sentence frames used in Experiment 1 were also used in Experiment 2, except that the unambiguous pronouns *she* and *he* were used instead of *I*. Slight changes were necessary to avoid possible coreference between the main subject of the sentence and the unambiguous pronoun (e.g., *The coach knew she missed practice*, in which *coach* and *she*

can refer to the same person, was changed to *The coaches knew she missed practice*). Whether the *she* or *he* was used was randomly determined from trial to trial, with the restriction that 12 of each pronoun appear in the critical sentences across the experiment.

Procedure. To shorten the experimental session, critical sentences in Experiment 2 were paired with one filler instead of two, as in Experiment 1. Thus, on each trial, speakers were only given two sentences at a time to encode and then recall. On 50% of the trials, speakers encoded the critical sentence first and recalled it second. On 25% of the trials, speakers encoded and recalled the critical sentence first, while on the remaining 25% of trials, speakers encoded and recalled the critical sentence second. The first ordering was broken into two identical conditions, and the resulting four levels of ordering were counterbalanced with the other experimental factors across speakers and sentences (and in all subsequent experiments, this order factor was fully counterbalanced across the other factors). Also, both dummy trials in Experiment 2 occurred before the 48 critical trials in the main block. The session lasted approximately 25 min.

Results

Speakers mentioned *that* 68.0% of the time in unambiguous sentences, and they mentioned *that* 70.2% of the time in ambiguous sentences. This 2.2% difference is in the direction predicted by the ambiguity-avoidance theory, though it is not statistically significant [$F1(1, 47) = 1.81$, $CI = \pm 4.3\%$; $F2(1, 47) < 1$, $CI = \pm 4.7\%$].

Speakers included more *thats* when a *that* was originally encoded [$F1(1, 47) = 119.35$, $CI = \pm 5.8\%$; $F2(1, 47) = 259.36$, $CI = \pm 4.2\%$]. The interaction between the ambiguity factor and whether a *that* was encoded was not significant (both F s < 1 ; CI s = $\pm 5.5\%$, $\pm 5.3\%$ by speakers and items respectively). In addition, *thats* were mentioned with *she* 65.1% of the time and with *he* 71.2% of the time; this 6.1% difference was not significant by speakers [$t(47) = 1.5$, $p < .15$].³

Discussion

In Experiments 1 and 2, speakers showed little tendency to include more disambiguating *thats* in sentences like *The coach knew you missed practice*, which include a garden path, compared to sentences like *The coach knew I missed practice*, which do not. There were slight differences in the direction predicted by the ambiguity-avoidance theory, but the differences were not significant by participants in either experiment and were not significant by items in Experiment 2.

In the Introduction, we suggested that the sentence recall task is informa-

³ Note that any difference between *she* and *he* should be interpreted with caution anyway, as this factor was randomly distributed and not counterbalanced across the other conditions.

tive about *that* mention to the extent that verbatim memory for the *that* was weak. Speakers' 63 and 67% memory accuracy of the *that* in Experiments 1 and 2, though better than a chance accuracy of 50%, was quite poor. Memory accuracy on the subsequent pronoun (*I* or *you* in Experiment 1) provides a good basis for comparison, since it is also a short closed-class word at a nearby sentence position. For the pronoun, memory accuracy was 84.9% in Experiment 1 and 85.5% in Experiment 2, not including trials where the speaker misencoded or forgot the sentence entirely. Thus, speakers are considerably more accurate at recalling the meaningful function words (*I*, *you*, *she*, and *he*) than the semantically weak ones (*that*), suggesting that recall proceeds from meaning rather than from sentence form.

It is possible to interpret these results as indicating that speakers have a moderate tendency to avoid temporarily ambiguous sentences, but that the experiments were not sufficiently powerful to detect this tendency. Indeed, claims based on the inability to reject the null hypothesis can never be completely answered. Nevertheless, Experiment 1 included 96 speakers and 48 sentences, so that a difference between the ambiguity conditions of 4.3% by speakers and 3.8% by sentences could have been detected with a probability of .8. Experiment 2 used 48 speakers and 48 items and could detect a difference between the ambiguity conditions of 6.0% by speakers and 6.6% by sentences with a probability of .8. Furthermore, if the data for Experiments 1 and 2 are combined into a single analysis (with *Experiment* as a between-speakers and between-items factor), the effect of ambiguity becomes only marginally significant by speakers [$F1(1, 142) = 3.8, p < .06$] and remains nonsignificant by items [$F2(1, 94) = 2.4, p < .13$]. (The analysis of the combined data had the power to detect a difference of 3.5% by speakers and 3.7% by items with a probability of .8.)

Overall, the results of Experiments 1 and 2 suggest that if there is any tendency to avoid ambiguity, it has only negligible effects on *that* mention. However, Experiments 1 and 2 might have found that speakers only weakly avoided temporarily ambiguous sentences because the sentence complement ambiguity is relatively weak compared to other temporary ambiguities that are known to be more profoundly difficult. To evaluate this possibility, Experiment 3 tested the more disruptive reduced relative clause ambiguity.

EXPERIMENT 3

In the reduced relative clause ambiguity, a fragment like *The astronauts selected . . .* tends to be interpreted with *selected* as a main verb (e.g., *The astronauts selected their spacesuits*), when *selected* is really a passive verb in a relative clause that modifies the subject noun (*The astronauts selected for the Apollo missions made history*). Although the general principles that underlie the reduced relative temporary ambiguity and the sentence complement ambiguity are the same, the reduced relative clause ambiguity disrupts comprehension more profoundly. For example, for sentence complement am-

biguities like those in Experiments 1 and 2, Ferreira and Henderson (1990) and Trueswell et al. (1993) found that (first-pass) reading times were 10% longer with ambiguous sentences than with unambiguous sentences. In contrast, for reduced relative clause ambiguities, the average disruption across four different experiments was 20% (F. Ferreira & Clifton, 1986; Trueswell, Tanenhaus, & Garnsey, 1994). Thus, assessing whether speakers avoid temporarily ambiguous reduced relative clause sentences can determine whether the small ambiguity-sensitive trends observed in Experiments 1 and 2 were due to the relative weakness of the sentence complement ambiguity.

If an ambiguity-avoidance production strategy is used, speakers should produce reduced relatives that would contain a temporary ambiguity as full relatives (e.g., *The astronauts who were selected for the Apollo missions made history*). One factor that permits the incorrect past tense reading is that for verbs like *selected*, the (correct) past participle form of the verb is identical to the (incorrect) past tense form. However, other English verbs, such as *chosen*, have distinct past participle and past tense forms. Since *chosen* cannot be interpreted as a past tense verb, a sentence fragment like *The astronauts chosen . . .* is not temporarily ambiguous and causes less difficulty during comprehension (Trueswell et al., 1994). Thus, if speakers choose to include optional words in accordance with an ambiguity-sensitive strategy, speakers should produce passive relative clause sentences with ambiguous verbs like *selected* as full relative clauses more often than sentences with unambiguous verbs like *chosen*.

To ensure that the sentences in Experiment 3 were likely to be disruptive, an independent production norming study was conducted. In the norming study, participants were given the main subject and first verb of each sentence in Experiment 3 (e.g., *The astronauts selected . . .* or *The astronauts chosen . . .*) and were asked to complete the fragment with the first continuation that comes to mind. Evidence showing that speakers tend to continue fragments containing ambiguous verbs (e.g., *selected*) with main clause completions suggests that the fragment tends to be interpreted with the initial verb as a main verb, consistent with the claim that sentences with those fragments include garden paths.

Method

Materials. Most of the unambiguous verbs were taken from Trueswell et al. (1994), though others were found in other sources (e.g., computer searches of Francis & Kucera, 1982), until a total of 12 unambiguous verbs were accumulated. Each unambiguous verb was matched with a semantically similar ambiguous verb. For each pair of verbs, two relative clause sentence frames were created that plausibly accepted both verbs. All 24 sentence frames are reported in the Appendix. Twenty-four new filler and 10 new practice sentences were created, representing a variety of structures.

Procedure. The procedure of Experiment 3 was almost identical to that of Experiment 2. To make the number of conditions divide evenly into the number of items, only three counter-balanced presentation order conditions were used instead of four. During recall, participants received the postverbal relative clause material (e.g., *for the Apollo missions*) of the critical

sentence (*The astronauts selected/chosen for the Apollo missions made history*) as the recall cue; the first noun and verb (*astronauts selected*) could not be used because that cue would include the manipulated verb and would span the optional material (*who were*). Because the sentences in Experiment 3 were longer, the progress bar was given 5 s to fill up before the deadline tone sounded. An entire session lasted approximately 20 min. Recalled productions were scored only if speakers produced an appropriate relative clause structure.

Experiment 3 (and subsequent experiments) used a different scoring scheme than Experiments 1 and 2. Instead of recording and transcribing all utterances, in Experiments 3–6, the experimenter coded the utterances during the session by checking off the category of utterance (e.g., whether the sentence was reduced) on a coding form. The session was recorded, and any trial that the experimenter missed was recovered from the audio recording.

