PROBLEMS IN ESTABLISHING THE PSYCHOLOGICAL REALITY OF LINGUISTIC CONSTRUCTS

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The "psychological reality" issue in linguistics - and in phonological theory in particular - has many more facets than seem to be generally recognized. The first problem is the recognition that a problem in fact exists. Under the influence of ideas which developed originally in comparative philology, the prevailing linguistic philosophy has long been one of autonomy: a language has been viewed as a kind of isolated "natural object" which could be investigated independently of the psychology of its speakers and hearers. In recent years, this misapprehension has led to a concept of linguistic "competence" (Chomsky 1965) which consists of nothing more than an arbitrary set of "coding principles" (Straight 1976) abstracted by linguists from linguistic data and treated as something quite distinct from the mechanisms of listening and speaking. Yet, in fact, a language is not an isolated "thing" at all, but is rather the product of various psychological and physiological processes which take place within human beings. Physically, the language product can be studied in the form of speech articulations, acoustic waves, or peripheral auditory events, but in none of these three observable, physical states can we find anything which smacks of linguistic "structure" (not even "phones," which already involve considerable processing by the human perceptual apparatus). Linguistic "structure," therefore, if this term refers to anything real at all, must refer to representations or interpretations imposed upon the speech signal by language users, normally as part and parcel of the communication event itself (Derwing 1973, 302-307). In short, psychological reality is not merely a convenient luxury which linguistic theory may or may not choose to be concerned with, but is rather the *sine qua non* for any linguistic construct which aspires to anything more than an epiphenomenal or artifactual status, and hence for any linguistic theory which can justifiably claim to go beyond the bounds of an arbitrary taxonomic system.

It is for this very reason, in fact, that all of modern "autonomous" linguistics suffers from an insoluble *non-uniqueness problem*: any set of language forms can be correctly (i.e., accurately) described in many different ways, as even the simple example of the English plural inflection clearly shows (Derwing 1974). This implies that pure "linguistic" or "internal" evidence (i.e., evidence about "static" language forms, etc.) is quite inadequate to distinguish a wide range of theoretical alternatives. The only apparent solution to this problem (apart from the adoption of arbitrary principles for grammar "evaluation") is to redefine the nature of the discipline: to say that the goal of linguistics is not merely to describe utterance forms, but rather to describe the knowledge and abilities which speakers have to produce and comprehend them. Linguistic claims now become subject to the test of *truth*: whereas the forms will admit of numerous possible descriptions, there are many psychological claims about what the speaker knows or does which can be shown to be wrong or inadequate. So an expanded domain of "psycholinguistic" evidence can help to sort out alternatives which the traditional kinds of "linguistic" data could not.

To recognize the need to psychologize linguistics is one thing, however, and the actual practice is something else again. Chomsky himself declared linguistics to be a branch of cognitive psychology a full decade ago (1968), yet he and his followers still continue to embrace many of the same old "pernicious ideas" (McCawley 1976) which prevent this conception from becoming anything more than a slogan. In other words, while the so-called "Chomskyan revolution" may well have entailed a *terminological* re-orientation in the direction of the psychologization of linguistic jargon and associated claims, no corresponding *methodological* revolution accompanied these changes, with the result that the generative grammarians "continued to practice linguistics as it has always been standardly practiced" (Sanders 1977, 165). Such linguists may thus claim to seek or establish "psychological reality," yet they still persist in evaluating their theories on the basis of various "simplicity" considerations rather than on the basis of independent psychological evidence (as if the more general theory were, in fact, the most psychologically "valid"; contrast Fromkin 1976, 56, with Steinberg 1976, 385-386.)

But we are still merely scratching at the surface of the problem. It has become commonplace nowadays to find exhortations in the linguistic literature to "expand the data base," often in much the same directions as outlined above, yet Greenbaum seems quite justified in expressing the doubt "whether linguists will abandon a particular linguistic formulation on the basis of psycholinguistic evidence" (1977, 127). Why should they? For, after all, since most linguistic theorizing was done within the non-psychological or "autonomous" linguistic tradition, it is seldom clear what particular psychological claims, if any, are to be associated with any particular linguistic analysis. Obviously, before we can ever hope to make use of new kinds of evidence to test or evaluate psychological claims, we must first know what the particular claims are that we are required to evaluate.

This is the crux of what I have called the interpretation problem for grammars (Derwing 1974). If grammars merely describe utterance forms, then evidence about such forms is the only kind relevant to the evaluation of grammars, and a selection from among competing grammars can only be made on the basis of criteria which are ultimately arbitrary. But if grammars relate in any way to psychological events or states, then we need to interpret grammars psychologically so as to make it clear what the new empirical implications of these grammars are. In other words, a formal grammar requires a psychological interpretation before it can become part of a psychological theory, and it is only the combination of the grammar plus the interpretation which can be put to an experimental test.

