THE CHARACTERIZATION OF SPEECH AND LANGUAGE DISORDERS: COMMENTS ON THE PLENARY SESSION

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The discussion of language in this plenary session has been very much influenced by the notion of 'levels' of linguistic organization (such as 'grammar', 'phonology', 'semantics'). It has been this way in linguistic enquiry for several decades. Some linguists (notably Roman Jakobson) have strongly emphasized the importance of making use of this notion in the study of language disability. And it is true that the levels approach has been helpful. It has enabled us to clarify our clinical linguistic terminology (Berger) by acting as a perspective within which we can provide more precise descriptions of symptoms, and it has helped us organize our thinking in such fields as language acquisition. Moreover, there is still plenty to do, in terms of this model. As has been pointed out (Ingram), the goals of comprehensive and properly graded descriptions have been only partially achieved in the field of phonology – and we could say the same for semantics and pragmatics too. And it is only now that progress is being made in applying the model to the study of comprehension, where there has long been a (regrettable) tendency to think in a unitary way (Bishop). Breaking comprehension down into components (or levels), insofar as this is possible, is an important task. There are, after all, as many variables to take account of in comprehension as there are in production.

In recent years, however, the limitations of the levels approach have become apparent, especially in the light of the developing focus of attention on the 'whole child'. The levels model breaks a child's language down into its constituent elements, but provides no way of bringing these elements into a relationship with each other. Although an analysis in terms of levels continues to be an essential first step in any clinical linguistic investigation, it urgently needs to be supplemented by a second stage of enquiry, in which the interdependence or interaction between levels is systematically taken into account. It is interesting that all speakers in the plenary session are (implicitly or explicitly) aware of the importance of this interaction. It is implicit, in a somewhat general way, in Martin's notion of transformations between consecutive levels (though it should be noted that this 'sequential' sense of 'level' is different from the hierarchical sense employed in linguistics). It is likewise present in Berger's conception of 'transactive' relations between such domains as memory, motor skills and language. More specifically, it is made explicit in Bishop's notion of levels which interact and simultaneously occur (e.g. the effect of familiarity on vocabulary), and in Ingram's hypothesis of an inverse relationship between stage of phonological acquisition and vocabulary size. Miller uses it by referring to an interaction between syntax and semantics, and between syntax and other 'so far unanalysed' categories, such as discourse factors. His mazes are identifiable in phonological terms, though the context for their analysis is grammatical. Prutting is concerned about how the child handles linguistic data under a number of contrasting conditions. And features from various linguistic levels are involved in Brown's set of disability factors. These indications provide further evidence of a fresh interest in the question of levels–interaction, which has prompted several papers in journals of child language and language disorder since 1980 (for a review of this literature, see Crystal 1987).

The focus on interaction is important because it provides a way of getting from description to explanation – a step which is nowadays felt to be of critical importance. Miller points out that we do not yet have a

characterization of grammatical disorder, in any deep sense, and Ingram and Prutting in effect say the same thing for phonology and pragmatics respectively. The same point would have arisen if there had been a paper on semantics in the session. We need more sophisticated models, in which the notion of interaction is central. And in this process we do indeed need to jettison the oversimple assumptions referred to by Miller (such as the conception of disorder in linear terms, or the view that samples are homogeneous) - though I do wonder whether there are many who would disagree with him here.

However, in moving from a conception of language disability in terms of levels to one in terms of interaction, there is bound to be a certain degree of tension - for example, it will no longer be possible to maintain that 'only components of the system can be disordered' (Martin). And it will be important not to move too fast. The first step is to demonstrate the existence of interactions empirically. It is premature, in my view, to opt for a hierarchical model of interaction, in which such notions as top-down/bottom-up are established. All we can do at present is posit a limited linguistic processing ability in language handicapped children, and look for evidence of the nature of the limitation. Miller's mazes illustrate the kind of evidence we should be looking for. Non-fluency does indeed seem to be a critical factor. It is something which anyone can look out for, as it is a common clinical experience that, in the course of teaching/therapy, children often become non-fluent when pressure is put upon them to learn a new point of grammar, phonology, semantics, etc. We often find that one aspect of their linguistic ability deteriorates when we are teaching them another (e.g. work on grammar leads to a temporary deterioration in segmental phonology - perhaps accounting for the so-called 'dyspraxic tendencies' found in many children). This is, in effect, diagnosis by treatment, and it is an important way of proceeding in our field.

