

Research Trends in the Study of
Child Language Disability

KEYNOTE ADDRESS

FIRST ANNUAL WISCONSIN SYMPOSIUM
ON RESEARCH IN CHILD LANGUAGE DISORDERS

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It is doubtful whether this symposium could have been usefully held a decade ago. A glance at the literature on child language disability published in the 1970s shows a staggering increase over the previous decades. Most of the clinical linguistic procedures in current use were engendered or achieved fruition in the 1970s, as did most of our present students' standard reading. The more precise shape given to child language studies during the decade provided a special inspiration, under the complementary influences of developmental linguistics and psychology. A rough count of some bibliographies of the period suggests that in 1970 a new paper or book on the subject came out only once every six hours, on average: today, it is more like one every two minutes!

A new decade is always a good excuse for taking stock, and the present symposium is thus timely. It provides us with an opportunity to reflect on the research questions we have been asking in recent years, and to evaluate whether they have been the right ones, in the light of the clinical results achieved. And of all these questions, the most basic is: Why do we wish to do research into child language disability? The ease with which we could all answer this question too readily obscures the complexity of its implications. We might commence with a discussion in terms of the distinction between pure and applied research, but this distinction never works well in the context of medical or paramedical enquiry. It is undoubtedly the case that some of us might be interested in seeing the data of disability as constituting a novel area of intellectual endeavour - a field as potentially respectable and challenging as, say, language, history, or volcanoes. Such a pure field of research would presumably have as its aim a theoretical explanation for the range and complexity of child language disability, it

would carry this aim a stage further, using the results of this research to influence their thinking in the evaluation of pure research in other domains-- linguistic theories, for example. But I do not think there are many of us who see this as anything other than a long-term goal, and one which only indirectly impinges on our daily motivation for research in this field. For the most part, surely, our daily motivation to research this area arises out of a quite different aim - a desire to help the children affected (and whose humanity is somewhat diminished by my reference to them above as 'data'!) I see no distinction here between the speech pathologist, who is professionally committed to this view, and the linguist or psychologist who chooses to collaborate with him. The linguist/psychologist may have additional aims, in his involvement with clinical data, but I have never met one who, having encountered children with language disability, does not wish to have his knowledge and skills put to use in the cause of remediation.

This is what makes the search for a theory of linguistic pathology so different from a linguistic theory or a psychological theory; any attempt to account for linguistic disability cannot be satisfied with diagnosis and assessment, which are relatively 'pure' aims, identifying the nature and severity of a condition; it must include the analysis of treatment, management and rehabilitation. The reason is simple; it is only by the results of our treatment that the validity of our diagnostic hypotheses can be evaluated. If we had a clearer understanding of the medical, psychological, neurological, social and other factors underlying the child's condition, things might be different; but we do not. In the vast majority of cases presenting with language problems, as we all know, there is no clear aetiology, and even when clear medical factors are present, there is never a one-to-one correlation between these factors and the linguistic symptomatology. We are stuck

with the child's behaviour, and all we can do is meticulously describe it and interfere with it, in the hope of controlling it, and thus explaining it. The medical factors are important in that they provide us with a sense of the child's physical limitations, within which our interventions must proceed; but they do not directly contribute to remediation, in the sense of providing the speech pathologist with information about what to teach next and why. It is this framework of reason which I want to see develop as part of the routine of clinical intervention in language problems (as opposed to the often arbitrary decision-making which I observe in the teaching of sounds, structures and meanings to individual children). I see no point in developing a theory of language pathology which does not take this into account.

