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WHAT BECAME OF LAD?

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Since about 1960 the interest of linguists and psycholinguists in the study of child language has been rapidly expanding. The new impetus derived especially from Chomsky's formal approach to the genesis of language, the so-called Language Acquisition Device, or LAD. This article is intended to be a short historical and critical note on what happened to LAD. It will be historical in that a description will be given of the early conception and impact of the model, as well as of its falling back into obsolescence; it will be critical in the sense that some major causes will be analyzed which can explain this latter fate. These causes are partly to be found in the structure of communication between formal and empirical disciplines, but mostly in the untenability of the empirical assumptions on which the theory was based. In a final paragraph a summary review will be presented of the main theoretical changes that were made to replace these empirical assumptions, and with them the whole LAD-model.

1. THE CONCEPTION OF LAD

The first steps towards a formal characterization of human language acquisition went somewhat as follows. Within the framework of his discussions about the goals of a linguistic theory, Chomsky (1955, 1957) presented the idea of formalizing linguistic discovery procedures as mechanisms which take a corpus as input, yielding a grammar as output. The first actual proposals with respect to the construction of such machines seem to have been made in a conference paper by Miller & Chomsky (1957). That paper was never published, and meanwhile Miller lost all his copies (see Miller, 1967). But the problem posed in that paper was roughly as follows: Given a language (natural or artificial) for which a (finite) grammar exists, could one conceive of a procedure for inferring the

grammar from a finite set of (linguistic) observations? It was clear from the outset that, without further qualifications, this question could not be answered. Both in the paper, and also subsequently many qualifications were indeed made, as we shall discuss in a moment. It seems also to have been immediately obvious that an answer to the question could be highly relevant for the understanding of the child's acquisition of language. At a conference in 1960, Chomsky (1962) stated that relation as follows:

> ..., we might attempt to construct a device of the kind
> (1) utterances of L → [___] → formalized grammar of L
> This represents a function that maps a set of observed utterances into the formalized grammar of the language of which they are a sample. Given as input a sufficiently large and representative set of utterances of *any* language (English, Chinese, or whatever), the device (1) would provide as output a formalized grammar of this language. A description of this device would therefore represent a hypothesis about the innate intellectual equipment that a child brings to bear in language learning.

If such a 'Language Learning Device', later rebaptized as 'Language Acquisition Device', could be conceived of, it could function as an ideal model for human language acquisition. As for any ideal model, the subsequent step should be to compare the model with the actual situation, i.e. the child's language acquisition, and to see how the model has to be adapted in order to work in real time and to display the typical characteristics of the child's growing linguistic competence. Chomsky (1965) denotes these two aspects of the problem by 'adequacy-in-principle' and 'feasibility' of LAD, respectively.

For an adequate understanding of the further developments since the conception of LAD, one should be reminded of the fact that Chomsky's formal approach to language was the main impetus to the rise of two rather independent disciplines. The first was transformational grammar, the second was the theory of formal grammars, a branch of mathematics and computer science. Both disciplines took up the notion of a language learning device and developed it according to their own needs. In linguistics and psycholinguistics the main interest was in the explanation of human language acquisition, and the term LAD became generally used for formal theories in this area. (In reaction to structuralism Chomsky (1957) ruled out as 'unreasonable' and 'very questionable' the formalization of linguistic discovery procedures, and consequently very little, if any, attention was given to the development of LAD for that purpose.) In computer science, on the other hand, one preferred

to speak about theories of 'grammatical inference'. They were designed to show the existence of effective procedures for inferring grammars from finite presentations of various formal languages (or their complements). Communication between the two developments, however, was minimal to the detriment of both as we will argue.

2. DEVELOPMENTS IN GRAMMATICAL INFERENCE

The challenge in the Miller & Chomsky (1957) paper was first taken up by Solomonoff (1959, 1964), whose work was subsequently greatly expanded by Gold (1967) and by Feldman (1967) and his coworkers at Stanford University. It is unnecessary to give anything but a very incomplete survey of the inference work in the present context. A good review is available in the literature (Bierman & Feldman, 1972), and we only need an indication of results that are directly relevant for LAD. For this we best start from Gold's formulation of grammatical inference.

