

18 The partiality and relevance of linguistic descriptions

It is a common assumption among language teachers that their subject should somehow be defined by reference to models of linguistic description devised by linguistics. This does not mean that they try to transfer such models directly into the pedagogic domain (although such attempts are not unknown): there is usually a recognition that they have to be modified in one way or another to suit a teaching purpose. But the basic theoretical orientation is retained. The same assumption dominates applied linguistics. The very name is a proclamation of dependence. Now I have nothing against linguistics. Some of my best friends, etc. But I think one must be wary of its influence. In this paper I want to question the common assumption, axiomatic in its force, that a linguistic model of language must of necessity serve as the underlying frame of reference for language teaching. And I want to suggest that it is the business of applied linguistics as the theoretical branch of language teaching pedagogy to look for a model that will serve this purpose. I think that applied linguistics can only claim to be an autonomous area of inquiry to the extent that it can free itself from the hegemony of linguistics and deny the connotations of its name.

So much for the clarion call. Now the argument. We may begin with a quotation which makes quite explicit the assumption of dependence that I want to question:

This is the main contribution that the linguistic sciences can make to the teaching of languages: to provide good descriptions. Any description of a language implies linguistics: it implies, that is, a definite attitude to language, a definite stand on how language works and how it is to be accounted for. As soon as the teacher uses the word 'sentence' or 'verb' in relation to the language he is teaching, he is applying linguistics, just as when he says 'open your mouth wider' he is applying phonetics. It is a pity then not to apply the linguistics best suited to the purpose. The best suited linguistics is the body of accurate descriptive methods based on recent research into the form and substance of language. There is no conflict between application and theory; the methods most useful in appli-

cation are to be found among those that are most valid and powerful in theory.

Halliday, McIntosh, and Stevrens 1964: 167.

But how can these authors be so sure that the 'best suited' description for language learning is one which derives from theoretical linguistics? I know of no evidence myself that would support such an assertion, and one looks in vain for any in the book from which this quotation is taken. I think one should interpret these remarks as a kind of propaganda or as a declaration of faith. It seems to me that actually there is reason to suspect that a description deriving from linguistic theory is *not* the best suited one for language teaching. I want now to try to give some substance to this suspicion.

Let us first consider how language is represented by these 'accurate descriptive methods' that Halliday *et al* talk about. One of the favourite pursuits of modern linguists is to poke fun at traditional definitions of parts of speech. No introduction to linguistics, it seems, is complete without it. Thus Palmer sets out to demonstrate the absurdity of Nesfield's definition of a noun as 'a word used for naming anything', that is to say, a person, place, quality, action, feeling, collection, etc. He comments:

In fact the definition is completely vacuous as we can see if we ask how on the basis of this definition can we find the nouns in *He suffered terribly* and *His suffering was terrible*? Is there any sense in which the last sentence has reference to things in a way in which the first does not? For these sentences are identical in meaning.

Palmer 1971: 39.

Are they identical in meaning? Who says so? Again I know of no evidence obtained from elicitation tests on informants that would support this assertion. What Palmer means, I suppose, is that it is convenient for the purposes of formal linguistic analysis to consider these two sentences to be identical in meaning. This is presumably why the argument he uses for rejecting Nesfield's definition is not a language user's argument but a language analyst's. It is the analyst, not the user who needs to *find* nouns. All the language user needs to do is *use* them. And if he is called upon to describe what they are he is likely to do so nationally in terms of their use rather than in terms of their formal and distributional properties. It is probable, it seems to me, that this would incline him to say that these two sentences are *different* in meaning.

The linguist tends to be intolerant of such 'folk' ideas, believing himself to be in a position to observe from outside and to distinguish what is true from what is not. In effect he simply exchanges one kind of ethnocentricity for another. And he clings to his beliefs in spite of contrary evidence with just as much tenacity as does the common man. Furthermore,

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it would appear that, with his frequent appeals to what seems to be 'intuitively correct', he cannot quite free himself from the influence of folk ideas. Thus Chomsky is reluctant to part with the notion of the kernel sentence:

The notion 'kernel sentence' has, I think, an important intuitive significance, but since kernel sentences play no distinctive role in generation or interpretation of sentences, I shall say nothing more about them here.

Chomsky 1965: 18.

