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7 Psychological Constraints on Language: A Commentary on Bresnan and Kaplan and on Givón

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Ever since Wundt's debates with the neogrammarians late in the nineteenth century, psychologists and linguists each have been interested in what the other has had to say about language. In Wundt's day, the study of language was a unified field to which both psychologists and linguists contributed. But as the two fields became more specialized, the study of language structure got cut off from the study of language use. Language structure was considered the province mainly of linguists, and language use the province mainly of psychologists.

Despite the split, language structure was still assumed to bear some relation to language use. The best known claim on this issue was Chomsky's (1965): "A reasonable model of language use will incorporate, as a basic component, the generative grammar that expresses the speaker-hearer's knowledge of that language [p. 9]." But for psychologists, it was difficult to see how the grammar Chomsky favored at the time could be incorporated into a theory of language use. There had been several attempts to do this, and all were failures. This was probably why Chomsky cautioned in his very next sentence: "But this generative grammar does not, in itself, prescribe the character or functioning of a perceptual model or a model of speech-production." This was an odd caveat. If the grammar is something that a person "puts to use in producing and understanding speech (Chomsky, 1970)," then shouldn't it partly prescribe, in the sense of constrain, the character or function of speech production and understanding?

The problem is clear. Linguists had generally come to study language structure with little regard for language use. Generative grammars were being devised without considering the role they would play in the models of speech production and understanding. This had to be short-sighted. It was as if one division of

General Motors were designing automobile engines without consulting the division designing the chassis into which these engines would be installed.

With the chapters by Bresnan and Kaplan, and by Givón, we have excellent examples of linguists bucking this tradition. More than most linguists, they are attempting to study language structure in its relation to language use—although their approaches are very different. Bresnan and Kaplan, like many of their predecessors, want to devise grammars of languages such as English. They differ from most predecessors in wanting the grammars to fit directly into models of speaking and listening. Givon, in contrast, wants to show how the functions to which language is put explain the form that various languages have evolved into.

What is common to Bresnan and Kaplan, and Givón, is the idea that the form and function of language are subject to cognitive, or psychological constraints. One of their aims is to describe these constraints and their consequences for language structure. For many cognitive psychologists, the constraints they offer may not look very psychological. They are sometimes couched in abstract formalisms in which the psychological content is obscure; they are generally not based on studies of psychological processes; they are sometimes based on psychological notions that are more speculative than proven. For these and other reasons, many psychologists may be wary of parts of Bresnan and Kaplan's and Givón's enterprises. Should they be?

The basic issue, we believe, is what constitues a "psychological" constraint. Psychologists and linguists expect very different things of such constraints. To bring out the differences, we first present a strong psychological view of cognitive constraints on language: we describe several constraints most psychologists would accept as psychological, see what makes them psychological, and apply these criteria to constraints proposed by Bresnan and Kaplan, and by Givón. Later, we describe what linguists would be more likely to consider a psychological constraint and see how these two views might be reconciled.

FOUR POSSIBLE PSYCHOLOGICAL CONSTRAINTS

In the past few years, there have been many proposals by psychologists and other non-linguists for how language is constrained by psychological factors. The constraints entertained range from perceptual to social constraints, as in this classification offered by Clark and Clark (1977):

> A priori, every human language must be susceptible of: (1) being learned by children; (2) being spoken and understood by adults easily and efficiently; (3) embodying the ideas people normally want to convey; and (4) functioning as a communication system in a social and cultural setting [pp. 516–517].

The same four types turn up in different guises in Slobin's (1979) "ground rules to which a communicative system must adhere if it is to function as a full-fledged human language. [p. 188]." Let us call the four categories *learnability, processibility, expressibility,* and *social utility.*

Most of the constraints in these categories would strike psychologists as clearly "cognitive" or "psychological" in character. To see why, we will take up four proposed constraints, one for each category. The four examples, admittedly, aren't equally convincing or well founded, but our purpose is to see what they have in common.

Learnability: Regular paradigms

According to Slobin (1973), children find it difficult to learn morphological paradigms that aren't regular—that don't adhere to regular rules. In English, for example, the paradigm for possessive constructions is highly regular: To form the possessive of a noun phrase, add *-s* (in its various phonological realizations) to the noun phrase. There are a few exceptions to this rule, such as *my, your, his,* and *their,* but they are few. The paradigm for the past tense of verbs, in contrast, is highly irregular. The general rule is this: To form the past tense of a verb, add *-ed* (in its various phonological realizations) to the present tense form. But for this rule, there are many exceptions—the so-called strong verbs such as *broke, ate, had,* and *rang.* As evidence for his generalization, Slobin noted that English children make systematic mistakes in acquiring past tense verb forms, saying *breaked* for *broken,* and *ringed* for *rang.* He also noted that Turkish inflectional morphology, which is highly regular, is acquired very early compared to English inflectional morphology, which is much less regular. There is other evidence as well.

If regular paradigms are easier to acquire than irregular paradigms, we should find them in abundance in languages, and we do. And all other things being equal, we should find systematic changes in languages toward regular paradigms, and there is considerable evidence on this score too. As reviewed by Bybee Hooper (1979), languages tend to extend regular endings (so-called "analogical extension") and to get rid of alternative endings (so-called "analogical leveling"). In English, for example, forms such as *wrought, dreamt, spelt,* and *shone* have been or are being replaced by their regular counterparts *worked, dreamed, spelled,* and *shined.* (As we note later, there are independent forces that lead to irregular paradigms.) Here, then, is a well documented feature of language learning that has been argued to constrain the form an evolved language can have by constraining the direction in which languages can evolve.

Processibility: Constituent structure

Words and their meanings live only briefly in listeners' short term memories. Not only do the phonetic shapes of words die out very quickly, but so do the several "automatically" available senses of ambiguous words, which disappear within tenths of seconds (see, e.g., Swinney, 1979). If two words need their meanings to be accessed at the same time to be interpreted correctly, they should be processed more efficiently the closer they are in the sentence, and most efficiently when they are adjacent members of the same constituent. The words that most depend on each other this way are those that have the same referents. In the noun phrase *the small elephant,* for example, the interpretation of *small*—a relative adjective—depends on the noun being modified, here *elephant,* and so does the speed of that interpretation (Rips & Turnbull, 1980). Then, all other things being equal, understanding should be optimal when words that are mutually dependent in this way belong to the same constituent.