Gold (1967) studied the question of adequacy-in-principle, or 'learnability' as he calls it, for various classes of formal languages. More specifically, he proved the existence or nonexistence of procedures for inferring ('learning') an adequate grammar for L from finite sets of observations from the language or its complement. 'Learnability' appeared to depend on what was called (a) the *hypothesis space*, and (b) the *observation space*. The hypothesis space is the a priori knowledge, available to the inference procedure. In Gold's paper it is defined as the class of languages to which L belongs. Gold studied the 'learnability' of L in case it is known beforehand that L is either finite, regular, context-free, etc., up to merely recursively enumerable. The observation space is defined by the observations available to the inference procedure. Gold assumes that observations are made one by one. They are either of the type 'string x is in L', or of the type 'string x is not in L'. The former is called a *positive instance*, the latter a *negative instance*. A string of instances is called an *information sequence*. If all of the instances in the sequence are positive, one has a *positive information sequence*; if negatives also occur, one has a *mixed information sequence*. A *complete information sequence* is a mixed information sequence in which all positive and negative instances are enumerated; such sequences are generally infinite in length. They are also called *informant presentations*, since it is as if each possible string of words is presented to an informant who provides the information 'grammatical' or 'ungrammatical'. A

complete positive information sequence is an enumeration of all positive instances. It is also called a *text presentation*, since it is as if one is reading a text containing all and only the grammatical strings of L.

A language L is called 'learnable' by text, respectively informant presentation, if an algorithm exists which for every complete information sequence performs as follows: (i) each time a new instance is presented, a grammar is produced of the predetermined class (context-free, etc) which is consistent with the information received up to that point; (ii) after a finite number of instances, the output remains constant: the grammar produced is the same or equivalent after each instance, and is a grammar of L. A class of languages is called 'learnable' if every language in it is learnable.

Gold could prove that under these definitions only finite languages are learnable by text presentation. Chomsky's question of adequacy-in-principle had to be answered in the negative for all classes of infinite languages considered. For informant presentation, however, 'learnability' proved to exist for a wide range of language classes (up to primitive-recursive).

Though one has to be quite careful in generalizing these results to natural languages, it seems safe to conclude that under reasonable assumptions natural languages are 'learnable' by informant presentation, but not by text presentation (see Levelt 1974 for a detailed discussion).

Further work in grammatical inference has added to Gold's findings in several respects. Stochastic models for learning by text presentation were developed (Horning, 1969) in order to find ways for inferring a least complex grammar for L (not just any grammar). One started experimenting with weaker definitions of learnability, such as the requirement that each non-adequate grammar in the hypothesis space should be rejected within finite time (Horning 1969, Feldman, 1970). And for cases where 'learnability' could be proven, one began studying the efficiency of different inference procedures (akin to Chomsky's ~~real-time~~ feasibility issue), often noticing that even very clever heuristics could not prevent astronomical learning times for languages of context-free and higher classes. It is also true for these later developments that generalizations to natural language is somewhat premature. It seems rather safe, however, to say that for text presentation there is either no 'learnability', or inference makes very unrealistic demands on computing time and tape space. Informant presentation certainly gives better prospects (see Levelt, 1974).

From the point of view of LAD three things are notoriously absent in the literature on grammatical inference:

(1) Very little attention has been given to other varieties of presenting the language than text or informant presentation. Both are so little restrained forms of input that they are quite unnatural with respect to the linguistic environment of the child, as we will discuss shortly. What is much needed, in our view, is the study of what we will call *intelligent text presentation.*

(2) The whole inference literature is purely syntactic. There is no formal work on inference where there is a *semantic* component to the grammar. One could imagine several varieties of such work. One might allow for input of the sort 'strings x and y are paraphrases', or 'if x is true, then y is also true', etc. One could choose among different semantic formulations, such as model theoretic approaches (cf. Suppes, 1971), propositional languages (cf. Cresswell, 1973), etc.