Several important changes in research emphasis follow from this view. The first, and the most basic, concerns the role of T (i.e., the teacher/therapist who is professionally concerned with language remediation). Is it possible to leave T out of consideration, and to study the child language problem 'isolated', as it were, from the clinic or classroom where it will ultimately be treated? Could we get an accurate picture of the child by studying him solely in terms of his home environment? I do not think this is desirable, and I am not even sure whether it is possible. We may obtain some very useful information from naturalistic studies of the disordered child at home, but I doubt whether a clear picture of a disability can emerge from a study solely in terms of home settings. Visiting the home ourselves, or leaving recording equipment, as we all know, introduces an element of self-consciousness into the environment which can distort the 'neutral' picture we are seeking; and it is doubtful whether, on occasional visits, we would find ourselves in a position to be able to evaluate any

causative factors other than at a very gross impressionistic level. There are much stronger grounds for studying the disordered child in the clinical setting, not only for the obvious reasons of practicability, but for various theoretical reasons, such as the possibility of systematic observation, and the clearer account it gives of the severity of the condition, in relation to the demands of the outside world, which will be the ultimate judge of the child's communicative abilities. But what about the usual objections which can be raised concerning the treatment of children in special settings - the unfamiliarity of the environment, of the clinician, of the tasks, and so on? These problems must certainly be anticipated, and indeed the main point I want to make is that they are so crucial to the success of the enterprise in which we are concerned that they must somehow be built into our putative theory of language pathology. A theory which does not take into account the integrating and guiding role of the clinician (or his substitute) is ultimately of limited value, as far as understanding child language disability is concerned. These disabilities cannot be studied in isolation from the role of the clinician, as in the end it is only through the clinician's function that the nature of the disability can be precisely identified. It is T who has to structure the environment, sometimes in order to get any data out of the child at all, sometimes in order to control the flood of data which many children too readily produce. This structuring, with its foundations in the varying attention, memory, personality, fatigue and other characteristics of the child (or indeed of the clinician!) inevitably becomes part of our operational definition of the disability. It thus needs a clear place in any model of child disability which we may construct; hypotheses

about the role of the clinician need to be generated; data gathered, and analyses begun.

If this is so, then I am struck by a curious omission in the proceedings of the present symposium, though this does no more than reflect a neglect that pervades work on child language disability throughout the 1970s: the almost total absence of research into the characteristics of T's language. We have today had several interesting papers on the characteristics of motherese comparing normal and linguistically disabled children; but what about the comparable characteristics of 'therapese', and the very difficult question of how this relates to motherese? There are certainly important distinguishing features of clinical discourse, for example in the early stages of treatment the use of three-part (as opposed to a 2-part) conversational turn (such as T what's that? P a car T very good, etc.), or the use of overt strategies of imitation and prompt. Then there is the question of how the child views the clinician and the tasks he provides. The point has been frequently raised in child language studies in recent years that many of our experimental findings can be vitiated by our failing to take into account the child's judgement as to what he thinks we are about. 'Why are these people asking such silly questions to which (if they are normal adults) they must know the answers?' The point applies equally to the language-disordered population. There are plainly many interesting theoretical and methodological questions to be addressed. But at present, it is the empirical weakness in the field which most concerns me--the lack of descriptions of what clinicians actually do with their patients, and of evaluations of how successful they, and others, think they have been. This is naturally a sensitive area, relating as it does to judgement of professional competence, but it is an area which must be seriously addressed, if speech

pathology as a profession, and the research studies which feed it, are to be taken seriously. In a frugal climate, there is no shortage of people ready to make cuts in health and education services, and if a subject is to be judged by its results (or research by its social relevance, as so often it is these days), then we must be prepared to ask these questions coolly. The required evidence, we must remember, is not simply that a patient has made progress (for he might have done this had he not come to therapy), but that the progress was due to the clinical intervention, using the training which qualified the clinician in the first place. It is this causative dimension, of course, which is of primary interest to the researcher into language pathology, for it will form part of his overall explanation of the disability in question.