Design. The main factor of interest was the ambiguity of the relative clause verb (referred to as *verb ambiguity*) as either unambiguous or ambiguous.

Norming study. Forty participants from the same population provided production norm data. Each participant was given 24 sentence onsets, consisting of the main subject and first (i.e., relative clause verb) of each sentence. Twelve sentence onsets contained unambiguous verbs and the other 12 contained ambiguous verbs. Each participant saw each sentence onset only once, and did not see the same sentence with more than one kind of verb.

Participants were instructed to “complete the sentence with the first completion that comes to mind” and were given an example of sentences with both reduced relative clause and main clause continuations. Each sentence was scored as using the presented verb as a main verb, as a relative clause verb, or as neither.

Participants interpreted unambiguous verbs almost exclusively as relative clause verbs, while ambiguous verbs were only sometimes interpreted as relative clause verbs. Looking only at grammatical completions, participants interpreted 98.9% of unambiguous verbs in sentence onsets as relative clause verbs and never interpreted such verbs as main verbs. On the other hand, participants interpreted only 36.7% of ambiguous verbs as relative clause verbs and 63.3% as main clause verbs. Thus, simple main clause interpretations occur often with fragments containing ambiguous verbs, but never with fragments containing unambiguous verbs, suggesting that only fragments with ambiguous verbs contain disruptive garden paths.

Results

Collapsing across all other factors, speakers produced 29.2% full relative clauses with unambiguous verbs, and they produced 28.9% full relatives with ambiguous verbs. This 0.3% difference in the direction opposite to that predicted by the ambiguity-avoidance approach was not significant [F_1 , $F_2 < 1$; CIs = 5.4%, 5.5% by speakers and items respectively]. Whether the presented sentence was full or reduced predictably affected full relative clause use [$F_1(1, 47) = 141.6$, CI = $\pm 5.1\%$; $F_2(1, 23) = 68.0$, CI = 7.5%]. The interaction between ambiguity and whether a *that* was originally presented was not significant [$F_1(1, 47) = 1.3$, CI = $\pm 6.5\%$; $F_2(1, 23) = 2.9$, $p < .11$; CI = $\pm 7.1\%$].

Discussion

Despite the strength of the reduced relative clause ambiguity, no hint of the numerical trends found in Experiments 1 and 2 were found in Experiment 3. Thus, it is unlikely that the nonsignificant trends in Experiments 1 and 2 were due to the relative weakness of the ambiguity tested in those experiments. (Experiment 3 had the power to detect a difference between ambiguity conditions of 7.7% by speakers and 7.9% by items with a probability of .8.)

At least under the conditions employed in these experiments, then, it seems unlikely that the inclusion of optional words in spoken sentences is based on the ambiguity of those sentences. Across three experiments, investigating two kinds of ambiguity, speakers did not consistently avoid producing sentences with temporary structural ambiguities.

However, research on sentence comprehension has revealed that the disruptiveness of a temporary ambiguity can be mitigated by other sentence factors. One relevant factor is *verb-specific structural frequency*—the frequency with which each verb occurs with the alternative structures involved in the temporary ambiguity. When reading a temporarily ambiguous sentence complement structure, for example, comprehenders will experience more difficulty with main verbs like *pronounce*, that typically occur with direct objects and therefore incorrectly bias comprehension, compared to verbs like *realize*, that typically occur with sentence complements and therefore correctly bias comprehension (Trueswell et al., 1993). Similarly, in a temporarily ambiguous reduced relative clause structure, verbs that occur less frequently as past participle verbs cause more difficulty than verbs that occur more frequently as past participle verbs (Trueswell, 1996). A second relevant factor is the plausibility or pragmatic fit of the postverbal word with the alternative structures involved with the temporary ambiguity. So, even though *warn* and *learn* both frequently occur with direct objects, the fragment *The police officer warned you . . .* is a sensible direct object fragment, while *The investigator learned you . . .* is not. Similarly, *The students handed the . . .* can plausibly be interpreted with *handed* as a main verb, but *The child concealed under . . .* can less plausibly be interpreted with *concealed* as a main verb. Thus far, only overall syntactic ambiguity has been considered, but it may be that speakers only produce a full sentence structure to circumvent a potential garden path if the corresponding reduced structure is temporarily ambiguous when verb-specific structural frequency and plausibility factors are taken into account.

To evaluate whether these frequency and plausibility factors affect production in our experiments, we regressed the rate that speakers produced full or reduced sentence types in the ambiguous sentence conditions on measures of verb-specific structure frequency and plausibility. For verb-specific structural frequency, the preferences of the verbs from Experiments 1 and 2 were determined from the production norms of Garnsey et al. (1994), which reported the rate at which sentence complement-taking verbs were completed as sentence complement structures, direct object structures, or any other kind of structure. From these norms, we calculated a *sentence complement preference* for each verb.⁴ For Experiment 3, the Francis and Kucera (1982) fre-

⁴ For a sentence completion with a particular verb, take SC to be the proportion of sentences completed as sentence complements and DO to be the proportion of sentences completed as direct objects; the sentence complement preference is $SC/(SC + DO)$. A sentence complement preference that included all alternative structures in the denominator only resulted in lower correlations.

quency of each verb in its past participle and past tense forms were used to calculate an analogous *past participle preference*. For the plausibility measures, we gave sentence fragments like *The police officer warned you . . .* and *The child concealed under . . .* to 38 subjects and asked for the first available completion. Completions were coded as sentence complement versus direct object for sentence complement sentences or reduced relative versus main verb for the relative clause sentences. From these completion proportions, we calculated a sentence complement preference and relative clause preference for each sentence. Since all frequency and plausibility measures were calculated in terms of the eventually correct (sentence complement or reduced relative) syntactic analyses, a negative correlation between a measure and speakers' mention of full sentences would indicate a sensitivity to the influence of these comprehension biases.

At least in the present data, however, neither the verb-specific structural frequency factor nor plausibility consistently affected the rate of full sentence production. For verb-specific structural frequency, the correlations between full sentence production rate and sentence complement or relative clause preference was $-.15$ (*ns*), $-.30$ ($p < .05$), and $+.15$ (*ns*) in Experiments 1, 2, and 3 respectively. Thus, it appears that speakers do not have a consistent tendency to produce more full sentences when verb-specific structural frequency biases that verb toward the eventually incorrect structural alternative. For plausibility, the correlations between full sentence production rate and sentence complement or relative clause preference was $+.02$ (*ns*), $-.02$ (*ns*), and $-.12$ (*ns*) in Experiments 1, 2, and 3 respectively. These weak correlation values suggest that speakers were no more likely to produce an unambiguous full sentence when its reduced counterpart was pragmatically as well as syntactically ambiguous. Hence, this correlational evidence from the present experiments fails to support the claim that speakers' productions are guided by the contributions to ambiguity of verb-specific structural frequency and plausibility.

On the other hand, mention of full sentences in Experiments 1–3 did correlate with other variables. In Experiments 1 and 2, the *that-preferences* of the verbs (how frequently each verb tends to be used with a *that* in a sentence complement structure, as measured in an independent production norm study; Garnsey et al., 1994) correlated significantly with the tendency to mention *that* during sentence recall (in Experiment 1, $r = .52$, $p < .01$; in Experiment 2, $r = .54$, $p < .01$; for sake of comparison to the previous analyses, these correlations are from the ambiguous sentences only). Also, the (log) frequency of the main verb in the sentence complement structures of Experiments 1 and 2 correlated with the production of *that* (Experiment 1: $r = -.31$, $p < .05$; Experiment 2: $r = -.44$, $p < .01$; the frequency of the passive relative clause verb correlated significantly with full relative clause mention in the unambiguous condition, $r = -.61$, $p < .01$, though not in the ambiguous condition, $r = -.25$, *ns*). (This effect of frequency in Experi-

ments 1 and 2 is similar to a comprehension effect reported by Juliano & Tanenhaus, 1993.) Thus, the use of full sentences in the sentence recall task is sensitive to some variables known to affect psycholinguistic performance in other experimental settings.

Although Experiments 1–3 used large numbers of speakers and items, the claim that speakers do not avoid temporarily ambiguous sentences is still based on null effects. Next, Experiments 4–6 investigate whether optional word mention is guided by availability-based factors, while factorially manipulating the potential ambiguity of the target sentences. If ambiguity-avoidance effects are still not found in Experiments 4–6 while availability-based effects are found, then it is unlikely that the absence of ambiguity-avoidance effects derives from an inability for the experimental paradigm to detect theoretically interesting differences.

EXPERIMENT 4

Experiment 4 used the same sentence complement structures that were used in Experiments 1 and 2, but with an important change. In Experiment 4, all sentences were like (1)–(4):

- (1) I knew (that) I had booked a flight for tomorrow.
- (2) You knew (that) I had booked a flight for tomorrow.
- (3) I knew (that) you had booked a flight for tomorrow.
- (4) You knew (that) you had booked a flight for tomorrow.