(3) The inference literature is exclusively linguistic. I know of no work where the inference of a language is studied in the wider context of inferring a representation of a world-to-be-talked-about, i.e. a model of *cognitive inference.* Appealing 'language plus world'-models have been developed in the artificial intelligence literature (cf. Winograd 1972, Schank 1972), but no inference work seems to be available as yet.

We shall now turn to the fate of LAD in the (psycho-) linguistic literature, and show that the empirical assumptions on which a formal approach to language acquisition was initially based could not be maintained. No formal models, however, were available for the description of new empirical challenges, since these implied precisely the existence of intelligent presentation of language, as well as roles for semantic and general cognitive factors in language acquisition.

3. DEVELOPMENTS IN LAD

The initial impact of Chomsky & Miller's LAD-model on the (psycho-) linguistic approach to language acquisition was enormous. Numerous researchers in the early sixties turned toward studying very early language development (Braine 1963, Brown & Fraser 1963, Ervin-Tripp 1964, McNeill 1966, and many others). For the first time in history grammars were written for the two-and three- word sentence stage in language development. A rather influential formalization was Braine's 'pivot-grammar'. Methods for the systematic sampling and analysis of early

child language were developed and yielded a wealth of new insights. Cross-linguistic studies were being initiated (Slobin 1966), and experiments on the effects of imitation, expansion and training on language learning were started (Brown & Bellugi 1964, Cazden 1965).

The theoretical framework in most of these studies was implicitly or explicitly the LAD-schema: the empirical work was often designed to substantiate the empirical assumptions underlying Chomsky's version of LAD.

It seems to me that the lion's share of these empirical assumptions fall into three categories: (1) the relative unimportance of input, (2) the marginal role of semantics, and (3) the cognitive independence of language. I will discuss these in turn.

3.1 *The Relative Unimportance of Input*

Most researchers were inclined to assume that the linguistic environment of the child is very little restricted. A typical and not at all far-fetched statement along these lines can be found in Fodor (1966):

> (...) the child gets a *corpus*. That is, he gets a sample of the kind of utterances fluent speakers of his language typically produce. It is conceivable that this sample is biased in certain respects in comparison to a purely random sample.

It is then added that the language addressed to children could be simplified and that research on this matter is going on.

> Until the results of this research are known, however, it would be methodologically sound to assume that the child's increasing linguistic proficiency is not to be attributed to any significant extent to the special character of the utterances he hears.

In fact, the corpus is assumed to extend far into the ungrammatical domain:

> If it is anything like a randomly selected corpus of adult utterances, it must contain a very substantial number of false starts, slips, grammatical mistakes, and so forth.

Since "much of what children hear is overheard and (...) all normal children learn to speak", language should be learnable under a very wide variety of input conditions. No wonder that, as Eve Clark (1973a) remarks,

From this, it has been concluded that the child could not possibly learn the syntax of his language unless he was endowed with some innate, language-specific, mechanism for just that purpose.

These nativist assumptions led to intensive search for early language universals. Since the pet idea of transformational linguists at the time was that the base grammar of different languages would be very similar or universal, whereas the transformational component would be more language-specific, it is not surprising to read:

Accordingly we should expect to find that the earliest grammatical production of children will contain the abstract features of the deep structure but few of the locally appropriate transformations. Young children should 'talk' deep structures directly. And that is precisely what an examination of children's early speech shows (Miller & McNeill, 1968).

Such dogmatic and empirically untenable positions had to be taken to protect the idea of spontaneous emergence of language. This tabu did not hold for transformational development. Transformations had to be required specifically through scrutinizing the linguistic input. The initial studies in early transformational development (cf. Bellugi 1967, Menyuk 1963, 1964, C. Chomsky 1969) were therefore much less prejudiced and still have not lost their significance.