There is, then, a certain element of paradox in our enquiry. We wish to investigate child language disability, but can only do this by the simultaneous study of the clinical setting, and of the clinician's role in particular. However, we have no guarantee that the clinician will be performing effectively on any given occasion, due to his limited knowledge of the nature of the disability that he is having to deal with (and, of course, the possibility that he may be having an off day!). All too often we are having to work in the dark with language disorders, and there is no certainty that the measures we introduce are correct, or even relevant to the condition. In which case, it is always possible that the data of disability--in effect, what we have constrained the child to say, in our session--may form a quite distorted picture. If we decide to work on pronouns, let us say, we shall elicit a picture of a pronoun-deficient child; whereas if we had worked on

adjectives, a different picture would have emerged. Nor does this bias readily come to be eliminated over time (at least, not in the short-term): progress in pronouns in week 1 will motivate further work on pronouns in week 2, and so on. It is not easy to step back from one's areas of progress and begin work on other areas; but of course if a balanced developmental ability is to emerge, this must be done. (It is here that profiles of development come into their own.) A major research initiative, then, must be made into the whole methodology of data collection and evaluation in this field, and a bridge built between our routine clinical practice and our research endeavours. How is this to be done? In my view, the answer lies in the conception of therapy as a hypothesis-testing procedure. The hypotheses about the disability are generated by the theoretical models we construct, as part of our studies of normal language acquisition, and the like. An attempt is made to predict the patient's behaviour, on the basis of an analysis of a sample of data, using such models. I can illustrate this process in action from a problem we recently had to analyse in our clinic at Reading. This was a language-delayed boy of five, who had considerable ability at the two- and three- element stages of clause structure, and a fair command of the associated phrase structure, but his use of auxiliary verbs and verb endings was apparently erratic. This can be seen by the following sample, in which the relevant verb phrases have been listed as they occurred in a five minute exercise describing the events in a picture book.

'man w\alking/

'man 'eating d\inner/

'man is 'fall d\own/

'man s\itting n\ow/

'man sm\iling/

'man r\unning/

'man 'kick b\all/

'man 'is j\ump/

At no point did P produce the correct forms of the present tense - man is walking, etc. Our profile was accordingly somewhat confusing, with approximately equal numbers of correct vs. incorrect uses of auxiliary and -ing. In order to clarify the problem, an obvious first step is to list the sentences on the basis of their formal characteristics - as if they were different word-classes in a foreign language:

man walking/

man is fall down/

man smiling/

man is jump/

man eating dinner/

man sitting now/

man kick ball/

man running/

The next step is to scrutinize the groupings to see whether there is any formal or semantic reason for the patterns being the way they are. For example, there are many verbs in English that do not normally take an -ing ending (e.g., seem, know, like) - but these do not seem to be the ones. Perhaps it is something to do with phonological structure - say, monosyllabic verbs allowing -ing, polysyllabic verbs not - but again, there is nothing obvious that we might say about one group that does not also apply to the others. From a semantic point of view, is there perhaps something in common between walk/smile/eat/sit/run which distinguishes them from the

verbs in the other groups? At this point, in dealing with the interrelationship between grammatical and semantic categories, it is important to be aware of any hypotheses which have been proposed in the psycholinguistic or language acquisition literature--especially the latter, where studies may have brought to light systems of grammatical classification and interpretation which are not those normally used in the adult language, and which might otherwise be missed by the process of normal adult introspection. One such system seems particularly relevant, namely, the way in which many children make a distinction in their use of verbs based on the salient characteristics of the activities involved--in particular, whether the action in question involves a change of state of the entities involved in the action or no such change of state. Activities such as 'fall over', 'kick' and 'jump' are all clearly changes of state activities, whereas activities such as 'think', 'look' and 'breathe' are not. Unfortunately, the picture-book presentation of the stimuli tends to reduce the potential of this distinction, in all but the most dramatic cases: pictures of people eating, running and jumping are invariably static--people frozen in mid-air, or with a fork half-way to their mouths. There would be very little to choose between the running and the jumping in this respect.

But the idea of the mode of activity is a good one, and has frequently been referred to in language acquisition studies. Perhaps there are other characteristics of change of state verbs which might attract the attention of a child learning language? There are presumably three main possibilities: activities which have a discrete starting-point; activities which have a clear limit to their duration; and activities which have a clear finishing-point. For the present sample of data, these distinctions are relevant

indeed: there are no grounds for using the first (when the person in the picture starts to walk is just as unclear as when he starts to kick) but the other two criteria provide a relevant basis of contrast: walk, smile, eat, sit and run are of indeterminate duration, whereas fall down, jump and kick have a more momentary duration; and whereas the former have no clear end-point, the latter have a definite end-point. There is a clear end to the activities of kicking, falling down and jumping, whereas there is no comparable definiteness about the finishing of the other activities.

