CONTRASTIVE GENERATIVE GRAMMAR AND THE PSYCHOLINGUISTIC FALLACY

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A recent development in the theory of Contrastive Analysis is the proposal made by Krzeszowski (1976, 1977) to use Contrastive Generative Grammar (CGG) to account for the process of simplification in foreign language learning. This proposal seems to be based on a fallacy similar to that which confused psycholinguistics during the 60's, a blurring of the distinction between the domains of the empirical and the formal.

The distinction is first briefly outlined here with reference to TG, and then discussed with reference to CGG.

I

In principle at least, language may be studied from three points of view: as a mental capacity (what people have in their heads), as a behavioural process or series of processes (what people do), or as a product of this capacity and these processes. Descriptions, theoretical models, may be constructed for any of these viewpoints. We have, then, the following set-up:

A — Capacity	A^1 — Description of capacity
B — Process	B^1 — Description of process
C — Product	C^1 — Description of product

(References will be made to these as A, A^1, etc. in what follows.)

The product is directly observable (sounds, etc.). The processes are observable if they are external (lips, hand movements etc.); some are observable only with difficulty (neurological), and some seem unobservable — so far at least (cognitive). The capacity is largely unobservable at the present time, but its existence (note: not its form) is legitimately inferred from the processes

and products it gives rise to. Theories of performance aim to describe A and especially B, while C — the utterances produced — has traditionally been considered the object of decription of grammar (C^1).

However, with the Chomskyan notions of competence and explanatory adequacy, the emphasis given to language acquisition, and the adoption of intuition as an additional data-source, there has arisen and evident desire to extend the realm of grammar into the areas of A^1 and B^1, and to shift linguistics into psychology. (See Derwing 1973).

We now have two possible aims for grammar. One, the traditional one, is that grammar should describe the product alone, making no claims about how people produce or perceive utterances, nor about the form of their mental capacity. The adequacy of such a grammar will be measured internally by the normal formal criteria for any scientific theory (consistency, explicitness etc.), and externally, if it is explicit enough, by testing whether the rules it sets up do actually account for the regularities of observed utterances. (It is unnecessary to scorn such a grammar as being "merely" taxonomic: quite apart from anything else it can be invaluable in pedagogical application.)

In the second case, a grammar would seek to describe actual psychological processes and capacities. As a branch of cognitive psychology it will be expected to formulate its claims in such a way that they can be empirically tested like any other hypothesis in the behavioural sciences. We shall be suspicious, therefore, if this grammar makes claims which appear to be *in principle* not empirically refutable. (See e.g. Itkonen 1978.) We would expect such a grammar to be experimentally based, closely linked to the study of memory, neurolinguistics, cognition, perception, etc.

It needs to be recognized that these two types of description are quite distinct. In particular, the processes involved in each are entirely different. In the product grammar processes are purely formal, such as those known as transformation, substitution, derivation, etc., and the motivation for postulating them is given by the internal requirements of the grammar. In the psychological grammar, on the other hand, processes are behavioural, genuinely dynamic, they take place in time, and they are thus claimed to have a definite objective existence external to the theory.

II

What may be called the psycholinguistic fallacy was to mistake a product grammar for a psychological grammar, to assume that the formal processes used by the product grammar were actually describing the production and perception processes of language behaviour. Hence the flood of (often rather dubious) experiments e.g. on the derivational theory of complexity, and the

significance of experimental evidence *against* the hypothesis (see e.g. Slobin 1968, Ingram 1971). The fallacy was further propagated by early models of performance or parts of performance based on TG, such as those suggested by Wales and Marshall (1966) and Halle and Stevens (1962).

That this work was based on a fallacy was frequently pointed out, not least by Chomsky himself, who had written: "A generative grammar is not a model for a speaker or a hearer ... (...) When we say that a sentence has a certain derivation with respect to a particular generative grammar, we say nothing about how the speaker or hearer might proceed, in some practical or efficient way, to construct such a derivation" (1965 : 9).

But the misunderstanding persisted, and in 1968 Osgood could write: "a transformational grammar is now being considered as a possible model for language performance" (1968 : 499).