Very generally in languages, words mutually dependent in this way *are* likely to belong to the same constituent, as in the English noun phrase *the small elephant.* This tendency has long been known in what Vennemann (1974) has called Behaghel's First Law: What belongs together mentally is placed close together syntactically (see also Moravcsik, 1971). As a specific example, take the relations between classifiers (C), quantifiers (Q), and nouns (N), as in the construction *two head of cattle,* which has the constituent structure ((Q + C) + N). As Greenberg (1972, 1975) has argued, the quantifier is semantically dependent on the classifier and not on the noun; hence the quantifier ought to form a constituent with the classifier and not the noun. In many languages, such classifiers are required in every noun phrase that contains a quantifier. In an extensive survey of these languages, Greenberg found no examples of C + N + Q, or Q + N + C, in which C + Q don't form a constituent. All he found were the other four possible orders of C, N, and Q, in which C + Q formed an independent constituent. So it can be argued that the processing needs of the language user constrain the way words get grouped into constituents in all or almost all languages.

Expressibility: Color terms

The components of the human eye are each more sensitive to some colors than others (see Kay & McDaniel, 1978; Miller & Johnson-Laird, 1976). Very roughly, one part of the visual system is geared to represent grays—from black to white—and another part to represent hues. The visual system is particularly sensitive to the four primary hues—red, green, blue, and yellow. If speakers of a language develop words for denoting brightness, the argument goes, they should first code black and white, the two ends of the grayness continuum. If they develop words for hues, they should find it easiest to add red, green, blue, and yellow. And if they develop still more words for hues, they should add names for certain in-between hues, such as brown, pink, grey, purple, and orange.

As Berlin and Kay (1969) demonstrated—with later emendations—languages of the world acquire their basic color vocabulary in a highly regular order. All languages have at least *black* and *white,* or *light* and *dark.* Languages with only three basic color terms also have *red,* and the next three terms to be added are *green, blue,* and *yellow.* The last terms to be acquired are *brown, pink, purple, orange,* and *grey.* The evolution of color terminology, then, follows the psychological constraints put on it by human visual perception.

Social utility: Politeness

In his study on face-to-face interaction in public, Goffman (1967) noted that two people in social contacts try to maintain face—Goffman's notion of the public self-image that people claim for themselves. People try to maintain their freedom of action, their freedom from imposition; they also try to maintain their positive self-image—their desirability to at least some other people. Brown and Levinson (1978) argued that if maintaining face is universal, then there is a set of strategies every language should exploit in polite expressions. For example, every language should have evolved indirect means for making requests. In requests, the speaker is, by definition, trying to impose on the addressee, and this can be threatening to the addressee's face, or self-image. To be polite, the speaker needs to reduce the threat and hedge his request—for example, by giving the addressee the option of not complying.

According to Brown and Levinson's survey of three unrelated languages—English, Tzeltal, and Tamil—this sort of device is universally used to enhance politeness in requests. In English, it is more polite to ask *Can you open the door?,* which gives or appears to give the addressee the option of not complying, than to say *Open the door,* which doesn't give him that option. These two examples translate almost directly into Tzeltal and Tamil, with the identical consequences. There are parallel realizations in the three languages of many other face-saving and face-preserving strategies too. Thus, a psychological process is argued to lead to a universal class of devices for requesting favors and for other face-threatening speech acts.

WHAT IS A PSYCHOLOGICAL CONSTRAINT?

These examples, whatever their status, illustrate the strong position psychologists could take toward "psychological" or "cognitive" constraints on language. They bring out features they might see as desirable, even necessary, for a constraint to be considered truly psychological. We will draw out four such features under the labels empirical grounding, structure independence, theoretical coherence, and linkage.

Empirical Grounding

A psychological constraint ought to be grounded in empirical findings. In support of the claim that regular paradigms are easier to learn than irregular paradigms, Slobin referred to a plethora of evidence. For the primacy of black, white, red, blue, green, and yellow in the visual system, Kay and McDaniel, and Miller and Johnson-Laird, pointed to an even broader and more substantial range of evidence. The evidence for the fast decay of phonetic shapes and meanings in short-term memory is quite firm, with new evidence appearing yearly, and the evidence for the psychological process of maintaining face, though of quite a different kind, is substantial too.

All this evidence has been verified for a variety of cultures and languages—or can be assumed to hold across language groups. Slobin's evidence is explicitly taken from many language groups. The psychophysical evidence on colors and the properties of short-term memory shouldn't depend on the language a person speaks. Maintaining face, like other anthropological features, may not be universal, but the claim is only that in those cultures in which it occurs, certain features should appear in the language. (Note that if there were a race of colorblind people, we wouldn't expect the same color names either.) The point is that if we wish to maintain that something is a universal constraint on language, we must show that it holds cross-linguistically and cross-culturally.

Structure Independence

All four constraints we have reviewed were grounded on evidence derived not from facts about the structure of language, but from facts about processes in language use. The evidence was, as we shall put it, *structure independent:* it was independent of language structure per se. The constaint on color terminology was derived from physiological and psychophysical evidence that has nothing to do with language at all, and the constraint on maintaining face came from observations of people in face-to-face situations, with and without the use of language. The constraints on regular paradigms and short-term memory take a little more explanation.

In the evidence on learning regular paradigms, Slobin didn't appeal to facts about language structure per se; for example, he didn't appeal to the presence or absence of regular paradigms in various languages of the world. Rather, he appealed to facts about how children learned paradigms with and without exceptions—how often they made errors in learning various paradigms. He could even have appealed to experimental analogues of learning regular and irregular paradigms of nonsense words, which show results parallel to the findings on children (Palermo & Eberhart, 1968; Palermo & Howe, 1970; Palermo & Parrish, 1971).