Thus, both the main subject and the embedded subject of the sentences in Experiment 4 were either the pronoun *I* or the pronoun *you*.

Availability-based predictions. In Experiment 4, we manipulated the availability of a word through repetition. We assume that when a word is selected for production, it should be especially available if it has just been recently selected (e.g., Wheeldon & Monsell, 1992). In sentences (1) and (4) above, each embedded subject is identical to its corresponding main subject, and thus should be more available for selection than the embedded subjects in sentences (2) and (3), which are not identical to their main subjects. Since the embedded subject immediately follows the optional complementizer *that*, the principle of immediate mention predicts that high availability of the embedded subject should lead to the omission of *that*. Thus, the availability-based approach predicts that speakers should include fewer *thats* in sentences (1) and (4) above compared to sentences (2) and (3).

Ambiguity-sensitive predictions. Because the embedded subjects in the critical sentences of Experiment 4 are either unambiguous (*I*) or ambiguous (*you*), just like Experiment 1, nearly the same predictions follow. In sentences (1) and (2), the embedded subject is the subject pronoun *I*, and it can therefore not be taken as a direct object. As a result, sentences (1) and (2) do not contain temporary ambiguities.

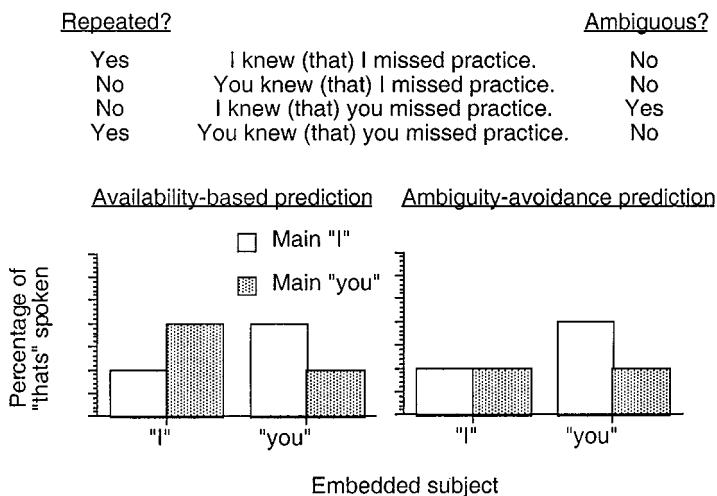


FIG. 1. Predictions of the availability-based and ambiguity-avoidance models for Experiment 4.

The potential ambiguity of sentences (3) and (4) is complicated by what are termed *binding principles* in linguistics (Chomsky, 1988). Among other things, the binding principles deal with the distribution of simple (e.g., *you*) and reflexive (e.g., *yourself*) pronouns in clauses and account for the fact that the second pronoun in *You knew you well* (rather than *You knew yourself well*) causes the sentence to be ungrammatical. However, *You knew you had booked a flight for tomorrow* is grammatical, essentially because the two *yous* are in different clauses (one is the subject of the verb *know*, while the other is the subject of the verb *booked*). Thus, the second pronoun in the fragment *You knew you . . .* is unambiguously an embedded subject because as a direct object it violates a binding principle (provided that comprehenders take the binding principles into account; see Badecker & Straub, 1996). Thus, only sentence (3) is ambiguous (*I knew (that) you . . .*) because only in that sentence is the embedded subject pronoun a possible direct object.

The predictions of the availability-based and ambiguity-avoidance approaches are summarized in Fig. 1. Note that both theories make the same prediction (though for different reasons) for the sentences with *you* as embedded subjects (the two bars on the right of each graph in Fig. 1). Thus, the conditions that will discriminate between the two approaches are those where speakers produce sentences with *I* as embedded subjects. Only the availability-based approach predicts that speakers should mention fewer *thats* in sentences like *I knew (that) I . . .* (which include repeated embedded subjects) compared to sentences like *You knew (that) I . . .* (compare the two left

bars in each graph in Fig. 1). We call this the *availability-based planned comparison*. On the other hand, only the ambiguity-avoidance approach predicts that speakers should mention more *thats* in sentences like *I knew (that) you . . .* (which include ambiguous embedded subjects) compared to sentences like *You knew (that) I . . .* (compare the middle two bars in each graph in Fig. 1). We call this the *ambiguity-avoidance planned comparison*. These planned comparisons are evaluated as measures of the degree to which production is availability-based and ambiguity-sensitive respectively. Note that both comparisons are confounded with main effects, so that any significant difference should be corroborated by a significant interaction between the main and embedded subject factors.

Method

Materials. The 48 sentences in Experiment 4 were adapted from those in Experiment 1. The main subject (e.g., *The coach*) was replaced by the pronoun *I* or *you* (depending on experimental condition). Also, the verbs in Experiment 4 were paired such that one member was a high-frequency verb and the other a low-frequency verb, and two sentences were created for each pair. The frequency manipulation had no effect on the inclusion of *that* and is not mentioned further.⁵ The materials are reported in the Appendix. The fillers and practice sentences were modified to be similar to the critical sentences.

Procedure. The procedure was identical to that of Experiment 3, except the following: The progress bar required 3 s to fill up. At recall, speakers received the main subject and main verb (e.g., *I knew* from *I knew (that) you missed practice*) as a recall cue. There were three levels of the order factor, each of which included at least one encoding or recall event between critical sentence encoding and critical sentence recall.

Design. Experiment 4 had two main factors of interest: main subject (*I* or *you*) and embedded subject (*I* or *you*).

Results

Collapsing across the factors not of theoretical interest, the results of Experiment 4 are shown in Fig. 2. Speakers produced roughly 9% fewer *thats* in the two conditions in which the embedded subject was repeated compared to the two conditions where the embedded subject was not, as supported by

⁵ This manipulation was meant as a systematic exploration of the significant correlations between main verb frequency and that mention in Experiments 1 and 2 (an effect that corresponds to the reported comprehension effect of Juliano & Tanenhaus, 1993). The nonsignificant effect in Experiment 4 suggests that in production frequency does not directly affect *that* mention, though the significant correlations in Experiments 1 and 2 are consistent with the possibility that frequency may affect some other property of sentence complement structures which subsequently affects *that* mention.

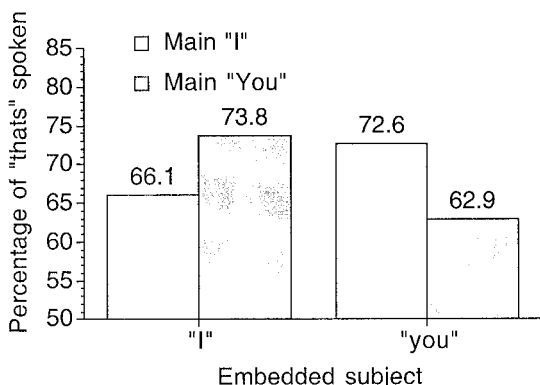


FIG. 2. Percentages of full sentences produced with different main and embedded subjects in Experiment 4.

a significant interaction between the main and embedded subject factors [$F1(1, 47) = 25.3$, $CI = \pm 4.9\%$; $F2(1, 47) = 23.5$, $CI = \pm 4.9\%$]. Assuming that repetition renders the embedded subject more available, this difference is predicted by the availability-based model. The only other factor that significantly affected production was whether the original sentence included a *that* [$F1(1, 47) = 108.1$, $CI = \pm 6.7\%$; $F2(1, 47) = 304.3$, $CI = \pm 4.9\%$].

The ambiguity-avoidance planned comparison, which measures the *thats* mentioned in sentences like *I knew (that) you . . .* compared to *You knew (that) I . . .*, revealed a -1.2% difference (both F s < 1). The availability-based planned comparison, which measures the *thats* mentioned in sentences like *I knew (that) I . . .* compared to *You knew (that) I . . .* showed that speakers mentioned 7.7% fewer *thats* in the repeated case [$F1(1, 47) = 8.2$; $F2(1, 47) = 10.1$]. Thus, these specific contrasts support only the availability-based approach.

Discussion

Speakers included fewer *thats* when the embedded subject was repeated and therefore more available, in line with the the availability-based approach. In addition to supporting the claim that availability affects optional word mention, this result shows that the experimental tasks and manipulations can induce nontrivial changes in optional word mention and that the designs are sufficiently sensitive to detect them. Thus, the lack of difference between ambiguous and unambiguous sentences in Experiments 1–3 is unlikely to be due to any insensitivity of the procedure. Furthermore, the ambiguity of the sentences in Experiment 4 was manipulated, but *that*-mention did not systematically vary according to this factor.