But if such sentences have important intuitive significance, then why, one is prompted to ask, do they have no role to play in the description of language? I suppose that what we must conclude from this really rather remarkable statement is that it is convenient from the analyst's point of view to deny the intuitive reality of the language user's sense of language. One might also suggest (while we are on the subject) that Chomsky's refusal to accept the generative semanticist position might derive in part from his intuitive (common) sense of the psychological reality of the word as a linguistic unit.

There is no reason to suppose, then, that linguists have any privileged access to reality. Yet they constantly talk as if they did. Here are Halliday *et al* again:

Conceptually defined categories can be held precisely because they are incapable of exact application; some of the definitions have survived to this day, protected by a cosy unreality. But it is doubtful if any English schoolboy, having to find out whether a certain word is a noun or not, asks: 'Is this the name of a person, living being or thing?' More probably he will test whether it has a plural in *-s*, or whether he can put the definite article in front of it. Since he is probably required to decide that 'departure' is a noun whereas 'somebody' is not, he is more likely to reach the right conclusion by this method.

Halliday et al 1964: 145.

Once more it is assumed that only the analyst's model can be a valid one. Clearly the schoolboy learning *about* language will be required to adopt an analytical point of view. But even so there seems no reason why he should be persuaded that this is the only correct way of seeing things, 'the right conclusion'. It is important to distinguish between education and indoctrination, but they are dangerously close in these comments. What, for instance, is all this about a 'cosy unreality'? The fact that these deplorable definitions have survived so long surely suggests that they correspond in some way to a deep-seated intuitive reality in the minds of language users. There is nothing 'cosy' or unreal about that. One could argue, I suppose, that this is only because such notions have been passed down through the

generations by misguided educators. But this is only to say that these notions have something of the character of folklore and persist because they are congruent with some kind of cultural reality.

It is interesting to note that Hudson makes a similar appeal to tradition in support of his recent proposals for a 'daughter dependency' grammar:

This similarity to traditional grammar lends not only academic respectability but also psychological support, since schoolchildren and their teachers, and other theoretically naive people, must have found this way of viewing sentence structure reasonably convincing for two thousand years. For all its claims to represent the views of the rationalist philosophers of the Enlightenment, transformational grammar can scarcely trace the notion of the phrase-marker back much further than the neo-Bloomfieldians, the inventors of 'immediate constituent analysis'.

Hudson 1976: 19.

How far such a similarity does in fact lend academic respectability and psychological support to a *formal* model of linguistic description is, I would have thought, open to debate. But my concern is with the relevance of the tradition that Hudson mentions to the validation of a user's model of language.

What I am suggesting, then, is that the language user's intuitive sense of the nature of language as expressed through these much maligned definitions has its own legitimacy and we would do well, I think, to respect it. Linguists may find it methodologically convenient to set themselves apart and to analyse the user's knowledge in disassociation from his experience of language, and in terms of categories that can be precisely formulated. But it does not follow that the only ideas that are 'real' or 'true' or 'correct' are those which can be given precise formulation. Consider again the definition of the noun. The case against it is that it cannot be reduced to the formal terms that define the concept of reality that the linguist happens to find convenient. The distinction, one might conclude, is not between conceptually defined categories which are invalid and formally defined categories which are not, but between categories conceptually defined in different ways.

This distinction has to do with the familiar anthropological question of the relationship between 'structural' and 'psychological' reality in ethnographic descriptions based on formal analysis. Chapter 4 of Tyler (1969) entitled 'Relevance: psychological reality', bears upon this issue. It presents a number of papers concerned with the basic question:

. . . how do arrangements constructed by the anthropologists correspond to arrangements used by the people being studied?

Tyler 1969: 343.

The definition of a kin category like 'uncle' by means of componential analysis, for example, may fail to capture the underlying sense of 'uncle-ness' in the minds of the members of the society concerned. This sense may be realized through the extension of the term in reference to non-kin, or in the creation of metaphorical expressions and so on. In much the same way the formally defined category 'noun' may fail to capture the language user's sense of 'nounness'.

With this in mind, we can return to Palmer and his strictures on traditional definitions. When he asks if there is any sense in which *His suffering was terrible* has reference to a thing whereas *He suffered terribly* does not, the answer is: Yes, there is. The language user senses that they are different in meaning, that yes, in some sense not easy to identify and define, the first sentence does refer to suffering as something, some thing, some entity having some notional correspondence with tables, books and bags, shoes and ships, and sealing wax. The language user senses that grammatical categories have an underlying semantic significance, that they represent a potential in the language for making subtle distinctions in meaning. This potential is sometimes actually realized and in a way that lends support to the user's intuition. I am thinking in particular of poetry. Consider, for example, Hopkins' line in *The Windhover*:

My heart in hiding.

