

EXPLANATIONS IN THE STUDY OF CHILD LANGUAGE DEVELOPMENT

Richard Martin Atkinson

Ph.D. University of Edinburgh 1978



ACKNOWLEDGEMENT

I would like to express my thanks to the many friends and colleagues in the Department of Linguistics at the University of Edinburgh and the Department of Language and Linguistics at the University of Essex whose stimulating conversation and discussion over the years has made this thesis possible. In particular, my ex-colleagues on the SSRC Language Acquisition Project in Edinburgh provided an environment which was both warm and intellectually exciting, a rare combination. They are Patrick Griffiths, Alison Macrae and the late Renira Huxley. A special debt of gratitude goes to John Lyons both for his involvement with the Edinburgh project and for his continued support and encouragement throughout the last few years. More recently Ron Asher was good enough to take on the burden of supervision and has shown considerable understanding on the run in.

Angela, Ben, Daniel and Sarah have been deserted many times particularly during the last twelve months. My thanks to them for being so good to come home to.

Sue Rhodes did the typing in a remarkably short space of time and remained efficient and understanding under a great deal of pressure. Thanks to her.

CONTENTS

CHAPTER 1: CRITERIA FOR ADEQUACY	1
CHAPTER 2: MODELS OF LEXICAL DEVELOPMENT	52
2.1 Clark's semantic feature model and early concrete vocabulary	54
2.2 Semantic feature theory and the acquisition of relational terms	81
2.2.1 Empirical work on relational terms	82
2.2.2 Antonymic dimensional adjectives	91
2.2.3 <u>More</u> and <u>less</u>	107
2.2.4 <u>Temporal</u> conjunctions	111
2.2.5 <u>Come</u> and <u>go</u>	120
2.3 Nelson's functional core concept and its application to lexical development	129
2.3.1 Nelson's critique of 'abstraction theory'	131
2.3.2 'Functional core concepts' as an explanatory theory	145
2.4 'Prototypes' and the development of colour terms	158
2.4.1 Basic colour terms and focal instances	159
2.4.2 Heider's experiments	163
2.4.3 A tentative model and its status	165
2.4.4 'Prototypes' and general referential vocabulary	173
2.5 Basic objects	177
2.5.1 'Informativeness' and 'cue validity'	178
2.5.2 'Basic objects' in language acquisition	183
CHAPTER 3: THE GRAMMAR WRITERS	189
3.1 McNeill on grammatical classes	194
3.2 Early views on the syntax of negation	204
3.3 Bloom's grammars	212
3.3.1 The syntactic development of Gia	213
3.3.2 The syntactic development of Eric	229
3.4 Bowerman's grammars of Finnish	239
3.4.1 The grammar for Seppo at MLU 1.42	241
3.4.2 The grammar for Seppo at MLU 1.81	247
3.4.3 Bowerman's grammars and explanatory adequacy	251
3.5 Derivational complexity and the acquisition of transformations	267
3.6 Fourteen grammatical morphemes	278
3.6.1 Semantic complexity as a determinant of acquisition order	281
3.6.2 Grammatical complexity as a determinant of acquisition order	289

CHAPTER 4: LANGUAGE ACQUISITION AND COGNITIVE DEVELOPMENT	302
4.1 Language and cognition: general considerations	304
4.1.1 The definitional position	304
4.1.2 A spectrum of positions	306
4.1.3 The Reduction Condition	311
4.2 Arguments from semantic development	324
4.2.1 McNamara's views on meaning, syntax and cognition	327
4.2.2 Cromer on Brown	330
4.2.3 Cromer on Bloom	337
4.2.4 Cromer on Cromer	341
4.2.5 Antinucci and Miller on the development of temporal reference	348
4.2.6 Greenfield and Smith on cognition and the function of one-word utterances	357
4.2.7 Sinclair-de-Zwart, language and conservation	360
4.3 Arguments from syntactic development	363
4.3.1 Sinclair on formal grammar	363
4.3.2 Grammatical construction in playing with cups	369
4.4 Strategies in language development	377
4.4.1 Bever on the cognitive basis of linguistic structures	378
4.4.2 Slobin and cognitive prerequisites for grammar	380
4.5 Cromer on autonomous linguistic development	396
CHAPTER 5: PHONOLOGICAL DEVELOPMENT	402
5.1 Jakobson and the laws of universal solidarity	409
5.2 Smith and phonological rule systems	427
5.2.1 A sequence which assumes that the child controls the adult form	430
5.2.2 A sequence which assumes that the child's phonology is idiosyncratic	447
CHAPTER 6: THE ACQUISITION OF SPEECH ACTS AND CONVERSATION SKILLS	454
6.1 Dore and primitive speech acts	456
6.1.1 The first speech acts	457
6.1.2 Primitive speech acts and syntax	467
6.2 Gruber on performatives and constatives	473
6.3 Attention drawing and reference	483
6.4 Vocatives and attention	490
6.5 Bruner on the ontogenesis of speech acts	495
6.6 Halliday's functional model	509
CHAPTER 7: CONCLUSIONS	525
BIBLIOGRAPHY	531

CHAPTER 1 : CRITERIA FOR ADEQUACY

This work is principally concerned with theories in several areas of language development and, more particularly, with the status of proposals as explanations in some domain of this general area of study. In this introductory chapter I shall develop a set of concepts for evaluating the status of theoretical proposals made in a developmental context. I shall be essentially defining a set of standards which we might reasonably ask such proposals to meet if they are to be seriously regarded as adequate developmental theories, i.e. as having explanatory force within the domain in question. The discussion is necessarily general and abstract but, wherever it is appropriate, I shall illustrate points using hypothetical or real examples from language development.

There are two principal reasons for believing that this is an interesting and reasonable undertaking. The first is that it is useful from time to time for any discipline to be reflective and examine its own methods and standards critically. In the natural sciences theoretical insight and metatheoretical discussion have often gone hand in hand and we should always be aware of a proliferation of uncritical and uncriticized (from the standpoint of general principles) theories. The second, and more particular, reason is that the field of child language development appears to be in great need of such a reflective treatment. A good deal of the theorising with which I am acquainted lacks rigour. In itself this may not be a bad thing as

the material the child language theorist has to deal with is undoubtedly resistant to systematic formal analysis and, in such a situation, it would be misguided to insist on rigour and precision for their own sake. In addition, however, much of the theorising seems to lack direction and to take on a somewhat desperate quality where it becomes divorced from any overall view of human development. This is clearly undesirable and it is part of the function of this work to pinpoint some of the problems and provide a tentative framework for their solution. From my own point of view the former of these goals is the more important as a successful attainment of it amounts to an increased awareness of the problem of explanatory adequacy in child language theories.

At the outset I wish to make clear that I have no well-argued position to take on whether the analysis which follows is best interpreted as descriptive of those properties of theories of language development which contribute to their acceptability in the relevant scientific community or as prescriptive of what any theory of language development should look like in order to be taken seriously. Largely through the work of such philosophers of science as Kuhn and Feyerabend the former view is favoured among those who reflect on the status of theories in the natural sciences but it seems to me that we are in a position in language development studies where no theories have won the sort of universal acclaim which would enable us to partition proposals into a successful set and an unsuccessful set with any confidence. Therefore, although I would

like to suggest that the proposals which fare best under the analysis which follows are just those theories which have been judged reasonably successful and which have left the consumer relatively content, I am eager that too much shouldn't hang on this suggestion. To my knowledge there has been little systematic discussion of the issues raised here in the literature on language development and very little in the general field of child development and so it shouldn't be expected that the answers to such fundamental questions will be easy.¹

This lacuna of reflective criticism should not occasion much surprise as far as work on language is concerned as it is commonly agreed that it is only in the last few decades that the tools for beginning to formulate theories at all adequately in this domain have themselves been fashioned. I refer here particularly to the technical apparatus of descriptive linguistics. Without this apparatus discussions of language development were conducted in an often stimulating but theoretically impoverished framework. One can point to a concern with very general issues concerning the functions of language and their ontogenesis such as that found in Bühler (1934), to discussions of the emotional-intellectual issue as, for example, in Leopold (1949) and

1. Exceptions can be found dotted around the child language literature and, of course, there are some systematic treatments of the concept of development in the literature on developmental psychology (see, for example, Harris (1957), Mischel (1971)). Very often these latter concentrate on such issues as 'purpose' and 'teleology' which will not be central in the discussion which follows.

to discussion of the role of imitation and reinforcement in language development such as is found in the work of early behaviourists such as Watson (1928) and persisting in the work of Mowrer (1960). With some exceptions early work was conducted within some dominant psychological paradigm by investigators who were largely ignorant of the tools of descriptive linguistics.² During the second half of this century, however, and particularly during the last fifteen years, we have seen an enormous growth in the number of studies of different aspects of child language and a greater reliance on the concepts and techniques of descriptive linguistics as well as a rapprochement between linguists and psychologists leading to the development of theoretical statements which, in many cases, are clearly formulated, reasonably precise, and, one would expect, capable of evaluation. It seems to me that the time is ripe to submit some of these proposals to an analysis within a meta-theoretical framework.

The central problem to be discussed can be summarised very simply. Let us suppose that a language acquisition theorist specifies some domain of language development which is to

2. This somewhat dismissive tone should not be taken as implying a dismissive attitude. I believe that the pioneering work of Leopold as well as that of such scholars as Grégoire (1937), Guillaume (1927), Lewis (1936) produced a great deal of fascinating data and is, in many ways, superior to much of what we have seen in the last 15 years or so. However, the fact remains that it was not theoretically sophisticated and, in many cases, atheoretical in emphasis. Cf. Leopold's manifesto in the preface to Leopold (1939).

be studied (more generally we could consider the specification of any domain of an organism's structure or activity by a theorist concerned with that organism's development but in what follows I shall restrict myself to more particular formulations in terms of language and the human organism). To mention a few possibilities, this domain might be any of the following: the human's ability to produce syntactically structured utterances; the human's ability to comprehend such utterances; the knowledge which underlies the human's ability to produce or comprehend such utterances; the human's ability to produce the forms of words; the human's ability to perform speech-acts; the human's naming behaviour in referring to objects; the human's ability to comprehend the relational terms more and less. In mentioning these domains I do not wish to suggest that all of them would constitute equally fruitful areas of research, but they are domains which have been studied with some intensity in the recent literature and here they are cited merely to exemplify the notion of a 'domain of language development'.³ The theorist now studies the behaviour of the organism which is relevant to the establishment of a theory in the domain in question and,

3. Nor do I wish to suggest at this stage that they all constitute clearly delimited and independent areas of research. Furthermore, it may be nonsensical, as certain philosophers have urged, e.g. Stich (1971), Cooper (1975), to talk about 'knowledge underlying abilities' but child language theorists have manipulated such concepts and have constructed theories in such domains. A study of the activity of theorising can remain neutral on these questions.

as a developmental theorist, he does this at, at least, two times, say, t_1 and t_2 .⁴ For ease of reference I shall refer to the specified domain under study as D and we can now say that the first task for the theorist is to construct, for each of the times at which he has sampled the relevant behaviour, say, t_1, t_2, \dots, t_n ,⁵ a theory, T_1, T_2, \dots, T_n , such that T_1 explains the child's behaviour in D at t_1 , T_2 explains the child's behaviour in D at t_2, \dots , and T_n explains the child's behaviour in D at t_n . For example we can imagine that D has been fixed as the knowledge⁶ which underlies the child's ability to produce syntactically structured strings, that the data assumed relevant to D are the structured strings which are in fact produced by the child and that the theorist produces a sequence of grammars, G_1, G_2, \dots, G_n such that each of the G_i ($1 \leq i \leq n$) is related to D in the required way for each of the t_i .⁶ As a further example we can assume that D is fixed as the ability to produce the forms of words, that the data are the child's actual productions of words perhaps under defined conditions and that the theorist produces, as part of his theory, a set of sets of phonological contrasts,

4. Obviously the exact nature of the behaviour studied will depend upon the domain under investigation and the theorist's own predilections as to whether he is entitled to consult intuitions, whether a non-interventionist methodology is 'scientific', etc. Nothing in what follows depends on taking sides in these issues.

5. It is clear that in practice the t_i must denote time-intervals and not time points.

6. So far, of course, I have said nothing by way of explication of what 'the required way' is.

P_1, P_2, \dots, P_n such that, along with other components, each of the P_i ($1 \leq i \leq n$) is related to D in the required way at each of the t_i .⁷

In general then what we have so far can be represented diagrammatically as:

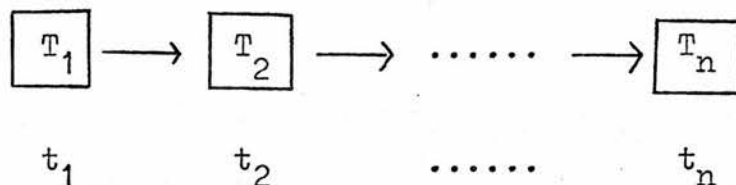


Figure 1.

where the arrows indicate the direction of development. I shall frequently refer to such sequences of theories by the ordered n -tuple (T_1, T_2, \dots, T_n) .⁸ The question which immediately arises is that of what is to count as an explanatory theory in D at t_i . This is not the topic with which I am going to be principally concerned in what follows although I shall mention it for the sake of completeness on many occasions without providing any further analysis.

7. As well as the set of phonological contrasts each theory will have to contain at least a set of procedures showing how a word form from the adult language involving contrasts not found in the child's system will be produced within the child's system. Such procedures could be phonologically conditioned substitution rules or, more simply, context-free rules.

8. Such a sequence of theories constitutes one aspect of a developmental theory. For another equally important aspect see the discussion in this chapter on p50 and the fuller discussion in Chapter 7.

Suffice it to say now that it is obviously a necessary condition on an explanatory developmental theory that it provide explanatory statements for any stage of the child's development in D.⁹ The problem as to what constitutes an explanatory statement for a stage in development seems to be indistinct from the problem of explanation in psychology and there are, of course, a number of positions taken by philosophers and psychologists on this complex issue. It is not my purpose to review these opinions here but perhaps the best known is that developed in Fodor (1968) where the author takes the view that an explanatory theory in psychology must meet at least two conditions:

- (i) the components of the theory are functionally specified i.e. in terms of what they do and how they are related to each other rather than in terms of how they are realised in some actual working model e.g. the brain.

9. The use of 'stage' here is to be interpreted neutrally. It is a necessary concomitant of a developmental study that behaviour will be sampled at a number of points. Analysis of the behaviour may yield discontinuous stages which correspond to points or sets of points in the time sequence or it may not. Nor is this to suggest that a stage theory can only emerge from prior consideration of data by purely observational and inductive methods. It is simply not relevant to my concerns whether the theories constructed at the time points have any stage related significance or not. See below for some discussion of discontinuities. Note further that often the developmental theorist restricts himself to just two stages t_C (a stage in childhood) and t_A (the stage of adulthood) and, although serious questions could be raised in this connection, I do not wish to suggest that theories of the form (T_C, T_A) should be ruled out as developmental theories.

(ii) the theory predicts behaviour, amounting to a specification of a behavioural repertoire, i.e. given certain input conditions the theory will produce an output which is interpretable in terms of the organism's behaviour in D and which, when so interpreted, is correct.¹⁰

To consider again particular examples, we can imagine D to be fixed as the ability to comprehend syntactically structured utterances and each T_i ($1 \leq i \leq n$) to consist of a grammar G_i ($1 \leq i \leq n$) and a functionally related model of short term memory M_i ($1 \leq i \leq n$). Each such T_i will be considered to provide an explanatory statement if the relationship between the G_i and the M_i is perfectly explicit and if T_i predicts that when presented with a certain utterance the child will behave in the fashion in which he does in fact behave when presented with that utterance. The difficulties with this notion of the child's behaviour in response to an utterance are, of course, well known but this hardly affects the logic of the situation. As a further example consider again that D is fixed as the ability to produce the forms of words and that T_i consists of a set of phonological contrasts, P_i ($1 \leq i \leq n$), plus

10. Fodor (1968, 136) says: "Hence, for an adequate simulation to be an adequate explanation it must be the case both that the behaviours available to the machine correspond to the behaviours available to the organism and that the processes whereby the machine produces behaviour simulate the processes whereby the organism does." Fodor later points out that the restriction to theories which are simulations is not necessary here.

additional components, X_i ($1 \leq i \leq n$). T_i will provide an explanatory statement in D if the relationship between each P_i and each X_i is perfectly explicit and if T_i predicts the child's behaviour in D under specifiable conditions. In this case we may go a little further and imagine that the specifiable conditions include an experimenter who asks a child to repeat certain word forms after him in which case each X_i will include at least components to do with understanding the structure of the experiment, understanding the instructions from the experimenter and certain preferences to do with pleasing the experimenter as well as those alluded to in fn.7 above. Again the vagueness of these proposals should not render the general argument opaque.

Whether or not we go along with Fodor's proposals, and they are certainly not applicable in any straightforward way to much of what follows,¹¹ it does seem that we are

11. This is because what follows is largely concerned with theories which have to be interpreted as involving the 'epistemological problem' and not the 'heuristic problem' to borrow a distinction from Pylyshyn (1973). In the transformational linguist's terms they are competence theories and, as is well known, the problem of providing criteria whereby such theories can be investigated by traditional psychological experimentation has proved an extremely difficult one. As far as developmental psychology borrowing standards from non-developmental psychology is concerned, Fodor hints that he might not go along entirely with what I am suggesting here: "I want to emphasise that what I have to say concerns a rather special kind of psychological explanation. The kind in which we account for the behaviour of an organism by reference to its

justified in asserting a first condition on explanatory theories in language development:

CONDITION 1

Given a theory T in the domain of language development D which amounts to a sequence of theories (T_1, T_2, \dots, T_n) then T is an explanatory theory in D only if T_1, T_2, \dots, T_n are explanatory theories in D at t_1, t_2, \dots, t_n respectively, where the predicate 'is an explanatory theory in D at t_i ' relies for its explication on adopting some view on the general problem of explanation in psychology.

This fundamental condition is not, as I have already pointed out, one on which I intend to focus in what follows. What it is vital to be clear about at this point is that the theory T is intended to be a developmental theory and that, as well as imposing conditions on each of the static theories, T_1, T_2, \dots, T_n , which make up T , we can also look to impose general constraints on the sequence of theories qua sequence such that the sequence provides us with a satisfactory theory of development. Put slightly more formally, we can search for a set of predicates, P_1, P_2, \dots, P_m , which take theories as

(cont from previous page) psychological states. This sort of account is perhaps more characteristic of certain branches of learning theory and perception theory than, say, social or developmental psychology." (1968, vii -viii). While agreeing that developmental theories admit of additional analysis it seems to me perverse to not regard the methodology of a static experimental psychology as contributing to the explanatory status of developmental theories.

arguments (I ignore here whether the predicates need all only be 2-place predicates and assume they can take as many arguments as there are theories in the sequence under consideration) such that $T (= (T_1, T_2, \dots, T_n))$ is an explanatory developmental theory only if $P_1 (T_1, T_2, \dots, T_n)$ $P_2 (T_1, T_2, \dots, T_n)$, \dots , $P_m (T_1, T_2, \dots, T_n)$.

There appear to be two immediate candidates for membership in the set of P's and I shall devote much of the rest of this chapter to exploring them. For simplicity's sake, except when providing general definitions, I shall restrict myself to a theory T which only samples two stages of development, i.e. $T = (T_1, T_2)$.

The first and most obvious requirement that we can impose on T is that T_1 and T_2 should be comparable, i.e. they should both be particular examples of theories constructed in accordance with general constraints on theories in D . What this amounts to is that the task of the theorist is seen as that of first constraining the notion 'theory in D ' in a completely general fashion. He then constructs, in accordance with his general constraints, and taking account of data from D at t_1 and t_2 , his two static theories, T_1 and T_2 . Because T_1 and T_2 are constructed in accordance with the same general theory they will have items in common. By 'items' here I wish to refer to all the constructs and construct-types which can occur in theories. It is a term which is neutral with respect to construct

and construct-type.¹² As an example we could consider again the case in which the T_i in a developmental theory are grammars, i.e. $T = (G_1, G_2)$ where G_1 and G_2 are constructed in accordance with some general theory of linguistic structure. Then G_1 and G_2 must have items in common at some level of generality. They may, for example, employ the same set of syntactic categories or, perhaps, one employs a subset of the set of syntactic categories employed by the other. The sets of rules occurring in the G_i may be identical, in a subset-superset relation or have a non-empty intersection. The intersections of both the sets of syntactic categories and of syntactic rules may be empty but both the G_i may employ rules of the same type and draw their syntactic categories from some antecedently defined set. In each of these cases I would wish to say that the G_i are comparable in the required sense. Thus the notion of comparability is only going to be valuable against a background of a previously defined general theory and we cannot, as a rule, assert that two theories, T_1 and T_2 , are or are not comparable simply on the basis of inspection of the theories.¹³ In

12. I borrow the term from Flavell (1971) where he introduces it as a "suitably noncommittal and general term for any sort of cognitive acquisition that a developmental psychologist might define and study. Thus, an 'item' might be a structure, skill, concept, rule, strategy, operation, belief, attitude, or any other cognitive element, large or small, that he has isolated for consideration." (422).

13. This is not to suggest that practice must always move from the general theory to the particular. Obviously the construction of theories should continue in parallel at both levels with suggestions and advances at one level having implications for the other level.

these circumstances we could always produce a disjunctive definition of a general theory such that T_1 and T_2 will be instances of it but this would amount to reducing the notion of comparability to vacuity. We would, of course, expect that, in the vast majority of cases, such a disjunctive theory would be a priori quite implausible and therefore we would not be tempted to compare the T_i in terms of it.

It should be clear that the notion of comparability, as explicated so far, is not an all or none notion, i.e. there are degrees of comparability. So to take again the example of $T = (G_1, G_2)$ it seems to make sense to say that G_1 and G_2 are less comparable if they merely make use of rules of the same general type and have no particular rules and no syntactic categories in common than if one or both of the intersections (of the sets of syntactic rules and the sets of syntactic categories) is non-empty. Nevertheless, in what follows I shall take reference to a general theory as diagnostic of comparability and thus treat the latter as if it were an all or none notion.

We can now examine, in a general way, some of the consequences of finding a $T = (T_1, T_2)$ such that T_1 and T_2 are not comparable (or only comparable to a small degree). The question as to whether this situation actually obtains in the language development literature is one to which I shall return on several occasions in the chapters which follow. To make the discussion slightly more concrete however we can imagine that D has been fixed as the knowledge

which underlies the ability to produce and understand syntactically structured strings and that T_1 and T_2 are grammars, G_1 and G_2 . First of all consider the extremely unlikely possibility of the theorist putting forward $T = (G_1, G_2)$ as an explanatory developmental theory and claiming that G_1 is a stratificational grammar whereas G_2 is a transformational grammar constructed in accordance with, say, the standard theory of Chomsky (1965). In this case G_1 and G_2 would be radically incomparable in that they would not share particular syntactic rules, arguably they would not share syntactic categories and they would not even share formal rule-types. Next consider the slightly less unlikely possibility (with D fixed in the same way) of the theorist putting forward G_1 as a case grammar constructed, say, along the lines of Fillmore (1968) and G_2 again as a standard theory transformational grammar. In this case G_1 and G_2 would share formal rule types, there would possibly be an overlap in the sets of syntactic rules and again it would be arguable as to whether the two grammars shared syntactic categories. In an obvious way the grammars presented in the second theory are more comparable than the grammars put forward in the first although not, we would probably wish to say, very comparable. As a final example we can consider D to be fixed in exactly the same way and for both G_1 and G_2 to be constructed in accordance with the standard theory of transformational grammar. In this case we would expect, at worst, a subset-superset relationship to hold between the sets of syntactic categories in G_1 and G_2

and to find a considerable overlap in the sets of particular rules employed in the theories as well as identity or near-identity of rule types. Straightforwardly in this last case the two grammars are more comparable than in the first two cases and this is seen as a direct reflex of the fact that they are constructed in accordance with the same general theory.

Returning now to the question at hand we can see that, in general to find that T_1 and T_2 are non-comparable or only comparable to a small degree would lead us to suspect the appropriateness of T_1 or T_2 or both and hence to suspect the status of $T = (T_1, T_2)$ as an explanatory developmental theory. This could lead us to search for reasons to abandon either T_1 or T_2 and to look around for a theory to replace the abandoned one that would be constructed in accordance with the general principles of the retained theory. However this is not the only possible response and an alternative which cannot be ruled out on a priori grounds is to believe that there is a more or less radical discontinuity in the child's development in D which is correlated with the switch from a theory of one kind to a theory of a totally different kind. This amounts to suggesting that the child develops for a certain period employing hypotheses of a certain sort in constructing his theories in D, suddenly abandons these and adopts, as it were, a completely new set of hypotheses.¹⁴ In what

14. The formulation of this point in terms of active hypothesis testing on the part of the child is, of course, not vital and a more neutral version could be formulated.

follows I shall assume that to adopt this position is to adopt the marked alternative as far as overall views of development is concerned and that, other things being equal, we should be suspicious of developmental proposals which embrace such discontinuities and, furthermore, that such discontinuities need to be argued for.¹⁵ We shall find a number of examples of such discontinuities in the literature and see that, in some cases, the theorist has felt obliged to search for explanations of the discontinuity or for arguments which render the discontinuity less radical. In other cases, however, the discontinuity simply stands without question and it is these cases which I am suggesting should arouse our critical spirit (see Schlesinger (1967) for the suggestion but little argument that such discontinuities should not be regarded as aberrant). Although the notion of comparability remains vague and unquantified except insofar as it is identified with the reference to a general theory, I feel that we can now propose a second condition on explanations in language development:

15. This might appear to be in conflict with the standard interpretation of Piaget's position on cognitive development which involves the postulation of qualitative changes at certain points (see Flavell and Wohwill (1969), Flavell (1971) for extensive discussion). However the Piagetian theory is cast within a framework where each 'novelty' can be seen as having its genesis in the items of earlier stages and, although it would be foolhardy to state categorically that the qualitative changes envisaged by Piaget do not involve the sort of discontinuity discussed in the text, it seems to me that analysis may show this to be the case.

CONDITION 2

Given a theory T in a domain of language development D where $T = (T_1, T_2, \dots, T_n)$ then T is an explanatorily adequate theory to the extent that T_1, T_2, \dots, T_n are comparable. This means that, at least, T_1, T_2, \dots, T_n must be constructed in accordance with the same general theory. If T_1, T_2, \dots, T_n are not comparable to a sufficiently high degree then additional argument is necessary to restore the explanatory status of T .¹⁶

Before leaving this analysis of comparability it is worth pointing out that it bears certain resemblances to the

16. One further question which is immediately raised by this general formulation and one which I am in no position to answer at the moment concerns the status of the predicate 'comparable' with respect to the logical property of transitivity. We can imagine that the notion of comparability has been quantified and that we have fixed some criterion, say c , such that the comparability relationship between T_i and T_j is deemed satisfactory only if T_i and T_j are comparable to some degree greater than c . The predicate we are then concerned with is 'comparable to degree greater than c '. It is easy to see that in a theory $T = (T_1, T_2, T_3)$, T_1 and T_2 may be satisfactorily related with respect to comparability as may T_2 and T_3 and yet this may not be the case for T_1 and T_3 , i.e. the predicate 'comparable to degree greater than c ' is not transitive. The extent to which this might be a problem does not become apparent in the analyses which follow and clearly its adequate treatment must await a more precisely formulated notion of comparability and fuller empirical knowledge about the child's progression in any one domain. Note also that considerations of transitivity can distinguish 'comparable to degree greater than c ' from 'constructed in accordance with T^* ' where the latter predicate when rephrased in a binary form is always transitive.

problem of commensurability as it has been discussed by philosophers of science (see, e.g. Lakatos (1970), Feyerabend (1975)) but it is not my intention, nor do I have the competence, to trespass in this field of enquiry. In addition, Chomsky's numerous discussions of the motivation for, formulation of and application of an evaluation measure as part of a general linguistic theory presuppose and sometimes make explicit the view that theories can only be meaningfully evaluated against each other (compared) if they are constructed on the basis of the same general assumptions.¹⁷ This idea has much in common with what I have been discussing above with one very important difference which will also be significant in a subsequent context. When two theories (grammars) are presented to the evaluation measure as part of the procedure of linguistic theorising one condition on them is that they both comprehend the same set of data: they are both observationally adequate. Using Chomsky's terminology the evaluation measure, if successful, will select (prefer) the theory which is not only observationally adequate but also descriptively adequate. But any theory presented to the evaluation measure is assumed to be observationally adequate and hence all such theories

17. "It is also apparent that evaluation measures of the kinds that have been discussed in the literature on generative grammar cannot be used to compare different theories of grammar; comparison of a grammar from one class of proposed grammars with a grammar from another class by such a measure, is utterly without sense." (1965, 38, emphasis in original).

are on an equal footing with respect to a privileged subset of the data,¹⁸ in this case the set of well-formed English sentences or the set of well-formed sentences in some other natural language. In the situation I have discussed above however the set of data in D do not remain constant from t_i to t_{i+1} and, of course, this remains true if we consider D to be a behavioural repertoire or a domain of competence rather than a set of attested data. To illustrate, if we again consider the ability to produce the forms of English words and collect a set of such forms at t_i from a child and another set at t_{i+1} producing theories T_i and T_{i+1} to explain the child's abilities, it is clear that, ignoring sampling differences which are of no importance in this context, the data to be explained by T_i and T_{i+1} will be quite different. The child will produce different forms for certain English words at t_{i+1} compared to t_i , he may be capable of producing some form for some English words at t_{i+1} whereas he had no such form available at t_i and so on. In addition there is no occasion for the comparison process I am concerned with to select or prefer one theory to another as they are theories for distinct sets of data. All of this is very obvious and we can safely conclude that, although Chomsky's evaluation measure is dealing in the same sorts of terms as the comparison process discussed

18. The reason I resort to this phrase is because it is clearly the case that additional data are brought to bear in formulating an evaluation measure, data distinct from that typified by 'S is a sentence of L'.

here as far as reference to general theory is concerned, the two concepts can in no sense be identified.

We can now turn to an examination of the second predicate definable on a sequence of theories. Of more importance in this work and also presupposing the notion of comparability, this second predicate involves a notion of simplicity. To prevent misunderstanding from the outset it must be emphasised that this concept of simplicity, whatever it is, cannot be identified with the idea of simplicity which has been extensively studied by philosophers of science and also by transformational linguists - this latter notion I shall refer to as 'the classical concept of simplicity'. Briefly the problem of simplicity in the philosophy of science has been construed in something like the following terms: suppose we have some domain of enquiry D and two or more theories which are equally supported with respect to observations in D . Then it has often been suggested that the simplest of the set of theories should be preferred where 'simplest' is understood as involving judgements to do with elegance and generality of rules, number of primitive terms, number of construct-types, etc. We can note also here the reliance in early transformational linguistics on a simplicity metric as part of the evaluation measure which, at its crudest, involved a straightforward symbol count (cf. Halle (1961)). For a refreshing and insightful treatment of the classical concept of simplicity arguing, among other things, that the linguist's concern, particularly in the domain of generative phonology, can be identified with

that of the general philosopher of science I refer the reader to Sober (1975) but I propose to spend no more time on it now the whole purpose of its introduction being so that it is perfectly clear that the classical concept is not the one I now wish to pursue. My certainty in this respect is derived from an observation already made concerning the data comprehended by theories. The point about the classical concept of simplicity was that it became applicable when two theories comprehended the same set of data but, as we have seen, in a developmental study the data comprehended by the theories we are interested in comparing, say T_i and T_{i+1} , are at least partially, distinct. So the concept of simplicity I am about to attempt to justify and pursue cannot be the classical one. How does the concept I am interested in arise? Rightly or wrongly it seems to be a working assumption of people working in child language research that, as children get older, they get more complicated and one immediate consequence of this is that we expect our theories of the child's behaviour or abilities to get more complicated as the child develops.¹⁹ To see this idea being used in a critical context it is possible to refer to Brown's (1973) criticism of Bloom's (1968, 1970) use of reduction transformations in the grammars she wrote for the children

19. Harris (1957) contains an enumeration of what the author considers to be essential aspects of development including: "... (3) movement over time towards complexity of organization; (4) hierarchization or the comprehension of parts or part systems into larger units or "wholes"..." (3, my emphasis - RMA).

under study on the grounds that the reduction transformation was something the children would have to 'unlearn' with the consequence that later grammars, in this very local sense, would be less complicated than earlier ones (see Chapter 3 for more extensive discussion). Fodor, Bever and Barrett refer to the position Brown criticises as 'mildly paradoxical' (1974, 487n) and so, although no logical incoherence attaches to the notion of the child and the theories which characterise his abilities becoming simpler as he gets older, there is the beginning of a tradition which is uneasy with such a suggestion.²⁰

To highlight the problem we can consider hypothetical examples. Let us assume again that D has been fixed as the domain of knowledge underlying the ability to produce and comprehend syntactically structured strings and that the theorist, sampling the relevant behaviour at t_1 and t_2 , has produced a theory $T = (G_1, G_2)$ where the G_i are grammars constructed in accordance with the standard theory. Suppose further that G_1 contains a set of 32 phrase-structure rules and 26 transformational rules (restricting attention to the most important rule-types in the standard theory) and that G_2 contains only 10 phrase-structure rules and 4 transformational rules and that each of these rules from G_2 also occurs in G_1 . In these circumstances, unless

20. It is necessary to add one important rider here. Some theories, particularly theories of phonological development are best interpreted as theories of constraints on the child's abilities or knowledge structures. Obviously we expect such constraints to be relaxed as the child gets older and the theories to become correspondingly simpler. This is discussed in detail in Chapter 5.

the theorist were to produce extensive argumentation to back his analyses and the manner of development, we would surely be justified in treating his theory with some suspicion and would be tempted to believe either that he has credited the child with too much at t_1 , perhaps because of overgenerous interpretation or that he has seriously undersampled the child's abilities at t_2 .

However the situation is not as simple as might first be supposed. To illustrate this we can consider another example which corresponds to an existing aspect of language development. Suppose we fix D as the ability to produce morphologically complex forms of English words and, in particular, we focus on the production of past tense forms. At t_1 we find a few regular past tense forms, e.g. walked, cooked, climbed and also some irregular pasts, e.g. came, went, slept, drew and, in addition, that the irregular past forms outnumber the regular past forms to a significant degree. At t_2 we find a relatively large number of regular past forms and approximately the same number of forms requiring an irregular past as we found at t_1 except that now the regular past tense ending is overgeneralised to these forms, i.e. we find such forms as comed, goed, sleped, drowed (I omit any consideration of the allophonic variation in the past tense morpheme as that is not what is at issue). Faced with these facts the theorist might very plausibly proceed by suggesting theories having the following characteristics: T_1 contains, in its relevant aspects, a list of basic verbal forms

paired with their appropriate past tense forms: on the basis of the data he has collected he doesn't appear to be justified in crediting the child with any generalisation. At t_2 , however, the child clearly does control a generalisation, in fact, an overgeneralisation and so T_2 , again in its relevant respects, will contain a version of the rule for forming past tense forms from basic verbal forms in English. Given the data it will be a rule which admits of no exceptions. Now it is easy to see that at some subsequent time, t_3 , when the child has sorted out his past tense morphology, the theorist will want T_3 to contain aspects of both T_1 (i.e. there will be a list which in this case amounts to a list of exceptions) and T_2 (i.e. there will be a rule which applies in all the non-exception cases) and, on this basis, we can intuit that T_3 is more complex than either T_1 or T_2 , but the difficult question arises in connection with the relative complexity of T_1 and T_2 . If it were the classical concept of simplicity in which we were interested then, presumably, T_2 would be preferred to T_1 on the grounds of elegance and economy of statement but, as has already been made clear, such notions are not at issue here and the question of preference simply does not arise. The situation we have is that of a formal device of a particular kind, in this case a context-sensitive rule, usurping the function of a formal device of a different kind, in this case a list, and it seems to me that there are no a priori judgements which we can bring to bear on this situation from the point of view of a developmental concept of

simplicity.²¹ There appear then to be three possibilities for what we might meet given the rough and ready notion of simplicity we are operating with and which awaits further analysis:

(i) of two theories, T_i and T_{i+1} , in a developmental theory $T = (T_1, T_2, \dots, T_n)$, T_i is simpler than T_{i+1} .

(ii) of two theories, T_i and T_{i+1} , in a developmental theory $T = (T_1, T_2, \dots, T_n)$ T_i is less simple than T_{i+1} .

(iii) two theories, T_i and T_{i+1} , in the developmental theory $T = (T_1, T_2, \dots, T_n)$ cannot be compared in any straightforward way with respect to their relative simplicity.

Before going on to examine the required notion of simplicity I would like to make three additional remarks. The first is that there are many cases of developmental theories in the literature which are amenable to analysis in terms of simplicity and which are not instances of the third possibility above as will become evident. This justifies further examination of the concept. Second, in connection with the troublesome case (iii), we might hope that arguments could be brought forward to substantiate the explanatory status of a theory which would render simplicity

21. A similar situation can be envisaged where, in developing grammars, transformational rules are presented as taking over the function of a complicated set of phrase-structure rules. Again on the grounds of formal elegance we might wish to say that the later grammar is simpler but for the developmental sense of simplicity no such assertion is justified.

criteria redundant. Without going into detail on this point which would take us too far afield, we can imagine that in some domain D the passage from t_1 to t_2 is marked in the corresponding theories, T_1 and T_2 by the introduction of theoretical constructs of type TC_2 perhaps over and above or perhaps entirely replacing constructs of type TC_1 ²² (in the morphological example above TC_2 would be 'rule' and TC_1 would be 'list'). Suppose further that in D', which, of course, need not be a linguistic domain, the passage from t_1 to t_2 is marked in the corresponding theories, T'_1 and T'_2 , by the introduction of theoretical constructs of type TC_2 , i.e. we assume there is some sort of isomorphism between the structures of the theories in the two disparate domains. Then, despite the fact that there is no obvious simplicity relationship between T_1 and T_2 , we could see the above state of affairs as contributing to the overall explanatory status of $T = (T_1, T_2)$.²³

The final remark I wish to make in connection with

22. If the constructs of type TC_2 are introduced over and above those of type TC_1 then it is possible that our complexity judgements would give us the unequivocal answer that T_2 is more complex than T_1 . It could nevertheless be argued that the considerations of this paragraph are relevant to this case: on the one hand we have additional arguments rendering simplicity considerations redundant and on the other we have similar arguments supplementing simplicity considerations.

23. To some extent this point anticipates the discussion of reduction in Chapter 4 and later discussion in the current chapter.

possibilities (i) - (iii) concerns our evaluation of overall theories of development on the basis of 'local' properties in the sequence of theories. Consider again a theory of development $T = (T_1, T_2, \dots, T_n)$ and assume that, according to some satisfactory notion of simplicity, T_1 is simpler than T_2 , T_2 is simpler than T_3 , ..., and T_{n-1} is simpler than T_n . In this situation we would obviously be justified in concluding that the theory T satisfies our simplicity criterion. But there is no reason to expect that we shall often come across this straightforward case and 'mixed' cases are going to cause problems. At this stage all it is possible to do is spell out alternatives. So, if we find a developmental theory $T = (T_1, T_2, \dots, T_n)$ such that, for all pairs (T_i, T_{i+1}) , $(1 \leq i \leq n-1)$, either T_i is simpler than T_{i+1} or there is no clear simplicity relation between T_i and T_{i+1} , then, particularly in the presence of an argument of the sort outlined above concerning the generality of the introduction of theoretical construct types, we should be prepared to assign T explanatory status, at least as far as considerations of simplicity are concerned. If, to consider another possibility, we find a T such that for all pairs of adjacent theories, (T_i, T_{i+1}) , either T_i is simpler than T_{i+1} or vice versa, then we should be suspicious of the status of the theory and further analyse the pairs which go the 'wrong' way in the simplicity judgements. Anything worse than the two 'mixed' cases described here would clearly deserve further analysis. Such analysis may simply involve the checking of sampling

techniques and, particularly in studies involving small numbers of children, attention to idiosyncratic variables such as the child's health at particular sampling sessions. In fact, in most of the cases which are analysed in the subsequent chapters of this work we are not presented with T's which embrace more than two developmental stages and so the problems are somewhat hypothetical for now.

In summary then we can describe the logic of our investigations so far in the following terms. Given a developmental theory $T = (T_1, T_2)$ we ask whether T_1 and T_2 amount to adequate explanations for the data in D at t_1 and t_2 . Assuming a positive answer to this question we then ask whether T_1 and T_2 are comparable or what is to some extent the same question, whether they are constructed in accordance with the same general theory. Often in what follows the question will centre not on whether theories in the sequence refer to the same general theory but rather on whether there is any general theory for any theory in the sequence to be related to. A negative answer to versions of this question leads us to search for special reasons for accepting what amounts to a discontinuity in development in D and a positive answer leads immediately to the question as to whether T_1 is simpler than T_2 in the required sense. An inability to answer this question in any clear way leads to an investigation of other domains looking for generalisations which will enable us to subsume the development in D as a special case; a negative answer leads us to question the status

of the theory while a positive answer leaves us content in at least this respect and we now move on to an explication of what is involved in providing a positive (or negative) answer. Before doing this we can state the third condition on the adequacy of explanations in language development:

CONDITION 3

Given a theory T in the domain of language development D which amounts to a sequence of theories T_1, T_2, \dots, T_n , then T is an explanatory theory in D only if for all i , $1 \leq i \leq n-1$, T_i is simpler than T_{i+1} , on the assumption that considerations of simplicity are applicable to the theories in T . A somewhat weaker version of this condition is formulable in accordance with the discussion above on 'mixed' cases.

I turn now then to a discussion of the relevant notion of simplicity as we find it in the child language literature.

The first category we must consider for the sake of completeness is the intuitive one. So, given T_1 and T_2 , we are told that T_1 is 'obviously' or 'self-evidently' simpler than T_2 and the sceptic only has to look at the two theories to check for himself. Without wishing to denigrate the role of intuition in any sort of scientific enquiry, by itself this intuitive notion of simplicity cannot constitute a sufficient foundation for T_1 being simpler than T_2 . It is an interesting question as to whether it constitutes a necessary condition but, in the

absence of a more formal theory of simplicity, we must put the question in abeyance. It will be one of the central arguments of sections of this work that in several domains of enquiry in language development this intuitive notion of simplicity is the only one which the investigator embraces and further analysis, in the terms to be developed below, often shows that intuition is not a transparent indicator. This is particularly true in the domain of syntactic development which receives extensive discussion in Chapter 3 and, to this extent, proposed explanations in this domain become questionable.

Of more substance and, in fact, forming one of the central themes of much that follows, is what we might refer to as additive or logical complexity. This is a notion which has already received some attention within the general theory of developmental psychology and so we find in Taylor (1971, 394) in a discussion of alternatives to a Piagetian account of cognitive development:

The major antagonist to a genetic psychology is thus an incremental view of learning, in which all development is seen as the addition (or sometimes subtraction) of homogeneous units such as Hull's SHr's (or "habits") linking stimuli and responses.

and again, in Nagel (1957, 17):

The connotation of development thus involves two essential components. The notion of a system possessing a definite structure and a definite set of pre-existing capacities; and the notion of a sequential set of changes in the system, yielding relatively permanent but novel increments not only in its structure but in its modes of operation as well. (My emphasis. RMA)

There are two remarks to make in connection with these statements. The first is that developmental theorists working within a Piaget-like framework which eschews the simple addition of elements of a single type in favour of the transformation and genesis of new structures will admit incremental development within stages and so, within such a framework, there is a place for the notion of additive complexity. There is also a sense in which later stages in the Piagetian view of development can be seen as containing and 'presupposing' earlier ones and perhaps the issue boils down not to additive vs. non-additive but rather to non-novel vs. novel (cf. fn. 15 above). The second remark is that theorists of language development, without regarding themselves as working within anything like a Hullean tradition, have nevertheless attended to aspects of development in additive terms. It therefore appears that we are justified in beginning to develop some general concepts in this regard.

Suppose, then, that we have a developmental theory $T = (T_1, T_2)$ and that we can regard the theory as involving a set of primitive substantive terms, $\{B_1, B_2, \dots, B_j\}$, and a set of formal principles, $\{F_1, F_2, \dots, F_k\}$ and that, furthermore, T_1 uses only a subset of both the set of substantive terms and the set of formal principles, say $\{B_1, B_2, \dots, B_h\}$ ($h < j$) and $\{F_1, F_2, \dots, F_i\}$ ($i < k$).²⁴

24. The distinction between substantive and formal aspects of theories is not vital to the discussion at this point although it will take on added significance in Chapter 4. It is based on the Chomskian distinction between substantive and formal universals although it obviously cannot be identified with that distinction.

Then, in an obvious sense, the vagueness of 'primitive substantive terms' and 'formal principles' aside, we can see that, if T_2 utilises the full set of substantive terms and formal principles, then it will involve everything that T_1 does plus some additional items and, in this sense, will be more complex than T_1 . Examples invoking a variant of this additive concept of complexity are legion in the child language literature although, to my knowledge, there has been no systematic analysis of it. One of the most careful pieces of work in this spirit and which is explicit with respect to its rationale is Brown and Hanlon (1970) which is an attempt to interpret the derivational theory of complexity first aired in experimental psycholinguistics in developmental terms. This study will be analysed in detail in Chapter 3. Less explicit but interpretable in a similar way, are such disparate pieces of work as E. Clark's (1973, 1974) attempt to construe the development of word-meanings in terms of the successive acquisition of features and Jakobson's (1968) speculations on phonological development which will be discussed in Chapters 2 and 5 respectively. As will become apparent these studies by no means exhaust the phenomena which have been examined in these terms. Before leaving the topic of additive complexity it is necessary to re-emphasise the problem which was raised in the general discussion of simplicity. There are undoubtedly a large number of instances of developmental theories which do not admit analysis in terms of simplicity and this will be even more frequently the case if we rule

out the intuitive notion of simplicity and insist on it being replaced by the additive one. There may be alternative notions of simplicity which will be applicable to the problematic cases or we may have to resort, as was suggested earlier, to alternative forms of argument to persuade ourselves of the explanatory status of our theory.²⁵

I want now to move on to what I consider to be a fundamental weakness in explanations which proceed simply on the basis of additive complexity and considerations of this issue raise what are undoubtedly the central problems of this work. Imagine that in some domain D we have a theory T which satisfies the conditions I have discussed so far, i.e. $T = (T_1, T_2)$, T_1 and T_2 are adequate explanations of the data in D at t_1 and t_2 respectively, T_1 and T_2 are both constructed in accordance with the principles of some general theory and T_1 is simpler than T_2 in the straightforward sense that T_2 utilises all the items T_1 utilises and some additional ones.²⁶ For the sake of

25. Obviously we have to be clear that Condition 3 is not a necessary condition on satisfactory explanations. If it is applicable then it must be satisfied but it need not be applicable. We can hope that, for theories to which it does not apply, alternative criteria will be developed but, as I have pointed out in the text, there appear to be enough proposals to which it does apply to justify its inclusion as a central condition.

26. In fact the considerations which follow apply equally well to theories to which Condition 3 is not applicable. Where necessary I shall indicate in footnotes alternative formulations but the main text will assume that we are discussing theories which do satisfy Condition 3 in an additive fashion.

clarity let us refer to the item structure of T_1 by 'X' and to that of T_2 by 'X+Y'. 'X' and 'Y' are best interpreted here as referring to 'chunks' of theory. They may, of course, be related by more than addition of items. What we have is a developmental sequence which can be represented as $X \rightarrow X+Y$ and the question to ask at this point is whether the theorist can provide reasons as to why we get this developmental picture rather than the alternative $Y \rightarrow X+Y$ because if T has the desirable features claimed for it then so does $T' = (T_2, T_1)$.²⁷ The question that is being asked here can, at a certain level of abstraction be compared to the request for explanatory adequacy in the theory of transformational grammar although the similarity shouldn't be pushed too hard (see Chomsky (1964, 1965 and many other places for extensive discussion). In both cases certain criteria for adequacy are established; in the case of transformational grammar, grammars must be observationally adequate and, in the case under discussion, theories must meet the sort of conditions described above. In both cases, the problem arises when it is realised that the criteria do not, in conjunction with the facts, provide us with a unique solution. In principle, the theorist is faced

27. Strictly speaking this is not true as Condition 1 will not be satisfied by T' if it is satisfied by T . This is one reason why the comparison which follows in the text should not be pushed too hard but I think it pinpoints the fact that we are now asking why development proceeds in the way it does and are not content to simply have a good theory which shows that it does.

with a surfeit of theories which satisfy the criteria; in transformational grammar we have a set of observationally adequate grammars and in language development we have a set of developmental theories. In both cases we require some additional machinery to determine the correct grammar (theory) from the presented set; in transformational grammar the correct grammar is the descriptively adequate one. In the case under discussion the correct theory is the one that actually corresponds to the course of development.

Although theorists of child language don't seem to have been systematically aware of this problem a large number of proposals found in the literature can be seen as attempts to deal with it and many of these involve reference to some other domain of the child's development which is regarded as more basic than the child's language development. In the examples I discuss in what follows this domain will often be the domain of 'general cognitive development' or, more particularly, perceptual development.

Before moving on to consider the logic of this situation it is useful to consider a special case where additive complexity can receive its justification from the actual structure of the theory in which the proposals are made. An answer to the question raised immediately above is then supplied by reference to Condition 2. Assume again that the theory can be described schematically as $X \rightarrow X+Y$. Then we can easily imagine situations where the theoretical constructs involved in Y depend for their intelligibility

on those involved in X, i.e. a theory which embraced those constructs which we find in Y without, at the same time, embracing those we find in X would not be a theory of the required type. To consider a particular example, we assume again that D is fixed as the domain of knowledge underlying the ability to produce and comprehend syntactically structured strings and that a theorist has put forward a theory $T = (G_1, G_2)$ where the G_i are transformational grammars constructed in accordance with the standard theory except that the degenerate case of a grammar without transformational rules (except for those involving lexical insertion) is permitted. Assume further that G_1 is such a degenerate transformational grammar and that G_2 is identical to G_1 in its non-transformational part and, in addition, contains transformational rules. In this case we have a very powerful reason for why we get the sequence $X \rightarrow X+Y$ rather than $Y \rightarrow X+Y$. This is that the theory represented here as 'Y' would simply not be a theory of the right sort, indeed, it would not be a theory of any sort,²⁸ since, within the context of transformational grammar, a set of transformational rules in a grammar presupposes a set of phrase-structure rules which build the structures on which they operate. Given the normal interpretation of transformational rules a grammar consisting exclusively of such rules is quite unintelligible.

28. What this means of course is that there can be no resort to a special argument for a discontinuity here as was discussed above. In this case we have something much worse than a discontinuity.

As a slightly more sophisticated example we can assume that D is fixed as before and that again $T = (G_1, G_2)$ where the G_i are as before. In this case assume that G_1 and G_2 are identical except that G_2 contains a single rule which is not contained in G_1 and which, furthermore, is intrinsically ordered with respect to some rules in G_1 , i.e. the rule, call it R_n , depends on the prior operation of some subset of $\{R_1, R_2, \dots, R_{n-1}\}$ for its operation. The situation as described then can be represented as $X \rightarrow R_n + X$ and again we can make the obvious point that in the schematic representation $R_n \rightarrow X + R_n$, R_n would not be a theory of the required type. However we can also go further and, on the assumption that, say, R_m ($m < n$) is one of the rules in G_1 with respect to which R_n is intrinsically ordered, we can see the theory - internal reason for why we find the sequence $X \rightarrow R_n + X$ rather than $(X - R_m) + R_n \rightarrow X + R_n$ where, I hope, the rather bizarre use of arithmetic operators is self-explanatory.²⁹

As one final example of a rather different sort we can consider the ability to produce or perform speech-acts. Assume that the successful performance of certain speech-acts depends on the fulfilment of appropriacy conditions and that, in addition, these appropriacy conditions can, in certain cases, only be realised by the performance of

29. Note that in this case it is not obvious that $(X - R_m) + R_n$ is not a theory of the required type unless the general theory contains a clause prohibiting vacuous rules.

There seems to be no reason why it shouldn't.

other speech-acts of a distinct type. Assume that a particular speech-act, S_n , has attached to its successful performance an appropriacy condition, A_m , which can only be realised by the performance of another speech-act, S_m . The analogies between this situation and that already discussed in connection with intrinsic ordering should be clear. Briefly, if the theorist produces an inventory of speech-acts as part of his theory, then we would expect to find the developmental sequence schematised as $X \rightarrow S_n + X$ where X contains S_m and we would not expect to find the sequence $(X + S_n) - S_m \rightarrow X + S_n$. Several examples of this sort of theory-internal explanation for the order of development will be found in the chapters which follow and it seems to me that, in the present state of research, this is probably the strongest way in which the question confronting us can be answered.³⁰ If a theory of language development can be justified in this theory-internal way, I shall say that the theory is logically grounded.

Leaving this special case we now turn to the attempts already alluded to to reduce a supposed explanatory statement in language development to one in some other domain of the child's activity and thereby to contribute

30. The extent to which this is true seems to depend on the confidence we have in our theories as theories of adult behaviour or adult knowledge-structures. If we have a theory in which we are extremely confident which is put together in a certain way then surely it follows that items can only be acquired in an order which reflects this structure and this should count as an explanation for the order of development.

to the explanatory status of the linguistic theory. The assumption is usually implicitly made that the explanatory statement in the new domain has whatever features we want it to have. So, beginning to set up a general framework, we have a theory T which can be represented as $X \rightarrow X+Y$ and we have no reason available for believing that this is the sort of theory we shall meet in D rather than some alternative, in particular, $Y \rightarrow X+Y$. The solution frequently proposed, although it is not proposed as the solution to this problem in these terms, is to refer the development to another psychological domain and in Chapter 4 the domain which will receive extensive discussion is the domain of general cognitive development. The assumption is that this latter is, in some sense, more basic than the domain of language development we start from and the solution requires, what often remains implicit, reference to a developmental theory of cognition which we could represent as $x \rightarrow x+y$ where 'x' and 'y' refer to 'chunks' of cognitive theories in an analogous way to 'X' and 'Y' for linguistic theories.³¹ There are a number of remarks to make in connection with this outlined procedure and one of the most difficult tasks facing an analytic approach to child language theories is to be precise about what is involved in the sort of claim we are going on to examine.

31. If we consider theories to which Condition 3 is not applicable then we have a theory in the relevant linguistic domain schematised as $X \rightarrow Y$ and implicit reference to a theory of cognitive development schematised as $x \rightarrow y$.

Let us begin with an obvious remark and that is that there is little point in transferring the burden of explanation from an autonomous theory of language development to a general theory of cognitive development if this latter doesn't satisfy the various criteria for constituting an adequate developmental theory, i.e. we can ask exactly the same questions of the sequence of cognitive theories, (C_1, C_2, \dots, C_n) as we have already asked of the sequence (T_1, T_2, \dots, T_n) in some linguistic domain.³² So far as I know there has been little systematic discussion of these issues in the literature on cognitive development (but cf. references cited earlier).

Equally important if slightly less obvious is the fact that there must be some reasonably effective way of pairing concepts in the two sequences of theories, i.e. there must be something like correspondence rules mapping the items of the theory of language development onto the items of the cognitive theory.³³ Whether this sort of reductionism is desirable or even possible is an interesting question. There now seem to be compelling reasons

32. Clearly at some point the buck must stop being passed and we must refer to non-reducible aspects of development operating with such concepts as 'maturation'. The fascinating question is that which asks exactly where this point is. I shall return to this question briefly in Chapter 7.

33. In Chapter 4 these issues are taken up again in much more detail where it is argued that the informal statement in the text constitutes one necessary condition on the reduction.

why we shouldn't expect a reduction of, e.g., psychology to physics via neurophysiology, biochemistry, etc. while still maintaining a materialist stance on the reference of theoretical terms in psychology (see Fodor (1976)), and, presumably, similar sorts of arguments could be constructed within the domain of developmental psychology although Fodor's discussion doesn't make clear his position on this issue. For the sake of argument, however, let us assume that such reductionism is possible. Then clearly it will be mediated by some sort of correspondence rules.

Note that even with these two conditions satisfied there remains at least one residual problem. The situation we have is a theory of some aspect of language development schematised as $X \rightarrow X+Y$ and a theory of cognitive development schematised as $x \rightarrow x+y$ with a set of correspondence rules such that for any substantive term or formal principle, Z , occurring in X or Y , there is a z occurring in x or y such that Z and z are related by one of the correspondence rules. However there is no logical reason why, once the child has developed the cognitive apparatus designated by ' x ', he should immediately realise the linguistic potential corresponding to x , i.e. X . There is no logical reason why he shouldn't merely retain a latent ability, then, when he acquires the cognitive apparatus designated by ' $x+y$ ' realise his linguistic abilities corresponding to y , i.e., Y , before he does the same thing for X . Thus we could have cognitive development in which x preceded y corresponding to linguistic development in which Y preceded X . It therefore appears

to be difficult to draw up a very satisfactory set of necessary conditions on the reduction involved in treating language development as an aspect of general cognitive development.³⁴ Nevertheless I believe that enough has been said to justify the extensive analysis in Chapter 4. Examples of attempts to reduce aspects of language development in this way can be found in Cromer's (1968) work on the development of the linguistic encodings of temporal notions, in Brown's (1973) claim that the semantics of the earliest structured utterances can best be understood in relation to the output of the Piagetian period of sensori-motor intelligence and in Sinclair-de Zwart's view (1969, 1971, 1975) that formal syntactic operations have their origins in non-linguistic cognitive development. For future reference I shall say that if a theory of some part of language development is related to a theory of cognitive development in such a way as to satisfy the conditions outlined above and developed more fully in Chapter 4, then the theory of language development is grounded in the theory of cognitive development.

It is not only in the area of general cognitive development that one can attempt to ground one's theory of language development. So, for example, we can refer to E. Clark's

34. In Chapter 4 I distinguish between necessary conditions and 'desirability' conditions the latter being patently concerned with peace of mind of the consumer. The set of necessary conditions is, as suggested here, impoverished almost to the point of being uninteresting.

(1973, 1974) attempt to ground a theory of the development of word-meanings in a theory of perceptual development. The extent to which she is successful in this respect constitutes the first section of Chapter 2. Similarly there are a number of recent attempts to ground a theory of the development of speech-acts (and ultimately a theory of the development of syntax) in a theory of non-linguistic communicative behaviour (see in this respect Ryan (1974), Bruner (1975a, 1975b)). There will be some discussion of such studies in Chapter 6.

Nor is it necessary to refer to some other psychological function (where this is construed widely) when attempting to ground theories. Heider's (1971) work on the acquisition of colour vocabulary can be seen as relating a restricted area of lexical development to do with the denotation of colour terms to biological properties of the developing child and, in a different vein entirely, Halliday's (1975) main thesis attempts to relate semantic development to what are best interpreted as sociological variables. These ideas will be discussed in Chapters 2 and 6 respectively. In each case we shall ask how successful the theorist is in grounding the theory in the relevant domain with respect to the conditions outlined above.

A further distinct type of explanation for the direction of development (when the development is additive)³⁵ can

35. This qualification is important here. The following considerations do not apply to theories which fail to satisfy Condition 3 for reasons which are quite obvious.

be sought in generalisations concerning properties of the world's languages. Such generalisations might be absolute in which case we shall be dealing with linguistic universals or they may be statistical. The logic of investigations of this type is somewhat different to what we have seen so far and so we may begin by considering the general case where in D we have a theory of development which may be schematised as $X \rightarrow X+Y$. We assume that we are concerned with just those aspects of X and Y which are linguistic and, therefore, we are justified in believing that the theoretical terms which we find in these sections of X and Y are the theoretical terms of some linguistic theory. Among other things this linguistic theory may formulate, or may make available the formulation of, generalisations to do with the distribution of linguistic features in the world's languages. In particular, we can imagine that the linguistic theory makes available the generalisation that all the world's languages possess the relevant features in X but that this is not true for Y. In such a case the explanation of the direction of development can be reduced to a meta-principle that the child learns in D just those features which are universal before proceeding to learn language specific features. Whether this meta-principle constitutes an explanation or, indeed, whether it is reasonable will not concern me here.³⁶ Examples

36. In that it brings two disparate sets of facts together it would seem to constitute at least the first part of an explanation. Cf. the total structure of the argument in Jakobson (1968) in this respect.

of such theorising, although again I must emphasise that it was not theorising designed to answer the questions in which I am interested, can be found in McNeill's early views (1966) on the primacy of standard theory deep-structures and grammatical relations in a theory of the knowledge underlying the ability to produce and comprehend syntactically structured strings, in Lyons' (1975) suggestion that there is a universal core to the grammars of the world's languages, at least as far as the features of language he is interested in there are concerned, and that it is this core that the child learns first and in Jakobson's (1968) view that there is a small set of universal phonological contrasts which are exactly those learned first by children.