The use of pronouns in the critical sentences may raise a concern with Experiment 4. Pronouns are highly frequent and phonologically simple

closed-class words, and the pronoun *I* specifically is personally relevant to any speaker of English. As such, the availability of pronouns may vary minimally, implying that the repetition manipulation may affect *that* mention for reasons other than increased availability. One possibility is that the repetition of the embedded subject results in *coreference*, in that the main and embedded subject refer to the same entity (e.g., the two pronouns in *I knew (that) I missed practice* refer to the same individual, whereas the two pronouns in *I knew (that) you missed practice* do not). Given that coreference has many linguistic effects (as reflected by the binding principles mentioned above), it is possible that coreferential sentences are different from non-coreferential sentences in a way that could be responsible for the observed effects (Elsness, 1984; Thompson & Mulac, 1991). Another possibility is that in normal language use, pronouns are so highly accessible that the variability in accessibility conditioned in Experiment 4 represents a situation that is difficult to generalize to more natural production. To address these concerns, Experiment 5 tested the availability-based approach not by repeating pronouns, but rather by manipulating the content of the recall cue itself.

EXPERIMENT 5

In Experiment 5, we used the recall cue in the sentence recall task to manipulate the availability of the sentence material directly. Given that the material that makes up the recall cue is given to the speaker, its recall should be especially easy compared to material that does not make up the recall cue. Thus, to evaluate the availability-based approach, we can compare production when the recall cue is made up of the post-*that* material (and is therefore highly available) compared to when the recall cue is made up of other sentence material (and therefore, the post-*that* material is relatively less available). For example, when attempting to produce *I suggested (that) you felt uncomfortable*, fewer *thats* should be mentioned when prompted with *you felt* compared to when prompted with *I suggested*. We call the condition in which the recall cue is made up of the post-*that* material the *available* condition and the condition in which the recall cue is made up of sentence-initial material the *unavailable* condition (since the availability of the post-*that* material is of theoretical importance).

Again, Experiment 5 manipulated whether produced sentences contained a temporary ambiguity. The embedded subject was either unambiguous, as in *You suspected (that) I felt uncomfortable*, or it was ambiguous, as in *I suspected (that) you felt uncomfortable*. Although the main and embedded subjects were always either *I* or *you*, they were never the same, avoiding coreference. The ambiguity-sensitive approach predicts that speakers should include more *thats* when the ambiguous pronoun *you* is the embedded subject.

TABLE 2
One Full Sentence Set (Item) in Experiment 5

Embedded subject ambiguity	Sentence frame No. 1	Sentence frame No. 2
Unambiguous	You suspected (that) I felt uncomfortable.	You felt that I suspected our neighbor of committing the crime.
Ambiguous	I suspected (that) you felt uncomfortable.	I felt that you suspected our neighbor of committing the crime.

Method

Materials. Sixty-four verbs were chosen from the Garnsey et al. (1992) set (most of which overlapped with the verbs in Experiment 1). A set of 32 pairs of verbs was created by matching two verbs in the set that could sensibly make a sentence together. For each pair of verbs, two sentence frames were created such that one member of the pair was the main verb and the other the embedded verb for one sentence and vice versa for the other sentence (since the recall cue could be either the main or embedded verb, it was necessary to counterbalance the content of the cue across recall conditions). The pronouns *I* and *you* were used to complete the set. A set of sentences is shown in Table 2, and all sentences are reported in the Appendix.

Procedure. The procedure was identical to that used in Experiment 4, except for the ordering of the sentences for encoding and recall. First, due to counterbalancing constraints, there were only two ordering conditions. Second, pilot studies revealed that speakers had more difficulty recalling the critical portions of the sentences in Experiment 5, so the critical sentence was always presented second and was then recalled either first (i.e., immediately) or second, depending on the order condition. (Reasons for the increased difficulty are discussed below.)

Scoring. Because the content of the embedded verb was counterbalanced (i.e., it was sometimes the main verb and sometimes part of the recall cue), an utterance in Experiment 5 could only be scored if the participant correctly recalled the sentence up to and including the embedded verb. That is, for a sentence like *I suspected (that) you felt uncomfortable*, a scored sentence always included *I suspected (that) you felt . . .* Note that this is a stronger scoring criterion than that used in Experiments 1, 2, and 4 with similar sentences. Speakers produced fewer scorable utterances in Experiment 5 (due at least partially to this stronger scoring criterion), and, as a result, one of 256 cells was empty in the item analysis. This missing value was estimated using the marginal means, as described in Winer (1971).

Design. There were two counterbalanced factors of interest: The material immediately following the optional *that* was either available or unavailable, corresponding to whether that sentence material comprised the recall cue, and the embedded subject pronoun was either the unambiguous *I* or the ambiguous *you*. In addition to the counterbalanced factors of the previous experiments, Experiment 5 also counterbalanced the assignment of verb to main or embedded position (providing the sentence pairs presented in Table 2).

Results

The results of Experiment 5 are reported in Table 1. Sentences in the available condition were produced with 8.4% fewer *thats* than sentences in the unavailable condition [$F1(1, 31) = 5.5$, $CI = \pm 8.6\%$; difference across speakers = 9.8%; $F2(1, 31) = 6.9$, $CI = \pm 5.8\%$], as predicted by the availability-based theory. Sentences in the unambiguous embedded subject condi-

tion were produced with 2.6% more *thats* than sentences in the ambiguous embedded subject condition. This nonsignificant difference [$F1(1, 31) < 1$, $CI = \pm 5.4\%$; $F2(1, 31) = 1.9$, $CI = \pm 6.4\%$] is opposite to that predicted by the ambiguity-avoidance theory. The only other factor that affected production was whether the presented sentence included a *that* [$F1(1, 31) = 213.2$, $CI = \pm 8.6\%$; $F2(1, 31) = 462.4$, $CI = \pm 5.9\%$]. No interactions were significant (all $F_s < 1$). The overall level of *that* production was lower in Experiment 5 than it was in the previous experiments, at least partially because the easier recall conditions in Experiment 5 (since half of the critical sentences were recalled immediately after they were encoded) allowed speakers to be more accurate at including *thats* in their sentences (see Table 1).

Discussion

Speakers included fewer *thats* in sentences with available post-*that* material. Importantly, this availability effect was revealed even when availability was manipulated more artificially by means of the recall cue. This supplements the result found in Experiment 4, when availability as manipulated by intrasentence repetition of simple pronouns (coreference) also resulted in fewer spoken *thats*. Thus, Experiments 4 and 5 provide converging evidence supporting the claim that the availability of material after an optional word can affect the decision of whether to use that optional word.

In contrast to the availability findings, the temporary ambiguity of the spoken sentences had little effect on production in Experiment 5, replicating the lack of effect found in Experiments 1–4. Thus, the experiments generally show that speakers do not use optional words to disambiguate sentences, at least under the conditions investigated in the first five experiments.

Two differences in speakers' performance in Experiment 5 (compared to Experiments 1–4) deserve mention. First, speakers had greater difficulty remembering these sentences generally, as reflected by the smaller percentage of utterances that could be analyzed (although the harder scoring criterion that was employed also lowered this percentage). This is probably due to the fact that the sentences in Experiments 1–4 were constrained by few experimental factors—usually, just a particular verb and a pronoun was necessary in each sentence. In Experiment 5, the main subject, main verb, embedded subject, and embedded verb had to be fully interchangeable, so that all factors could be counterbalanced. As a result, only sentence complement-taking verbs could be used in the main and embedded verb positions (e.g., "I *suspected* that you *felt* uncomfortable"), restricting the verbs that could be used with each sentence and affecting the naturalness of the stimuli, which in turn was likely to have affected the ease with which the sentences could be remembered. Second, memory accuracy for the *that* was higher in Experiment 5 than it was in Experiments 1–4, probably because for half of the critical sentences, speakers recalled the sentence immediately after presenta-

tion with no intervening filler (in fact, memory accuracy on *that* was 86% on immediately recalled sentences, while it was only 73% when the recall of a filler intervened). As noted, this was necessary because speakers were having trouble adequately recalling the sentences when one or two fillers intervened. Note, however, that both of these differences should have made it more difficult to find a significant effect, because fewer analyzable utterances decreases the stability of each comparison and because greater accuracy with *that* restricts the variability that the availability or ambiguity factors might account for. Nevertheless, the effects of availability of the embedded material was still found.

A remaining possible difficulty with the conditions investigated in Experiments 1–5 concerns the communicative nature of the task that was used. From the perspective of the participants in Experiments 1–5, the task involved memory, not communication. To evaluate whether the noncommunicative nature of the task is crucial, Experiment 6 investigated the same conditions as Experiment 4, except that for half the speakers, the task was portrayed as a communication task.

EXPERIMENT 6

The primary goal of Experiment 6 was to evaluate whether incentives to communicate change the ambiguity-sensitive or availability-based nature of the productions that are elicited in the sentence recall task. Half of the speakers addressed listeners (or more precisely, addressees), who were instructed to rate the clarity of the speakers' sentences. Speakers were told of their addressee's task and were asked to maximize the rated clarity of their productions. (Because credibility was crucial to this experiment, the addressees were not confederates, but instead were taken from the same population as the speakers). The other half of speakers performed essentially the same memory task that was performed in Experiment 4.