Stirred for a bird,—the achieve of, the mastery of the thing!

Here, we might say, Hopkins has created a noun which realizes the semantic quality associated with 'verbness' and so gives direct expression to the language user's sense of meaning potential inherent in the category 'verb'. *The achieve of* just does not mean the same as *the achievement of*: we recognize the difference by reference to the same underlying sense of conceptual distinctions as lead us to distinguish between *He suffered terribly* and *His suffering was terrible*. It may be extremely difficult to describe these differences, and so one falls prey to the linguist and the sharp edge of his precision, but this does not mean that the differences are any the less real. Much of poetry, and other forms of creative expression, crucially depend for their effect on such a vague awareness of the implicit and not normally exploited semantic resources of the language. The persistence of the notion of a noun being the name of a person, place or thing and the capacity to appreciate poetry have a common origin in the language user's intuitive sense of the communicative possibilities in his language.

If, then, one takes these pronouncements about the noun as deriving from the 'accurate descriptive methods' that Halliday *et al* refer to, it would seem that the concept or model of language that is developed by analysts does not correspond with that acquired by users. There are signs, however, that with an increasing concern with the communicative properties of language the two models are beginning to converge. It is hard to

see how it could be otherwise. If one is to talk about communication one has to be involved in the attitudes and beliefs of users: one can no longer separate out an underlying knowledge to be treated as an idealized abstraction in isolation from its natural context of experience. A communicative orientation to description must take into account the orientation of communicators. One interesting consequence of this converging of models is that the 'conceptually defined' categories that Halliday *et al* and Palmer (among many others) are so scornful of take on a more respectable character. The criteria for a 'good' description now change in their favour. Whereas previously it was assumed that formal distributional criteria could be invoked to define parts of speech as discrete and invariant categories, it now appears that they cannot be dealt with so neatly. Thus Ross (1973), for example, points out that the four items, *Harpo*, *headway*, *there*, and *tabs*, exhibit degrees of 'nounphrasiness' which can be specified in terms of their tolerance of certain transformational operations. He is thus able to range them along an implicational scale which in effect is a variable definition of the notion noun phrase. Instead of a well-defined category, we have a 'squish'. The most 'nounphrasey' of the items that Ross considers is *Harpo*—he describes it in his characteristically whimsical way as a 'copperclad, brass-bottomed' NP. What needs to be noticed here is that such copperclad and brass-bottomed NPs turn out to be names of persons, places, and things, so that the old-fashioned conceptual definition does not seem to be quite so vacuous after all, even with reference to formal criteria. We can now account for the user's definition by saying that he simply associates the whole range of the squish with the semantic features which characterize the full blown nominals at the end of the scale. Thus his conceptual definition has just the same degree of exactitude as the formal distributional one: the difference is that he generalizes from the semantic characteristics of the copperclad nominals, whereas Palmer, Halliday, and others generalize from their syntactic characteristics. It is not a matter of precision but of point of view.

I think we should recognize that there is no model of language which has the monopoly on the truth, that captures reality. All descriptions, no matter how apparently objective they might appear, are really only projections of personal or social attitudes. Skinner is disposed to see language in terms of operant behaviour and tends in consequence to equate human beings with pigeons. Chomsky is disposed to see language in terms of mathematical systems and so tends to equate human beings with automata. Which is to be preferred depends on such factors as usefulness, prejudice, and correspondence to the prevailing intellectual and cultural climate. Theorists are just as subject to the forces of fashion as anybody else: they really have no vantage point from where they can observe in privileged detachment. All this is obvious. And yet, in our search for security, we constantly fall into the same error of supposing that an absolute

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truth is attainable and we look for revelation. It might be instructive to consider another case. In Hockett (1968) a powerful argument is presented against the adequacy of Chomskyan grammar. The central point of the argument is expressed as follows:

Since languages are ill-defined, mathematical linguistics in the form of algebraic grammar is mistaken.

Hockett 1968: 61.

Hockett acknowledges that, although mistaken, such a grammar may achieve a 'practically useful approximation'. But he has his reservations:

This may be so if one's concern is, say, the programming of computers for the helpful manipulation of language data. But an approximation is always made possible by leaving some things out of account, and I believe the things left out of account in order to achieve an approximation of this particular sort *are just the most important properties of human language*, in that they are the source of its openness.