This analysis now becomes a hypothesis against which to measure the usage of the patient. There is of course no way of knowing in advance why the patient may have chosen to classify his verbs in this way; on the other hand, it should be pointed out that, if he is going to classify his verbs at all, there are a very limited number of logical paths available for him to follow. He may choose any one of six possible interpretations for the use of is vs. ing in relation to this classification:

(1) he may think that the way English marks end-point verbs is by using the morpheme is, with ing or zero being used for other verbs; if he speaks according to this hypothesis, he will produce

man is fall over/		man walk(ing)/
	vs.	
man is kick ball/		man eat(ing)/

(2) he may think that the way English marks end-point verbs is by using the morpheme ing, with is or zero being used for other verbs; if he speaks according to this hypothesis, he will produce

man falling over/		man (is) walk/
	vs.	
man kicking ball/		man (is) eat/

(3) he may think that the way English marks verbs without end-points is by using the morpheme is, with ing or zero being used for other verbs; if he speaks according to this hypothesis, he will produce

man is walk/		man fall(ing) over/
man is eat/	vs.	man kick(ing) ball/

(4) he may think that the way English marks verbs without end-points is by using the morpheme ing, with is or zero being used for other verbs; if he speaks according to this hypothesis, he will produce

man walking/		man (is) fall/
man eating/	vs.	man (is) kick/

(5) he may think that the way English marks end-point verbs is by adding the morpheme is, and verbs without end-points by adding the morpheme ing; if he speaks according to this hypothesis, he will produce

man is fall over/		man walking/
man is kick ball/	vs.	man eating/

(6) he may think that the way English marks end-point verbs is by adding the morpheme ing, and verbs without end-points by adding the morpheme is; if he speaks according to this hypothesis, he will produce

man falling over		man is walk/
man kicking ball/	vs.	man is eat/

Given this range of possible interpretations listed earlier, it would seem that P is operating according to the fourth hypothesis. The point can be checked immediately, by introducing a wider range of verbs in the next remedial session, and seeing whether we can predict P's behaviour, on the basis of the hypothesis. If we are right, he ought to say man swimming and not man swim or man is swim, for example. If we are wrong, the exercise has not been wasted, for it has eliminated a possibility, and suggested a

promising direction for further thinking. It may be, for instance, that the general line of reasoning is correct, but that we were wrong to restrict the field to be and ing in the first place. Several other factors may need to be followed up and eliminated before a solution to the child's problem is found.

The hypothesis-testing approach to therapy can of course be illustrated from any level of linguistic enquiry. What should be noted, from this kind of predictive thinking, is the way we are forced into a consideration of systems of linguistic contrast, thus approaching the notion of a linguistic differential diagnosis. But this is possible only by taking into account factors in the clinical environment, in the child's behaviour, and in the normal language acquisition literature. Once again, an integrated view of research is advocated.

This was an example of integration at a fairly micro-analytic level of investigation. But for a hypothesis-testing approach to work, we need to ensure that it is capable of coping with the whole range of behaviour likely to be encountered in clinical situations. There is little point in developing a research methodology which is in principle unable to be extended to meet the needs of a wide variety of clinical situations and types of patient. In our field, we are all of us dealing with an enquiry which is inevitably a comparative one. We are always comparing - patient with patient, patient with normal, patient at time A with same patient at time B, patient in setting A with same patient in setting B, and so on. Unless our approach is capable of allowing these comparisons to be made, we are introducing a serious limitation. But achieving satisfactory comparative procedures is not an easy matter, as can be seen by looking

briefly at the demands made by two aspects of any such procedure--the need for comprehensiveness and gradability.