Within the LAD-model, the nativist position could be very easily formalized. Learnability can either be increased by narrowing LAD's hypothesis space, or by making the inference procedures very powerful or 'clever'. Chomsky & Miller (1963:276-277) do not hesitate to make the nativist choice:

The proper division of labor between heuristic methods and specification of form remains to be decided, of course, but too much faith should not be put in the powers of induction, even when aided by intelligent heuristics, to discover the right grammar. After all, stupid people learn to talk, but even the brightest apes do not.

And in Chomsky (1965) we read:

This requires a precise and narrow delimitation of the notion 'generative grammar' – a restrictive and rich hypothesis concerning the universal properties that determine the form of the language.

In terms of LAD the rationalist position means: relative unimportance of the observation space plus very restrictive hypothesis space, whereas the empiricist position would be formalized as: a very wide or unspecific hypothesis space plus an important role for the observations which are analyzed by powerful inductive heuristics. Chomsky (1965) tries to give

his choice the appearance of a logical necessity. Discussing the earlier mentioned questions of 'adequacy-in-principle' and 'feasibility' of LAD as a model for human language acquisition, he remarks:

> In fact, the second question has rarely been raised in any serious way in connection with empiricist views (...) since study of the first question has been sufficient to rule out whatever explicit proposals of an essentially empiricist character have emerged in modern discussions of language acquisition.

Here, Chomsky is using the shield of non-existing results in computer science. As we have noticed above, the first definite results in learnability were obtained two years later by Gold (1967). These results moreover, if generalizable to natural language would indicate that no adequate procedure exists for inferring a natural language by text presentation, irrespective of computational power, i.e. both a rationalist and an empiricist version of LAD would be inadequate-in-principle for text presentation (see Levelt 1974). Braine (1971) makes it rather likely that, from the point of view of syntax the child is very much in a situation of text presentation since speech to children is highly grammatical (we will return to this), syntactic corrections are seldom made, and marked negative instances are hardly ever presented. (Braine uses these observations as an argument against the rationalist version of LAD, but it applies to the empiricist version as well. See Levelt 1974 for a more detailed discussion.)

The applicability of results in computer science to natural language is still very much an open issue, as we have seen. But it should be clear that already as early as 1965, Chomsky had lost contact with relevant developments in computer science, a situation which remained also characteristic for all (psycho-)linguists working in the field of language acquisition. (A notable instance is Peters' (1972) article on inferring grammars. The paper, though quite interesting in itself, lacks any reference to the post-chomskian literature on grammatical inference. The inference problem is introduced from scratch, so to say.)

In a later section it will be discussed how the empirical assumption of a rather unrestricted linguistic environment for early language development became challenged, but we first turn to the second empirical assumption on which the early LAD-studies were based.

3.2 *The Marginal Role of Semantics*

LAD had been conceived as a device for learning a grammar. In 1957 Chomsky's study of grammar was independent of semantic considera-

tions, and he tried to realize the same for the study of language acquisition. The role of semantic input in the learning of language was minimized:

> For example, it might be maintained, not without plausibility, that semantic information of some sort is essential even if the formalized grammar that is the output of the device does not contain statements of direct semantic nature. Here, care is necessary. It may well be that a child given only the input of (1) [LAD] as nonsense elements would not come to learn the principles of sentence formation. This is not necessarily a relevant observation, however, even if true. It may only indicate that meaningfulness and semantic function provide the motivation for language learning, while playing no necessary part in its mechanism, which is what concerns us here (Chomsky, 1962).

And in *Aspects* (1965) Chomsky repeats essentially the same arguments.

Consequently, most work in early grammars was purely syntactic in nature, and one tried to argue for the correctness of this approach in several ways. We find methodological arguments, such as Fodor's (1966):

> The difficulty with relying upon 'semantic' considerations in explaining language learning is not, then, that such considerations are known to be irrelevant but simply that we do not know how to describe them in any revealing way.

And we find empirical arguments. As Eve Clark (1973a) notes, an experiment on the learning of an artificial language by Miller & Norman (1964) seemed to have reinforced Chomsky in his claim, since "subjects learning the language with semantic reference appeared to learn in exactly the same way as subjects not given any semantic information". In her paper, Eve Clark then shows by reference to the work of Moeser & Bregman (1972) how much these early results were determined by the experimental procedure used, and how important the role of semantic input turned out to be in Moeser & Bregman's study.