Whether speakers are engaged in a communicative task may affect ambiguity-avoidance for a number of reasons. Speakers may detect which of their produced sentences are ambiguous (or at least, "funny sounding") and spontaneously clarify such sentences. Or, speakers may speak with different modes or registers depending on communicative demands. Regardless of the precise reason, if speakers treat the ambiguity of their utterances differently depending on communicative pressures, then Experiments 1–5 may have not detected a tendency to avoid garden paths because the communicative pressure was weak.

Experiment 6 also had two secondary goals. First, to evaluate the relative strength of the availability-based and ambiguity-sensitive factors, Experiment 6 used the materials and design of Experiment 4, which competitively evaluated the predictions of the two approaches. If a communicative goal moves speakers toward a more natural mode of speaking that is sensitive to

comprehension difficulties, then the availability-based effect should diminish at the expense of the ambiguity-avoidance effect. Second, Experiment 6 used a procedure that was as similar as possible to that used in Experiments 1–5. If Experiment 6 replicates the previous experiments, then procedural similarity will permit the conclusions of Experiments 1–5 to be generalized to the more communicative situation tested in Experiment 6.

Method

Materials. The materials of Experiment 4 were used without the frequency manipulation, so that each verb occurred with one sentence frame. The sentences are reported in the Appendix.

Procedure. The experimental software used in Experiment 4 was also used in Experiment 6, with some procedural changes to accommodate the communicative aspect of the experiment. Speakers paired with addressees sat in front of the computer, next to their addressees (who could not see the monitor). Speakers did not read aloud when encoding the sentences because addressees would hear the sentences, which might affect speakers' emphasis on communication during subsequent production. Although this change risks including trials with misencoded sentences, such errors were rare in Experiments 1–5 (between 0.5 and 5.8% of trials).

Upon recall, one group of speakers (the *communication* group) looked at and spoke to their addressees when they produced the sentence. Addressees rated the clarity of speakers' productions with pen and paper on a 7-point scale. Note that by use of the term 'clarity,' addressees rated the form rather than the content of speakers' utterances; because the potential listener difficulty related to temporary ambiguity—a form-based difficulty—we felt that an addressee focus on form was appropriate. Speakers were informed of the addressees' task and were asked to maximize the addressees' ratings by changing the sentence if necessary. Although no explicit feedback was given, speakers could see the addressees' rating sheet as the experiment proceeded. Because the progress bar might encourage speakers to look at the computer screen during production, the timing bar was eliminated, although the deadline was still marked with an auditory signal (which also indicated that the next recall cue was to be presented, so speakers could redirect their attention to the screen). The duration of the recall period was increased to 5 s, so that speakers would experience minimal time pressure. The recall task was presented to participants as a method to provide the material to be spoken. The group of speakers not paired with addressees (the *memory* group) performed the identical task, except that they did not speak to an addressee and no instructions about maximizing clarity were given. Except for the different instructions and the presence of the addressee, the situation for the communication group and the memory group speakers was identical.

Scoring. Scoring was conducted as in Experiment 4. The large number of counterbalanced factors left 13 of 768 cells empty in the item analysis; these values were estimated as in Experiment 5. In the communication condition, 78.3% of all utterances were scorable, while in the memory condition, 81.6% of all utterances were scorable, a difference significant only across items [$F(1, 46) = 1.1$; $F(1, 47) = 4.5$].

Design. In addition to the factors manipulated in Experiment 4, speakers in Experiment 6 were tested in the communication task or memory task. This factor was manipulated between speakers and within items and was counterbalanced across all other factors.

Results

Collapsing across the factors not of theoretical interest, the results of Experiment 6 are shown in Fig. 3. Regardless of whether speakers were in the communication or memory group, speakers produced roughly 9% fewer *thats* when the main and embedded subjects were identical, compared to

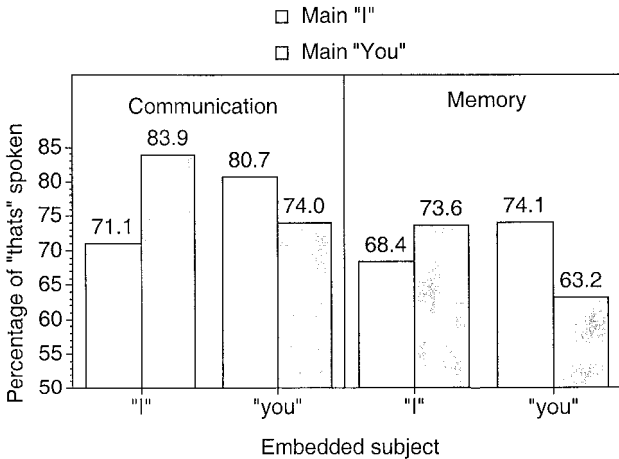


FIG. 3. Percentages of full sentences produced with different main and embedded subjects by speakers in a communication or memory task in Experiment 6.

when they were not [the interaction between the main and embedded subject factors was significant, $F_1(1, 46) = 20.4$, $CI = \pm 5.7\%$; $F_2(1, 47) = 18.0$, $CI = \pm 5.7\%$]. Task had only an overall effect on *that*-mention in that communication-task speakers produced 7% more *thats* than memory-task speakers [this main effect was not significant in the independent groups speaker analysis, $F_1(1, 46) = 1.4$, $CI = \pm 11.0\%$, but was significant in the repeated measures item analysis, $F_2(1, 47) = 13.8$, $CI = \pm 4.1\%$]. Importantly, the availability and the ambiguity effects did not vary as a function of task; the difference between sentences with repeated and new embedded subjects was 9.7% for speakers in the communication task, while it was 7.6% for speakers in the memory task [the three-way interaction between task, main, and embedded subject was not significant, $F_1(1, 46) = 1.1$, $CI = \pm 8.1\%$; $F_2(1, 47) < 1$, $CI = \pm 8.5\%$].

The theory-specific contrasts described in Experiment 4 were also evaluated in Experiment 6. Collapsing across task, the ambiguity-avoidance planned comparison (which contrasts *that*-mention in the ambiguous *I knew (that) you . . .* sentences vs. the unambiguous *You knew (that) I . . .* sentences) was -1.4% (both $F_s < 1$). Within the communication condition, this difference was -3.2% [$F_1(1, 46) = 1.3$; $F_2(1, 47) < 1$], whereas in the memory condition, this difference was 0.5% (both $F_s < 1$). These small differences fail to support the predictions of the ambiguity-avoidance approach. The availability-based planned comparison (which contrasts the repeated-embedded-subject sentences, *I knew (that) I . . .*, with the nonrepeated *You knew (that) I . . .* sentences) was 8.8% [$F_1(1, 46) = 10.7$; $F_2(1, 47) = 6.5$]; the difference was 12.8% with the communication group [$F_1(1, 46) = 14.4$; $F_2(1, 47) = 6.7$], but was only 5.2% with the memory group (both $F_s <$

1). Thus, on this comparison, the availability-based effect was more in evidence in the communication than in the memory task [though the simple interaction of main and embedded subject for the memory task group was significant, $F1(1, 46) = 6.1$; $F2(1, 47) = 7.0$].

Speakers included more *thats* when the presented sentence included a *that* [$F1(1, 46) = 108.6$, $CI = \pm 5.3\%$; $F2(1, 47) = 122.3$, $CI = \pm 4.7\%$]. No other main effect or interaction approached significance (all F s < 2.9), except for an interaction between task and main subject [$F1(1, 46) = 4.9$, $CI = \pm 5.0\%$; $F2(1, 47) = 2.9$, $p < .1$, $CI = \pm 5.2\%$]. This latter interaction reflects the fact that speakers in the communication task included (roughly) 3% more *thats* when the main subject was *you* (78.9%) compared to when it was *I* (75.7%), whereas speakers in the memory task included 3% more *thats* when the main subject was *I* (71.2%) compared to when it was *you* (68.5%). It is unclear why such an interaction would arise from either theoretical perspective.

The addressees in Experiment 6 provided clarity judgements of the speakers' sentences, which may vary as a function of the manipulated factors or of the features of the productions. However, the clarity ratings ended up being uniformly high, ranging between 5.24 and 5.44 (on a 1-to-7 scale) between the four main- and embedded-subject conditions. Furthermore, these ratings did not vary depending on whether speakers included or omitted the *that* (5.30 and 5.43 respectively). Evidently, these clarity ratings are not as sensitive to comprehension preferences as more online measures of comprehension with these sentences, such as eye tracking (Rayner & Frazier, 1987).

Discussion

The results of Experiment 6 closely replicated the results of Experiment 4, regardless of whether speakers participated in a task that emphasized communication or memory. Except for the main effect of *that* mention, the results in the communication and memory tasks were similar. Thus, Experiment 6 extends the conclusion of Experiments 1–5 that the language production system does not use an ambiguity-avoidance strategy when producing optional words under conditions where understandability is emphasized.

Note, however, that whether speakers were engaged in a communication or a memory task quantitatively affected the use of optional words. That is, communication-task speakers used 7% more *thats* than memory-task speakers, an effect that was robust in the item analysis. This difference suggests that the task manipulation was effective in that performance differed in an interpretable way between the two task conditions.