Obviously one can produce the same sort of theorising on the basis of statistical linguistic universals (for the three-way distinction of absolute, statistical and implicational universals, see Greenberg (1966)), but more interesting, perhaps, are attempts to utilise implicational universals into explanatory developmental statements. An implicational universal has the form: if a language has the feature F (where F is here neutral between category, rule, rule-type or any other sort of linguistic construct), then it will also have the feature F'. Such statements can be imported into a developmental context when we find the situation I am representing as $X \rightarrow X+Y$. For assume that Y is identical to F, i.e., development has proceeded to exactly that point where the feature F has been added to the system.

If we find F' in X we can invoke the implicational universal given above to explain for us why we find this order of development and not that which we can represent as $X' \rightarrow X'+F'$ where X' includes F. Examples of this sort of argumentation being used in the child language literature are, surprisingly, quite difficult to come by but there are at least two to which I shall be paying attention in what follows: Jakobson's attempt to relate the progress of the child's phonological development to the Laws of Universal Solidarity once the child has passed beyond the universal 'core' stage already alluded to above and Heider's investigation of Berlin and Kay's (1969) ordering of colour terms in a developmental context. Although in both cases the authors do not formulate their conclusions explicitly in terms of implicational universals, it is straightforward to do so and the logic of the child language investigation then becomes quite clear. Interestingly both Jakobson and Heider feel that they have to go beyond relating the developmental facts to the linguistic facts and so it is evident that authors have been somewhat dissatisfied with this sense of 'explanation'. If a developmental theory can be related to distributional facts in the above fashion I shall say that it is linguistically grounded.

In summary of the above, often inconclusive, discussion it seems that we can impose a fourth condition on the explanatory status of theories:

CONDITION 4

Given a theory $T = (T_1, T_2, \dots, T_n)$ in the domain of language development D , then T is an explanatory theory in D only if Conditions 1 - 3 above are satisfied (assuming Condition 3 is applicable) and, in addition, for all i , $1 \leq i \leq n-1$, the relationship of simplicity between T_i and T_{i+1} can be related to either

(i) logical relationships between constructs in T_i and T_{i+1} . In this case we say that T is logically grounded.

(ii) a theory of development in some other sphere of the child's activity, T' (typically a theory of cognitive development), or some domain not solely in the child's activity (e.g., the child's biological development or his social development). In this case we shall say that T is grounded in T' .

(iii) a set of generalisations concerning the world's languages. In this case we shall say that T is linguistically grounded.³⁷

Failure to satisfy Condition 4 leaves us with a theory which may well work and which is satisfactory in several, important respects but which, nevertheless, fails to provide us with any reasons as to why it works.

It is my claim that Conditions 1 - 4 form a set of

37. Another version of this condition can easily be formulated for theories to which Condition 3 is not applicable. Such a formulation would contain an equivalent of only the second clause.

minimal conditions on the adequacy of theoretical proposals in the various domains of language development. I do not claim that they constitute a set of sufficient conditions and, in fact, it is quite likely that no formulable set of sufficient conditions exists. It is a further claim that the vagueness which resides in some of the conditions should not be seen as a complete inhibitor of analysis as I believe that only by taking what are perhaps poorly formulated proposals to the analytic problems shall we be able to pinpoint their inadequacies and produce more satisfactory versions. The rest of this work is concerned with attempting to get clear the exact nature of the theories put forward by child language theorists and to set these theories against Conditions 1 - 4. Often this will involve a good deal of interpretation of theories and often I may be accused of not giving the theorist the benefit of the doubt in my interpretations. I contend that it is a valuable exercise to discover where the doubt resides. We shall meet some examples of at least partial success but generally the picture that emerges is that, although we now have a considerable amount of acquisition data, our theorising is still at a very rudimentary stage. This should occasion no great surprise nor should it be seen as criticism of the theorists whose work is discussed. The hope is that we can begin to take a more critical view of ourselves as theorists. It seems to be a truth of most domains of enquiry that the critics of the theorists are not usually the theorists themselves, and

to a large extent this is true of what follows although a small section is devoted to an analysis of some of my own theorising. Not surprisingly I find it difficult to be wholeheartedly critical in this section! For now I would merely like to acknowledge the theorists whose work is discussed below for making the discussion possible.

Before closing this opening chapter I would like to make one further remark to allay a possible confusion. When talking about theories in a developmental sequence, we can ask, as I have here, questions concerning the formal relations between the theories. In fn. 8 above I pointed out that the sequence of theories was only one aspect of a developmental theory. We can also ask questions concerning the processes and mechanisms by which the child passes from the stage characterised by one theory to that characterised by the next in the sequence and we can enquire as to the logical cogency of such postulated processes. So far I have raised no questions of this type which would include reference to such mechanisms as hypothesis formation, reinforcement, etc. and to the role of such variables as expansion, imitation, etc. depending on one's view as to what is involved in learning.

In Chapter 7 I tentatively develop what I can introduce informally here as Condition 5:

CONDITION 5

A developmental theory $T = (T_1, T_2, \dots, T_n)$ in the

linguistic domain D is adequate only if it includes a mechanism, M , which explains how the child progresses from T_i to T_{i+1} ($1 \leq i \leq n-1$).

In many ways this is the most interesting condition put forward so far and leads to a general formulation of developmental theories as ordered $n+1$ -tuples, $(T_1, T_2, \dots, T_n, M)$ and even preliminary analysis of this conception reveals many complications some of which are discussed briefly in Chapter 7. It is an assumption throughout this work that it is possible to ask and, at least tentatively answer, the purely formal questions without paying attention to the nature of the mechanisms which give the sequence its 'dynamic' aspect. I may be wrong in this respect but to give it full consideration would demand another opening chapter at least as long as this one.



CHAPTER 2 : MODELS OF LEXICAL DEVELOPMENT

In this chapter I shall consider, from the point of view of the framework developed in Chapter 1, a number of proposals which can be broadly construed as being concerned with the lexical development of the child. The proposals are not to be seen as rivals and, indeed, although some indeterminacy exists in fixing the domain of enquiry, in some cases, it seems likely that they are not all concerned with the same set of problems. Nevertheless, two broad alternatives emerge on how the meanings of the child's early words are to be conceptualised. One view has it that word-meanings can be broken down into semantic features (we meet this view in 2.1, 2.2, and, less explicitly, in 2.3 below) and the other that word-meanings make vital reference to a prototype or 'focal instance' from the extension of the word (this view appears in 2.4, less centrally in 2.5 and, perhaps, although this is difficult to be clear about, in 2.3) . Depending on the emphasis adopted by different authors, D may be fixed as the child's lexicon viewed simply as a set of word-forms, as the meanings attached to words in the lexicon, as the range of reference of particular words in the lexicon or as a combination of these. It is intended that the discussion will have the virtues of illustrating in fairly concrete terms the rather abstract framework of Chapter 1 as well as making a substantive contribution to the evaluation of hypotheses in the above domain.

In 2.1 I shall consider Eve Clark's semantic feature model as it has been applied by her and others to the child's acquisition of forms having concrete reference in the earliest stages of language development. 2.2 considers the extension of Clark's theorising to the domain of relational terms for children who fall principally in the 3 - 5 age group. Katherine Nelson's functional core concept model as developed in Nelson (1973a, 1973b and, particularly, 1974) forms the subject matter of 2.3. The relationship of her proposals to theories of cognitive development is examined in some detail. In contrast to 2.1 - 2.3 which are concerned with general, if not global, theories of lexical development, 2.4 focusses on a restricted problem concerning the range of reference of colour terms. Heider's (1971) work on the experimental investigation of the early use and comprehension of colour terms and its relationship to the implicational universals postulated by Berlin and Kay (1969) are considered. The extent to which some of the suggestions made here can be extended to handle other areas of lexical development is discussed in connection with the proposals of Griffiths (1976). Finally 2.5 is concerned with the recent work of Rosch and her associates (Rosch, Mervis, Gray, Johnson and Boyes-Braem (1976)) manipulating the notion of 'basic object' and the sense in which this can be seen as attempting to explain the presence of particular words in the early language of the child as a reflex of a general principle to do with the maximisation of category information obtainable from incomplete cues - what we

might want to see as a fundamental cognitive principle.

2.1 Clark's semantic feature model and early concrete vocabulary.

Eve Clark's work represents one of the most systematic investigations of early lexical reference and it is this aspect of her theorising that I wish to concentrate on first. The main ideas were put forward in two papers, E.Clark (1973a, 1974), and have been criticised from different standpoints by Nelson (1974)³⁸ and Griffiths (1976). I shall begin with a short account of the main features of the theory and then go on to examine its explanatory status in terms of Conditions 1 - 4.³⁹ Where criticisms such as those developed by Nelson and Griffiths are relevant they will be raised but a point to be clear about from the outset is that I am not concerned to establish a rival theory as were those authors but merely to evaluate Clark's proposals against what I

38. For further discussion of Nelson's criticism of Clark, see 2.3. In addition it is worth pointing out that the view of word-meanings subscribed to by Clark has come under concerted attack recently from philosophers such as Putnam (1970, 1975) and Kripke (1972) whose ideas are themselves much more akin to those of psychologists such as Rosch and Rips and his associates (Rosch (1973a, 1975a and many other references), Rips, Shoben and Smith (1973), and Rips (1975)).

39. Condition 5 will be explored, and then only tentatively, in Chapter 7.

hope are uncontroversial criteria.⁴⁰

The pedigree of Clark's model is somewhat difficult to determine. In its notational devices it clearly owes a lot to the componential semanticists of anthropological linguistics and to the linguistic semantics of Katz and his associates and, from a slightly different perspective, Bierwisch. However, it lacks the methodological constraints of the former, i.e., whereas the anthropological linguist can ask his native informant about the semantic relations between words and about the denotation classes of words, the child language theorist, in general, cannot, and it lacks the philosophical motivation and sophistication of the latter, i.e., there is no concern on Clark's part for an explication of such notions as synonymy, ambiguity, paraphrase, analyticity, etc.⁴¹ We can safely assert that

40. To put this another way, we can say that the criticisms raised by Nelson and by Griffiths are largely, though not exclusively, concerned with the fact that Clark's theory cannot handle certain attested phenomena from lexical development, i.e. it makes incorrect predictions. Clearly this can be related to Condition 1 but a more profound way to fail to satisfy Condition 1 is met when we can demonstrate that the theory cannot, in principle, make predictions in the relevant domain. Insofar as I shall be concerned with Condition 1 in what follows it will be this sense of failure which is attended to.

41. This should not be seen as implying any positive evaluation of Katz's or Bierwisch's approach to semantics. It seems to me that the arguments of Putnam referred to in fn. 38 are conclusive in this respect but the point still stands that Katz is motivated by more than a simple desire to break down the meanings of words into components.

the theory borrows little of substance from these approaches beyond the notational devices and, furthermore, we might note that there is no clear statement as to whether the theory only countenances binary features or admits n-valued features.⁴²

The first question to get clear is the identity of the domain D of linguistic activity or linguistic knowledge in which the theory purports to provide explanatory statements. The most central phenomenon tackled by the theory is that referred to as overextension where a small child uses a word which is recognised as a token of a word-form which exists in the adult language and does so in a non-standard fashion, i.e., uses it to refer to some object or event for which an adult would not judge it appropriate (cf. fn. 43 below for a possible complication). Figure 2 presents some examples collated by Clark in her 1973a paper.

Assuming then that the phenomenon of over-extension is well-attested and a good deal of evidence is now available from modern studies to show that this is so (see, in particular, Grieve and Hoogenraad (1977) and, for a

42. Cf. E.Clark (1973a, 74) : "No theoretical issues will be raised here, although I will use a binary type of notation to represent the child's semantic knowledge about particular sets of words. This notation does not imply any theoretical commitment to binary features and will simply be used for clarity's sake in presenting the data." From some of the examples subsequently discussed it is clear that the intention expressed in this passage is not adhered to by the author cf. p 66 below.

Figure 2. Some examples of overextension in child speech (adapted from E. Clark, 1973a, 79 - 82). 43

<u>Language being learned</u>	<u>Child's Form</u>	<u>First referent</u>	<u>Subsequent overextensions</u>
English	bird	sparrows	cows, dogs, cats, any animal moving
Georgian	buti	ball	toy, radish, stone spheres at park entrance
Serbian	bebe	reflection of self in mirror	photograph of self, all photographs, all books with pictures, all books
English	kotibaiz	bars of cot	large toy abacus, toast rack with parallel bars, picture of building with columns
French	bebe	baby	other babies, all small statues, figures in small pictures and prints
Russian	dany	sound of bell	clock, telephone, door bells
French	cola	chocolate	sugar, tarts, grapes, figs, peaches
Russian	va	white plush dog	muffler, cat, father's fur coat

43. One thing which is immediately obvious from Figure 2 is that there is an intolerable amount of vagueness in the right-hand column; vagueness which is going to be crucial in any study interested in determining the principles which fix the extensions of terms in the child's vocabulary. What this amounts to is one aspect of a general reservation concerning the sources from which Clark drew up her generalisations particularly with respect to their variant methodologies and differing standards of precision. A second point which emerges and which, in fact, is presupposed in the use of the term 'overextension' is the assumption that the word which is overextended has been used appropriately by the child. That this is not an obvious consequence of Clark's theory will emerge presently.

cautionary note on the generality of the phenomenon, Griffiths (1976, 167ff), it is clear that the aspect of linguistic activity in which Clark is interested is small children's use of concrete nominals in a referential fashion and coupled to this activity is the domain, D, the child's lexicon which plays a crucial role in the activity.⁴⁴ At this point it is worth noting the distinction drawn by Lyons (1975, 1977a) between reference and quasi-reference, where the former is tied to a concomitant notion of predication whereas the latter can stand alone and is, to Lyons' way of thinking, indeterminate between reference and predication. Since a great deal of the data which can be used in evaluating Clark's proposals comes from a stage where it is difficult to distinguish reference from quasi-reference in the speech of the child (Lyons would say that there are a large number of instances of quasi-reference), my usage of 'referential' above should be seen as neutral with respect to Lyons' distinction. This distinction and its implications will be discussed in Chapter 6. Clark's theory then is concerned with the development of the child's lexicon insofar as this is revealed by his concrete naming behaviour and,

44. Note that it is possible to make a distinction here between the lexicon containing information governing the application of a nominal and it containing the meaning of the nominal. That these two notions should not be conflated lightly has been argued, in a different context, by Kripke (1972). Clark appears to conflate the two and I shall assume that, as far as her intentions are concerned, nothing untoward follows from this.

in particular, with the development of the meaning representations in that lexicon. It might also be seen as making claims about the order in which we can expect particular forms to enter the child's vocabulary but this is of somewhat marginal interest in the present context. I turn now then to what I regard as the central theses of the theory. It can be seen as embodying at least six such theses:

(i) the meaning of a word is to be understood as specifiable in terms of a set of features.⁴⁵

(ii) the set of features which comprise the meaning of a word in the adult language may not be identical with the set of features comprising the meaning of the word for the child at the stage under investigation.⁴⁶

(iii) the mismatch referred to in (ii) is to be explained in terms of the child sampling from a certain set of features when he first encounters an application of

45. Clark says (1973a, 74) : "One of the basic assumptions of the theory clearly is that the meanings of words can be broken down into some combination of units of meaning smaller than that represented by the word."

46. Although the child's semantic features are seen as drawn from the set also used by the adult whether this is also true for each item acquired by the child is not made clear by Clark. The possibility is raised but nowhere explored that the child may abandon features as he develops. The clearest position Clark adopts demands that this is viewed as unlikely if not impossible. See Bloom (1973) for examples of 'underextension' which clearly violate the subset requirement.

the word.⁴⁷

(iv) the subset sampled by the child is a subset of a set of perceptual (as opposed to conceptual or functional) features which is somehow contingent on the perceptual properties of objects the word is typically used to refer to, or, more particularly, to properties of the object the word is being used to refer to when the child first assimilates it into his lexicon.

(v) development in this domain consists of the child' simultaneously learning new words and features to distinguish them from words already learned resulting in richer feature specifications for the words which have already been learned with a gradual approximation to the adult meaning.⁴⁸

(vi) features of meaning are acquired in an order from most general to least general.

47. This notion of 'sampling' can, of course, be seen as a first programmatic statement on the identity of a mechanism, M, required by Condition 5.

48. 'Simultaneously' here hides the problem of the directionality of the relationship between the words and the features which are held to make up their meanings which is not discussed in Clark's work although it is obviously crucial to a fully explicit theory. Is it the case that the child hypothesises that certain perceptual features are important in determining the applicability of words and looks around for words to hang on to the distinctions made available by the features or is it that the child 'realises' that a plurality of words is being used in circumstances where he would only use one and looks around for a basis for this distinction?

There are many questions which one might wish to raise in connection with these theses some of which are discussed below but, for now, all I wish to do is cast the theory into terms which render the framework of Chapter 1 applicable. I submit that this procedure does not involve a significant perversion of Clark's thinking.

What Clark appears to be proposing is an unstructured associative model of word-meaning in which, in some sort of mental dictionary, words are paired with sets of perceptual features which give the meanings of the words and which provide necessary and sufficient conditions for the application of a word to an instance (cf. fn. 44). This is a proposal which we might represent diagrammatically as in Figure 3.

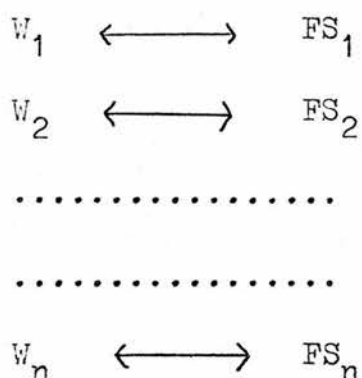


Figure 3

Here the W_i ($1 \leq i \leq n$) represent the child's word-forms at the time in question ⁴⁹ and the FS_i ($1 \leq i \leq n$) designate sets of perceptual features which constitute the meanings

49. Strictly speaking this should be restricted to the child's 'concrete' lexicon at the stage under analysis. This qualification should be taken as read in what follows.

of the W_i and necessary and sufficient conditions for the applicability of the W_i to instances. Obviously the feature sets will have members in common and it is Clark's hope that a relatively small list exists out of which the word-meanings are formed and which can be related to, and, perhaps, identified with, a set of universal semantic primitives such as is suggested by Postal (1966) or Bierwisch (1970).

We now consider a later stage in the child's lexical development and the most obvious characteristic of this stage, when compared to the earlier one, will be an increase in the number of words which have a referential function. Clark's scheme demands that, at this stage, there is also an increase in the number of features employed by the child enabling him to make finer semantic discriminations resulting in him having more meanings to relate words to. A coarse-grained analysis of the situation could be represented diagrammatically as in Figure 4.

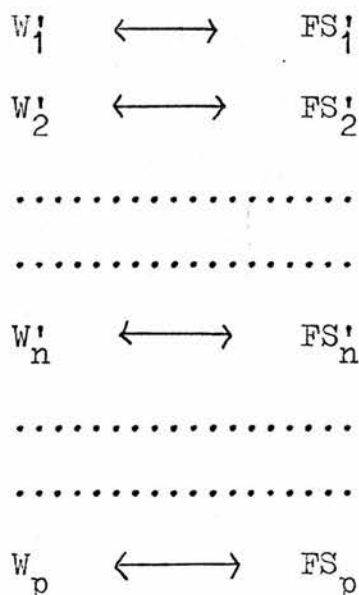


Figure 4

where $n < p$, W_i' 'corresponds' to W_i from Figure 3 ($1 \leq i \leq n$) and the W_j ($n < j \leq p$) are the 'new' forms. The FS_i ($1 \leq i \leq p$) are the associated meanings as before. But it is possible to make a much more detailed analysis on the basis of a hypothetical example Clark uses to investigate the notion of 'restructuring'. Her version of hypothetical development in that restricted part of the lexicon dealing with common animals is reproduced here (p 64) as Figure 5.

Taking account of the fact that the figure doesn't represent the actual process of lexical development for any one child but is a, supposedly plausible, route based on observations Clark finds in the literature, we are justified in assuming that it will be favourable to Clark's position and that, therefore, negative points arising from discussion of it should be given their full weight. At Stage I the child is presumed to have learned the word bow-wow and to use it appropriately (cf. the remark above on the necessity for this stage).⁵⁰ At Stage II the child overextends the use of bow-wow to include all common 4-legged animals and it is suggested that a plausible candidate for the basis of the over-extension is a feature to do with shape. The child is assumed to have sampled this feature, on acquaintance with an instance of a dog, as the most 'salient' one.⁵¹

50. Exactly how we are supposed to represent the meaning of bow-wow at this stage is not something Clark expands upon. From Figure 5 it might appear that something like +dog-shaped would be in order but this is clearly absurd.

51. see p 65

Figure 5. Hypothetical instance of overextension and restructuring (reproduced from E.Clark, 1973a, 85).

	<u>Word</u>	<u>Semantic Domain</u>	<u>Possible criterial feature(s)</u>
Stage I	bow-wow	dog(s)	shape
Stage II	bow-wow	dogs, cows, horses, sheep cats	shape
Stage III	(a) bow-wow	dogs, cats, horses, sheep	
	(b) moo	cows	sound (horns?)
Stage IV	(a) bow-wow	dogs, cats, sheep	
	(b) moo	cows	sound
	(c) gee-gee	horses	size, (tail/mane?)
Stage V	(a) bow-wow/ doggie	cats, dogs	size
	(b) moo	cows	
	(c) gee-gee/ horsie	horses	
	(d) baa	sheep	sound
Stage VI	(a) doggie	dogs	
	(b) moo	cows	
	(c) gee-gee/ horsie	horses	
	(d) baa lamb	sheep	
	(e) kitty	cats	shape, sound

Elsewhere Clark talks about a feature, \pm 4-legged, while admitting that this is not a plausible candidate for a primitive perceptual feature, so we can assume that at Stage II the relevant part of the child's lexicon is as in Figure 6.

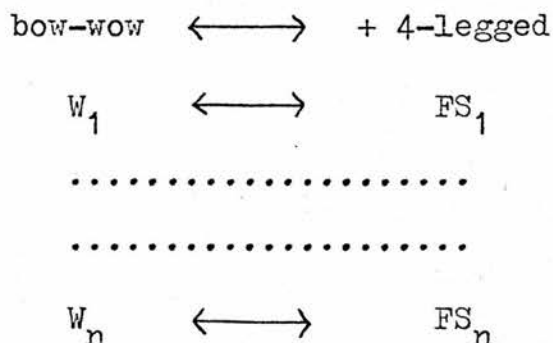


Figure 6

An important point which will receive more extended discussion in 2.2 is that, in order for the feature, $+ 4$ -legged, to have a coherent status, it is necessary that the negatively specified version, $- 4$ -legged, appears somewhere in the FS_i ($1 \leq i \leq n$).

At Stage III there is a restructuring of the system resulting in a reduction of the overextension associated with bow-wow and the suggestion is that this is mediated by the child's use of a perceptual feature encoding the sound that cows make. For the sake of argument let us

51. How might the child never use an overextended form appropriately? He may meet an instance of category C being referred to using the form a, sample the perceptual feature, F , of the instance as the most salient and assume that this feature governs the applicability of the form a. However subsequent instances of F may not be instantiated in any member of C . Then we would have consistent inappropriate use of a without a single appropriate instance.

refer to this feature as +moo and assume that no other lexical changes have occurred between Stage II and Stage III. Then the relevant part of the lexicon will have the structure shown in Figure 7.

<u>bow-wow</u>	↔	+4-legged, -moo
<u>moo</u>	↔	+4-legged, +moo
W_1	↔	FS_1
.....		
.....		
W_n	↔	FS_n

Figure 7

At Stage IV there is further restructuring resulting in a further reduction in the extent of the overextension of bow-wow and this time the suggested criterial feature is derived from the perceptual dimension of size. Unless we are to invoke such unlikely features as +horse-sized we shall have to admit a non-binary feature at this point, say, 1Size, 2Size, ..., nSize, such that the relevant part of the child's lexicon now has the structure shown in Figure 8.

<u>bow-wow</u>	↔	+4-legged, -moo, iSize
<u>moo</u>	↔	+4-legged, +moo, jSize
<u>gee-gee</u>	↔	+4-legged, -moo, jSize
		etc.

Figure 8.

Obviously this sort of treatment can be extended to the further stages hypothesised by Clark but there is little point in pursuing this here as the general lines of the argument

are clear enough. In general as the child develops his lexicon in a semantic domain he also extends his inventory of semantic features without (at this stage at any rate) going beyond perceptual features. If we have a set of forms, W_1, W_2, \dots, W_n , covering, in their applications, a particular conceptual field (see Lyons (1977b) for discussion of this notion originating with Trier), then the learning of an additional item whose application falls within the same conceptual field involves the utilisation of (at least) one additional perceptual feature which, as well as providing part of the meaning of the new form, will also have a role to play in restricting the extension of a form already existing in the system. If, following the procedure of Chapter 1, we now consider theories being constructed at times, t_1 and t_2 we can see that we shall have a situation which can be represented as in Figure 9.

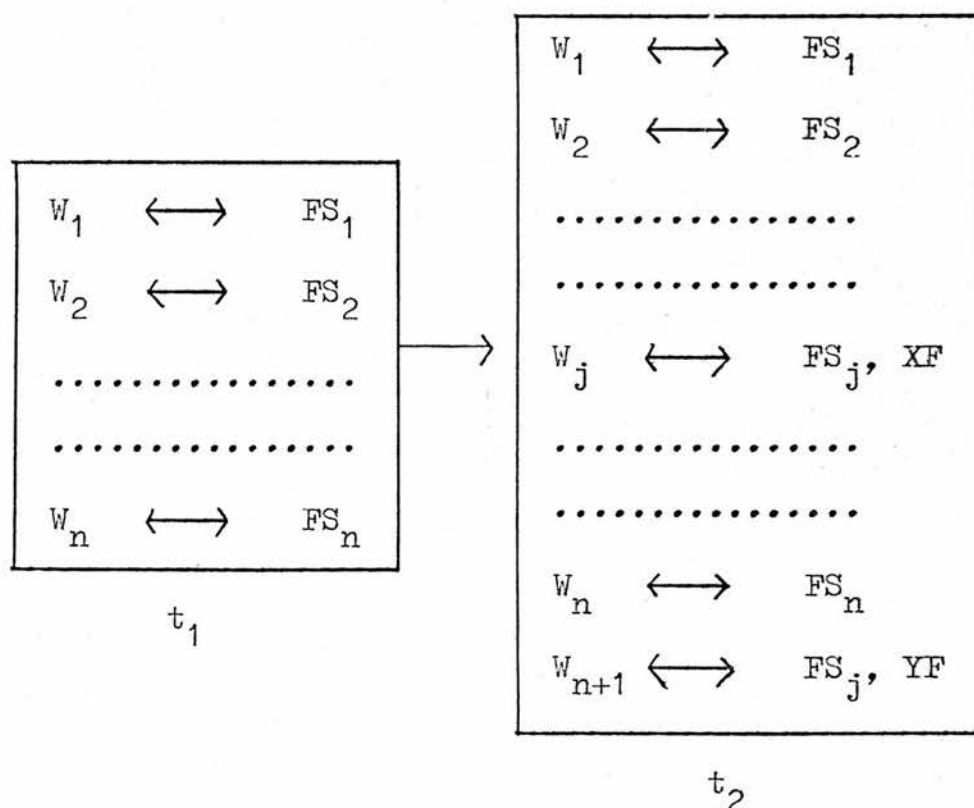


Figure 9

where the W_i ($1 \leq i \leq n+1$) designate child word-forms, the FS_i ($1 \leq i \leq n$) designate feature sets, F is the new feature involved in the meaning of W_{n+1} and restricting the extension of W_j and which is a perceptual feature and X and Y are variables ranging over the set of appropriate prefixes for the feature, F . That is, if F is a binary feature, then X and Y will range over the set $\{+, -\}$ and, if F is an n -valued feature, X and Y will range over the set $\{1, 2, 3, \dots, n\}$. In addition we can note that $X \neq Y$. This general formulation seems to cover the case discussed by Clark and to make clear the sense in which lexical learning proceeds by way of the accumulation of new perceptual features.

We are now in a position to consider Clark's theory in terms of Conditions 1 - 4. One immediate point to make is that, as presented so far, the theory cannot plausibly be regarded as providing a model of naming behaviour as it makes no input-output predictions, i.e., it says nothing about when a particular form will be used appropriately or otherwise nor anything about the conditions under which it would be so used. The theory does not admit of interpretation in terms of an infinite set of counterfactuals. This is a common feature of the theories we shall meet in language acquisition and a common tactic is to resort to the view that the model is not intended to be one of behaviour, naming or any other sort, but rather is a model of the knowledge which underlies such behaviour and makes it possible, knowledge which also

constitutes an essential component of a model of the behaviour in question. In this case we might suggest that in addition to the lexicon a theory of naming behaviour would have to include aspects of the child's attentional mechanisms, motivation, communicative intent and so on. In this work I don't wish to question the legitimacy of this 'retreat' to competence models. It has, of course, been searchingly examined by philosophers and psychologists in connection principally with Chomsky's claim for the psychological significance of grammatical theory (see, e.g., Schwartz (1969), Atherton and Schwartz (1974), Stich (1971, 1972), Derwing (1973), Botha (1970, 1973), Bever, Fodor and Garrett (1974) for extensive discussion, and Chomsky (1976) and papers by Chomsky and associates cited there for replies to many of the above). It follows that, if we are to see Clark as explaining the development of naming behaviour, we should have to consider a theory which we can designate as T + X where T is a theory of the lexicon and X denotes whatever additional machinery is necessary. It is left open, of course, whether the study of X falls into the domain of 'mysteries' (Chomsky (1976)) for which, in principle, no solutions will be forthcoming because of inherent limitations on the human organism qua empirical scientist. From now on I ignore X and, so as not to get bogged down in further discussion of this point, I shall assume that Clark's theory can be modified in such a way as to satisfy Condition 1 or that an attenuated version of

Condition 1 can be formulated - this would amount to taking a different line as to what constitutes a psychological explanation - and that the theory as it stands would satisfy such a version.

It seems to me that Clark's theory satisfies Condition 2 in a straightforward fashion as, once the assumptions are made explicit, as I have tried to make them above, the theory at each stage of development seems to be constructed with reference to general considerations. These considerations include claims like that the meaning of a word, for a child, consists of a set of features, that this set consists entirely of perceptual features and so on. In short the considerations amount to a subset of the theses (i) - (vi) from pp59 - 60 above. In accordance with these constraints the theories constructed at t_1, t_2, \dots, t_n will be theories of the same type and, in the sense of Chapter 1, they will be comparable.⁵²

Clark's theory also seems to have a simple interpretation in terms of Condition 3 and therefore I assert that that condition is applicable. From t_1 to t_2 , in the example above, there is a complication of the theory describing the child's lexicon; a complication of an additive nature.

52. Of course a great deal needs to be done in delimiting the set of perceptual features and specifying a procedure for recognising features as perceptual or not perhaps by reference to a perceptual theory. But, by itself, the modifier 'perceptual' surely carries some intuitive constraining power.

And, in general, given a sequence of theories, (T_1 , T_2 , ..., T_n), it will be the case that T_{i+1} is more complicated (additively) than T_i ($1 \leq i \leq n-1$) if the course of development suggested by Clark is being followed. Obviously new lexical forms enter the child's repertoire⁵³ leading to one sort of additive complexity and, in addition, new features enter the system in what appears to be an additive fashion. It is not the case that combinatorial relationships between features change because, although one can easily imagine a situation where word-meanings, after admitting only conjunctive feature definitions, suddenly admit disjunctive or implicational definitions, it appears that conjunction is the only such relationship contemplated at all stages of development. Obviously if we were to find changes in combinatorial relationships of the type alluded to, we could conclude that these relationships satisfied Condition 3 additively. So what we have is the situation which was depicted in Chapter 1 as $X \rightarrow X+Y$ where X and Y can be interpreted as sets of lexical forms, sets of features or a combination of the two.⁵⁴ Therefore we can now ask what is perhaps

53. We should note here the phenomenon of 'lexical mortality' which is found in the early stages of language development particularly with reference to the child's idiosyncratic 'words'.

54. On several occasions in what follows I have found it convenient to isolate instances of a particular theoretical type and consider additive complexity with respect to this type rather than with respect to the theory as a whole (e.g. in the example in the text there are lexical forms and perceptual features). This leads to a great deal of simplification in presentation and doesn't, in the examples considered, distort the discussion.

the most interesting question concerning Conditions 1 - 4 with respect to Clark's theory; given T_i and T_{i+1} in the sequence of theories, (T_1, T_2, \dots, T_n) , can the simplicity relationship which obtains between them be grounded in any of the ways specified in Condition 4?

Consider first the possibility that the relationship may be logically grounded. There doesn't seem to be any clear sense in which this is the case. Concentrating on the sets of features which obviously carry the main explanatory burden rather than the lexical forms themselves, there don't appear to be any relationships between particular features of the sort we would need. So, if we examine the example analysed above on pp64 - 66, we can see that it demands a sequence of features, $(\pm 4\text{-legged}, \pm \text{moo}, \text{iSize}, \dots)$ and there is no logical reason why we should find this sequence rather than, say, $(\pm \text{moo}, \text{iSize}, \pm 4\text{-legged}, \dots)$ or any of the other numerous possibilities. In principle, it is easy to imagine sequences of features which do exhibit the sort of dependency in which we are interested. For example, we could consider a binary feature, $\pm \text{chromatic}$, and a multi-valued (perhaps infinitely valued) feature, $n\text{Wavelength}$, which 'presupposes' the positive value, $+\text{chromatic}$. Within such a miniature system there are obvious reasons, built into the theoretical vocabulary, for why we should find the developmental sequence, $(\pm \text{chromatic}, n\text{Wavelength})$ rather than the sequence, $(n\text{Wavelength}, \pm \text{chromatic})$. Again we could consider the existence of an n -valued feature concerned with Dimensionality and another n -valued feature to do

with shape, where we would want to say that the former is 'presupposed' by the latter and this would provide us with our explanation for why we find the sequence, (nDimensionality, nShape) rather than (nShape, nDimensionality). These examples are, of course, fictitious and perhaps rather implausible (it is an interesting fact that Clark found no overextensions in the materials she studied which were based on the perceptual dimension of wavelength). However the important point is that such relationships could exist in a theory articulated in an appropriate fashion; but in the analyses proposed by Clark based on attested or hypothetical data they are not obvious.⁵⁵ We shall see later in this chapter (2.2) that, with regard to later stages of development, and the acquisition of relational terms, Clark is on somewhat stronger ground in this respect. We must conclude that the possibility of providing a logical grounding for early lexical development when the latter is construed in terms of the acquisition of perceptual features is remote.

Consider now briefly the third possibility under Condition 4 which involves reference to facts about the world's

55. Clark, as has already been indicated, has recourse to the view that general features are acquired first but unfortunately she gives no indication as to what is to count as a general feature. Griffiths (1976) suggests that she means to equate 'general' with 'occurs in the analysis of many (vague) words' and, although this may be a reasonable explication of the required notion of generality, it will hardly serve the required function in this context given the early appearance of such features as ±moo.

languages. Here the theory is on even weaker ground because of the lack of any systematic attempt to analyse the vocabulary of human languages in terms of perceptual features. Apart from such remarks as can be found in, e.g., Bierwisch (1967) semantic theorists have tended to eschew reference to perceptual properties of the human organism.⁵⁶ In particular cases one can even go so far as to say that it is quite clear that lexical items which are used to refer to concrete objects in the adult language cannot be analysed semantically in terms of a conjunctive set of perceptual features. In the case of, say, cow, given the sort of development envisaged by Clark, it will presumably have an entry something like:

cow \longleftrightarrow + 4-legged, + moo, jSize, etc.

but as Putnam (1970, 1975) has pointed out, we can't consider such a conjunctive set of features as determining the extension of cow let alone providing the meaning of the word because we immediately know what to call an animal which is exactly like a cow except that it has only three legs and also an animal which is exactly like a cow except that it makes a peculiar noise which can't be described as mooing and so on. In all such cases we have a cow which is peculiar in some way but not to the

56. There is a concern for psychological reality in the literature on componential analysis (see, e.g., Romney and d'Andrade (1964)) but little attention to components of meaning which can be seen as perceptual. An isolated exception, although not within the same tradition, is Adams and Conklin (1973).

extent of not being a cow. I therefore conclude that, as linguists have not, on the whole, utilised perceptual features in their analyses of word-meanings in adult languages and as there seem to be insuperable problems attached to the idea of a set of features providing a conjunctive definition of the meaning of a word, there is little hope of Clark's theory finding any explanatory backing via this third possibility. One additional remark which can be made here points to the seriousness of this situation in a different fashion. Assume that Clark's theory is correct for the stages of development in which she is interested and assume further that the above remarks concerning semantic feature theory are also correct and that some different notion of semantic structure is going to be necessary in order to explain the adult's abilities. Then we have the predicament described in Chapter 1 where, viewing development as a whole, there appears to be a radical discontinuity somewhere between the age of 18 months and adulthood. Note that this is the case even if we subscribe to the view that a semantic feature theory is adequate for the adult language and merely insist that reference to only perceptual features will not be sufficient in it. At some stage the child will have to move from a system in which he relies wholly on perceptual features to one in which he uses features of some other kind and this represents a discontinuity. As pointed out in Chapter 1 theories which involve this sort of discontinuity are not necessarily incorrect but the discontinuity is something which needs to be argued

for. This leaves us then with the second possibility of Condition 4 to which I now turn.

It would appear that it is this second possibility that will provide the most obvious explanatory basis for Clark's theory. After all the features comprising the meanings of words are perceptual features and there should be no great problem in relating them directly to aspects of theories of perceptual development in the visual, auditory and other sensory domains. That Clark sees this as a possibility is put forward in her 1973 paper and argued more fully in Clark (1974).⁵⁷ In the later paper we find several references to research work in sensory perception covering a number of modalities. For example, in connection with shape, Clark cites the findings of Gibson (1969) on the infant's attention to lines and vertices and tentatively relates this to Hubel and Wiesel's well-known work (see, e.g., 1962) on the visual fields of cells in the visual cortex of the cat. Evidence of a different sort is provided by Ricciuti's work (1963) showing that children in the 12 - 24 month age group used shape as their main criterion in a free sorting task. As far as sound is concerned, we are directed to Kaplan (1969) and the conclusion that infants of four months can distinguish male and female voices and to Eimas, Siqueland, Jusczuk and Vigorito (1971)

57. For a rather more cautious statement of the whole theory, albeit in an introductory context, see Clark and Clark (1977).

on infant's perception of speech sounds. Further evidence is presented concerning the infant's perception of movement, of size and of texture and it isn't my purpose here to review this evidence in detail. Rather what I want to do is take the evidence at face-value and enquire as to exactly what Clark demonstrates by citing it. Taking the example of sound, the evidence presented clearly demonstrates the child as capable of some fairly sophisticated discriminations in this domain, discriminations which might, in principle, be conceptualised in terms of the child having available a set of auditory perceptual features. But there are three points I would like to make in this connection:

(i) The evidence cited by Clark all concerns the child's perception of linguistic materials and, as she herself points out (p.35):

The overextensions based on sound are mainly based on characteristic non-speech sounds, e.g., the sound of a train. However there has been much less research on the child's ability to identify or recognise these kinds of sound than there has been on speech sounds.

In short the evidence to which Clark draws our attention provides no support for the child controlling the perceptual features in his perception with which she wishes to credit him in explaining his overextensions. Contrary to what we supposed there is no perceptual theory such that the terms of the theory of lexical development can be related to the terms of that theory in a systematic way.

(ii) assume, contrary to what is suggested in (i), that the evidence does argue for the existence of the required perceptual features in perception. Then we have to attend to the relative ordering of the appearance of the features in the perceptual theory and the lexical theory. Some of the evidence cited by Clark concerns children who are beyond the typical age-range for overextension and therefore cannot be used as a basis in a theory explaining the overextensions. Even when the relevant features appear in perception before they are needed in the lexicon, this hardly shows that such features have any role to play in the child's lexical development. Clark's position would be undermined by showing that the child does not control the distinctions which are necessary in the appropriate perceptual dimensions but the best that the situation we are concerned with can do is indicate that the theory is consistent with facts about perceptual development.

(iii) although there may be a chance of satisfying the necessary conditions on the reduction required in Condition 4 and developed in Chapter 4, there is little room for optimism as far as satisfaction of the 'desirability' conditions is concerned. First much of the perceptual learning which Clark refers to takes place in the first few months of life and so the relationship between it and the related lexical learning would be somewhat indirect. Second, and more to the point, it is not the case that a sequence of acquisition of perceptual features has been

established.⁵⁸ To see this in its proper perspective we can imagine a set of findings which would be supportive of Clark's theory. Suppose that in research on the development of form perception we come to the conclusion that such perception can be seen as mediated by a set of features whose availability to the child is fixed in some ontogenetic sequence, F_1, F_2, \dots, F_n . Suppose, further, that lexical development can be seen, as Clark would have it, in terms of the successive acquisition of features which can be represented as a sequence, f_1, f_2, \dots, f_n .⁵⁹ Finally assume that the F_i can be systematically related to the f_i ($1 \leq i \leq n$) and that we find f_i being utilised in the lexical system after F_i is utilised in the perceptual system and before F_{i+1} appears in that system. In such a situation, Clark's hypothesis would have extremely strong support (cf. Chapter 4 for extended analysis of this and similar states of affairs). Compare this then to what we in fact have. We have some, fairly fragmentary, evidence that infants utilise perceptual features in their perception. None of this evidence points to a developmental sequence. Ex hypothesi we have the sequence, f_1, f_2, \dots, f_n , this being what we are seeking

58. That Clark would dearly love to be able to refer to such a sequence is shown by her statement that (1973a,101): "Since learning to attach meanings to words involves the interpretation and encoding of perceptual data, we might expect to find an analogous sequence of development in perception..."

59. I don't wish to seriously suggest this sort of total ordering on sets of features and I resort to it simply for expository purposes.

to explain. We do not have any systematic relationship between the perceptual features and the features used in the lexical system (what we have is a few suggestive remarks) nor do we have any evidence of the required kind to do with the relative orderings of the F_i and the f_i . Given the lack of any definable ordering on the F_i this is hardly surprising. Insofar as we do have any evidence at all on this issue it would indicate that a large proportion of the relevant F_i are used by the child in his perception long before he learns any lexical items at all. This is so transparently a sorry state of affairs that I feel justified in concluding that further discussion at this stage is pointless. Although Clark's theory satisfies Conditions 1 - 3, it falls down badly on Condition 4 and therefore fails to meet our set of minimal criteria on explanatory adequacy in theories of language development. The logic of the above analysis is something we shall be repeating on a number of occasions in the pages which follow.⁶⁰

60. Press (1974) is an attempt to investigate children's reliance on perceptual features in a similarity judgement study and to establish an hierarchy of such features. While producing a number of interesting findings the experiment failed in its main purpose with reliance on perceptual dimensions being largely determined by the nature of the judgement required. No hierarchy of features emerged. A further relevant study which has only just come to my attention is Thomson and Chapman (1975). They showed that in some cases of overextension children were capable of making judgements of goodness of category membership and that these judgements went in the direction of exemplars that would not constitute misapplications of the terms according to adult usage. Such a finding (cont. p 81)

2.2 Semantic feature theory and the acquisition of relational terms

One of the attractions of semantic feature theory which has not emerged from the above discussion is that, using essentially the same conceptual machinery, i.e., conjunctive sets of semantic features, Clark feels that she can account for a number of phenomena which have been studied with some intensity during the last ten years or so. At the outset we should note that features employed in the analyses which follow are not perceptual features and that the interplay between perceptual and non-perceptual features in an overall theory is nowhere discussed. This casts doubt on the coherence of any overall theory.

The work I wish to consider concerns the development of comprehension, in experimental situations, of such relational adjective pairs as more and less and same and different, of antonymic dimensional adjective pairs such as big and small, fat and thin and wide and narrow, of the temporal conjunctions, before and after and of the deictic verbs of motion, come and go and their 'causative'

60.(cont) is more consistent with a prototype view of the child's lexicon (cf. below) but in fairness I should point out that Thomson and Chapman's results were not completely general and that there were instances of over-extension which seemed to invoke 'unstructured' domains as is required by a semantic feature theory of the sort Clark embraces.

counterparts, bring and take.⁶¹ In 2.2.1 I shall briefly present the main research findings in these areas. 2.2.2 will examine explanations for the facts concerning dimensional adjectives, 2.2.3 will focus on more and less paying brief attention to same and different, 2.2.4 is concerned with the temporal conjunctions and 2.2.5 with come and go.

2.2.1 Empirical work on relational terms⁶²

Donaldson and Balfour (1968) showed, in a seminal study, that Scottish nursery school children with ages falling in the range 3 years 5 months to 4 years 1 month appear to go through a stage where they understand less as having the meaning of more while understanding more correctly. Their technique involved confronting children with two cardboard apple trees on which could be hung metal apples. For one set of questions conditions were arranged so that the trees held different numbers of apples and the children were asked either 'Does one tree have more?' or 'Does one tree have less?' The children were almost 100% correct in producing affirmative answers to these questions but, when subsequently asked either 'Which tree has more?'

61. This is only a sample of the work which could be discussed in this section although, I hope, not an unrepresentative one. Notable omissions are Eve Clark's own work on the spatial forms, in, on and under (see E. Clark (1972a), that in collaboration with Haviland on kinship (Haviland and Clark (1974) and Gentner's work on 'transactional' verbs (Gentner (1975)).

62. 'Relational terms' is here a convenient label with no theoretical significance.

or 'Which tree has less?', they remained substantially correct for the first question but a large proportion of responses to the second question consisted of choosing the wrong tree, i.e., the one which had more apples. These results were consistent with the children understanding the meaning of more and understanding less as if it had the meaning of more.⁶³ The possibility that the children simply didn't understand less is argued against by the fact that they did respond to the questions involving less rather than look baffled and, furthermore, they responded quickly and confidently. This main result of the original experiment has been replicated under several different conditions and various factors pointed out by H.Clark (1970) and not taken account of in Donaldson and Balfour's study have been accommodated (see the work of Palermo and his associates, particularly, Palermo (1973, 1974), Holland and Palermo (1975)). Palermo (1973) provides independent evidence for the claim that less has the meaning of more at the stage in question using a modified semantic differential technique and, although the exact interpretation of the findings is likely to remain controversial for some time, it does appear that a reliable set of phenomena are in need of explanation. As we shall see in 2.2.3 E.Clark has, at different times, considered two alternative explanations.

63. In fact Donaldson and Balfour studied the children's responses under a set of carefully constructed conditions but the finding reported in the text was uniform across these conditions.

Similar results were obtained in a subsequent study by Donaldson and Wales (1970) concerning the child's comprehension of the pair, same and different. In situations in which children of nursery school age were asked either to give the experimenter an object which was the same as some specified reference object, different from that object, the same as that object with respect to some particular attribute or different from that object with respect to some attribute, the experimenters found that, while apparently understanding same perfectly, children understood different as if it had the meaning of same. Without being very explicit, E. Clark proposes that the same general principles as she invokes to explain the data from the more and less studies will serve for explanation in this case too but, as we shall see, this lack of explicitness covers up a serious gap in the argument.⁶⁴

64. At this point it is convenient to note a fundamental distinction between the sort of data considered relevant in the domain of relational terms and that already discussed, in 2.1, in connection with early lexical reference. Clark, in treating both, along with several other phenomena as instances of overextension, is pointing to what she considers to be essential similarities but, on reflection, one notes that there are some quite alarming asymmetries between the two situations. In the case of early lexical reference we have a term, X, which, as well as being used appropriately, also usurps the domain of another term, Y. The result is that what should be called 'Y' gets called 'X' but there is no suggestion that the child understands instances of 'Y' as if they had the meaning of 'X', i.e., it is not the case that the child misunderstands, say, horse as if it had the meaning of cow and, when (cont.p 85)

Donaldson and Wales (1970) also examined Scottish children's ability to comprehend the antonymic dimensional adjectives, big and wee, thick and thin, tall and short, etc. The situation they used was one in which the children were presented with an array of objects varying along the appropriate dimension and were asked to respond to a series of instructions which included 'Show me the X-est one', 'Show me one that is X-er than that', etc. Two findings are of particular interest to the subsequent discussion:

(i) children, on the whole, responded much more accurately to the 'general' pair, big and wee, than they did to any of the more specific adjective combinations (but see Maratsos (1973) for caution in this regard).

(ii) children responded more accurately to the positive member of the pairs showing a tendency to interpret

64. (cont.) presented with instructions of the sort, 'Give me a cow' consistently gets it right whereas, when presented with 'Give me a horse', he consistently hands over a cow (assuming, of course, that there is a cow available). This latter would be analogous to the situation I have been describing for relational terms where we find the child misunderstanding 'Y' as if it had the meaning of 'X'. For such terms, however, there is no evidence with which I am familiar to indicate that the child produces instances of 'X' when he is meaning 'Y', e.g., the child says more in a situation which demands less (see Macrae (1976) for remarks indicating the lack of inappropriate usage of come and go in the spontaneous speech of two-year olds). The exact import of these remarks is not clear to me and I certainly don't wish to suggest that the two sets of observations are quite distinct. However, clearly, we should be careful about imposing similarities where they may not exist. See also Huttenlocher (1974).

instructions including the negative member as if they contained the positive member (Eilers, Oller and Ellington (1974) contains evidence which contradicts this conclusion). In this sense the adjective pairs appeared to have important similarities to the more-less and same-different phenomena already discussed.

The second result is of more general interest to the discussion which follows but the former result can be related to a study by E. Clark (1972b) which she again, sees as providing evidence for the semantic feature hypothesis and which is interesting because it employs a very different experimental paradigm to most of the studies reviewed in this section. Rather than involving the child in performing some action on the basis of his understanding one of the forms in question, Clark (1972b) employs a game where the task of the child is to produce the antonym of the form supplied by the experimenter. These forms are drawn from the set of dimensional adjectives under discussion and may be either positive or negative instances from that set.⁶⁵ What Clark found was that responses to the 'general' forms, big and small were considerably more accurate than responses to the more specific items but that these latter items could be

65. The concern with antonymic adjective pairs only constituted half the experiment. The other half took as its subject matter the semantic field of spatial prepositions and will not be analysed in this work. It is worth pointing out here that the fact that Clark found no systematic effect of the polarity of the supplied form on the accuracy of the response would argue against the generality, across experimental situations, of some of the results already discussed.

ranked in a way which accorded with an analysis in terms of semantic features. Further she found that 'errors' tended to be in the direction of the more general terms and this was consistent with the sort of incomplete feature specification countenanced by the theory.

Results from a different domain have been interpreted in similar fashion (E. Clark (1970, 1971, 1973)). This work has concerned the child's understanding of instructions involving the temporal conjunctions, before and after and has required children to act out instructions, using farm animals, of a small number of specifiable types.⁶⁶

These are: 'P before Q', 'Before P,Q', 'P after Q', 'After P, Q', where 'P' and 'Q' refer to linguistic encodings of events which the farm animals and dolls can be made to perform in the apparatus used in the experiment, e.g., 'The dog jumped over the gate before the woman sat down'. The results of this work are somewhat complicated and certain aspects of Clark's interpretation are questionable, not least the constitution of the groups which are supposed to behave according to one or another pattern, but one claim which does clearly emerge is that children appear to go through a stage where they interpret after as if it meant before and so consistently get right the instructions involving before and consistently get wrong instructions

66. Other aspects of Clark's work in this area concerned the spontaneous production of temporal conjunctions as well as attention to forms such as when. In the analysis which follows I shall concentrate on the results of the experimental work briefly described in the text.

involving after. This is to be compared with what one might expect if the children simply didn't understand after. Then we would expect a good deal of confusion, non-responding and randomness in response.⁶⁷ Again these phenomena prove amenable to an analysis in terms of features which I discuss in 2.2.4. In connection with her work on before and after, however, Clark has introduced a further concept into the discussion which has subsequently been widely employed by her to examine phenomena which she treated originally exclusively in terms of semantic features. This concept is that of a non-linguistic strategy in one of the several senses in which that phrase has recently been employed (see Cromer (1976) for a recent review). Briefly, and in general terms, if we have two forms, X and Y, and the child consistently understands instructions involving X and consistently misunderstands instructions involving Y as if they involved X, there are at least two approaches that the theorist can adopt. He may, as has been implicit in the previous discussion in this section, assume that the child has a fully specified (in the adult sense) meaning for X which is also attached to Y. Alternatively, he can assume that, in the experimental situation, the child responds on the basis of applying a non-linguistic strategy to meanings of X and Y neither of which is fully articulated and this

67. Note again, that there is no evidence, to my knowledge, of children meaning after when they say before nor of their simply using before inappropriately when after is called for. Cf. fn. 64.

leads him to behave, in that situation, as if he fully understood X and understood Y as having the meaning of X. This recourse to non-linguistic strategies has found application in Clark's work on the comprehension of simple locative expressions (E.Clark (1972a, 1974) and in her interpretation of the more-less data described above as well as in the work reported below. It can obviously be seen as an attempt to base explanations of the development of linguistic abilities on what are, in essence, non-linguistic variables.

To conclude this brief resumé of some of the major research relevant to these topics I shall discuss the theme of a paper which relies, almost exclusively, on the coherence of the notion of non-linguistic strategy. The study of Clark and Garnica (1974) involves requests for children in the age range, 5 years 6 months to 9 years 5 months, to identify the speaker or addressee of utterances involving the deictic verbs, come, go, bring and take when these utterances are presented to the child as being uttered by one of a number of potential speakers to one of a number of potential addressees in carefully controlled circumstances. The results of the experiment bear out the following conclusions:

(i) children perform more accurately on utterances containing come than they do on utterances containing go.

(ii) children perform more accurately on utterances containing bring than they do on utterances containing take.

(iii) the pair, come and go, are responded to more accurately as a pair, than the pair, bring and take.

(iv) it is not the case that, when the child is performing more accurately on, say, come sentences than go sentences, this is because he comprehends come and fails to comprehend go. Rather, because of the skewing of the results away from what would be expected if the child were responding at random and in a way which is intimately related to the particular configuration of potential speakers and addressees adopted on the different trials, the experimenters feel justified in crediting the child with a developing set of non-linguistic strategies which he evolves to deal with the problem situation in which he finds himself. These strategies are such that following them can lead to the impression that the child does understand come and is either performing at random on go or understanding it as if it had the meaning of come. Although Clark and Garnica are not specific in this respect it is necessary to assume that the non-linguistic strategies are taken to be operating on incompletely specified feature representations of come and go and so, to this extent, these results also can be assimilated to the general framework of the semantic feature hypothesis.

Enough has been said now to give the reader an impression of the large amount of work which has been done in the last few years in these related areas and also some idea of the tendencies which have been repeatedly found in the results. I now wish to consider the value of the semantic

feature hypothesis as an explanation for development in these domains. In addition, where non-linguistic strategies have been introduced into the theorising, I shall consider the status of theories which, as well as employing a feature-based lexicon, embrace some notion of non-linguistic strategy which is seen as interacting with whatever information appears in the lexicon.

2.2.2 Antonymic dimensional adjectives

Above it was pointed out that the work of Donaldson and Wales (1970) led to the formulation of two conclusions of interest in the present context and it is the second of these concerning children's better performance on the 'undifferentiated' adjective pair, big and wee (small), that I wish to attend to first.⁶⁸

In connection with this phenomenon, Clark provides the following account (1973, p.93) :

The data on dimensional terms can also be represented in terms of components of meaning known by the child at different stages in the acquisition process. Big is substituted for other unmarked dimensional terms because it is specified (like them) as

68. I would refer again here to Maratsos (1973) (cf p 85) and his conclusion that understanding of the word, big, actually decreases when the child learns more specific dimensional vocabulary like tall and short. This would indicate that the simple additive view of the development of word-meanings I am about to examine is, at best, over simple. However, from the point of view of current concerns, we can say that Maratsos' results have no bearing on the discussion of the coherence of Clark's proposals considered as a developmental theory.

+Dimension (3) and +Polar but the child at this stage has not yet worked out how many dimensions are necessarily presupposed by the other terms such as long and tall. He has yet to differentiate between the dimensional properties of linearity, surface and volume. While big simply applies to three dimensions, tall is more complex since it supposes (sic) that all three dimensions are present, and then talks about one specific dimension: +Vertical. The child appears to learn first the feature of dimensionality, then, later on, he specifies further what kind of dimensionality he is talking about; for instance whether the dimension is +Vertical as in tall or high, or -Vertical as in long, deep, far, etc.

While some of the phrasing in the above is somewhat bizarre, the nature of the proposal and the way in which it can be cast to make the framework of Chapter 1 applicable is clear enough. The domain of enquiry, I shall regard as a part of the child's lexicon which is involved in his ability to produce and comprehend simple sentences including the dimensional adjectives as well as his ability to produce and comprehend these forms in isolation (E.Clark (1972b)). The data relevant to this domain are, in the case of Donaldson and Wales (1970), instances of comprehension or lack of it of sentences having a limited set of structures and, in the case of E.Clark (1972b), instances of the comprehension and production of isolated forms.⁶⁹ It is necessary to assume that, in the studies

69. Obviously this could be extended to take account of the different experimental procedures adopted in the several studies of the acquisition of dimensional adjectives carried out recently, e.g. Maratsos (1973), Eilers, Oller and Ellington (1974), Bartlett (1976) but there is little point in complicating the presentation in this manner at this stage.

producing relevant data, there were no additional factors of a systematic nature affecting the comprehension or production of the sentences or forms. Calling this domain of enquiry D, Clark is claiming that in D at least the following two stages can be isolated where we can regard the claims as theories of the relevant part of the child's lexicon at two times, t_1 and t_2 . At t_1 that part of the lexicon has something like the structure shown in Figure 10, ignoring differences between positive and negative adjectives as this will be discussed later.

<u>big</u> - <u>wee</u> (<u>small</u>)	↔	+Dimension (3), ± Polar
<u>long</u> - <u>short</u>	↔	+Dimension (3), ± Polar
<u>wide</u> - <u>narrow</u>	↔	+Dimension (3), ± Polar
		etc.

Figure 10

and at t_2 this same part of the lexicon will have a structure like that shown in Figure 11.

<u>big</u> - <u>wee</u> (<u>small</u>)	↔	+Dimension (3), ± Polar, ±Vertical
<u>long</u> - <u>short</u>	↔	+Dimension (3), ± Polar -Vertical
<u>high</u> - <u>low</u>	↔	+Dimension (3), ± Polar +Vertical
<u>wide</u> - <u>narrow</u>	↔	+Dimension (3), ± Polar -Vertical
		etc.

Figure 11

Obviously further stages would be necessary in order to chart the full development in this domain but the general picture is clear enough from this simple case.⁷⁰ What happens between t_1 and t_2 is that the child begins to use an additional feature, \pm Vertical, which enables him to make new distinctions in his lexical system. In this case, unlike the case of early referential vocabulary this additional feature is not accompanied into the system by a new lexical item utilising that feature in its meaning specification. Rather it is accompanied by an 'enriched' ability to understand a lexical form in a way more appropriate to the adult norm, although this form is assumed to have existential status in the earlier system. Thus the semantic feature hypothesis applied to relational terms does not make any predictions about the appearance of forms in the lexicon but only about the order of development of understanding of those forms.

Turning now then to Conditions 1 - 4, I shall assume that some version of Condition 1 can be satisfied and give no further discussion to this point.

Condition 2 is somewhat problematic as there is no

70. Clark (1972b) is of some help in this respect where, in order to handle such pairs as thick and thin, a feature \pm Secondary is introduced following Bierwisch (1967) the idea being that such adjectives are not applicable to the most extensive (and hence primary) dimension of objects. Thus one can see a third stage where the pair, thick-thin would have an entry in the lexicon something like:

Thick-thin \leftrightarrow +Dimension (3), \pm Polar, \pm Vertical, +Secondary and so on.

generally accepted feature theory which addresses the analysis of dimensional adjectives. Clark relies a great deal on the work of Bierwisch (1967) but, while this work is significant and tackles a number of fundamental problems in an interesting way, it does not provide a well-motivated inventory of features necessary for analysing the meaning of dimensional adjectives. Such an inventory would appear to be one necessary component of a general theory of the required sort. The qualifier 'perceptual' must, of course, be dropped in this part of the theory because of the presence of such features as \pm Polar which, while one might wish to argue that they are induced on the basis of perceptual experience, have no straightforward perceptual interpretation and this emphasises the need for a general theory. The extent of the problem can be seen by citing a passage from Bartlett (1976,206) which should be compared with the quotation from Clark on p.91 above.