The confusion appears to have arisen from Chomsky's own work (see Derwing 1973, especially ch. 8). Competence, together with the emphasis laid on the native speaker's intuition, does suggest a capacity postulated in the mind, part of A. As Ingram (1971) says: "to refer to the grammar as describing the competence of the speaker must imply that the rules of the linguist govern the behaviour of the speaker" (1971 : 344). And constructing a theory of grammar that aims at explanatory adequacy ultimately means making "a hypothesis about innate schemata", about "the nature of mental structures and processes" (Chomsky 1965 : 27, 53). If these processes are mental ones, they must presumably take place in time and be genuindy dynamic. But in that case they are not the same as the static, purely formal processes of a product grammar. (Of course, such formal processes, explicitly formulated, are of the type that might be used to programme a computer, a point which has been made many times and as many times refuted.)

There are fundamental ambiguities here, in the stated aims and claims of generative grammar, which can be resolved in three ways. First, TG is to be taken purely as part of C^1, a description of the product C, in which case an infelicitous choice of terminology has led many psycholinguists up the garden path. This view of TG is adopted e.g. by Lipińska (1974): "TG is not a realistic theory but instrumentalistic, i.e. is designed for the sake of convenience in [the] description of languages" (1974 : 10). (See also Derwing 1973, Prideaux 1971.)

The second solution would be to interpret TG as a part of A^1 and B^1, in which case the problems are formidable. For example, it would have to formulate all its major claims in such a way that they could be empirically tested: you can test whether a given output-sentence is accepted as grammatical, but how do you test whether a given sentence is behaviourally produced in a given way, via a given set of transformations from a given deep struc-

ture? And how is it to account for the differences between perception and production? If generation is to be interpreted as behavioural production, what corresponds to perception, decoding, in the grammar? It would also, of course, have to explain away the experimental evidence against such an interpretation.

The third, least satisfactory solution would be to assume that TG is in fact trying both to have its cake and eat it, to be both a product and a psychological grammar, or simply to be vacillating between the two. What seems to happen in practice is that TG focuses on the product C, and its description of C is just pushed into A and B; theoretical and experimental contributions from psychology, neurology etc. have little effect on its form.

Having said all this, it must nevertheless be stressed that one of the ways in which linguistics as a whole may develop is precisely in the direction of psychology; psychological grammars are undoubtedly going to be more important in the long run. But at the present stage of the art we have a right to be suspicious about product grammars which are *a priori* claimed to represent psychological reality, when they offer little empirical evidence for this or even appear to rule out in principle the possibility of their ever being empirically tested.

III

Contrastive Analysis is a way of describing utterances from two languages it is a part of C[1]. A contrastive analysis may take the form of a contrastive generative grammar, such as that proposed by Krzeszowski (1974).

In Krzeszowski's CGG there are five levels of representation:

— semantic (fundamental meaning relations acting as input to sentence derivation)

— categorial (where language-specific categories are assigned to the input)

— syntactic (where transformations produce the basic linear order of the categories)

— lexical (where lexicalization takes place)

— post-lexical (where cosmetic transformations deal with minor categories, concord, etc.).

These levels are connected to each other and to the dictionary by means of formal processes such as transformation, mapping, lexicalization.

I shall not be concerned here with CGG as such (see van Buren 1976 for a detailed review), but with its proposed application to language learning, for it is here that CGG appears to succumb to the psycholinguistic fallacy.

(I refer to Krzeszowski 1976, which also appears more or less unchanged as Krzeszowski 1977.)

From the study of learners' target language utterances there has recently arisen the notion of simplification (Richards 1975, Widdowson 1977, Corder 1977). This is argued to be a basic strategy in the learner's approach to and use of the target language, underlying for example the various psychological processes suggested by Selinker (1972), such as generalization, transfer, L2 communication strategy, etc. Thus Widdowson (op. cit.) claims that such processes are "tactical variations of the same underlying simplification strategy", and that these simplifying procedures "lie at the heart of communicative competence" (op. cit., quoted in Krzeszowski 1977:7). Corder suggests further that language learning may be seen as a process of complication, by means of which the early simplified codes, similar to those of pidgins and creoles, are gradually elaborated into a standard code.