The constraints from short-term memory too are based on structure independent sources. The evidence that the phonological shapes of words in short-term memory are transient comes from studies on genuine utterances, on isolated words, on nonsense phonological strings, and on digits. It doesn't come from evidence about what constitutes a sentence or other language structures. The availability of multiple word senses also comes from studies on utterances, on isolated words, independent of language structure. Even though this evidence

pertains to language, it is structure independent if it is evidence of psychological processes behind speaking, listening, and acquisition.

The point of this criterion is to keep psychological constraints from becoming vacuous. Imagine a linguist—let us call her the autonomous linguist—who is not in the least interested in explaining language structure by way of psychological constraints. She is interested only in getting as general a description of language structure as possible by referring to other facts about language structure. In deciding how to write a rule in English, she might note that Finnish, Japanese, and Dyirbal all require a certain form of rule, hence the English rule would be most general if it were in the same form. She would be appealing here to facts about language structure as an "explanation" for her particular formulations of the English rule.

But as evidence for a *strong* psychological constraint (in the sense we have characterized it), this reasoning is circular. A feature found in all languages is prima facie evidence that there *may* be a psychological constraint leading to that feature, but the feature itself doesn't constitute the constraint. To claim it does would be to fall prey to the fallacy *post hoc, ergo propter hoc*. Suppose the autonomous linguist discovered Berlin and Kay's facts about the universality of *black, white,* and the other color terms in languages of the world. It would be circular for her to posit a psychological constraint expressed this way: "All languages must have color terms for black and white, and the next four terms acquired must be ones for red, green, blue, and yellow." This is surely just a redescription of the data. As Givón noted, the autonomous linguist's constraints are constraints on the *description* of language structure, not on its explanation. By this criterion, a good number of what passes for psychological constraints in the linguistic literature go by the board.

Theoretical Coherence

Most psychological constraints don't lead to single isolated features of language—like piano keys that each produce just one note. Constraints such as short-term memory, and color vision, generally have a host of consequences—like organ keys that trigger rank upon rank of pipes. One must show how each constraint fits within a coherent theory of psychological constraints.

Consider Brown and Levinson's theory for the expression of politeness in language. The constraint they posited wasn't intended to account just for indirect requests. It had many different consequences—in the form of different politeness "strategies": presuppose common ground, joke, use in-group identity markers, exaggerate interest in the addressee, give reasons, seek agreement, hedge, be pessimistic, show deference, apologize, and so on. Or consider color terminology. In their arguments, Kay and McDaniel (1978) could have restricted themselves to the primacy of black, white, red, blue, green, and yellow in the visual system, but they recognized that their account ought also to fit into a broader account of vocabulary acquisition. Reasoning this way, Brown (1977, 1979) has appealed to related principles in accounting for the order in which languages acquire the botanical terms for tree, shrub, grass, herb, and vine and the zoological terms for bird, snake, fish, bug, worm, and mammal. Rosch, Mervis, Gray, Johnson, and Boyes-Braem (1976) have expanded on the criteria that appear to apply in determining which categories of objects get "basic" names and which do not. So the psychological constraints on politeness and on color terminology aren't isolated constraints. They each belong to systems intended to explain a range of linguistic features.

Linkage

For a psychological process to explain a feature of language, it must be accompanied by a theory about how it is *linked* to the language feature. It isn't enough to say that languages have regular paradigms because children learn them more easily than irregular ones. One must specify how children's learning brings this about. Providing such a link isn't easy, and it is the part of the argument most often left unspecified. The linkage between constraint and language feature almost always requires a complex model about why languages have the features they do.

What, for example, *is* the linkage between children's learning and regular paradigms in language? Bybee Hooper (1979), Bybee and Slobin (1982), and Slobin (1977) have proposed a theory in which children's (and adults') overregularizations get incorporated into languages through a process of language change. In support of their argument, they have appealed to evidence of how languages change in the directions predicted by their theory. Slobin has also appealed to evidence on the creation of pidgins and creoles. Most linkage theories will have this character. They must explain how a psychological constraint molds the creation of languages, as in the invention of pidgins, and how it shapes the evolution of languages both in ordinary language change and in the creolization of pidgins.

The linkage problem is complicated by the fact that language is influenced by many diverse, often conflicting, psychological constraints operating at once. If the push toward regular paradigms from children were the only influence on paradigms in languages, after so many years of evolution there should be only highly regular paradigms. But as Vennemann (e.g., 1973) has argued, another force on paradigms is phonological reduction. Over long periods of time, suffixes tend to get reduced phonologically and even disappear. This is the way English has lost its case and gender systems, and Dutch is in the process of losing them now. When this happens, languages also tend to find other ways of expressing what was lost. Suppose English eventually loses its plural suffix *-s*, as is already happening in certain non-standard dialects. Then English will have no way of marking singular versus plural, and if it is like other languages, it will evolve a motley collection of new devices to do the work of the old. In this way, English will have evolved a highly *irregular* paradigm for the plural, but because of a *different* set of psychological constraints—those that lead to phonological reduction and to semantic contrast. Vennemann has argued that these forces of language change are cyclic. So for the linkage theory to be complete, it must specify how the relevant constraints interact.

To summarize briefly, we suggest that a strong psychological constraint on language has four properties. It is grounded on firm psychological evidence. The evidence it is grounded in is independent of language structure. It belongs to a coherent theory that leads to a body of related predictions. And its linkage to language structure is explicit and theoretically sound. These four criteria, as ideals, will rarely be met in practice. Yet they provide a standard for examining any psychological constraint that is proposed.