The SFH (semantic feature hypothesis - RMA) bases its predictions about the acquisition of these features on the notion of feature generality. Thus it predicts that the dimensional feature [size] will be acquired first since it can be applied without restriction to any of the terms in the domain. While Clark (1973) makes no further explicit predictions about the acquisition of other dimensional features, she does refer to the analysis of these features in H.Clark (1973). According to this analysis, the feature [dimension] (corresponding roughly to the notion of 'extended edge' or 'extension along one dimension') will be acquired next, followed by features which express orientation of extension

(i.e. [verticality]) and relative length of the edge to which the adjective applies (i.e. [secondary] which refers to the second-most extended edge of an object).

Questions which immediately arise are whether the feature, \pm Dimension (3) in E.Clark's discussion, is to be equated with the feature, [size] in Bartlett's, whether Clark's "feature of dimensionality" can be identified with Bartlett's feature, [dimension], and exactly what confusion is lurking under the qualification, "corresponding roughly to the notion of 'extended edge' or 'extension along one dimension'" in the Bartlett passage. If something is going to correspond to both an edge and an extension along a dimension the correspondence must indeed be rough. Contrary to what I believe to be the case, I shall assume, for the sake of further discussion, that the proposal does make reference to a precise general theory.

Condition 3 is applicable and satisfied in a straightforward additive fashion and, in the passage from t_1 to t_2 , we have one feature added : \pm Vertical.⁷¹

So, giving the proposal the benefit of the doubt on a number of crucial issues, we approach Condition 4 and ask whether the theory can be grounded in any of the three

71. Following from fn 70. we can consider another transition from t_2 to, say, t_3 which will involve the introduction of the feature \pm Secondary and, again, this transition will satisfy Condition 3 additively. I won't pursue the analysis of this transition any further in the text.

senses mentioned in that condition. Is there any reason for finding the development which we can schematise as $X \rightarrow X+Y$ rather than the development $Y \rightarrow X+Y$ where, in this case, X denotes the feature, \pm Dimension (3), and Y denotes the feature, \pm Vertical? Given the lack of precision in interpretation of the features this is difficult to answer. The third possibility mentioned in Condition 4, that of linguistically grounding the proposal, can immediately be ruled out on the grounds that the relevant analyses of the world's languages don't exist and that there is no a priori reason for believing that they would support the point at issue if they did exist. Consider the possibility that the proposal might be logically grounded and here the interpretation of the features becomes crucial. The passage cited from Clark is little help in this regard although she can be seen as suggesting otherwise when she says : "While big simply applies to three dimensions, tall is more complex since it supposes that all three dimensions are present and then talks about one specific dimension: \pm Vertical". As it stands this statement is false as is witnessed by the existence of tall rectangles and, presumably, what Clark is striving towards is the claim that anything to which the contrast tall - short is applicable must also be amenable to analysis in terms of the contrast, big - small. Bartlett's choice of terminology is more transparent in this regard where we can see that having a vertical dimension 'presupposes' having size and we can thus suggest that there is the sort of relationship we are looking for between the

features, [size] and [verticality] . It seems, therefore, as if aspects of the development of dimensional adjectives may be logically grounded but, clearly, the lack of a well-formulated general theory is detrimental to any confidence we may place in this assertion. As far as the second possibility under Condition 4 is concerned little can be said at this stage. If we were to postulate 'concepts' corresponding to the semantic features under discussion and investigate their development, we would, presumably, find the 'concept' of size before the 'concept' of verticality but this, insofar as it is based on the logical relationships of the 'concepts', wouldn't take us beyond the notion of logical grounding.

In summary, then, I would say that crucial weaknesses invade the formulation of the semantic feature theory with regard to the acquisition of 'undifferentiated' and 'differentiated' antonymic adjective pairs. These weaknesses mean that, while suggestive avenues remain to be explored, judgement on the overall theory must be reserved.

I wish to turn now to the second major finding of Donaldson and Wales (1970) : that unmarked dimensional adjectives appear to be acquired before their marked counterparts. E.Clark (1973) largely following H.Clark (1970) offers an account along the following lines: for the pairs of adjectives in question, the positive member of each pair is unmarked according to a number of criteria.⁷²

72. For extensive discussion of the notion of 'markedness' and its several senses covering the senses which are relevant to the points here, see Lyons (1977b, 305ff).

In particular, the positive member, as well as being used in explicit or implicit comparison (e.g. x is taller than y, x is tall) also has a non-comparative use when it can be seen as simply identifying the dimension in question. Thus we find such phrases as 6 feet tall, 5 miles wide, etc. but not, unless special assumptions are made about the context of utterance, phrases such as ? 6 feet short, ? 5 miles narrow, etc. The non-comparative interpretation can be seen as involving one less feature than the comparative interpretation and, at this point, Bartlett can take up the story (1976, 207).

Thus, the SFH predicts that initially both terms in a pair (of antonymic dimensional adjectives - RMA) will have a nominative (sic) meaning which indicates the appropriate dimension of comparison, without regard for polarity (e.g. both long and short will mean 'having some length'). For any given pair, the SFH further predicts that the [+pol] feature will be acquired prior to its [-pol] counterpart, a prediction based on the assumption that children have a bias towards picking the greater of two objects and that this bias makes it easier for children to acquire meanings which encode 'greater' relationships.

From this it is clear that three stages at least are envisaged in the development of the child's lexicon, the domain of enquiry not having changed from that specified earlier in this section. At t_1 , using Bartlett's features, we have the situation represented in Figure 12.

<u>tall</u>	↔	+size, +vertical
<u>short</u>	↔	+size, +vertical
<u>wide</u>	↔	+size, -vertical
<u>narrow</u>		+size, -vertical
.....		
<u>big</u>	↔	+size
<u>small</u>	↔	+size

Figure 12

The important facts to note about this partial lexicon are the synonymy of each of the antonymic pairs and the lack of any feature corresponding to their comparative senses in the analyses. At t_2 we shall have a transition to the situation depicted in Figure 13.

<u>tall</u>	↔	+size, +vertical, +pol
<u>short</u>	↔	+size, +vertical, +pol
<u>wide</u>	↔	+size, -vertical, +pol
<u>narrow</u>	↔	+size, -vertical, +pol
.....		
<u>big</u>	↔	+size, +pol
<u>small</u>	↔	+size, +pol

Figure 13⁷³

73. That Bartlett accurately represents Clark's position in this regard can be seen in the following statement from Clark (1973, 94): "...both are treated as if they contain the feature +Polar; they have not quite reached the stage where the unmarked adjective +Polar is in contrast with its opposite, which is eventually specified as -Polar."

Again here the antonymic pairs are treated as synonyms but now they are supposed to have an explicitly comparative use encoded in the feature [+pol], and, furthermore, a comparative use restricted to that of saying or implying that something exceeds the relevant mean for the dimension(s) in question. By t_3 the system will approximate to the adult lexicon and we shall have the structure shown in Figure 14.

<u>tall</u>	↔	+size, +vertical, +pol
<u>short</u>	↔	+size, +vertical, -pol
<u>wide</u>	↔	+size, -vertical, +pol
<u>narrow</u>	↔	+size, -vertical, -pol
.....		
<u>big</u>	↔	+size, +pol
<u>small</u>	↔	+size, -pol

Figure 14⁷⁴

Turning now then to discussion of Conditions 1 - 4, we can see that we have a developmental theory embracing three stages, a theory which we can represent as (T_1, T_2, T_3) . I shall again assume that Condition 1 can, in some sense, be satisfied by this theory and move immediately to Condition 2.

Much the same unease arises as in connection with the development of undifferentiated and differentiated

74. Again, of course, additional features will be needed to distinguish pairs within one dimension.

adjectives and there is little point in repeating the arguments advanced earlier. There is, however, one additional cause for concern with the present theory. If the authors of the theory have a standard version of a feature theory in mind, the role of semantic features in their theory must be to distinguish the meanings of lexical items. It follows that, in a system countenancing only binary features, each such feature will occur somewhere in the system with both a positive and a negative value. Unfortunately, in the theory T_2 , the feature [pol] occurs but only positively specified and it has no distinguishing role to play in this part of the lexicon (nor, presumably, in any other part). Thus T_2 is not a theory utilising semantic features in the 'normal' way and it behoves its originators to come up with a definition of a general theory such that T_2 is an instance of it.

This point has an immediate reflex as far as Condition 3 is concerned because we cannot see either the transition from T_1 to T_2 or the transition from T_2 to T_3 in terms of the addition of features. Rather we must talk about the addition of values of features. Even this isn't quite correct because, as the above discussion has shown, [pol] cannot be a semantic feature in the accepted sense but, for now, I wish to assume that this way of talking is not too misleading and go on to discuss Condition 4.

As far as the transition from T_1 to T_2 is concerned, we are asking why the child acquires the feature value, [+pol] after he has acquired the features, [size] and [vertical],

rather than the other way round. Subject to the qualification expressed in the previous discussion of 'differentiated' and 'undifferentiated' adjectives concerning the lack of any clear statement of logical relationship between the features, it appears that the same sort of considerations as were met there could lead to the transition being logically grounded. The feature value, [+pol], interpreted as something like 'having more than average extent', assumes some dimension along which extents are being compared and the feature, [vertical], refers to just such a dimension. Therefore, although the theoretical term, [+pol], does not rely specifically on the term, [vertical], for its intelligibility, it does rely on terms of this type and, given the tentative nature of the data anyway, this seems good enough. Considerations of linguistic grounding and grounding in some more basic theory add little to the discussion at this point. Consider now the transition from T_2 to T_3 . In this case there is no logical reason why we should find the development schematised as $X \rightarrow X + [-pol]$ (where X includes the feature value, [+pol]) rather than the development schematised as $Y \rightarrow Y + [+pol]$ (where Y includes the feature value, [-pol]). Nor is there any chance of the transition being linguistically grounded: any semantic theory using a polarity feature in the description of a language would make reference to both values of that feature for the reasons spelled out above. Thus it is inconceivable that there could be more languages with semantic structures demanding use

of [+pol] than those with semantic structures demanding use of [-pol]. E.Clark is aware of the problem here and invokes an assumption made by H.Clark (1970, 274) that "the best exemplar of a dimension is an object with the most extent".⁷⁵ H.Clark refers to this as possibly "a perceptually motivated fact" and so, to the extent that it can be used to explain the appearance of [+pol] before [-pol], it can be seen as an attempt to ground the transition from T_2 to T_3 in some more basic theory of perception. It is not my intention here to examine the status of the assumption in the light of what is known about perception: what interests me is the nature of the theory which is supported by such an assumption. It is clear from H.Clark's discussion that he sees the principle as operative before the child controls the comparative sense of the dimensional adjectives. It is a principle which can be seen as causing the child to act as if he understood both adjectives as having the comparative sense of the unmarked member of the pair when, in fact, he understands both adjectives as being non-comparative in sense. So, for example, in the situation devised by Donaldson and Wales, given an instruction to the child to show the experimenter the tallest or the shortest from a set of rectangles, at the stage under discussion the child is going to understand

75. To be fair to H.Clark on this I should point out that he does cite a small amount of indirect evidence which would indicate that he is entitled to refer to this principle as something more than an assumption.

either of these non-comparatively, i.e. as something like 'show me the one with vertical extent'. Coupled with the principle introduced by H.Clark the child will respond correctly to an instruction including 'tallest' and incorrectly to an instruction involving 'shortest'. But now we have a different theory to the one put forward by E.Clark and it is not a theory which necessarily embraces a stage where the child's lexicon includes the feature value [+pol] and does not include [-pol]. Consistent with this theory we could have a first stage where the child simply understands all adjectival forms non-comparatively and possesses no principles for making it appear that this is not so, a second stage where his understanding is unchanged but the principle makes it appear as if he understood the unmarked form and understood the marked form as having the meaning of the unmarked form, and a third stage, mediated by the introduction of the feature, [pol], (both positively and negatively specified) where the child does understand the comparative senses of the adjectives. It seems to me that something like this is the position which E.Clark is moving towards (see subsequent discussions in this chapter of non-linguistic strategies and principles interacting with lexical meaning) although I have found it impossible to find a clear statement of it in her work. For such a position the transition from the second stage to the third could be logically grounded for exactly the reasons put forward above in connection with the transition from

T₁ to T₂ and the transition from the first to the second stage would be marked by the emergence of H.Clark's principle and we would expect to look to a theory of perceptual development for reassurance in this regard. However, it is not my purpose here to draft alternative proposals nor, indeed, to tease out what may be implicit in the proposals we are examining but to take what we find in the literature at face-value. From such a rationale it emerges that the formulations of the semantic feature hypothesis which we meet in E.Clark (1973) and in Bartlett (1976) are inadequate in several crucial respects, respects which have become obvious in terms of Conditions 1 - 4.⁷⁶

76. Nor is it possible to turn to H.Clark's later work (1973) involving what the author calls 'rules of application' to find an explanation as to why the child should understand the meaning of the unmarked forms before the meaning of the marked forms (if indeed he does) rather than having procedures which enable him to act as if he understood the meaning of the unmarked forms before he understands the meaning of the marked forms. In particular, the number of 'reference points' involved in their application will not provide the necessary complexity measures as both the marked and unmarked adjectives require the same number of reference points when used comparatively. Incidentally, while Clark's claim that (36) "each adjective has two points of reference" is clearly true for the pair he considers, high and low, as well as for other pairs such as near and far, it isn't true for pairs such as tall and short. If the suggestion is that tall, like high, makes implicit reference to ground level, I would counter that a tall pole buried so that only a small amount sticks out of the ground is still a tall pole. Of course, in all cases, the unmarked form requires less reference points than the marked form when the latter is used non-comparatively.

2.2.3 More and less

A similar conclusion is justified for the analysis, in terms of the semantic feature hypothesis, of the data cited on the acquisition of more and less. Again the domain of enquiry, D, is fixed as a section of the child's lexicon and data relevant to D include the child's ability to comprehend simple instructions including more or less and also, somewhat anecdotally, the child's spontaneous use of these forms. Clark is more explicit than she is for dimensional adjectives when she says (1973, 90 - 91):

First the child uses more and less in the nominal non-comparative sense only. Next, since the nominal term refers to extension rather than to lack of extension, the child will use both more and less to refer to the extended end of the scale, and finally, he will distinguish less from more and use it contrastively to apply to the less extended end of the scale. At the first stage, therefore, more is simply taken to mean "amount" or "quantity of" and its comparative nature is not understood ... While this nominal interpretation of both words would explain why more and less were treated as synonyms, it does not account for why more and less both mean "more" ... one has to make one assumption at this point: that the notion "having extent" is always best exemplified by the object with the most extent ... At the last stage more and less will be used comparatively in their contrastive sense and less is then differentiated from more.

Here we have a direct analogy to Clark's theorising in connection with dimensional adjectives and we can

represent the theory as a three-stage theory as in Figure 15.

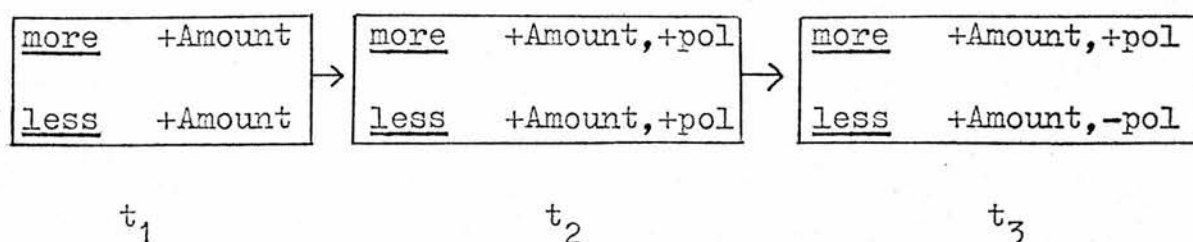


Figure 15

Exactly the same set of problems as arose in the context of dimensional adjectives arises again when we attempt to analyse this proposal in terms of Conditions 1 - 4 and, again, it is possible to formulate a somewhat less problematic theory by introducing a non-linguistic principle to explain the child's behaviour at t_2 . There is no point in repeating these considerations here. However, there is an additional problem concerning the supposed non-comparative use of more at t_1 , which deserves brief discussion.

While it is possible to argue a weak case for more being the unmarked member of the pair, more and less, on the basis of its relatedness to much in the pair, much and little, it is not the case that more, unlike the unmarked dimensional adjectives, has the specialised task of naming a dimension. To my way of thinking there is nothing marked about the phrase, 15 less when

compared with the phrase, 15 more.⁷⁷ And what exactly is the non-comparative use of more which Clark refers to? She gives no references but Bloom (1973) (see also Brush(1976)) provides extensive and relevant discussion of the use of more in the speech of her own daughter, Alison. In summary, she suggests that in the earliest stages more has two distinct uses. In the first use, an instance of a category exists in the environment, ceases to exist, and then appears again (or a new instance of the category appears) to be designated 'more', and, according to the second, an instance of X is joined in the environment by another instance of that category which is designated 'more'. What she did not find is an instance she refers to as "the comparative" where two instances of a certain stuff together make more of that stuff and this result of putting the two instances together is designated 'more'. Without wishing to quibble with Bloom's interpretation of the phenomena it seems to me that the choice of the term 'comparative' for the third usage is unfortunate. It carries the

77. This is to disagree with H.Clark (1970, 272) where he says that the sentence, John has more apples than Dick, is ambiguous in that it may or may not carry an implication that both John and Dick have many apples. According to the first interpretation, more is being used nominally, says Clark. He goes on to say that the sentence, John has less apples than Dick is unambiguous in this respect and always "implies that John and Dick have a paucity of apples". It seems to me that Clark's judgements are simply incorrect in this respect and I find nothing odd about a use of the second sentence when both John and Dick are well appled.

implication of 'non-comparative' onto the first two usages and to refer to them in this way is misleading as they do involve comparison and it is certainly not the case that they can be seen as instances of more which refer simply to amount or extent. If it is this sort of usage to which Clark is alluding it seems to me to require a good deal of further argument.⁷⁸

Without going into any more details, then, it seems reasonable to conclude that the semantic feature hypothesis suffers from the same defects when applied to the acquisition of more and less as it does for the dimensional adjectives. To the extent that the explanation the hypothesis embodies depends upon the notion of markedness it would appear that it is on even weaker ground in the context of more and less.

Turning briefly to same and different, we come across a situation which is even worse. Clark doesn't even begin to speculate as to what the feature composition of these words might be during the time at which they are confused and it is transparent that considerations of markedness

78. All of this, of course, leaves untouched the data from Donaldson and Balfour (1968) and from Donaldson and Wales (1970) indicating that children were interpreting more and less as having the meaning of some under some conditions. But it also becomes difficult to relate this non-comparative usage shown in the comprehension of 3-year olds to the non-comparative usage discussed in the text which is from the beginnings of lexical development.

and non-comparative interpretations are going to be of little use in this respect. There is no dimension of 'sameness' for same to name as same is inherently relational and, as far as my judgements are concerned, there are no situational variables rendering the question, Are X and Y different?, somehow specialised, when compared to the question, Are X and Y the same? I conclude that the semantic feature hypothesis has nothing to say about the acquisition of same and different.

2.2.4 Temporal conjunctions

The work which E. Clark has done on the comprehension of before and after provides an interesting example of the interplay of the acquisition of semantic features and non-linguistic strategies. Clark claims to discern four stages in the acquisition of these terms insofar as this acquisition process is revealed by the experimental situation she employs described briefly in 2.2.1. Using Clark's numbering, at Stage I the child, while comprehending that before and after involve the ordering of events in time, relies on an order-of-mention strategy: whatever is mentioned first happens first.⁷⁹ The

79. The extent to which this sort of partial understanding of before and after is justified isn't clear to me. Obviously if the child had an order-of-mention strategy and treated before and after as 'semantic noise' we would get Stage I behaviour. We would also expect to get Stage I behaviour if the conjunction were missed out, replaced by a nonsense form or by some other conjunction the child didn't understand. In the discussion in the text I shall assume that the children did manifest partial understanding - an assumption which is clearly attractive to the semantic feature hypothesis.

conjunction of these two hypotheses leads to the child apparently comprehending utterances of the form, P before Q and After P, Q and consistently getting wrong his attempts to act out utterances of the form, Before P,Q and P after Q. At Stage IIa the child understands utterances containing before but still has an incomplete specification for the meaning of after and so, for utterances containing after, he continues to resort to the order-of-mention strategy. In terms of performance on the experimental task what this means is that the child now correctly acts out sentences containing before, irrespective of the position of before in the sentence, and, while still getting right sentences of the form, After P,Q he consistently gets wrong sentences of the form, P after Q. At Stage IIb we find the situation we have already come across where after is interpreted as if it had the meaning of before and so now the child is presumed to have a more complete specification of the meaning of after than at Stage IIa but it is a specification which is inappropriate in certain details. The child's performance at Stage IIb is such that he is consistently correct for sentences including before and consistently incorrect for sentences with after. With the more complete specification of the meaning of after the order-of-mention strategy is dropped (but see below). Finally, at Stage III the child acquires the vital feature value enabling him to distinguish the meaning of before from that of after and leading to

consistently correct responding in the experiment. Before going on to examine the exact nature of Clark's proposals it is necessary to make two remarks. The first is that I have described the children's behaviour at the different stages as if it were completely clear-cut. This is not true, of course, and, in some cases the figures cited by Clark are not totally convincing. The second, and related, point is that the groups of children were established post hoc on the basis of trends which became apparent in the results and the children were not assigned to groups at the beginning of the experiment on the basis of their ages.⁸⁰ Group IIb had only three children in it and what this amounts to is an indication that the empirical support for the Stages I - III is not compelling. In the context of this work, I don't wish to regard this as crucial and I shall proceed on the assumption that each of the stages is well-established.

The domain of enquiry, D, can again be fixed as a part of the child's lexicon but this time, in addition, we must consider the child's strategies which he resorts to in cases of partial understanding and the interaction of these strategies with the lexicon. Behaviour which is relevant to D is restricted, in this discussion, to the child's comprehension of sentences involving before and after as evidenced by his abilities to act out the events

80. There was a tendency for older children to belong to the more 'advanced' groups but Clark does not provide any analysis to show whether the correlation between age and stage of development is significant.

depicted by the sentences in the situation described by Clark (1971).

At Stage I, largely following Clark's informal remarks, one may infer that the part of the lexicon in which we are interested will have the structure represented in Figure 16.

before \longleftrightarrow +Temporal, -Simultaneous
after \longleftrightarrow +Temporal, -Simultaneous

Figure 16

Another component of the theory is the following strategy:

S_T : when presented with a structure of the form

(P) (+Temporal, -Simultaneous) (Q)

or a structure of the form

(+Temporal, -Simultaneous) (P) (Q)

assume that the event referred to by 'P' precedes the event referred to by 'Q'. T_I (the theory for Stage I) is thus a two component theory, which we could represent as $L_I + S_T$ where L_I designates the lexicon in its essential respects at Stage I.

At Stage IIa the lexicon changes to the structure L_{IIa} , represented in Figure 17 and S_T will be unchanged (in order for this to be so there will have to be a convention that S_T only applies to structures in which its feature specification is exactly met so that it does not apply to sentences including before). Thus $T_{IIa} = L_{IIa} + S_T$

before \longleftrightarrow +Temporal, -Simultaneous, +Prior

after \longleftrightarrow +Temporal, -Simultaneous

Figure 17

At Stage IIb the lexicon, L_{IIb} , is as in Figure 18 and S_T is dispensed with.⁸¹ Thus we have $T_{IIb} = L_{IIb}$.

before \longleftrightarrow +Temporal, -Simultaneous, +Prior

after \longleftrightarrow +Temporal, -Simultaneous, +Prior

Figure 18

Finally, at Stage III the lexicon reaches its putative adult state as in Figure 19 and, again, no non-linguistic strategies play a role.

before \longleftrightarrow +Temporal, -Simultaneous, +Prior

after \longleftrightarrow +Temporal, -Simultaneous, -Prior

Figure 19

Thus we have a sequence of theories, $(T_I, T_{IIa}, T_{IIb}, T_{III})$, and we can consider the various transitions in this

81. To say that the strategy is 'dispensed with' is something of an oversimplification as there is a good deal of evidence indicating that adults use a similar strategy in their perception of sentences. However, there is a considerable difference between the child relying on a strategy to produce an interpretation and an adult finding it easier to deal with sentences which accord with a particular strategy. The sense in which the adult is using the strategy is somewhat opaque to me and so, while not wishing to discount the psycholinguistic evidence (e.g. Clark and Clark (1968)), I shall assume in the text discussion that the strategy does disappear.

sequence with regard to Conditions 1 - 4.

As far as Condition 1 is concerned we seem to be in essentially the position we have met repeatedly in this chapter and there doesn't appear to be any reason to adopt a different stance in this case.

Condition 2 is complicated by the fact that some of the theories in the sequence are hybrid consisting of lexicons and non-linguistic strategies. As far as the lexicons are concerned we merely need to express the same reservations as have already been expressed with regard to earlier proposals concerning inventories of semantic features. Similar reservations must be expressed concerning the notion of 'strategy', i.e. we need some general formulation of what can count as a strategy and how such strategies can interact with partially specified lexical information resulting in a form of comprehension. At the very least then we can ask for two general theories. One of these is a theory of the lexicon which ignores strategies and this is not distinct from the general theory required by the earlier proposals we have considered. The other is a general theory of lexicon, strategies and their interaction. For future reference I shall refer to these, purely hypothetical, general theories as L and LS. Clark provides no insights as to the nature of these theories and, as we have seen, the sequence of theories under consideration draws its members from the two types, i.e. we have a sequence of theory-types which

we can represent as (LS, LS, L, L) involving a discontinuity between Stage IIa and Stage IIb which we might expect to produce problems in the application of Condition 3.

Consider, then, Conditions 3 and 4 with respect to the transition from T_I to T_{IIa} . It is accompanied by the introduction of the feature value (cf. discussion in 2.2.2), +Prior, as part of the meaning of before and there is no change in the set of strategies. If we are prepared to countenance the introduction of single feature values this looks like straightforward addition thus satisfying Condition 3 and, furthermore, the mixed sequence of features and feature values, (+Temporal, +Simultaneous, +Prior) appears to be logically grounded in much the same way as was the sequence (using Bartlett's notation again), ([size], [vertical], [+pol]), that is the feature, +Simultaneous, only becomes intelligible in the context of temporal notions and the feature value, +Prior, 'presupposes' lack of simultaneity. There is nothing of value to be gained by considering the other possibilities for grounding the theory at this stage.

The transition from T_{IIa} to T_{IIb} is more problematic - as we might expect, given that we have theories of distinct types involved. The strategy, S_T , is lost in the transition and there is no concomitant complication in the lexicon. In a sense, therefore, we might wish to say that we have a straightforward simplification of the theory

resulting in a violation of Condition 3.⁸² I hesitate to jump to this conclusion but only because the theory presents what are, if anything, more profound grounds for unease via its failure to satisfy Condition 2 for this transition. Additional argument, of course, could render the situation more respectable but, so far, this has not been forthcoming.

Finally, consider the transition from T_{IIb} to T_{III} . It is easy to see that all that is involved here is the introduction of the feature value, -Prior, and, again, we have satisfaction of Condition 3 additively. As far as Condition 4 is concerned, however, there is no logical reason why the feature value, -Prior, should enter the system after the feature value, +Prior nor, clearly, is there any linguistic reason since, in any semantic analysis of an adult language, a theorist using one of the values will also use the other, given the usual conception of semantic feature theories. So we are left with the possibility that there might be some developmental theory, T' , in some domain, D' , which is regarded as more basic than the domain, D and which is such that our theory, T , for the transition from T_{IIb} to T_{III} , can be grounded in T' .

82. At this point it is useful to remind the reader of the weakness of the empirical evidence in favour of Stage IIb. Even if it were scrapped though the problem of discontinuity would still arise in connection with the transition from Stage IIa to Stage III.

Attempts to provide such a grounding can be gleaned from the work of the Clarks (see, particularly, H. Clark (1973, 48 - 52)). These attempts proceed from the view that the system of prepositions, conjunctions, etc. encoding temporal concepts in English is based on a spatial metaphor and they employ the 'equations':

before (temporal) = before (spatial) = in front of (spatial)

and

after (temporal) = after (spatial) = behind (spatial)

These 'equations' establish correspondences between the lexical items in which we are interested and lexical items which encode spatial notions. Now, the argument goes, in the domain of the child's developing perceptual space, it is reasonable to assert that his concept of the space in front of him develops more quickly and is more elaborate than his concept of the space behind him and, furthermore, the space in front of him can be regarded as positive when compared to the space behind him thus establishing links between the pair, before and after (positive and negative) and the pairs of words discussed in earlier sections of this chapter. Whether this procedure matches up to the rather strong constraints on reductions discussed in Condition 4 and developed more fully in Chapter 4 is debatable but I believe that there is at least the beginning of a sound argument here. It is to be noted, however, that it is an argument to show why the child learns before at an earlier stage than after and, strictly speaking, doesn't address the cognitive or

perceptual status of the feature values, +Prior and -Prior.

In summary, we can say that the semantic feature hypothesis fares badly in the domain of temporal conjunctions because of the lack of a general theory incorporating a lexicon and interacting strategies. There appears to be a difficult discontinuity countenanced by the theory but not argued for and, against these major flaws, the successes in grounding which I have discussed above have a debatable significance.

2.2.5 Come and go

Some of the summary results of Clark and Garnica (1974) have already been mentioned in 2.2.1 and I now wish to focus on the central role played by strategies in the explanation of the data they advance. In what follows, I shall concentrate on the authors' treatment of come and go; their analysis of bring and take is similar in all essential respects.⁸³ The data obtained in the study seemed to justify the setting up, post hoc (cf. the discussion of a similar procedure in 2.2.4), of four developmentally ordered groups. The task the children faced was that of identifying the speaker or addressee

83. The main conclusion that the pair, bring and take, was more difficult than the pair, come and go, has already been mentioned. That bring and take can be seen as having one additional semantic feature, encoding their 'causative' nature, when compared with come and go makes this conclusion consistent with the predictions of the semantic feature hypothesis.

of an utterance containing either come or go in a carefully constructed farm-yard situation and the results for the four groups were distributed as follows in Figure 20.

Group	Speaker to be identified		Addressee to be identified		Mean
	<u>Come</u>	<u>Go</u>	<u>Come</u>	<u>Go</u>	
A	90	24	100	1	54
B	30	80	73	23	52
C	76	87	94	33	73
D	83	90	94	70	84

Figure 20 Percentage of semantically correct responses produced by each group (reproduced without indication of figures consistently below 50% from Clark and Garnica (1974, 12)).

A gross analysis of these results taking account only of the means in the right-hand column would lead to the conclusion that Groups A and B were performing at chance level and knew nothing about the meaning of either come or go and that Groups C and D did know the meanings of both the words. A somewhat less gross analysis, averaging responses to come-sentences and to go-sentences would lead to the following conclusions for each group:

- A : understands come and consistently goes wrong on go.
- B : performs at chance level on both come and go, i.e. doesn't understand either.
- C : understands come and performs at chance level with go.
- D : understands both come and go.

Clearly, insofar as the groups A - D are roughly ordered with respect to chronological age, what we have above, as a developmental sequence, would be difficult to make sense of. Clark and Garnica claim, however, that, by taking account of the detailed structure of the results as presented in Figure 20 and by utilising the notion of 'strategy', it is possible to reconstrue the development in such a way that it becomes more intelligible. The strategies they propose are presented in Figure 21.

Group	Speaker to be identified	Addressee to be identified
A	Choose goal	Choose goal
B	Choose non-goal	Choose goal
C	1. If <u>come</u> , choose goal 2. If <u>go</u> , choose non-goal	Choose goal
D	1. If <u>come</u> , choose goal 2. If <u>go</u> , choose non-goal	1. If <u>come</u> , choose goal 2. If <u>go</u> , choose non-goal

Figure 21. Rules used to identify speaker and addressee (from Clark and Garnica (1974, 13)).

Following these rules will enable the child in Group A to be consistently correct in his responses to utterances containing come and consistently incorrect to those containing go and, in fact, to act as if they contained come. For a child in Group B following the rules will lead to his being consistently correct in identifying the speaker of an utterance containing go and in identifying the addressee of an utterance containing come while consistently misidentifying the speaker of an utterance

containing come and the addressee of an utterance containing go. The child in Group C will consistently identify correctly in response to utterances with come; in effect, he now controls the adult procedure. The addressee of utterances containing go will, however, continue to be misidentified. Finally the child in Group D controls the adult rules and performs correctly under all conditions.

Two remarks need to be made immediately in connection with this way of looking at things. The first is that the strategies or rules must be presumed to operate on partially specified semantic specifications for the verbs, come and go. Such a specification will provide a necessary condition on the rules operating at all although, as was mentioned in connection with before and after, it is not clear that the authors have done anything to allow us to distinguish between the children partially understanding the verbs (e.g. as verbs of motion) and their treating them as semantic 'noise'. However, assuming that the former possibility is the correct one we can see that this wholesale resort to strategies does not imply complete abandonment of the semantic feature hypothesis although this hypothesis has a very attenuated role to play in the present context. The second point is that it is difficult to be clear that the study tells us very much about the children's knowledge of the meaning of the verbs. The child has a problem to solve, a problem which a mature acquaintance with the verbs, come and go, will render transparent but we cannot conclude, from the child's

inability to solve the problem and his resort to strategies, that he does not control the meanings of come and go. A similar point is made by Richards (1976) who shows that, in what he regards as more natural and more child-oriented surroundings, it is possible to demonstrate control of come and go at ages considerably younger than those studied by Clark and Garnica. This control demands that the child be credited with some deictic information in his meanings for come and go (cf. also Macrae (1976)). This argument is strengthened by the recent views of Shatz (1977) pointing to consistent variation in children's performances with certain linguistic materials according to task demands. In short, although an adult knowledge of the meanings of come and go will entail the use of the mature strategies for identifying speakers and addressees, we cannot conclude that failure to use these mature strategies has any implications for the nature of the child's representations of the meanings of come and go. Thus there is a substantive and difficult issue here which raises problems in the delimitation of the domain of investigation, D. Presumably, Clark and Garnica would say that they are studying the interaction of incomplete lexical knowledge and strategies but the alternative that they are studying problem-solving strategies which are highly task-specific and which make minimal reference to the contents of the lexicon cannot be lightly dismissed.

With this qualification in mind, then, we have a sequence

of theories, (T_A , T_B , T_C , T_D), where each of the T's is a hybrid theory consisting of a partial lexicon, a set of strategies and a statement of the interaction of these two components. Neither Condition 1 nor Condition 2 lead to any new questions for such a sequence. The reservations and problems we have already met merely need to be re-emphasised.

Consider Condition 3 for each of the transitions in the sequence. The change from T_A to T_B , although involving no alteration as far as features are concerned,⁸⁴ does introduce a strategy at B that was not found at A, viz., 'Choose non-goal'. Similarly, for the transition from T_B to T_C ; in the latter we find the conditional strategies, 'if come, choose goal' and 'if go, choose non-goal', which are not used in T_B although, 'Choose non-goal', a strategy which the child does use at B, is not found in T_C . We therefore have this 'simple' strategy being replaced by the two conditional strategies and, while it might be tempting to resort to the intuitive notion of

84. In fact this is true of the whole sequence. The sense in which a correspondence between strategies and meanings can lead to the relatively early acquisition of the latter, while insisted on (see, particularly, Clark (1972a, 1974) in Clark's work, is never made clear. At some point, presumably, the child has to learn features which will distinguish the meanings of come and go and exactly how following strategies which lead him to behave as if he already controlled the meanings of come and go facilitates this acquisition is a mystery. Similar remarks apply to the learning of +Polar and +Prior in the earlier discussion.

simplicity discussed in Chapter 1, we have to conclude that Condition 3 does not apply to this case. Finally, the transition from T_C to T_D is not marked by the development of any new strategies and what we find is an extended use of the two conditional strategies developed at C together with the dropping of the 'simple' strategy, 'Choose goal'. On the face of it this looks like a failure to satisfy Condition 3 as we have a simplification occurring in the developmental sequence and there is no suggestion that the strategies can be interpreted as constraints.⁸⁵

Condition 4 gives rise to interesting problems in connection with the transition from T_A to T_B . The question we have to ask can be formulated: why is it that we find the developmental sequence, $X \rightarrow X +$ 'Choose non-goal', where X includes 'Choose goal' rather than the sequence $Y \rightarrow Y +$ 'Choose goal' where Y includes 'Choose non-goal'? What this boils down to is the question as to why the goal of the movement should have some sort of ontogenetic primacy when compared to the non-goal. Clark and Garnica are aware of the problem and they say (19):

The goal, however, plays a basic role in the child's strategies from the beginning. This could be because the goal was always named in the deictic

85. Note that there can be no resort to a more complex lexicon to off-set this, resulting in the conclusion that Condition 3 does not apply. As pointed out in the text, the lexicon must be assumed to be unchanging throughout the period studied.

utterances. In addition the one animal at the goal may have been more salient because the goal was a distinct location.

But such an explanation, while possibly valid, reveals the limitations of the Clark and Garnica study. Insofar as there is an attempt to ground the subsequence, (T_A, T_B) in some more basic theory, this latter is intimately related to the features of the testing situation. If we are interested in the general question of the acquisition of come and go, it is not the case that goals are always mentioned in utterances including these verbs nor is it always the case that the goal constitutes a definable location. This strengthens the possibility that the Clark and Garnica strategies are created by the children to handle a particular problem situation and tell us little about lexical development. Even putting this aside there is no clearly-stated theory for the Clark and Garnica proposal to rest on. What would be needed would be an investigation of the child's developing attention, how it is related to the content of utterances and how it is related to 'definable locations'. If such an investigation exists, Clark and Garnica do not cite it. It is possible then, to give the authors credit for realizing that there is a problem here but nothing they say can be regarded as a solution to the problem. The other two possibilities for grounding the transition, logical and linguistic, do not arise and the reader can quickly see this without further discussion on my part.

What can we say of the transition from T_B to T_C given that we have already noted that Condition 3 does not apply? The question which Condition 4 leads us to formulate is: why do we get the developmental sequence, $X + \text{'Choose non-goal'} \rightarrow X + \text{'if come, choose goal'} + \text{'If go, choose non-goal'}$ rather than the possible sequence, $X + \text{'If come, choose goal'} + \text{'If go, choose non-goal'} \rightarrow X + \text{'Choose non-goal'}$? There is no straightforward answer to this question. There is a temptation to look for a logical grounding in that the instruction comprising the non-conditional strategy, 'Choose non-goal', appears as the consequent of one of the conditional strategies but it is not the case that the conditional strategy is unintelligible outside a system in which the non-conditional strategy also exists; T_C is a case in point. Although there is obviously the possibility of grounding this transition in a cognitive theory, as Clark and Garnica do not speculate along these lines, I shall remain non-committal. The idea of linguistic grounding doesn't apply because of the inapplicability of Condition 3.

Finally, as the transition from T_C to T_D has failed to satisfy Condition 3, the question as to whether it satisfies Condition 4 does not arise.

Summarising, it seems clear that the semantic feature hypothesis and its association with the notion of 'strategy' does as badly in the area of the acquisition of come and go as it does elsewhere. To the extent that the domain of enquiry is difficult to determine it is tempting to

conclude that it does worse. It is less distressing to know what you are doing and doing it badly than not to know what you are doing. This lack of any clear statement on the domain of enquiry and about the way in which the study in question engages it renders much of the above discussion otiose.

2.3 Nelson's functional core concept model and its application to lexical development

Nelson presents her central ideas on lexical development in her monograph (1973a) and two papers (1973b, 1974) and it is the last of these which contains the most forceful and systematic attempt to put forward a coherent theory and on which I shall concentrate in this section.

The set of facts to be explained by Nelson's proposals overlap with those addressed by E. Clark but are not co-extensive with them and so this renders any direct comparison difficult. Nelson wishes to take account of a rather wider range of observations than Clark although she does mention overextension as one of the crucial phenomena to be explained by an adequate model (see below). The phenomena she regards as basic are:

1. The fact that there are important commonalities between sets of lexical items when the vocabularies of a large number of children are examined. As Nelson puts it (269)⁸⁶: "The one outstanding general characteristic

86. Unless otherwise indicated all page references in this section are to Nelson (1974).

of the early words is their reference to objects and events that are perceived in dynamic relationships." Objects which don't move, which the child cannot interact with, which don't emit noises, etc. are not named by children at the earliest stages of language development (see Nelson (1973a) for data and extensive discussion).

2. A small child will often invent a word which expresses a concept for which he can find no linguistic expression in the adult language.⁸⁷

3. Words, once acquired, are subsequently generalised in their application. In this respect, Nelson has in mind a phenomenon which embraces Clark's idea of 'overextension' and includes all subsequent applications of the word to similar instances. Her main line of disagreement with Clark is contained in the claim that (269):

87. It might be pointed out here that this issue is more controversial than Nelson suggests. She cites Leopold for support but his conclusion to the relevant discussion is hardly encouraging to her view. He says (1949, 117): "My frantic search for standard bases (for apparently invented forms - RMA) proves that my experience had by that time convinced me that all of Hildegard's words proceeded from standard words. Students who approach the problem with the conviction that children do invent words will undoubtedly take these words as proof of their thesis." In case those readers familiar with Halliday (1975) should feel that Nigel's early development is supportive of Nelson I would point out that those forms which Nigel used which were clearly not based on the adult lexicon were not used to refer to classes of objects in the world, i.e. they did not encode concepts in the sense in which Nelson seems to want to use this latter notion.

Similarity may be based on many different dimensions of which the static perceptual dimension of shape is only one; others include function, action, or affect.

From these three points it is clear that Nelson's interests go beyond merely explaining the referential use of early vocabulary and, in particular, beyond explaining overextensions. As well as being interested in the structure of the entries in the child's primitive lexicon she is also concerned with the relationship of this , lexicon to a set of developing concepts and with the implications this might have for the order in which words enter the lexicon as opposed to the order in which features or attributes come to be used in lexical entries. In 2.3.1 I shall present Nelson's critique of Clark's semantic feature hypothesis, take issue with it, consider the alternative she presents and argue that it is subject to the same criticisms as she levels at Clark and that it might also be defended in the same way as Clark's thesis. 2.3.2 will attempt to put her proposals into a form where Conditions 1 - 4 can be applied to them and to evaluate them against the conditions.

2.3.1 Nelson's critique of 'abstraction theory'

'Abstraction theory', which is seen as embracing Clark's semantic feature theory, among other things⁸⁸ is an

88. Nelson identifies this position with that adopted by Locke and its ancestry can probably be traced to at least Aristotle.

approach to the development of concepts. An abstraction theory claims that the establishment of a concept involves the abstraction of a set of similarities from a set of exemplars of the concept. Thus, according to this view, given a set of instances of a concept, each of which is analysed as a set of attributes, say $I_1 = A_1, A_2, \dots, A_m, B_{11}, B_{12}, \dots, B_{1i}$; $I_2 = A_1, A_2, \dots, A_m, B_{21}, B_{22}, \dots, B_{2j}$; ...; $I_n = A_1, A_2, \dots, A_m, B_{n1}, B_{n2}, \dots, B_{nk}$ where the A's and the B's refer to attributes and none of the B_{ij} occur in all instances, the concept learner abstracts the similarities from these exemplars, ignoring the differences, and ends up with, what is essentially, a conjunctive set of properties which he treats as providing necessary (and perhaps sufficient) conditions for a new instance being an instance of the concept. In the case above the learner would end up with a concept, C, which we could represent as $A_1 \& A_2 \& \dots \& A_m$. Citing Cassirer, Nelson points out what is considered to be a fatal inadequacy in this view. This is that it presupposes that the concept learner is already acquainted with the concept to be learned and this guides the establishment of the set of exemplars. It is only if the concept learner knows that I_1, I_2, \dots, I_n are instances of C that he knows to compare them and thus abstract the definition of C. But how does he know that I_1, I_2, \dots, I_n are instances of C in the first place unless he already possesses a definition of C?

It seems to me that the force of this criticism can be

dissolved by two additional, and not unreasonable, assumptions. These assumptions are independent and, either of them alone or both together will provide a version of an abstractionist theory which does not suffer from the defect Nelson points to. The first assumption is that some criterion is fixed (biologically?) which determines whether an instance shall be judged similar to another instance or not (irrespective of the dimensions of comparison). A new instance, analysed into attributes, will be compared against the set of attributes constituting a concept. An uninformed comparison of the sort envisaged by Nelson will, in most cases, lead to a modified and probably useless concept but a comparison utilising a criterion will yield a number either above or below criterion. If the number is above criterion the instance will be assigned to the concept with an accompanying modification of the concept to take account of those properties which the original concept had and which the new instance does not have. In the event of the new instance not being assigned to the concept in question because of failure to reach criterion, further comparisons could be made leading ultimately to success or, in the limiting case, to the establishment of a new concept corresponding, to begin with, to the new instance. Setting the criterion high would lead to a proliferation of concepts and setting it low would lead to a small number of very general ones. This idea clearly has much in common with Wittgenstein's (1953) notion of 'family resemblance' which, while developed in an entirely

different context, has recently been explored with regard to adult systems of categorisation by Rosch and her associates (see, particularly, Rosch and Mervis (1975)). It is not important whether this comprises a good model of conceptual development at this stage⁸⁹ but what is important is to see that an abstraction model of concept development does not necessarily involve the logical absurdity Nelson wishes to credit it with.

The second assumption, also avoiding the thrust of Nelson's argument, is that certain dimensions may be picked out by the concept formation process as instrumental in determining similarities whereas other dimensions are ignored. For example, we could suggest that visual attributes of instances are what count in the establishment of concepts and, given access to a theory which made explicit the notion of 'visual attribute', an abstraction theory could be saved. What this amounts to is an assumption that the child comes to concept learning equipped with a tendency to compare instances along certain dimensions, an assumption which even a philosopher with the empiricist yearnings of Quine finds necessary and is encapsulated in his postulate, an

89. In 2.5 below I shall be investigating some of the work of Rosch which includes assumptions of a similar nature.

'innate quality space' (see Quine (1960)).⁹⁰

Claiming to avoid the pitfalls of abstractionist theories, Nelson develops the notion of a 'functional core concept'. She argues that (276):

...analysis (into attributes - RMA) is not the prerequisite to the synthesis of concepts. Rather a dual process is found to be at work - first categorising according to some principle and then identifying common attributes. (first emphasis in original, second emphasis mine - RMA)

The question that immediately arises then concerns the principle on which the categorisation is initially based. Nelson's answer to this is to consider instances, not in isolation, as abstractionist theories do, but "in the context of their relations to other instances and concepts." (ibid). She claims further that:

90. These remarks become important again in connection with Nelson's own positive proposals to be discussed below. I ought to point out here that Clark is not, on the face of it, concerned with concept development and that, therefore, one or two additional steps are necessary in Nelson's argument in order for it to be directly relevant. These steps concern the directness of the relationship between concepts and lexical entries and I have seen nothing in the writings of Clark to indicate that she would see this relationship as anything other than direct. If that is so, what we can say is that Clark's views on lexical development can be seen as committing her to an abstractionist view on conceptual development with the child abstracting (to begin with) perceptual attributes from instances and assigning conjunctions of such attributes as the values of concepts. Clearly contact is made here with the second assumption discussed in the text (see below for more extensive discussion).

Whole elements ... take on definitions as concepts in terms of the synthesis of their functional or dynamic relations. Subsequently, other whole elements that enter into the same set of relations can be granted concept status within this previously defined concept. Analysis of parts of the whole is unnecessary to this initial concept formation process.

The logic behind this move is clear although the details of the move itself are murky. What is lacking, according to Nelson, in an abstractionist account of concept learning, is some parameter with respect to which instances can be compared which is distinct from concept-membership. She proposes to provide such a parameter in the "functional and dynamic relations" of the whole to other wholes. Analysis of instances into their component attributes will only follow on this initial concept formation and will have a part to play in concept identification when the functional and dynamic relations employed in the genesis of the concept are not satisfied.

To illustrate, Nelson considers the hypothetical development of the concept, 'ball'. She suggests that conceptual development in this domain begins with the child's first encounter with a ball, assuming that this is at an age where he is 'ready' to form concepts, which may result in the schema shown in Figure 22 which is little more than a representation of the situation in which the child has met the ball.

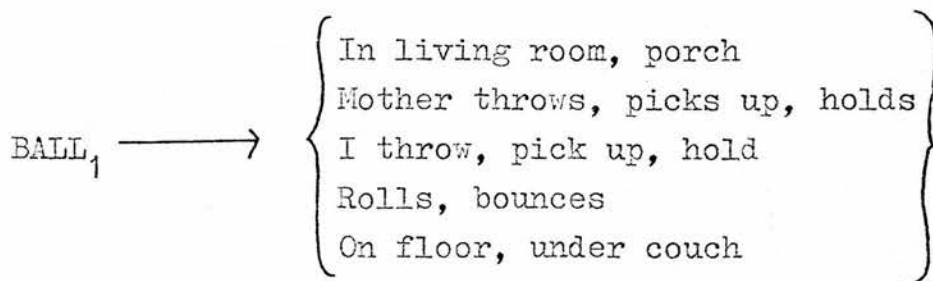


Figure 22 (from Nelson (1974, 277)).

To be noted especially in connection with this is the fact that, at this stage, the child is not presumed to control any lexical representation of 'ball', i.e. BALL₁ is a concept and not a lexical item, and, furthermore, that what is represented on the right-hand side of the arrow is intended to be relational and dynamic information. This information is presumed to be developed on the basis of the child's acquaintance with a single instance of a ball, i.e., the child experiences one ball in the living room and on the porch which is thrown by mother, picked up by mother, etc. Nelson refers to this blending of experiences with a single ball as 'functional synthesis'. Further development involves the child acquiring experience of another ball leading to the functional synthesis which can be represented as in Figure 23.

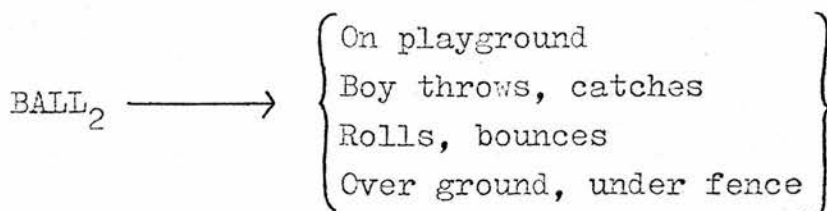
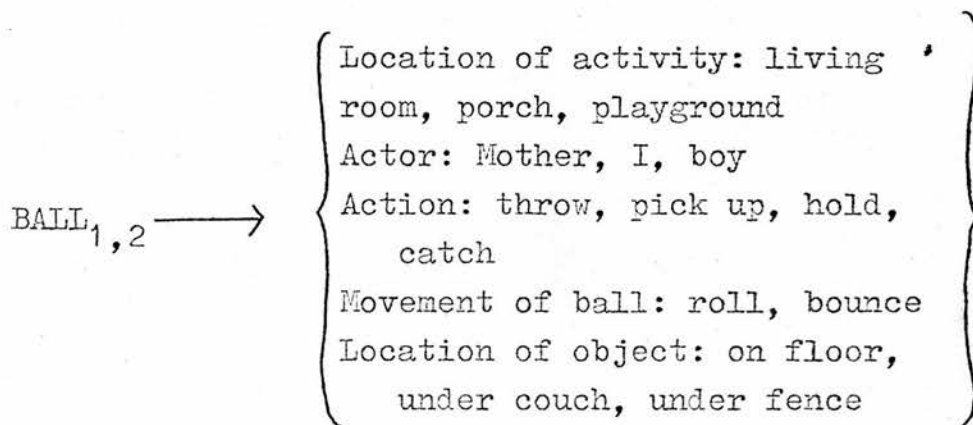


Figure 23 (from Nelson (1974, 277)).

We approach the crucial part of the argument. The next

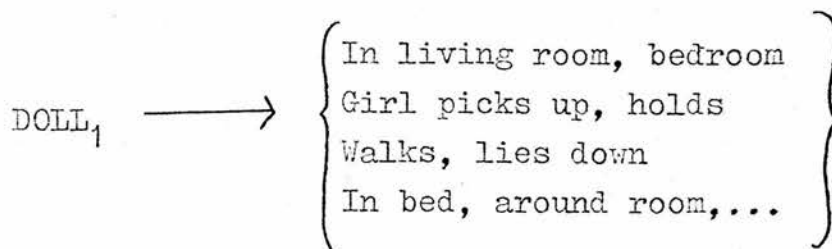
stage involves an amalgamation of the two results of functional synthesis and, in the respect, Nelson suggests (277 - 8):

Certain functions here (in BALL₂ - RMA) are the same as those for BALL₁: throwing rolling and bouncing; although the relations of location and actor are different. Boy stands in the same relation to the functions of ball as do Mother and I. Applying labels to these relationships yields the following scheme:

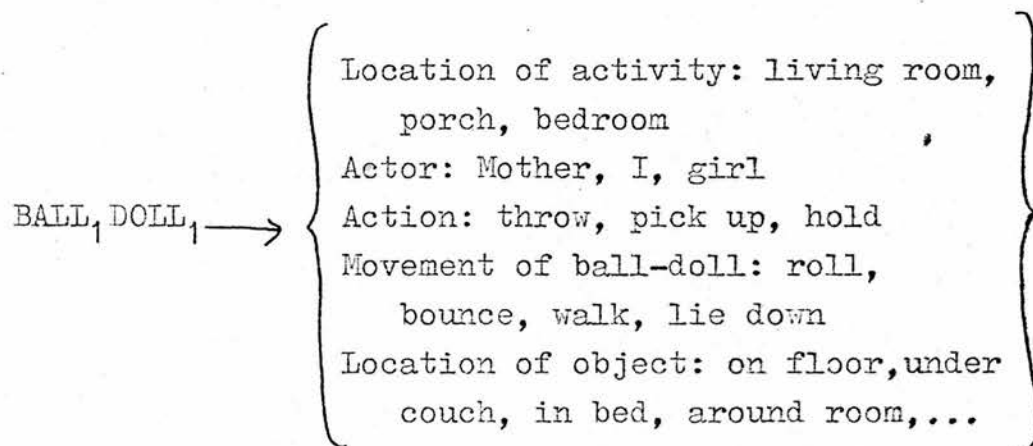


Some relations will eventually be identified as irrelevant to the defining functional core, for example, location of activity. The child must learn, therefore, which relations are concept defining and which are not. For some concepts the child may retain relations that the adult regards as superfluous...

But from this it is clear that Nelson is subject to exactly the same criticisms as she herself has levelled against the abstraction theorist because she has provided no clear criteria for why the functional syntheses, BALL₁ and BALL₂, should be regarded as instances of the same concept. To highlight the problem we can consider another hypothetical functional synthesis as in Figure 24.

Figure 24

There is no mechanism within Nelson's approach to prevent the formation of the concept schematised in Figure 25.

Figure 25

The reason why this is so is evident : "certain functions (in DOLL₁ - RMA) are the same as those for BALL₁" and, apart from a number of programmatic remarks, Nelson has said nothing about the identity of those functions. The way out of the dilemma is, of course, perfectly clear. What Nelson has to do is establish a criterion for matching instances of functional syntheses and, presumably, she would wish this criterion to take account of certain aspects of the synthesis at the expense of others. But this was exactly the way out for the abstraction theory according to the second assumption and, logically, there doesn't appear to be any difference between Clark's proposals and Nelson's in this respect. Clark's theory

is abstractionist but includes the assumption that the abstraction takes place along perceptual dimensions: Nelson's theory is abstractionist and includes the assumption that the abstraction takes place along certain dimensions specified in the functional syntheses. Such a comparison, of course, assumes that Clark's model is being interpreted as a model of concept development (see above for discussion).

Having got this much clear we can go on to consider how Nelson construes subsequent conceptual development and, more particularly, how language comes to be related to the developing concepts. She recognises that the functional core concept will not serve the child to identify all instances of the concept. This is because it is necessary to identify, say, a ball as a ball even when it is not partaking of the functional relationships which make up the core concept. Nelson says (278):

In order to do this, he needs to analyze the whole (object) into its relevant parts (attributes). It is assumed that this process begins to take place any time a concept is formed. Thus, although it is secondary, it is not discontinuous with the primary formation process. For this purpose, the child may pick out one or two salient static perceptual attributes and rely upon them... (first emphasis in original, second emphasis mine - RMA)⁹¹

What we seem to have here is an intolerable weakening of

91. Note that there is a certain tension between this claim and the implied temporal sequence pointed to in the passage cited above (p 135).

the functional core concept hypothesis to a point where, apart from a slant towards functional and relational attributes, it becomes indistinguishable from an unmodified abstraction theory. To see this we can consider another hypothetical development of the concept, 'ball' beginning as in Figure 26.

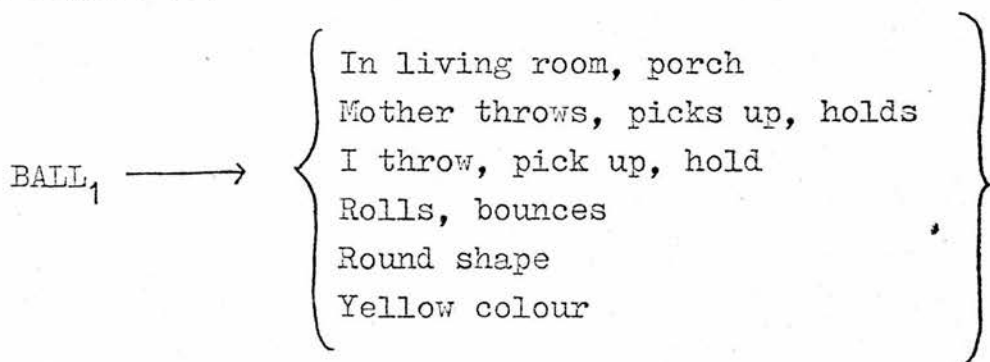


Figure 26

It is permitted, within Nelson's approach, that the perceptual features which are the last two entries in this schema may be stored with the concept even at this stage. She says (278):

Initially, when the concept consists of only one member a number of object-identifying attributes that will prove to be irrelevant may be stored with the concept.

Obviously, if irrelevant object-identifying attributes may be stored, then relevant attributes may also be stored. The next phase of the development may be another meeting with a ball leading to a second functional synthesis as in Figure 27.

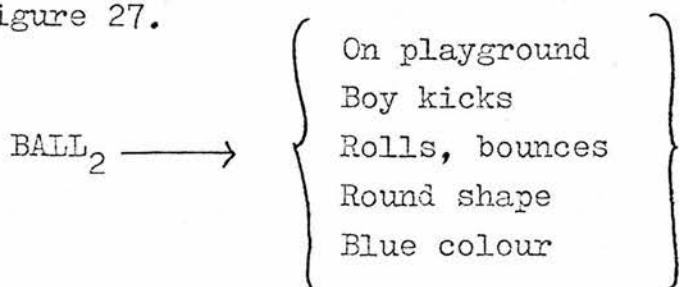


Figure 27

At this stage we might get the disjunctive amalgamation which Nelson seems to envisage at the beginnings of development (cf, her version of BALL_{1,2}, p138) but, on the assumption that, at this point, the child begins to discard irrelevant information from the concept, as he must at some stage, and given that the possibility has now been raised that he must develop a set of object-identifying attributes, there seems nothing to prevent the child relying on the shape of the ball and its facility for rolling and bouncing for identifying instances of the concept from here on, i.e., the child abstracts the concept schematised in Figure 28.

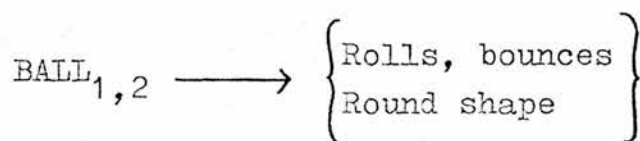


Figure 28

In order to prevent this collapse into an unmodified abstraction theory it is clear that Nelson must be far more precise in stating the form and role of the functional and relational properties which can enter into the functional core concept. This will become crucial when we consider the explanatory adequacy of the proposals in the domain of lexical development.

Turning to this domain, Nelson claims that the name of an object may, in certain cases, be attached to the structure corresponding to a concept. In cases where this is not so we shall find instances of the child inventing forms in an attempt to communicate about his concepts for which either the adult language does not

provide an appropriate expression or the child has not been acquainted with the adult expression. Again, it seems that what we have is an associative view of the lexicon where a lexical form is associated with a concept which corresponds to its meaning and which has the sort of structure of the functional core concepts we have been investigating. Provision is made, in the model, for the existence of concepts without attached lexical forms but not for lexical forms without attached concepts. Before turning to the explanatory adequacy of the proposals we can briefly summarise the virtues Nelson identifies in her approach in terms of the phenomena she sees as central (pp 129 - 130).