3.3 *The Cognitive Independence of Language*

The LAD-model was not only purely syntactic: it also implied the tacit assumption that language development could be satisfactorily explained *in vitro*. LAD would only need linguistic input, and the procedures would be sufficient to derive a grammar. Neither non-linguistic (i.e. visual, kinesthetic, etc) input, nor non-linguistic foreknowledge would be essential in a model of language acquisition. Notably missing in the early LAD-studies are discussions of the knowledge structure that the child has acquired before the first grammatical structures arise. This language-*in-vitro* approach

was closely related to dominant opinions on the status of linguistic competence in the adult. Competence was considered to be an autonomous faculty of mind, which might interact with other psychological factors in the causation of linguistic performance, but which could never be confused with these factors (see Levelt 1972 for an analysis of the psychological status of competence).

Syntactic development is a respectable field of study, but negating the importance of cognitive factors for its explanation is less respectable:

> It is tragic to cutt off from the domain of research the large field of cognitive relations which are found in early sentences (...) by assuming *a priori* that there are no interesting problems in their acquisition. Dogmatism without evidence is to say the least presumptuous (Ervin-Tripp, 1971).

So it appears that the early work on LAD showed the same limitations as those we observed for grammatical inference theory: little attention to varieties of language presentation, ignorance of semantics, and ignorance of non-linguistic variables.

During the second half of the sixties linguistic attention turned to these much neglected areas, leading to the obsolescence of LAD, and to the rediscovery of older European and American traditions in the study of language acquisition. In a last paragraph we will touch on each of these three areas in a very summary fashion.

4. LATER DEVELOPMENTS

All three characteristic empirical assumptions on which LAD was based were challenged by later developments as we shall now discuss.

4.1 *The Linguistic Environment*

The assumption that the child has to acquire his language in a virtually unlimited linguistic environment, mainly consisting of overheard material full of lapses, false starts and errors became rejected on good empirical grounds. Brown & Bellugi (1964) had already noticed that the speech of adults to children is mostly very simple and grammatical. However, it was only around 1970 that a real boom of studies appeared about how adults speak to children. Eve Clark (1973a) gives a summary, but additional work is appearing fast. We mention studies by Berko Gleason (1973), Bowerman (1973), Broen (1972), Brown & Henlon (1970).

Clark (in press), Ervin-Tripp (1970, 1971), Farwell (1973), Friedlander et al. (1972), Holtzman (1972), Moerk (1972), Phillips (1973), Remick (1972), Sachs et al. (1972), Shatz & Gelman (1974), Shipley et al. (1969), Snow (1972a, b), and Van der Geest et al. (1973).

From these studies it appears that adults in addressing children use short, simple sentences with little embedding and inflection (Sachs et al., Snow). Sentence boundaries are well marked in speech to young children (Broen). More generally, intonation is high and 'exaggerated', clearly marking for the child what he should attend to. Overheard speech is therefore not to be considered as important imput. (Labov (1970) moreover showed that such adult-to-adult speech is not as ungrammatical as had been generally supposed.) The syntactic complexity of adult's speech grows with the child's syntactic competence. More specifically, it seems that new semantic features are introduced by the child, to which the adult reacts with the more advanced syntactic construction by which they can be expressed (Van der Geest et al.). Much adult effort goes into elicitation of specific reactions. Eve Clark (in press) shows that conversational patterns are trained (*Where's the ball? Here's the ball.*), by means of slow and explicit routines. There is a high incidence of questions in adults' speech to children (Ervin-Tripp), apparently to check whether the child is still following.