BRESNAN AND KAPLAN ON PSYCHOLOGICAL CONSTRAINTS

In their chapter, Bresnan and Kaplan take up the issue of psychological reality in grammars: What is the relation of grammars to theories of speaking, understanding, and acquisition? Briefly, they argue that the grammar of a language should represent the linguistic knowledge people use in speaking and understanding it. Certain forms of grammar, such as transformational grammar, cannot in principle represent that knowledge. Indeed, if one looks at language processes, one can posit certain "theoretical" constraints that grammars must adhere to. So the relation between linguistic knowledge, as represented in grammars, and language processes, as represented in theories of speaking and understanding, is reciprocal: each constrains the form the other can take.

Bresnan and Kaplan's arguments are inventive and clear, and there is much in them we can only agree with. Yet as commentators, we will take a harder look at some of the assumptions behind the arguments, especially behind what they call "theoretical" constraints. In the original version of the chapter—the version for which we first prepared comments—these were called "cognitive" constraints, which is what piqued our interest. Despite the change in name, the issue remains the same: What is the grounds for positing these constraints?

The Competence Hypothesis

The superordinate constraint behind Bresnan and Kaplan's enterprise is, as they say, Chomsky's *competence hypothesis:* "A reasonable model of language use will incorporate, as a basic component, the generative grammar that expresses the speaker-hearer's knowledge of the language (Chomsky, 1965, p. 9)." This hypothesis can be taken in many ways, and they rightly argue against Chomsky's position that, as they say, "psychological reality [of grammars] is whatever linguistic theory is about." They go on to argue, "[The competence hypothesis] requires that we take responsibility not only for characterizing the abstract structure of the linguistic knowledge domain, but also for explaining how the formal properties of our proposed linguistic representations are related to the nature of the cognitive processes that derive and interpret them in actual language use and acquisition."

The competence hypothesis is clearly a psychological constraint. If grammars must be incorporatable into models of language use, then only certain forms of grammars, and languages, are possible, because people can only process certain types of utterances. Part of Bresnan and Kaplan's arguments against transformational grammars is based on this logic. But if the hypothesis expresses a psychological constraint, how does it fare against the four standards we listed? The problem is that it has rarely been held up to standards like these, because most work on grammars is done by linguists whose interests lie elsewhere. Still, Bresnan and Kaplan are explicitly concerned with it as a constraint on language, so it is appropriate to ask the question anyway.

The competence hypothesis is susceptible to test both as a whole and in part. We will take up just one part—central to the hypothesis—as expressed in the *single representation* assumption: the representation of grammatical knowledge is the same, or isomorphic, in speaking and understanding. Of course, "it is uncontroversial that stored knowledge structures underlie all forms of verbal behavior," as Bresnan and Kaplan say. But it isn't so uncontroversial that the *same* stored knowledge structure underlies all these forms.

There are a number of facts people take for granted that suggest quite the reverse. A native Californian can understand a range of accents of spoken English—Australian, Indian, Scottish, the American south—yet not have the slightest competence for producing them. He can understand syntactic forms in these dialects, as well as in Shakespeare, Joyce, and even Bellow, over which he has no productive control. He can understand a large number of words—much of his recognition vocabulary—that he couldn't use himself. His deficiencies in production lie in syntax, vocabulary, morphology, phonology, and semantics, suggesting that at all levels of language structure, the process of listening has access to more "knowledge" than does the process of speaking. There are also systematic differences between what children can understand and what they can produce (E. Clark & Hecht, 1983). As with adults, the ability to comprehend particular constructions precedes the productive control over the same constructions.

These observations can be viewed several ways. One is to suppose there is a body of linguistic knowledge people can access in understanding but not in speaking. This premise allows one to preserve the single-representation assumption. A more radical view is to suppose that comprehension and production access distinct representations of linguistic knowledge, even though, in normal people, the two representations code much the same information and are closely coordinated: people use their comprehension system to monitor and adjust what they produce, bringing production into line with comprehension. Under this view, the single-representation assumption is incorrect.

As evidence for the more radical view, consider phonology. The processes of hearing speech sounds—all the acoustic, phonetic, and phonological processes that investigators of speech perception have learned so much about—bear little resemblance at any level of abstraction to the processes of sound production—planning phonetic sequences, creating articulatory programs, and executing these programs. The first involves the ear and theories of auditory perception, and the second, the mouth and tongue and theories of motor movements. The two processes appear to involve distinct parts of the cortex as well. All that theories of phonetic perception and phonetic production need have in common is that the phonemes identified in perception, when veridical, are the same phonemes the speaker intended to articulate. Even the intention to produce a phoneme, and the recognition of that intention, need not make reference to the same representation, as long as they are coordinated in some way. In any case, the language representations that the two processes make reference to in realizing and recognizing these intentions don't need to look alike.

As an analogy, consider a television set that produces images and an eye that interprets them. The representation to which the television "refers" in its complicated chain of electrical activity may not look at all like the representation that people make reference to in recognizing the objects being imaged. Yet if we erected a window between the television set and the viewer, we could describe at many levels of abstraction the patterns of light passing through the window—according to the intuitions of the viewer. In a way, this is what grammar writers do. They describe, in a relatively neutral representation, the sound patterns that emerge from the mouth and strike the ear and how these are correlated with the speaker's meanings. That representation may not describe either what is in the producer of those sounds or in the perceiver. Indeed, they cannot describe both if the "knowledge" exploited in speaking is not the same in all respects as that exploited in listening. We can't assume that just because the production and comprehension systems coexist in the same brain, or are under the control of the same mind, they share a single representation system. The motor and perceptual systems in the brain are distinct, so why shouldn't their linguistic components—the production and comprehension systems—be distinct too.

So much for the empirical grounding of the competence hypothesis. How does it fare against the second, third, and fourth of our criteria? Any empirical grounding we could think of would be structure independent. Until there is an explicit model of how linguistic knowledge is put to use in models of speaking and understanding, it is impossible to ask about theoretical coherence. Probably the most challenging criterion is linkage, and on this, there has been some work. Several investigators have studied whether transformational grammar (Wexler & Cullicover, 1980) and Bresnan and Kaplan's lexical functional grammar (Pinker, 1980) are in principle learnable by children, and this is a first step in linking the competence hypothesis to the possible forms a language can take. Yet these investigations haven't been concerned with specific models of speaking and understanding, nor with theories of language change or language creation (in pidgins and creoles).