In the light of the significance of these notions of simplification and complication, and recent evidence that transfer may not be as important as previously thought, Krzeszowski wonders whether these developments "suggest a further regress of relevance of CA conducted for pedagogical purposes" (1977 : 10). He goes on to imply that this need not be the case, since CGG can accommodate this "vertical" dimension of complication as well as its established "horizontal" dimension of relations between native and target language; CGG thus "seems to be well fitted for providing a fairly explicit account of the process of complication associated with second language learning" (1977 : 12). Krzeszowski then discusses how the simplified code of the learner can be described in terms of the place of lexicalization in the generation of a sentence; how in principle CGG could formulate the various complication routes from the simplified code to the standard language; and how it could account for certain errors by formulating a wrong complication route which had led to them.

True, a *code* can be formally described in this way. But can "complication" and "simplification" be so treated? Are these terms as ambiguous as the TG use of words like "process"? If they are linguistic processes like generation, well and good: CGG can in principle be used to relate a simplified code to a complicated code, and suggest all manner of formal complication routes by which the latter may be derived from the former. But: are they not intended to be *psychological* processes, part of what the learner *does* when he learns? Presumably, that is, the learner is understood to simplify the target language input for memory storage, to simplify what he knows of the target language in his own production of it; and as learning proceeds he gradually "complicates" his target language repertoire. Admittedly, even as psychological terms, complication and simplification are at the moment so wide that they

explain virtually nothing; yet it would surely seem that they are indeed intended to be psychological, not formal, in which case CGG, as a description of a product, can have nothing to say about them.

On the one hand then, Krzeszowski seems to be trying to smuggle behavioural processes into his product-description; on the other, genuine linguistic processes are pushed out of the product-description level onto the behavioural process level. Lexicalization, for example, is evidently no longer a part of the description of the code, but something the learner does: "... the degree of syntactic complication of a construction actually uttered by the learner is directly dependent upon the stage of derivation at which *the learner lexicalizes his construction*" (1977 : 13, my italics). This looks like the psycholinguistic fallacy again: an aspect of a formal description of a product is being applied to the description of a behavioural process; formal generative processes are being taken to represent what goes on in the learner's head.

This confusing of linguistic and behavioural processes is even more explicitly revealed when the two non-like terms appear in parallel as likes: "a foreign learner may ... lexicalize prematurely ... or he may pursue a complication route ..." (loc. cit.).

But: what empirical evidence is there that a learner "lexicalizes" from a structure? More important, how could an experiment be set up to test this claim? *Could* one be set up?

Apart from the suspicion that CGG is trying to do the impossible here, the model proposed also projects a rather counter-intuitive image of the process of language learning, in particular of the learner's production of a target language utterance, since it claims that the learner at some stage has a "lexically empty" target language construction available and then puts words into it. Is it not much more likely, on the contrary, that the learner starts off with words, choosing these first, and then tries to create a structure around them? (See e.g. Bolinger 1970.) To some extent at least, perception may work in much the same way: i.e. the lexical items, the main content words, are registered and interpreted first, and then semantic, logical, situational and structural cues are used to relate the items in an appropriate structure. Full structural decoding may not even be necessary at all, and lexicalization seems an irrelevant notion here. (See e.g. Sutherland 1966, Cohen 1966, Kelly 1970, Carton 1971.)

The blurring of the distinction between the formal and the empirical also leads to a confusing view of the practical relevance of CA, and its relation to Error Analysis. As a description of a product CA may or may not make use of formal generative processes; the aims of Error Analysis, on the other hand, explicitly include the description of genuine behavioural processes such as transfer, generalization, etc. CA has pedagogical relevance, therefore, in

that it provides invaluable information about the similarities and differences between the mother tongue and the target language, and it may thus suggest formal (but not efficient) causes for errors. (It is perhaps worth pointing out that *pedagogically* the most useful contrastive analyses are probably the simple, surface-structure ones advocated by Twaddell back in 1968.) However, at least as it is done at present, CA is not of direct behavioural relevance; it is not the right tool to describe the learning process.

Finally: a plague on all double-think terminology!

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