1. The child's selectivity with regard to his first lexical items is explained as the child can only use lexical items which correspond to concepts and his first concepts are the functional core concepts which have, as instances, objects which enter into dynamic relations with the child and others. Evidence on this point is presented in a convincing fashion in Nelson (1973a) where a survey of the first 50 words appearing in the vocabularies of a number of children showed up the stated properties.

2. The child's invention of lexical forms is explained by his having available some concept which does not correspond to a concept which is lexicalised in the adult language or some concept, the lexicalisation of which the child is not familiar with. Nelson presents no new

evidence to this point and, as pointed out above, existing evidence is equivocal.

3. Generalisation to further instances is explained by the functional core concept not restricting the application of a term to a single instance but to anything which satisfies the properties contained in the concept. This leaves open the possibility that there will be overextensions, that these overextensions will be on a functional basis at the beginnings of language development and that they may shift to a static perceptual basis as development proceeds. On this question of the basis of the overextensions one obviously has to be cautious given the remarks above and the fuller discussion below. The model also leaves open the possibility of over-restriction in the use of lexical items but here too it is difficult to make a substantive point as it is obvious, from Nelson's examples, that she countenances disjunctive conditions in concepts and she says nothing about the sort of mechanism which would lead the child to drop a complicated (and, perhaps, becoming more complicated) disjunctive condition. For evidence on this point Nelson refers to some of her own work again, notably Nelson (1973b), where she shows that small children's comprehension of ball was affected by the dynamic relationships which the children had been allowed to enter into with a set of more or less ball-like objects. Some of Clark's overextensions are also cited in this connection, in particular, those which are based on movement or sound

which Nelson assimilates to her idea of functional and dynamic relations. In opposition to this it is appropriate to refer to recent work of Bowerman (1975, 1978) where it is claimed that the majority of overextensions she could identify in the speech of her two daughters were based on perceptual properties of objects rather than on function. The extent to which such evidence is damaging to Nelson's hypothesis will become clear in the next section.

2.3.2 'Functional core concepts' as an explanatory theory

We consider now details of Nelson's model against Conditions 1 - 4 of Chapter 1. In order to have a reasonably directed discussion it will be necessary to make a number of assumptions at critical points not all of which may be true to Nelson's inclinations. It is my hope that making these assumptions explicit will prevent misunderstanding and it is my contention that, without such assumptions, the theory cannot be intelligibly evaluated.

First, as far as fixing the domain of enquiry, D, is concerned, it is apparent that Nelson's interests extend to the production of a theory of the development of concepts. However, as we have seen, these concepts are intimately related to lexical items entering the child's vocabulary and it seems reasonable to say that Nelson's linguistic interests can be identified, like Clark's, as

the development of the lexicon.⁹² The behaviour relevant to D is varied but consists mainly of the child's spontaneous productions.

Given this, Nelson's theory can be cast in essentially the same form as Clark's. At a particular time, t_1 , the child's lexicon (restricting ourselves to items which are used referentially) will consist of a set of lexical forms each associated with a concept which gives the meaning of the word,⁹³ i.e., we have the sort of structure we can schematise as in Figure 29.

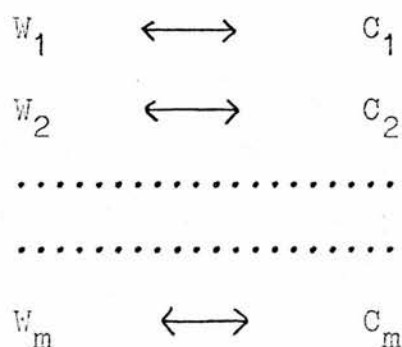


Figure 29

At a later time, t_2 , the child's lexicon will have increased in size (ignoring lexical mortality) along

92. To be fair to Nelson she does suggest that her views on lexical development can be seen as having implications for the development of syntax and, arguably, the domain of the enquiry can be extended in that direction. These are not ideas which I shall pursue here.

93. As Nelson has it the word form becomes part of the concept and it is unclear that she accepts the notion of a separate lexicon. Nothing in what follows depends upon the formulation adopted in the text.

with the associated set of concepts as in Figure 30.

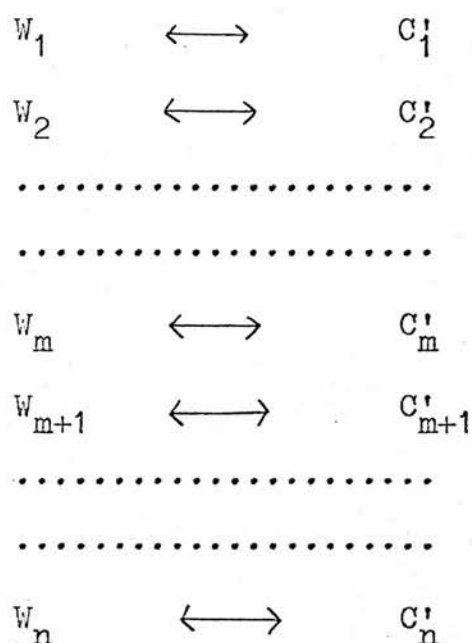


Figure 30

Here the C'_i ($1 \leq i \leq m$) may be identical to the C_i ($1 \leq i \leq m$) in Figure 29 or, if there has been a change in the meaning of W_i between t_1 and t_2 they will be different. Note that the assumption is not that the concepts C_i ($m+1 \leq i \leq n$) enter the child's conceptual system for the first time between t_1 and t_2 but only that they have lexical items attached to them for the first time in this period. Eschewing more detailed analysis for the moment, consider the satisfaction of Conditions 1 - 4.

The proposal fares no better and no worse than Clark's as far as Condition 1 is concerned and, in accordance with developing habit, I shall assume that it can be given the benefit of the doubt.

Condition 2 immediately raises problems for the theory. Obviously, at a suitably abstract level, two theories, T_1 and T_2 , constructed according to the above outline,

will have general principles in common. Lexical forms are associated with concepts giving their meanings, the manner of association is consistent from one theory to the next and it is intended that the available concepts be specified by some general theory. It is with respect to this last claim that the most pertinent questions arise, however. What is the exact nature of the general theory which specifies the notion, 'possible concept', in the acquisition model? It might be thought that an adequate formulation resides in Nelson's discussion of 'functional core concept' but a close examination of Nelson's paper reveals that no clear definition of this crucial notion emerges. We are told (277):

...in order to form a concept of the ball or the "idea of ballness" rather than ball as many different objects in different relationships, the child must synthesise over time the various relations into which the ball enters. This functional synthesis is the core of the child's concept.

and further (ibid):

Once functional synthesis has taken place with regard to an object, other objects may acquire status within the same functional synthesis or concept.

The functional core concept referred to in the first of these passages is the analysis of BALL₁ we are already familiar with from Figure 22. This contains a combination of locational, relational and predicative information about the ball in question but nowhere is there any attempt to accurately delimit the information which might

figure in the functional core. Reference to "various relations" is hopelessly vague. As far as the second passage is concerned this leads into Nelson's analysis of $BALL_2$ and $BALL_{1,2}$ (cf. pp 137 - 8 above) but $BALL_2$, apart from displaying a rather sloppy structural similarity to $BALL_1$, hardly seems to require reference to the "same functional synthesis or concept" (my emphasis - RMA).

When this sort of looseness is met it does seem to make sense to ask for conditions of identity on functional cores. The upshot of this discussion, and, of course, it would be possible to raise further questions concerning the occurrence of 'non-functional' information as discussed in the previous section, is that Nelson appears to have little clear idea as to what a general theory of functional core concepts would look like.⁹⁴ In the absence of such a theory Condition 2 can only be met in a discursive and informal manner, i.e., we can examine purported instances of concepts and ask ourselves whether they 'feel' right given the intuitions we are working from but these

94. In addition, as discussed in 2.3.1, at some point the concept may (?must) change its status and manipulate object-identifying attributes. This could be seen as involving a discontinuity although it is difficult to see Nelson as making such a precise claim. Interestingly, she is one worker in child language who explicitly makes an assumption of continuity when she says (1973a,2): "There is a basic continuity in developmental processes and structures. The same types of structures and processes are utilized throughout development, and changes in them are gradual and continuous."

intuitions are too poorly formulated to give us the answer by themselves. Nevertheless, for the sake of further discussion, as with Clark's theory, I shall assume that Condition 2 can be met and move on to Conditions 3 and 4.

To begin the discussion of Condition 3 I shall make the assumption that the information which can appear in a concept can, with the exception of the information giving the name of an instance of the concept, be partitioned into sets corresponding to relational and dynamic information on the one hand and static information on the other, i.e. I assume that we are equipped with a procedure for assigning information unambiguously to one of these categories. Looking at the schematisations on pp 146 - 7 we can see that from t_1 to t_2 there is an increase in the number of lexical forms and in the number of associated concepts and, at this level, we would appear to have a straightforward case of additive complexity and satisfaction of Condition 3. However, this is not the interesting level at which Condition 3 operates and, taking account of the internal structure of the concepts, it seems that there may be a more detailed analysis available. What Nelson appears to be saying, in a number of places, is that, as far as the development of concepts is concerned, it will be possible to find a t_1 and a t_2 such that the information represented in the child's concepts at t_1 is exclusively drawn from the set

corresponding to dynamic and relational information and at t_2 this information will not be so restricted, i.e. assuming that the concepts can be partitioned into a set, RD (of 'pure' relational/dynamic concepts), a set, NRD (of 'pure' non-relational/non-dynamic concepts) and a set, M (of 'mixed' concepts) we have the development within the set of concepts which we might schematise as $RD \rightarrow RD + M$. Unfortunately, as should be clear from the previous discussion, it is far from obvious that Nelson is making a claim as specific as this. Sometimes she appears to suggest that there is a fixed order in the development of concepts of the form:

1. Use relational and dynamic information only in concepts
2. Supplement relational and dynamic information with static information
3. Attach names to concepts,

as when, regarding a slightly more elaborate ordering, the spirit of which is nonetheless the same as the above, she says (276):

The order in which these processes are listed implies a usual temporal or sequential order for initial concept formation and naming. (my emphasis - RMA)

But if this is so, switching our attention away from concept development to the development of the lexicon, the emphasis on relational and dynamic information is of no particular interest as, by the time lexical development gets started, the relational and dynamic information

in the concept is already supplemented to an unknown degree by static (presumably, including perceptual) information. Unfortunately, an even more confusing picture can be deduced from the following statement (284):

It is important to bear in mind that this process is not proposed as a stage theory of development. Although language development may depend on the acquisition and elaboration of concepts ... there is no "functional stage" or "attributive stage". Rather all concept acquisition is assumed to involve both of these processes, whether the concepts are formed in infancy or adulthood.

One way in which we can reconcile the two views embodied in these passages is to say that the first one concerns the development of a particular concept whereas the second has, as its subject matter, concept development considered as a whole. Thus it is consistent to claim that, in the ontogeny of every concept, functional criteria for application to an instance precede attributive criteria while maintaining that the child uses functional and attributive criteria simultaneously over the whole set of concepts so that there would be no functional stage preceding an attributive stage.⁹⁵ This interpretation requires a revision of the schematisation of Nelson's theory as

95. I am by no means convinced that Nelson has this interpretation in mind, particularly in the light of the discussion in 2.3.1 where it was shown that, even in the development of a particular concept, it was possible for non-relational and non-dynamic information to play a criterial role right from the start. It should therefore be regarded as one of the assumptions referred to at the start of this section.

presented in Figures 29 and 30 with regard to which we shall be able to reconsider Condition 3.

What is demanded, according to this assumption and extending it into the domain of lexical development, is that we should be able to find times, t_1 and t_2 , such that there are forms in the child's lexicon at t_1 which are associated with members of RD and the same forms at t_2 are associated with members of M. It should not be possible to find times, t'_1 and t'_2 , such that there are words in the child's lexicon at t'_1 associated with concepts from the set, 'NRD', while at t'_2 the same words are associated with M U RD. If this view is correct what we have is a 'spreading' of non-relational and non-dynamic information from the set of concepts at t_1 to the set of concepts at t_2 in such a way that information of this type plays a wider and more systematic role in the structure of the lexicon at t_2 than it did at t_1 . Of course, it may be the case that new non-relational and non-dynamic information is utilised in the transition from t_1 to t_2 , and, if this were always true, we would have a clear case of satisfaction of Condition 3. As it is, however, there is nothing in Nelson's discussion which indicates that it is true and, therefore, we must conclude that there is no reason to expect Condition 3 to be satisfied. Nor, of course, is it the case that Condition 3 is not applicable as no new theoretical constructs are introduced in the second theory at the expense of ones appearing in the first theory. Quite simply, it looks as if Condition 3 should be applicable

but, because of the inherent vagueness of the theoretical framework (stemming directly from failure to satisfy Condition 2) we are in a position of not being able to apply it.

It follows from this that Condition 4 cannot be addressed in a systematic fashion for the theory as a whole. We could address it if Condition 3 were not applicable or if it could be applied with a positive outcome but neither of these is the case. It would do less than justice to Nelson, however, to leave off discussion at this point and, although the questions raised by Condition 4 cannot be asked of the theory as a whole, we can consider restricting the domain of the theory in such a way as to make Condition 4 applicable. This move involves restricting the domain of the theory to the development of the lexical representation of a single item, say, *W*. With respect to this domain, given the assumption that functional criteria are always used before attributive criteria, Condition 3 could well be satisfied and lead to the formulation of the following question in connection with Condition 4: why do we find the development we can schematise as $RD \rightarrow RD + NRD$ rather than the development schematised as $NRD \rightarrow RD + NRD$ with respect to the development of the lexical representation of *W*?

There is no answer to questions of this type in the logical structure of the theory, i.e. there is nothing about the information, 'I throw ball', which makes it

necessary that it should play a role in concept formation and in lexical representation before the static information, 'Round shape'. Thus we can discount the first possibility in Condition 4 as providing us with a grounding for the development.

Similarly there doesn't seem to be any reason to entertain the third possibility seriously. As already mentioned in connection with Clark's work, there simply hasn't been enough work done on the lexical structure of the world's languages for us to be able to decide whether there is a tendency for languages to use relational and dynamic 'features' in the semantic representations of lexical items rather than static ones. There is certainly no a priori reason for believing that such a tendency would be the outcome of an investigation conducted in the appropriate terms and, following from the vagueness of Nelson's proposals, it is unclear how such an investigation would proceed anyway.

The second possibility remains the only candidate and it is reasonably clear from Nelson's writings that she has something like this in mind. Is there, then, a reason existing in a theory regarded as more basic than the theory of the development of the representation of W for why the development of the meaning of W should proceed

in the way it does?⁹⁶ Nelson feels that there is and that this reason resides in Piaget's account of cognitive development. She says (279) :

It (the account she puts forward - RMA) is...in accord with what is known of the development of cognitive structures in infancy, for example, Piaget's account, although much remains to be discovered about specific cognitive constraints and structures of the pre-language and beginning language periods. (my emphasis-RMA)

Certainly the emphasis on action in Nelson's theorising is consistent with Piagetian slant but this, at best, is a vague and general statement of affiliation. More worrying, however, is the fact that aspects of Nelson's theorising can be seen as inconsistent with the Piagetian view on the cognitive structures of children of the relevant age. The argument is simple. Most small children begin to use words and generalise their use early in the second year. According to Nelson's scheme this assumes that the child is acquainted with a concept to which the word is related. The Piagetian period of

96. It is worth reiterating here that even this question doesn't make a lot of sense within Nelson's system as, strictly speaking, given the facts about ordering discussed above, pp151 - 2, her theory makes no predictions about the order of acquisition of meanings but only about the order of acquisition of concepts, acquisition of which can be complete before the concepts enter the lexicon as meanings. In the text I am assuming that the lexical development mirrors the conceptual development which is necessary to make the theory linguistically significant.

sensori-motor intelligence is usually taken to extend up to about 18 months (Flavell (1963)) and the output of this period is usually taken to include a mature object concept, a concept of location, a concept of causality, etc. Before this period draws to a close the child is assumed not to control mature versions of these concepts. However, Nelson's functional core concepts seem to assume an acquaintance with these concepts from the end of the first year. She herself says (277):

The concept (BALL₁ - RMA) depends upon a prior notion of the boundaries of objects, events and their relationships. The ball is not confused with self, floor, mother or play-pen, nor is it seen as an unbounded collection of attributes; identity as whole object has already been conferred upon it.

One can quibble with the interpretation of Piaget's 'mature object concept' - perhaps Nelson's position does not demand acquaintance with this concept but with one of the more primitive versions which the child experiences during the first eighteen months of his life - but the first impression is one of contradiction. Thus, in a situation in which Nelson turns to Piaget for support, that support is not transparently forthcoming.

To sum up, it seems to me that, as a theory of lexical development, the functional core concept hypothesis is extremely weak. Because of a failure to specify precisely the manner of interaction between the functional and static features which can enter into the concepts we end up with a theory which makes no predictions in the domain

of lexical development.⁹⁷ Once concepts enter the lexicon and become meanings they can be specified in any of a number of ways and the emphasis on relational and dynamic information becomes quite gratuitous. As a theory of concept development, the proposal is also weak because of a failure to provide any general definition of the central theoretical notion, the functional core concept. I would suggest further that the attention I have given to the conditions of Chapter 1 has helped to clarify these conclusions and that this should count as vindication of these conditions.

2.4 'Prototypes' and the development of colour terms

The work to be discussed in this section is, with the exception of 2.4.4, considerably more limited in scope than that of the previous sections of this chapter, being concerned with only a small area of lexical development. In addition, no fully-fledged developmental model exists which embraces the assumptions we shall examine and so much of the discussion will be speculative and not tied to actual proposals. I feel that this is justified because a considerable body of opinion is moving away from the

97. It should be evident at this point why the evidence of Bowerman (1975, 1978) should not be damaging for Nelson's position. Presumably, the only prediction she could make is that, if a lexical item, W, is overextended at some points in its history on both functional and static criteria, then the functional overextensions will occur first. So far as I know there is no evidence bearing on this issue.

view of word-meanings as bundles of features and towards the view that a representation of a prototype from the extension of a word has a central role to play in the specification of what the word means (see fn 38 for references) and so it will be useful, even at this preliminary stage, to get some idea of the problems arising with such a view. Heider (1971a) owes its inspiration, as does a great deal of other work by the same author (Heider (1972), Heider and Oliver (1972), Rosch (1973a, 1973b, 1975b)), to the cross-cultural work of Berlin and Kay (1969). Therefore, in 2.4.1 I shall present a brief summary of their findings. In 2.4.2 I shall describe the experiments of Heider (1971a) and in 2.4.3 discuss the explanatory status of a theory which would be consistent with the findings of those experiments. 2.4.4 will briefly discuss the views of Griffiths (1976) that this sort of approach can be extended beyond colour terms to general vocabulary.

2.4.1 Basic colour terms and focal instances

A common assumption among linguists and anthropologists has been that the colour space represents an ideal domain for demonstrating that the way in which language structures reality is essentially arbitrary (see Lyons (1968, 1977b) for this notion of 'arbitrariness' and linguistic relativism). Berlin and Kay (1969) claimed that this assumption was the result of a mistaken emphasis in research whereby an investigator would focus attention on the boundaries of colour terms. Having access to

native speakers of a number of languages, they were able to show that, if a subject were instructed to indicate on a colour chart the boundaries of a basic colour term (see below for this notion), then

- (i) there was a good deal of variability within speakers from one testing session to the next.
- (ii) there was a great deal of variability across speakers of the same language.
- (iii) There was no clear pattern of colour boundaries when data from more than one language were compared, a finding consistent with a hypothesis stressing arbitrariness.

If, on the other hand, the instruction to the subject was that he should indicate, on the colour chart, the best example of a colour term, then the authors found that

- (i) there was a great deal of consistency within speakers from one testing session to the next.
- (ii) there was a great deal of agreement across speakers of the same language.
- (iii) most importantly, there were significant clusterings of best examples of colour terms when different languages were compared.

This means that one can justify talking about a colour term, RED, existing in other languages so long as it is clear that RED corresponds to that colour term in the language in question which has, as its best example, the best example of the English colour term, red. One is not thereby committed to identity between the extension

of that colour term and the English term, red.

It was a further claim of Berlin and Kay's study that, by establishing, what they considered, a plausible set of criteria, it was possible to construct a 'filter' into which the total colour vocabulary of a language could be fed and the output of which would be the set of basic colour terms in that language, a justification for this being that it would get rid of individual differences in colour vocabularies which might be contingent on employment, interests, etc. This 'filter' includes such conditions as:

A basic colour term shall be morphologically simple

A basic colour term shall not be hyponym of any other colour term

A basic colour term shall be 'psychologically salient' for speakers of the language

and various others. When applied to English this 'filter' yields a set of 11 basic colour terms: black, white, red, yellow, green, blue, brown, pink, orange, grey and purple. The important point now is that these 11 basic colour terms (with one or two noted exceptions) exhaust the set of basic colour terms in the languages studied by Berlin and Kay, i.e. any language in the sample drew its basic colour terms from the set, BLACK, WHITE, RED, YELLOW, GREEN, BLUE, BROWN, PINK, ORANGE, GREY, PURPLE. Furthermore, Berlin and Kay argue, languages do not draw their colour terms from this set in a completely random manner. Rather, there is a partial ordering defined on the set

of terms which can be represented as in Figure 31.

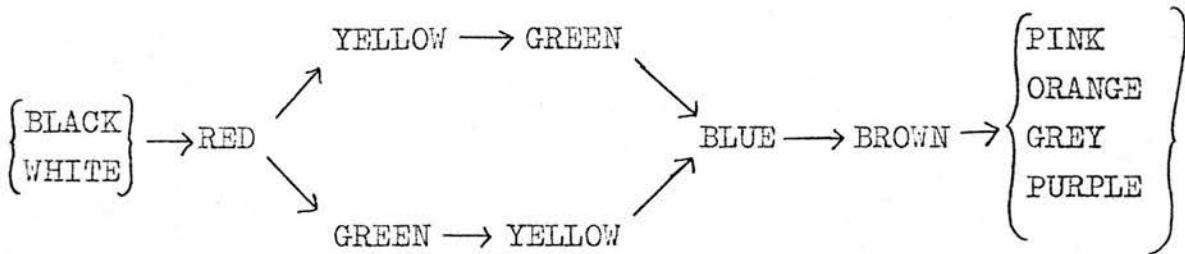


Figure 31

From this partial ordering it is easy to construct a set of propositions which express universals and which summarise Berlin and Kay's findings. There is one absolute universal:

All languages possess basic colour terms, BLACK and WHITE and a set of implicational universals including:

If a language has a basic colour term RED, then it also has basic colour terms, BLACK and WHITE, although not necessarily vice versa.

If a language possesses a basic colour term, BLUE, then it also has basic colour terms, BLACK, WHITE, RED, YELLOW and GREEN, although not necessarily vice versa.

and it is straightforward to construct the remaining propositions.⁹⁸

98. Before leaving this uncritical presentation of Berlin and Kay's findings it is important to note that their work has been made the subject of a good deal of discussion casting doubts on both the substance of their findings and on the methodology they adopted in producing them. See, for example, Hickerson (1971), Durbin (1972), McNeill (1972), Collier (1973). Collier et al. (1976) takes account of some of the methodological problems and substantially supports the original conclusions while Kay (1975) takes account of some of the substantive objections and attempts to integrate them into a modified partial ordering. Since Heider's work on the development of colour terms refers to the original hypothesis and since we are interested in the use she put that hypothesis to rather than in whether it is correct, I shall pay no attention to these modifications in what follows.

2.4.2 Heider's experiments

Heider (1971a) conducted three experiments in her exploration of the implications of Berlin and Kay's work for cognitive and lexical development. The first of these involved the child (from the age range, 2 years, 11 months to 3 years, 10 months) in choosing, from a small array of colours, one to show to the experimenter who has her eyes covered. The arrays were constructed so that each one contained a single focal chromatic colour which was embedded in a set of colours which differed from the focal colour either in brightness or in saturation. Such arrays were constructed for the eight chromatic basic colour terms, red, yellow, green, blue, brown, pink, orange and purple. For the second experiment children (from the age range, 3 years, 11 months to 4 years 10 months) were asked to match a coloured chip with one from a set of chips in an array constructed in such a way that the original chip appears only once in it. The arrays again included one chromatic focal colour and the chips in which this was embedded varied along either the dimension of brightness or the dimension of hue. The chip with which the child was presented could be focal, boundary (falling on the edge of those areas in the colour space which were innominate in the Berlin and Kay study) and internominal (falling in the centre of an innominate area). Matching accuracy, is of course, the variable in which the experimenter is interested. Neither of these experiments explicitly involves the child's lexical knowledge but in the third study the

child's task (children ranging in age from 3 years to 4 years 7 months) was to choose from an array of coloured chips, varying in hue and containing a chip corresponding to a focal instance of the chromatic basic colour term, X, in response to the question, 'Which is the X one?' or the instruction, 'Show me the X one'. As far as adult usage was concerned, there were a number of chips in each array which could be appropriately referred to as 'X'.

The results of the series of experiments were quite clear-cut. In the first experiment children showed a significant tendency to choose focal chips, a typical result being that, when 3 out of 24 children might have been expected to choose focal blue by chance (there were 8 chips in each array), in fact 9 children did so. In the second experiment children were much more accurate in matching focal colours than either boundary or inter-nominal colours, there being no significant difference between this latter pair and, in the final experiment, many more children chose the focal example of a colour term, X, in response to 'Show me the X one' than would have been expected by chance given that there was more than one adult-correct response in these cases. As a typical example, 5 chips in the array containing focal green were judged to be green by adults and, of 27 children, 22 chose one of these thus exhibiting behaviour which is consistent with them knowing something about the meaning of green. From these 22 children we would expect 4.40 to choose the focal colour by chance and, in fact,

12 children chose this chip. This pattern was repeated throughout the eight chromatic colour terms. What this research appears to show is that focal colours tend to control a child's attention (first experiment), are easier to match (second experiment) and that, once the children are familiar with a colour term, this colour term is attached to a focal instance of the colour rather than having an unstructured extension over the whole range covered by the colour term in the adult language. Heider is at pains to argue that it is the perceptual salience demonstrated in the first two experiments which accounts for the phenomenon uncovered in the third experiment, although she admits that, given lack of knowledge concerning the child's previous experience with colour terms, parental tuition, etc., it is impossible to isolate perceptual saliency as the causal factor.⁹⁹

2.4.3 A tentative model and its status

Whatever the explanation for the development of what appear to be structured colour categories (see Rosch (1973a, 1973b) for her work with the Dani of New Guinea and stronger arguments for perceptual saliency being the causal factor), the above experiments appear to be consistent with the following view of the development

99. Interestingly, even if it were the case that parents in teaching colour terms to their children, tended to use objects exhibiting focal colours, this itself would be in need of explanation.

of that part of the lexicon devoted to colour terms while, for the reasons pointed out above, they do not demand this view. Initially the child attaches to each colour term that he learns a representation of a focal instance of that colour term (perhaps in the form of a visual image) which, to all intents and purposes, counts as the meaning of the colour term at that stage. Subsequently he develops additional representations which enable him to apply the colour term beyond focal instances and to approximate to the poorly defined boundaries of adult usage. During this subsequent development he retains his representation of the focal instance in the form of a prototype which, while no longer determining the extension of the colour term, functions as a cognitive reference point (see Rosch (1975c) for an explication of this notion).¹⁰⁰ This is a development which, with respect to a particular colour term, could be schematised as:

$$CT \longleftrightarrow X \longrightarrow CT \longleftrightarrow X+Y$$

where CT is the colour term in question, X is a representation of a focal instance of the colour term and Y represents whatever additional machinery is necessary to account for the subsequent extension of CT. Looking at

100. Evidence for the child going through a stage where the 'prototype' determines the extension of the colour term is not obviously available. The fact that the children, in Heider's third experiment, responded at all can be seen as providing a measure of support since, presumably, adults in such a situation would be likely to respond with 'Which X one?'

the relevant section of the lexicon as a whole, we might expect, according to this view, that the theorist would be able to isolate two stages in the development of colour terms which we could represent as in Figure 32.

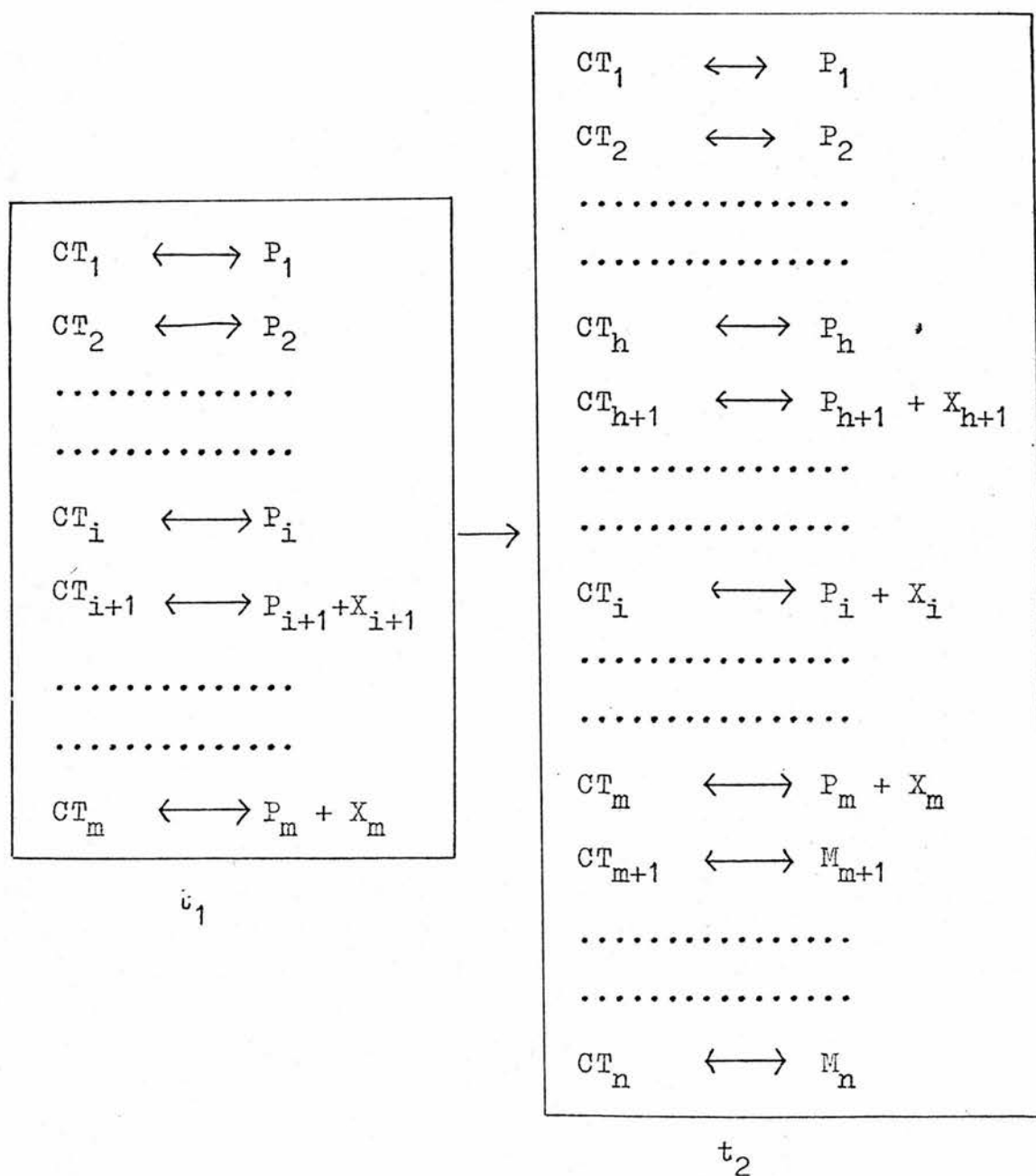


Figure 32

Here the CT_i , $1 \leq i \leq n$, are colour terms, the P_i , $1 \leq i \leq m$, are representations of focal instances or prototypes, the X_i , $h+1 \leq i \leq m$, are the 'additional machinery' and the

M_i , $m+1 \leq i \leq n$ may either be prototypes or representations of a combination of prototype and 'additional machinery'. This general formulation has it that the CT_j ($h+1 \leq j \leq i$) are 'extended' beyond the range defined by their prototypes between t_1 and t_2 and new colour terms, CT_{m+1} , ..., CT_n , enter the lexicon and, by t_2 , may either be extended or restricted to a prototype. With this in mind let us now consider Conditions 1 - 4.

The domain of enquiry, D , can be fixed as that part of the child's developing lexicon which deals with colour terms and the data relevant to an investigation of D will include the results of Heider's third experiment described above. It seems to me that, not only Condition 1, but also Condition 2 have to be passed by in silence on this occasion. The vagueness of the description of the general model above makes it only too obvious that I don't have a general theory in mind and, while even the nature of prototypes is obscure, the references to 'additional machinery' are a transparent cloak for ignorance.

Despite the inadequacy of the proposal with regard to Condition 2, it seems that interesting questions arise in connection with Condition 3. Between t_1 and t_2 in Figure 32 there is an increase in the set of colour terms the child has in his lexicon, there is an increase in the set of prototypes (each of CT_{m+1} , ..., CT_n involve a prototype which was not present in the lexicon at t_1)

and an increase in the set of 'extension-fixing mechanisms' (each of CT_{h+1}, \dots, CT_i involve one of these for the first time and, additionally, they may also appear in CT_{m+1}, \dots, C_n). On these grounds, I conclude that Condition 3 is applicable and that it is satisfied.¹⁰¹

The question raised by Condition 4 is clearly difficult to formulate of the theory as a whole and I would, therefore, like to consider two subsidiary questions which have their origins in this condition.

1. Restricting ourselves to a single lexical item in the set, CT_{h+1}, \dots, CT_i why is it that we get the development we can schematise as:

$$CT \leftrightarrow P \longrightarrow CT \leftrightarrow P+X$$

rather than the alternative:

$$CT \leftrightarrow X \longrightarrow CT \leftrightarrow P+X$$

2. Restricting ourselves to prototypes, why is it that we find the development we can schematise as:

$$\{P_1, P_2, \dots, P_m\} \longrightarrow \{P_1, P_2, \dots, P_m, P_{m+1}, \dots, P_n\}$$

rather than the alternative:

$$\{P_{m+1}, \dots, P_n\} \longrightarrow \{P_1, P_2, \dots, P_m, P_{m+1}, \dots, P_n\} \quad 102$$

101. Its applicability is, in fact, guaranteed by the claim that the prototype 'survives' into a stage when it no longer determines the colour term's extension.

102. For the sake of completeness we might also consider a similar question referring to 'extension fixing mechanisms' but I take this latter notion as too vague to warrant further discussion.

Consider the first of these questions. The first possibility of Condition 4 provides no clues as there is no obvious logical connection in the theory between the prototype and the 'extension fixing machinery'. Given the vagueness of the constructs this is hardly surprising. The third possibility, however, is more interesting. It follows from the work of Berlin and Kay that focal colours enjoy a status in the languages of the world which is not shared by whatever principles fix the boundaries of colour terms. It seems likely, although it is not obvious without access to a good deal of data, that, given the correctness of Berlin and Kay's views, it should be possible to formulate statements of the form:

If a colour term, X, in a language, L, has an approximate boundary, B, then that colour term will have, as its best example, the focal colour, F. That is to say, one ought to be able to argue from boundaries to focal instances but, of course, the reverse should not be the case. But the above statement is nothing less than an implicational universal which has the very form we need in order to ground the development under discussion linguistically.

Heider, in her attempt to relate the results of her third experiment to those of the first two, can be seen as resorting to the second possibility under Condition 4 and, thereby, providing an explanation for the behavioural facts and for the linguistic facts. It is difficult to put the argument in a form such that the conditions developed under this aspect of Condition 4 can be applied,

but, in outline, the proposal is clear enough. Just as in lexical structure focal colours enjoy a privileged status, so it is in visual perception, where Heider's experiments have demonstrated the extent to which they control the child's attention and the extent to which they facilitate the child's performance in a matching task. It seems likely then, that in a theory of perceptual development there will be constructs related to focal colours and, when such a theory exists, it may be possible to formulate the necessary correspondence rules in a satisfactory way. For the moment, though, such suggestions must remain speculation.¹⁰³

Consider now the second question concerning the development of the set of prototypes. Clearly the partial ordering argued for by Berlin and Kay can be invoked here to provide a linguistic grounding for lexical development. To do this depends on finding an order of acquisition of colour terms (each one initially attached to a focal instance)

103. Nor is it obvious that we would have to stop at this point, for there might be the equivalent of what Fodor (1968) has termed a 'phase two explanation' available by reference to the biological properties of the organism. The well-known work of deValois and Jacobs (1968) on the visual system of the macaque monkey has demonstrated that in a system very similar to that of man there are specialised cells for processing information concerning the wavelength of light and that these cells have peaks of sensitivity which might be related to the properties of at least the primary focal colours. This is, of course, some way from a developmental theory of colour vision.

which matches the partial ordering. Heider is conscious of the possibilities in this regard but points out that her study says nothing about the actual order in which lexical items are acquired. In connection with the interpretation of her third experiment, she says (452):

The hypothesis behind the third experiment was that colour names initially become attached (come to denote) focal areas for children. This hypothesis could not be tested directly with American children because their history of colour naming prior to coming to the experiment was unknown; that is, the kind of explicit teaching of colour names a subject had already received was unknown and the colour of objects that had been previously used as colour name exemplars for him was unknown.

This reasoning can be carried over directly to the order of acquisition of colour terms. Heider feels, however, that it might still turn out that the Berlin and Kay partial ordering will be matched by the performance of the children in the experiments she undertook, an outcome which, while not establishing that the partial ordering is reflected in order of acquisition, would, nevertheless, be consistent with such a claim and lend it plausibility. Unfortunately this is not what the author found. In her own words (454):

Neither the saliency order of the focal colours in Experiment I, the matching accuracy order obtained from Experiment II, nor the frequency with which focal colours were chosen to represent the category name in Experiment III matched Berlin and Kay's proposed evolutionary order. Only one measure of the present

study, the number of subjects who knew each colour name, did not, with the exception of orange, contradict the proposed evolutionary order.

Thus, all we can conclude in this respect is that evidence which it was hoped might be supportive is not forthcoming. It should be emphasised, though, that it is clear what the evidence at issue is and this should count to the positive evaluation of the theory.

By way of general conclusion it seems to me fair to say that Heider's proposals and the suggestions I have constructed on the basis of them, while not matching up to Conditions 1 - 4 at all well, deserve to be taken seriously because of the contact which is established with what is known about the world's languages and with aspects of visual perception. Unfortunately, with regard to this contact with the world's languages, while the available data are not, strictly speaking, relevant to the question of acquisition order, they do not give any reason to believe that this order will match the distributional facts from the world's languages.

2.4.4 'Prototypes' and general referential vocabulary

Griffiths (1976) is an ambitious attempt to establish the position that a theory of lexical development employing some notion of 'prototype' is of more value, when studying early vocabulary being used referentially, than is a theory using 'critical features' or 'critical attributes'. Most of the evidence he cites is not easily given a developmental interpretation, that is, the fact that it

might be possible to demonstrate that a child is making crucial use of a prototype at a certain stage does not tell us anything about the genesis of that prototype nor about its subsequent development. There is, however, one piece of evidence cited by Griffiths which indicates that he might be prepared to subscribe to the view, to all intents and purposes, analogous to that discussed in the previous section, that, when a lexical item is learned, there is a period where a prototype actually fixes the extension of that item. Subsequently additional devices, of an obscure nature, must be developed so that the extension of the lexical item for the child approximates to the adult norm. The evidence in question is that provided by Reich (1976) who claimed that his son's understanding of shoe, when he first showed any comprehension of the word, was restricted to it referring to a particular pair of shoes in a particular location. Of course, this is nothing more than an extreme case of overrestriction and, while it is difficult to demonstrate the existence of this phenomenon with certainty, it has achieved a degree of acceptance amongst people working on early vocabulary growth. Reich's conclusions, cited approvingly by Griffiths, are that (120):

...the very first word meanings are formed by associating a sequence of sounds with essentially everything that is perceptually and functionally salient about the objects or actions in the environment that co-occur with that word.

This would appear to be consistent with a generalised

model along the lines of Figure 32 where lexical items, upon entering the lexicon, are associated with prototypes and later these prototypes are supplemented with additional machinery for fixing extensions corresponding to those of the adult.

Such a proposal suffers from the same defects as the earlier one with respect to Conditions 1 and 2 although, again, Condition 3 will be satisfied. Extending the analogy we can ask at least two questions on the basis of considerations of Condition 4.

1. Restricting ourselves to single lexical items, why is it that we get the development we can schematise as:

$$W_i \leftrightarrow P \longrightarrow W_i \leftrightarrow P+X$$

rather than the alternative:

$$W_i \leftrightarrow X \longrightarrow W_i \leftrightarrow P+X$$

2. Restricting ourselves to prototypes, why is that we find the development we can schematise as:

$$\{P_1, P_2, \dots, P_n\} \longrightarrow \{P_1, P_2, \dots, P_m, P_{m+1}, \dots, P_n\}$$

rather than the alternative:

$$\{P_{m+1}, \dots, P_n\} \longrightarrow \{P_1, P_2, \dots, P_m, P_{m+1}, \dots, P_n\}$$

and of course, this second question, as in the case of colour terms, reduces to a question about the order of acquisition of lexical forms.

In 2.4.3 we saw that Heider had a number of bases for tackling the variant on the first question which was considered there. Unfortunately, in this case, these

bases are not available. That is to say that it is not the case, in general, that there are language-independent 'prototypes' which will, inevitably, form the cores of semantic categories. It is unrealistic to assume that the prototype of, say a dwelling-place, is the same for a British speaker of English as it is for a speaker of Kpelle or a speaker of Dani assuming, for linguistic relevance, each of these languages lexicalises this notion.¹⁰⁴ This indicates that prototypes need have no more universal status than have whatever principles fix extensions.. Nor is it the case that we can resort to a notion of 'psychological salience' in these cases. To this extent the general proposal lacks the explanatory status of the more restricted one from the previous section.

But now consider the variant of the second question. This is concerned with the order of acquisition of lexical items and depends, for its investigation, upon there being an attested order of acquisition, and, for its

104. Note, that this point does not cast any doubt on the validity of the notion of 'prototype'. I am merely trying to point out that particular prototypes probably do not have cross-cultural significance. Also, given the empirical work reported in Rosch (1973a, 1973b), it is clear that the set of prototypes which do have cross-cultural significance is not restricted to focal colours. Rosch comes to similar conclusions in the domain of simple geometrical shapes and there is no reason to believe that her findings could not be extended to other perceptual domains. Similarly, one cannot prejudge the issue to the extent that I do in the text, with regard to non-perceptual domains but our intuitions surely favour relativism here.

satisfactory answer, upon there being facts about the world's languages or facts about perception in which the order of acquisition can be grounded. In the previous section we had the linguistic facts which would have provided a satisfactory answer but, unfortunately, no order of acquisition to match them. We would appear to be in a worse position here but I believe that the beginnings of at least a partial answer to this question are apparent in the work of Rosch and her colleagues on 'basic objects' which is to be discussed in the next section. Holding this problem in mind, then, I shall immediately proceed to this discussion.

2.5 Basic objects

Rosch, Mervis, Gray, Johnson and Boyes-Braem (1976) argue that within a taxonomy of concepts or categories there may be one level which is privileged in a particularly interesting way. They say:

...categories within taxonomies of concrete objects are structured such that there is generally one level of abstraction at which the most basic category cuts can be made. In general, the basic level of abstraction in a taxonomy is the level at which categories carry the most information, possess the highest cue validity and are thus the most differentiated from one another (385).

In 2.5.1 I shall attempt to spell out what is involved in a category possessing 'high cue validity' and in 2.5.2 I shall consider the implications that Rosch et al. see, for lexical development, in these concepts.

with second level categories and that the same is true for second level attributes and third level attributes. Obviously such a taxonomy could be extended to any depth. To take a concrete example we can consider the (partial) taxonomy of Figure 34.

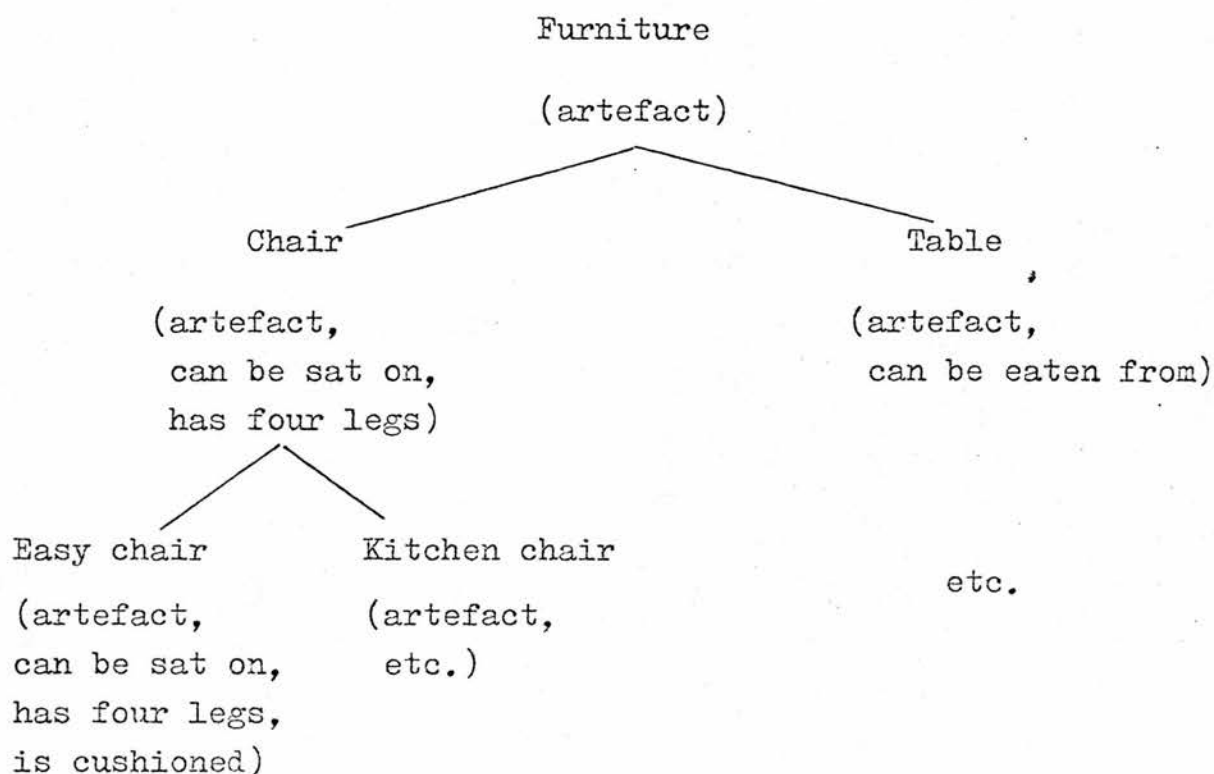


Figure 34¹⁰⁵

Note that the attributes in this taxonomy are not to be taken as providing sufficient (or even necessary) conditions for an instance belonging to a category. Rather they should be seen as referring to characteristics of objects which provide clues to category membership and, as such, they can be construed as providing a rough description of a typical member of a category. The extent to which a

105. This taxonomy is not intended to have any psychological validity. For properties associated with categories see the experiments in Rosch et al. (1976)

category is informative can be equated with its cue validity and I now turn to an explication of this latter notion.

For each of the attributes associated with a category we can ask about its reliability as a predictor of category membership. To this end, for each category and each characteristic attribute, we can compute the ratio of the probability that an instance belongs to the category given that it has the attribute, i.e., if we have a category, C_i , and an attribute, A_j , we can compute

$$\frac{\text{Pr}(C_i/A_j)}{1 - \text{Pr}(C_i/A_j)}$$

This figure can be referred to as the cue validity of A_j with respect to C_i . Now we can define the cue validity of C_i as

$$\frac{\sum_{j=1}^m \text{Pr}(C_i/A_j)}{\sum_{j=1}^m 1 - \text{Pr}(C_i/A_j)}$$

where A_1, \dots, A_m are the attributes associated with the category, C_i . This figure will be increased by the presence of attributes which are strongly associated with C_i and not with other categories. An increase in the number of attributes associated with C_i will not, in itself, necessarily lead to an increase in cue validity for the category as, for these attributes to be also associated with other categories, will increase the denominator in the above summation leading to an overall

decrease in cue validity. To illustrate with the simple example from Figure 34, assume that we have already computed $\text{Pr}(\text{furniture/artefact})$ as 0.1. Then the cue validity of the category, Furniture, will be

$$\frac{0.1}{0.9} = 0.11$$

Assume further that the categories, Chair and Table, exhaust the category, Furniture, that the two categories are equally distributed throughout the superordinate category and that we have the further figures available:

$$\text{Pr}(\text{chair/can be sat on}) = 0.5$$

$$\text{Pr}(\text{chair/has four legs}) = 0.4$$

$$\text{Pr}(\text{table/can be eaten from}) = 0.7$$

Then the cue validity of the category, Chair, can be computed as

$$\frac{0.05 + 0.5 + 0.4}{0.95 + 0.5 + 0.6} = \frac{0.95}{2.05} = 0.46$$

and the cue validity of the category, Table, comes out as

$$\frac{0.05 + 0.7}{0.95 + 0.3} = \frac{0.75}{1.25} = 0.6$$

So we see that, despite the fact that two additional attributes are associated with the category, Chair, as we move down the taxonomy whereas only one is associated with the category, Table, the cue validity of Chair is less than the cue validity of Table. This is because the cue associated with Table is a relatively reliable one. Consider further the cue validity of the category,

Easy chair, on the assumption that the taxonomy includes only Easy chair and Kitchen chair subordinate to Chair and that these two are equally distributed in the superordinate category. Assume that we know that $\text{Pr}(\text{easy chair/cushioned}) = 0.6$. The cue validity for this category can then be calculated as

$$\frac{0.025 + 0.25 + 0.2 + 0.6}{0.975 + 0.75 + 0.8 + 0.4} = \frac{1.075}{2.925} = 0.34$$

and this demonstrates that it is not necessarily the case that, as one moves down a taxonomy, the cue validities of the categories will increase.

It is clear from the above that two factors are at work in determining the cue validity of a category: the number of attributes associated with the category and the reliability of these attributes. Rosch et al. suggest that there is, conceptually, a level of 'basic objects' at which these factors combine to maximise cue validity. They say (385):

Suppose that basic objects (e.g., chair, car) are at the most inclusive level (in the taxonomy - RMA) at which there are attributes common to all or most members of the category. Then total cue validities are maximised at that level of abstraction at which basic objects are categorised. That is, categories one level more abstract will be superordinate categories (e.g., furniture, vehicle) whose members share only a few attributes among each other. Categories below the basic level will be subordinate categories (e.g., kitchen chair, sports car) which are also bundles of predicate attributes and functions, but contain many attributes which overlap with other

categories (for example, kitchen chair shares most of its attributes with other kinds of chairs).

Of course, in order to demonstrate that cue validity is maximised at this level it would be necessary to undertake much more detailed investigations than those found in Rosch et al's paper but, certainly, the results of their experimental studies lend credence to their position.

2.5.2 'Basic objects' in language acquisition

Rosch et al. (1976) studied the vocabulary of Sarah,¹ one of the children studied in great depth by Roger Brown and his team, and categorised her concrete nouns in Stage I using the notions of 'superordinate', 'basic' and 'subordinate'. The results were as shown in Figure 35 (p 184) and led Rosch et al. to conclude (425): "basic level names were essentially the only names used by Sarah in Stage I".

On the basis of this conclusion, then, we can schematise the lexical development of the child as:

$$BL \longrightarrow BL + X$$

where we are concerned simply with the child's vocabulary, rather than the meanings he attaches to lexical forms and where 'BL' denotes the class of basic-level names and 'X' covers superordinate and subordinate names.¹⁰⁶ Insofar

106. It is worth mentioning that Rosch et al. found that subjects were more capable of associating a mental image with basic level names than with names from other categories. Insofar as we can associate mental images with prototypes and prototypes with meanings at the beginnings of lexical development the relevance of this to the work of Griffiths discussed in 2.4.4 should be obvious.

CONCRETE NOUNS USED IN STAGE I OF LANGUAGE ACQUISITION

Category	Taxonomic level of word used					
	Superordinate		Basic level		Subordinate	
	Tokens	Types	Tokens	Types	Tokens	Types
Nonbiological						
Musical						
instrument	0	0	13	6	0	0
Fruit	0	0	7	3	0	0
Tool	0	0	37	13	0	0
Clothing	2	1	91	18	4	1
Furniture	0	0	75	16	1	1
Vehicle	0	0	50	11	3	2
Biological ¹⁰⁷						
Tree	0	0	0	0	0	0
Fish	19	1	0	0	0	0
Bird	13	1	0	0	0	0

Figure 35. From Rosch et al. (1976, 425).

107. The distinction between biological and nonbiological categories is motivated by the earlier experiments reported in Rosch et al. where it became obvious that adults treated the superordinates, tree, fish and bird as if they were basic level names with the result that the biological taxonomies are only of depth 2. Similarly the categories used in Table 35 are derivative on the experimental studies of Rosch et al.

as theoretical machinery is involved here it would appear that the schematisation satisfies Condition 3 (again I pass over Conditions 1 and 2) and Condition 4 causes us to search for reasons for why we find this development rather than the alternative which can be schematised as:

$$X \longrightarrow BL + X$$

and an answer to this question will constitute at least a partial answer to the question we raised at the end of 2.4.4.¹⁰⁸

Taking the first possibility under Condition 3, it is evident that there is nothing logically basic about basic-level names and the categories they are associated with. One could start at the top of a taxonomy and add the more specific parts or start at the bottom and add the more general parts and either of these processes is as intelligible as the one we are confronted with, starting in the middle and working both ways. The second possibility is the one on which Rosch et al. lean most heavily and the greater part of their paper can be seen as an attempt to establish the primacy of the cognitive categories corresponding to basic-level names. As far as the development of these categories is concerned, a series

108. It will not, of course, enable us to say anything about the order of acquisition of items within the three gross categories we are considering and it seems to me that, at that point, the child's individual experience will become of overriding importance overcoming the influence of any general theoretical considerations.

of experiments were conducted showing that children as young as 3 years were capable of sorting objects into basic-level categories while it has been known for some time that children of that age do not exhibit control of superordinate categories in sorting tasks. It seems, therefore, that there is an important parallel between cognitive and linguistic development with, on the one hand, basic-level concepts preceding superordinate concepts and, on the other, basic-level names preceding superordinate names. Unfortunately, for any attempt to ground the linguistic development in the cognitive development, the ages of the children involved create difficulties. Sarah, the slowest of the children studied by Brown in this respect, left Stage I at about 35 months (see Brown (1973, 80) for details) and this means that she was already displaying the appropriate linguistic behaviour before the youngest children in the Rosch et al. study were indicating that they controlled the relevant concepts in the sorting task. It is, of course, quite conceivable that children considerably younger than 3 years would be capable of evidencing the control of basic-level categories through some means other than their language but, for the moment, it is necessary to conclude that the reduction has not been entirely successful.¹⁰⁹ The

109. Note that there is no suggestion here that the child might use his lexical items appropriately before he controls the relevant concepts. The point is merely that if one is to exhibit a causal connection between cognitive and linguistic development then it is necessary to be able to identify the cognitive competence before the linguistic competence appears.

cognitive theory envisaged by Rosch et al. is itself crucially dependent on the principle that an organism will establish categories which are maximally informative in its environment and, while such a principle has fascinating implications, I shall regard discussion of it as beyond the scope of this work.

Interestingly there is one further avenue to explore in connection with this theoretical proposal and this involves reference to the third possibility under Condition 4. If there were languages which were impoverished as far as taxonomic depth is concerned and if, furthermore the levels in the taxonomy which were absent in such cases were never the basic levels then we would have a linguistic grounding for the lexical development. In the final study of their paper, Rosch et al. investigate American Sign Language with this point in mind and what they found was that it was deficient at the superordinate and subordinate levels to a much greater extent than it was at the basic level. American Sign Language could thus be cited as the first step in an argument towards a linguistic grounding for the proposals and it remains to be seen whether there are 'standard' languages which exhibit the same behaviour (see Berlin, Breedlove and Raven (1973) for some relevant observations).

'Basic objects' seems to me to an exciting and useful construct which will almost certainly lead to fresh insights into cognitive and linguistic growth. The difficulty at the moment, as with most of the proposals

in 2.4 is that of specifying the exact nature of the theory. Clark and Nelson put forward reasonably articulated theories although close examination demonstrated crucial inadequacies. It is more difficult with the proposals of Heider and Griffiths to pin-point inadequacies and, being optimistic, one might wish to suggest that this is because such gross inadequacies don't exist. At the same time I feel that the doubt must be expressed that our failure to pin-point them is a direct reflex of the fact that the exact nature of prototypes is never spelled out, that the way in which a semantic model using prototypes enables its wearer to successfully refer to an instance which differs from the prototype is not specified, and so on. This boils down to the different ways in which Condition 2 gets handled. For Clark and Nelson it made sense to consider Condition 2 because they were putting forward what looked like a general theory, for Heider and Griffiths it seemed fair to never raise Condition 2.

CHAPTER 3 : THE GRAMMAR WRITERS

In this chapter I shall focus attention on the aspect of child language which most preoccupied theorists in the 1960's and early 1970's : the child's ability to produce syntactically structured utterances in an, apparently, creative fashion. In most cases the emphasis was on the description of a system which was neutral between comprehension and production and which was characterised as the knowledge underlying the relevant abilities although, with isolated exceptions such as Shipley, Smith and Gleitman (1969), the data used in the construction of such systems were instances of the child's production. The creativity alluded to above entails the control, by the child, of some sort of rule-system and, in the period under consideration, these rule-systems were usually described using generative grammars. The actual form of generative grammar employed typically owed something to one or other version of transformational grammar and, of course, Chomsky's influence in this regard cannot be overemphasised. From this perspective, then, the child's developing knowledge of syntax can be represented by a sequence of grammars, (G_1, G_2, \dots, G_n) , where the child is credited with the grammar, G_1 , as soon as he manifests behaviour which indicates any grammatical knowledge; this has usually been taken to be when he first employs two-word utterances although, if the well-subscribed view that comprehension precedes production is correct, then the child may have been in possession of grammatical

knowledge before this time.¹¹⁰ In the interests of general presentation, we can assume that G_n is a theory of adult syntactic competence and that G_2, \dots, G_{n-1} represent grammars at arbitrary sampling points in the passage from G_1 to G_n . As was pointed out in Chapter 1, this is not intended to preclude the possibility that there may be systematically identifiable 'stages' in the sequence but merely to make it clear that we cannot pre-judge such an issue. In practice, most people working in this domain have restricted themselves to a small number of points in the sequence, often concentrating their attention on the earliest stages of syntactic development.

The sense in which linguistic theory has informed theorising in this area can be rapidly spelled out. An adequate linguistic theory, according to the Chomskyan conception, supplies, among other things, a definition of the notion, 'possible grammar of a language', and can be seen as restricting the space of grammars through which the child has to search in order to arrive at the grammar of his linguistic environment. The child's 'transition grammars', the guesses he makes as to the grammar of

110. For arguments that this view may not, in general, be correct, see R. Clark (1974). It may also be thought that the mere production of two-word utterances is not, in itself, sufficient to demonstrate syntactic knowledge of the sort supposedly captured by a transformational grammar. This difficult issue will not be pursued further here.

his native language as he learns it, then, must be seen as constrained by the same general theory. The exact nature of an adequate general linguistic theory has, not surprisingly, resisted discovery, although a large number of more or less tentative suggestions in this direction have been made. Clearly, compliance with a coherent set of such suggestions on the part of the child-language theorist will go a long way towards the satisfaction of Condition 2.

