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Part II

Part I

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Exceptional Language Data as Linguistic Evidence: An Introduction

Scientists studying planets to learn about Earth
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We see three general reasons for linguistic theory to pay attention to exceptional types of language or language use. Each chapter of this book presents explicit or implicit instances of those reasons, which we will label (*a*) grounding explanations (*b*) testing connections (*c*) highlighting. These terms are short for the following respective considerations:

1) Explanation of linguistic phenomena must ultimately lie outside linguistics. We look especially to neurolinguistics (via psycholinguistics) to account for what messages and codes the mind can deal with, and to social communication studies to account for the messages which we need to send and receive. Similar dependencies are found in all the biological sciences; for example, the eventual explanation of genetic phenomena lies in the substratum of molecular biology on the one hand, and in the ecology of replication and survival on the other.

2) Separating the essential properties of language from those which are matters of historical accident cannot be carried out by examining language as a static system. Only by observing how parts of a language cohere as perturbations are introduced—say by language contact, artificial experiment, or accident to the brain—can we tell whether apparent linkages are real; we may also discover links which were not previously apparent. As Kiparsky (1973) put it, "Changes in phonological systems may reveal ordinarily hidden structure, as a tiger lurking on the edge of a jungle, his stripes blending in with the background, becomes visible the moment he begins to move [cited in Bailey 1981, p. 49]."

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3) Language in exceptional settings may display particular aspects more prominently; once they have been studied in these cases, they can be traced back into the language of ordinary speaker–hearers. Exceptional language studies frequently use this third reason as their claim to theoretical relevance, and some very interesting phenomena in this volume are best discussed under this rubric. However, the "tracing back" process is not always easy to carry out properly. Our final concern in this chapter will be to remind the reader of some problems with the validity of certain claims about linguistic theory that have been based on observations in exceptional areas.

Let us consider each of these topics in some detail.

Grounding Explanations

An extended family of linguists is familiar with a parable about "explanation," the story (said to be about the philosopher William James) with which J.R. Ross started off his dissertation. In the story, a naive character remarks to James that the world rests on a huge turtle; when he asks her what that turtle stands on, she replies that "it's turtles all the way down!"

In linguistics, there are two senses of the word "explanation" which are used and quarrelled over; one of the most delicious of the polemic pieces is Givón 1979. In one sense, we "explain" a fact of language by showing that it fits in with a more general class of linguistic phenomena: New verbs are coined with a certain ending "because" that is the productive one; relativization of a certain noun in a certain sentence is not possible "because" it is dominated by an S-node. But as Givón points out, such explanations are very limited. They are essentially just generalizations, and they can be seen as no more than economical restatements of the phenomena observed. This becomes apparent when we ask "How do you know that this is a productive verb ending?" Answer: "Because new verbs are formed with it." "How do you know that an S-node dominates this noun?" Answer: "Because in this construction and in a set of similar constructions, certain movement rules and/or deletion rules do not produce grammatical sentences."

What one eventually wants, however, is to determine why these generalizations are true and others are not. Why are there only a few productive verb-making endings in languages which may have many unproductive ones? Why should there be something like an A-over-A principle?

In short, we want to get to the next turtle down—to be able to say that

a particular structure results from the nature of the human signal processor, the message content, and the channels available. This goal has been formulated in various programmatic statements about linguistics, but it is not uniformly espoused. It has remained a popular goal only in one area, articulatory–acoustic phonetics. There, we can use physics to deal with reasonable models of the resonances of the vocal tract, and to some extent with vocal fold vibrations; cineradiography and other techniques give information about the articulatory configuration.

But no other link in the 'speech chain' is so observable. In syntax and semantics, lack of sufficient information about the brain's resources and mechanisms and about the speaker's communicative goals has virtually forced us to abandon this level of explanation. Early attempts have proven premature, as Bailey (1981) points out, and the intermediate goal of simulation of human performance has been substituted by some recent researchers.