The fact that communication-task speakers tended to produce more *thats* suggests that the two groups of speakers produced sentences differently. Evidently, a communicative emphasis encourages speakers to speak more clearly or formally overall, as reflected by the communication-task speakers'

increased use of *that*. However, the increased use of *that* does not compromise the availability effect, nor did it introduce an ambiguity-avoidance strategy. Speakers are thus capable of making gross changes to their sentences in response to communicative demands, which are likely to be the kinds of changes that speakers make when addressing communicatively impaired individuals (e.g., in child-directed speech, e.g., Snow, 1972; or when addressing foreign-language speakers or retarded individuals, e.g., DePaulo & Coleman, 1986), or when the clarity of a sentence is questioned (Valian & Wales, 1976). However, speakers do not seem to adjust their production based on subtle properties of sentences that have measurable impact on comprehension, such as garden paths that are contingent on the case properties of an embedded subject pronoun.

GENERAL DISCUSSION

The experiments make two general points. First, speakers do not systematically mention optional words to disambiguate sentences. With sentence complement structures in Experiments 1, 2, and 4–6, and with relative clause sentences in Experiment 3, speakers did not consistently include more optional function words with sentences that would otherwise contain temporary ambiguities, compared to those that would not. Second, the use of optional words is sensitive to the availability of the material that is spoken. When embedded subjects were repeated, as in Experiments 4 and 6, or when the recall cue consisted of the embedded clause material, as in Experiment 5, speakers were more likely to omit optional complementizers compared to nonrepeated or noncued conditions. The results of Experiment 6 suggest that these conclusions hold even when the production task emphasizes communication rather than memory performance.

An interesting way to evaluate the contribution of the present work with respect to availability-based effects is to contrast the present results, which have revealed availability effects on optional word mention (e.g., the use of optional complementizers in sentence complement structures), to past demonstrations, which have revealed availability effects on order of mention (e.g., the use of actives or passives). The standard demonstration of availability effects involves, for example, showing that priming the agent of a sentence, like *tortoise*, increases the likelihood that an active is used, like *The tortoise defeated the hare*, whereas priming the patient of a sentence, like *hare*, increases the likelihood that a passive is used, like *The hare was defeated by the tortoise* (e.g., Bock, 1986a). Such a result is consistent with a model where availability effects result from a “race” among lexical items for early placement in a sentence; if a lexical item is selected first and wins the race, it is mentioned early. In short, availability-based effects on order of mention (like the choice of a transitive structure) are consistent with a fully lexical model of sentence production.

This race-based model could be applied to the optional-word-mention effects revealed in the present experiments. A speaker, having already prepared the main subject and verb of a sentence complement structure, like *The coach knew . . .*, must choose next either the optional complementizer (*The coach knew that . . .*) or the embedded subject (*The coach knew you . . .*). Within a lexical race-based model, this choice might result simply from a race between these lexical items—the complementizer *that* on the one hand or the embedded subject *you* on the other. The complementizer may win the race, get mentioned immediately, resulting in a full sentence complement, or the embedded subject may win and get mentioned immediately, causing the complementizer to be omitted and resulting in a reduced sentence complement.

However, a difficulty for this characterization comes from research in language production (Garrett, 1980, 1982, 1988; Humphreys, 1998; Marin & O'Seaghdha, 1996) which suggests that *function* words, possibly including the kinds of optional complementizers under investigation here, may not be retrieved and produced in the same way that *content* words are. Instead, function words may be processed in a manner that suggests that they are inherently associated with their accompanying syntactic structures. Thus, by this characterization, the choice of producing a full or reduced sentence complement is not simply the choice of including or omitting a lexical item corresponding to the word *that*, but rather, it's the choice of using one syntactic structure that includes a *that*, or another syntactic structure that does not.

Returning to the race metaphor, then, because function words may be more syntactic than lexical in nature, to claim that the complementizer *that* and the embedded subject *you* race for selection may be to postulate a race between two unlike elements. There is, however, a more parsimonious alternative: Rather than saying that syntactic structure choices are fully lexically determined, lexical items may influence wording decisions *through* syntactic mechanisms. That is, it may be that a high level of activation of an embedded subject argument encourages the use of a syntactic structure (the reduced sentence complement) that permits its early mention and an absence of a high level of activation in the embedded subject leads to the use of a syntactic structure (the full sentence complement) that delays its mention. The implications are more profound if this account is extended to availability effects on order of mention: By this characterization, it is not that a passive or active is used simply because one lexical item or the other was selected first; rather, high activation of a patient argument may influence syntactic mechanisms to produce a passive structure, and high activation of the agent argument may influence syntactic mechanisms to produce an active structure. In short, these results suggest that production is not fully lexically based, but rather that the structure choices that speakers make when influenced by availability result from the interplay between lexical activation and syntactic production mechanisms.

In sum, the availability effects discovered until now, which have concerned variation in order of mention, confound the choice of one syntactic structure over another with the choice of one lexical item over another. As a result, it is impossible to distinguish whether the word order variation is due directly to the selection of a lexical item (consistent with a fully lexically based model of production) or whether the variation is due to the selection of a syntactic structure that is consistent with the early mention of the active lexical item (a lexical–syntactic interactionist model). Availability effects on optional word mention, however, remove this confound, since the choice of a syntactic structure varies with the activation of only one lexical item. Given that in optional word mention effects, speakers evidently choose one syntactic structure over another without choosing between alternative lexical items, it suggests that generally, lexical availability effects are mediated by syntactic structure mechanisms.

In this vein, it is interesting to ponder the nature of register effects, such as the effect in Experiment 6 where communication-task speakers produced a higher percentage of *thats* than memory-task speakers. Generally, by the syntactically mediated characterization just described, whenever lexical availability affects a syntactic structure choice *without* changing order of mention (such as affecting the use of an optional function word or affecting whether an optional argument is produced in the current sentence or not), syntactically more transparent utterances and simpler utterances will result whenever the activations of the individual arguments of a to be produced sentence are less anticipatory. So, for example, when the embedded subject activation is less prominent in a sentence complement structure, a complementizer *that*, which makes the sentence complement structure more transparent, gets mentioned; or, in an optionally transitive sentence, a weak level of activation in the direct object argument might lead to the production of a simpler intransitive sentence (with the direct object presumably being mentioned in a subsequent simple sentence). Thus, a straightforward way to account for register effects within this model is to claim that speakers switch to a more syntactically transparent and simpler mode of producing sentences as a result of attending less to each individual argument of a sentence—especially arguments that would be produced at the end of a sentence. One might expect speakers to attend less to individual arguments late in a sentence if they are concentrating on lexicalizations early in the sentence (say, to produce a more frequent word) or because they are attending more to the prosodic contour of the sentence (as in child-directed speech).

Turning next to the observation that speakers do not avoid temporarily ambiguous sentences, this result converges with the conclusions of the natural speech and text corpus studies (Craig et al., 1995; Elness, 1984) and further suggests that the production system is unlikely to use a syntactic mechanism that implicitly avoids structures that are difficult to comprehend. Proposals for architectures where the representations and processes used for

syntactic comprehension are tightly linked to (or identical with) those used for syntactic production are affected by this implication (e.g., MacKay, 1987, proposes such a tight linking). Under such architectures, structures that cause difficulty in comprehension might be expected to cause corresponding difficulty in the tightly linked (or identical) production system, implying that difficult-to-comprehend structures should similarly be difficult to produce. The present experiments find no support for this claim.

It is important to place the negative findings regarding ambiguity avoidance in perspective. Although the temporary ambiguities that we investigated are substantial from the perspective of the comprehension system, it is unlikely that speakers are aware of them during online production. Thus, the present experiments can be interpreted as showing that speakers do not *implicitly* avoid disruptive structures in that the production system is not automatically biased against creating such garden path structures in its basic operation. However, this does not mean that speakers never avoid difficult-to-comprehend utterances. Specifically, the present evidence does not speak to the possibility that speakers cooperatively adjust the syntax of their utterances when they are aware that comprehension failure is likely. Such awareness is likely to be instrumental in the kinds of register differences that were noted above.

Given this perspective on ambiguity avoidance, there are several concerns that deserve mention. First, performance may have been affected by participants having read the sentences before production. Specifically, since one-quarter of the critical sentences in each experiment contained garden paths, it may be that reading a garden path sentence affects the subsequent production of that sentence. One possibility is that a garden path marks a sentence as distinctive when read, so that speakers reproduce it more faithfully upon production. Such a tendency might obscure ambiguity-avoidance effects, since speakers would faithfully reproduce ambiguous sentences.

However, performance across the experiments suggests that this markedness explanation is unlikely to hold. This explanation predicts that sentences presented without a *that* should have higher memory accuracy when the pronoun was ambiguous because speakers would be especially likely to accurately reproduce the ambiguous sentence without a *that*. However, accuracy was not consistently affected by the ambiguity of the presented sentences. Three experiments showed differences in the direction predicted by this alternative explanation, while the other three show differences in the opposite direction. Across all experiments, production of ambiguously presented sentences was only 0.3% more accurate than for unambiguously presented sentences. It thus seems unlikely that reading the ambiguous sentences marked those sentences and thereby affected production in these experiments (though other effects from reading might still be possible). While speakers' reading experience was probably disrupted by the ambiguity of the presented sentence, this disruption seems unrelated to subsequent production.