It should be pointed out at this stage that Chomsky himself has never seriously presented his theory as a theory of language acquisition in the sense in which we are pursuing an explication of this concept here. This is most obvious from the idealisation to instantaneous learning which his position embraces (see Chomsky and Halle (1968) and, for extensive discussion, Chomsky (1976, pp 119ff)) but whether this idealisation leads to serious problems for linguistic theory is distinct from its inappropriacy as a predictor of the course of syntactic development.¹¹¹

The proposals which I shall consider in this chapter are, perhaps more than any others discussed in this work, out of date and no longer subscribed to by their authors. This is irrelevant to my purposes as it could turn out that such proposals have highly desirable features distinct from their empirical (in)adequacy and, if they have, it

¹¹¹ For a discussion of some of the issues involved in this idealisation and its relationship to linguistic argumentation see Churma (1975).

will be instructive to discover them. The topic of 3.1 is McNeill's (1966) early views on grammatical classes developed within the framework of pivot and open grammars. We shall see here that McNeill is sensitive to the issue of discontinuity raised in Chapter 1 and makes an attempt, albeit unsuccessful, to deal with it. Klima and Bellugi's earliest work (1966) on the development of the syntax of negation is discussed in 3.2. Again we shall come across the problem of discontinuity and see that, in this case, the authors take no steps towards its resolution. 3.3 covers sections of the work of Bloom, first presented in her dissertation, Bloom (1968), and subsequently published as Bloom (1970), which comprises one of the first systematic attempts to trace the grammatical development of a number of children through several stages taking account, in a fairly rigorous fashion, of the linguistic theorising on which the grammatical formalism adopted is based. Here we shall encounter several formal problems and difficulties of interpretation and the question of the comparison of theories from different stages will become more involved. Following Bloom to a large extent is Bowerman (1973) and in 3.4 I shall discuss her attempts to trace the syntactic development of two Finnish children using grammars which are constructed roughly in accordance with the linguistic theory put forward in Chomsky (1965). Bloom's and Bowerman's work can be seen as the climax of attempts to produce systematic grammatical statements covering the child's total syntactic knowledge at a

particular stage and the subject-matter of 3.5 is more restricted. Brown and Hanlon's (1970) attempt to interpret the derivational theory of complexity in terms of developing linguistic structures within a restricted syntactic domain provides one of the most detailed and systematic attempts to approach explanation in syntactic development and in this section I shall spell out the logic of this attempt and evaluate the extent of its success. Brown's (1973) work on the relative complexity of grammatical morphemes follows logically from the discussion of 3.5 and, as 3.6 concludes this chapter. While not being wholly concerned with syntactic development, it shares important methodological characteristics with the work of 3.5 and also provides a convenient bridge to the semantic considerations of Chapter 4.

Obviously these studies do not exhaust the very large number of those undertaken on syntactic development in the period under discussion. I claim that they do represent a reasonable sample and I would go so far as to suggest that many other studies in the period can be seen as having similar successes and failures with respect to Conditions 1 - 4 as we shall meet in the following pages. Aspects of the more recent position that syntactic development is crucially intertwined with semantic development adopted by such authors as Schlesinger (1971, 1974, 1975) and Antinucci and Parisi (1973, 1975) will be considered in Chapter 4.

3.1 McNeill on grammatical classes

The characteristics of pivot and open grammars and the extent to which McNeill manipulated the concept in adopting original proposals of Braine (1963), Brown and Fraser (1963) and Miller and Ervin (1964) and synthesising them into a reasonably well-defined theoretical position are too well-known to merit more than the briefest statement here (for detailed treatment and criticism, see Park (1970), Bowerman (1973), Brown (1973) and many other places). According to the proposal, at the beginnings of syntactic development, it is possible to define, on distributional grounds, two, or sometimes three, grammatical classes. If the distributional analysis yields two such classes, they are referred to as the pivot class and the open class and, if three classes emerge, they are referred to as the first-position pivot class, the second-position pivot class and the open class. Distributional criteria and frequency criteria come together such that the classes have the characteristics shown in Figure 36 (restricting ourselves from now on to the situation in which there are only two classes and assuming that all pivots are first-position pivots). (see p 195 for Figure 36)

These distributional facts can be summarised, in formal terms, by saying that, at the stage in question, at time t_1 , the child has a grammatical competence which recognises two grammatical categories other than Sentence which can

Pivots

Occur frequently in a corpus (some arbitrary criterion of frequency has to be fixed)

Always occur in first position in two-word utterances

Never occur as single-word utterances

Never occur in construction with another pivot

Open class words

Occur infrequently in a corpus

Occur in variable position in two-word utterances

Sometimes occur as single-word utterances

Sometimes occur in construction with another open-class word

Figure 36

be referred to as 'P' and 'O' and which are organised in a grammar having the rules of Figure 37 (ignoring how lexical items are handled).

$$S \longrightarrow \left\{ \begin{array}{l} (P) + O \\ O + O \end{array} \right\}$$

Figure 37

From our point of view, the most important remark concerning a grammar such as this is to do with lexical membership of the categories, P and O. They are claimed to be typically heterogeneous with respect to the adult set of grammatical categories, i.e., in general, it is not the case that there is an adult grammatical category, X, such that X can, even intuitively, be identified with P or O.

At a later stage of development it will be found necessary

to postulate a different grammar to characterise the child's syntactic knowledge and we might expect that such a grammar, while being related to the grammar at t_1 , will begin to resemble an adult grammar to a greater extent. As far as grammatical categories are concerned, this expectation might be realised by us being able to credit the child with one or more grammatical categories which can, intuitively, be identified with those comprising the adult set. As a hypothetical example we can imagine postulating the grammar of Figure 38 for the child at t_2 .

$$S \longrightarrow \left\{ \begin{array}{l} (P') + O' \\ (\text{Adj}) + O'' \\ \left\{ \begin{array}{l} O' \\ O'' \end{array} \right\} + \left\{ \begin{array}{l} O'' \\ O' \end{array} \right\} \end{array} \right.$$

Figure 38

This grammar could be seen as formalising the observations that a class of morphemes which can intuitively be identified with the adult category, Adj(ective), now has privileged distributional properties - it can only appear before a subclass of the original open class, a subclass referred to here as O'' - and, therefore, is assigned category status in the grammar, that a modified pivot class, P' , which will not contain any adjectives can only provide the first member of a two-member construction just as at t_1 , that members of P' are restricted to the extent that they can only occur in construction with members of O' ,

a modified open class, and that members of O' and O'' can occur freely together as well as occurring alone. This developmental process, or something like it, will continue until the child possesses the adult grammar and, in particular, utilises the full set of adult grammatical categories, S, NP, VP, Det, N, etc., where we can assume that this set is supplied by an appropriate linguistic theory.

In general terms, then, what we have is a developmental sequence of grammars, (G_1, G_2, \dots, G_n) , where G_1 is a pivot and open grammar as in Figure 37, G_2 is a modified pivot and open grammar which, in the hypothetical case of Figure 38, recognises a class of adjectives and G_n is a grammar of adult English. It is a sequence such as this against which I wish to test Conditions 1 - 4.

Considering first Condition 1, McNeill and others working within the framework of transformational grammar have always been explicit on the point that they are attempting to characterise the child's syntactic competence and are not producing a model which will predict what the child will say on a particular occasion nor predict what he is capable of understanding. There is thus no sense in which there is a domain, D, in the child's behaviour in which the theory makes predictions and the version of Condition 1 discussed in Chapter 1 entails failure for the proposal. However, this is nothing new and obviously the brief discussion of Chapter 2 can be seen as applicable

here too, although such discussion hardly contributes to a deepening of our understanding of the problems. For small children the immediate problem is whether one can make sense of some proposed grammar as a theory of syntactic knowledge and not whether the concept of syntactic knowledge itself makes sense in the context of psychological explanations. For the sake of further discussion, I assume that this less urgent question can be answered affirmatively along the lines of the inconclusive discussion in Chapter 2.¹¹²

Turning to Condition 2, one's first impression is that, in this domain, we find some of the more sophisticated attempts to interpret linguistic theorising in a developmental context and, indeed, McNeill says at the outset of his 1966 paper (p.15):

112. It is worth pointing out that there are at least two ways of construing theories of linguistic performance neither of which has been the concern of those people who have bothered themselves with writing grammars (but see remarks below in connection with aspects of the work of both Bloom and Bowerman). Linguistic performance can be identified with a native-speaker's 'real' abilities and the move from competence to performance of this type would be mediated, principally, by the imposition of short-term memory constraints and plan execution constraints. An alternative is to identify the domain of a theory of linguistic performance with what people actually say and do, and this will involve reference to contextual and sociological variables which are not necessary in characterising 'real' abilities. Note that the first type of performance theory is no nearer satisfying Condition 1 than is a competence theory.

The intention of this paper is to examine (the intersection of linguistic theory and empirical studies of language acquisition - RMA) in an effort to interpret empirical studies in the light of linguistic theory. The aim is to develop a theory of language acquisition that will be consistent with linguistic theory and will cover the facts of acquisition as they are now known.

To what extent can we see the sequence of grammars, (G_1, G_2, \dots, G_n) , as being constructed in accordance with the general principles of some theory of language structure? To approach this question we can cite a relevant passage from Chomsky (1965), a work with which McNeill was familiar, and which can be seen as hazarding suggestions as to the contents of a general linguistic theory. Chomsky says (pp. 28 - 9):

Traditional universal grammar was also a theory of substantive universals... [It] advanced the position that certain fixed syntactic categories (Noun, Verb, etc.) can be found in the syntactic representations of the sentences of any language and that these provide the general underlying syntactic structure of each language.... Consider the proposal that the syntactic component of a grammar must contain transformational rules (these being operations of a highly special kind) mapping semantically interpreted deep-structures into phonetically interpreted surface structures....

According to this view, and it must be emphasised that Chomsky is, to a large extent, being speculative, we can hope to find a universal inventory of grammatical categories and certain formal constraints on rule-types in the

general linguistic theory.

G_1 , the pivot and open grammar, uses the formalism of phrase-structure rules and, although it doesn't contain any transformational rules, it could be viewed formally as a transformational grammar of a degenerate sort. That such a view is not totally implausible can be seen from considerations of the logical structure of the theory (cf. Chapter 1 and fn 115 below) but this is not the aspect of the theory that I wish to concentrate on here. As far as the inventory of grammatical categories is concerned, G_1 has a strange look, recognising the categories, S, P and O, which, with the exception of S, are foreign to the grammar of adult English and, so far as I know, to the grammar of any human language.¹¹³ Similar remarks can be made in connection with G_2 except that there we find a move towards the adult set of categories via the introduction of the category of adjectives. Therefore, we can conclude that, within the developing set of grammatical categories, we have a serious discontinuity:

113. John Lyons has pointed out to me that there may be some connection between O and P and either "full" and "empty" words of the Chinese grammatical tradition or "content" and "function" words as understood by such scholars as Fries (1952). It seems to me that, while this connection may be plausible in some respects, there are instances of forms identified as pivots which would be ascribed to the class of full words or content words in either of the above usages. See Bowerman (1973) for examples and discussion.

the child begins with grammatical categories, P and O, subsequently dropping them to take over the set of adult grammatical categories, going through stages where he uses other categories which are also heterogeneous with respect to adult categories and equally alien to the grammars of the world's languages, e.g., P', O', O'' in G_2 . As pointed out in Chapter 1, such a discontinuity does not necessarily amount to a demonstration of incorrectness but it does deserve discussion and argument. McNeill does not argue for the discontinuity but, significantly, claims that what we have is an apparent discontinuity which analysis will reveal as quite benign. His analysis of this issue (later abandoned on empirical grounds) is well-known. Briefly, he claims that, while it is true that the categories, P and O, are heterogeneous with regard to the adult grammatical categories (at least a subset of which is assumed to be provided by the general theory of grammar), nevertheless, they honour those categories generically. What this amounts to is a claim that there is no pair of morphemes, X and Y, such that X and Y belong to the same adult grammatical category, X belongs to P and Y belongs to O. Another way of putting this, which should be interpreted informally but which is useful for the point at issue, is that, if we regard the category labels as class-names denoting the set of morphemes which fall in the class, then we can assert the identities

$$P = X_1 \cup X_2 \cup \dots \cup X_i$$

$$O = X_{i+1} \cup \dots \cup X_n$$

where the X_j ($1 \leq j \leq n$) are a subset of the set of adult grammatical categories. This view would be supported by the hypothetical development we are considering from t_1 to t_2 where the pivot class at t_1 would simply lose the class of adjectives at t_2 . Obviously, if development does proceed along these lines, McNeill has solved the discontinuity problem by showing that, in effect, the discontinuity does not exist. The alien categories, P and O, just are our familiar categories in disguise and what is important from my point of view is to see this analysis as inspired by an implicit recognition of the necessity to satisfy Condition 2.

For the sake of completeness we may quickly consider McNeill's proposals with regard to Conditions 3 and 4. He presents, as an instance of development within the set of grammatical categories, the tree-structure of Figure 39.

Time

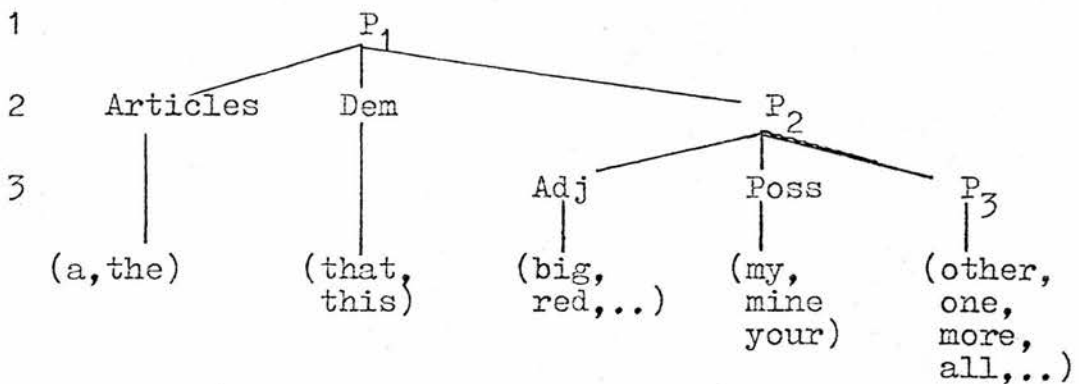


Figure 39

(from McNeill (1966,27))

All that is important from the point of view of Condition 3 is that there is a gradual increase in the number of grammatical classes from t_1 to t_2 and from t_2 to t_3 and this might lead us to think that Condition 3 is satisfied. But, because of the modification of the original pivot class, this conclusion is not justified. It is not the case that we get the categories, Articles and Dem, added to the category, P_1 , in the transition from t_1 to t_2 but rather that these categories are introduced along with another new category, P_2 , and P_1 disappears from the grammar so, unless we have some independent way of evaluating the complexity of the set, $\{\text{Articles, Dem, } P_2\}$, against the set, $\{P_1\}$, we are forced to conclude that Condition 3 is not applicable. Given this, only the second possibility in Condition 4 is a real one for providing an answer to the question as to why we find the development we can schematise as

$$\{\text{Articles, Dem, } P_2\} \longrightarrow \{\text{Articles, Dem, Adj, Poss, } P_3\}$$

rather than the development we can schematise as

$$\{\text{Adj, Poss, } P_2\} \longrightarrow \{\text{Articles, Dem, Adj, Poss, } P_3\}$$

with regard to the transition from t_2 to t_3 .¹¹⁴ But no attempt is made by McNeill to ground the development of grammatical classes in some theory regarded as more basic and it is unclear, if we consider the case in question, what such a grounding could conceivably look

114. Because of the indeterminacy of P_1 in Figure 39 no such question can be formulated with respect to the transition from t_1 to t_2 .

like. Certainly it would be extraordinary if one could find cognitive or perceptual reasons for why the child controls the categories, Articles and Dem, before he controls the categories, Adj and Poss.¹¹⁵

In conclusion we can say that the most interesting aspect of McNeill's proposals arise in connection with Condition 2 and it is apparent that satisfaction of the condition, perhaps implicitly recognised, can be seen as motivating theoretical claims.

3.2 Early views on the syntax of negation

Klima and Bellugi (1966) is the first systematic attempt to investigate the development of the expression of negation in English.¹¹⁶ It is explicitly modest in aims

115. At this point it can be noted that, if we are concerned with the transition from degenerate (i.e., no transformations) transformational grammars to 'proper' transformational grammars, as McNeill's theory demands, we shall find logical theory-internal reasons for the development proceeding in this direction. In the text I concentrate on grammatical categories in isolation as this leads to an interesting analysis in terms of my conditions, and a system integrating formal and substantive aspects of syntax is not presented in detail in McNeill (1966).

116. This treatment was subsequently extended and deepened by one of the authors (Bellugi (1967, 1968)) but these developments will not concern me here. Klima and Bellugi's paper also contains a section devoted to the development of question forms which interacts with the section on negation. However, ignoring this section as I shall does not prejudice evaluation in the sense we are concerned with here.

and restricted to considerations of form, entirely leaving aside questions of meaning and the interaction of form and meaning. As far as their aims are concerned, Klima and Bellugi have this to say (p 191):

It should be understood that when we write rules for the child grammar it is just a rough attempt to give substance to our feeling about, and general observations demonstrating, the regularity in the syntax of the child's speech (my emphasis - RMA).

and, as for the neglect of semantic considerations:

We want to emphasise here that we are not dealing with the expression of semantic concepts on the part of the child, or of basic grammatical notions like subject function and transitivity; rather we are concerned with the way he handles lower-level syntactic phenomena like position, permutability and the like (ibid).

Given these qualifications, it is questionable whether Klima and Bellugi's proposals deserve to be considered as serious explanatory theories and it is unclear that any psychological domain is being investigated. Nevertheless, a consideration of their work does point to a number of difficulties within a relatively simple framework which will prove useful as an introduction to similar difficulties which we shall meet within more complex frameworks later in this chapter.

The authors claim to discern three stages in the development of the syntactic expression of negation.¹¹⁷ Typical

117. The data on which the generalisations are based came from Roger Brown's longitudinal project.

of the data produced at Stage 1 are the utterance-forms, No...wine finger, more...no, No singing song and Not a teddy bear. The generalisation which emerges from these data is that there are no sentence-internal negative elements nor are there any auxiliary verbs. To formalise this generalisation the child can be credited with a fragment of a grammar along the lines of Figure 40.

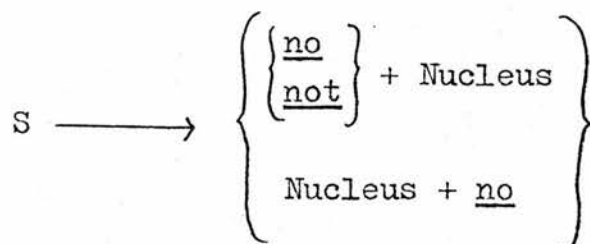


Figure 40¹¹⁸.

which is interpreted as that part of the child's grammar which is implemented in explaining the syntactic patterns found in the child's negative utterances.

At Stage 2 a more complex picture emerges which is evidenced by the following sample of utterance forms: I can't catch you, you can't dance, I don't want it, No pinch me, This a radiator no, Don't bite me yet, That not 'O', that blue, that no mummy. As well as finding utterances which exemplify the patterns found at Stage 1, we now also find sentence-internal negative elements which may take the form of a simple negative such as no or not or may be a negated auxiliary verb. An important

118. Obviously this grammar could be complicated by indicating the optionality of the negative elements and expanding the constituent, Nucleus, but this is hardly necessary for the present discussion.

point for Klima and Bellugi is that, at Stage 2, there are no occurrences in their corpus of non-negative auxiliary verbs. In summarising these facts, they say (pp. 194 - 5):

Let us begin with a basic structure something like:

$$S \longrightarrow \text{Nominal} - (\text{Aux}^{\text{neg}}) - \left\{ \begin{array}{l} \text{Predicate} \\ \text{Main verb} \end{array} \right\}$$

... This first rule can be related to the shape of sentences by the following rules:

$$\text{Aux}^{\text{neg}} \longrightarrow \left\{ \begin{array}{l} \text{Neg} \\ \text{v}^{\text{neg}} \end{array} \right\}$$

$$\text{Neg} \longrightarrow \left\{ \begin{array}{l} \text{no} \\ \text{not} \end{array} \right\}$$

$$\text{v}^{\text{neg}} \longrightarrow \left\{ \begin{array}{l} \text{can't} \\ \text{don't} \end{array} \right\}$$

v^{neg} restricted to non-progressive verbs

where the particular selection of the negative is determined by the Main Verb with don't and can't restricted to occurrence before instances of non-progressive main verbs.

All of this is reasonably clear and the last rule could easily be converted to a context-sensitive phrase-structure rule if we are allowed reference to a subcategory of verbs, say v^{prog} or $\text{v}^{\text{non-prog}}$. These rules, along with the rule from Stage 1, comprise the relevant part of the child's grammar at Stage 2.

Finally, at Stage 3, we find utterances like Paul can't have one, You didn't eat supper with us, Donna won't let go, That was not me, It's not cold, He not taking the

walls down, Don't touch the fish, I not see you any more
and, in addition, auxiliaries now occur unnegated. These
observations are seen as implying the conclusion (p.197):

...so we can now begin with a basic structure like:

$$S \longrightarrow \text{Nominal} - \text{Aux} - \left\{ \begin{array}{l} \text{Predicate} \\ \text{Main verb} \end{array} \right\}$$

and suggest such rules as follows:

$$\text{Aux} \longrightarrow T - v^{\text{aux}} - (\text{Neg})$$

$$v^{\text{aux}} \longrightarrow \left\{ \begin{array}{l} \underline{\text{do}} \\ \underline{\text{can}} \\ \underline{\text{be}} \\ \underline{\text{will}} \end{array} \right\}$$

where be is restricted to predicate and progressive
and is optional, can and do to non-progressive main
verbs.

Transformations

I. Optional be deletion

$$\text{NP} - \underline{\text{be}} \implies \text{NP}$$

II. Do deletion

$$\underline{\text{do}} - V \implies V$$

The details of much of this need not concern us. What
we assume, perhaps contrary to Klima and Bellugi's
intention (cf. above), is that the three grammar-fragments
illustrated in Figure 40 and in the two passages cited
from the original, are to be interpreted as theories of
a domain D, the child's syntactic knowledge, where that
knowledge is restricted to negative structures, i.e., we
have a sequence of grammar fragments, (G_1, G_2, G_3) , which
can be analysed in terms of Conditions 1 - 4.

Condition 1 deserves the usual qualification except that, on this occasion, they should perhaps be emphasised to an even greater extent because of Klima and Bellugi's own reservations as to what they are involved in.

Condition 2 is, as it was in 3.1, perhaps the most interesting. The theories we are considering are presented in the form of grammars and again the linguistic influence is clearly Chomsky's although Klima's (1964) pioneering work on the syntax of negation in English supplies many of the more detailed concepts. Rather than treating the grammars holistically we consider different aspects of the grammars in turn and so, concentrating on rule-types, the sequence, (G_1, G_2, G_3) , appears to satisfy the minimal requirements that all rules are either phrase-structure rules (or translatable into equivalents which are) or transformational rules. To the advantage of the proposals is the observation that the transformational rules which occur in G_3 , while deletion rules, do not fail to satisfy the recoverability of deletions condition on such rules. This condition, first formulated in Chomsky (1965) and subsequently extensively discussed, allows for the deletion of specified lexical material and in both I and II of G_3 this is what we find (cf., in this respect, remarks made in connection with deletion rules formulated by Bloom and by Bowerman which are discussed in subsequent sections of this chapter). Turning to grammatical categories however, things are not so clear-cut. The inventories

of grammatical categories used by the sequence of grammars is as follows:

- G_1 : Sentence, Nucleus
 G_2 : Sentence, Nucleus, Nominal, Aux^{neg}, Predicate, Main Verb, Neg, V^{neg}
 G_3 : Sentence, Nominal, Aux, Predicate, Main Verb, T, V^{aux}. Neg. 119

Now the trouble with these categories is that, with one or two exceptions, they are not categories which are systematically and consistently used in any of the general theories of grammar to which Klima and Bellugi owe their allegiance. This is particularly true for Nucleus, Aux^{neg}, and V^{neg}. What this amounts to is the undesirable consequence that Klima and Bellugi must subscribe to one or more discontinuities in the set of grammatical categories as the child learns the system of negation. McNeill, encountering a similar situation, attempted to argue the problem away. Klima and Bellugi neither do this nor make any attempt to insulate their analyses against this sort of point. We can see the problem arising in a particular case for the restricted section of development which they deal with. There we have the grammatical category, Nucleus, being utilised in G_1 and G_2 only to be dropped in G_3 119 and 119. It is unclear, for G_3 , whether the category, Nucleus, is supposed to survive or not. Klima and Bellugi never mention it but there are utterance-forms in their sample which were used to justify its use in G_1 and G_2 , e.g., No, I don't have a book, No, it isn't, the obvious difference between these and the earlier 'Nucleus containing' structures being that, in these, the putative Nucleus itself contains a negative element.

furthermore, dropped in a completely unrevealing way. If it were to be dropped in a revealing way this could involve it being split into two or more categories as McNeill suggested for the categories, P and O. Similarly the category, Aux^{neg}, appears at G₂ and has disappeared in G₃. It appears that we can conclude, with some justification, that the Klima and Bellugi analysis of the development of negation fails Condition 2 with respect to the inventories of grammatical categories employed at the various stages. Given this, there is little point in pushing the analysis further to see how it matches up against Conditions 3 and 4. However, it is worth pointing out that in the aspect in which it satisfies Condition 2, with regard to rule-types, it also satisfies Condition 3 (although not, of course, with respect to particular rules) and this satisfaction of Condition 3 can be grounded in the structure of the general theory of transformational grammar.

This section then has important similarities to the previous one. It is interesting that, in both sections, we have found it possible to apply Condition 2 in a fairly detailed and serious way whereas this was only true to a limited extent in Chapter 2. Even so the application of Condition 2 has led to the posing of serious questions for the theories under consideration and the cynic will probably assume that problems of a similar magnitude would arise for models of lexical development if we had well-developed theories of the lexicon and the semantic information

it contains to which we could refer them. This centrality of Condition 2 will accompany us through much of this chapter.

3.3 Bloom's grammars

Lois Bloom's work (1968, 1970) represents the first attempt to write complete grammars for a number of children and to trace the detailed developments of these grammars. It can also be seen as containing the first attempt to codify children's utterances in terms of the context in which they occur according to an argued scheme of categorisation and to integrate the results of this into a developmental syntactic theory which relates to the then current views on syntactic structure.

Bloom presents a total of five complete grammars for different stages in the development of the three children she studied, one for Kathryn, two for Gia and two for Eric. Obviously, from the point of view of the present enterprise the one grammar for Kathryn is of no consequence as no developmental conditions can be tested against it.¹²⁰ Accordingly, 3.3.1 is devoted to a discussion of Gia's grammars and 3.3.2 to those presented

120. Conceivably the one grammar for Kathryn could be compared to the grammars from the other children and, in fact, Bloom indulges in informal comparisons of this kind. It requires an assumption of commonality across the children to be feasible and I have taken the view that comparison of within-child grammars provides quite enough material to begin to see the advantages and weaknesses of the general approach.

for Eric. All page references in this section are to Bloom (1968).

3.3.1 The syntactic development of Gia

The sample on which Gia's first grammar was based was collected when the child was 19 months and 1 week old with an MLU of 1.12 morphemes. The grammar contains phrase-structure rules and 'lexicon-feature' rules and is shown in Figure 41.

Phrase-structure rules

$$1. S \longrightarrow \begin{Bmatrix} N \\ Q \\ \underline{Hi} \end{Bmatrix} + \begin{Bmatrix} VB \\ N \end{Bmatrix}$$

Lexicon feature rules

- i. $N \longrightarrow [+N, \underline{+animate}]$
- ii. $[+animate] \longrightarrow +[\underline{\quad} N]$
- iii. $VB \longrightarrow -[\underline{Hi} \underline{\quad}]$
- iv. $Q \longrightarrow [+quantifier]$
- v. $[+quantifier] \longrightarrow \underline{more}, \underline{'nother}$

Figure 41

(adapted from Bloom, p 165)

A few clarificatory comments are in order in connection with the relationship between this grammar and the framework developed in Chapter 2 of Chomsky (1965).

The phrase-structure rule is straightforward enough but the lexicon feature rules contain a number of peculiarities.

(i) is a simple subcategorisation rule but (ii) violates

the condition that only lexical categories appear on the left-hand side of such rules. Chomsky (1965) contains a discussion of this restriction arguing that "it may be a bit too severe" (p.112), and it is not insignificant that already we come up against one of the less well-understood aspects of the general theory. (iii) is also a subcategorisation rule and does have a lexical category as its left-hand side but the right-hand side suggests that not being able to follow a particular morpheme, hi, is an inherent feature of verbs but no rule of this sort, mentioning a particular lexical item, figures in the theory Chomsky develops. (iv) is again a subcategorisation rule of the type found in (i) except that, in this case, it doesn't subcategorise. To be told that the syntactic category Q (= quantifier) has the feature, [+quantifier] is not to be told anything although syntactic features of this sort may be necessary as part of a larger enterprise within a theory achieving lexical insertion via matching of features. Finally, the last rule, (v), seems to be an attempt to incorporate an aspect of lexical insertion into this component of the grammar, a move which, again is at variance with the general theory.

Along with these rules goes a lexicon which has the entries shown schematically in Figure 42 among others.

<u>baby</u>	[+N]	<u>Gia</u>	[+N, +animate]
<u>bag</u>	[+N]	<u>away</u>	[+VB]
<u>fly</u>	[+N, +animate]	<u>go</u>	+ [____Part]

Figure 42 Adapted from Bloom, p 437

It is difficult to see exactly how this lexicon interacts with the other parts of the grammar but two points are transparent:

(a) that there is no reason why the quantifiers should be given their privileged status within the lexical feature rules rather than appearing in the lexicon with simple entries of the form:

more +quantifier

(b) that the verb, go, will never be inserted into a phrase-marker as it requires the presence of a following Part (= Particle) which is not introduced in the phrase-structure component of the grammar.

As far as points such as these are concerned, it is perhaps best to leave the last word with the author who, when presenting her first lexicon for the grammar she wrote for Kathryn, says (p.431):

The form in which the lexicons are presented may be considered unorthodox, but there does not appear to be a concensus regarding the form for lexical entries in a dictionary. Moreover, the form of the lexicon is not the issue; attention has been given to the children's use of words - in syntactic contexts and in isolation.

It would be possible to agree with the sentiments expressed in this passage regarding a lack of an accepted view of the lexicon and still demand a more cogent treatment than we are presented with. These, by no means trivial, problems aside, what are the characteristics of the first Gia grammar?

The phrase-structure component of the grammar, given the absence of any recursive rules, generates only a finite number of structures. There were, in the corpus, instances of $\exists+N$, $\exists+V$, $Q+N$, $Q+VB$, $N+N$, Hi +N, and $N+V$. There were no occurrences of Hi +V and this is accounted for by (iii) above. In cases of $N+N$ constructions the first N could usually be categorised as an animate noun and (ii) accounts for this. Only a small number of utterances in the corpus are outside the scope of the grammar and the mysterious reference to particles in the lexicon' is, to some extent, explained by discussion of occurrences of verbs with particles.

The second sample on which a grammar was based was collected from Gia when she was 20 months and 2 weeks old and the MLU for the sample was 1.34 morphemes. The second grammar consists of a phrase-structure component, lexicon feature rules and a transformational component as shown in Figure 43. (see p. 217)

Again before proceeding further, some comments are in order. The phrase-structure rules of Figure 43 are unobjectionable but the lexicon feature rules and the transformational rules have several worrying aspects. As for the former, similar remarks apply to (i) - (vi) as have already been made for lexicon feature rules in Gia's first grammar. This leaves (vii) and it is difficult to make any sense of this as Bloom offers no discussion of its function. Strictly speaking, in order

Phrase-Structure Rules

1. $S_1 \longrightarrow N+(Q)+\left\{\begin{array}{l} NP \\ VP \end{array}\right\}$
2. $S_2 \longrightarrow \underline{Hi} +N$
3. $VP \longrightarrow VB+NP$
4. $NP \longrightarrow (\emptyset)+(N)+N$

Lexicon Feature Rules

- i. $N \longrightarrow [+N, \pm animate]$
- ii. $[+animate] \longrightarrow +[____ VB]$
- iii. $Q \longrightarrow [+quantifier]$
- iv. $[+quantifier] \longrightarrow \underline{more}$
- v. $VB \longrightarrow [+VB]$
- vi. $[+VB] \longrightarrow \pm [____ NP]$
- vii. $\emptyset \longrightarrow +[/d/____], - [Q____]$

Transformational Rules

(1) $T_{\text{placement}}$ (optional)

S.D. : away + X

S.C. : $x_1 - x_2 \implies x_2 - x_1$

(2) $T_{\text{reduction}}$ (obligatory)

S.D. : $\cancel{\#} - X - Y - Z$, where X, Y, Z,
are category symbols

S.C. : $\cancel{\#} - x_1 - x_2 - x_3 \implies \cancel{\#} - x_i - x_j$,
where $0 \leq i < j \leq 3$

(3) $T_{/\emptyset/}$ Placement (optional)

S.D. : X - VP, where X may be Q or null

S.C. : $x_1 - x_2 \implies \emptyset - x_1 - x_2$

to be intelligible at all, it must assume that 'ə' is either a syntactic category or a complex symbol and it is self-evidently neither of these. What seems to be the most likely interpretation of the facts leading to the formulation of (vii) is that we find /ə/ and also /də/ in free variation with it, except that /də/ is never found following a quantifier. But is this sort of information appropriately represented in this component of the grammar? Not in any grammatical framework with which I am familiar and one can only conclude that (vii), in that form, is fundamentally misconceived.

The transformational rules also contain some mysteries. (1) is a permutation rule designed exclusively to make sure that away can occur in both sentence-initial and sentence-final position. Permutation transformations are deemed undesirable in the theoretical literature since Postal (1964) because of the absurdities they lead to in derived constituent structure and so the rule would have to be recast as an amalgam of deletion and adjunction to accord with the canons of the theory. What implications this might have for derived constituent structure, one can only wonder at. Much more interesting is the case of (2), the reduction transformation (see also discussion below of Bowerman's employment of similar devices). The motivation for the reduction transformation is that, with a small number of exceptions, Gia's utterances were restricted in length to two morphemes yet there was evidence, on the basis of adults' interpretation of what

the child was 'meaning', that she controlled structures which could only be clearly expressed in utterances which were three morphemes long. Briefly, the corpus of 1015 utterances, containing 451 two or more morphemes in length, included 15 interpretable as subject-verb strings, 23 interpretable as subject-object strings and 38 interpretable as verb-object strings. These interpretations are reflected in the theory of the child's syntactic competence by crediting her with the phrase-structure rules, 1 and 3, on p 217 which allow the construction of derivations like that shown in Figure 44.

S₁
 N VP
 N VB NP
 etc.

Figure 44

which can be associated, by well-known principles, with trees like that in Figure 45 from which the functional notions, 'subject-of', 'main-verb of' and 'direct object-of' can be read as described by Chomsky.¹²¹

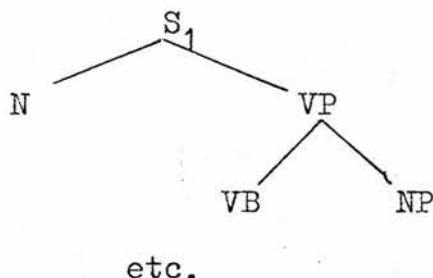


Figure 45

121. This is not exactly true given the left-most N in these structures as Chomsky's definition of 'subject-of' refers to NP but, in the face of the other difficulties we are discussing, this seems a trifling objection.

The reduction transformation is then seen as operating on structures of this kind and getting rid of one of the categories while maintaining, in the theory of the child's competence, a level of representation at which it makes sense to credit her with these functional notions. The trouble is that, from the point of view of the general theory, the reduction transformation is not a possible transformational rule violating, as it does, the condition of recoverability of deletions. Informally, this condition states that material deleted by a transformational rule must either be specified lexical material (cf. the discussion of Klima and Bellugi's Do-deletion rule above) or that the deleted material leaves a copy behind and is thus recoverable.¹²² Whether the condition is one which a linguistic theory of the sort assumed in this discussion must insist on in the final analysis is not a question to be settled here. All I wish to point out is that, in embracing a rule which blatantly violated this important condition, Bloom calls into question her interpretation of the theoretical literature on which she is supposedly basing her acquisition model. Of course, it could be argued that the necessity of reduction transformations in acquisition studies should lead to a modification of the

122. Failure to satisfy the condition permits the general linguistic theory to make available grammars which generate all recursively enumerable languages and this was viewed as an intolerable laxity in the general theory by Chomsky (see Peters and Ritchie (1971) for much more extended discussion and Sampson (1973, 1975) questioning the substance of the debate).

general theory but Bloom nowhere suggests this and, since she doesn't attempt to present a different general theory from which her grammars can be seen as derived, I can only conclude that she has no such intention in mind. This leaves (3) for discussion and, so far as I can make out, this is a permissible format for a transformational rule merely involving the adjunction of new material on the left of a category node. It would be possible to ask questions with regard to the intended derived constituent structure, (e.g., is the α intended to be part of the Q when it is present and part of the VP when it isn't?) but enough has been said already to cast some doubt on the status of the transformational component of the second Gia grammar.¹²³

The grammar accounts for almost all of the utterances in the second Gia corpus, exceptions being a number of three-term strings and some N+N constructions with interpretations which were not consistent with any of the structures available from the grammar. With this success in mind we can now turn to consideration of Conditions 1 - 4 with regard to the sequence of grammars, (G_{G1}, G_{G2}) .

Condition 1 presents exactly the same problems as it

123. It seems to me that the importance of the above remarks cannot be over-emphasised. The theoretical machinery being adopted cannot be manipulated at will and still inspire confidence and whatever plausibility Bloom's suggestions get by virtue of their allegiance to the standard theory must be vigorously disputed.

always has and the usual qualifications have to be made concerning its satisfaction. One additional comment which is relevant to the immediate proposal is that, if G_{G1} and G_{G2} are intended to be theories of the child's syntactic knowledge, then it might be more appropriate to remove the reduction transformation from them and include it or some analogue in a theory of linguistic performance, where it could be interpreted in terms of planning constraints or some similar notion. This would, of course, have the additional advantage of circumventing the questions raised in connection with the formal status of the reduction transformation.¹²⁴

Condition 2 again seems to be the most interesting one and details of it have been discussed already. Whether one agrees with the theory or not, Chomsky (1965) puts forward a coherent set of views on the nature of general linguistic theory including speculations on the identity of syntactic categories and the types of rules to be found in grammars and so, adopting the strategy of isolating components of the grammars, consider first syntactic categories. Bloom's proposals are on firmer ground than Klima and Bellugi's discussed in the previous section. Gia's development in this regard can be represented by the two inventories of syntactic categories:

$G_{G1} : S, N, Q, VB$

$G_{G2} : S_1, S_2, N, Q, NP, VP, N, VB$

124. For remarks along these lines, see Schaerlaekens (1973) and, for more detailed comments, section 3.4. below.

and, with the exception of the reference to two sentence-types in G_{G2} , these inventories are impeccable. The remaining categories are all to be found in respectable theoretical proposals and would probably be on most lists of universal syntactic categories.¹²⁵ Again, in the case of syntactic features, there seems to be little to take exception to, apart from some of the obscurity already discussed, and we have the following inventories for the two grammars:

G_{G1} : \pm animate, \pm quantifier (and, presumably, although Bloom doesn't mention it, \pm VB)

G_{G2} : \pm N, \pm animate, \pm quantifier, \pm VB

In addition, the two grammars use the following sets of contextual features:

G_{G1} : \pm _____ N, \pm Hi _____

G_{G2} : \pm _____ VB, \pm _____ NP, \pm /d/ _____, \pm Q _____

and, as has already been made clear, problems arise here because included are feature-types which it is difficult to see having a place in any general theory of grammar. I refer here, particularly, to the types exemplified by \pm Hi _____ and \pm /d/ _____ which surely go beyond the bounds of what Chomsky had in mind for a set of contextual features. In general, however, embracing a principle of charity, we might be tempted to conclude that, such aberrations aside, G_{G1} and G_{G2} are constructed in accordance with the general principles of Chomsky (1965) at

125. It is worth noting here that the necessity for a category, VP has been questioned by, e.g. Lakoff and Ross (1967).

least as far as syntactic categories and syntactic features are concerned.

What now of syntactic rules? The position here is much less encouraging. There are three rule-types to consider: phrase-structure rules, lexicon feature rules and transformational rules. No problems arise from the first type, rules of the second type are very heterogeneous with no clear principles being evident in their construction and serious questions have been raised in connection with the examples of the third type employed in the grammars. We therefore conclude that, as far as rule-types are concerned, G_{G1} and G_{G2} are not constructed in accordance with the general principles of Chomsky (1965) nor, so far as I can see, in accordance with any other set of well-articulated principles. The impression one gets from a close examination of the rules is that they are purely ad hoc and motivated principally by a desire to account for as large a portion of the data as possible while paying lip-service to the familiar theoretical notions.

Given this damning diagnosis, it is perhaps, unnecessary to consider Conditions 3 and 4 in detail. It is, however, interesting, in the light of what has just been said above, that, in comparing G_{G1} and G_{G2} , Bloom feels that she can provide an affirmative answer as far as satisfaction of Condition 3 is concerned. She says (p.187):

The Gia II grammar is more complex than the Gia I grammar and reflects syntactic maturity in a number of important aspects - although the grammars are also similar in a number of important ways.

It should be apparent that, given the failure to adhere to any clear theoretical framework, this is more a statement of faith than a reasoned conclusion and the extent to which this is true can be seen by again considering the grammars in terms of their different components. We find that Condition 3 is satisfied in just those cases where departure from general theory isn't radical.

So, consider again sets of syntactic categories. There is a simple increase in the membership of this set from G_{G_1} to G_{G_2} with the exception of the replacement of the unitary category, S , by the two categories, S_1 and S_2 , which is not vital to the proposals. Nor do the syntactic features present any problem as membership of this set remains static between G_{G_1} and G_{G_2} but, predictably, the contextual features are a complete mess. G_{G_2} employs more of these than G_{G_1} but any hopes we might have of comparing the two sets beyond this gross numerical measure founder on the peculiarities of the items involved. The kindest conclusion we can draw is that Condition 3 does not apply. As far as rule-types are concerned, we can diagnose satisfaction of Condition 3 as G_{G_2} contains what are called 'transformational rules' and G_{G_1} doesn't and I suspect that it would be possible to make similar claims with respect to the particular sets of phrase-structure rules, although Bloom doesn't present her grammars in a way which makes this easy to see.

To explore this matter a little further, consider the

possibility of replacing rule 1, in the phrase-structure component of G_{G1} , with something like:

$$1'. \quad S \longrightarrow \left\{ \begin{array}{l} \left(\left(\begin{array}{c} N \\ Q \end{array} \right) + \left(\begin{array}{c} NP \\ VP \end{array} \right) \right) \\ (\underline{Hi}) + (N) \end{array} \right\}$$

and adding to the grammar the rules:

$$2' \quad NP \longrightarrow N$$

$$3' \quad VP \longrightarrow V$$

removing (iii) from the lexicon feature rules. If we then amalgamate rules 1 and 2 from the phrase-structure component of G_{G2} , we get:

$$1'' \quad S \longrightarrow \left\{ \begin{array}{l} N + (Q) + \left\{ \begin{array}{c} NP \\ VP \end{array} \right\} \\ \underline{Hi} + N \end{array} \right\}$$

However (1''), like 1 in the original G_{G2} , doesn't allow for the possibility of utterance-initial more which does occur in the corpus and so, better would be:

$$1''' \quad S \longrightarrow \left\{ \begin{array}{l} \left\{ \begin{array}{c} N + (Q) \\ Q \end{array} \right\} + \left\{ \begin{array}{c} NP \\ VP \end{array} \right\} \\ \underline{Hi} + N \end{array} \right\}$$

which is now simply 1' above with the addition of the option for the generation of structures of the form, $N + Q + NP$ or $N + Q + VP$, i.e., 1''' can be seen as additively more complex than 1'. It seems to me that a similar case could be made for the relationship between 2' and 3', on the one hand, and 3 and 4 from G_{G2} , on the other, but Bloom doesn't present enough data to make consideration of these possibilities worthwhile. All we are justified

in concluding, therefore, is that, with regard to particular phrase-structure rules, the case for increasing complexity between G_{G_1} and G_{G_2} is not clearly established. The lexicon feature rules only allow us to say that there are more of them in G_{G_2} than in G_{G_1} but the relationship between the two sets is not an additive one and, finally, Condition 3 can only be applied^I in an empty fashion to the sets of transformational rules as none of these occur in G_{G_1} .

In those respects in which Condition 3 is satisfied do we have a grounding as specified in Condition 4? As we have seen, Condition 3 is satisfied for syntactic categories and for syntactic rule-types and for the former of these we are concerned with the question as to why we find the development schematised as

$$\{S, N, Q, VB\} \longrightarrow \{S, N, Q, VB\} \cup \{NP, VP\}$$

rather than the development schematised as

$$\{NP, VP\} \longrightarrow \{S, N, Q, VB\} \cup \{NP, VP\}$$

continuing to ignore differences between S_1 and S_2 . Condition 4 can be seen as partially satisfied via certain notional reflections on the status of the syntactic categories and we might want to go so far as to call this a partial logical grounding. Thus, we can argue that the category, NP, presupposes the category, N, within the theoretical vocabulary and similarly for the relationship between VP and V. This relationship is not a symmetrical one - a system including members of V without members of VP would

be intelligible - and, therefore, we can say that there are theory-internal reasons for why N precedes NP and V, VP developmentally. Such reasoning is not, of course, available for other pairs of categories in the inventories hence the reference here to 'partial' grounding.

Again we have a positive answer to the question raised by Condition 4 in connection with rule-types because, as has been spelled out already, transformational rules assume the presence of phrase-structure rules in the theory and, therefore, the latter should appear developmentally before the former.¹²⁶

In both cases then where Condition 3 is satisfied we can see at least an outline to an answer to the problem posed by Condition 4. The fact remains, however, that Condition 3 is not satisfied in general and, more importantly, Condition 2 is also severely strained this being seen as the source of all the other problems. As far as the development of Gia is concerned, the grammars Bloom presents have about the same status as the partial descriptions of stages put forward by Klima and Bellugi - they are crucially weak in detail, although the broad outline appears to embrace strict adherence to established linguistic theories.

126. Similar claims for the order of appearance of lexical feature rules relative to transformational rules cannot be substantiated as a theory in which rules introducing features followed the application of transformational rules would be intelligible.

3.3.2 The syntactic development of Eric

The first sample collected from Eric when he was 19 months and 1 week old produced only 19 utterances which were more than one morpheme long. As Bloom says (p.204):

A 'grammar' of Eric's language at this point would be presumptuous - the data were meagre.

At the time of the collection of the second sample Eric was 20 months and 2 weeks old and 87 from a total of 490 intelligible utterances were more than one morpheme long. The MLU for the sample was 1.19 morphemes and the grammar Bloom proposes is a simple phrase-structure grammar having just one abbreviated rule and shown in Figure 46.

$$\begin{array}{c} \text{Phrase-structure} \\ S \longrightarrow \left(\left(\begin{array}{c} \text{Pivot} \\ (\emptyset) \text{ VB} \end{array} \right) \right) ((\emptyset) N) \end{array}$$

Figure 46. From Bloom p.218

This grammar generates a finite number of structures, Pivot, Pivot + N, Pivot + a + N, VB, a + VB, VB + N, VB + a + N, a + VB + N, a + VB + a + N, N and a + N¹²⁷ and operates in conjunction with a lexicon which, among others, has the entries of Figure 47.

<u>air conditioner</u>	[+N]
<u>apple</u>	[+N]
<u>broke</u>	[+VB]
<u>build</u>	[+VB]
<u>turn</u>	[+VB, +___Part]

Figure 47. Adapted from Bloom p443

127. A minor point is that the grammar allows the generation of the empty string as a sentence. This was also the case for G_{G1} and some notational device like linked parentheses is therefore necessary.

There are peculiarities about this lexicon. Not least of these is that no lexical feature rules appear in G_{E2} (shown in Figure 46) and such rules are obviously necessary if the lexicon is to function at all. The entry for turn is confusing in that it not only refers to a contextual feature which is not introduced by a lexical rule but also assumes, in that contextual feature, a syntactic category, Part, which is nowhere introduced in the categorial part of the grammar (cf. similar remarks above in connection with G_{G1}). Finally, the lexicon does not provide any means for inserting lexical material under the Pivot node in the generated structures. In order to be consistent Bloom should use a syntactic feature, +Pivot, and have entries in the lexicon of the form:

<u>Hi</u>	[+Pivot]
<u>'nother</u>	[+Pivot]

In short, the meshing of the rules of the grammar and the lexicon leaves a great deal to be desired even in such a simple grammar as this. Only a small number of utterances in the corpus are not accounted for by the grammar. Among these are N+N constructions, two occurrences of the Pivot, 'nother, with non-noun forms and the two occurrences of attributive constructions.

The third sample from Eric was collected when he was 22 months/old and it had an MLU of 1.42 morphemes. Bloom suggests two alternatives for the phrase-structure component of G_{E3} and they are shown in Figure 48.

$$\begin{array}{l}
 \text{(A)} \quad S_1 \longrightarrow \left(\begin{array}{c} \{\varnothing\} \\ \{\underline{I}\} \end{array} \right) + \text{VB} + \left(\begin{array}{c} \{\underline{it}\} \\ \{(\varnothing) + N\} \end{array} \right) \\
 S_2 \longrightarrow (\text{Pivot}) + (N), \text{ where Pivot is } / \varnothing /, \\
 \quad \quad \quad \underline{\text{nomore}}, \underline{\text{more}}, \underline{\text{there (is)}} \\
 S_3 \longrightarrow \left(\begin{array}{c} \{N\} \\ \{\underline{no}\} \end{array} \right) + (\text{VB}) \\
 \\
 \text{(B)} \quad S_1 \longrightarrow (\text{Pivot}) + \left(\begin{array}{c} \{NP\} \\ \{VP\} \end{array} \right) \\
 S_2 \longrightarrow (N) + (\text{VB}) \\
 VP \longrightarrow \text{VB} + \left(\begin{array}{c} \{\underline{it}\} \\ \{(\varnothing) + N\} \end{array} \right) \\
 NP \longrightarrow (Q) + N
 \end{array}$$

Figure 48. From Bloom pp.244-5

Of these alternatives, Bloom says (pp.245 - 6):

...(B) consisted of collapsing the S_1 and S_2 rules in (A)...with a feature representation which specifies:

- (i) Pivot \longrightarrow no, there (is), / \varnothing /
- (ii) / \varnothing / \longrightarrow "I"/ VB
- (iii) Q \longrightarrow more
- (iv) no + NP \longrightarrow no + Q + N

Finally, there was some evidence for the necessity of a reduction transformation of the type we have already met in G_{G2} , "to account for the negative sentences in which the negative element had direct effect on an intervening constituent" (p.248). The form of the reduction rule Bloom suggests is:

$$\begin{array}{l}
 T_{\text{reduction}} \text{ (obligatory)} \quad \text{S.D.: } \left\{ \begin{array}{c} \underline{\text{no}} \\ \underline{\text{no more}} \end{array} \right\} - X - Y \\
 \text{S.C.: } x_1 - x_2 - x_3 \implies x_1 - \left\{ \begin{array}{c} x_2 \\ x_3 \end{array} \right\}
 \end{array}$$

the function of the rule being to delete one of two constituents which occur with either of the negative elements, no or no more.

There is much that is strange about G_{E3} but, to clear familiar ground first, we can note that exactly the same objections apply to the reduction transformation above as applied to the rule with a similar function in G_{G2} . In the case of G_{E3} the constructions leading to the formulation of the rule were only marginally productive anyway but the repetition of the folly deserves repeated emphasis.

Consider now the non-transformational aspects of what we might refer to as $G_{E3}(A)$ and $G_{E3}(B)$. The two versions of phrase-structure rules differ crucially in that A credits Eric with three sentence types all of which are linearly structured whereas B credits him with a modicum of hierarchical structure.

Taking A first and noting that it apparently does not operate with a "feature representation", we can see that each of its three rules are well-formed phrase-structure rules except that the qualification on the expansion of S_2 is quite out of place here and should be handled in a lexicon. There are odd things like the fact that I, it and no are generated directly without being assigned to a syntactic category which must result in a non-uniform treatment of lexical insertion but this is not something which calls into question the formal status of the rules. It is, of course, something which renders obscure any

comparison between this sort of grammar and that developed in Chomsky (1965) where an attempt is made to develop a coherent framework for lexical insertion.

Turning to B, the situation is much more obscure because, while each of the phrase-structure rules is formally correct, the "feature representation" is of indeterminate status. Note first that it is not a set of lexicon feature rules like those we have already met in G_{G_1} and G_{G_2} . In those grammars there was at least a semblance of the type of rules linguists have talked about when introducing syntactic features. In $G_{E_3(B)}$, however, that semblance has completely disappeared. Yet (i) - (iv) are referred to as a "feature representation". Rule (i) is an old-fashioned rule of lexical insertion as found in Chomsky (1957) and the same is true of (iii). (iv) is a context-sensitive phrase-structure rule and properly belongs in the phrase-structure component of the grammar except that the phrase-structure rules of $G_{E_3(B)}$ will never produce the relevant lexical context for this rule to operate. This leaves (ii) which appears to involve the contextually specified re-writing of a phonological form (or is it a lexical item?) as a specific lexical item. Needless to say such monstrosities had no place in the linguistic theories from which Bloom was drawing her inspiration and, in summary, it is difficult to believe that the author had any clear intentions in mind when suggesting $G_{E_3(B)}$ as a serious grammar for Eric at this stage. One is forced to the conclusion that accounting for the data at whatever cost is the prime

motivating force behind this grammar.

Can we consider satisfaction of Conditions 1 - 4 in this rather unrevealing framework? There are two putative developments to take into account:

$$G_{E2} \longrightarrow G_{E3(A)}$$

and

$$G_{E2} \longrightarrow G_{E3(B)}$$

and, again, it is useful to isolate components of the grammars and consider syntactic categories, syntactic rule-types and particular rules as different aspects of the proposals.

Consider first syntactic categories with regard to Condition 2. G_{E2} employs the set of categories, $\{S, \text{Pivot}, \text{VB}, N\}$ and $G_{E3(A)}$ employs the set $\{S_1, S_2, S_3, \text{VB}, N, \text{Pivot}\}$. With the exception of Pivot and the subscripted S's these are all respectable syntactic categories. The subscripted S's are clearly not essential to the proposals and the category, Pivot, and possibilities for dealing with it have already been discussed at length in 3.1. $G_{E3(B)}$ uses the set of categories, $\{S_1, S_2, \text{Pivot}, \text{NP}, \text{VP}, N, \text{VB}, Q\}$ and similar remarks apply here as for $G_{E3(A)}$. In short for syntactic categories, we may safely assume that Condition 2 can be satisfied.

For rule-types, we have already seen some of the problems and there is little point in repeating them here. Suffice it to say that $G_{E3(B)}$ is not constructed, insofar as it is

possible to understand its construction, to accord with the general principles of any theory. In particular, the rules in the "feature representation" are a pastiche of formal devices with no theory informing them. $G_{E3}(A)$, by largely ignoring questions of lexical insertion, remains immune from such strictures and, at this point, I shall drop further consideration of $G_{E3}(B)$ in the interests of economy of presentation. Enough should have been said to persuade all but the most demanding reader that it does not deserve to be taken seriously.

There is nothing to be said about Condition 3 with respect to either syntactic categories or rule-types. There is no development in either of these regards between G_{E2} and $G_{E3}(A)$. Restricting attention to particular phrase-structure rules though yields more interesting possibilities. The single rule of G_{E2} can be expanded according to the usual conventions to give the list of rules in Figure 49.

- | | | | |
|-----|---|---|----------------|
| (a) | S | → | Pivot |
| (b) | S | → | Pivot + N |
| (c) | S | → | Pivot + ə + N |
| (d) | S | → | VB |
| (e) | S | → | ə + VB |
| (f) | S | → | VB + N |
| (g) | S | → | VB + ə + N |
| (h) | S | → | ə + VB + N |
| (i) | S | → | ə + VB + ə + N |
| (j) | S | → | N |
| (k) | S | → | ə + N |

Figure 49.

Those in $G_{E3}(A)$, if we ignore differences between S_1 , S_2 and S_3 , can be expanded as in Figure 50.

(a')	S	→	VB
(b')	S	→	ə + VB
(c')	S	→	<u>I</u> + VB
(d')	S	→	VB + <u>it</u>
(e')	S	→	VB + N
(f')	S	→	VB + ə + N
(g')	S	→	ə + VB + <u>it</u>
(h')	S	→	ə + VB + N
(i')	S	→	ə + VB + ə + N
(j')	S	→	<u>I</u> + VB + <u>it</u>
(k')	S	→	<u>I</u> + VB + N
(l')	S	→	<u>I</u> + VB + ə + N
(m')	S	→	Pivot
(n')	S	→	N
(o')	S	→	Pivot + N
(p')	S	→	N + VB
(q')	S	→	<u>no</u> + VB
(r')	S	→	<u>no</u>

Figure 50¹²⁸

Bloom's own comparison between these two sets of rules is instructive (p.243):

The two rules, S_1 and S_2 (expanded here as (a') - (o') - RMA) differ from the earlier Eric II phrase-structure ...only in the specification of "I" as an alternant of /ə/ in sentence subject position and the inclusion of "it" as a direct object alternant. The rule

128. I ignore here the fact that the same structure can be generated in different ways in the original grammar, e.g., VB strings can come either from a rewriting of S_1 or a rewriting of S_3 , which is justified once the distinction between the S's is removed.

generating S_3 (expanded here as (p') - (r') - RMA) was innovative and, even though only marginally productive, it represented an important developmental difference in structure between the texts at Eric II and III.

The claim is clear and true. The rules in Figure 50 which don't appear in Figure 49 are (c'), (d'), (g'), (j'), (k'), (l'), (p'), (q') and (r'). Of these, (p'), (q') and (r') are quite new, (c'), (k') and (l') are covered by the first of Bloom's claims, (d') and (g') are covered by the second and (j') is covered by a combination of the two. Unfortunately for the neatness of the claim, there are two rules present in Figure 49 which do not appear in Figure 50: (c) and (k). (k) can be generated by expanding the Pivot category in (o') as /ə/, a possibility which is permitted and so, perhaps, we should be prepared to admit that, these exceptions aside (and, as they involve the rather marginal element, /ə/, they are not central exceptions), there is a case for straightforward additive complexity and satisfaction of Condition 3 as far as particular phrase-structure rules are concerned. This could be interpreted in the context of the view that there is a core of rules which the child learns first and subsequently adds to as he learns the adult grammar, although it is not clear that Bloom would subscribe to such a view (cf. brief discussion in Chapter 1).

If we can take additive complexity as demonstrated we can go on and ask whether Condition 4 is also satisfied

in this restricted part of the total proposal. The position we are in is that we have two rule-sets, say R_1 and R_2 , and we know that $R_2 = R_1 + X$, i.e., we have development which we can schematise as:

$$R_1 \longrightarrow R_1 + X$$

and satisfaction of Condition 4 requires that we produce a reason for why we find this rather than the alternative

$$X \longrightarrow R_1 + X$$

There are no clear logical reasons for why the rules' in X should enter the child's grammar after the rules in R_1 . All the rules we are concerned with expand the symbol 'S' and so there is no question of intrinsic ordering which might form the basis for a positive answer to the question on theory-internal grounds. Similarly, recourse to statistical facts about the distribution of rules in the world's languages won't get us very far. The facts are simply not known but, even if they were, there is no a priori reason to believe that they would throw light on the development we are interested in. In fact, there is an a priori reason for believing that they wouldn't, since the rules in R_1 use the syntactic category, Pivot, unlike the rules in X , and this category has not played a large part in the syntactic analyses of the world's languages as has already been pointed out. Finally, there is no more basic theory to which the development can be referred. It would be possible to make vague pronouncements concerning the rules involving negative elements,

(q') and (r'), relating these to the 'cognitive difficulty' of negation but these pronouncements would remain vague because of the lack of a cognitive theory to base them on, because of the lack of comparable cognitive categories to which to relate the rules in R_1 , thus making a cognitive comparison possible and because of the restricted set of rules in X which involve negation, i.e., we would still be left with several rules in X unexplained.

Summarising, we can say that the grammars Bloom presents for Eric are no better, when considered as embodying a developmental theory, than those she discusses for Gia. Again there are serious formal problems relating to the satisfaction of Condition 2. If we ignore these, as it is possible to do by restricting attention to those areas of the grammars where they don't arise, we find that Condition 3 is applicable and is satisfied in a potentially interesting way with regard to the transition from G_{E2} to $G_{E3(B)}$. Unfortunately, though, no attempt is made by the author to satisfy Condition 4 and it appears that either this condition cannot be satisfied or that, at present, the direction of its satisfaction is so vague as to be worthless.