The general role of fluency has been discussed by Fletcher, in his ongoing MRC research (Fletcher 1986) and it is something which has emerged in an MRC project of my own (Crystal 1986). In the latter case, 30 language handicapped children who had attended the Reading University assessment clinic were placed into five groups on the basis of clinical experience, and samples of language were analysed using over 70 criteria derived from grammatical, semantic and phonological profiles, to determine whether any criteria consistently differentiated the groups. It turned out that only two types of criteria provided to be consistently useful in this way: semantic (primarily the number of major lexical items) and prosodic (primarily the frequency and type of non-fluency features). And of the two, the fluency factors proved to be the better discriminators.

In conclusion, it seems to me that there are four major topics which require our urgent attention, in relation to the theme of this plenary session. First, it is right to work towards replacing our unclear clinical terminology (such as 'aphasic') by descriptions in terms of symptom-complexes, looking out for behavioural patterns of importance (Berger). But we must remember to include within this approach an error-analysis, and not just look for scores based on positive features (Bishop), and we must focus more on the nature of linguistic deviance, which has traditionally been neglected (Ingram). We must beware adultocentric models, as Berger rightly says (though I have no idea what to put in their place, if we dispense with them!). And we need to do more by way of motivating and grading the features we identify as important: there are still too many unordered lists about (e.g. Prutting's 30 factors), and too many criteria whose usefulness is still intuitive (e.g. the notion of NP/VP ratio used by Miller - and also by me, in the MRC study) or which are cited solely

because they are easy to apply (e.g. Brown's ELM measures). Easy-to-observe measures are valuable, but of course they are not necessarily the most significant ones (two good examples being the development of intonation contours, and the organization of vocabulary into semantic fields, both of which are crucial to development, but neither of which are easy to identify).

Secondly, it is essential that the interlocutor (T - teacher, therapist, or whoever) be brought into the equation. Because language handicap is primarily an interactive phenomenon (i.e. socially interactive), it is necessary to study the effect of T's input on the child's performance. Only in this way will it be possible to take into account the problem raised by Ingram of clinicians in effect inducing symptoms (something which can happen in relation to any area of language). This perspective is important, also, when it comes to working out realistic expectations about how long it takes to establish a structure in children's competence (Ingram). And an analysis which considers input is the only way in which comparisons can be made between clinic, school, home, and other settings, and thus make it possible to investigate problems of (lack of) 'carryover'.

Thirdly, we need to reflect on where our descriptions and analyses are to come from. The termites which Prutting has encountered in this field seem conspicuously absent when it comes to studies of individual teaching and therapy. There is an urgent need for the study of individual therapy regimes (Ingram), but there is still very little in print focussing on the principles and practices of clinical teaching. The journal Child Language Teaching and Therapy was established to provide a forum for such matters, but so far it has received hardly any case studies of teaching practice (dealing with specific sounds, words, or grammatical constructions) - a point which contrasts with the field of foreign language teaching, where articles on teaching methodology are routine. (In passing, the parallel with FLT is instructive in other respects: for example, the functionalism inherent in Prutting's approach is very similar to that encountered in the functional syllabuses which have been devised in relation to communicative language teaching, and we might profitably refer to them.) Likewise, as Miller points out, there is a pressing need for more follow-up studies, to determine the long term efficacy of our intervention.

Such matters, lastly, raise the question of time. It is not possible to eliminate this problem entirely, of course: although microcomputers can speed up our techniques of analysis and computation (Ingram), and although working with parents can help enormously (Brown), there is no likelihood of equipment helping us speed up our transcriptions of language samples (which is where the greatest outlay of time lies). But here, the medical model is helpful, for it indicates the standards of excellence trowards which we should be working. No doctor would consider the argument 'we haven't time' seriously, when it comes to matters of diagnosis and treatment, and members of the public would not stand for it, if their approach to a doctor received such a reply. On the other hand, doctors have facilities available which language professionals do not have - in particular, pathological laboratories, which carry out the analysis of samples on their behalf. There are already a few centres where clinical language samples are routinely processed (for example by students, who are paid for their labour) to help make best use of clinician's time. I wonder whether such a process could not be instituted on a larger scale, perhaps in relation to the proposed international centre, which is under discussion at this symposium? For until the problem of time can be overcome, there is no chance of language clinicians and teachers developing the level of professionalism to which they aspire.

References

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