However, this nondisruption raises a second concern: Perhaps the experiments did not reveal an ambiguity-avoidance effect because the temporarily ambiguous sentences were not disruptive. That is, the ambiguity-avoidance account assumes that speakers will avoid temporarily ambiguous sentences because they hinder comprehension. However, if the particular sentences in these experiments were not difficult, then speakers may not have avoided the temporarily ambiguous versions because they would not have been difficult in the first place.

Experiment 3 was designed to address this concern. There, speakers showed no tendency to use optional words to disambiguate reduced relative clause sentences, standardly assumed to be quite disruptive. Furthermore, the potential difficulty of the sentences used in Experiment 3 was verified with the norming study, which revealed that 63% of participants' responses took ambiguous verbs as main verbs, suggesting that these temporarily ambiguous fragments are indeed biased toward the incorrect main clause interpretation (also, it should be noted that 63% is likely to be an underestimate of the bias, since the ambiguous onsets in the norming study were mixed with unambiguous onsets that were nearly always interpreted as relative clause onsets, which is likely to increase the availability of relative clause interpretations for ambiguous verbs). Finally, F. Ferreira (personal communication, February, 1999) tested the materials of Experiment 3 in an eye-movement monitoring paradigm and found the expected ambiguity effect: Reduced relative clause-sentence reading times were longer than full relative clause reading times, but only when the verb was ambiguous (like *selected* vs. *chosen*).

Nevertheless, it can be argued that production norms do not index online comprehension difficulty and that reading time measures of isolated sentences may not reflect the disruption that comprehenders experience in full paragraphs or discourses, so that the ambiguous sentences used in the experiments may not have been sufficiently disruptive. The present research assumes that to the extent that sentence complement and reduced relative-clause structures engender disruption during typical comprehension, a tendency to avoid that disruption is reasonable to expect (and therefore the absence of such a tendency is notable). The strongest demonstration would measure the production preferences and the comprehension disruptiveness of the same set of sentences with the same subjects, using comprehension measures that are most likely to reflect the difficulties that comprehenders experience during typical language use (e.g., performance during action-based eye tracking). Such verification awaits future research.

Also, it should be noted that these experiments do not address the possibility that speakers may be sensitive to other forms of ambiguity, such as referential ambiguity (when a sentence does not make clear who did what to whom, as in "Bill saw Tom and then *he* punched *him* in the mouth") or discourse ambiguity (i.e., when a sentence is unclear taking into account the

discourse in which it is situated). For example, it may be that speakers only use syntactic devices to circumvent a sentence that is ambiguous with respect to its discourse, so that sentences presented in isolation are never considered ambiguous (though isolated sentences do affect reading time and ERP measures). Furthermore, these experiments do not address the possibility that speakers will use other devices like prosody or gesture to present more easily understood sentences. Whether speakers use prosody to communicate syntactic information in the face of ambiguity is unclear; for example, Albritton, McKoon, & Ratcliff (1996) found that speakers do not use prosody to disambiguate *fully* ambiguous sentences, which suggests that speakers might not prosodically disambiguate the temporarily ambiguous sentences under scrutiny here. On the other hand, Mims and Trueswell (1999) tested the sentence complement temporary ambiguity with ambiguous and unambiguous pronouns, like the sentences tested in Experiments 1 and 2, and found preliminary evidence suggesting that some speakers may use prosody that eliminates temporary ambiguity (though the preliminary results were not significant in conservative analyses). Furthermore, Schafer, Speer, Warren, and White (1999) found that under quite natural circumstances, naive speakers do provide reliable prosodic cues to syntactic phrasing, although they did not address the issue of whether speakers provide *different* prosodic cues for the same syntactic structure under ambiguous and unambiguous circumstances. However, even if other communicative dimensions disambiguate disruptive sentences, it would still be a reasonable production strategy to provide additional cues to structure with the syntactic information signaled with optional function words, since such optional words could be expected to have at least as powerful an influence on comprehension as prosodic information (see Ferreira et al., 1996). The evidence presented here, however, suggests that the syntactic information provided by optional function words does not serve such disambiguation purposes.

Finally, though Experiment 6 manipulated the communicative context of the task, the experimental situation is still notably artificial compared to natural language situations. Addressees rated the form of speakers' utterances rather than acted on the content of their utterances (as in a more action-based experimental task). Thus, Experiment 6 generalizes to performance in Experiments 1–5, but at the expense of straightforward generalization to natural production. Furthermore, in all experiments, conversational factors (e.g., turn taking) were eliminated, speakers' choices of words and structures were highly constrained, and a significant memory component was introduced because of the use of the sentence recall task. Given the powerful influences that conversational pressures have, and the degree to which language users exploit the creativity of language for communication (see Clark, 1996, for a description), the conclusions drawn in this work await further verification from naturalistic investigations before generalization to fully natural production (Craig, Nicol, & Barss, 1995; Elness, 1984).

The results of these experiments might be taken to indicate that speakers are selfish, exploiting the flexibility of language to ease only the task of creating sentences. Such a conclusion, however, overlooks two considerations. First, communicative pressure, as manipulated in Experiment 6, indeed affected optional word mention. That is, speakers can change their overall level of *that*-mention when understandability is important. However, Experiment 6 shows that variability around this overall level of *that*-mention is still affected by retrieval pressures. Second, a range of challenges face language users when communicating. As noted by Clark (1996), one pressure that language users experience in a communicative setting is the need to “hold the floor” in a timely manner. The results of the present experiments are testament to the importance of this pressure. The need for language users to communicate in a timely fashion implies that specific strategies, like the availability-based one explored here, are necessary so that speakers can manage the complexities that are involved in producing language.

Appendix: Stimuli Used in All Experiments

Experiments 1 and 2: Sentence Complement Structures

The unambiguous (Experiment 1: *I*; Experiment 2: *she* or *he*) or ambiguous (Experiments 1 and 2: *you*) pronoun was positioned at the asterisk. Immediately preceding the pronoun was the optional *that*, when present. A hyphen in the Experiment 2 column indicates that the sentence was identical to the sentence in Experiment 1.

Experiment 1	Experiment 2
The therapist felt * had overreacted to the problem.	The choir felt * had a great voice.
The salesman guaranteed * would be completely satisfied.	The sales contract guaranteed * would be completely satisfied.
The speedometer indicated * had travelled too fast.	—
The mayor declared * acted heroically.	The proclamation declared * acted heroically.
The gambler guessed * had bluffed on the previous hand.	The class guessed * had not read the text book.
The receptionist denied * had an appointment.	The assembly denied * had attended the meeting.
The inspector doubted * repaired the porch properly.	The inspector's office doubted * had repaired the porch properly.
The dentist asserted * need to floss more often.	The dentist's analysis asserted * needed to floss more often.
The doctor suggested * consult a specialist.	The medical test suggested * needed a specialist.
The librarian noted * had an overdue book.	The library noted * had an overdue book.
The plumber confessed * had been overcharged.	The telephone company confessed * had been overcharged.
The handwriting analysis revealed * had a creative personality.	—
The psychic predicted * would win the state lottery.	The psychic hotline predicted * would win the state lottery.
The professor realized * studied the material.	The examining committee realized * studied the material.
The attorney argued * had done nothing wrong.	The defense team argued * had done nothing wrong.
The chiropractor observed * couldn't stand up straight.	The dance troupe observed * had a lot of talent.
The paramedic explained * had fainted.	The paramedics explained * had fainted.
The wedding planner proposed * visit the chapel.	The actor's union proposed * represent their interests.