3.4 Bowerman's grammars of Finnish

Bowerman (1973), working within essentially the same tradition as Bloom, presents a total of four grammars for children at different stages in the acquisition of Finnish. These grammars are, on the whole, more thoughtfully

constructed than those we have examined in the previous section but, unfortunately, only two of them can be used for my purpose. This is because one of them is explicitly constructed using a different theoretical framework, taking as a basis Fillmore (1968) and adopting certain modifications.¹²⁹ This is the grammar constructed for the child, Seppo, at MLU 1.42, and, although we have a grammar for Seppo when his MLU had risen to 1.81 it is constructed with reference to Chomsky (1965) and there is no suggestion from Bowerman that the child moves from a case-grammar to using a grammar of the more familiar kind. If such a suggestion were made we would have a developmental theory but it would involve an obvious discontinuity and the starting point of any analysis would be to examine the justification for this. A second grammar, that constructed for the child, Rina, when her MLU was 1.83 is the only grammar for this child discussed by Bowerman and, therefore, unless we are going to indulge in cross-child analysis, we are unable to test this against developmental conditions. We are left, then, with the two grammars for Seppo which are constructed with reference to Chomsky (1965) and these two grammars are considered to constitute theories of Seppo's syntactic competence at MLU 1.42 and at MLU 1.81.

129. It might be pointed out that Bowerman was interested in comparing the merits of case-grammar and the standard theory according to quite different criteria to those pursued here. Her work represents one of the most detailed and careful attempts to apply the concepts of transformational generative grammar in the sphere of language development.

In 3.4.1 I shall present and discuss the first of these grammars, G_{S1} , in 3.4.2 I shall focus on the second, G_{S2} , and in 3.4.3 I shall consider the status of the sequence, (G_{S1}, G_{S2}) as a developmental theory.

3.4.1 The grammar for Seppo at MLU, 1.42

This grammar has, like those of Bloom's, a number of components. In fact, as far as labels are concerned, the components are identical to components appearing in earlier grammars: there are phrase-structure rules, lexicon feature rules, transformational rules and a lexicon. On the whole, though, these components are more carefully constructed and the problem of lexical insertion is discussed in some detail. G_{S1} is shown overleaf as Figure 52.

We can consider each of the sets of rules in turn for formal coherence but, for completeness, we might mention that the lexicon which is intended to function with these rules includes entries like those shown in Figure 51.

<u>isa</u> 'father'	[+N, +animate, -vehicle]
<u>auto</u> 'car'	[+N, -animate, +vehicle]
<u>kahvi</u> 'coffee'	[+N, -animate, -vehicle]
<u>ajaa</u> 'drives'	[+V, +____(N)]
<u>avaa</u> 'opens'	[+V, -____(N)]
<u>pikku</u> 'little'	[+Adj]

Figure 51. Adapted from Bowerman pp.244-5.

Phrase-structure rules

1. $S_1 \longrightarrow (M\bar{N})$
2. $S_2 \longrightarrow (N) VP$
3. $M \longrightarrow \begin{Bmatrix} \text{Adj} \\ N \end{Bmatrix}$
4. $VP \longrightarrow \begin{Bmatrix} V (N) \\ \text{Loc} \end{Bmatrix}$
5. $\text{Loc} \longrightarrow \begin{Bmatrix} N \\ \text{Proloc} \end{Bmatrix}$

Lexicon Feature Rules

1. $N \longrightarrow [+N, \pm\text{animate}]$
2. $[-\text{animate}] \longrightarrow [\pm\text{vehicle}]$
3. $[+\text{animate}] \longrightarrow [-V \text{ ____}, -N \text{ ____}]$
4. $[-\text{animate}, -\text{vehicle}] \longrightarrow [-\text{ ____} V, -\text{ ____} N]$
5. $V \longrightarrow [+V, \pm \text{ ____} (N)]$
6. $\text{Adj} \longrightarrow [+Adj]$
7. $\text{Proloc} \longrightarrow [+Proloc]$
8. $[+Proloc] \longrightarrow \text{tuossa, 'there'}$

Transformational rules

1. $T_{\text{reordering}}$: optional
 - (a) Placement of pois, 'away', and kiinni, 'closed'

$$\text{S.D.: } \left\{ \begin{array}{l} \text{pois} \\ \text{kiinni} \end{array} \right\} X$$

$$\text{S.C.: } x_1 - x_2 \implies x_2 - x_1$$
 - (b) Placement of prolocative

$$\text{S.D.: } N + \text{Proloc}$$

$$\text{S.C.: } x_1 - x_2 \implies x_2 - x_1$$
2. $T_{\text{verb deletion}}$: optional

$$\text{S.D.: } N + V + N$$

$$\text{S.C.: } x_1 - x_2 - x_3 \implies x_1 - x_3$$

The phrase-structure rules are all formally correct and the only point worth remarking on is the use of linked parentheses in the first rule to ensure selection of at least one of the elements on the right of the rule, a device which has already been mentioned in connection with Bloom's grammars.

The lexical feature rules are not so straightforward. That Bowerman is familiar with the relevant parts of Chomsky (1965) is apparent when she says (pp.77 - 8):

These rules rewrite lexical category symbols like N and V as complex symbols (CS), which are sets of specified syntactic features. These syntactic features include:

1. A feature representing the lexical category which dominates the CS, such as [+N] or [+V].
2. A context-sensitive feature specifying the local category frame of the CS to the limits of the node that dominates it, such as, for a noun, [+Det _____] (occurs after a determiner), or, for a verb, [+_____ NP] (takes a direct object). This is called strict subcategorisation.
3. For noun CS's only, context-free inherent syntactic features such as [+animate] or [-animate], [+human] or [-human].
4. For verb and adjective CS's, context-sensitive feature frames, called selection restrictions, which specify the inherent features of nouns with which the CS can occur in construction such as [+_____ NP, +animate] (takes as direct object a noun phrase containing a noun marked positively for animacy).

Although this discussion enumerates types of features rather than types of rules, there is no serious misrepresentation

and we might expect that each of the lexical feature rules would fall clearly into one of the relevant types. Taking the rules in turn, (1) is a context-free subcategorisation rule as is (2). (3), however, is of more doubtful status. It cannot be interpreted as a strict subcategorisation rule as such rules only operate on lexical categories (cf. the first line of the passage cited above), nor as the statement of a selection restriction for the same reason. Therefore, it too must be viewed as a subcategorisation rule adding inherent features' to any complex symbol specified as [+animate]. But it is a fact about the rules in Chomsky (1965) that the categorisation rules adding inherent features do not subcategorise with respect to syntactic environment; they are more 'semantic' in their reference (cf. the examples cited by Bowerman under 3.) I have been unable to find a statement which explicitly outlaws rules such as (3) but these considerations should be seen as providing grounds for unease. Similar remarks apply to (4) where, again, its function is transparent but the extent to which it fits into any of the established rule-types is open to question. (5) is not easy to interpret for different reasons. It says that verbs may be classified as [+____(N)], i.e., they may occur before a noun or in final position or as [-____(N)], i.e. they may not occur before a noun nor in final position. But this is not what Bowerman wants as there is a class of verbs which, while not appearing before a noun, does appear in final position and

The transformational rules in G_{S1} are subject to much the same sort of criticism as has already been levelled at Bloom. Briefly, (1) is a permutation transformation in both of its parts. As such it leads to absurd derived constituent structures. (2), a deletion rule, violates the condition of recoverability of deletions just as did Bloom's rules with comparable function. Bowerman's reduction transformation is more specific than Bloom's, mentioning the deleted constituent, V, but this has no effect on the formal objection. That Bowerman is aware of a certain tension in including rules of this sort in a model of the child's syntactic competence is revealed when she says, after a lengthy discussion of the motivation for this transformation (p.102):

The optional verb deletion transformations of the grammars written for Seppo and Rina are regarded as specifications of performance variables which were important enough to warrant representation in the grammars. (my emphasis - RMA)

As she has just taken the view that grammars are theories of the child's competence, nothing could be a firmer indicator of Bowerman's uncertainty as to her domain of enquiry.¹³⁰

Overall then, our efforts to interpret G_{S1} reveal it as weak in essentially the same respects as were the grammars for Gia and Eric although not to the same extent. The phrase-structure component is impeccable but lack of a

130. For more extensive discussion, see Atkinson (1975)

firm theoretical foundation lurks in the lexical feature rules while the transformational rules, although clear as far as their functions are concerned, are formally dubious. The problem of derived constituent structure, surely of some importance in theories of a developing syntactic competence, is never raised.

3.4.2 The grammar for Seppo at MLU 1.81

The second 'standard theory' grammar for Seppo, G_{S2} , has the same set of components as we have already met although each of them is somewhat larger than in any of the grammars we have considered so far. The grammar appears as Figure 53, (see pp 248 - 9)

Once more there is nothing exceptionable about the phrase-structure rules from a formal point of view and similar remarks to those directed at the lexical feature rules in G_{S1} can be made for that component of G_{S2} . In particular, (1), (3), (4) and (5) are straightforward context-free subcategorisation rules but (2) and (6), while probably having to be construed as of the same type, subcategorise in terms of syntactic context and are thus subject to the same reservations as were expressed for (3) and (4) in the lexical feature component of G_{S1} . (7) is a hybrid including aspects of context-free categorisation, ($V \longrightarrow [+V]$), aspects of strict subcategorisation ($V \longrightarrow [+ \text{---} (NP)]$), and note again the problem of the optional NP which can be overcome in the way outlined for G_{S1} and aspects of selectional restriction

Phrase-structure rules

1. $S_1 \longrightarrow \text{Wh-loc} \left\{ \begin{array}{l} \text{NP} \\ \text{V} \end{array} \right\}$
2. $S_2 \longrightarrow \text{Neg} \left\{ \begin{array}{l} \text{N} \\ \text{V} \\ \text{Adj} \end{array} \right\}$
3. $S_3 \longrightarrow (\text{NP} \downarrow \text{VP})$
4. $\text{VP} \longrightarrow \left\{ \begin{array}{l} \text{Predicate} \\ \text{V (NP) (Loc) (Adv)} \end{array} \right\}$
5. $\text{Predicate} \longrightarrow \left\{ \begin{array}{l} \text{Adj} \\ \text{N} \\ \text{Loc} \end{array} \right\}$
6. $\text{NP} \longrightarrow (\text{M}) \text{N}$
7. $\text{M} \longrightarrow \left\{ \begin{array}{l} \text{Adj} \\ \text{N} \end{array} \right\}$
8. $\text{Loc} \longrightarrow \left\{ \begin{array}{l} \text{N} \\ \text{Proloc} \end{array} \right\}$

Lexicon feature rules

1. $\text{N} \longrightarrow [+N, \text{+pronoun}]$
2. $[\text{+pronoun}] \longrightarrow \left[\pm \text{---} \left\{ \begin{array}{l} \text{V} \\ \text{N} \end{array} \right\} \right]$
3. $[\text{-pronoun}] \longrightarrow [\text{+animate}]$
4. $[\text{-animate}] \longrightarrow [\text{+vehicle}]$
5. $\text{Proloc} \longrightarrow [+Proloc, \text{+directional}]$
6. $[\text{+directional}] \longrightarrow [-\text{NP} \text{---}]$
7. $\text{V} \longrightarrow \left[+V, \pm \text{---} (\text{NP}), \pm [\text{-animate, -vehicle}] \right]$
 $\text{---}, \pm \text{---} [\text{+directional}]$
8. $[\text{+---} (\text{NP})] \longrightarrow \left[\pm \text{---} [\text{+animate}] \right]$
9. $\text{Adj} \longrightarrow [+Adj]$
10. $\text{Adv} \longrightarrow [+Adv]$
11. $\text{Neg} \longrightarrow \text{enää}, \text{'any more'}$
12. $\text{Wh-loc} \longrightarrow \text{missä}, \text{'where'}$

(cont.)

Transformational rules1. $T_{\text{reordering}}$: optional

(a) Reversal of prolocative and NP

S.D.: NP + Proloc

S.C.: $x_1 - x_2 \implies x_2 - x_1$

(b) Fronting of adverb or prolocative

S.D.: $X + V + Y \begin{Bmatrix} \text{Adv} \\ \text{Proloc} \end{Bmatrix}$ Where X and Y may be NP or null, but at least one must not be null

S.C.: $x_1 - x_2 - x_3 - x_4 \implies x_4 - x_1 - x_2 - x_3$

(c) Reversal of verb and direct object, locative or adverb

S.D.: $X + V \begin{Bmatrix} \text{N} \\ \text{Loc} \\ \text{Adv} \end{Bmatrix} Y$ Where X may be NP, Adv or null, and Y may be Adv or null

S.C.: $x_1 - x_2 - x_3 - x_4 \implies x_1 - x_3 - x_2 - x_4$

(d) Reversal of subject and verb

S.D.: $X + \text{NP} + V + Y$ Where X and Y may be Proloc or null

S.C.: $x_1 - x_2 - x_3 - x_4 \implies x_1 - x_3 - x_2 - x_4$ 2. $T_{\text{verb deletion}}$: optionalS.D.: $N + V \begin{Bmatrix} \text{N} \\ \text{Loc} \end{Bmatrix}$ S.C.: $x_1 - x_2 - x_3 \implies x_1 - x_3$

(V → [± -animate, -vehicle ____, ± ____ +directional]).

(8) appears to be a selectional restriction formalising the observation that a verb which may or may not appear in final position may or may not have an animate direct object. In Chomsky's framework selectional restrictions operate on lexical categories and, to this extent, (8) is curious. If the suggestion is adopted for the subcategorisation of V via the CS notation, the optionality of NP in the input to the rule would be removed to the benefit of the rule's intelligibility. (9) and (10) are included to make the lexicon work but no reason is provided as to why (11) and (12) are included rather than having additional 'subcategorisation' rules of the form:

Neg → [+Neg]

and

Wh-loc → [+Wh-loc]

together with entries in the lexicon such as:

enää, 'any more' [+Neg]

missä, 'where' [+Wh-loc]

as this would, at least, make for a unified treatment of lexical insertion.

The transformational rules too appear to have exactly the same problems associated with them as we found for G_{S1} . All parts of (1) involve permutation and lead to absurd derived constituent structures and one could also question some of the conditions which are attached to the rules which might be seen as inconsistent with a restrictive theory of transformations. (2) violates the condition of the

recoverability of deletions as we have already seen.

The conclusions we reach then are, in all essential respects, identical to those emerging from our discussion of G_{S1} .

The phrase-structure component does not involve any problems, there are difficulties of interpretation with the lexical feature rules and associated lexicon but these might not be insuperable and the transformational rules violate important constraints on rules of this type. We now consider some of the implications of these conclusions for (G_{S1}, G_{S2}) , considered as a developmental theory.

3.4.3 Bowerman's grammars and explanatory adequacy

The domain of enquiry, D , is again the child's syntactic competence and I shall assume that it makes sense to talk about a psychological model of this competence and that grammars may be deemed explanatory in this context.

The above discussion of the details of G_{S1} and G_{S2} can be seen as an attempt to investigate Condition 2 and while there is a certain amount of attention paid to the parent general theory in Bowerman's theorising, there is still a fair amount of neglect of fundamental issues. I assume that this requires no further elaboration and that the degree of divergence from the conditions laid down by the general theory is not so severe, at least in certain

respects, to make it impossible to move on to Condition 3.¹³¹ Bowerman's own statement on the relationship between the two Seppo grammars would lead us to expect that satisfaction of Condition 3 is going to be relatively easy to achieve. She says (p.132):

Provisions of the early grammar for Seppo remain the backbone of the later grammar. Most changes in Seppo's speech were combinations and elaborations of existing patterns.... The second grammar provides for the generation of a few constructions which are qualitatively new, not just elaborations of pre-existing patterns.

Both of these aspects of development, elaboration of existing patterns and qualitatively new constructions, can be seen as involving additional elements and to investigate the validity of these claims it will be convenient to consider Condition 3 in connection with different aspects of the grammar in turn.

Consider first the inventories of syntactic categories employed in G_{S1} and G_{S2} . The former uses the set,

$\{S_1, S_2, M, VP, Adj, V, Loc, Proloc\}$

131. Another possible justification for moving on would be that the divergences from the general theory infect both G_{S1} and G_{S2} in the same way and that, therefore, we should see both of them as constructed in accordance with some slightly different general theory. This justification is made more plausible by Bowerman's treatment of lexical insertion which doesn't accord in detail with either of the possibilities suggested in Chomsky (1965). Unfortunately, in connection with the other areas of divergence discussed above, she provides no such explicit statement.

and the latter, the set,

$$\{S_1, S_2, S_3, \text{Wh-loc, NP, V, N, Adj, VP, Predicate, Loc, Adv, M, Proloc}\}$$

Comparison of these two sets is easy if we do away with the distinctions between the differently subscripted S's. Failure to do this will require that we consider the two S's of the first set being replaced by three new S's in the second set, say S_3 , S_4 and S_5 , as a cursory inspection reveals that S_1 and S_2 in G_{S1} are not notionally equivalent to S_1 and S_2 in G_{S2} , and this will immediately entail that Condition 3 is not applicable. A version of Condition 4 could be formulated from this conclusion and, if interested, the reader can easily investigate this avenue. I shall assume, though, that the set of categories in G_{S2} is an additive expansion of the set in G_{S1} and formulate a version of Condition 4 accordingly. Referring to the two sets of categories as C_1 and C_2 , we can, on this assumption, assert the identity:

$$C_2 = C_1 \cup \{\text{Wh-loc, NP, Predicate, Adv}\}$$

and we can schematise the child's categorial development as:

$$C_1 \longrightarrow C_1 + X$$

where X denotes the set, $\{\text{Wh-loc, NP, Predicate, Adv}\}$

We can approach the sets of syntactic features in exactly the same way distinguishing, in order to keep the presentation manageable, between inherent and contextual features. The set of inherent syntactic features appearing in G_{S1} is $[\pm\text{animate}, \pm\text{vehicle}]$ (excluding those

features which correspond directly to syntactic categories) and the corresponding set in G_{S2} is $\{\pm\text{animate}, \pm\text{vehicle}, \pm\text{pronoun}, \pm\text{directional}\}$. Obviously, referring to the inherent features appearing in G_{Si} by IF_i ($i = 1, 2$) we have the identity:

$$IF_2 = IF_1 \cup \{\pm\text{pronoun}, \pm\text{directional}\}$$

and the child's development, in this respect, can be schematised as:

$$IF_1 \longrightarrow IF_1 + X$$

where X denotes the set, $\{\pm\text{pronoun}, \pm\text{directional}\}$. So again we have a positive conclusion. With respect to the set of inherent syntactic features, Condition 3 is satisfied additively by the sequence, (G_{S1}, G_{S2}) .

For contextual features, the situation is not so clear-cut. Restricting ourselves just to those mentioning lexical categories, we find that G_{S1} uses the set

$$\{\pm V \text{ ____}, \pm N \text{ ____}, \pm \text{ ____} V, \pm \text{ ____} N, \pm \text{ ____} (N)\},$$

and G_{S2} uses the set

$$\{\pm \text{ ____} \begin{matrix} V \\ N \end{matrix}, \pm NP \text{ ____}, \pm \text{ ____} (NP)\}.$$
¹³²

I think it is fair to say that there is no clear additive relationship between these two sets and the most serious discrepancy is that, whereas in the first set there is a feature subcategorising items as to whether they occur post-verbally or not, there is no identical, or even similar, feature in the second

132. Obviously the make-up of these sets will be altered considerably if the suggestions mooted above for the subcategorisation of V are implemented. I shall not pursue such possibilities here.

set. Condition 3, therefore, does not apply and again, while it would be possible to formulate a version of Condition 4 for investigation on this basis, I shall not attempt this in the subsequent discussion.

There remains one set of features which have not been mentioned, those which are used in the formulation of selection restrictions, and, as none of these appear in G_{S1} while a small number do in G_{S2} , it is clear that, with regard to them, Condition 3 is satisfied. However, since no version of Condition 4 can be constructed in this connection, I shall not discuss these features further.

There is one further aspect of the development which should be briefly mentioned and that concerns the development, not of particular features, but of feature-types. Referring to the type of contextual features as CF and the type of features used in selectional restrictions as SF, we can see that G_{S1} uses the set of feature-types, $\{IF, CF\}$ and G_{S2} uses the extended set, $\{IF, CF, SF\}$. Therefore, if the set of feature-types in G_{Si} is FT_i ($i = 1, 2$), we can assert the identity:

$$FT_2 = FT_1 \cup \{SF\}$$

and this aspect of development can be schematised as:

$$FT_1 \longrightarrow FT_1 + X$$

where X denotes the set, $\{SF\}$. We conclude that, with respect to the set of feature-types appearing in the grammars, the sequence, (G_{S1}, G_{S2}) , satisfies Condition 3 additively. Overall, as far as substantive aspects of

the grammars are concerned, (G_{S_1}, G_{S_2}) does well against Condition 3 with only contextual features providing problems for a straightforward additive interpretation of development. We now turn to rule-types and rules and see whether it attains a similar degree of success there.

For rule-types there doesn't appear to be any interesting development in the sequence. It might be suggested that, within the set of lexicon feature rules, a new type emerges, viz., selectional restrictions, but this would be to say nothing more than has already been pointed out with respect to feature-types. It is also the case that the elementary operations involved in the transformational rules remain the same in the two grammars and we find no transformations using adjunction and/or substitution of new material which might lead us to investigate a change in transformational rule-type. So, we consider Condition 3 with respect to sets of particular rules.

Consider the sets of phrase-structure rules from G_{S_1} and G_{S_2} . In order to have a clear picture it is necessary to expand the rules following the usual notational conventions and the rules of G_{S_1} expand as in Figure 54 (ignoring the differences between subscripted S's in the original).

Expansion of the phrase-structure rules in G_{S_2} yields the set shown in Figure 55 (For Figures 54 and 55 see p.257.)

Of the rules occurring in Figure 54, 1.4, 1.6, 1.7, 1.8, 1.11 and 1.12 also appear in Figure 55 but the remainder

1.1	S	→	M
1.2	S	→	N
1.3	S	→	M + N
1.4	S	→	VP
1.5	S	→	N + VP
1.6	M	→	Adj
1.7	M	→	N
1.8	VP	→	V
1.9	VP	→	V + N
1.10	VP	→	Loc
1.11	Loc	→	N
1.12	Loc	→	Proloc

Figure 54,

2.1	S	→	Wh-loc + NP
2.2	S	→	Wh-loc + V
2.3	S	→	Neg + N
2.4	S	→	Neg + V
2.5	S	→	Neg + Adj
2.6	S	→	NP
2.7	S	→	VP
2.8	S	→	NP + VP
2.9	VP	→	Predicate
2.10	VP	→	V
2.11	VP	→	V + NP
2.12	VP	→	V + Loc
2.13	VP	→	V + Adv
2.14	VP	→	V + NP + Loc
2.15	VP	→	V + NP + Adv
2.16	VP	→	V + Loc + Adv
2.17	VP	→	V + NP + Loc + Adv
2.18	Predicate	→	Adj
2.19	Predicate	→	N
2.20	Predicate	→	Loc
2.21	NP	→	N
2.22	NP	→	M + N
2.23	M	→	Adj
2.24	M	→	N
2.25	Loc	→	N
2.26	Loc	→	Proloc

Figure 55

don't. This means that we cannot view the phrase-structure rules in Figure 55 as a simple additive extension of the rules in Figure 54. Nevertheless, for most of the rules which cause problems, it is possible to see their effects being achieved by a sequence of rules in Figure 55 and, in this sense, we may regard these rules as having a derivative status in the second grammar. Thus, if we consider the rule, 1.2, what this rule does is generate strings consisting of single nouns and assign them to the category, S. The same effect is achieved, in the second set of rules, by the sequence of rules, 2.6 and 2.21, which has the additional effect of assigning the noun to the category of NP. It seems to me not to do too much injustice to our intuitions of additive complexity to say that the sequence of rules, 2.6 and 2.21, can be seen as including the rule, 1.2, and also adding to it.¹³³ Note that, in this case, the addition in question, revolving around the introduction of the category, NP, has already been raised in our discussion of developments in the set of syntactic categories. Similar arguments can be used in other cases. 1.3 can be seen as included in 2.6 and 2.22, 1.5 in 2.8 and 2.21, 1.9 in 2.11 and 2.21

133. This may be rendered more convincing by considering the "is a" relation defined on phrase-structure trees. Corresponding to every phrase-structure rule of the form, $XAY \longrightarrow XBY$, there is, in an appropriately constructed tree, a string, B, and a node, A, such that B is an A. 1.2 guarantees that, in cases, N is an S and so does the sequence of rules, 2.6 and 2.21.

and 1.10 and in 2.9 and 2.20. In each of these cases, the sequence of rules in G_{S_2} introduces additional structure, when compared to the single rules from G_{S_1} , involving either of the categories, NP or Predicate, and, of course, both of these are introductions into the set of categories used in G_{S_2} when this is compared with the set used in G_{S_1} . A problem still remains and that is the position of the rule, 1.1, which, in no sense, exists in G_{S_2} . It seems that, in this case, we have to assume that the child completely abandons the rule rather than using it as a basis for later development and, while such an assumption is intelligible, it would be instructive to see it discussed. Obviously this fact, in itself, entails that Condition 3 is not applicable to this aspect of the development but I shall ignore 1.1, accept the equivalence of single rules in G_{S_1} with sequences of rules in G_{S_2} as outlined above and conclude that the sets of phrase-structure rules, PS_1 in G_{S_1} and PS_2 in G_{S_2} , do satisfy Condition 3 and that we have:

$$PS_2 = PS_1 \cup \{2.1, 2.2, 2.3, 2.4, 2.5, 2.11, 2.12, 2.13, 2.14, 2.15, 2.16, 2.17, 2.18\}$$

and that we have development which can be schematised as:

$$PS_1 \longrightarrow PS_1 + X$$

where X denotes the set of phrase structure rules, $\{2.1, 2.2, 2.3, 2.4, 2.5, 2.11, 2.12, 2.13, 2.14, 2.15, 2.16, 2.17, 2.18\}$.

The lexicon feature rules require less lengthy discussion. Taking the rules in G_{S_1} in turn, (1) is preserved, as far as its function is concerned, by (1) and (3) from G_{S_2} ;

(2) remains as (4) in G_{S_2} but there is no place in the later grammar for anything resembling either (3) or (4) and it is impossible to see their function as being distributed between a set of new rules; (5) resembles (7) in G_{S_2} in important respects and the latter can plausibly be regarded as an elaboration of the former (we note again here the substitution of NP for N which we have met in the phrase-structure rules); (6) survives as (9), (7) is expanded into (5) in an obvious way and, finally, (8) perishes but, since it was never clear why it was construed as a lexical feature rule to begin with, this shouldn't be regarded as crucial. The main difficulties for an additive notion of complexity reside in the effective disappearance of (3) and (4) from G_{S_1} and, because of this, we conclude that Condition 3 does not apply to these sets of rules.

This leaves the transformational rules of which there are two in G_{S_1} , the first of which has two parts. (1(a)) seems to disappear entirely in G_{S_2} (this is, perhaps, hardly surprising given its totally ad hoc nature). Concentrating on its function, for pois, this could be seen as being taken over by (1(c)) in G_{S_2} which reverses the order of a verb (in this case, pois) and a following noun, but, unfortunately for this tactic, kiinni is not even classified syntactically in the lexicon for G_{S_2} and so can never enter into a structure which will be input to one of the transformations and, thus, it is impossible to see (1(c)) in G_{S_2} as an extension of (1(a)) in G_{S_1} .

(1(b)) of G_{S1} can be seen as growing into (1(a)) of G_{S2} , the only modification here being the by now familiar one of NP replacing N in the structural description of the rule. Finally, (2) of G_{S1} is obviously related to (2) of G_{S2} , the latter comprising an extension of the former by way of loosening the specification of its structural analysis to allow the possibility of locatives in the third term. If we ignore the first of these three rules, we can conclude that the two sets of transformational rules satisfy Condition 3 additively but, of course, whether we are entitled to ignore this rule is open to question. If it is included then Condition 3 does not apply and this will lead to a different formulation of Condition 4 to that briefly considered below.

I shall now consider Condition 4 for a small number of examples which have satisfied Condition 3.¹³⁴ Consider first syntactic categories. Condition 4 requires that we demand a reason for why we find the development schematised as:

$$C_1 \longrightarrow C_1 + X$$

rather than the alternative:

$$X \longrightarrow C_1 + X$$

134. Some components of the overall theory will only be mentioned briefly without analysis. Similarly, for those components of the theory to which Condition 3 did not apply, while it would be possible to consider Condition 4, I shall not do so. The essentially negative conclusions reached with regard to Condition 4 in the text are readily exten^atable to these other cases.

where C_1 denotes the set of categories, $\{S, M, N, VP, Adj, V, Loc, Proloc\}$, and X denotes the set of categories $\{Wh-loc, NP, Predicate, Adv\}$. For a number of pairs of categories we have the basis for a logical explanation. So, as noted in connection with Bloom's grammars, it can be maintained that the phrasal category, NP, presupposes, within the theory, its head category, N, and thus we have an explanation for why N preceded NP into the set of categories manipulated by the child. One could speculate along rather different lines for the relationship between V and Adv where the latter category only makes notional sense within a system embracing the former category and along still different lines for the categories, Wh-loc and Loc, arguing that the more specific category requires acquaintance with the general category. However, there is little point in pushing such speculations too far as, in general, no logical basis for the development is apparent. Why, for example, should Predicate follow M in the development of the set of categories? It is difficult to conceive of the theory which would have this follow from its own structural make-up. Nor is much hope attached to an examination of the syntactic categories of the world's languages. To my knowledge no reasonably well-attested universals, absolute or implicational could be brought to bear on the sets of categories we are scrutinising. It is easy to see what such universal statements would be like, e.g. 'All languages have a grammatical category, M but not all languages have a grammatical category, Predicate',

or, 'If a language has a grammatical category, M, then it has a grammatical category, Predicate' but, as well as being unattested, such statements are unlikely. This leaves the second possibility of Condition 4, that there might be some more basic theory to which we can reduce the linguistic development, but such a theory does not readily suggest itself. In particular, if we consider cognition and continue to focus on the same pair of categories, I know of no claim that those properties of objects which are usually encoded by attributives are cognised earlier than those properties which are normally encoded by predicates.¹³⁵ Note that several claims of this sort would have to be made and substantiated before we began to take the possibility of a reduction of this grammatical phenomenon seriously. We are led to the conclusion, therefore, that, with regard to the development of syntactic categories, the sequence (G_{S1} , G_{S2}) fails to satisfy Condition 4. A similar conclusion can rapidly be reached with respect to inherent syntactic features.

For feature-types, we require a reason for why we find the development schematised as:

$$FT_1 \longrightarrow FT_1 + X$$

135. In fact, what evidence is available would tend to support the opposite prediction as children are known to be more interested in action and change than static attributes and it is the former of these which are normally encoded by grammatical Predicates (cf. Nelson's theory discussed in Chapter 2).

rather than the alternative:

$$X \longrightarrow FT_1 + X$$

where FT_1 denotes the set of feature-types, $\{IF, CF\}$, and X denotes the set of feature-types, $\{SF\}$. There is a partial answer to this built in to the logical structure of the theory as the type of selectional features, SF , depends for its cogency on the prior introduction of inherent features, i.e., the type of selectional features can be seen as theoretically presupposing the type of inherent features and development which included the ' schema:

$$SF \longrightarrow SF + IF$$

would be quite impossible in this sort of theory. Unfortunately, there is no comparable reason for why the type of contextual features should have precedence over the type of selectional features. Thus, this aspect of syntactic development is only partially grounded in the logic of the theory. Nor does it seem plausible to suppose that either facts concerning the distribution of feature-types in the world's languages or reference to some more basic theory will be particularly useful in this connection. Restricting attention to the development of syntactic feature-types, the sequence (G_{S1}, G_{S2}) fails to satisfy Condition 4 in a general way, although there are logical grounds for partial satisfaction.

The sets of phrase-structure rules in the two grammars give rise to the question as to why we find the development

schematised as:

$$PS_1 \longrightarrow PS_1 + X$$

rather than the alternative:

$$X \longrightarrow PS_1 + X$$

where PS_1 denotes the set of rules, 1.2 - 1.12, from p 257 and X has the same reference as it does on p258. Again no totally satisfactory answer is forthcoming in terms of the framework we are working with. The logical approach demands that we investigate the intrinsic ordering of the rules in X when compared with the rules in PS_1 , and, although some of the rules in X do depend on rules in PS_1 for their intelligibility (e.g., 2.12 - 2.17 are intrinsically ordered with respect to 1.4 and 1.5), this is not generally the case (e.g., 2.1 - 2.5 in X are not intrinsically ordered with respect to any of the rules in PS_1). So, again we have partial satisfaction of Condition 4 on logical, theory-internal grounds and, again, there is little point in turning to the other clauses of Condition 4 in the hope of enlightenment. Statistical facts are unknown but probably useless, and while it would be possible to refer to a theory of cognition in making piecemeal observations to the effect that rules introducing negative and questioning elements appear in X but not in PS_1 , such observations would be piecemeal, they would not constitute a systematic attempt to relate the terms of the linguistic theory to those of a cognitive theory and, inevitably, many aspects of the development would be omitted from such speculation.

Finally, consider the transformational rules. Why is it that we get the development schematised as:

$$T_1 \longrightarrow T_1 + X$$

rather than the alternative schematised as:

$$X \longrightarrow T_1 + X$$

where T_1 denotes the set of two transformational rules, (1(b)) and (2), from G_{S1} (we continue to ignore (1(a))) and X denotes the set of rules, $\{(1(b), 1(c) \text{ and } 1(d))\}$, from G_{S2} ? In this case we have no basis for answering the question. The five rules are devoid of any intrinsic ordering relationships between them, given that they are not well-formed examples of transformational rules, we are not going to find them cropping up in theories of the world's languages and the nature of a more basic, e.g. cognitive, theory to which they could be related defies imagination. With respect to the sets of transformational rules appearing in the grammars, the sequence, (G_{S1}, G_{S2}) , fails to satisfy Condition 4.

It seems reasonable to conclude that overall Bowerman's theory of Seppo's syntactic development falls down crucially on Condition 4. We have seen that the sequence, (G_{S1}, G_{S2}) does quite well on Condition 3 and that a much greater effort is made to pay attention to general theory, and hence Condition 2, than was the case in Bloom's work. There are still serious grounds for misgiving in connection with Condition 2, particularly concerning the transformational component of the grammars, but what the failure to approach Condition 4 means is that, even if we have a theory which

works in a reasonably satisfactory way and gets complicated as the child gets complicated, we are completely in the dark as to why it works. It is a theory which, in terms of the notions being explored here, fails to be explanatorily adequate.

3.5 Derivational complexity and the acquisition of transformations.

Brown and Hanlon (1970) is a piece of research which, on the face of it, is concerned with a direct investigation of Condition 3 within the domain of syntactic competence. However, closer examination reveals that the theoretical proposals the authors adopt are not as fully interpretable, with regard to this condition as might originally be thought.

In their own terms they are interpreting the derivational theory of complexity, put forward and manipulated in experimental psycholinguistics, in a developmental sphere. At its crudest this theory claims that the more grammatical 'operations' that are involved in the generation of a sentence in a grammar, the more complex that sentence will be psychologically. Psychological complexity, typically, is indexed by some measure like time to respond with the truth-value of a generic sentence, time to respond in a sentence content-picture content matching task, ability to remember a sentence verbatim, etc. (for an exhaustive survey of the work in this area, see Fodor, Bever and Garrett (1974)). In this crude form the derivational

theory of complexity requires the assumption that each grammatical 'operation' contributes equally to psychological complexity in order to give rise to experimental predictions. This assumption has not been an attractive one for psycholinguists. Because of this, a more refined version of the theory, the version investigated by Brown and Hanlon, has been developed which may be referred to as the derivational theory of cumulative complexity according to which, a sentence, S_1 , is predicted to be psychologically more complex than a sentence, S_2 , if the generation of S_1 in the grammar requires all the operations employed in the derivation of S_2 plus some additional ones. This can, of course, be compared to our discussion of simplicity in Chapter 1 which gravitated towards the additive notion of simplicity because we had no way of weighing the relative simplicity of distinct theoretical entities against each other.

Brown and Hanlon are concerned with eight sentence-types which are listed in Figure 56.

- | | | |
|---------|---|---|
| 1. SAAD | - | <u>We had a ball</u> |
| 2. Q | - | <u>Did we have a ball?</u> |
| 3. N | - | <u>We didn't have a ball</u> |
| 4. Tr | - | <u>We did</u> |
| 5. NQ | - | <u>Didn't we have a ball?</u> |
| 6. TrQ | - | <u>Did we?</u> (also used in positive tag questions) |
| 7. TrN | - | <u>We didn't</u> |
| 8. TrNQ | - | <u>Didn't we?</u> (also used in negative tag questions) |

Figure 56 Adapted from Brown and Hanlon, pp.18 - 19. SAAD is simple, active, affirmative declarative, 'Q' is question, 'N' is negative and 'Tr' is truncated.

In connection with these sentence-types and positive and negative tag questions, the authors formulate a set of transformational rules, which, while not identical to those found in any linguistic treatment of the sentences, explicitly indicates allegiance to such works as Klima (1964), Katz and Postal (1964) and Chomsky (1965). No particular set of phrase-structure rules is assumed and the seven transformational rules are:

- TI. Tag-question formation (optional)
- TII. Predicate truncation schema ¹³⁶ (optional)
- TIII. Preverbal placement of neg (obligatory)
- TIV. Question transformation (obligatory)
- TV. Affixation (obligatory)
- TVI. Do-support (obligatory)
- TVII. Negative contraction (optional)

The reader is referred to p.22 of Brown and Hanlon's article for a detailed presentation of each of these rules. In introducing them, they say (p.21):

We...offer [these rules as] a set of imperfect rules based on what we have learned from what has been written and on what we have been able to work out on our own.

136. Brown and Hanlon recognise that their formulation of this rule violates the condition of recoverability of deletions and this is why it is dubbed 'schema'. The implications of this for the status of the set of formal operations, TI - TVII are obvious. For recent views that this sort of process is best handled in a different component of a linguistic theory - a discourse grammar - see, e.g. Sag (1976), Williams (1977).

and a word or two of amplification is in order with respect to their status. TI has two parts both of which are essentially copying rules with one being responsible for the generation of negative tags and the other with positive tags. As well as copying the original sentence, the rules introduce a feature, [+pro], to the subject NP in the new portion and, if a main verb is present, it is deleted in the tag. TII has already been mentioned in fn. 136 but I might also point out that its optional status is problematic given that it contains a term 'in the structural analysis of the rule, Pro, which has the function of triggering the operation of the rule. TIII is a version of the familiar rule of Neg-placement but the formulation leaves open the question as to exactly how the neg is adjoined to the right of the auxiliary constituent.¹³⁷ TIV is intended to be a version of subject-auxiliary inversion but is odd in that the term, Q, should not survive on the right-hand side of the rule as this amounts to letting it appear in terminal strings unless, of course, a later rule is going to delete it. TV is based on the well-known rule of affix-hopping, identical in most respects to the version formulated

137. The formulation of all the rules leaves open questions of derived constituent structure. Perhaps this is understandable for a topic which was so badly neglected by a large number of transformational linguists but it is difficult to talk about the rules representing 'structural knowledge' unless we take it seriously.

in Chomsky (1957), except that the -en suffix of perfect aspect is missing from the structural analysis of the rule.¹³⁸ The rule of Do-support is the well-known one and, finally, TVII, the rule of negative contraction, while clear enough in function, doesn't give us any reason for why it should work. What the rule does is move a negative element out of one term of the structural analysis and adjoin it to the right of the Tense morpheme but there is no indication in this process that the negative will thereby be contracted. Presumably the answer lies in the morphophonemic rules but Brown and Hanlon don't formulate any of these and so the reader is left very much in the dark. TI - TVII thus contain some obscurity but, putting this on one side for the moment, what can we say of the role they play in the eight sentence types plus positive and negative tags? The authors suggest that the transformations involved in the generation of each sentence-type are as shown in Figure 57.

(For Figure 57 see p 272)

From these analyses it is possible to derive a number of predictions concerning relative psychological complexity in accordance with the derivational theory of cumulative

138. It is a general feature of these rules that perfect aspect is neglected. Thus TI - TIV should each have an option for have in their structural analyses and Brown and Hanlon don't discuss why this doesn't appear.

SAAD	<u>We had a ball</u>	TV
Q	<u>Did we have a ball?</u>	TIV, TVI
N	<u>We didn't have a ball</u>	TIII, TVI, TVII
Tr	<u>We did</u>	TII, TVI
NQ	<u>Didn't we have a ball?</u>	TIII, TIV, TVI, TVII
TrN	<u>We didn't</u>	TII, TIII, TVI, TVII
TrQ	<u>Did we?</u>	TII, TIV, TVI
Tag	<u>We didn't have a ball, did we?</u>	TI, TIII, TIV, TVI (twice) TVII
TrNQ	<u>Didn't we?</u>	TII, TIII, TIV, TVI,, TVII
NTag	<u>We had a ball,didn't we?</u>	TI, TIII, TIV, TV, TVI, TVII

Figure 57

complexity.¹³⁹ The psychological measure investigated is the age at which the children show sufficient evidence to credit them with control of the grammatical knowledge involved in each of the sentence-types and the claim is,

139. Strictly speaking, SAAD's cannot enter into these predictions as they involve TV, and none of the others, with the exception of NTag's, do. Brown and Hanlon's discussion of TV and its interaction with TVI is confusing. They say (p23): "[TVI] is not applied in the derivation of SAAD sentences, but is applied in Q, N, and Tr sentences whenever the only auxiliary is "tense". So TVI is a rule which is added to the child's grammar with Q, N, and Tr. Nevertheless, it does not strictly operate so as to make one sentence more complex, cumulatively, than another, because, whenever it is not applied, TV must be." There is nothing here to suggest that SAAD's and other sentence types can be cumulatively compared.

to take a particular example, that TrNQ sentences will be acquired later than N sentences because, in their grammatical derivation, they involve every rule involved in the latter sentence-type plus some additional ones, in this case, TII and TIV. These predictions are spectacularly confirmed by an analysis of the data collected from the three children in Roger Brown's longitudinal study. 19 predictions are made for each child and, of 57 individual predictions, 47 are confirmed, 6 cannot be evaluated and only 4 go in the wrong direction.¹⁴⁰

Before assessing the general significance of these results in terms of the framework of Chapter 1, it is necessary to remark on one additional point concerning the correlation of psychological and derivational complexity, a point which Brown and Hanlon are aware of. If we consider some pairs of sentences in the list of Figure 56, not only do they differ with regard to their transformational derivations but also with regard to the structures which are input

140. Actually these figures are somewhat inflated as all the predictions are not independent. If, say, one predicts correctly that SAAD sentences will appear before N sentences and that N sentences will appear before NQ sentences, one can hardly cite an independent correct prediction that SAAD sentences will appear before NQ sentences. Taking account of this, there are only 36 independent individual predictions of which 26 are confirmed. Removing SAAD sentences from the predictions as fn.139 urges reduces this still further to 27 predictions of which 17 are confirmed. As usual, the extent of empirical support for the theory is not my main concern.

to the transformational rules. Thus, comparing SAAD sentences with Q sentences, it is not simply the case that SAAD sentences only involve the application of TV whereas Q sentences involve the application of TIV and TVI. In addition, a Q sentence has a Q-morpheme generated as part of the structure which is input to the transformational rules and it is implausible to argue that this doesn't contribute to the overall psychological complexity of Q sentences, particularly as it is semantically significant. But, if this is so, we are on very unsure ground if we try to identify the increased psychological complexity of Q sentences with complications in sets of transformational rules - the complexity could reside in one or both of two aspects of the derivation (at least) and we have no way of identifying which.

We must try to cast Brown and Hanlon's proposals into a form which makes them amenable to analysis in terms of Conditions 1 - 4. Ignoring much of the above criticism, their views can be seen as imposing a partial ordering on the eight sentence-types as shown in Figure 58.

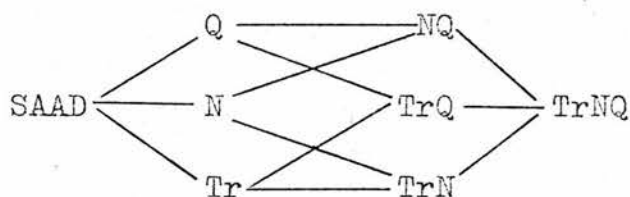


Figure 58

and what this amounts to is a constraint on the possible orders of acquisition of sentence-types such that the

ordering:

(SAAD, Q, N, Tr, TrQ, NQ, TrN, TrNQ)

is possible, whereas the ordering:

(SAAD, Q, N, TrQ, Tr, NQ, TrN, TrNQ)

is not. The theory which is to explain this is one which assumes a domain of enquiry, D, the child's syntactic knowledge, and that the possession of syntactic knowledge can be explicated in terms of the child controlling, in some sense, transformational rules. It is sets of transformational rules which comprise the (partial) theories in which we are interested. Under all possible orders of acquisition of the sentence-types, SAAD sentences are acquired first. SAAD sentences require the operation of only one transformational rule, TV, and so we can say that there is a time, t_1 , at which the transformational component of the child's grammar contains only the rule, TV. Next, however, the child may acquire any of the sentence-types, Q, N, and Tr and, depending on which he does acquire, he begins to use the transformational rules, TIV, and TVI (for Q), TIII, TVI, and TVII (for N), or TII and TVI (for Tr). But, of course, these possibilities exhaust the set of transformations under discussion except for TI.¹⁴¹ So, all we can conclude is that there is a time, t_2 , at which the child not only utilises TV but also

141. Brown and Hanlon, commenting on similarities in the structural analyses of transformations involved in the generation of Q, N and Tr sentences, point out, reinforcing the point in the text, that these three sentence-types are learned at about the same time.

TII, TIII, TIV, TVI and TVII (I omit any further discussion of TI) and we have development which we can schematise as:

$$\begin{array}{ccc} \{TV\} & \longrightarrow & \{TII, TIII, TIV, TV, TVI, TVII\} \\ t_1 & & t_2 \end{array}$$

The reason we get this rather unexciting outcome is that the three sentence-types Q, N and Tr are not ordered relative to each other in the partial ordering and that, in their derivations, they use the full set of transformations under discussion. Unexciting or not, we have a developmental theory against which we can test our conditions.

Ignoring Condition 1, Condition 2 has already been discussed in some detail above. Although several difficulties of detail were alluded to there, the only urgent question to my mind surrounds TII and its violation of the condition of recoverability of deletions. TIV, as presented, looks like a permutation transformation but, generally, there appears to be a somewhat closer adherence to standard practice than in the previous instances we have discussed. Condition 3, the one which first impressions suggested might prove very interesting, is satisfied in a rather unexciting fashion as the above schema demonstrates.

What about Condition 4? The question it raises is whether we are provided with any reason as to why we find the development schematised as:

$$T_1 \longrightarrow T_1 + X$$

rather than the alternative:

$$X \longrightarrow T_1 + X$$

where T_1 denotes the set of transformational rules, $\{TV\}$, and X denotes the set of transformational rules, $\{TII, TIII, TIV, TVI \text{ and } TVII\}$. Turning immediately to questions of intrinsic ordering, we can see that TV is intrinsically ordered before $TVII$ but that it is ordered after TII , $TIII$ and TIV , while not interacting with TVI . In a case such as this the dubious value of claiming that the theory is even partially grounded in the structure of the grammatical theory is quite apparent. Nor do either of the other two possibilities for grounding the theory seem to have anything to offer. Once more the facts about the distribution of rules in the world's languages are not known but it seems likely, and in cases where particular lexical items are mentioned, it is certain, that the rules, TII - $TVII$ are specific to English and, therefore, the question of their distribution in the languages of the world simply does not arise. Nor does there appear to be any plausible cognitive or perceptual reason for why TV should appear first in the grammars of the children.

The conclusion is clear that Brown and Hanlon's theory concerning the development of a restricted set of sentence-types, interpreted as a theory of the successive acquisition of transformational rules, while, perhaps, satisfying Conditions 1 - 3 in a more or less convincing fashion, fails totally to satisfy Condition 4. As far as

the theory is concerned, it would be equally plausible for the child to learn most of the transformations in X before learning TV. The theory is thus judged to be lacking in explanatory adequacy.

3.6 Fourteen grammatical morphemes

The logic underlying the study investigated in the previous section has been extended by Brown, in the second half of his monumental work on the early stages of language acquisition, to the development of fourteen grammatical morphemes, paying particular attention to the order of acquisition within the set and to possible determinants of this order. Cumulative complexity occupies a central place throughout the discussion (see Brown (1973, pp.289ff)). The fourteen morphemes investigated are: the present progressive inflection, the regular past tense morpheme, the irregular past tense morpheme, the regular third person singular present indicative morpheme, the irregular third person singular present indicative morpheme, the regular plural inflection, the possessive morpheme, the preposition, in, the preposition, on, the 'article morpheme' (a notion which does not distinguish the indefinite from the definite article), the uncontracted copula, the contracted copula, the uncontracted auxiliary and the contracted auxiliary. The constitution of this list and the arguments for splitting up some categories while leaving others intact will not concern us here (see Brown, pp. 300 - 313, for details). Given a criterion for acquisition whereby a

morpheme is deemed to be acquired when it appears in 90% or more of obligatory contexts, Brown, averaging rankings over three children, produced the summary of order of acquisition shown in Figure 59 and considered three possible determinants of this order: frequency of the morphemes in parental speech, semantic complexity of the morphemes and grammatical complexity of the morphemes, and it seems fair here to assume that a demonstration that one of these is positively correlated with the order of acquisition would amount to at least a partial theory of morphological/grammatical development in this restricted area.

<u>Morpheme</u>	<u>Average Rank</u>
1. Present progressive	2.33
2-3. <u>in</u> , <u>on</u>	2.50
4. Plural	3.00
5. Past irregular	6.00
6. Possessive	6.33
7. Uncontractible copula	6.50
8. Articles	7.00
9. Past regular	9.00
10. Third person regular	9.66
11. Third person irregular	10.83
12. Uncontractible auxiliary	11.66
13. Contractible copula	12.66
14. Contractible auxiliary	14.00

Figure 59 From Brown, p 317

From the outset, it should be pointed out that Brown, quite sensibly, construes complexity, both syntactic and semantic, cumulatively, although, of course, in this case the elements which are contributing to the complexity of an item are not exclusively transformational rules as was the situation in 3.5. This, necessarily, restricts the scope of this investigation and, in the analysis that follows, I am anxious that the scope should not be restricted in this way. It will be seen that Brown's considerations of cumulative complexity emerge as special cases in the treatment I shall give. The important thing to be clear about is that many of the suggestions I shall discuss are not Brown's but represent the sort of claim that someone, adopting either the semantic or syntactic proposals developed by Brown, would have to adopt if they were to argue that those proposals provide full explanations of the order of acquisition in Figure 59. Brown explicitly refuses to speculate along these lines on a number of occasions.

Of the three possible determinants of acquisition order, I shall consider only semantic complexity (in 3.6.1) and syntactic complexity (in 3.6.2). Frequency in parental speech was demonstrably not a determinant of this order but, even if it had been, it would have been difficult to construct a theoretical statement around it that would be amenable to analysis in terms of the conditions of Chapter 1. Of the other two both were reasonably successful

within the limited terms of reference set by Brown and are, therefore, considered as developmental theories in this domain.¹⁴²

3.6.1 Semantic complexity as a determinant of acquisition order

The nature of the semantic theory Brown resorts to is obscure. It appears to consist of little more than a set of notional semantic categories which play a loose role in characterising the meanings of the morphemes. Within the full set of morphemes, there are four pairs which don't involve any semantic contrast; regular past and irregular past, uncontractible copula and contractible copula, third person regular and third person irregular, and uncontractible auxiliary and contractible auxiliary. Taking the view that the acquisition of one member of each of these pairs should signal the acquisition of the relevant semantic notions which are vital to both members of the pair, we reduce the number of morphemes in the developmental order to ten with each of the pairs above being represented by the morpheme which is acquired earlier as in Figure 60¹⁴³

142. Strictly speaking, consideration of semantic complexity should await discussion until Chapter 4. but, as the logic of the argument for semantic complexity is identical to that for syntactic complexity, I prefer to see it treated here and as providing something of a bridge between this chapter and the next one.

143. This reduction immediately indicates that semantic complexity cannot be the sole determinant of acquisition order as Brown himself points out.

1. Present progressive
- 2.5. in
- 2.5. on
4. Plural
5. Past irregular
6. Possessive
7. Uncontractible copula
8. Articles
9. Third person regular
10. Uncontractible auxiliary

Figure 60

For each of these ten morphemes, Brown provides a discussion of their semantics in notional terms, essentially trying to isolate the dimensions of meaning which they encode. These discussions lead to Figure 61 where each morpheme is paired with its notional semantic definition.

<u>Morpheme</u>	<u>Meaning</u>
1. Present progressive	Temporary duration; (process-state)
2.5. <u>in</u>	Containment
2.5 <u>on</u>	Support
4. Plural	Number
5. Past irregular	Earlierness
6. Possessive	Possession
7. Uncontractible copula	Number; earlierness
8. Articles	Specific-nonspecific
9. Third person regular	Number; earlierness
10. Uncontractible auxiliary	Temporary duration; number; earlierness; (process-state)

Figure 61. From Brown p 240¹⁴⁴

144. Some remarks are in order to assist interpretation here. The child's usage is taken as the yardstick against which the various dimensions are matched and the entries we have in the 'Meaning' column should be taken as based on this usage and not on adult usage. So, for example, as well as signifying 'earlierness' the past tense morpheme can be used in English to indicate 'hypotheticalness' or some such when it is used in conditional clauses. However, there were no conditional clauses in the speech of the children at this time, and so this aspect of the past tense morpheme's meaning was not credited to the child. For the present progressive morpheme there was clear evidence that the child intended to refer to the temporary duration of events but no clear evidence that he controlled a semantic distinction between processes and states, although this additional possibility could not be ruled out. In, on, Plural and possessive are self-explanatory. For the uncontractible copula the claim is that it encodes number (redundantly this being marked in almost all cases in the subject of the sentence) and 'earlierness' because there is a contrast between the present and past tense forms of the copula. Therefore, appropriate use of the copula presupposes acquaintance with these two semantic dimensions. Drawing heavily on the work of Karttunen (e.g. (1968)) and Maratsos (1976), Brown suggests that the children's use of articles demands that they be credited with knowledge of the specific-non-specific dimension and, finally, the entries for third person regular and uncontractible auxiliary can be analysed and justified in the same way as for the uncontractible copula.

Development in this domain of grammatical morphemes can therefore be seen, partially, at any rate, as involving the successive acquisition and combination of semantic dimensions (cf. Clark's proposals in Chapter 2). We can represent this development as a nine-stage process - not ten because in and on occupy the same position in the ranking - as shown in Figure 62. (see p.284)

The development is now in a form to which Conditions 1 - 4 can apply where we, presumably, regard the domain of enquiry, D, as being fixed as something like the semantic knowledge (in the form of a partial lexicon) which makes the child's production and comprehension of the fourteen grammatical morphemes possible and this imprecise characterisation is all we can offer towards satisfaction of Condition 1.

Condition 2, not surprisingly as we return to the domain of semantics, is not satisfied in any clear way. We are not provided with anything like a theory of possible semantic dimensions which could be seen as informing the child's progress from T_1 to T_9 . Nevertheless, because of the extremely simple relationship between the successive theories in the sequence, it is possible to compare them and investigate the satisfaction of Condition 3.

A moment's inspection reveals that Condition 3 is satisfied over the whole sequence as each T_i contains everything contained in each T_{i-1} ($2 \leq i \leq 9$) and, in addition, something else. The exact nature of what is added in the transition

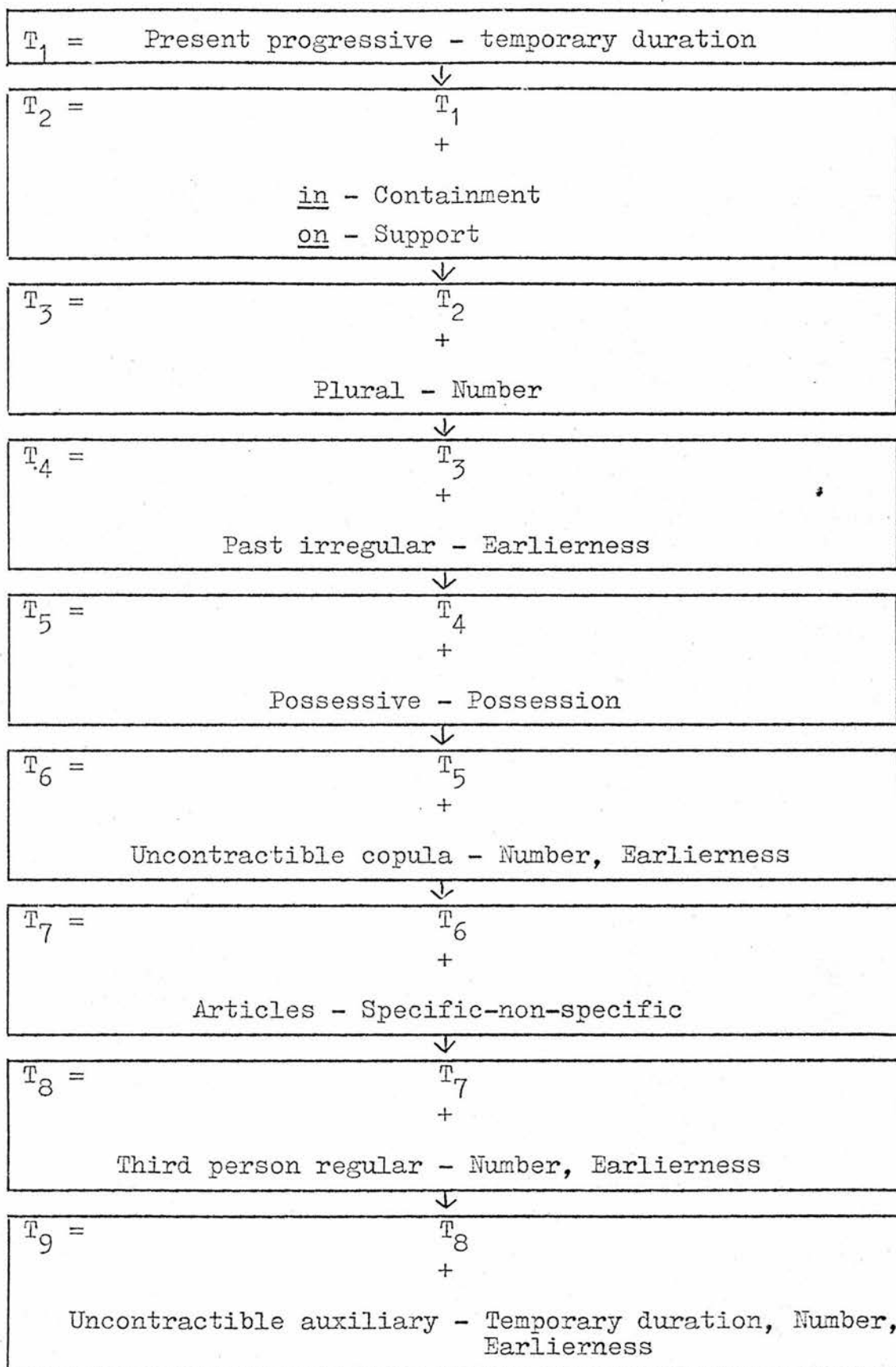


Figure 62 145

145. Note that here I have ignored the possibility of including Process-state as a semantic dimension as it has no bearing on subsequent discussion. It should be pointed out that the reason a stage-type theory like this can be constructed in this case whereas it couldn't for cumulative transformational complexity in 3.5 is that Brown does supply the order of acquisition for the grammatical morphemes averaged across the children whereas Brown and Hanlon didn't do this for the sentence-types they were interested in.

varies. It may, as in the transitions from T_1 to T_2 , from T_2 to T_3 , from T_3 to T_4 , from T_4 to T_5 and from T_6 to T_7 , involve a new semantic dimension. On the other hand, it may simply involve the combination of semantic dimensions which already have some status in the system, in a novel way. This latter possibility is demonstrated by the transitions from T_5 to T_6 , from T_7 to T_8 and from T_8 to T_9 . We can expect Condition 4 to be approached differently under these two sets of circumstances.