The fate of the competence hypothesis lies ultimately in the models of speaking and understanding that achieve practical success. The way most models are being developed today, they will probably *not* make reference to identical or isomorphic representations of linguistic knowledge, even though the form they take will be constrained by the grammar. If this happens, the competence hypothesis will be robbed of its central assumption.

Creativity and Finite Capacity

Creativity and finite capacity are two constraints that lead to important decisions about how to write grammars. Creativity specifies that the grammar must be capable, in principle, of producing an infinite number of grammatical strings. Finite capacity has two parts. Part 1 specifies that words and syntactic relations must be finite, and Part 2 specifies that people's *mental* capacity for storing knowledge must be finite. As a result of creativity and finite capacity, Bresnan and Kaplan argue, "the mapping [from representations to sentences] must consist of the recursive composition of finitely many operations that can project a finite store of knowledge of a particular language onto infinite sets of data [Bresnan & Kaplan, p. 123]."

As strong psychological constraints, these two constraints are circular. Creativity is really an observation about language structure—that there seems to be no principled limit on the amount of recursion possible in languages of the world. So is Part 1 of finite capacity, which expresses the observation that all languages consist of a finite vocabulary put together by a finite set of syntactic operations. A grammar would need to reflect both creativity and finite capacity Part 1 just to be an adequate description of a language. So creativity and finite capacity Part 1 fail on the criterion of structure independence.

Part 2 of finite capacity, that people's mental capacity for storing knowledge is finite, is clearly a psychological constraint with much empirical support. But its linkage with linguistic phenomena is problematic. Bresnan and Kaplan assume that if a language is creative and uses finite means, it must be recursive. This clearly needn't be so. In American sign language, signers could in principle exploit the ability to mimic, through infinitely gradable gestures, any movement they wished to denote. In English, too, we could use vowel and fricative length in an infinitely graded way to represent, say, the physical extent of some object. With such analogue devices, signers and speakers could get infinite expressibility from finite means without the use of recursion. The point is that, in principle, a language could fit the creativity and finite capacity constraints and yet not have recursion.

The way Bresnan and Kaplan use finite capacity is to constrain the sets of elementary words and relations to be finite, not the possible length of sentences or depth of recursion. Bresnan and Kaplan could have claimed instead that the set of elementary words was infinitely expandable and used finite mental capacity to constrain sentence length and depth of recursion. Applying finiteness of mental capacity to elementary words and grammatical relations and not to sentence length or depth of recursion was a strategic decision made to fit the observed recursion in language. The decision is the right one, but it is based on adequacy of description—a structure dependent criterion—not on the constraints per se.

Reliability

According to this constraint, "the syntactic mapping can thus be thought of as reliably computing whether or not any string is a well-formed sentence of a natural language." As evidence, Bresnan and Kaplan suggest that "independently of knowledge of specific context, even independently of meaningfulness, speakers can reliably classify sentences as grammatical or ungrammatical," suggesting that "classification of strings as grammatical or ungrammatical is based on an automatic procedure."

The alternative, as Bresnan and Kaplan point out, is that language users use so-called heuristic strategies—strategies that do not constitute an "effectively computable characteristic function." It has long been argued, of course, that listeners use such strategies (see Clark & Clark, 1977; Fodor, Bever, & Garrett, 1974, for reviews), and computational systems have been implemented that understand almost entirely on the basis of non-syntactic information (see Birnbaum & Selfridge, 1981; Reisbeck & Schank, 1978). Even if listeners use non-heuristic procedures, they also use heuristic procedures. In some cases, contrary to Bresnan and Kaplan's claim, they even seem incapable of accessing the appropriate recursive procedure. How else are we to explain why many of Wason and Reich's (1979) informants could never see what was wrong with *No head injury is too trivial to ignore?* Or why many of Gleitman and Gleitman's (1970) informants consistently misinterpreted noun compounds like *bird-house boot?*

Bresnan and Kaplan, of course, don't deny people use heuristic procedures. But to maintain the reliability constraint, they must demonstrate that there exists *no* syntactic construction that is consistently interpreted by means of such procedures. Given how widespread, useful, and powerful such procedures are, that proposition seems implausible.

Order-free Composition

With order-free composition, the grammatical relations derivable from an arbitrary fragment of a sentence, like *not told that,* must be included in the grammatical relations derivable from the entire string, like *I was not told that she was coming*. When people are given *not told that,* they can compute all grammatical relations the fragment could ever have as part of a complete sentence. To figure out the possible relations, they don't need the complete sentence at once.

As empirical grounding for the constraint, Bresnan and Kaplan argue that "complete representations of local grammatical relations are effortlessly, fluently, and reliably constructed for arbitrary segments of sentences." Although this holds for many segments, Bresnan and Kaplan provide their own counterexample with *to by for,* which, as they note, could be a fragment of *The one that he should be spoken to by for God's sake is his supervisor*. It seems unlikely that people could compute, or conceive of, all the possible relations derivable from this segment. If we allow for unlimited embeddings, the possibilities are indeed infinite. It might be countered that people can't compute them because of "performance limitations"—that is, because they are limited by *other* psychological constraints. If so, the empirical grounding for order-free composition is incomplete: Some local grammatical relations are computed "effortlessly, fluently, and reliably," and others are not. Without a model of how the process is otherwise constrained, we can't tell whether the data support, or disconfirm, order-free composition.

Bresnan and Kaplan's real motivation in proposing the constraint appears to be computational and, therefore, not structure independent. It would be convenient for writing grammars if, as Bresnan and Kaplan argue, "sentential context may determine the *choice* of one of a set of locally computed grammatical relations for a segment, but the computation of grammatical relations for a segment may not involve the computation of the grammatical relations of the context." Like recursion, languages appear to allow this possibility in principle, so it is reasonable to require order-free composition of a grammar. That makes order-free composition a constraint on possible *descriptions* of languages, not a psychological constraint on the form languages can take.