Comprehensiveness is a deceptively simple principle: it means primarily that everything the child says in a sample must be explicitly accounted for in our description. The more an analytic procedure leaves out, the less valuable it is as an objective tool, and the more difficult it will be to compare with other samples. The information that has been omitted, due to the analyst feeling it intuitively to be less important, may well turn out to be significant, when later comparing the sample with others. In the present state of the art, there is no way of guaranteeing in advance that a particular linguistic topic will be of no value in deciding on an assessment or a remedial path. The only safe procedure to follow, accordingly, is to allow a place for everything. And this means not only everything in our corpus, but everything in principle - i.e. the set of possibilities which constitute the normal adult language. It may seem paradoxical to insist that one must begin one's inquiry into child language disability by considering the perspective of the adult language, but there are good reasons as to why this should be so. Firstly, the adult language provides the terminus ad quem of our clinical efforts and it makes good sense to be able to clearly identify one's goal before beginning to move towards it. Secondly, the adult language is inextricably involved in our enquiry, as we have seen in relation to the clinician's role: until such time as children come to therapy themselves, adults will be doing it for them! Thirdly, we have to work on the principle that 'everything that can go wrong, will, sooner or later, on some patient': there are no sacrosanct linguistic structures. The truth of this axiom is perhaps most readily revealed by attempting to state the reverse: what structures do I not need to incorporate

into my framework of reference, because I can guarantee that they will never cause difficulty to a child?

The usual interpretation of the notion of gradability is in terms of the developmental dimension of normal language acquisition; but it is important to appreciate why this dimension is so important in the study of child language disorders. Its value is that it permits the development of a single procedure which can integrate the three basic clinical operations of screening, assessment and remediation. So often, these tasks are carried out separately. An assessment tool, such as ITPA or Reynell, may be used to establish levels of achievement; but having done this, there has been no systematic guidance about subsequent remediation. We may have learned a great deal about the child, in carrying out the test, and some ideas for therapy may have sprung to mind, but there is no way in which these hints and impressions can provide the rationale for a therapeutic programme. The question 'What structure to teach next?' is still very much open. Conversely, if we take a remedial procedure, such as one of the language development kits or series, which list a definite sequence of stages for the therapist to follow, then there will be plenty of guidance concerning therapy, but no principled basis for assessment or screening. The question 'What level of achievement has the child reached?' is still very much open. What is needed is a procedure which can relate these operations, showing how the skills of screening/assessment and remediation are functionally interdependent, and how information gained about any one can provide insights into the way in which the others may be implemented. The developmental linguistic

dimension, more than any other at present, permits this relationship to be made. I stress that this is the best available basis. If and when independent measures of cognitive/linguistic complexity come to be established, they may well perform better than acquisitional guidelines; but there is no likelihood of such measures becoming available in the near future, and the same applies to any of the other potential psycholinguistic measures (e.g. scales of memory or attention). Language acquisition studies, while presenting some difficulties of their own, are much more detailed and much less problematic.

It is this combination of comprehensiveness and gradability which characterises the approach which I and my colleagues have been trying to develop in our clinical language work in the last ten years, based on the notion of profiles. A profile is no more than a first approximation to an accurate description; but it does imply that the salient, identifying features of disordered language have been isolated. It should also be noticed that a profile of an object becomes unrecognizable or confusing if either too few distinguishing features are given or too many. Nor is there any magical way in which the right number and kind of features can be discovered in advance: they must emerge empirically, and it is usually a lengthy process of trial and error to construct a profile chart of maximum value. In this respect, language profiles differ, from, say facial profiles: a forensic photofit kit works because of the limited range of variables involved in facial identification; the linguistic kit is far more complex, but the principle is the same--every feature included in a profile chart should be there because of its potential diagnostic value. There would be no point in having an item on the chart that was never used to discriminate individuals or groups. This was how the range of syntactic and morphological features on

the LARSP chart came to be compiled, for example. The hundred or so linguistic features that occupy the bulk of the chart are there because they have been found to be useful, in the sense that contrasting assessments and remedial paths make use of these features. Naturally, some features, or groups of features, turn out to be more regularly used than others-- and in this the prospect of being able to make diagnostic judgements moves enticingly nearer--but all are demonstrably relevant to the task of coping with child (or, for that matter, adult) disability.