Performance simulation is certainly an important avenue of approach, but it cannot get us off the present turtle. To do that, we still need experimental and observational psycho- and neurolinguistics, as the chapters by Shattuck-Hufnagel and Blumstein (this volume) indicate. Work by Whitaker (1970) and his students Buckingham (1978) and Schnitzer (1975) and by Caplan (1980), and Zurif and Caramazza (1976), to list a few examples, also promises advances in this direction. The study of simplified and other special registers should be helpful in moving in the other explanatory direction, the "ecological" consideration of communicative needs. For example, it is suggested that certain linguistic devices, such as the contrastive structure of phonemic oppositions, the postponement of heavy noun phrases until the end of a sentence, or the placement of bound morphemes in highly constrained orders, facilitate the decoding of a message. Such devices should then be exaggerated in some settings where the hearer has decoding problems, and be relaxed in some cases when the hearer's burden is light.

We argue, then, that the study of exceptional speakers and language under exceptional conditions is essential to constructing the notions we will need to explain linguistic phenomena in terms of the nature of the speaker and the speaker's goals.

Testing Connections: Essence and Accident

Linguistic theory traditionally attempts to determine which of the patterns that appear in languages are "real" and which are "accidental." The search for language universals is one approach to this problem, but

it has inherent limitations. The most serious of them is the fact that a correlation between two phenomena can do no more than suggest a linkage. It tells nothing about which causes the other, or whether a third factor causes both. Historical linguistics offers one approach to the question of which linguistic phenomena are causally connected and which merely co-occur, but exceptional language studies are also rich sources of such information, and many of them have already been recognized as such (Slobin 1977).

In order to test the notion that a configuration of phenomena is not a mere accident, one must perturb it in some way (or look for a naturally occurring perturbation), and then see how the ripples propagate. The classic experiment of this type in morphophonology was Berko's (1958) introduction of nonsense words and her observation of how children used them when derivational and inflectional affixes were called for; the corresponding "experiment of nature" occurs when a loanword is brought into a language, or when a poet like Lewis Carroll creates *Jabberwocky*.

Indeed, hypotheses in any science can be cast in terms of connections or interactions among observed phenomena. The hypothesis that children learn to use inflectional morphemes productively is a claim that a particular connection exists between all the instances of a morpheme. The claim that sound-symbolism plays a role in the meanings of deictic words (Cooper & Ross 1975, Tanz 1971) is obviously a claim that certain aspects of sound and meaning are connected. The claim that output constraints should be represented in a model of a speaker's knowledge can be restated as a claim that resemblance between the outputs of different derivations are not accidental (Kisseberth 1970; Menn this volume). Martinet's hypotheses that sound-shifts form 'push-chains' or 'drag-chains' are again transparently claims about connections.

This list could be extended indefinitely. One would ideally hope to find, for each case, some artificial or natural perturbation experiment that would support the claimed connection. Unfortunately, the perfect experiment can seldom be devised or found. While efforts to work in this direction are very important, it seems equally productive to take natural experiments in which the speaker or hearer's resources have been impaired, enhanced, or skewed, and search for issues which they might illuminate. In this sense, we may consider the fields of experimental psycholinguistics and exceptional language study as complementary. In exceptional psycholinguistics one starts from a question and devises an experiment to answer it, while in the study of exceptional language, one starts with a natural experiment, and, by working out its correct characterization, deduces the questions as well as the

answers. (Note that this conception of "natural experiment" is broad enough to include the artifice, deliberate or unconscious, of poets and other players of language games.)

Highlighting Particular Aspects of Linguistic Structure

In many exceptional settings, particular aspects of language become more apparent. For example, the relaxation of time constraints for composition and comprehension in written language allows the transmission of exceptionally complex messages; in these, we can see an enhancement of some of the parallel, allusive channels available to speakers and hearers—channels whose messages qualify and give context to the denotative, core "meaning" of its words. The notion of parallel channel is also applicable to delivery (e.g., sarcasm), levels of diction, and choices of metaphor that project a particular subculture (e.g., class, ethnos, profession, or sexual orientation).

Numerous other examples of highlighting can be found in this book. The fact that writers get less feedback than speakers about the incomprehensibility of their utterances permits the linguist to see the ways in which the sentence construction mechanism can run on, producing the multiply ambiguous and even undecodable sentences of bureaucratese (Charrow this volume).

Again, the study of critical communication, as Lehrer indicates (this volume), leads us back to two aspects of "core" language: the making of names, and the study of ordinary discourse in which nonconventionalized meaning travels along with the conventional, thereby eluding word-based semantics.