So as desirable as order-free composition might be for writing grammars, it doesn't seem to be a *psychological* constraint by the strong standards set up earlier. It doesn't seem to express a mental capacity, or ability, or bit of competence so much as it characterizes a property of languages.

Universality

In the universality constraint, the procedure for grammatical interpretation is assumed to be the same for all natural language grammars. There is assumed to be a "universal effective procedure" for constructing mental representations for sentences. The idea is that there is a universal mental representation for natural languages that is induced by a universal learning function. It is also plausible, then, that the mapping induced by that learning function is also universal.

This constraint is motivated by psychological concerns. The autonomous linguist could, if she wished, design very different grammars for each language, proposing, for example, one kind of grammar for mainly free-word-order languages and another for fixed-word-order languages. But if grammatical representations play a role in speaking and understanding, Bresnan and Kaplan suppose, it is implausible that people's mental representations should take radically different *forms* depending on the language they speak.

The grounds for this constraint, then, is plausibility. But is the alternative so implausible? Even within English, different people could well process utterances differently. Consider the evidence, reported by Peters (1977), that different children learn their first language according to different styles. Some are analytic about word structure, whereas others treat words as Gestalts. If, as Bresnan and Kaplan suppose, the procedure for constructing mental representations is induced by the learning function, then these two groups might develop mutually exclusive strategies for handling certain structures, ending up with two distinct representations of language, both of which, however, fit English in use. Indeed, adults with varying amounts of spatial ability have been shown to use fundamentally different strategies in certain areas of comprehension, one type relying heavily on spatial abilities and the other type not (MacLeod, Hunt, & Mathews, 1978; Mathews, Hunt, & MacLeod, 1980). In speaking spontaneously, adults also appear to hesitate, monitor, and correct themselves according to consistently different styles (Maclay & Osgood, 1959). Both adults and children are known to vary tremendously in size of vocabulary, fluency, size of short-term memory, and spatial abilities, and languages have surely evolved to accommodate this variation. They could also have evolved to accommodate distinct types of mental representations, as may be needed to account for the differences among children, and among adults, in both comprehension and production. If it is reasonable to assume heterogeneity in language processing, it is also reasonable to assume heterogeneity in procedures for grammatical interpretation from one person to the next.

By the same argument, the principal procedures used for grammatical interpretation could vary from language to language, too. Speakers of a mainly free-word-order language might exploit a class of procedures that speakers of mainly fixed-word-order languages never use. If this turned out to be true, there would be little reason for constraining the procedures to be the same in the two languages.

So Bresnan and Kaplan's five theoretical constraints on grammars don't fare too well against the four standards set out earlier, and the explanation is obvious. They weren't designed to. They were motivated not so much by psychological concerns—by examining psychological theories to see how they might constrain grammar—as by linguistic concerns—by trying to rationalize the constraints that languages seemed obviously subject to. We return to this alternative approach to psychological constraints later.

GIVÓN ON PSYCHOLOGICAL CONTRAINTS

Givón, in his chapter, takes a very different tack to the study of language. His main goal is to contrast "formal-logical systems" of representation with pragmatic systems. He argues that, because formal-logical systems are fundamentally closed, context-free, discrete, and based on deductive inference, they are inadequate as representations of pragmatic systems of language, which are fundamentally open-ended, context-dependent, continuous, and based on inductive inference. To make this argument, he appeals to several case studies in "the meaning system in natural language." Unlike Bresnan and Kaplan, Givón doesn't explicitly state psychological constraints for his proposals. Yet the arguments he advances presuppose such constraints, which he exploits in many explanations.

What aspects of language is Givón talking about? One of the most basic distinctions in linguistics is between form and content, or between structure and function—that is, between the structure of what is produced (phonetic segments, words, constituents, sentences, and so on) and the function or use to which that structure is put (to refer, ask questions, denote, and so on). Although language function may be open-ended, context-dependent, continuous, and inductive, language structure certainly isn't. English, like apparently all languages, has a finite vocabulary of elementary words and a small number of syntactic devices for combining words. When Givón argues that language ought to be treated as open-ended, he can only be talking about *function.* Givón's complaint, then, comes down to this. The finiteness, context-independence, discreteness, and deductive nature of linguistic form have often been assumed to hold for linguistic function as well, and any model built on this assumption is necessarily incorrect.

The bulk of Givón's chapter is devoted to showing that language (read "language function") is at every point open-ended, context-dependent, continuous, and inductive. For each set of functions he considers, he argues: (1) here is a well-known property of language structure; (2) it reflects a continous, open-ended, context-dependent, or inductive set of functions; and (3) here is a plausible psychological process that would lead to 2. We consider this argument, because it is used repeatedly, against our four standards—empirical grounding, structure independence, theoretical coherence, and linkage. We select only a few of Givón's many examples to illustrate his appeal to psychological constraints.

Co-reference and Definite Description

The empirical grounding Givón appeals to are almost always facts about language use. In his discussion of co-reference and definite description, he uses a variety of informal examples to argue that reference devices vary along a dimension of "identifiability of referent" or "surprise value of referent." In English, the most identifiable and least surprising referents are introduced with null anaphora—ordinary ellipsis—and the least identifiable and most surprising with definite NP's in left-dislocation, as in *My sister, she just left.* Givón puts four other intermediate constructions between the two extremes. He uses the examples to show that the speaker's choice along this continuum depends on his belief about the hearer's ability to identify referents unambiguously, the hearer's ability to follow the thematic content of the discourse, the speaker's knowledge of the hearer's expectation about themes and topics in the discourse, and other such things. All these factors, he suggests, are characterized by the four properties of pragmatic systems he has given, which amounts to the claim that the referential devices lie on the continuum they do because speakers need to be able to make graded distinctions in "identifiability" or "surprise value."