From the point of view of the syntactic structure of the child's 'observation space', all this amounts to what I called earlier 'intelligent text presentation': the child is presented with grammatical strings from a miniature language, which is systematically expanded as the child's competence grows. As we have seen, the literature on grammatical inference gives little attention to intelligent information presentation, but it is noteworthy to cite a remark by Horning (1969), which was made before these studies became available. After having discussed the real time problems into which even succesful procedures for grammatical inference are running, he writes:

> does language acquisition by children suggest means for improving our grammatical inference procedures? We believe that it does, and we conjecture that an important distinction between the child's experience and that we have assumed for our procedures is this: The child is not initially presented the full adult language he is ultimately expected to learn. Rather, he is confronted with a very limited subset, both in syntax and vocabulary, which is gradually expanded as his competence grows.

The conclusion, then, is

We should not expect our inference procedures to perform well when confronted directly with complex languages,

and it is suggested that the procedure should first be exposed to small sublanguages, which are later combined and expanded.

These perceptive remarks have not been followed up in computer science, though work on interactive programming (cf. Klein & Kuppin, 1970) seems to go in the right direction. The result is that at present no formal models of the LAD variety are available to psycholinguists for the analysis of their new empirical findings on adults' speech to children. It should, however, be obvious that from the purely syntactic point of view the urge for strongly nativist assumptions has been diminished by these findings. Nativist assumptions now enter at other places as we shall see.

4.2 *The Role of Semantics*

Chomsky's assumption that semantic information is non-essential for the manner in which syntax is learned, has not only been challenged by work on the learning of artificial languages (such as Moeser & Bregman's), but especially by careful study of language development in children. Examples of such studies are Slobin (1970), Bloom (1970), and Schaerlaekens (1973). In these studies one derived the *intention* or *semantic function* expressed by an utterance from the context in which it was spoken. It is obvious that the child masters such semantic functions long before the two-word stage, i.e. he may already know that something is typically *located* at a certain place, that something *belongs* to somebody, etc. At learning a language, the child tries to cast such semantic relations in grammatical form by choosing a particular word order, inflexion, etc. One could of course still think of the existence of a priori and universal grammatical means to express such intentions. But Schaerlaekens (1973) shows rather convincingly that this cannot be maintained: the child tends to use a particular word order to express a certain semantic function, and more often than not the order chosen is the dominant word order in the native language or better: the dominant order of concepts, since categorial knowledge is notably missing in early child language. *Airplane by* can stand for an actor/action relation, though *by* is a preposition, not a verb. This latter example is taken from Schlesinger (1971), who gave a first formal exposition of this intentional approach to language learning (see Levelt 1974 for a more detailed discussion).

With respect to universality the obvious new insight is that the *intentions* expressed in early language are universal, there is no need to assume a priori knowledge about syntactic categories or word order. Slobin (1970) compared two-word sentences which he collected from children (aged 1;6-2;0) in six different language communities. He remarks the following about their striking correspondence:

> If you ignore word order, and read through transcriptions of two-word utterances in the various languages we have studied, the utterances read like direct translations of one another (...). There is a great similarity of basic vocabulary and basic meanings conveyed by the word combinations. There is a small class of frequently-occurring operators performing basic functions, and a large number of content words.

As examples of basic functions Slobin mentions ostension request, negation, question. Typical semantic relations are conjunction, attribution, genetive, locative, subject-object, etc.

It was noted earlier that no inference models for this sort of data are available in computer science. Interesting is to observe that though there is a near absence of syntactic corrections in mothers' speech to children, frequent corrections are made with respect to the truth value of the child's utterances (cf. Brown & Henlon, 1970). This could be conceived of as a semantic form of informant presentation, as Eve Clark (1973a) remarks.

4.3 *Language Development as Part of Cognitive Development*

Closely related to the recognition of semantic factors in the causation of grammar is the rejection of the third assumption underlying the original LAD-approach: the cognitive independence of language. In retrospect it is impossible to indicate which publication was first to challenge this assumption. One can find early suggestions along these lines even in the most orthodox LAD-literature. An example is McNeill's (1970) proposal to distinguish between strong and weak linguistic universals. The first would be the reflection of a specific *linguistic* ability "and may not be a reflection of a cognitive ability at all" (*nota bene* the contrast). The second reflects a universal *cognitive* ability. Cognitive abilities can, therefore, cause specific linguistic structures. This latter point is rather more strongly made in two influential papers by Bever (1970a, b). He argues that

certain grammatical rules themselves may be shown to be structural accomodations to behavioral constraints. Thus certain universal structural properties of language may express general cognitive constraints rather than particular innate linguistic structures (1970 b).