Consider, then, those transitions which introduce a new combination of semantic dimensions already present in the system. In each case, referring to the combination by Y and to the semantic dimensions found in the theory previous to that in which the combination first appears by X , Condition 4 leads us to ask whether there is any reason for finding the development which we can schematise as:

$$X \longrightarrow X + Y$$

rather than the alternative:

$$Y \longrightarrow X + Y$$

Taking a particular example, for the transition from T_5 to T_6 , X denotes the set of semantic dimensions, {Temporary duration, Containment, Support, Number, Earlierness, Possession}, and Y denotes the set, {Number + Earlierness}. It might be thought that this progression could be grounded in the logic of the theory but this is not so because, while it is true that for the child to use the combination of semantic dimensions, Number + Earlierness, he must, in

some sense, be acquainted with each of the components, it doesn't follow that he needs to have put those components to use singly in his semantic system. Nevertheless, with the additional assumption that the child must use dimensions singly before he uses them in combination, the transition from T_5 to T_6 could be seen as grounded in the logic of the theory and it is significant that it is this sort of case which lends itself to analysis in terms of cumulative semantic complexity. Because of this, Brown feels able to conclude that the fact that the Uncontractible copula follows Plural and Past irregular in the acquisition order is determined by semantic complexity. Much the same goes for the transition from T_8 to T_9 where it is necessary to assume that the child can only use combinations of three dimensions after he has used combinations of two but the transition from T_7 to T_8 is rather more problematic. In terms of the above schematisation, what we have is that X denotes the set of dimensions and dimension combinations, {Temporary duration, Containment, Support, Number, Earlierness, Possession, Number + Earlierness, Specific-non-specific} and Y denotes the set, {Number + Earlierness}. Now, of course, Y is a subset of X and there is nothing in the logic of the theory to explain why one morpheme involving this combination of semantic dimensions should be acquired later than another morpheme involving exactly the same combination, a fact which Brown is well aware of and which arises anyway in connection with the reduction of the original list of

fourteen morphemes to ten.

Consider next those transitions where it is the addition of a new semantic dimension which is crucial rather than a novel combination of previously acquired dimensions.

On whether the theory has any explanatory status in this regard, Brown is pessimistic. He says (p.421):

There is no general theory of semantic complexity that makes it possible to assign complexity values to the seven independent unitary meanings [Temporary duration, Containment, Support, Number, Earlierness, Possession, Specific-non-specific - RMA]. It is my impression that Specific-non-specific is the most complex of these, in some sense or other, and so perhaps the fact that it is the last of the meanings to be acquired is an indication that semantic complexity is a determinant of acquisition order. Without a theory of complexity, however, which predicts the difficulty of the Specific-non-specific meaning, no real importance attaches to this result. (my emphasis - RMA)

We can see exactly what would be involved in diluting Brown's pessimism if we consider the transition from T_1 to T_2 in connection with the satisfaction of Condition 4. We are required to produce a reason for why we should get the development schematised as:

$$\{\text{Temporary duration}\} \longrightarrow \{\text{Temporary duration, Containment, Support}\}$$

rather than the alternative:

$$\{\text{Containment, Support}\} \longrightarrow \{\text{Temporary duration, Containment, Support}\}$$

There is nothing in the notional theory of semantics Brown adopts to provide such a reason. If there were,

this would be an answer to Brown's plea for a theory of semantic complexity, but, whereas some sort of theory-internal justification appears to be the only type he considers, in the scheme I am using there are two others. Unfortunately, however, there is no reason to believe that pursuit of either of them would be rewarding in our current state of ignorance. As far as examining different languages for the different dimensions is concerned, this could have interesting consequences (cf. Brown's own remarks on the absence of an article system in Japanese, a property shared with many other languages) but, in the absence of relevant data from many more sources, it is pointless to speculate further. An attempt to ground the theory in a more basic theory could also lead us to formulate interesting questions but, again, we lack the more basic theory and there is no reason to believe that, were it available, a reduction would have the desired consequences.¹⁴⁶

In conclusion, it appears that the semantic analysis offered by Brown fares best with Condition 3 and struggles against the requirements of Condition 4. This is almost certainly related to the fact that the proposals do not have their origins in some general theory of semantic

146. One only has to speculate on the transition from T_1 to T_2 to appreciate the problem. This leads us to speculate on the nature of cognitive or perceptual reasons for the child acquiring the semantic dimension of Temporary duration before he acquires those of Containment and Support.

structure and, until such a theory is available in a form usable by child-language theorists, it is difficult to be optimistic about success in this domain.

3.6.2 Grammatical complexity as a determinant of acquisition order

Brown takes as his reference grammar the theory presented in Jacobs and Rosenbaum (1968) and the fact that this grammatical framework is no longer widely accepted in detail has no bearing on the value of the present investigation. All that is important from my perspective is that it does represent a more or less coherent set of views on language structure and on the sort of formal device available to the grammarian. For each of the fourteen morphemes, Brown exhibits the various features and rules which are directly involved with that morpheme in the Jacobs and Rosenbaum framework, embellishing the framework where he believes he is justified in doing so. The result is a rather complex tabulation in which features, rules, etc. are mentioned as instances of feature-types, rule-types, etc. Figure 63 contains a sample of entries from this tabulation. (see p 290)

While the notation and ideas expressed in Figure 63 will be intelligible enough to anyone familiar with Jacobs and Rosenbaum's work or Brown's adaptation of it, it is quite likely to contain several mysteries for readers meeting it for the first time and so I shall briefly discuss one or two of the entries. Consider first the

<u>Morpheme</u>	<u>Lexical features</u>	<u>Segment structure features</u>	<u>Transformations</u>	<u>Features introduced by transformation</u>	<u>Regular allomorphs</u>	<u>Irregular allomorphs</u>
1. Present progressive	+action -action	+progressive	Progressive affix T	+affix		
2-3. in-on	+ on-N + on + in-N + in	+ on + in	Preposition segment T			
10. Third person regular	+III	+ singular + present - singular - present	Auxiliary agreement T Verbal agreement T Verb suffix T	+affix	/-s -z -iz/	
14. Contractible auxiliary	+action +I +II -action +III	+progressive +singular +present -singular -present	Progressive affix T Progressive segment T Auxiliary Incorporation T Auxiliary agreement T	+affix +copula	/-s -z -iz/	2(-m -r)

Figure 63. Adapted from Brown, pp. 354-5.

fairly simple case of the present progressive. In the Jacobs and Rosenbaum grammar the feature $\langle +\text{progressive} \rangle$, is introduced by a segment structure rule which operates on a segment already specified as $\langle +V \rangle$. Hence the entry under 'segment structure features' in the figure. The relative unacceptability of some verbs with progressive aspect is treated as a grammatical fact and, to this end, it is suggested that verbals (adjectives and verbs) should be subcategorised via a feature, $\pm\text{action}$. On this issue Brown says:

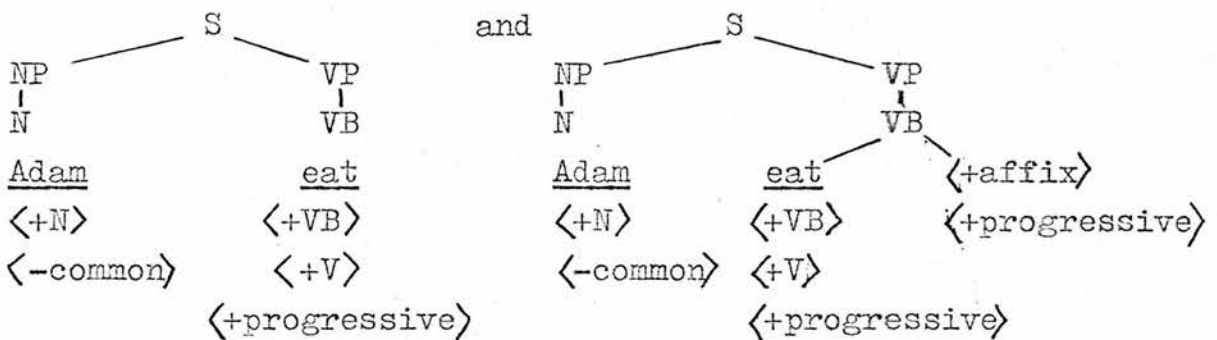
It is to be presumed that when a verb ($\langle +V \rangle$) segment has acquired the feature $\langle +\text{progressive} \rangle$ then only those verbs may be substituted for it....that are marked $\langle +\text{action} \rangle$. In fact, Jacobs and Rosenbaum do not provide an explicit mechanism for accomplishing this effect, and this is one point in which their representation of progressive aspect is sketchy (p.349).

Here then is the explanation for the entry in the 'lexical features' column: these features are, in some sense, presupposed by the correct use of progressive aspect. Whether they might not have other antecedent roles to play in the child's developing grammar is a point Brown doesn't discuss. A transformation, the progressive affix transformation, introduces a segment following the verbal with the features, $\langle +\text{affix} \rangle$ and $\langle +\text{progressive} \rangle$, which is ultimately spelled out as the progressive morpheme, -ing. Thus we have the entry in the 'transformations' column

and also in the 'features introduced by transformation' column.¹⁴⁷ The progressive inflection undergoes no allomorphic variation.

Now consider a rather more complicated example, that of the third person regular morpheme. The choice of a third person form of the verb must be determined by the choice of a third person subject. Thus, in a well-formed sentence, the subject noun will have in its lexical entry a feature corresponding to third person represented here as +III. Note that this differs from the entries in the second column in that it is regarded as an inherent feature of certain nouns in the lexicon and not as an optional development of a category node. The choice and form of the morpheme also depend on the number of the subject. In the Jacobs and Rosenbaum framework this number will be specified by means of a segment structure rule which adds <+singular> or

147. It is not entirely clear why <+progressive> should not also be entered in this latter column as Brown sees the transformation as relating the two structures:



(adapted from Brown, pp.348, 350)

where the feature, <+progressive>, has been introduced by the rule in exactly the same way as the feature <+affix>.

<-singular> to noun segments. Hence the presence of these feature values in the 'segment structure features' column. An additional determinant of the form of the morpheme is Tense and a segment structure rule introduces <+present> or <-present> into the Aux category so these feature values are also entered in this column. As far as the transformational rules are concerned, when there is an auxiliary verb, a transformation is needed to copy number and person features from the subject noun onto the auxiliary. This is the Auxiliary agreement transformation. When no such auxiliary is present, a different transformation, the verbal agreement transformation, must copy this same information onto the main verb segment.¹⁴⁸ Finally, it is necessary to introduce a segment with the feature, <+affix> to the right of the verb which is eventually spelled out as the appropriate inflection. This also is achieved by a transformation, the Verb suffix transformation, and these three rules constitute the entry under 'Transformations' in Figure 63. We have already mentioned the introduction of the <+affix> segment which is entered in the 'features introduced by transformation' column and the last point is that the fact that there is regular allomorphic variation is taken as contributing to overall grammatical complexity and the allomorphs are entered in the figure accordingly.

148. In fact, this information is copied from the auxiliary segment where it ended up as a result of the Auxiliary agreement transformation and this copying is made contingent on the absence of the feature, <+copula>.

I hope that this abbreviated discussion of two examples will enable the uninitiated reader to sample something of the flavour of this approach to grammatical description without necessarily grasping all the details of the analyses. These details are not crucial to following the subsequent discussion.¹⁴⁹

Just as we did for the case of semantic complexity we can consider Brown's analysis as suggesting a developmental theory in which there are a number of stages; in this case, given that the arguments for uniting the regular and irregular variants no longer obtain and given that the possessive morpheme is not included, we end up with twelve stages, $T_1 - T_{12}$, and in each T_i ($2 \leq i \leq 12$) the morphemes in T_{i-1} along with the new morpheme(s) characterising this stage will be listed along with their grammatical properties. The most manageable way in which this can be considered is to isolate types of grammatical information as has already been done in 3.3 and 3.4. So, for example, if we consider only the transformations involved in the appropriate use of the morphemes, we can investigate a theory which has the structure shown in Figure 64.

Figure 64 presents the theory in a form to which Conditions 1 - 4 are applicable and, noting the usual remarks for Condition 1 contingent on us defining the domain of

149. It should be pointed out that Brown felt unable to complete a set of entries for the possessive morpheme. Henceforth, the possessive morpheme will not be included in my analyses.

$T_1 =$	Present progressive	-	Progressive affix T
		↓	
$T_2 =$		T_1	
	<u>in</u> - <u>on</u>	+	
		-	Preposition segment T
		↓	
$T_3 =$		T_2	
	Plural	+	
		-	Noun suffix T, Article T Nominal agreement T
		↓	
$T_4 =$		T_3	
	Past irregular	+	
		-	Verbal agreement T
		↓	
$T_5 =$		T_4	
	Uncontractible copula	+	
		-	Copula T, Auxiliary incorporation T, Auxiliary agreement T
		↓	
$T_6 =$		T_5	
	Articles	+	
		-	Article T
		↓	
$T_7 =$		T_6	
	Past regular	+	
		-	Verbal agreement T, Verb suffix T
		↓	
$T_8 =$		T_7	
	Third person regular	+	
		-	Auxiliary agreement T Verbal agreement T, Verb suffix T
		↓	
T_9		T_8	
	Third person irregular	+	
		-	Auxiliary agreement T, Verbal agreement T
		↓	
T_{10}		T_9	
	Uncontractible auxiliary	+	
		-	Progressive affix T, Progressive segment T, Auxiliary incorporation T, Auxiliary agreement T



cont. p 295a

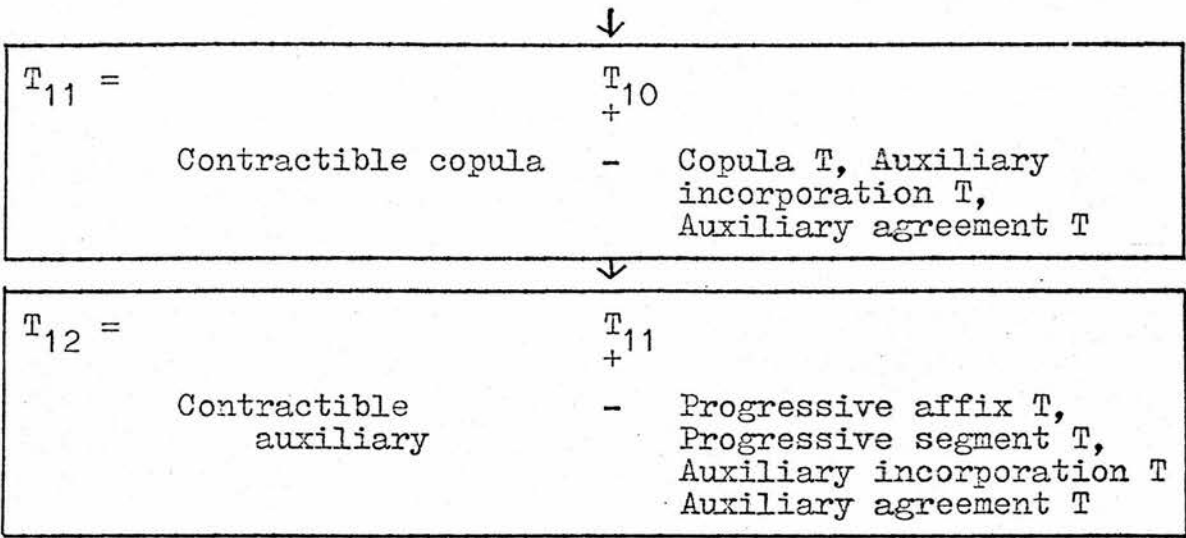


Figure 64

investigation, D, as something like the grammatical aspect of the child's lexical knowledge which enables him to use and understand a particular set of morphemes appropriately, we can see that Condition 2 is satisfied to the extent that we regard Jacobs and Rosenbaum's theory as embodying a general theory of grammar. I shall assume that it does embody such a theory and that, therefore, Condition 2 is satisfied.

Condition 3 also appears to be satisfied for most transitions. What characterises the move from T_i to T_{i+1} ($1 \leq i \leq 11$) is usually the introduction of either new transformational rules or new combinations of such rules and this was exactly the situation we found in our investigation of semantic complexity. It fails to be true in just two cases: the transition from T_5 to T_6 , involving the introduction of articles and the Article T where the latter has already been used in combination with other transformations in the transition from T_2 to T_3 , and the transition from T_8 to T_9 , involving the introduction of third person irregular and requiring a cumulatively simpler combination of transformations than the transition from T_7 to T_8 . As far as the first of these observations is concerned, it has to be pointed out that the Article T does not play a part in the formation of plurals as such but merely handles co-occurrence restrictions between particular articles and singular and plural forms of the noun. As articles have not reached the criterion for acquisition in T_3 , we can assume that the Article T was not involved in the

grammar of plurals at this stage and that its inclusion in Figure 64, while appropriate from the point of view of the adult model, is not justified by the child's usage.

For the transition from T_8 to T_9 , Brown has this to say (p.358):

Jacobs and Rosenbaum note that for irregular verbs like has and does the verb suffix transformation must be blocked ... In the table (= Figure 63 - RMA) I have used the lexical feature, +irregular to mark the existence of the problem but it must be understood that the representation of irregulars is incomplete.

So, arguably, although we have what appears to be the introduction of some simplified transformational machinery at T_9 , it might be the case that this is compensated by the introduction of the lexical feature, +irregular, and whatever else proves to be necessary in the grammar of irregular verbs, in other parts of the grammar. Such a suggestion amounts to a claim that Condition 3 is not applicable to the sequence of theories, (T'_8, T'_9) , where T'_8 and T'_9 are theories involving all the grammatical information relevant to the morphemes and could be seen as an argument against the strategy adopted here of isolating components of the grammar for separate discussion. The upshot of this is that, in principle, there may be avenues to explore in answer to the charge that the transitions from T_5 to T_6 and from T_8 to T_9 fail to satisfy Condition 3 and, on this basis, let us assume that, in general, Condition 3 is satisfied by the sequence $(T_1, T_2, \dots, T_{12})$.

What of Condition 4? An exhaustive survey of the difficulties arising from attempts to satisfy this condition for each of the transitions would be time-consuming and not lead to conclusions distinct from the ones we are now familiar with in this chapter. If we had cases which could be treated in terms of cumulative transformational complexity, we would appear to have the beginnings of a logical grounding for the sequence if we are prepared to subscribe to additional assumptions concerning the child's ability to use n transformational operations only after he has used $n-1$ such operations, etc. We have such cases¹⁵⁰ but they are few and far between and for most pairs of theories in the sequence we have to seek an alternative. Consider a simple case. The transition from T_1 to T_2 can be schematised, as far as transformational rules are concerned, as:

$$\{\text{Progressive affix } T\} \longrightarrow \left\{ \begin{array}{l} \text{Progressive affix } T, \\ \text{Preposition segment } T \end{array} \right\}$$

and we are to provide a reason for why we find this rather than the alternative:

$$\{\text{Preposition segment } T\} \longrightarrow \left\{ \begin{array}{l} \text{Progressive affix } T \\ \text{Preposition segment } T \end{array} \right\}$$

In this case, there is nothing in the logic of the grammatical theory to provide an answer; the Preposition segment T is not, in any sense, a complication or elaboration of the Progressive affix T . Nor is it possible

150. The pairs of theories which are related in this way are, T_1 and T_{10} , T_1 and T_{12} , T_4 and T_7 and T_7 and T_8 .

to discern any ordering relationship between the two transformations which could constitute an alternative logical grounding. Facts concerning the distribution of such rules in the languages of the world are unlikely to be useful so, again, this leaves us with the task of grounding the theory in some other, regarded as more basic. But no such theory suggests itself. Thinking loosely of cognition and formal operations, both of the transformations in question involve the creation and insertion in a string of new material. Formally, there is no tangible difference between them and certainly not the sort of difference which would lead us to claim that one is cognitively more demanding than the other. We can safely conclude, then, that the transition from T_1 to T_2 fails to satisfy Condition 4 and the same goes for other transitions in the sequence forcing the view on us that the whole sequence is not grounded and cannot be considered an adequate developmental theory.

One immediate response to this conclusion would be to insist that the tactic of isolating components is misleading and that, whatever the difficulties, one should attempt to take account of development in each of the components simultaneously.¹⁵¹ The difficulties of this

151. We should note that Brown himself, when comparing stages feels obliged to restrict himself to one sort of theoretical construct and, in fact, only considers transformations from the point of view of cumulative complexity. To the extent that development in other areas of the grammar might invalidate the attempted comparisons in the text, they will also invalidate Brown's own procedures.

procedure have already been discussed in Chapter 1 and we would inevitably end up with sequences of theories to which Condition 3 was not applicable and versions of Condition 4 which, when possible to work with at all, would fare as badly as the example just considered. A partial move in this direction is, of course, to restrict our attention to some grammatical construct other than transformations and to consider sequences of theories for such a construct. For lexical features such a series would begin as in Figure 65 and for segment structure features as in Figure 66.

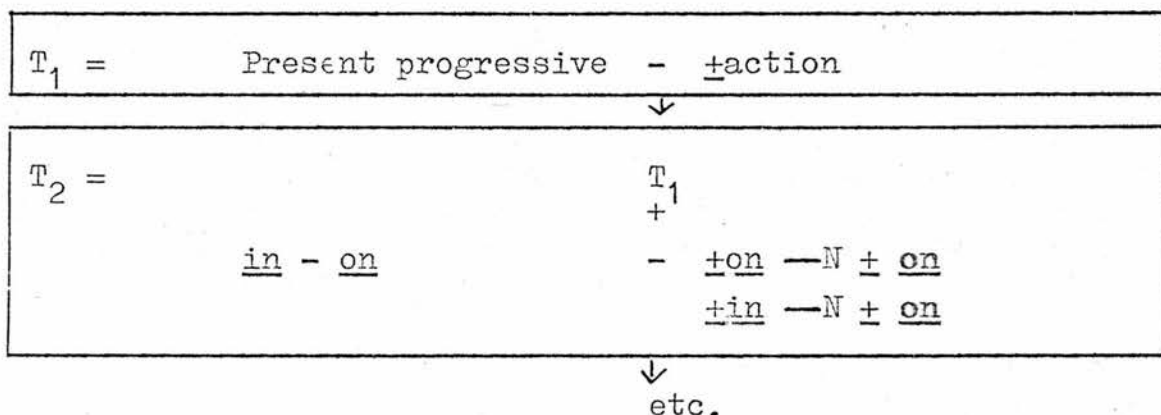


Figure 65

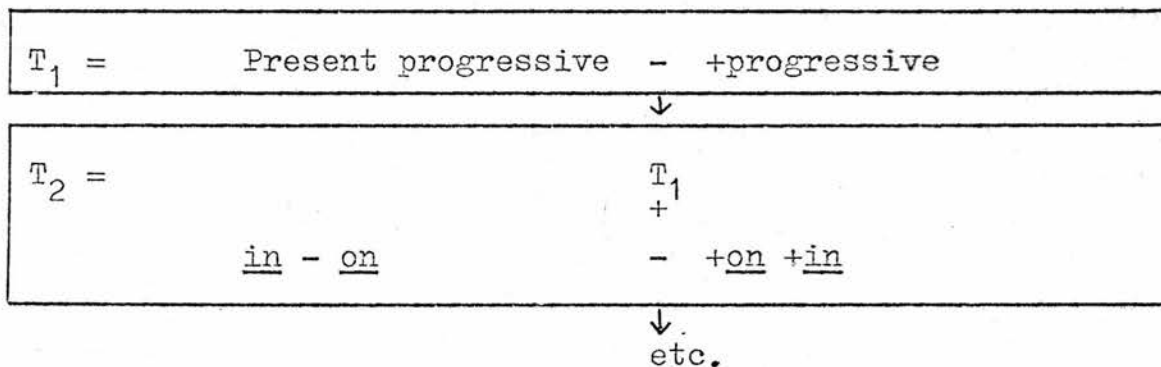


Figure 66¹⁵²

152. As Brown points out, such a sequence will, in all relevant respects, parallel the sequence of Figure 62.

We might find that, at just those points where it appears that Condition 3 is not satisfied for the sequence of sets of transformational rules, it is satisfied for another construct enabling us to conclude that it is inapplicable to the full theories but, without going in to details, I think it should be apparent that such sequences will fare no better with Condition 4 than that already considered.

Overall then it seems that both the semantic and syntactic approaches fail to satisfy Condition 4 and the latter is particularly wholehearted in its failure. This is a conclusion which has characterised the analyses of this chapter and one which will be focussed on again in the next one.

CHAPTER 4 : LANGUAGE ACQUISITION AND COGNITIVE DEVELOPMENT

This chapter differs in structure from the two previous ones. The position to be discussed is that which claims that some, or even all, aspects of linguistic development can only be understood and explained in terms of an antecedently given theory of cognitive development, a position which has come to be widely accepted in the last few years but which lacks critical analysis. Depending on whether one holds that it is only a restricted set of linguistic abilities which are explainable in this way or whether one believes that general cognitive development infects the whole gamut of linguistic competences, one could be regarded as adopting weak or strong forms of the hypothesis that 'thought' precedes and, in some sense, determines language structure.

Obviously an investigation of this issue amounts to an analysis of one aspect of Condition 4 from Chapter 1 and part of the first section of this chapter is devoted to expanding the relevant part of this condition. Conditions 1 - 3 will not concern us at all here. 4.1 examines two further general problems. The first of these, and relatively unimportant, is the extent to which the relationship between language and cognition can be made definitional by regarding it as a matter of stipulation that any sort of linguistic activity is also a cognitive activity and the second concerns the elaboration of a number of positions which can be taken on this relationship, once we admit

that the definitional approach removes a number of interesting questions from the arena of discussion. 4.2 concentrates on arguments which focus on one or another aspect of semantic development whereas 4.3 considers attempts to reduce developmental syntactic phenomena to a general cognitive base. The study of 'strategies' in language development, both as an instrument in learning and as a mechanism of comprehension, forms the subject-matter of 4.4 and, finally, in 4.5, I examine the structure of the arguments in Cromer (1974) for autonomous linguistic development. In selecting studies and analyses for discussion I have necessarily been very selective. The literature in this area is vast and expanding but, to my knowledge, there are no existing proposals which fare spectacularly better against the criteria I develop than the ones which are explicitly considered.¹⁵³ Of course, even if there are, the point of this chapter will not be lost as it may provide us with a framework for investigating why such proposals are more convincing than others.

153. I should point out here that some of the work discussed in Chapter 2, particularly that of Nelson, would repay examination in the terms developed below. However, I have already said enough for the reader to discern the outline of a fuller analysis. Similarly some of the work analysed in Chapter 6 strives to make contact with cognitive development and my decisions to treat things in the order found here is, to some extent, to facilitate organisation but also to take account of emphases in the original work.

4.1 Language and cognition: general considerations

In this section I examine three general problems. 4.1.1 considers the definitional approach to the relationship between linguistic and cognitive development, 4.1.2 enumerates a number of distinct positions on the issue of this relationship, comparing them informally to some of Chomsky's (1974) views on the interaction of a theory of language and a system of 'common-sense understanding', and 4.1.3 develops a refined version of the relevant part of Condition 4 which is referred to in this Chapter as the Reduction Condition.

4.1.1 The definitional position

It is possible to insist that the relationship between language development and cognitive development is uninteresting because, by definition, any sort of linguistic activity is a cognitive activity and any sort of linguistic structure to which psychological reality is imputed is also a cognitive structure. There is little point in arguing with definitions, but it seems to me that to adopt this stance is to rule out of court a number of very interesting questions. As a particular example, consider the formal nature of the rules (of different types) employed in a transformational grammar and discussed, to some extent, in Chapter 3. Assume that it makes sense to credit the child with such a grammar at some stage in his syntactic development. Then, when presented with the question as to whether the rules of the grammar could

be related to the child's general cognitive development, a proponent of the definitional position has to respond that the answer is positive, but uninteresting; and exactly the same response would be forthcoming, no matter what the domain of linguistic enquiry. Clearly there is nothing contradictory about this position and we shall see in 4.4.2 below that such an eminent theorist as Slobin can be seen as adopting it, albeit implicitly. But, with the grammar example still in mind, consider the following question: is it the case that the formal structure of the various rule-types used in a sample grammar are found in other cognitive domains? For transformational rules it could be claimed, and has been, somewhat tentatively, by Chomsky (1968), that the formal properties which characterise them are not found in any other domain of human or animal cognition. Such a claim is refuted by indicating a domain in which a similar formal device is necessary to explain the organism's behaviour, knowledge, etc. (cf. the work of Clowes (1969) on visual perception which could be seen as an investigation of this very problem among other things). The claim and its possible refutation are surely clear and interesting, and I intend, in what follows, to regard as a non-cognitive linguistic construct any such construct which is not identifiable in a cognitive domain other than language. This is not to claim that such constructs exist and, indeed, it is the burden of this chapter to investigate opinions on this matter, but it is to claim that, understood

in these terms, issues concerning the relationship between language development and cognitive development cannot be reduced to vacuity definitionally.

4.1.2 A spectrum of positions

This general issue raises again the question of the precise identity of the domain of enquiry in a language acquisition study (cf. Chapter 1). Earlier discussion enumerated a number of such domains and recognition of this multiplicity immediately indicates that we should be suspicious of the suggestion that language development can be explained by reference to cognitive development.¹⁵⁴ What could such a suggestion amount to? It might mean a number of things and the strongest position that it would be possible to adopt would insist that all aspects of language development require such reference for understanding and explanation. Whether anyone has subscribed to this rather extreme position is not a matter to be settled here, for it is apparent that alternatives exist which still offer a central role to cognitive development in explaining language acquisition.

Referring to a set of domains of linguistic investigation, each of which we regard as reasonably delimited, as $\{D_1, D_2, \dots, D_n\}$, we can imagine that theories¹⁵⁵ have

154. I assume that such a suggestion is not made by someone subscribing to the 'definitional' position of 4.1.1

155. Strictly speaking, for the purposes of this chapter, the theories should be developmental but the point can equally well be made in a non-developmental context.

been constructed in each of these domains and that these theories have, not necessarily disjoint, sets of formal properties, P_1, P_2, \dots, P_n .¹⁵⁶ Thus P_i is the set of formal properties associated with theories in D_i ($1 \leq i \leq n$). Then it may be the case that some or all of the properties in the sets, P_i ($1 \leq i \leq n$), can be identified with properties associated with theories in other domains of non-linguistic cognitive development and this may be the case in a number of ways each one leading to slightly different formulations of the relationship between linguistic and cognitive development. For example, we might find that all the members of a particular set of properties, say P_j , were identified in theories of the non-linguistic cognitive domain, D_x . In such a case we would be in a position to investigate the explanatory value of development in D_x for our understanding of development in the linguistic domain, D_j , and a positive outcome to such an investigation would lead to the conclusion that linguistic development in D_j could be explained by reference to cognitive development in D_x . On the other hand, we might find that only a subset of a particular P_j can be identified in the non-linguistic domain, D_x (and, for simplicity, that those

156. To simplify the presentation here I concentrate on formal properties of theories rather than on their substantive terms. As an example, we could consider theories of the lexicon which have the formal property, 'contains binary features' or theories of grammatical knowledge with the property, 'contains structure-dependent rules'.

members of P_j not found in theories in D_x are also not found in any other domain of non-linguistic cognitive activity). In this case too we would proceed to investigate the explanatory value of development in D_x for our understanding of development in the linguistic domain D_j , but now a positive outcome would not lead us to conclude that linguistic development in D_j is explained by reference to cognitive development in D_x but only that it is partially explained in these terms. We are left with the residue of properties in our theories of D_j which have not been identified in any non-linguistic domain and, at this stage of the enquiry, we would be forced to conclude that they constituted autonomous aspects of language development. Remarks along similar lines can be found in Chomsky's recent speculations, although within a different framework and a different context (see Chomsky (1974, 1976)). In these publications Chomsky discusses the "thesis of independence of grammar" and the "thesis of autonomy of formal grammar". The first of these is concerned with the possibility of there being an interaction between the speaker's knowledge of his language and his system of 'common-sense understanding' such that the latter may serve as an explanatory basis for the former, and the second with the possibility of there being a similar interaction between the speaker's knowledge of linguistic form and his semantic knowledge. It is in connection with the latter that Chomsky makes his clearest statement when he says (1974, pp.15 - 16):

Suppose that among the primitive notions of linguistic theory we can distinguish some that are "semantic" and others that are "formal". Thus we might take such notions as "synonymous", "significant", "denotes", "satisfies", "refers to concrete objects", to be core notions of semantics, let us say, primitive in our linguistic theory; while the primitives of phonetic theory...may be taken to be formal notions. Given a bifurcation of the primitive notions into "formal" and "semantic" we can ask, for each defined concept, whether terms of one or the other category appear in its definition. There are then purely formal concepts. We may refer to the theory concerning just these as "the theory of linguistic form". We might discover that this theory - which excludes the core notions of semantics - is virtually null, or quite uninteresting. Or, at the other extreme, we might find that it includes an interesting concept of "grammar" and "structure", perhaps all linguistic levels apart from semantic representation.

The latter possibility is referred to by Chomsky as the "absolute autonomy thesis" and is contrasted with the "parameterized autonomy thesis" which represents something between the two extremes pointed to in the cited passage. Clearly, a similar distinction can be made for the interaction between systems of linguistic knowledge and systems of 'common-sense understanding' and it seems to me that it is not implausible to regard the above discussion of the possible relationships between linguistic and cognitive development as entailing the same sort of spectrum of positions. Thus, consider again the possibility of theories in the linguistic domain, D_j , involving formal

properties, $P_{j1}, P_{j2}, \dots, P_{jm}$. It may be the case that none of these properties can be identified in any non-linguistic cognitive domain and, in this situation, we could talk of the "absolute autonomy of development in D_j with respect to cognitive development". Alternatively, we may discover that a subset of $\{P_{j1}, P_{j2}, \dots, P_{jm}\}$ can be identified in theories of the non-linguistic domain D_x , in which case we could talk of the "parameterized autonomy of development in D_j with respect to cognitive development". Finally, it might be the case that the full set of properties in P_j could be identified in theories in D_x , in which case we would speak of the "absolute dependence of development in D_j on cognitive development".¹⁵⁷

Taking the discussion one step further we can now consider the full set of domains, $\{D_1, D_2, \dots, D_n\}$, and again, a set of possibilities emerges. It may be that development

157. Both of these latter positions assume that additional conditions still to be investigated are satisfied to justify the assertion of total or partial dependence. One further possibility which could be mentioned but which seems to be sufficiently remote from serious investigation at the moment as to not warrant further discussion is that whereby some properties in the set, $\{P_{j1}, P_{j2}, \dots, P_{jm}\}$ are identified in the non-linguistic domain, D_x , while others are identified in theories of the distinct non-linguistic domain, D_y . Perhaps it would be justified, at this stage, to assume that cognitive development is monolithic once language development is excluded and so simply avoid this sort of possibility.

in each of D_1, D_2, \dots, D_n is absolutely dependent on cognitive development, that development in each of them is dependent on cognitive development but that, in some cases, this dependence is only partial, that development in some of them is totally dependent while development in others is totally independent of cognitive development and so on. Only for the first possibility would we be justified in asserting that language development depends on cognitive development without qualification.¹⁵⁸

4.1.3 The Reduction Condition

This section is concerned with revision and extension of the second clause of Condition 4 (p 48). I shall not be concerned with any domain outside the child's activity and, for the purposes of this chapter, the "other sphere of the child's activity" is the sphere of general cognitive development. The relevant part of Chapter 1 leading up to the statement of Condition 4 laid the groundwork for some of what follows.

The situation we are to consider is one in which we have a sequence of theories, (L_1, L_2, \dots, L_n) , in some domain of language development, D , and a sequence of

158. As an example, consider the possibility that syntactic knowledge is, to some extent, independent of cognitive development whereas the development of speech acts or 'communicative competence' is totally dependent on such development. The only thing to be clear about is that there is no conflict in this hypothetical state of affairs.

theories, (C_1, C_2, \dots, C_m) , which constitutes an adequate theory of cognitive development.¹⁵⁹ Furthermore, we know the temporal ordering across the two sequences as well as within the sequences, i.e., we can collapse the two sequences to give one composite sequence.¹⁶⁰

Having obtained a collapsed sequence of the sort described what can we demand of this sequence if we are to claim that the cognitive development explains the linguistic development? Already discussed in Chapter 1 is the requirement^e that there must be some significant relationship

159. It is not my task here to investigate what may be involved in this notion but I would anticipate that something along the lines of the discussion in Chapter 1 would be relevant.

160. I shall assume that such a collapsed sequence is a total ordering but nothing hangs on this assumption. If we were to allow cognitive and linguistic theories to appear 'simultaneously' in the ordering, it would only complicate some of the subsequent definitions while not materially affecting the arguments. Note that no one-one correspondence between theories in the two sequences is entailed by this procedure and I leave open the possibility that in a collapsed sequence we shall find adjacent linguistic theories and adjacent cognitive theories. This is merely a reflection of the fact that we are not entitled to prejudge the issue of the number of theories of either type and the most conservative strategy assumes that the stages on which our theories are based are quite arbitrary and a function of the methodology of the individual studies, time available, etc. As pointed out in Chapter 1, this is not to deny the possibility of there being a particular number of significant stages of development in any domain.

between the substantive terms and formal properties of the two sequences of theories.¹⁶¹ Some of the linguistic theories we have considered and some of those still to be considered have very poorly developed formal structures but, while we can hope that such a situation will change, this fact in itself is immaterial to the task at hand. I now claim that, in order for the cognitive theory to be explanatory in the domain of linguistic development under investigation, it must be the case that:

- (1) the substantive terms of the linguistic theory can be translated into substantive terms of the cognitive theory

and:

- (2) the formal properties of the linguistic theory must be identifiable in the cognitive theory

So, as an example, we can imagine a developmental theory of syntax which consists of a sequence of grammars utilising a set of grammatical categories and a set of rule-types. If there is to be the sort of relationship we require between a cognitive theory and this theory of syntactic development, then it must be the case that:

- (i) each of the grammatical categories can be translated into some substantive term of the cognitive theory (e.g., NP is translated as 'Entity', VP is translated as 'Action', where 'Entity' and 'Action' are theoretical terms in the cognitive theory in question)

161. I am assuming here that the distinction between substantive and formal aspects of theories is an intuitively clear one.

and:

- (ii) each formal property corresponding to each rule-type must be identifiable in the cognitive theory (e.g., if the linguistic theory includes phrase-structure rules, then formal operations building hierarchical structures must exist in the cognitive theory; if the linguistic theory includes transformational rules, then the cognitive theory must embrace processes which are structure-dependent in the required sense.

Similarly, if our linguistic theory is concerned with the development of the lexicon and uses binary semantic features, then, in order for the cognitive theory to be viable as a provider of an explanation, it must be the case that:

- (i) for each of the features used in the linguistic theory (which I take to be the substantive aspects of such a theory), there is a translation into some term of the cognitive theory (e.g. \pm object is translated as 'Entity', \pm animate is translated as 'Animacy' where 'Entity' and 'Animacy' are theoretical terms in the cognitive theory in question).

and:

- (ii) each formal property in the linguistic theory (in this case we would appear to have only the two properties of binary categorisation and set-formation) can be identified in the cognitive theory.

Clearly, while this much is necessary if the theories are to be related in the required way, it is not sufficient and the reader will have noted that (1) and (2) take no

account of the ordering relations in the collapsed sequence. Imagine, then, that we have some linguistic theory from the sequence, L_i , which uses substantive terms, S , and which has formal properties, P . Assume further that the translations of the substantive terms required by (1) can be located in the sequence of cognitive theories, (C_1, C_2, \dots, C_j) , and that (C_1, C_2, \dots, C_j) also manifests the formal properties required by satisfaction of (2). Finally, assume that for all k ($k < j$), it is not the case that the sequence, (C_1, C_2, \dots, C_k) , contains all the relevant translations and the required properties, i.e., (C_1, C_2, \dots, C_j) is the first subsequence of cognitive theories from the full sequence, (C_1, C_2, \dots, C_m) , beginning with the first theory in the sequence, which contains all the structure relevant to L_i . If we are to maintain that the cognitive theory explains the linguistic development, then it is apparent that C_j must precede L_i in the collapsed sequence. If this were not the case, we would have the beginnings of an argument for the linguistic development explaining aspects of cognitive development, namely those which did not appear in the cognitive theories until after L_i in the collapsed sequence. So we can now formulate a third condition:

- (3) The translations of substantive terms and formal properties required by satisfaction of (1) and (2) must appear in the sequence of cognitive theories before they are required by a linguistic theory.

To illustrate, imagine that we have the collapsed sequence, $(C_1, C_2, L_1, C_3, L_2, C_4, L_3)$, where C_1, C_2, C_3 and C_4 are cognitive theories and L_1, L_2 and L_3 are linguistic theories in the specified domain, D . Then, in order for the cognitive theory to have the necessary relationship with the linguistic theory, the substantive terms of L_1 must be translatable into substantive terms of the subsequence, (C_1, C_2) , and the formal properties of L_1 must be identifiable in this subsequence, the substantive terms of L_2 must be translatable into substantive terms in the subsequence, (C_1, C_2, C_3) , with its formal properties being identifiable in this subsequence and the substantive terms of L_3 must be translatable into the substantive terms of the whole cognitive sequence, (C_1, C_2, C_3, C_4) , which must also manifest L_3 's formal properties. This formulation leaves open the possibility that the 'new' terms and properties of a linguistic theory are already translatable or identifiable in a sequence of cognitive theories which does not include the cognitive theory immediately preceding the linguistic theory in the collapsed sequence. This appears to be entirely correct for the reasons discussed in Chapter 1 and no stronger necessary condition can be imposed (but cf. below).

To give a more concrete example, we can revert to syntactic development, conceived of in terms of transformational grammars. Assume that we have the collapsed sequence, (C_1, L_1, C_2, L_2) , where L_1 and L_2 are grammars and C_1 and C_2 are theories of cognitive development. Assume further

that L_1 is a phrase-structure grammar and L_2 contains also transformational rules. Finally, assume that C_1 contains operations which construct hierarchical structures and that C_2 contains structure-dependent operations. Then, restricting our attention to formal properties, the condition under discussion is satisfied and the view that the cognitive development explains the linguistic development is consistent with such a sequence. If, under the same assumptions, we had obtained the sequence, (C_1, L_2, C_2, L_1) , the condition would not have been satisfied and the claim that cognitive development explained syntactic development would be falsified.¹⁶² Just as for the first sequence considered in this paragraph, so for the sequence, (C_1, C_2, L_1, L_2) , and the sequence, (C_2, C_1, L_1, L_2) , although these latter two do not exhibit the degree of 'cohesion' that we find in the first sequence (cf. below).

So far, then, we have three conditions on the reduction, one concerning translatability of substantive terms, a second concerning identifiability of formal properties and a third referring to the sequencing of the theories of different types. It appears that these are the only necessary conditions we can impose and what they amount to, if satisfied, is a demonstration that the relevant

162. The fact that this developmental sequence would be ruled out anyway on the grounds that we could hardly have a grammar with transformations without a previous stage of a phrase-structure grammar is beside the point in the present context.

cognitive development precedes the linguistic development under consideration. But such a demonstration does not explain the linguistic development. For such an explanation we need to see some causal link between the cognitive and linguistic spheres and nothing we have said so far precludes them from developing independently. In particular, terms and formal properties may enter the cognitive and linguistic theories in quite different orders and, as was argued in Chapter 1, it is impossible to rule this out by a necessary condition, i.e., it might be the case that there is a causal relationship between cognitive and linguistic development without there being any strict similarities between the orders of appearance of cognitive and related linguistic constructs. Nevertheless, we could increase the plausibility of the claim if certain further conditions were met and it is the purpose of the last part of this section to introduce two such conditions.

Consider first a collapsed sequence which satisfies the conditions (1) - (3). The satisfaction of (1) and (2) entails the existence of a mapping, F , from the primitives (substantive and formal) of the linguistic theory to those of the cognitive theory such that, for substantive terms, F is the translation mapping required by satisfaction of (1) and, for formal properties, F is the identity mapping. The sets of primitives in both sequences of theories, (L_1, L_2, \dots, L_n) and (C_1, C_2, \dots, C_m) , can be seen

as partially ordered by the total-orderings on the sets of theories (cf. fn. 160) and we can enquire as to whether F preserves the partial ordering of the linguistic primitives in the cognitive primitives, i.e., we can consider whether $X_i \leq X_j$ entails $F(X_i) \preceq F(X_j)$ where ' \leq ' is the partial ordering induced on the linguistic primitives by the total ordering on the set of linguistic theories, ' \preceq ' is the corresponding partial ordering for the cognitive primitives and X_i and X_j denote primitives (substantive or formal) of the linguistic theory.¹⁶³ If this condition is satisfied we shall say that the two theories are order-isomorphic and refer to this as a 'desirability condition' on the reduction, the satisfaction of which contributes to the plausibility of the view that cognitive development explains language development in D (for a plea for order-isomorphism, see fn. 58 in Chapter 2).

As a further possibility, consider the collapsed sequence, $(C_1, L_1, C_2, L_2, \dots, C_n, L_n)$ where each C_i is a subsequence (possibly containing only one member) of cognitive theories and each L_i is a subsequence (possibly containing only one member) of linguistic theories. If we assume that the sequence of (subsequences of) linguistic theories, (L_1, L_2, \dots, L_n) , satisfies Conditions 1 - 5 of Chapter 1, it follows that for every pair, (L_i, L_{i+1}) , in the sequence the latter member will contain 'new' constructs

163. In other words, we are enquiring as to whether the two sets of primitives enter the sequences in the same order.

when compared with the former member either of the same type or of a different type. Let us refer to these 'new' items in L_{i+1} as N_{i+1} . We also assume that the sequence (of subsequences) of cognitive theories, (C_1, C_2, \dots, C_n) satisfies a set of conditions on explanatory theories of cognitive development and, in particular, that it will satisfy some analogue of Condition 3.¹⁶⁴ So, for every pair, (C_i, C_{i+1}) , from the sequence, C_{i+1} will contain 'new' items when compared to C_i . Let us refer to these as M_{i+1} . Now recall that we require the substantive terms of L_{i+1} to be translatable into substantive terms in the sequence, $(C_1, C_2, \dots, C_{i+1})$ and that the formal properties of L_{i+1} should be identifiable in this sequence. Some of the substantive terms and formal properties in L_{i+1} will be the 'new' ones, i.e., N_{i+1} , and we can ask where in the sequence, $(C_1, C_2, \dots, C_{i+1})$, we find the translations of substantive terms in N_{i+1} and the formal properties in N_{i+1} for the first time. An interesting state of affairs ensues if these constructs first appear in C_{i+1} , i.e., if they are members of M_{i+1} . According to this state of affairs, the sequence of C's and L's is arranged in such a way that each innovation in the

164. Strictly speaking rather than satisfying Condition 3 we merely require that sequences don't violate it by permitting theories to become simpler as the sequence progresses. However, as a moment's reflection shows, both possibilities, satisfaction or failure to violate, involve the introduction of 'new' material in the sense required by the argument.

linguistic theory is immediately preceded by the appropriate innovation in the cognitive theory.¹⁶⁵

As a concrete example, we can consider again the collapsed sequence, (C_1, L_1, C_2, L_2) , where the sequence, (L_1, L_2) , is a developmental theory of syntax with L_1 a phrase-structure grammar and L_2 a grammar including transformational operations. C_1 is a cognitive theory including operations for forming hierarchical structures and C_2 contains additional structure-dependent operations. The innovation in the sequence, (L_1, L_2) , is the implementation of transformational rules and this is exactly paralleled by the innovatory aspect of C_2 when compared to C_1 , the appearance of structure-dependent operations. In a case such as this I shall say that the theories are intermeshed and see this as a further 'desirability condition' on the reduction.

To see a simple example in which theories are not intermeshed we can consider the collapsed sequence, (C_1, L_1, C_2, L_2) where, again, the sequence, (L_1, L_2) , is a developmental theory of syntax with the properties it had in the previous example. In this case, however, assume that the structure-dependent rules in L_2 are restricted to those involving the elementary operation of adjunction, that C_1 contains operations for forming hierarchical structures and structure-dependent operations restricted to adjunction

165. Of course, in addition, there may be a large number of innovations in the cognitive theory which will be of no concern to the linguistic theory.

and that C_2 contains everything in C_1 plus deletion operations. Then the innovative aspects of L_2 over L_1 include the introduction of adjunction rules and, as is necessary to satisfy the earlier conditions, the formal operation corresponding to adjunction is identifiable in the sequence, (C_1, C_2) . However, what is innovative in the transition from L_1 to L_2 cannot be identified with what is innovative in the transition from C_1 to C_2 as this latter involves the introduction of deletion operations which are unknown to both L_1 and L_2 . In this case, therefore, we would not be justified in concluding that the two theories are intermeshed although the possibility remains open that, with more stages sampled, they will be order-isomorphic. In fact it is easy to see that two theories being intermeshed is a special case of their being order-isomorphic and, if theories of cognitive development and language development in D are related in this way, we should be strongly tempted to see cognition as determining the progress of language development in D , with each innovation in cognitive development making available new concepts or new formalisms to the linguistic system and with the linguistic system taking up its options immediately.

I come finally to the statement of the revised second clause of Condition 4 which, since it is the only condition of direct interest in this chapter, I shall refer to as the Reduction Condition.

REDUCTION CONDITION

Given a theory $T (= (L_1, L_2, \dots, L_n))$ in the domain of language development, D , then T is an explanatory theory in D just in case Conditions 1 - 3 of Chapter 1 are satisfied (assuming Condition 3 is applicable¹⁶⁶), and, in addition, the relationship of simplicity obtaining between L_i and L_{i+1} ($1 \leq i \leq n-1$) can be related to a theory of cognitive development such that the following conditions obtain:

(1) for all substantive terms appearing in the sequence of linguistic theories it is possible to provide a translation into the substantive terms of the cognitive theory.

(2) for all formal properties appearing in the sequence of linguistic theories it is possible to identify them in the sequence of cognitive theories.

(3) given satisfaction of (1) and (2), it must be possible to collapse the two sequences of theories such that, for each substantive term in the linguistic theories, its translation (as required by (1)) appears in the collapsed sequence before the term itself appears and, for each formal property in the linguistic theories, that formal property must occur in a cognitive theory before it occurs in a linguistic theory.

166. What follows in this formulation is neutral between whether Condition 3 is satisfied or inapplicable (cf. similar remarks in Chapter 1)

Failure to satisfy (1) or (2) indicates the inadequacy of an attempted reduction whereas satisfaction of (1) and (2) coupled with failure to satisfy (3) amounts to a refutation of the view that cognitive development explains linguistic development in D.

If in addition,

(4) the two theories are order-isomorphic

or, as a special case of (4),

(5) the two theories are intermeshed

this will count as additional support for the reduction, contributing to its plausibility. However, nothing can be concluded from failure to satisfy either (4) or (5).

I now wish to attempt to apply this extended condition to some of the best-known arguments for the dependence of aspects of linguistic development on cognitive development.

4.2 Arguments from semantic development

In this section I shall discuss a number of studies which have concentrated on aspects of the child's semantic development. The main difference between these studies and those discussed in Chapter 2 is that whereas the latter focussed on the acquisition of lexical items and their meanings, we are concerned here more with the structural expression of meaning via syntax. This is not strictly true in 4.2.6, although even there the emphasis is on the use of single lexical items to encode complex

propositions rather than on what we might refer to as the 'referential meaning' of these items.

It is necessary to clear up a general point before commencing any analysis. As far as the necessary conditions in the Reduction Condition are concerned, it is clear that they are satisfied quite vacuously by any theory of semantic development at least with regard to the substantive terms in such a theory. Thus, if a particular semantic analysis credits a child with a formally simple meaning, M, then it is apparent that the child must also control the concept 'corresponding to' M and that he must control this concept before he expresses it linguistically. Therefore M will be translatable into some substantive term of a cognitive theory (satisfying (1)) and, furthermore, this substantive term will appear in a collapsed sequence of cognitive and linguistic theories before M (satisfying (3)). It follows, then, that any discussion in this area, to avoid trivialising the issues, must focus on formal aspects of semantic development (e.g., modes of combination of simple meanings to form complex meanings) or on satisfaction of the desirability conditions, (4) and (5). There is, however, one further issue which will emerge in what follows and that is that, although we can be assured of the existence of a concept corresponding to a meaning and that there is little point in making an issue of this, the existence of the cognitive theory in which the concept is a substantive term is not guaranteed.

Two possible misunderstandings may arise at this point. The above view does not commit me to the identity of systems of concepts and semantic categories, although I must accept that the set of semantic categories is a subset of the set of concepts available to the child at any one time. This seems to me quite uncontroversial. The second point is that it is possible for a child to use a linguistic expression or structure without controlling the appropriate meaning (and hence concept) and the door is still open for language to have some effect on conceptualisation if one wished to argue in this direction (see Carter (1975) for a restricted argument of this type). The child's linguistic expressions are not an infallible guide to his set of concepts. But, if an investigator is prepared to credit a child with a certain meaning no matter how it is expressed - the child's meaning for the expression - then he must also be prepared to credit him with the corresponding concept.

In 4.2.1 I shall briefly consider McNamara's (1972) influential paper arguing that most of its content is irrelevant to the considerations of this chapter. 4.2.2 - 4.2.4 examine a set of arguments which have been constructed by Cromer (1974) using findings of Brown and his associates (4.2.2), Bloom's work on the development of negation (4.2.3) and Cromer's own research on the acquisition of temporal reference (4.2.4). The structure of the arguments in each of these sections is very similar.

4.2.5 analyses the argument in a recent paper by Antinucci and Miller (1976) and 4.2.6 briefly considers relevant sections of Greenfield and Smith (1976). Finally, in 4.2.7, I shall examine some of the work of Sinclair-de-Zwart (for additional and largely unrelated comments on her research, see 4.3.1), that with the most obvious semantic relevance, and discuss what is a rather different type of argument to what we shall meet in the rest of the section.

4.2.1 McNamara's views on meaning, syntax and cognition

McNamara (1972) made a big impression in the child language field and is often cited as one of the seminal papers on the topic of this chapter. Yet, on close inspection, its relevant content is remarkably slight despite the author's summary which says (p.11):

All that is needed for my position is that the development of those basic cognitive structures to which I referred should precede the development of the corresponding linguistic structures. (my emphasis-RMA)

Such a statement would seem to indicate that McNamara has explicitly discussed the three necessary clauses of the Reduction Condition for a range of linguistic phenomena but this is mere fancy. The body of his paper is devoted to a plausible argument that the child, in learning some aspects of syntax, must have access to semantic information which he can use to provide a foothold on what is otherwise an apparently impossible task. But this is quite

consistent with the view that syntax, conceived of as a formalisation of knowledge or as a set of procedures for converting meanings into forms, is largely independent of semantics and that, correspondingly, syntactic development is autonomous of cognitive development. To show otherwise it is necessary to consider the properties of particular syntactic theories and attempt to satisfy (1) - (3) of the Reduction Condition but nowhere does McNamara attempt to do this.

An exception to the above analysis might be thought to exist in McNamara's discussion of vocabulary development where he makes, among others, the following two points:

- (i) names for entities are learned before names for certain attributes

and:

- (ii) names for varying attributes are learned before names for permanent attributes.

As far as the first of these is concerned, McNamara says (p.4):

It is obvious that an infant has the capacity to distinguish from the rest of the physical environment an object which his mother draws to his attention and names. It seems clear too that in such circumstances he adopts the strategy of taking the word he hears as a name for the object as a whole rather than as a subset of its properties, or for its position, or weight, or worth, or anything else.

This could have at least two interpretations of present concern. The first of these has it that the child, at

the stage in question, possesses a strategy along the lines of:

Apply names to objects rather than attributes which, for some reason, McNamara wishes to refer to as a cognitive strategy. Clearly, however, it is a linguistic strategy given that it manipulates the linguistic concept 'name', and it is not possible to see it as a particular example of a more general cognitive (non-linguistic) strategy. The second interpretation credits the child with a cognitive category, 'Entity', at the stage in question and assumes that only later does he develop the cognitive category, 'Attribute'. This may be plausible and could be seen as providing a basis for the child learning the syntactic distinction between Nouns and Adjectives but one looks in vain for a clear statement of the cognitive theory which would make it more than plausible.

An exactly parallel argument can be constructed for the second point above, in connection with which McNamara says (p.4):

If there is a differential set in small children to attend to varying states and activities rather than unvarying attributes, we need look no further for an explanation for the order in which the corresponding terms are learned.

Again we may be being asked to credit the child with a strategy for language learning but, if so, it is patently linguistic, or we may be faced with the suggestion that the child controls a cognitive category, 'Varying attribute',

before he controls a cognitive category, 'Unvarying attribute' or 'State', a suggestion which, while plausible, requires to be backed up with some firm cognitive evidence before we can subscribe to it wholeheartedly.¹⁶⁷

In both cases we are concerned with what are essentially semantic distinctions within the linguistic system and so in both cases there is no real issue about the existence of the relevant concepts before the child encodes them in language. But there is an issue concerning the clear articulation of these concepts within a cognitive theory and McNamara goes no way towards resolving this issue.

4.2.2 Cromer on Brown

The second major section of Cromer (1974) is entitled 'Cognitive effects on grammar' and carries most of the weight of his arguments in favour of the Cognition Hypothesis.¹⁶⁸ One of the arguments he uses draws on the work of Brown

167. These speculations can, of course, be related to those of Nelson discussed in Chapter 2.

168. The first section of his article bearing the title 'Cognitive effects on babbling and first words' defies any reasonable analysis in the terms of this chapter. Despite the title, it seems to me to be concerned with semantic development and no relationship with any cognitive theory is even pretended. Also several readings of Cromer's article have failed to provide any succinct statement of the content of the Cognition Hypothesis but it seems clear that it amounts, at least, to a claim for the truth of the three necessary clauses of the Reduction Condition.

and his associates and is concerned with the earliest verbal inflections used by children. It boils down to the fact that the 'standard' meaning associated with each of these inflections can be identified, in the speech of the child, before he marks any of these meanings formally, such identification transparently involving the method of 'rich' interpretation, whereby the investigator, by taking account of context of utterance, attempts to get at the child's intended meaning. Cromer summarises the observations thus (p. 210):

Brown noted that for all three children the verb was initially in an unmarked form, i.e., it did not have any inflectional endings. Nevertheless, such a verb was understood by the parents in one of four ways, depending not only on the utterance itself but on the situational context in which the utterance occurred. One of these was the imperative, as in 'Get book'. A second meaning which was communicated at this stage was reference to the past, as, for example, in 'Book drop' where the book had just dropped. A third meaning ascribed to the child was that of intention or prediction, as in 'Mommy read' in a context where Mommy was about to read to the child. Finally there was the expression of present temporary duration as in 'fish swim' where the context would call for an adult utterance using a progressive such as 'The fish is swimming'.

When we align this set of findings with the fact that the children in Brown's study subsequently began to mark distinctions in the verb and that the first distinctions so marked were exactly those encoding the meanings with

which the child is credited before he indulges in any formal marking,¹⁶⁹ we can construct an argument having the following structure. There is a stage of development¹⁷⁰ at which it is necessary to credit the child with grammatical devices which end up being realised as past-tense allomorphs, semi-auxiliaries, catenatives, progressive inflections and please. Although no particular linguistic theory is referred to, I shall assume that the child is to be credited with the possession of a set of abstract morphemes or syntactic categories, Past, Modal, Prog(ressive) and Imp(erative) at this stage. In addition, there is a stage of cognitive development, evidenced by the child's earlier linguistic behaviour, at which he has a notion of Pastness, Prediction, Intentionality, Ongoingness, Temporariness and Ordering (in the sense of imposing obligation). Finally, there is the third claim that the relevant stage of cognitive development precedes the

169. Of course, these are not all marked by inflections and Brown refers to the emergence of catenative verbs to mark intentionality, etc. and of please to co-occur with imperative interpretations of unmarked verbs.

170. At this point the reader may wonder whether it is more appropriate to talk of syntactic or semantic development in this connection. Cromer urges the former but this view can be countered by the observation that there is a one-one correspondence between grammatical devices and meanings at this stage. So, for example, the simple past inflection is not used to express hypotheticalness in conditional clauses as it is in the adult language.

related stage of linguistic development. So, we have a collapsed sequence, (C,L.), where C contains, as substantive terms, Pastness, Prediction, etc., and L contains, as substantive terms, Past, Modal, etc. and a translation from the substantive terms of L to the substantive terms of C mapping Past to Pastness, Modal to Prediction and Intentionality, etc. Not surprisingly, (1) and (3) of the Reduction Condition are satisfied and (2) is not applicable but the identity of C, beyond the fact that it contains those 'notions' listed above, must be a source of worry. It would be comforting if there were an existing cognitive theory making available the appropriate cognitive categories to the child at the appropriate age and if the categories were ascribed to the child on the basis of something other than linguistic evidence. Without this sort of basis, I am left convinced of the correctness of the claim but unexcited by it, for note that questions of order-isomorphism do not arise. Although the grammatical devices realised as inflections are the first of their kind to appear in linguistic development, no similar claim can be made for the corresponding concepts in the cognitive theory, i.e., there is no guarantee that cognitive theories earlier than C do not contain concepts which correspond to grammatical devices which only appear in linguistic theories subsequent to L and this is entirely due to the ad hocness of C. Additionally, no order-relationships exist within the set of cognitive categories, although we do know from Brown (1973) that children acquire

progressive inflections before past-tense inflections (cf. Chapter 3) and we can speculate that the cognitive categories, Ongoingness and Temporariness (corresponding to Prog(ressive)) are acquired before Pastness (corresponding to Past) but there is no a priori reason to accept this and certainly no cognitive theory with which I am familiar to render it plausible.