- The investigator learned * misplaced the wallet.
 The surgeon promised * would heal soon.
 The landlord discovered * had a cat.
 —
 The jury believed * told the truth.
 The applicant surmised * had already hired someone.
 The pedestrian maintained * had changed lanes illegally.
 The prosecutor concluded * should gather more evidence.
 The police officer warned * would get a ticket.
 The article emphasized * caught the biggest fish.
 The security guard demanded * leave the store immediately.
 The teacher noticed * skipped class.
 The coach knew * missed practice.
 The power company acknowledged * didn't have electricity.
 The x-ray proved * had a broken bone.
 The dog sensed * might be in danger.
 The boss remembered * had a vacation coming.
 The director announced * would be playing the part.
 The travel agent confirmed * had a hotel reservation.
 The reporter heard * made a large donation.
 The accountant estimated * would pay a lot of taxes.
 The usher insured * could see the ceremony.
 The secretary recalled * had been there before.
 The broker advised * invest in the fund.
 The catcher protested * should have been called out.
 The taxi driver anticipated * would turn left.
 The contest official pronounced * had been disqualified.
 The mechanic mentioned * could use a tune-up.
 The ticket agent feared * would not get a window seat.
 The newspaper reported * had been robbed.
 The pirate suspected * hid the treasure.
- The investigators learned * misplaced the wallet.
 The surgeon's prognosis promised * would heal soon.
 The rental agency discovered * had a cat.
 —
 The fire brigade surmised * had started the blaze.
 The witnesses maintained * had changed lanes illegally.
 The prosecutors concluded * should gather more evidence.
 The alarm warned * should leave the building.
 The article emphasized * won the fishing tournament.
 The security guards demanded * leave the store immediately.
 The T.A.'s all noticed * skipped class.
 The coaches knew * missed practice.
 The power company acknowledged * didn't have electricity.
 —
 The dogs sensed * might be in danger.
 Management remembered* had a vacation coming.
 The casting list announced * would be playing the part.
 The travel agency confirmed * had a hotel reservation.
 The audience heard * lost the speech.
 The accounting firm estimated * would pay a lot of taxes.
 The ushers insured * could see the ceremony.
 The secretaries recalled * had been there before.
 The financial firm advised * invest in the fund.
 The losing team protested * should have been called out.
 The pedestrians anticipated * would turn left.
 The official statement pronounced * had been disqualified.
 The mechanic's report mentioned * could use a tune-up.
 The ticket agency feared * would not get a good seat.
 —
 The pirates suspected * hid the treasure.

Experiment 3: Relative Clause Structures

The second field (e.g., “that was”) comprised the optional material. The first verb listed for each sentence is the unambiguous verb, and the second verb is the ambiguous verb.

- The astronauts (who were) chosen / selected for the Apollo missions made history.
- The actor (who was) chosen / selected to play Macbeth had a strong British accent.
- The team (that was) beaten / defeated in the Super Bowl vowed revenge next season.
- The countries (that were) beaten / defeated in World War II have been rebuilt.
- Children (who are) bitten / attacked by dogs tend to fear them later in life.
- The hiker (who was) bitten / attacked by the snake didn't survive the night.
- The students (who were) given / handed the new assignment were extremely unhappy.
- The baby (who was) given / handed to the politician wailed loudly.
- The child (who was) hidden / concealed under the bed wasn't discovered by the intruders.
- The car (that was) hidden / concealed in the shadows had been vandalized.
- The guy (who was) mistaken / confused for the rock singer was mobbed by the crowd.
- The mailman (who was) mistaken / confused for a cop was killed by a gang member.
- A person (who is) proven / found innocent in court can often seek civil damages.
- The student (who was) proven / found to have cheated was kicked out of school.
- The child (who was) seen / spotted near the train station had been reported missing.
- The man (who was) seen / spotted on the freeway was wanted for murder.
- Some buildings (that were) shaken / damaged in the 1906 earthquake still stand today.
- The cargo (that was) shaken / damaged by the bumpy delivery was refused by the customer.
- The prize terrier (that was) stolen / kidnapped at the dog show was never returned.
- The infant (that was) stolen / kidnapped by the terrorists was held for ransom.
- All hostages (that are) taken / held during wartime are considered prisoners of war.
- A suspect (who is) taken / held for questioning must be released within 24 hours.
- The stories (that are) written / described in the D.I. are usually about campus life.
- The police report (that was) written / described in the paper was needed for evidence.

Experiments 4 and 6: Sentence Complement Structures

Asterisks indicate where the pronouns (*I* or *you*) are placed. The first verb listed is the low frequency verb, and the second is the high frequency verb. Underlined verbs only were used with the corresponding sentence frame in Experiment 6.

- * acknowledged \ indicated * had not studied before the exam.
- * acknowledged \ indicated * had not attended the organizational meeting.
- * advised \ suggested * would need to drive slowly in the bad weather.
- * advised \ suggested * should explore the unknown part of the forest.
- * anticipated \ realized * would get some great presents.
- * anticipated \ realized * would be nervous during the date.
- * argued \ proposed * should get to drive the sports car.
- * argued \ proposed * should accompany the child home.
- * asserted \ mentioned * had done nothing to be arrested.
- * asserted \ mentioned * could play the saxophone really well.
- * concluded \ demanded * should get a pot-bellied pig.
- * concluded \ demanded * should not have to sleep on the floor.
- * confessed \ revealed * saved the drowning child.
- * confessed \ revealed * had been seeing someone else.
- * confirmed \ knew * had booked a flight for tomorrow.
- * confirmed \ knew * got hurt in the car accident.
- * doubted \ denied * agreed to sit through the entire opera.

- * doubted \ denied * won the state lottery.
- * emphasized \ proved * had memorized the words to the national anthem.
- * emphasized \ proved * had nothing to conceal from the police.
- * estimated \ noted * would finish writing the book in six months.
- * estimated \ noted * had plenty of cash in the wallet.
- * feared \ learned * needed to see a doctor about the cough.
- * feared \ learned * had made a big mistake on the test.
- * guaranteed \ declared * would enjoy the delicious food.
- * guaranteed \ declared * would finish the marathon.
- * guessed \ observed * had accidentally reset the computer.
- * guessed \ observed * became upset because of the bad news.
- * insured \ explained * would not lose any money on the investment.
- * insured \ explained * would repay the loan as soon as possible.
- * predicted \ maintained * would get a huge tax return.
- * predicted \ maintained * could easily program the VCR.
- * promised \ believed * would write back by next week.
- * promised \ believed * really got a great deal at the auction.
- * pronounced \ announced * had won the medal of honor.
- * pronounced \ announced * would scale Mount Everest.
- * protested \ noticed * had not really stacked the deck.
- * protested \ noticed * deserved a much better grade.
- * recalled \ remembered * had spent the summer on Cape Cod.
- * recalled \ remembered * had spent all my money on beverages.
- * sensed \ felt * would sleep late the next morning.
- * sensed \ felt * could have a lousy sense of humor.
- * surmised \ discovered * had fallen asleep on the bus.
- * surmised \ discovered * had an article published in yesterday's newspaper.
- * suspected \ reported * had lost my driver's license.
- * suspected \ reported * became mildly ill during the night.
- * warned \ heard * could have an explosive temper.
- * warned \ heard * had been considered a dangerous driver.

Experiment 5: Sentence Complement Structures

Asterisks indicate the location of the pronouns (*I* or *you*).

Sentence set 1

- * reported * feared an IRS audit.
- * emphasized * learned five different languages.
- * charged * replied with offensive language.
- * suspected * felt uncomfortable.
- * checked * figured out the gratuity properly.
- * asserted * doubted the testimony of the witness.
- * believed * insured the antique car for its full value.
- * indicated * explained the situation very clearly.
- * pronounced * maintained a rigid position in the debate.
- * protested * revealed the secret to everyone.
- * realized * sensed the danger of the dark streets.
- * heard * warned the President about the assassination.
- * recalled * noted the mistake in the bank statement.
- * concluded * announced the judge's decision too quickly.
- * acknowledged * knew what would be on the exam.
- * argued * mentioned the surprise party to the birthday boy.
- * confirmed * observed the crime while it was happening.
- * estimated * guaranteed the product for a sufficient period.
- * discovered * proved the existence of Atlantis.
- * guessed * remembered the questions on the SAT.
- * pretended * perceived the dishonesty in her voice.
- * boasted * accepted the big defeat in the tennis match.
- * wrote * appreciated the apology.
- * concealed * overheard the military secrets.
- * admitted * recognized the foreign actor immediately.
- * found * regretted not going on the blind date.
- * repeated * agreed to the questionable contract.
- * disclosed * complained about the boss.
- * insisted * understood the complicated Physics problems.
- * read * disputed the grades of the last exam.
- * worried * confided in too many people.
- * dreamt that * forgot to go to the big concert.

Sentence set 2

- * feared * reported the income tax fraud.
- * learned * emphasized the importance of working hard.
- * replied * charged too much for the computer.
- * felt * suspected our neighbor of committing a crime.
- * figured * checked the apartment for bugs.
- * doubted * asserted the truth on the witness stand.
- * insured * believed in the tooth fairy.
- * explained * indicated the wrong route to the stadium.
- * maintained * pronounced many words in a funny way.
- * revealed * protested against the government.
- * sensed * realized the implications of the election results.
- * warned * heard a false rumor about the assassination.
- * noted * recalled the date of the Challenger explosion.
- * announced * concluded the talks with the union representative.
- * knew * acknowledged the people who deserved credit.
- * mentioned * argued with the professor about the question.
- * observed * confirmed the hotel reservation.
- * guaranteed * estimated the value of the used car correctly.
- * proved * discovered Atlantis.
- * remembered * guessed at many questions on the SAT.
- * pretended * pretended to like jazz.
- * accepted * boasted about winning the tennis match.
- * appreciated * wrote the apology.
- * overheard * concealed the military secrets.
- * recognized * admitted lying to the court.
- * regretted * found the blind date interesting.
- * agreed * repeated the story way too often.
- * complained * disclosed too much sensitive information.
- * understood * insisted on studying for the Physics test.
- * disputed * read all the material for the exam.
- * confided * worried too much about the friendship.
- * forgot that * dreamt about the big concert.

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