Givón's evidence here is structure independent, because it was derived from observations about language use and not merely language structure. Further, Givón has offered what is really psychological evidence for each of the factors suggested. He provides fragments of discourse in which, say, stressed and unstressed pronouns are used, and he asks us, his readers, to go along with his intuitions about how these pronouns would be interpreted. Through a network of examples, he argues for the factors that go into his "identifiability" continuum.

Although identifiability and surprise value have some empirical grounding, they don't come from a clear psychological theory about how they combine to determine the form of reference. What Givón's examples show is that people make certain distinctions among the six syntactic forms he discusses, and that very roughly, they fall along a continuum of "identifiability" or "surprise." But this continuum probably consists of a number of overlapping scales collapsed onto a single scale. For example, a speaker could use *the woman* instead of *she* for many reasons. He may believe that the woman being referred to isn't readily available in the listener's memory—isn't "on stage in consciousness" as Chafe (1974) would say. Or he may believe that, even though this woman is readily available in memory, the listener might confuse her with another woman also readily available in memory. Or, even though he believes the woman is readily available, he wants to indicate a change in topic. Each of these reasons is different, and they cannot all be called "identifiability" or "surprise." The same goes for many other contrasts that are collapsed in this continuum.

The continuum-like appearance of these six forms may arise from their hierarchical nature. The main contrast on the scale is between pronouns (forms 2 and 3 on Givón's continuum) and definite descriptions (forms 4, 5, and 6). If the use of any pronoun presupposes "ready availability in consciousness," then that single contrast accounts for the main break in the scale (between pronouns and definite descriptions). All other contrasts among the six forms must then presuppose at least this contrast. The scale only *looks* continuous, then, because the individual contrasts are hierarchically nested, with some presupposing others.

As for linkage, Givón says little, here at least, about how the speaker's desire to distinguish along the continuum of "identifiability" or "surprise" happens to map onto these six forms. Intuitively, the idea is probably that the more uncertain the speaker is that his addressee will be able to identify his reference, the more information he will include in his reference phrase. Indeed, the six forms lie on a continuum of how much information they express and at what point in the reference process. But how did English, for example, happen to evolve these six forms instead of two, or thirty, other forms, which might also vary this way in informativeness? For linkage to be complete, this question needs answers for languages in general.

Lexical Meaning

For quite a different attack on "formal-logical systems," Givón argues that word meanings are inherently context-dependent and open-ended. *Slowly* means different things depending on the type of change or event or movement involved, the norm or average speed for that type of movement, and so on. Words like *hill, mound, heap, pile, peak,* and *mountain* are applied according to inherently fuzzy criteria for dividing up the corresponding conceptual domain. From these and many other examples, Givón concludes, "Meaning is in principle a pragmatic matter, a frame-dependent entity." Yet there are "great areas in our cognitive map where *relatively stable* frames have been established by the organism, most obviously in the areas of our construing the physical universe [Givón's emphasis]."

Givón appears to suggest that these features of language—context dependence in the lexicon along with areas of stability—are a consequence of the way people are, of a set of cognitive constraints on language. We might state the main two constraints implicit in Givón this way: People don't think in discrete categories, yet they establish relatively stable categories in certain areas of thought. Givón doesn't discuss evidence for these notions except to take up the informal language examples and show how they suggest lack of discrete categories. As for any empirical grounding for the psychological claims here, then, Givón provides almost none.

What is striking about the lexicon of a language like English, given Givón's views, is how regular and stable it is. What *slowly* is used to mean on an occasion does depend on the context, but in a highly predictable way: if something is slow, it is below the average or normal speed expected of that type of object in this context. The same remarks apply to other words and their meanings. That is, most words have conventional meanings, presumably listed in people's mental lexicons, that are *not* open-ended and context-dependent; nevertheless, they can be used on particular occasions to denote meanings that are highly open-ended and context-dependent (Clark & Clark, 1979; Clark, 1978, 1983; Nunberg, 1979). There is something right about the long-held intuition that words like *dog* and *slowly* have sets of stable meanings. What needs to be explained is how they can have stable meanings, yet be used in such highly context-dependent ways.

With this added complication, it is hard to see how Givón will link his apparent assumption that people don't think in discrete categories with the way language is—both stable yet context-dependent. How should languages change because of this? So, however convincing Givón is on the context dependence of utterance meaning, he doesn't let us see beyond to a genuine theory of word meaning and word use, and how this theory may be a consequence of his assumptions.

Language Change

When Givón speculates on the history of certain changes in meaning, morphology, syntax, and word order, he appeals to a variety of psychological constraints. He suggests, for example, that *can* and *know* derive from the same Indo-European root, having got split from one another through reanalyses forced by such pragmatic inferences as "If one *can* do something because one *knows* how to do it, perhaps one can do it for other reasons as well, such as: (1) physical/mental power; or (2) being unrestrained." In metaphoric extension, he appeals to "contextual inferences" that involve "the pragmatic judgment of 'relevance' and 'similarity.'" In morphological and syntactic changes, he appeals to such inferences as "If an object is *dative,* it has a high probability of also being *human* and *definite.*" and "If the *identity of the agent* is to be *suppressed,* the next most likely participant in the clause will be *likely* to become the *topic* of the clause [all emphases are Givón's]." In explaining a certain word order change, Givón appeals to an " 'over-kill' communicative strategy, whereby the speaker decides that—just for safety's sake—he will use a more marked device to insure beyond a shred of doubt that the hearer got the message."

The empirical grounding for most of these constraints doesn't exist. Many seem to have been assumed just to make Givón's analysis work; in that way, they are structure dependent. Others, such as the idea that people can and do judge "relevance" and "similarity," are surely correct, but in the form given don't constrain much. Still others, like the "over-kill communicative strategy," are at best doubtful. Contrary to the over-kill strategy, for example, speakers normally give no more information than is needed—it's impolite to provide too much information, which may implicate that the listener is incompetent—and, instead, let listeners *ask* for more information as they need it (see, for example, Sacks & Schegloff, 1979).