Now the problem of interpretation is not nearly so severe with respect to some of the older, more concrete linguistic notions as in the case of many of the more recent, abstract developments. In Derwing & Baker (1977), for example, a summary is provided of various straightforward interpretations, relevant tests, and new experimental data which help to answer the question of which, if any, of several obvious ways of describing the English plural inflection is psychologically real. Serious problems arise, however, when we come to analyses of the type discussed in Anderson (1974, 54-61), which involve the positing of single "underlying" lexical representations and "extrinsically ordered" phonological rules. Even ignoring the major problem of what psychological interpretation to place upon the general notion of the grammatical "generation" of forms (cf. Crystal 1974, 303), what psychological

sense can possibly be made, first of all, of a notion of rule "ordering" which has no relation to real time? To my knowledge, no one has ever even proposed a sensible real-world analogue of this idea, and without an interpretation, to repeat, it is impossible to tell what kind of experimental test is even relevant to evaluate its validity. Fortunately, in this instance, at least, the concept is one that linguistic theory seems to be able to get along very nicely without, merely by reformulating all rules in such a way that no arbitrary ordering relations are required among them (cf. Derwing 1973 & 1975). But we are still left with the problem of what psychological content we can associate with the linguist's notion of the "underlying" or "base" form in phonology. A few suggestions have at least been made in this case (e.g., Linell 1974; Ingram 1976; Birnbaum 1975), but none of them have yet seemed compelling enough for anyone to risk taking one out onto an experimental limb. There is, in any event, another, less direct route which can be taken in connection with this particular evaluation problem. The keystone argument is that there is no basis for positing a single "underlying" lexical representation for any set of supposed "morpheme alternants" unless the alternants in question can indeed be shown to represent the "same morpheme" for speakers. Thus a test which assesses a speaker's ability to "recognize morphemes" can indirectly provide evidence relevant to the question of the extent to which psychological theories might plausibly be constructed which incorporate the linguistic notion of the "underlying" form. For example, on the basis of "morpheme recognition" data collected by means of tests described in Derwing (1976), there is reason to believe that typical speakers judge a word-pair to contain a common morpheme only if the two words involved share a certain "critical" degree of both semantic and phonetic similarity, as independently assessed (Derwing & Baker in press). On this evidence, therefore, any linguistic analysis which posits a common lexical representation for words such as fable and fabulous, which lie outside of this "critical" area, is not even psychologically feasible for more than a very small minority of speakers.

While the recognition and solution of the interpretation problem represent, I think, the main barrier to the establishment of the psychological reality of linguistic constructs, there are still quite a number of smaller obstacles which also have to be faced and overcome. For one thing, we must learn to resist the temptation to be "bathtub experimentalists" (i.e., prone to the cry of "Eureka!"). For even an investigator who fully recognizes the need both to interpret and to test linguistic theories on psychological territory may well (for lack of laboratory experience, for example) fail to anticipate many of the difficulties which can arise out of the very activity of devising, carrying out, and finally evaluating experiments. The most insidious of these difficulties, no doubt, is the one associated with the experimental artifact. For just as (autonomous) linguistic theorizing has yielded many concepts which have no real-life analogues in the knowledge or skills of real language users, so a particular experimental technique can also yield data which are more representative of the technique (or of his subjects' ingenuity) than of the subjects' control of the phenomenon of interest. A particular experiment does not always test in practice what the experimenter thinks it is testing in theory. I have encountered this problem at least twice in my own research (cf. Derwing 1976, 43-50) and Fromkin (1976) properly takes a few experimenters to task for perhaps jumping too fast to conclusions because of it. But in the last analysis there is only one sure way to dispel doubts about the "experimental artifact" and that is via the very painstaking route of cross-methodological verification: each evaluation problem must be approached by means of a variety of alternative experimental routes, in order to insure that the results obtained are independent of any particular experimental procedure.

There are, of course, other methodological problems to be mentioned, as well. There is always, for example, the possibility of the "just plain goof" whenever experimental data are collected, interpreted and evaluated, a danger that springs from causes as trivial as the mispunching of data cards to others as abstruse as failure to attend to assumptions which underlie a particular statistical model. Yet the most common type of error to sneak through a data analysis unattended, perhaps, is the one that results from a failure to take due cognizance of uncontrolled confounding variables (cf. Derwing & Baker 1977, 100), with the result that one's interpretation may be based on an apparent cause rather than the real one. But, again, there is no sure or simple formula to

guarantee safe passage through such treacherous and unpredictable waters as these; one can only take the utmost care possible in his own work, then hope that his readers and critics will pick out whatever errors and oversights may remain.

Finally, there is also the problem of the extraneous or "nuisance" variable, so called, no doubt, because it is often so very hard to eliminate from the experimental situation, even when the investigator may know full well that it is there. In my own "morpheme recognition" research, for example, the interpretation of the data is continually muddled by the factor of possible orthographic interference. How much are "linguistic intuitions" conditioned by the academic task of learning how to read, thereby complicating our efforts to understand the "natural" course of language acquisition through mere exposure to spoken language forms under normal circumstances of use? (A very similar question is the one concerning the very validity of the "linguistic intuitions" of subjects who have already been exposed to any significant degree of formal linguistic training; cf. Derwing in press.) Answers to such questions can only be partially and very tentatively answered so long as one is forced to deal with literate (or "non-naive") experimental subjects. I am very happy to see, therefore, that some aspects of my work are soon to be replicated and extended to the study of Lapp morphology by R. Endresen of the University of Oslo, for included in his population samples will be many speakers who are not only linguistically untrained, but also illiterate in their own language, thereby making it possible to investigate systematically at least some effects of the orthographic variable. Unfortunately, not all "nuisance" factors can be so conveniently dealt with, and these others will continue to constitute one of the more troubling aspects of trying to advance our knowledge by means of controlled experimental research. But since this is the way of science and the only secure route we know of for establishing knowledge about the world and its inhabitants, we have little real choice but to face them all head on.

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