It is this principle which explains the varying amount of information at different points within a profile chart. Some points in the language development process are pivotal, hence they need more attention if a profile is to 'catch' what is going on. For example, the Stages I-V of the LARSP chart are much more detailed than the later stages; and within these, the transitions between certain Stages are given great attention, because of their importance in remedial work. But Stage VII, for example, is extremely thinly treated, so much so that some clinicians have wondered why it is there at all. Indeed, this Stage has largely mnemonic significance: it is included to remind people that several important grammatical features are still in the process of acquisition after age 5, and that a clinical disability could be grounded here. But the cases are uncommon, and most clinicians who use this procedure attend more routinely to the earlier stages, which is where most of their caseload lies. As one clinician put it, 'If I have a child at Stage VII, I've more important things to do than profiles'. This attitude is not entirely valid. It is not an argument against profiles as such, but it is a limitation of this particular profile. The LARSP chart was not designed with Stage VII children primarily in mind.

A profile chart, in short, is an attempt to summarise the most frequently occurring indices of normal and abnormal linguistic development, and to provide a sufficient basis for plotting patterns of progress in this development. The need for longitudinal studies of child disabilities is absolutely crucial, but few have been attempted--perhaps because sufficiently comprehensive frames of reference have not been available. Thumbnail sketches of linguistic difficulties, so often found in the clinical literature, will never be sufficient to provide a good basis for understanding the progress of a disorder over time. The need for descriptive and developmental detail is paramount, but allowing for the demands of clinical realities, as expressed above. How are these notions reconciled? Superficially, it might seem that there is a contradiction between the demands of descriptive and developmental comprehensiveness, on the one hand, and clinical selectivity, on the other. It is the notion of profile which resolves the contradiction, and in particular the notion of 'Other'--which is the most important word on the LARSP chart, for instance. Other refers to the less frequently used structures at given stages: any item not separately specified is placed under Other. Accordingly, a high figure opposite Other, in any profile analysis of a patient, is indicative of a departure from norms of language development, and would immediately attract clinical attention.

Profiles are in principle applicable at all linguistic levels, and can be constructed for any desired degree of detail. For example, we may have a phonological profile, on which one incorporates the whole of the system; or one may profile the consonant system only; or the plosive system only; and so on. The profile may also be multidimensional, in the sense that it permits the multiple analysis of the data in terms of different phonological theories. In the phonological profile chart we use at Reading, for example, child

data is conventionally recorded on a chart in terms of phonetic type of articulation and syllabic distribution of the various phones in the sample. Subsequently, repeated scans through the data are made in terms of phonemic type, use of distinctive features, and phonological processes. The importance of an eclectic approach of this kind cannot be underestimated. I have repeatedly seen children whose analysis in, say, phonemic terms was unilluminating, but whose analysis in process terms was very helpful; and likewise for all the other possibilities. Comprehensiveness in profile work does not mean only comprehensiveness in accounting for the data; it also means comprehensiveness in viewing the data, using the available theoretical models.

As a further example of a profile approach at work, I include the current version of the prosodic profile which has been developed at Reading over the past few years, and which will be published next year, now that it has been used a fair amount. In the case of the 9-year-old whose profile is illustrated, a very abnormal pattern of development is observed. Tone unit structure is displaying an erratic and deviant relationship to grammar. Half the tone-units are delimiting words or word sequences that do not coincide with major grammatical units (unlike the normal function of tone units), e.g. you can/ move/ about ten time/ and how/ many/ time/. The child's nuclear tones are also well away from the normal sequence of development, as represented vertically on the chart: where normal children would use falling tones, this child often uses falling-rising ones, thus producing a permanently dubious, cautious tone of voice, usually inappropriate to the context, and often causing the clinician problem, in that when these tones occur in utterance final position, it is not clear whether he has stopped talking and is waiting for a replay, or whether he is about to continue, after a non-fluent pause.