Ferguson's chapter points out that "marginal" phones (such as the glottal stop in English), are highlighted in special registers, specifically in talk to babies. Once they have been brought so sharply to our attention, we are obliged to extend phonological theory to deal with them. This extension will also connect with the phonology of language games and poetics, for Sherzer (this volume) and O'Connor (this volume) both note that creative resources are restricted to the use of phonological patterns which are distinct from the meaning-encoding repertoire of the language.

Formulaic aspects of language, overlaid in sentence-based grammar, are revealed under a suggestive variety of exceptional circumstances. In language acquisition—most especially in "natural" (immersion) second language acquisition by younger children; in rituals of law and religion, in the speech of aphasic and dementing patients, and in traditional oral

literature. The existence of formulaic utterances highlighted in these fields of exceptional language reminds us that linguistic theory must deal with language as a continuum from the most creative to the most stereotypic communication.

The field of natural experiments which stand to highlight linguistic essence is not exhausted in this book. Among the variations which have been partially explored elsewhere, we may list:

1. Language acquisition in hearing children of deaf parents—imperfect information about the target language, impoverished grammar input (Sachs *et al.* 1981).
2. Oral traditional literature—competing cognitive load on the speaker, listeners with much shared experience (Lord 1965).
3. Language in dementing patients—speaker–hearers with limited cognitive resources (Obler forthcoming).
4. Language of conference participants—heavy competing cognitive load (Maclay & Osgood 1959).
5. Language in theatre, including puppetry—conversation honed to an artistic edge (Proschan 1980).
6. Language marking of real or assumed sexual, ethnic, professional, age cohort, or other social identity (Labov 1972).
7. Language to computers—as hearers with highly constrained, rule-governed, and limited auditory and cognitive resources (Lea, Ed. 1980).

Caution: The Art of Reasoning from Exceptional Language Data

Since we are advocating the use of data from exceptional language as a tool for linguistic theory, we must, in conscience, point out that it is a slippery tool. Some types of flawed arguments are especially frequent in the study of exceptional linguistics—indeed, we as well as others have occasionally been guilty of these errors and may very well stumble into them again.

The first type of flaw arises from the assumption that similar units of analysis are appropriate for all areas of linguistics. This is not true in general, and the applicability of analytic units or other concepts must be treated as a hypothesis to be demonstrated in each area. Indeed, it is precisely by observing how traditional units of analysis may fail to be appropriate for the exceptional fields that we should be able to advance linguistic theory.

In cross-linguistic studies, this problem already occurs to some extent; consider the debates on the universality of units like Verb Phrase or the comparability of "case endings" in agglutinative versus inflectional languages. But the difficulty seems to be intensified in exceptional language areas: the usual notions of "phoneme," for example, are inadequate to handle some phenomena of early child phonology (Menn in press), Obler's paper in this volume suggests that these notions do not fit the bilingual's phonological structure either, and description of aphasic segmental errors in terms of any school's categories of phonemic versus phonetic is also very difficult.

To take another example, the notion of "rule" in child phonology is known to be rather more like the notion in historical linguistics—a relation between surface forms—than it is like the generative notion of rule, in spite of shared notations. On the other hand, rules written for children's syntax are conceptually like rules in generative grammar. Yet perhaps they ought not to be, given the heavy formulaic component of children's output; consider the evidence for children "performing without competence" in various constructions (see Berko Gleason's chapter this volume).

The possible nonequivalence of similarly named notions is especially treacherous when we try to strengthen linguistic arguments by what is often called "triangulation": calling on data from several different areas to support an argument (e.g., Jakobson 1941, Carroll 1972). This can be a very valuable way of avoiding the errors likely to arise from relying on any single method of investigation, but great care is required in making sure that the same phenomenon is really what is under discussion in each of the areas. To pick perhaps the most famous example, Jakobson (1941) assumed that the presence or absence of phonemic contrasts in child language, aphasic language, and "normal" adult language were comparable phenomena. However, as we claimed earlier in this section, this is not the case. Many similar examples of non-comparability are examined in Caramazza and Zurif's (1978) collection of papers, *Language Acquisition and Language Breakdown: Parallels and Divergencies*.