These constraints are to be found especially in features of perceptual *processing*, as Bever demonstrates by means of various examples.

Slobin (1971), after noticing that

the first and most obvious point that comes to mind is that language is used to express the child's cognitions of his environment — physical or social — and so a child cannot begin to use a given linguistic form meaningfully until he is able to understand what it means,

puts the critical question without restrictions:

Is it possible, then, to trace out a universal course of linguistic development on the basis of what we know about the universal course of cognitive development? (Can one take Piaget as a handbook of psycholinguistic development?)

The latter addition puts the new concern in due historical context. Not only Piaget's work, but most of the pre-chomskian tradition in language acquisition research had been based on the assumption that the development of language proceeds from and is part of the general cognitive development of the child. Such was also the position taken by the pioneers like Preyer (1882), Wundt (1885), C. & W. Stern (1907), Bühler (1918), and many others. The special mention of Piaget, however, is not without significance. Firstly, Slobin acknowledges the importance of the Genevian studies on language acquisition by Sinclair-de Zwart (1967, 1969), which form the *trait-d'union* between the Piagetian and Chomskyan traditions in language acquisition (see also Sinclair-de Zwart, 1973). Secondly, Piaget is apparently referred to as an encyclopedia. And indeed, if one wants to know how the child builds a knowledge structure through interaction with his environment, the most sensible step to take is to start from Piaget's epistemological theory, since it is the most elaborate and best founded today.

Cognitive studies of language development are so numerous these days, that an even very summary review is unfeasible in the present context. Several conferences have been devoted to the relation between cognition and language development, and we better refer the reader the following proceedings: Hayes (1970), Flores d'Arcais & Levelt (1970), Moore (1973), Ferguson & Slobin (1973), Connolly & Bruner (1974).

Essential in the present context, however, is the question what sort of experimental paradigm is required to demonstrate the correctness of the

new (and old!) empirical assumption. One should be able to show that a certain knowledge structure and certain information processing strategies are available before a particular linguistic form emerges, and that initially linguistic information is analyzed *in terms of* that non-linguistic knowledge structure by means of these non-linguistic processing strategies. This is not easy to accomplish. One mostly relies on a weaker paradigm: the demonstration that a particular way of processing linguistic material by the child is accompanied (and preceded) by the same way of processing certain non-linguistic material. This paradigm requires therefore a linguistic plus a non-linguistic experiment. The latter, however, is often omitted. Slobin's (1971) paper, which was especially written to comment upon the link between early non-linguistic processing and language acquisition does not give a single example of an independent non-linguistic experiment. In this way one goes around a vicious circle: in order to proof the cognitive basis for a particular fact of language behavior one 'translates' a particular linguistic processing strategy in general cognitive terms, and the latter is then taken to be the basis of the former. Fortunately, examples of correct application of the paradigm are available. A beautiful case with very positive results is presented by Eve Clark (1973b).

Returning to LAD, and after rejection of all three empirical assumptions on which it was based, one should ask what remains of a nativist approach to language acquisition. We have already noticed the existence of a remarkable universality in the semantic functions that are expressed in early language. The cognitive approach might provide an explanation for such universals, and the explanation is nativist to a certain extent though most empiricists would have no problems with it. We close with a citation from Herbert Clark (1973) in which the idea is clearly expressed:

> the child acquires English expressions of space and time by learning how to apply these expressions to the a priori knowledge he has about space and time. This a priori knowledge is separate from language itself and is not so mysterious. The knowledge, it will be argued, is simply what the child knows about space given that he lives on a planet, has a particular perceptual apparatus, and moves around in a characteristic manner. The exact form of this knowledge, then, is dependent on man's biological endowment – that he has two eyes, ears, etc., that he stands upright, and so on – and in this sense it is innate.

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