Hockett 1968: 62.

What Hockett's complaint amounts to is that Chomskyan grammar leaves unaccounted for certain aspects of natural language which he is inclined to value highly. He is not content that it should be useful (which he admits it is): he also wants it to be comprehensive. He wants it to reveal the truth. This is the absolutist expectation: doomed to be disappointed at the outset. Interestingly enough, Chomsky himself uses the same argument for rejecting a model of language which does not suit *his* predilections. Hockett objects to a well-defined model on the grounds that natural language is not well-defined, and in the same way Chomsky objects to a finite-state model on the grounds that English at any rate is not a finite-state language. Chomsky, too, acknowledges that the model he rejects might be useful. Here is the relevant passage:

In view of the generality of this conception of language, and its utility in such related disciplines as communication theory, it is important to inquire into the consequences of adopting this point of view in the syntactic study of some languages such as English or a formalized system of mathematics. Any attempt to construct a finite state grammar for English runs into serious difficulties and complications at the very outset, as the reader can easily convince himself. However, it is unnecessary to attempt to show this by example, in view of the following more general remark about English:

English is not a finite-state language.

Chomsky 1957: 20-21.

But, of course, as Hockett points out, English is not a 'formalized system of mathematics' either, although it is clear that Chomsky is disposed to

think of it in these terms, and there are aspects of the language that can be explored most effectively by assuming that it is. The observation that English is not a finite-state language is itself no argument for rejecting finite-state models of linguistic description. The justification of such an assumption, like the equally false assumption that English is a well-defined language, must lie in the kind of insights it allows into certain aspects of language and in the relevance of such insights for the different groups of people who need linguistic descriptions.

It does not seem to make very much sense, then, to talk about a 'good' description as if there were some universal criteria for assessment. It is interesting to note that Halliday is inconsistent on this point. In the passage already cited and discussed, he seems to be expressing the view that there are such criteria. A few pages earlier, however, he seems to be expressing the opposite view:

Linguistics has various aims, and different types of statement are appropriate to different purposes: a grammar for the teaching of language does not look like a grammar written for linguists while a grammar written for speech pathologists would differ from either.

Halliday et al 1964: 150.

This attitude (further elaborated in Halliday (1964)) is not, however, allowed to prevail and ten pages on we find it contradicted. We are invited to play the familiar game of exposing the absurdities of language teachers. Thus we are given a quotation from Thomson and Martinet's *A Practical English Grammar for Foreign Students*.

It is used to replace an infinitive phrase at the beginning of a sentence; e.g. instead of: 'To be early is necessary' we usually say: 'It is necessary to be early'.

This, say Halliday *et al*, is a 'fiction' and they add:

It is not clear what is gained by 'explaining' one pattern by reference to another which it is held in replace.

Halliday et al 1964: 162.

I suppose that one answer to this is that what is gained is a transformational-generative grammar of English. Thomson and Martinet are stating here, in appropriately informal and pedagogically orientated terms, syntactic relations which the transformational-generative grammarians formalize in terms of precise rules. And this, surely, is one way in which 'a grammar for language teachers does not look like a grammar written for linguists'. What Thomson and Martinet are doing here is making a statement about syntactic relations which will be consistent, with a less formal model of language which has to meet conditions of pedagogic adequacy. Talk of one structure 'replacing' another may not meet the approval of

linguists; they will prefer to talk about extraposition transformational operations. But then they are developing a different model for different purposes, and they have other conditions of adequacy to consider.

What, then, we might ask, are the conditions of adequacy attaching to a pedagogic model of linguistic description? One of them, I would venture to suggest, is that it should be in some way congruent with the language user's concept of the nature of language rather than the analyst's. Once again, Halliday has insightful things to say that touch on this matter. In a paper entitled, significantly enough in the present context, 'The relevant models of language', he distinguishes between a number of different concepts or models of language which the child develops in the acquisition process. One of these is what Halliday calls the 'representational model'. He defines it as a 'means of communicating about something, of expressing propositions', and he comments:

This is the only model of language that many adults have; and a very inadequate model it is, from the point of view of the child.

Halliday 1973: 16.

But this is precisely the model that Palmer appeals to when he declares that the two sentences we considered earlier (*He suffered terribly* and *His suffering was terrible*) are identical in meaning. It is the model which is generally given preference by the linguistic analyst. Halliday goes on:

. . . this presents what is, for the child, a quite unrealistic picture of language, since it accounts for only a small fragment of his total awareness of what language is about.