I use this topic to make another point--namely, to emphasize the extremely primitive stage which research into child language disorders is still at, despite the upsurge in publications I referred to at the beginning of my paper. It is still quite remarkable how little actual descriptive work has been published, relating to our field. Where does one go, if one wants simply to refer a student to a good description of a type of disability--much as one would in linguistics send a student to a good description of a certain type of language? There are hardly any primary sources, and those which do exist are partial in their description. In particular, a full suprasegmental description of child samples is usually avoided, and punctuation used instead--though the ambiguity and misleading nature of punctuation as a guide to speech patterns can hardly be underestimated. Particularly these days, when the language acquisition studies of the first two years have repeatedly put such emphasis on intonation, rhythm and other such features, it is a pity that a similar awareness has not developed in the field of disability.

In the 1980s, then, I would hope to see a much stronger awareness of the role of empirical research into child language disability. I think we have more than enough theories about aspects of the phenomenon, an unending flow of reviews of the literature in fat volumes and certainly plenty of terminology and classificatory apparatus available to enable us to talk about symptoms (as recent dictionaries of speech pathology nomenclature illustrate very well). What we lack are bodies of published data against which our theories can be properly tested--data which is characterized by its comprehensiveness in both synchronic and diachronic terms.

PROSODY PROFILE

Name IAN P. Age 9;2 Sample date 6-6-79 Duration 5 min Type FREE Conv.

TONE UNIT Total 56 Av. words 2.4 (cf. norm of 5)

Incomplete Indeterminate Stereotyped Imitation

STRUCTURES	CLAUSE	LH1 LH1 II	} (26)	FUNCTIONS V, / in final position for statements
	PHRASE	LHT LHT IIII		
	WORD	LHT LHT LHT	} (30)	
	OTHER	CL+ CL- LHT LHT LHT		

TONE Data variants *vvvv / / / vv v+ vv " v / v /*
v v v v " v / / / v v v / v / v / v /
v v v " v / / +

Deviant

Summary	Other	N	W	↓	?↑	↑	"		
									-
								LH1 III	/
						LHT		LHT LHT	/
									^
	II	II				IIII	II	LHT LHT LHT LHT !	v (31)
								II	C
									?

TONICITY

		Imitation	Stereotyped		
SIMPLE	MeL BE-C	Non-final		Final	
			✓	x	✓
	II			III	
	LHT !		LHT III	LHT LHT LHT LHT LHT LHT LHT II	
COMP		N.F. + W.F.		N.F.+F. II	

OTHER

Tone unit pitch
Tone unit Other

Prosodic features (TU+)
Paralinguistic features

Secondly, and especially, I want to see research into clinical behaviour as observed in remediation, and not just by way of assessment. This orientation has its counterpart in clinical practice, too: I would very much like to see the eradication of 'clinician's assessment syndrome', i.e., when in doubt about what to do with a patient, do an assessment. I would like to see it replaced by: when in doubt, follow through the consequences of a remedial strategy upon the child's linguistic system as a whole.

Lastly, I would hope to see clinicians turning more routinely to the normal language acquisition research literature, and conversely, and less obviously, to see the needs of clinicians met more fully by researchers in normal language acquisition. There are so many quite elementary questions whose answers would be invaluable to the clinician, but the normal acquisition research has not been done. There too we are paying the price of an earlier decade of theoretical speculation which produced all too little empirical work. I am perfectly well aware of the reasons for the current state of affairs, but I still find it somewhat embarrassing when clinical colleagues ring me and ask for a reference on such basic points as, say, the frequency of certain phonemes in motherese, or the frequency of lexical items during the second and third years of life, and suchlike, and I have to say sorry, but the facts are not known. More help of this kind will surely be forthcoming in the next decade, to provide a more solid normative foundation for our own enquiries. I look forward to seeing a report on the point, when your symposium reaches its tenth birthday.