Givón would probably be the first to say that the theoretical coherence and linkage of these constraints with language universals has yet to be worked out. What he has given us is not so much a model of how pragmatics constrains language, but an illustration of how pragmatics might conceivably constrain language. Still, arguments of this kind would fare better if they stuck closer to highly plausible constraints related to well established psychological processes.

Givón, to summarize, appeals to psychological constraints of many types, but the constraints rarely live up the four standards of strong psychological constraints. Most are not well grounded in independent empirical evidence. Most do not belong to explicit theories about how psychological constraints shape language change, or mold creoles that emerge from pidgins. In Givón's approach, there remains a wide gulf between the "psychological constraints" appealed to and the "psychological constraints" most psychologists would want to call their own.

STRONG AND WEAK CONSTRAINTS

Although Bresnan and Kaplan's and Givón's ultimate aims are very different, they approach psychological constraints in much the same way. They examine language closely for features they can quite safely claim to be universal and then posit psychological or cognitive constraints that might plausibly explain them. Bresnan and Kaplan focus mainly on syntactic features, whereas Givón casts his eye over a range of features of both form and function. We have tried to point out how far these constraints are from those we have called strong psychological constraints. To keep them distinct, we might call the constraints proposed by Bresnan and Kaplan, and by Givón, *weak* psychological constraints.

Although we have been championing strong psychological constraints, there is a clear place in the study of language for weak psychological constraints too—so long as they are seen for what they are. What *are* they? If Bresnan and Kaplan's and Givón's constraints are any example, they are conjectures, proposed mostly on the basis of observations about language and its use, about how the mind must be constituted for language to be the way it is.

Reasoning from language universals to potential psychological constraints can lead to powerful conjectures. When Berlin and Kay (1969) discovered there was but a small set of color terms used by all languages, it was easy to conjecture that the color vocabulary was constrained by the nature of the visual system, and it remained for Kay and McDaniel (1978), Miller and Johnson-Laird (1976) and others to provide the psychophysical rationale. In this way, a weak psychological constraint was promoted to a strong one. Not all conjectures have been followed up so directly. Greenberg (1963), in his classic article on universals of word order, wrote informally of harmonic and disharmonic relations—for example, how an adjective-noun order in noun phrases was "harmonic" with a possessive-noun order in noun phrases. Greenberg clearly had in mind a type of psychological constraint: harmonic relations are more easily processed than disharmonic relations. Bartsch and Vennemann (1972) later raised Greenberg's harmony to the status of a "principle of natural serialization," which they clearly intended to be a psychological constraint—a weak one in our sense. Unfortunately, there have been no attempts to find structure-independent evidence for this constraint, though such evidence would take us a long way toward

explaining both sides of the problem at once—the psychological processes that lead to harmonic relations, and the explanation of the harmonic relations. So weak psychological constraints may have their most important value as provocateurs: they goad us to look for strong psychological rationales for universal features of language.

Not everyone sees language universals this way. There have evolved two very different working assumptions about the origins of language universals, and these have led to a good deal of misunderstanding. Most linguists and some psychologists work from what might be called *Chomsky's wager*.

> It is highly likely that most aspects of language that are universal are a result not of general cognitive constraints, but of constraints specific to language functions—specific to an autonomous language faculty. It is therefore appropriate a priori to assume autonomous psychological constraints and to leave it to others to prove otherwise.

Many psychologists and some linguists, on the other hand, make the opposite bet, which might be called *Wundt's wager*.

> It is highly likely that most language universals are a result not of linguistically autonomous constraints, but of constraints general to other cognitive functions. It is therefore appropriate a priori to assume that language universals derive from general cognitive constraints and to leave it to others to prove otherwise.

Bresnan and Kaplan seem to accept Wundt's wager, although when they retreated from calling their constraints "cognitive" to calling them "theoretical," they may have been trying to hedge their bet. Givón seems to hold unwaveringly to Wundt's wager.

Chomsky's wager—which reflects Chomsky's own beliefs closely though probably not exactly—seems ultimately unsound. Its chief problem is that it encourages investigators not to look for structure-independent explanations of language universals, but to be satisfied with a linguistic *description* of a universal, assuming it is also a description of a feature of the human language faculty. If Kay and McDaniel had accepted Chomsky's wager, they would never have sought an explanation for color terminology in the workings of the human visual system. Many investigators appear to accept Chomsky's wager in syntax but Wundt's wager elsewhere in language. This too seems premature. It seems impossible a priori to distinguish those universals whose explanations probably lie within an autonomous language faculty, if there is one, from those whose explanations lie without. It is difficult even to see how one would draw a conceptual line between those processes that are strictly language autonomous and those that aren't. Our own wager is that as work continues, there will be fewer and fewer language universals that cannot be explained from outside such a faculty, and that the autonomous language faculty will go the way of the medieval humors—it will cease its scientific existence.

So we commend Bresnan and Kaplan, and Givón, for raising a number of weak psychological constraints that with time may be transformed into strong psychological constraints. The constraints they have offered are significant not because they represent psychological reality today but because they hold promise for psychological reality tomorrow.

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III PSYCHOLOGY

The chapters in this section deal with a number of important empirical and theoretical methodological issues. The first two chapters by Swinney and by Van Lehn, Brown, and Greeno have a common structure. The authors discuss several methodological issues and then illustrate their arguments with applications from their own work. Charniak, using historical examples, justifies the diverse empirical and theoretical methodologies from the various disciplines that make up cognitive science.

Swinney's chapter is a fine demonstration of the effective use of empirical methods to explore fundamental questions in language comprehension. Swinney provides a qualitative analysis of the processes of comprehension into a collection of subprocesses. The question he focuses on is whether or not the various subprocesses are weakly or strongly coupled. That is, are there significant semantic influences on the processes involved in lexical access. Whether or not a system is nearly decomposable, is a fundamental question in the analysis of any complex information-processing task. Swinney shows that empirical techniques can be developed to provide a very clean answer to this question. We view Swinney's chapter as supporting a contention made in Chapter 1 concerning the role of psychology's empirical methodology in cognitive science. We claimed that empirical methods have a central place in theo-