Halliday 1973: 16.

One might make the same sort of observation about the language user in general to the extent that the analyst only captures a small fragment of *his* total awareness of what language is about. It would appear from this that it is the linguist's model rather than the language user's that is 'protected by a cosy unreality'.

In the remarks that I have been making in this paper I have been circling around two points. The first is that models of description are developed from models of language in a conceptual sense which derive from predispositions to see language in a certain way. They are, therefore, inevitably partial, in both senses of the word. It is, of course, a common human failing to assume that personal predisposition and universal truth are the same. The first point, then, is ontological.

The second is heuristic. It is, simply enough, that the model of description to be preferred should be that which is likely to be relevant to a particular purpose. Our particular purpose in what we call applied linguistics is the teaching of language. The question then is: what model can be developed which realizes the necessary coincidence of partiality and rele-

vance? As I said at the beginning, the common assumption has been that it must be some version of the most recent model emanating from theoretical linguistics. And so we have been led off dancing in the footsteps of one pied piper after another.

It seems to me that the purpose of applied linguistics is not to take random pot shots at pedagogic problems using the occasional insights from linguistics as ammunition, but to devise in a serious and single-minded way a coherent model of linguistic description which will be relevant to language teaching. I have no very definite idea about what such a model might look like in detail but I would expect it to embody the user's concept of language rather than the 'detached' view of the analyst, to be participant rather than observer orientated. This does not mean that we would not continue to exploit whatever insights are offered by studies which are methodologically precise in their partiality, but I suggest that these insights would need to have their relevance assessed against the independently motivated applied linguistic model. Eclecticism should not serve as an excuse for irresponsible ad-hocery as it sometimes has in the past. Whatever piper we choose to play, we should make quite sure that we call the tune.

For we must, I think, accept the possibility at least that the analyst's model is not only inadequate but actually incongruent in certain crucial respects with that of the user. For example, whereas linguistic analysis based on scientific principles must of its nature be exact, communication of *its* nature, cannot be. This is not a matter for regret. If meaning could be conveyed by exact specification, if it were signalled entirely by linguistic signs, then there would be no need of the kind of negotiation that lies at the very heart of communicative behaviour, whereby what is meant is worked out by interactive endeavour. There would be no room for the exercise of practical reasoning, for the ongoing accomplishment of making sense that is the central concern of ethnomethodology (see Turner 1974, Cicourel 1973, Gumperz and Hymes 1972).

A number of recent studies in language acquisition and use have a bearing on the development of the kind of user model I have in mind. The exploration of a communicative orientation to description and the consequent consideration of social context and variation are, as I suggested earlier, favourable to its emergence. And such a model is perhaps beginning to assume some shape through recent work on natural semantax (see Traugott 1973, 1977), natural sequences of acquisition (see Ervin-Tripp 1974, Dulay and Burt 1974, Bailey, Madden, and Krashen 1974, Krashen 1977) and other work on performance analysis (see Corder 1975 for a brief review). Also of relevance, of course, is the work of Labov and others on the varying prominence given to different functions of language by different social and ethnic groups within a speech community (see, for example, Bernstein 1971, Labov 1972). Related to such ideas of functional

difference are the suggestions that have been made about broad distinctions in cognitive style and mode of conceptualization between serialists and holists (see Pask and Scott 1972), between convergers and divergers (see Hudson 1966) or for that matter between hedgehogs and foxes (see Berlin 1957). All of these are sources of insight for applied linguistics in its quest for an appropriate way of representing language to the learner as a phenomenon congruent with his own experience as a language user.

The description of language in terms of *process*, of natural cognitive and communicative strategies for the exploitation of linguistic resources, is likely to be very different from one which treats language as a *product*, as an acquired and complete body of knowledge which can be reduced to component parts and rules for their operation. The former is likely, too, to conform more closely to the user's own concept of language, since presumably such a concept has its intuitive roots in experience.

It is this kind of description, participant rather than observer oriented, deriving from the beliefs and behaviour of learners as users and not as analysts of language, that I believe applied linguistics needs to develop as relevant to its concerns. And it is one that we need to develop actively and strategically, on our own initiative, and not simply as a tactic in expedient response to recent linguistic research. Such a description will be necessarily partial, and it will probably not meet the approval of others with different axes to grind. This should not be allowed to trouble us. We have our own conditions of relevance to meet and our own independent way to make in the world.

Notes

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