The second type of flaw appears all too frequently in arguments for the "psychological reality" of one or another linguistic construct. It consists of the assumption that if a certain construct has been employed in the description of a phenomenon of exceptional language, then that construct has psychological reality. Let us consider a hypothetical argument, from the field of language breakdown, where the literature contains valid as well as fallacious arguments for "psychological reality."

With a little effort, one can employ a large number of linguistic con-

cepts in describing the anomalous character of a single sentence used by a dementing patient in recounting a familiar story: "The wolf took a liking to Red Riding Hood's basket."

There is a peculiar lexical selection here. The patient, we might guess, failed to retrieve a lexical item or phrase appropriate to interest in inanimate objects, such as *be attracted to* or *want*, and could only find *take a liking to*, from the same semantic field. We therefore might argue that the odd word choice supports the reality of the notion of semantic field. Also, *take a liking to* might be considered wrong because the register is slightly inappropriate for story telling, thus providing putative evidence for discourse analysis and register. Since the error involves the breaking of the lexical cooccurrence constraint that *take a liking to* requires an animate object, we might say that semantic features, animateness in particular, and lexical cooccurrence constraints are validated as psychologically real by the fact that this sentence was produced. Moreover, the fact that the error *take a liking to* occurred in the middle of the sentence might be taken to support yet other theoretical notions, namely, 'lexical insertion,' 'target meaning,' and 'syntactic frame,' since one way to describe the commission of this error would be to say that the patient made a mistake in lexical selection after the target meaning had been generated and the syntactic frame had been generated. Which, if any, of these inferences is justified? They all begin to look suspect when they are lined up side by side!

The question has two kinds of answers. Let us consider the patient's utterance again. To begin with, it has two important properties:

1. It is anomalous.
2. It was really spoken by a patient with a degenerative brain disease who probably did not intend it to be anomalous.

Now, we know that all of the hypothetical linguistic constructs listed—semantic fields, animacy, register, etc.,—are of value in explaining what is anomalous about the sentence. In this sense, the fact that we judge the sentence anomalous supports the "reality" of the concepts.

But that also would have been the case if the sentence were merely a starred example created by a linguist. What additional evidential weight does this sentence gain from having been spoken by a dementing patient? Some reflection is required to answer this question. Consider: we are looking for evidence that the speaker or hearer is using some computational entity which corresponds well to a linguistic no-

tion. In the present case of *take a liking to,* the patient's choice was neither correct nor random. Instead, some sense of semantic nearness seems to have constrained his response. From this example and others like it, we obtain evidence for what the psychological measures of semantic distance might be, and we hope eventually to judge whether semantic fields, semantic features, prototypicality structures, or some other construct gives the best fit to this kind of performance.

Note that if the speaker had not made a semantic error, we would have been given no opportunity to look for evidence of semantic structure. From a correct performance, we learn nothing that description of normals has not already given us, and from absence of response or error along some other dimension we learn nothing about semantics. The crucial cases for the study of any linguistic structure are those in which an error of substitution has been made, and in which that substitution appears to have been constrained by that linguistic structure.

This is not to say that such structures are necessarily used in sentence formulation by intact speakers—they may have other routes to speech production—but it is to say that such information is or can be made available to the speaker. (We do make the assumption that brain damage does not enhance one's language knowledge. There is a possibility that the brain-damaged speaker may find novel channels of access to information. If this turns out to be true, it will require eventual modification of this assumption.)

So far, then, we have argued that the psychological reality of a notion is supported if a speaker commits an error and yet appears to be obeying the constraints specified by that notion. On the other hand, if the constraints associated with some linguistic notion are violated, we cannot draw any conclusion about the reality of those violated constraints. In the present case, the patient could have violated the animacy and co-occurrence constraints either because their presumed psychological counterparts have been impaired, or because those counterparts never existed in the first place. To establish psychological reality for, say, animacy, we need to look for other exceptional speakers (among whom we include normals making slips of the tongue or perturbed by psycholinguistic experiments) who happen to show performance errors of the relevant kind—in this case, errors constrained by animacy in some way.

One more kind of information is available, however, from the present patient's behavior. His disregard of register and co-occurrence in the face of preservation of semantic field, gender, number, and syntax suggests that the latter several constructs are independent of the first two.