A similar argument concerns the spatial prepositions, in and on, and their use to encode spatial notions. Before children use the prepositions in this way they give evidence that they intend to talk about spatial relationships and we can conclude that there is a stage at which the child controls the relevant cognitive categories of Inness and Onness which precedes the linguistic stage at which he gives evidence of controlling the category, Spatial Preposition. Parallel remarks can be made with regard to the acquisition of the possessive inflection and its anticipation by the "concept of possession" (p.213). In the case of prepositions, Cromer clearly feels that the work of Parisi and Antinucci (1970) can provide the sort of systematic basis he needs, particularly since it attempts to explicitly relate its findings to Piaget's views on the development of spatial concepts (Piaget and Inhelder (1948)). Unfortunately, as Cromer points out, the Parisi and Antinucci study was not longitudinal and, therefore, can at best be suggestive for the sort of issue we are concerned with here.

An argument with a different structure concerns the first syntactically-structured utterances of the child and their relationship to the Piagetian notion of sensori-motor intelligence. Cromer uses this aspect of Brown's work as the basis of an argument for independent linguistic development, (see 4.5) but it can also be seen as relevant to the concerns of the present section. Brown (1973, pp.236-9) claims that the vast majority of the early sentences of the child can be seen as expressing the semantic relationships contained in two, partially overlapping, taxonomies. These are:

Operations of reference: Nomination
 Recurrence
 Notice
 Nonexistence

and:

Basic semantic relationships: Agent and action
 Action and object
 Agent and object
 Action and locative
 Entity and locative
 Possessor and possession
 Entity and attributive
 Demonstrative and entity

and Brown says of them (p.236):

The Stage I meanings have proved to have some generality in a sampling of child speech studies, and I do feel tempted to hypothesise universality. But not innateness. Not innateness because, although I have not worked out the relation in any detail, it is my impression that the first meanings are an

extension of the kind of intelligence that Jean Piaget calls sensori-motor. (my emphasis - RMA)

Brown could be seen here as assuming that there is a substantive issue concerning semantic development and cognitive development and, of course, the response is that it is transparent that before the child can encode the semantic notion of nomination, he must have some conception of an object which can be nominated, before he can encode the relationship of entity and location, he must have some concept of an object and some concept of a location, etc. We should be perfectly clear that the relations listed above are semantic and that, at this stage in his work, Brown is not remotely concerned with how these relations are expressed in different languages. He says (p.239):

...let me make it clear that this section concerns meanings and not grammatical relations... The formal relations which express semantic relations are peculiarly linguistic, and I see nothing quite like them in sensori-motor intelligence.

Thus we have a collapsed sequence, (C_{SM}, L_I) , where C_{SM} is the cognitive theory at the end of the sensori-motor period in Piagetian theory and L_I is a linguistic theory of the set of meanings linguistically encodable by the child at Stage I. What is novel about this sequence is that C_{SM} is substantial and, if the translation and identification required by the necessary clauses of the Reduction Condition can be carried through we would

appear to have a finding of some importance. But, as Brown admits, he himself has not considered these questions in detail and, although there are instances where the mapping is self-evident (e.g., 'object' in the linguistic theory may be translated as 'Entity' in the cognitive theory), there are several others where this is not so. What, for example, in the theory of sensori-motor intelligence would correspond to the semantic distinction between Nomination and Notice? This is not to suggest that Brown's optimism is ill-founded but merely to point out that a good deal of analysis and argument is still necessary. Similarly, there are no easy answers to the question of order-isomorphism although we would appear to be justified in assuming that C_{SM} and L_I are not inter-meshed to any interesting extent. In order to begin investigating this question we would have to consider any ordering which exists between the semantic relations in L_I and compare this with ordering in the corresponding elements of C_{SM} .

4.2.3 Cromer on Bloom

The development of negation is another area which Cromer sees as fertile for evidence in favour of the Cognition Hypothesis. Drawing on the work of Bloom (1968), he reiterates her conclusion that, when a notional set of distinctions is made, dividing negative utterances into those which express Non-existence, Rejection or Denial, it transpires that these 'negative concepts' are encoded

syntactically in a "fairly constant developmental order" (p.214) with Non-existence being the first category to be syntactically expressed followed by Rejection with Denial bringing up the rear (for work which indicates more caution here, see Lord (1974)). The nature of the claim here is far from clear as far as the relationship between linguistic and cognitive development is concerned. It would appear necessary that it include at least the following propositions:

- (i) in the grammar which the child is acquiring, there must exist syntactic objects of some sort corresponding to the semantic/cognitive distinctions between Non-existence, Rejection and Denial, which play a part in the syntactic generation of sentences which are interpreted as expressing one or other of these semantic/cognitive notions.
- (ii) there must be a cognitive theory which recognises the substantive terms corresponding to Non-existence, Rejection and Denial.
- (iii) it must be the case that the child passes through a stage where he can be seen as controlling the relevant cognitive categories before he utilises them in his syntax.

So far as I can make sense of these propositions, the first two are false. 'Non-existence', 'Rejection' and 'Denial' are not substantive terms in any syntactic theory with which I am familiar, nor is it the case that there is a syntactic theory recognising substantive terms which

are obviously relatable to these.¹⁷¹ Non-existence, as a cognitive category, may be seen as having a role to play in Piagetian theory. In particular, it may be seen to be in opposition to a cognitive category of Existence which might be fundamental in the development of object permanence, but the same is not true of Denial and Rejection. To my knowledge, terms relatable to these do not play a role in any theory of cognitive development. As far as the third proposition is concerned, we can follow the lead of the previous section and turn to the child's negative utterances before they are syntactically complex and examine whether it is possible to categorise them in terms of the notional distinctions. If we can, we would have an argument for a collapsed developmental sequence, (C, L_1, L_2, L_3) , where C is a cognitive theory involving the three cognitive categories and L_1, L_2 and L_3 are linguistic theories employing successively the syntactic devices corresponding to the three categories (but see the above comments).

Now it is not clear that Bloom has any such argument in mind. She reports a stage before negation is syntactically

171. John Lyons has suggested to me that by broadening the notion of 'syntactic object' to include sentence types we could contemplate a correlation between declarative and imperative/desiderative sentences on the one hand and non-existence and rejection on the other. In addition, he has pointed out that some languages e.g., Turkish distinguish denial from non-existence in terms of the negative used. Cf. also McNeill and McNeill (1970) on the development of negation in a Japanese speaking child.

expressed in which "Eric produced the isolated utterance no 13 times" (1968, pp.316 -17) but has to conclude that "its interpretation was indeterminate more often than not" (p.318). At the next stage she sampled there were already instances of syntactic negation expressing Nonexistence and Rejection (the latter is regarded as only marginally productive) and, along with this, there are a number of single-word utterances interpretable as expressing both of these notions. But from this it would appear that, as soon as we are justified in assigning the semantic/cognitive categories to the child, on the basis of his non-syntactic negatives, we also find them being syntactically expressed and, therefore, even given the implausible assumptions we are starting from, the sort of argument we are looking for cannot be readily constructed.¹⁷²

In conclusion, I feel that if Cromer is arguing that before the child can mean Non-existence, Rejection and Denial he must have corresponding concepts, then he is

172. From this brief discussion of Eric's negation there appears to be the possibility that the cognitive category of Denial only emerges after the syntactic expression of Nonexistence and Rejection giving a collapsed sequence of the form, $(C_1, L_1, L_2, C_2, L_3)$, where C_1 includes the substantive cognitive terms, Non-existence and Rejection L_1 includes the linguistic or, more properly, grammatical 'reflex' of Non-existence, L_2 the 'reflex' of Rejection, C_2 contains the substantive cognitive term, Denial, and L_3 contains the grammatical 'reflex' of this. Such a sequence would be partially intermeshed but we cannot have much confidence in it given the discussion in the text.

successful, but that this is hardly surprising. If, however, he is insisting that there is an established order of cognitive development within this set of categories which is reflected in syntactic development, then he has failed to establish his point. The fact that the cognitive/semantic categories which Bloom postulates are not always transparently applicable to her data - a fact which concerns Cromer - can only contribute further to our unease.

4.2.4 Cromer on Cromer

The final battery of evidence presented by Cromer in his section on 'grammar' refers principally to his own work (1968) on the development of various sorts of temporal reference and time concepts. He puts forward arguments from several related areas, the first being concerned with the expression of the order of events in time. The sentences he is interested in each contain two verbs which have distinct temporal references. So we can compare, for example,

Can I put it on his chest so it be a button	
(future)	(later future)

in which the linear order of the sentence preserves the temporal order of the events referred to by the verbs ¹⁷³ with

D'you know the lights went off	
(present)	(past)

173. These are Cromer's judgements and whether one agrees with them or not doesn't affect the structure of his argument.

in which the order of events referred to by the verbs is reversed in the linear order of the sentence. With this distinction in mind, Cromer says (pp.218 - 19):

At the earliest stages almost all utterances with relations between two points in time preserved the occurring order of events, and it was not until after four years in Adam and four years two months in Sarah that the children began to reverse these relations occasionally.... the ability to reverse the order of events in time did not arise with new linguistic forms such as the acquisition of particular conjunctions ...Most of the reversals use linguistic forms which were available to the child at an earlier age.

What can this mean in terms of our present framework? There appear to be at least two interpretations and the first of these requires a number of dubious assumptions. Recalling that Cromer is discussing cognitive effects on grammar we could assume:

- (i) there are grammatical devices which are instrumental in reversing the order of mention in sentences away from the order in which the events referred to occurred^r.
- (ii) there is a cognitive theory employing a substantive term somehow corresponding to this notion of reversability.
- (iii) we have evidence for the child going through a stage where he controls that cognitive substantive before he acquires the grammatical devices of (i).

Now (i) is highly questionable. English grammar has no brief for taking account of the actual order in which events occurred and so a device which is sensitive to this order can hardly be part of English grammar. With this

first assumption gone it makes little sense to pursue the rest, although it is clear that the Piagetian notion of 'reversibility' might be useful in elucidating (ii) and (iii).

More profitable is the second interpretation which notes that there is a stage at which the child possesses all the necessary formal devices to compose utterances which reverse the temporal order of events in their order of mention of these events. At this stage, nevertheless, the child does not compose such sentences. Therefore, in order to explain the time-lag between the child's acquisition of the relevant formal apparatus and his using these devices to express reversals, we have to postulate an intervening cognitive stage where the child learns something about reversibility. Talking in terms of sequences, this becomes an argument for a collapsed sequence, (L_1, C, L_2) , but a question immediately arises concerning the identity of the L's. They can hardly be grammars as Cromer admits that there is no significant grammatical difference in the forms available to the child at the two postulated stages. So we are left with theories which cannot be identified with any recognisable linguistic theory, and the observation that L_2 contains some device to ensure reversibility whereas L_1 lacks such a device. C, intervening between the two linguistic theories, contains some correlate of this device which can be invoked to explain its genesis. But all this is impossibly vague and no independent evidence is adduced

for the intervention of C. It is merely proposed as an explanation for the lag in linguistic expression; such proposals can hardly be seen as adding to our understanding.

Another type of evidence comes from the acquisition of hypotheticals. Cromer says (p.220):

Hypothetical and counter-factual statements ... require complex cognitive abilities which include the ability to refer to 'possibilities' as well as the ability to change one's vantage point in a time sequence.

and the argument he constructs around this observation is of the sort considered in the second interpretation above. At a certain stage the child controls the various linguistic devices necessary for the expression of possibility and hypotheticalness this being supported by the presence of such utterances as Maybe that's my daddy, In case you're hungry I got grain, See if the flowers would like to watch me at that stage. However, these utterances do not themselves express possibility or hypotheticalness. Rather, when the child uses them, he is "asking for a determination of facts or conditions, the nature of which is unclear to him" (221). Accepting this interpretation, we now note a later stage at which the child does express possibility and hypotheticalness using grammatical devices from the earlier stage and, in order to explain the lag, postulate a cognitive stage intervening between the two linguistic stages. Again we have a collapsed sequence, (L_1, C, L_2) , and again it is apparent that the L's cannot be grammars as the same formal devices are used

in L_2 as were used in L_1 .¹⁷⁴ In this case though, unlike in the case of reversals, there does appear to be a plausible candidate as a referent for the L's; they must be theories of the sets of meanings available to the child where these sets of 'meanings' are exactly those which are capable of linguistic expression. And now we can see that the necessary clauses of the Reduction Condition are immediately satisfied. Of course the child cannot express the meaning of 'possibility' or the meaning of 'hypotheticalness' until he has acquired the related concepts of possibility and hypotheticalness and the only substantive question concerns the status of the intervening stage, C, and, in particular, its place in a theory of cognitive development. Unfortunately, as before, no independent evidence is cited for its existence.

A third argument concerns the notion of 'relevance' and its connection with the use of the Perfect in English. Taking it for granted that some such notion is involved in the appropriate use of the Perfect (see Palmer (1965)) and without committing ourselves as to the exact nature of this involvement, we can consider Cromer's argument in detail. The important observations are that the Perfect was very rare in the speech of the children studied by Cromer, only appearing marginally productive

174. Obviously there is no reason for this to be true over the whole range of syntax but we can, for this argument, restrict D to the sub-domain of syntax involved with the expression of possibility, hypotheticalness, etc.

at 4½ years. Nevertheless, the children possessed all the substantive and formal features necessary for the production of Perfects at a much earlier age, i.e., they used have as an auxiliary, they used some participle forms and, by their use of be-ing in Progressives, they indicated that they controlled a version of Affix-hopping. Just as in the case of hypotheticals, the gap between the acquisition of the formal devices and their use in the production of Perfect sentences is explained by postulating a cognitive stage, C, at which some concept of Relevance emerges, i.e., we again have a collapsed sequence, (L₁, C, L₂). It is apparent in this case too that the L's must be construed as theories of the sets of meanings available to the child and capable of linguistic expression 175

175. It might be thought that these L's could be regarded as grammars as L₂ will contain at least one rule not appearing in L₁: the generalisation of affix-hopping to apply to the -en of have-en. There are two points which must be considered in this connection. The first is that, according to Cromer's own emphasis, nothing has changed as far as the grammatical system is concerned. Secondly, and more importantly, if we treat the L's as grammars, admitting that L₂ does have features not found in L₁, it is a consequence that the collapsed sequence will not satisfy the necessary clauses of the Reduction Condition. To see this we only have to note that the vital cognitive notion in C is Relevance and we would expect this to be a translation of some new substantive term in L₂. But it is easy to see that no such substantive term exists; all we have is the generalisation of Affix-hopping to apply to -en and, by no stretch of the imagination, is it possible to see this as translatable into Relevance.

and, on the assumption that the notion of 'relevance' is involved in the semantics of Perfect sentences, we can say that L_2 differs from L_1 in employing the semantic substantive term, Relevance_S and, while this is acceptable enough, it hardly excites the imagination. As far as the intermediate cognitive stage is concerned, Cromer is on slightly stronger ground than in the previous two examples. This is because he can refer to the children's use of certain forms, before they used Perfects, which seemed to involve the notion of Relevance. So Adam, produced utterances such as Hey, what else you bring the pyjamas for?, How come you didn't bring your car today?, This one is the mostest tight you ever saw, all of which appear to involve this notion and Sarah, who produced no Perfects in the samples examined by Cromer, nevertheless, at about 4½ years began to use now and yet to relate past events to the present. To the extent that this evidence exists, the cognitive stage, C, has that much more plausibility but it would be reassuring to have independent non-linguistic evidence for C and assimilation of C into a theory of cognitive development.

The final temporal category introduced into the discussion by Cromer is that of 'timelessness'. The logic of the argument is identical to that just considered so there is little point in going into it in detail. Briefly, the syntactic devices used in the expression of timeless generics or habituals (e.g., I save dem, I keep falling

down) are very simple, involving the use of the simple present form of the main verb. Long before the age of 4 years the children studied were using simple present forms, but not to make habitual statements, an ability they only acquired at about 4 years. Therefore, to explain the lag between the acquisition of the formal devices and their utilisation in the expression of habituais, Cromer postulates a cognitive stage, C, in a sequence (L_1, C, L_2), where the L's have to be seen as linguistically encodable meanings and the claim becomes that the child must control the cognitive category of Timelessness, the essential aspect of C, before he can make habitual statements. Finally, as was the case for hypotheticals, but unlike Perfects, there is no independent evidence for the existence of C.

Taken together it is probably true that these four arguments point to a cognitive change in the child's conception of time and related notions round about 4 years. But, until they are integrated into a wider cognitive framework they can hardly be seen as contributing to our understanding except at a ^rvery superficial level.

4.2.5 Antinucci and Miller on the development of temporal reference

An analysis of Antinucci and Miller (1976) is included here for two reasons. On the one hand, it is a recent study and we might expect it to take account of any

advances with respect to argumentation in this broad area of enquiry and, on the other, being concerned with temporal reference, it will provide some perspective for the arguments of Cromer just discussed.

The authors begin with a statement of intent (p.168):

In what follows, we will try to argue that a correct understanding of the child's first past-tense forms and their gradual development cannot be attained unless we place them in relation to their cognitive prerequisites. We will see that the meaning the child encodes in his past forms is strictly based on his construction of the cognitive dimension of time.

(my emphasis - RMA)

and already we see a familiar fallacy. How else could it be other than that the child's intention to communicate particular meanings follows on his conceptualisation of notions related to those meanings? We can, however, hope that the more substantial issue of order-isomorphism is approached within the domain of cognition and the semantics of time.

Of the two common Italian past-tense forms, the imperfetto and the passato prossimo, the latter appears from the beginning of data collection (children in the study were aged 1;6 - 2;5) except that the auxiliary (corresponding to have or be) is not present and the participle forms of transitive verbs were marked for agreement in number and gender with the direct object of the verb, a situation which only obtains in Italian when the direct object is

a pronoun. By way of explanation of this fact, Antinucci and Miller offer (p.172):

The agreement of the past participle with the object signals that the children are focussing on the result of the event described by the verb. They seem to have assigned a function of CONTRIBUTION to the past participle. In other words, the children treat the past participle as an adjective...

This suggestion is combined with a semantic analysis of the verbs which the children used in their past-tense forms, which recognises three reasonably self-explanatory classes: STATE verbs, CHANGE OF STATE verbs and ACTIVITY verbs and the generalisation which emerges is that (p.174):

Stative verbs and activity verbs, i.e., those verbs which describe an event without an end result, are never used in the past tense in the children's speech, although they do occur in the present tense. (my emphasis - RMA)

This generalisation is shown to receive some cross-linguistic support from an analysis of the verbal forms produced by one of Brown's subjects, Eve, which reveals that STATE verbs and ACTIVITY verbs were never inflected for past tense at a stage when CHANGE OF STATE verbs did carry the past tense inflection.

The moves to explain these facts in terms of cognitive development now follow. The only past tense forms used by children are those which refer to events which resulted in changes in the present state of affairs. Thus John broke the doll is related to the state of affairs

represented by the doll is broken which is usually supposed to obtain at the time of the past tense utterance.¹⁷⁶ John ran down the street, using an ACTIVITY verb is not, or, at least, not an intimately, related to some existing state of affairs, e.g., that which would be referred to as John is down the street. For CHANGE OF STATE verbs (p.183):

...the past event (process) and the present moment (end-state) are related not simply by an abstract temporal relation but by a more concrete effectual relation. This concrete link is exactly what enables the child to represent the past event once he has access to the present end-state.

Thus we have an intuitive sense in which past reference involving CHANGE OF STATE verbs might be regarded as more simple than past reference involving the other verbal categories and the relationship of this intuitive analysis to cognitive development is developed in the following way (p.185):

As Piaget has emphasised (1954, 1971) the construction of the temporal dimension has its roots in the practical co-ordination of sensori-motor schemata, in which the first is preparatory to the second or the second is the result of the first. Therefore, it seems reasonable

176. Note that this is not necessary as witnessed by the non-contradictoriness of John broke the doll but now its fixed and what is being exploited here is not a linguistic fact concerning the semantics of the verb break but probabilistic knowledge about the world. See Chomsky (1976), Pulman (1977) for extensive discussion.

that the child is first able to represent and encode past events only in those situations in which this concrete, practical co-ordination is present. This offers the child a support which leads him from the present, observable state of an object to the representation of the preceding event involving the object.

It is not entirely clear how we are to construe this in terms of our current framework.¹⁷⁷ What we appear to have is a claim that when the domain of investigation is the 'semantics of pastness', there is a stage, before the stage where 'past' means something like 'before the time of utterance' at which 'past' has a more restricted meaning. Compare, in this connection, Antinucci and Miller's statement that (p.183):

The meaning of the child's past tense is at this point rather limited. He is able to encode a past event, but only if it results in a present state. Looking at this fact from a linguistic point of view, we could say that the past 'tense' has more of an aspectual than a temporal value.

Exactly how we should gloss this more restricted 'aspectual' value is far from obvious but let us refer to it as 'a'. Then the claim embodied in the passage cited above which refers to Piaget seems to be that there is a collapsed

177. One thing which seems clear is that there is a good deal of antagonism between this claim and Cromer's discussion of Perfects which could be taken as implying that the endurance of a state of affairs resulting from a past event into the present encounters the notion of Relevance. This possibility will not be pursued here.

sequence of cognitive and linguistic theories, (C, X, L, Y), where the latter are concerned with the semantics of 'pastness', where C is the cognitive stage referred to in the cited passage, L is the linguistic stage where 'past' means 'a' and X and Y are each sequences of cognitive and linguistic theories (they may of course be empty sequences or they may contain a mixture of the two theory-types). It seems to me that such a sequence fails to satisfy the necessary clauses of the Reduction Condition as there is no transparent translation from the substantive term 'a' of the linguistic theory into a substantive term of C. Certainly 'concreteness' can be seen as a concept characteristic of both theories but this is merely an intuitive observation and can hardly warrant being called 'explanation'. Taking this one step further, we might expect that, if the child's first references to past events are somehow rooted in the co-ordination of his own action schemata, then those references will be to changes of state which he himself has initiated. But a quick survey of the data presented by Antinucci and Miller gives no indication that this is so. We find such examples as E arrivato il cane ('The dog arrived') and Che ha mangiato tutti i pulcini ('That ate all the chicks'). It could be, of course, that at an earlier stage there was a consistent bias towards encoding of past events only involving the child's own initiated changes of state and it may be that this suggestion is an illegitimate extension of the authors' position. It

is, however, a fairly concrete and tangible interpretation and, it seems to me, necessary, if the proposal is to be rescued from destructive vagueness.

Contrary to what the above suggests, let us assume that the necessary conditions on reduction are satisfied. Is there more we can say? At this point the lack of any information concerning the constituency of X and Y in the sequence becomes crucial. It might be suggested that we have the beginnings of order-isomorphism with the 'first' cognitive theory being matched up with the 'first' linguistic theory but, without more details about the developing cognition and the way in which it can be related to the developing tense and aspect system, it is difficult to see such a suggestion as having much content. It thus appears that, in connection with the child's first reference to past events in English and Italian, Antinucci and Miller's attempt to reduce the phenomena to cognitive development is not particularly helpful. This is not to say that the phenomena are uninteresting, nor to say that such a reduction is impossible in principle, but merely to make clear that the introduction of Piaget's theorising does little to elucidate the intuitive semantic description in terms of verb types.

A second problem discussed by Antinucci and Miller in this paper which has implications for the above discussion is the subsequent emergence of reference to past events encoded by STATE verbs and ACTIVITY verbs. The first

thing they note is that, when this sort of reference appears, the child uses the imperfect tense and not the past form involving a participle. An examination of the contexts in which these early uses of the imperfect occurred indicated that nearly all of them appeared in 'stories' (p186):

The examples of story-telling share an interesting characteristic. The child is not narrating a past event, and in most cases is not even narrating a story that someone previously told him. The child is inventing a story at the moment ... These examples show that the first uses of the imperfect do not mark a past event at all.

The attempt to relate this fact to cognitive development is more straightforward than in the case of CHANGE OF STATE verbs and the participial past (pp.186 - 7):

Our claim with respect to the child's linguistic development is that the first instances of the imperfect form mark linguistically the cognitive distinction of pretend world vs. real world. The ability to make this distinction, as Piaget shows, is more complex, and later to develop than the ability to take account of physical transformation (which, we have argued, is the basis for the use of the participial form).

This may explain why the imperfect forms appear later.

Here the argument is clear. There is a cognitive stage, C, which includes the substantive terms, REAL and NON-REAL and a linguistic stage, L, which contains the linguistic substantive terms, 'present' and 'past'. In addition, we have the collapsed sequence, (X, C, Y, L, Z),

where X, Y and Z may be empty or may contain a mixture of cognitive and linguistic theories and the reduction of the linguistic development to the cognitive development is achieved via the translation:

'present' → REAL
 'past' → NON-REAL

along with the fact that C precedes L in the collapsed sequence.¹⁷⁸ With this account, the earlier treatment of the participial past becomes somewhat more convincing as we now have a collapsed sequence, (C₁, X, C₂, Y, L₁, Z, L₂) where X, Y and Z are as above, C₁ is the cognitive stage containing the translation, whatever it is, of the substantive term 'a' from L₁, C₂ contains the substantive terms, REAL and NON-REAL, and L₂ contains the substantive terms, 'present' and 'past'.¹⁷⁹ This sequence, apart from the difficulties in translation from L₁ to C₁, appears to satisfy the necessary clauses of the Reduction Condition

178. Note that the translation is obviously too simple as it stands because the past forms occurring with CHANGE OF STATE verbs are not taken account of and they do not involve only NON-REAL. Also the prohibition of present tense forms being used in imaginative verbal play appears unjustified but I shall ignore such complications for the sake of the argument.

179. I have assumed that this is a more appropriate sequence than (C₁, X, L₁, Y, C₂, Z, L₂) as the cognitive distinction between REAL and NON-REAL is seen as a property of the mature sensori-motor intelligence and, as such, would precede any linguistic reference to past events.

and, insofar as its structure is specified, to manifest order-isomorphism.

Antinucci and Miller's argument concludes with some observations on subsequent uses of the imperfect in 'non-actual' contexts which are used to substantiate their claim that 'non-actual' is the core meaning of 'past'. These are interesting suggestions and, as a semantic hypothesis, the thesis of the paper is cogent and probably correct. The fact remains, however, that it doesn't provide evidence for the reduction of linguistic phenomena to their cognitive counterparts except in the uninteresting sense that the cognitive system must provide the concepts before linguistic encoding can take place. The possibility that we have the beginnings of order-isomorphism, a possibility which renders the issue much more substantive, is worthy of fuller investigation.

4.2.6 Greenfield and Smith on cognition and the function of one-word utterances

Greenfield and Smith (1976) begin their relevant discussion with an explicit prediction of order-isomorphism between the semantic and cognitive domains (p.169):

We would expect that semantic development of one-word utterances should occur in the same sequence as the requisite non-verbal cognitive development, but would lag behind it.

Here 'requisite' encompasses the first two of our necessary clauses, 'lag', the third, and 'same sequence' commits the

authors to order-isomorphism. To investigate their prediction they cite a number of phenomena from semantic development and, in each case, present some evidence from prior cognitive development which can be seen as supporting the isomorphism.

Their first argument revolves around the claim that the child's one word utterances encode change of state before they encode process, e.g., down typically precedes dance or eat. This is related to some unpublished work by Mundy-Castle and Anglin showing that infants will anticipate a change of position of an object with a direct eye movement before they will follow an interpolated trajectory. The former is assimilated to the semantic notion of change of state and the latter to process and, while this is plausible enough, it remains the case that we still lack a cognitive theory in which analogues of change of state and process are defined as theoretical terms.

A further observation is that one-word utterances using 'predicates', the interpretation of which only demands one place to be filled, precede those requiring two or more places to be supplied by the interpreter. This is related to the observation that, for example, infants will act on an object before they place an object in a location. The authors say (p.171):

... action development, as it relates to object manipulation, parallels development of the expression

of semantic functions involving two objects, but precedes it in time.

It is not too difficult to imagine these ideas being related to properties of Piagetian schemata which could be seen as substantive terms in a theory of cognitive development although Greenfield and Smith are not explicit in their commitment to this interpretation.

The fact that in sensori-motor development the child moves from a stage of treating a barrier between himself and a goal as a goal in itself to one in which he will remove the barrier to reach his original goal is claimed to parallel the emergence of one-word utterances which name, for example, the intended recipient of the goal of an action, as when a child reaches for an object and utters the name of the person he intends to give the object to. Here the relationship with cognitive theory is less clear and remains at an intuitive level but it is probably fair to say that, with their explicit attempts to relate linguistic development and aspects of non-linguistic cognitive development paying attention to order, Greenfield and Smith come nearer than anyone we have discussed so far to satisfying the Reduction Condition in an interesting way.¹⁸⁰ There remains the doubt ^{that} they themselves do not articulate a theoretical position on cognitive development to put at the foundation of their arguments and, to some

180. There are a number of further examples cited by Greenfield and Smith but the above will give some idea of the quality of their work.

extent, the impression is created of piecemeal sniping at the problem, sometimes referring to visual scanning, sometimes to spontaneous action and sometimes to problem solving. Such doubts can only be allayed by future research.

4.2.7 Sinclair-De-Zwart, language and conservation

Cromer resurfaces in this last section of 4.2 in connection with his use, to support the Cognition Hypothesis, of an experimental study by Sinclair-de-Zwart (1969). In this study a group of children were tested on a standard Piagetian conservation task and, on the basis of the results, divided into conservers, non-conservers and those at an intermediate stage. Each of these groups was then tested on a set of verbal tasks involving vocabulary which was considered relevant to the cognitive processes underlying the ability to conserve. This vocabulary includes comparatives, differentiated terms (e.g., the use of dimension-specific adjectives like thick and thin as opposed to non-specific adjectives like big and small) and structures involving conjunctions of properties such as longer and narrower, shorter but fatter. These tests investigated both comprehension and production and the outcome was that, as far as comprehension was concerned, there was no significant difference between the groups of children whereas there were such differences on the production tasks. Briefly, the conservers were much more ready to use the vocabulary and constructions mentioned above than were the non-conservers. Now, as Cromer points

out, this fact in itself does no more than establish a correlation between certain cognitive abilities and related linguistic skills¹⁸¹ and the next part of the experiment investigated the question of a causal relationship between the two sets of abilities by attempting to teach the non-conservers the vocabulary and structures most relevant for conservation. Depending on the linguistic elements, Sinclair-de-Zwart experienced varying degrees of difficulty in teaching, but the vital conclusion of the experiment was that, of those children who did successfully acquire and use the vocabulary and structures, only 10% advanced on the conservation task. Similar conclusions were reached by Holland and Palermo (1975). What is the exact significance of these conclusions for the relationship between linguistic and cognitive development? It is evident that such studies show that the relevant linguistic development is not a sufficient condition for the related cognitive development and this could be seen as a refutation of a strong form of linguistic determinism. But such a refutation is not an argument for the Cognition Hypothesis. Sinclair-de-Zwart has not demonstrated that a collapsed sequence of the form, (L_V, C_0) , does not obtain where L_V is a semantic theory for the stage of linguistic development at which the child produces and comprehends that

181. Given the success of the non-conservers on the comprehension tasks, it is tempting to say that their poor showing on the production tasks represents a rather superficial linguistic disability, i.e. one would hardly be justified in concluding that the children didn't know the significance of the relevant expressions.

vocabulary and those structures considered most relevant to conservation and C_0 is the cognitive theory corresponding to operational thinking. She has demonstrated that C_0 does not follow immediately on L_V and this would seem to suggest a certain autonomy for the development of C_0 from L_V :

It is readily apparent that, if she has not demonstrated that the sequence, (L_V, C_0) , does not obtain, then she has certainly not demonstrated that the sequence, (C_0, L_V) , does obtain. Obviously, if it did, it would constitute evidence for the Cognition Hypothesis and for the reduction of the acquisition of comparatives, differentiated vocabulary, etc. to a prior cognitive development. But things are even worse for the proposed reduction because it seems that what Sinclair-de-Zwart has succeeded in doing is teaching some children enough to credit them with the linguistic theory, L_V , while admitting that they do not control the cognitive theory, C_0 . Thus, while the strong form of determinism mentioned above claiming that cognitive development follows immediately on and is directly determined by linguistic development is refuted by this study, the argument does nothing for the plausibility of the Cognition Hypothesis. This discussion, of course, presupposes that questions concerning translatability of substantive terms and identification of formal properties could be answered affirmatively.

4.3 Arguments from syntactic development

In 4.2 we have seen that arguments from semantic development have to struggle for significance in the terms of this chapter. However, there is every reason to believe that this will not be the case with arguments whose point of departure is syntactic development. While formal properties must assume some cognitive organisation, it is far from obvious that this organisation will extend beyond the linguistic domain and we can anticipate that satisfaction of the necessary clauses in the Reduction Condition will be non-trivial. 4.3.1 will consider a set of arguments advanced by Sinclair (1971) and 4.3.2 will take up a study by Greenfield, Nelson and Saltzman (1972) both of which can be interpreted in this light.

4.3.1 Sinclair on formal grammar

Sinclair (1971) notes several formal properties of grammar. These include concatenation, categorization in the formation of syntactic categories, functional notions such as 'subject-of' and 'direct object-of', and recursiveness. For each of these she points to an aspect of sensori-motor development which can be seen as accounting for it. Thus, concatenation is related to the child's ability to order things temporally and spatially, categorization to classification by sets of action schemas, functional notions to the ability to relate objects and actions and recursion to the ability to embed one schema inside another.

It is clear from this that Sinclair is concerned with syntactic development and, furthermore, that she is happy to conceive of this development in terms of formal grammars. The various linguistic properties she focusses on have all been most clearly articulated within the transformationalist approach to syntax and so it seems reasonable to construe the linguistic theories she is assuming in terms of a sequence of grammars, (G_1, G_2, \dots, G_n) , employing concatenation as a basic formal operation, including category symbols, e.g., NP, VP, containing the sort of device which makes possible the definitions of functional notions and containing recursive rules. Sinclair's claim can be seen as involving a theory of cognitive development, (C_1, C_2, \dots, C_m) , such that the necessary clauses of the Reduction Condition are satisfied by the collapsed sequence, $(C_1, C_2, \dots, C_m, G_1, \dots, G_n)$, or abbreviating sequences of cognitive and linguistic theories in an obvious way, by the two-term sequence, (C, G) .¹⁸² To what extent is this claim true? To evaluate this we need to consider each aspect of the claim separately against each of the necessary clauses.

Consider first, then, the relationship between the child's

182. The assumption that all the cognitive theories precede the grammars seems to be justified by the norms which are usually cited for the end of the sensori-motor period and the acquisition of the grammatical structures manifesting the properties in question. However, nothing of importance follows from this assumption.

ability to order things and the presence of a concatenation operation in the linguistic theories. It seems fair to say that an analogue of this formal operation must reside in a cognitive system which makes possible the sort of ordering phenomena to which Sinclair refers. It follows, therefore, that (2) of the Reduction Condition is satisfied with respect to the formal property of concatenation. For formal properties, the satisfaction of (1) does not arise and, if we go along with Piaget's and Sinclair's views on the achievements of the sensori-motor period, then the relevant formal property turns up in C before it does so in G thus satisfying (3). The first of the claims does well.

Consider the second claim. This concerns the relationship between the child's ability to classify in action and the presence, in the linguistic theories, of syntactic categories, particularly NP and VP. These latter are, of course, substantive terms in the linguistic theories and so it is satisfaction of (1) rather than (2) in the Reduction Condition that we look to. In order to satisfy (1) we need to specify a translation of NP and VP into substantive terms of the cognitive theory, C. It is not clear that we are provided with anything like such a translation by Sinclair's exposition. What seems to be necessary is the provision of a cognitive basis for the notional concepts of 'thingness' and 'doing' traditionally seen as forming the core of categories such as NP and VP.

But, while classification in action can lead the child to form categories, it is not apparent that they are of the level of generality required here. Rather, we end up with particular categories of objects (which fit into a certain schema) and categories of action (which can apply to particular objects), but it is difficult to see how the further level of abstraction required to arrive at 'thingness' and 'doing' is achieved. This is not to say that it is impossible within the Piagetian framework but we must be clear about the programmatic nature of Sinclair's remarks in this connection. Cromer (1974), in a discussion of Sinclair's paper, is perhaps aware of this deficiency when he chooses to back up her analysis with a statement made by Lyons (1966), the essential part of which is (p.131):

By the time the child arrives at the age of eighteen months or so, he is already in possession of the ability to distinguish 'things' and 'properties' in the 'situations' in which he is learning and uses language.

Lyons here is concerned more directly with the issue at hand than is Sinclair, but he provides no independent cognitive evidence on the ontogenesis of 'thingness' and 'propertyness' and so his remarks too remain programmatic. Thus, it seems that (1) is not satisfied convincingly by Sinclair's argument and, therefore, strictly speaking, the question of the satisfaction of (3) does not arise.

The argument concerning functional notions is weak. The

grammatical relations, 'subject-of' and 'object-of', depend for their definitions on both formal and substantive aspects of linguistic theory. The formal aspects include concatenation and dominance and the substantive aspects involve reference to such categories as NP, VP and S. Relevant to this we are offered the ability to relate objects and actions which clearly presupposes some notion of 'thingness' and some notion of 'doing' but we can't get much further than this. In particular, there is nothing in the ability to relate actions and objects to correspond to the category, S, and, in addition, there is nothing in this ability with which we can identify the formal property of dominance. Again, I do not wish to suggest that definitions cannot be produced in the spirit of Sinclair's analysis which would meet this sort of objection. The important point is that Sinclair's suggestion as it stands lacks rigour and precision and forces us to the conclusion that neither (1) nor (2) is satisfied by this aspect of the argument, both of them being relevant. In this situation, (3) cannot seriously be raised.

Finally, consider recursiveness. The claim is that the formal property of recursion which is necessary in the sequence of linguistic theories, G, can be identified in the sequence of cognitive theories, C, via the observation that the child can embed action schemas inside one another. But it is important to note that this sort

of embedding, in itself, does not qualify as recursion, unless it is related to a theory which makes precise the idea of a process applying to its own output. In G this is achieved by invoking a recursive rule or a sequence of rules the output of which can be input to the sequence again. So, there must be a level of abstraction in the cognitive theory at which it makes sense to talk about embedding an object of a certain kind in itself. Talk of embedding one action schema inside another does not necessarily satisfy this requirement and would appear to have more to do with such linguistic notions as 'hierarchical structure' and 'dominance' than recursiveness. Presumably, no major revision would be necessary in the cognitive framework to ensure recursiveness and so I shall assume that (2) can be satisfied for this property. (3) is then also satisfied by way of the observation that the formal property of recursion is identifiable in C before it appears in G.

Summarising, then, in Figure 67.

	Condition (1)	Condition (2)	Condition (3)
Concatenation	NR	+	+
NP and VP	-	NR	DNA
Functional grammatical relations	-	-	DNA
Recursion	NR	+	+

'+' = 'satisfies', '-' = 'fails to satisfy', 'NR' = 'not relevant', 'DNA' = 'does not apply'.

Figure 67

This is not a startlingly impressive result and we have to conclude that Sinclair's attempt to reduce grammatical development to cognitive development fails in important respects.¹⁸³

4.3.2 Grammatical constructions in playing with cups

Greenfield, Nelson and Saltzman (1972) attempt to establish parallels between a certain kind of non-linguistic cognitive behaviour and formal aspects of grammatical structure. They say (p.291):

Systematic observation of American children from
11 to 36 months of age playing with seriated nesting

183. There are two further negative remarks which could be developed. The first is that we have only been concerned with a small subset of both substantive and formal aspects of the linguistic theories presupposed by the investigation and, even if the reduction were successful with regard to the categories and properties considered here, this would still leave a large residue of linguistic constructs untouched. The exact constitution of this residue would, of course, depend upon the linguistic theory being manipulated but it is safe to say that no linguistic theory is exhausted by the terms and properties in Figure 67. The second point is that there is no question of the cognitive and linguistic theories being order-isomorphic or intermeshed because of the unstructured nature of the collapsed sequence. The stronger of these two conditions would not be satisfied by virtue of the cognitive development being complete in the relevant respects before the linguistic development gets started but it is an interesting question (not investigated, to my knowledge) as to whether any parallels can be drawn between the orderings within C and G.

cups tested the existence of a developmental sequence of rule-bound, that is, consistent strategies for combining the cups. A related objective was to investigate the question of formal homology between strategies for cup construction and certain grammatical constructions.

Briefly, the procedure adopted by the experimenters was to present the child subject with a set of nesting beakers in one of two configurations manipulating the initial conditions by, for example, handing the child the smallest cup. The effects of the different configurations and initial conditions are not relevant to the discussion which follows. Three identifiable strategies used by the children in playing with the beakers were isolated. Strategy 1, or the Pairing Method, involved placing one cup in or on a second cup and this strategy yields one or more pairs of cups. Strategy 2, or the Pot Method, involves placing two or more cups in or on another cup resulting in a structure of three or more cups which, when ordered by size, is referred to as a 'pile'.

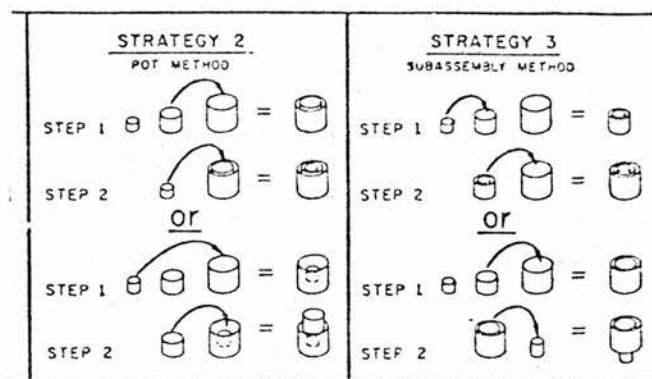


Fig. 68

Fig. 69.

From Greenfield, Nelson and Saltzman (1972, p.295).

Figure 68 shows the two alternatives for an application of the Pot Method to a set of three cups. Strategy 3, or the Subassembly method, involves the movement of previously constructed structures as units into or onto other cups or structures and Figure 69 shows two alternatives for an application of the Subassembly Method to a set of three cups.

Most children in the study had a dominant strategy and the authors conclude (pp.297 - 8):

The consistency with which a single strategy is employed by a given child demonstrates that these strategies function as internal "rules" governing the child's play over a range of concrete situations ... the term "rule" seems preferable to the term "habit" because the dominant strategy manifests itself in the child's very first approach to the task in 56 out of 64 cases.

The development of the strategies with age is summarised in Figure 70.

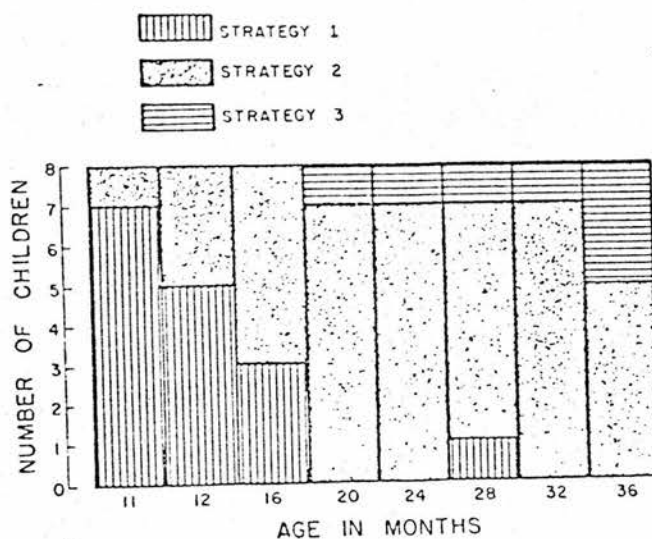


Figure 70. From Greenfield, Nelson and Saltzman (1972, p. 298).

STRATEGY 1 : PAIRING METHOD

Action Relations	ACTOR	ACTION	ACTED UPON
Descriptive Sentence	<i>cup a</i>	<i>enters</i>	<i>cup b</i>
Grammatical Relations	SUBJECT	VERB	OBJECT

STRATEGY 2 : POT METHOD

Action Relations	ACTOR	ACTION	ACTED UPON		ACTOR	ACTION	ACTED UPON
Descriptive Sentence	<i>cup a</i>	<i>enters</i>	<i>cup b</i>	and	<i>cup c</i>	<i>enters</i>	<i>cup b</i>
Grammatical Relations	SUBJECT	VERB	OBJECT		SUBJECT	VERB	OBJECT

STRATEGY 3 : SUBASSEMBLY METHOD

Action Relations	ACTOR	ACTION	ACTED UPON	→	ACTOR	ACTION	ACTED UPON
Descriptive Sentence	<i>cup a</i>	<i>enters</i>	<i>cup b</i>	→	<i>which</i>	<i>enters</i>	<i>cup c</i>
Grammatical Relations	SUBJECT	VERB	OBJECT	→	SUBJECT	VERB	OBJECT

Figure 71. From Greenfield, Nelson and Saltzman (1972, p.303).

From this it is evident that the majority of the youngest children have Strategy 1 as their dominant strategy, that Strategy 2 reaches its peak in the children aged 20 - 24 months and that the oldest children in the study use Strategy 3 the most, with three out of eight 36-month olds having it as their dominant strategy.¹⁸⁴

Turning to the relationship between this developmental sequence and the development of linguistic abilities, Greenfield et al. represent their views as in Figure 71, (p.371a and, by way of explanation of the entries in the table, they say (pp.303 - 4):

Each time one cup acts upon another to form a structure there is a relation of actor-action-acted upon, a relation most simply realised in sentence structure as subject-verb-object. In this conception, cups are the units equivalent to noun phrases in a sentence. Thus, in Strategy 1, ...the basic strategy can be represented as [in Fig.71.] . The equivalent sentence in the middle line of the figure is one possible illustration of the action relations of the top line, along with the grammatical relations of an analogous sentence on the bottom line ... Strategy 2 ... comprises multiple actor-action-acted upon sequences, each involving a different acting cup ... A convention governing the parallels between language and action presented [in Fig. 71] is that grammatical relations must appear in the same temporal order as the corresponding action relations ... in

184. Greenfield et al. continue (p.299):

At 36 months of age, seven out of eight children used this strategy at least once. This proportion is in sharp contrast to the 11-month old children who never achieved the Subassembly strategy.

Strategy 3, the Subassembly Method ... the first cup that is acted upon (b) becomes the actor in relation to cup c, just as the first object in the sentence becomes the subject of the following clause. (my emphasis - RMA)

What the claim amounts to, then, seems to be that, if we are interested in the development of a set of sentence-types, then we can translate the substantive terms that we use in describing that set of sentence-types into terms which are used in the theory of the child's strategies for manipulating cups, thus satisfying (1) of the Reduction Condition. More fully, we have the three sentence-types which we may refer to as Simple, Conjunction and Object Relative. These may be described, using the terminology of Figure 71 as:

Simple: Subject - Verb - Object

Conjunction: Subject - Verb - Object - and - Subject - Verb - Object

Object Relative: Subject - Verb - Object → Subject - Verb - Object.

and, of the substantive terms here, Subject translates into Agent, Verb translates into Action and Object translates into Acted upon. We now meet the additional argument (p.304):

We have seen that the cup strategies develop in ... sequence, but do the corresponding sentence-types follow the same developmental order? Certainly simple sentences appear first. One source of evidence on the relative ordering of the other two types of grammatical construction is provided by data from two of the children participating in Brown's longitudinal

study of speech development. The conjunction of two sentences by and was frequent in the corpus of both children before relative clauses were a regular feature of their speech.

Therefore, we have a claim that the three relevant linguistic stages marked by the introduction of a new sentence-type from the above set are, at least, order-isomorphic with three relevant non-linguistic stages and the argument appears to be one of the stronger ones we have considered in this chapter.¹⁸⁵

Nevertheless, there is a basic weakness in it. Greenfield et al. suggest that, rather than seeing the cup-manipulative ability as causing the emergence of grammatical structures in the order in which they appear, it would be more realistic to consider "a single competence underlying certain forms of action and grammar" (p.308) and this possibility leads them to claim that "evidence as to the universality of the action forms is desirable" (ibid). Preliminary work on Tzotzil speakers is reported which provides supporting evidence, but to appreciate the problem which now arises it is necessary to be aware that the sentence-types considered not only employ substantive terms but also have formal properties and so, as well as asking for satisfaction of (1) of the Reduction Condition,

185. Without more detailed information it is impossible to say that the sequence is to any degree intermeshed giving us, say, a collapsed sequence of the form, $(C_1, L_1, C_2, L_2, C_3, L_3)$, but there is nothing in the age norms presented by Greenfield et al. to rule out this possibility.

we can also look to satisfaction of (2). What are the formal properties in question? If the substantive terms, subject, verb and object are being used in anything like their standard sense, one thing entailed immediately is a measure of hierarchical structuring in the sentence-types. So, traditionally, in the structure of the simple sentence-type the verb and the object would be taken as comprising a higher order unit, the verb phrase. Is there any indication that, in the action sequences with the cups, the Action and the Acted upon have some sort of integrity not possessed by, say, the Actor and the Action? The answer appears to be negative and, indeed, there is no a priori reason for expecting an affirmative answer. The Object Relative sentence type involves an embedded structure and the question we must ask in connection with this is whether there is any evidence that the child's action sequences, when he uses the Subassembly Method, recognise the placement of cup b in cup c as somehow subordinate to the placement of cup a in cup c which is the result of the overall action sequence. Again, it appears that the answer is negative and it becomes apparent that there is essentially only linear structure involved in the action sequences with which the child manipulates the cups. Thus, hierarchisation and its particular case, subordination, are not available in these action sequences.

This might be taken as merely demonstrating that the reduction is partial (cf. the discussion in 4.1.2) but even within the limits imposed by linear structure there

are serious problems. Recall that action sequences are supposed to be universal and the emphasised part of the passage cited on pp.372 - 3 above. It follows that, whatever the language being learned by the child, his simple sentences should manifest the order, subject-verb-object. But, of course, there are now numerous recorded instances of children using a dominant word-order which is distinct from SVO.¹⁸⁶ Thus, to mention just two examples, Gvozdev's Zhenya used SOV before switching to the most common word order of SVO in Russian and Seppo, one of the children studied by Bowerman (1973), used SOV more frequently than the dominant SVO when acquiring Finnish. For these two children, then, the parallel between the structure of action sequences and grammatical structures breaks down. Obviously, the temporal sequencing of events involving the manipulation of cups is inflexible. If you're going to put cup a inside cup b you have to pick up cup a, move it and place it in cup b; there is no way in which you can move it first!

In summary, it seems to me that, so long as one attends to very simplistic ideas about sentence structure, the analysis of Greenfield et al. has a certain plausibility. However, syntactic structure is not simply linear and, as soon as we begin to take account of constituency relations in even simple sentences, it proves impossible to identify any parallel for them in the action sequences. Even as

186. It is remarkable that Tzotzil, a VOS language, should be cited in this respect since one would expect at least some Tzotzil children to use this dominant word-order.

far as linear structure is concerned, there remains an important problem, given the supposed universality of the action sequences. At best, Greenfield et al. have effected a partial reduction of linguistic phenomena to a type of non-linguistic development.

In my view, the arguments considered in this section are interesting principally in the extent to which they reveal the vast gap between what we know about syntactic structure and the fragment of this which has even been tentatively explored in terms of cognitive development. The successes have not been spectacular so far, and certainly anyone wishing to tackle the problem of a total reduction of grammatical development to cognitive development should not underestimate the task.

4.4 Strategies in language development

In two very influential papers, Bever (1970) and Slobin (1973) introduced and attempted to systematise a set of observations on language development by reference to 'strategies' of one sort or another and, since then, such strategies have come to play an increasingly important role in language acquisition research (for a recent review, see Cromer (1976)). In the case of Bever, strategies were seen as important principally in the perception of sentences and formed part of a mapping from 'external' to 'internal' forms.¹⁸⁷ His attention to the development

187. In this role, they were opposed to grammatical transformations. For development and summary of Bever's position, see Bever (1974, 1975), Fodor, Bever and Garrett (1974) and for effective criticism of some of Bever's suggestions Grosu (1975).

of such strategies was limited but a few suggestions were made. Slobin's speculations were presented in a developmental framework and, for him, the notion of 'strategy' seemed somewhat wider than for Bever, accommodating Bever-like processes but also including what we might regard as 'heuristics for language learning' which govern the course of a child's learning rather than his perceptions as he learns. Both authors view their proposals as intimately connected with cognitive development and the extent to which they are justified in this regard is considered in this section. Bever's ideas are briefly discussed in 4.4.1 and Slobin's theorising is the subject-matter of 4.4.2.

4.4.1 Bever on the cognitive basis of linguistic structures

Bever's theorising need not detain us long since it has received its fullest application outside the developmental sphere. From our point of view all that is important is his discussion of the reliance of children, at certain stages of their linguistic development, on behavioural strategies rather than linguistic rules in their comprehension of sentences. He produces evidence that children operate with the following strategies at different stages and claims that, whereas for the adult such strategies are heuristics, which can be abandoned in favour of knowledge of linguistic rules if the circumstances demand it, for the child they can be seen as completely determining his behaviour on certain tasks:

Strategy B: The first N...V...(N) clause is the main clause unless the verb is marked as subordinate

Strategy C: Constituents are functionally related internally according to semantic constraints

Strategy D: Any NVN sequence within a potential internal unit in the surface-structure corresponds to actor-action-object

(Labelling of the strategies follows Bever (1970))

Although Bever is not explicit in this respect he seems to want to consider the possibility that these strategies are a reflection of general cognitive capacities and he says (p. 312):

Just as certain linguistic structures may be 'innate' and some learned, certain perceptual strategies may be basic to all perceptual processes, and some derived from linguistic experience.

It is the first of these possibilities which concerns us here and, unfortunately, Bever gives no clue to what role B, C and D might play in other perceptual processes. This is hardly surprising since inspection of them shows that they are exactly the sort of process we might expect to be induced on the basis of linguistic experience and so come under the second possibility in the passage cited above. As they stand, they are obviously specific to language and there is no readily available translation to make them applicable in the domain of, say, visual

perception.¹⁸⁸ So, for the few examples which are argued to have a developmental role, there is no case established for their relationship to cognitive or perceptual development and, even if the notion of 'perceptual strategy' plays a role in language acquisition, nothing has been said to show that such strategies extend beyond the perception of language. Of course, the general notion of 'perceptual strategy' is poorly defined, as Bever would readily admit, but until it is elucidated there is little room for constructive speculation.¹⁸⁹

4.4.2 Slobin and cognitive prerequisites for grammar

Slobin (1973) can be seen as adopting the definitional position on the relationship between linguistic and cognitive development discussed in 4.1.1. He says (pp.175 - 6):

Every normal human child constructs for himself the grammar of his native language. It is the task of developmental psycholinguistics to describe and attempt to explain the intricate phenomena which lie beneath this simple statement. These underlying phenomena are essentially cognitive. In order for the child

188. Bever's attempts to draw parallels between linguistic strategies and general perceptual strategies in later sections of his article are confounded by a profusion of counter-examples (see Grosu (1975) for details). Also, none of these later strategies are examined in a developmental context.

189. I might also mention that, although B, C and D are developmentally ordered, no corresponding order is suggested for general perceptual principles for obvious reasons.

to construct a grammar: (1) he must be able to cognize the physical and social events which are encoded in language, and (2) he must be able to process, organize and store linguistic information. That is, the cognitive prerequisites for the development of grammar relate to both the meanings and the forms of utterances. (first emphasis mine, others in original - RMA)

As suggested in 4.1.1 this view seems to remove an interesting set of questions from the arena of discussion. We can agree with Slobin that the phenomena are "essentially cognitive" but still ask whether we can identify 'similar' phenomena in non-linguistic domains of cognition. As we shall see, Slobin himself is not consistent in adopting this definitional stance.

His argument is launched by asking whether it is possible to "trace out a universal course of linguistic development on the basis of what we know about the universal course of cognitive development" (p.180), and he presents the following summary of data in support of a positive answer to the question (p.180):

The earliest grammatical markers to appear in child speech seem to express the most basic notions available to the child mind. For example, in languages which provide a vocative inflection, this is typically one of the earliest grammatical markers to emerge in child speech ... One of the earliest semantic relations to be formally marked in child speech is that of verb-object. In order languages like English, this relation is marked early by consistent word-order. In languages which provide an inflection for marking the object

of action (accusative) this is typically an extremely early inflection to emerge - often the first. In Luo the first inflections are subject and object affixes on verbs ... In every language for which relevant data are available, there is an early form of negation in which a negative particle is affixed to a simple sentence. In languages as diverse as English, Arabic, Czech, Latvian, Japanese and Samoan, early yes-no questions are formed by rising intonation.

All of this is true and well backed-up by research findings but what exactly does it show?. It would appear to demand that there is a ranking of 'concepts' in some cognitive theory such that each of the linguistic phenomena mentioned in the above passage can be translated into a 'concept' which is relatively early in the ranking. But, to my knowledge, no cognitive theory with properties amenable to treatment in these terms exists and it is not a priori clear why 'vocative', 'verb-object' and 'negation' (presumably among others) should comprise the "most basic notions available to the child mind".¹⁹⁰ It seems to me that citing data in the fashion in which Slobin does

190. Note that I am not suggesting that the child can use, say, the vocative inflection meaningfully and correctly before he has the relevant concept of an addressee to be communicated with or whatever. Such a suggestion would clearly be false, as pointed out in 4.2. But its converse is rather uninteresting in itself and only becomes more interesting when it is embedded in a theory of cognitive development which will enable the child language theorist to make predictions about the course of grammatical development.

here adds nothing to the substance of the debate.¹⁹¹

The more substantial aspects of Slobin's paper develop from his discussion of the work of Mikeš and Vlahović with children being brought up bilingually in Hungarian and Serbo-Croatian focussing on their acquisition of the locative systems in the two languages (see Mikeš (1967)). The two systems are acquired at different rates and this cannot be explained by reference to the content of locative utterances since this content is assumed to be identical whichever language is being spoken. However, the system of expression (or, according to some usages, the grammar) of Serbo-Croatian locatives is considerably more complex than that of Hungarian locatives and the organization of these two systems is seen by Slobin as a cognitive task (adopting the definitional stance again) although, as I

191. Interestingly, at this point, Slobin makes remarks which would seem to indicate that he is not a definitionalist (p.181): ... although one can talk about order of acquisition in terms of semantic or cognitive complexity, there is clearly a point at which formal linguistic complexity also plays a role.

The observations which lead to this remark concern Bowerman's work on yes/no questions in Finnish and Omar's work on Arabic plurals. The most interesting way to look at these findings from my point of view is discussed at length in 4.5. Whether Slobin is adopting a definitional stance or not is, to all intents and purposes, not interesting for my aims here. Both the definitional stance and the position adopted in the passage above beg the most interesting questions and it is largely the purpose of this chapter to raise those questions for the phenomena Slobin is concerned with.

argue in 4.5, it is perhaps more rewarding to view this as an empirical question awaiting further research in linguistic and other cognitive domains. Of more immediate interest is the fact that differential complexity of 'expression-system' is not the only variable which Slobin identifies in the two languages. He points out that the Hungarian locative is consistently expressed by noun suffixes and a considerable amount of evidence is amassed to indicate the facilitatory effect, for acquisition, of coding locative notions by suffixes (rather than by prepositions or prefixes).¹⁹² This leads Slobin to postulate a developmental universal (p.191):

Universal: Post-verbal and post-nominal locative markers are acquired earlier than pre-verbal and pre-nominal markers.¹⁹³

and, because it is extremely unlikely that the universal is limited to the expression of locatives, he goes on to

192. Serbo-Croatian uses a combination of suffixes and prepositions and the suffixes are the part of the locative system which is learned first.

193. It is worth noting that this universal must embrace an 'other things being equal' clause. To see this, it is easy to imagine a language marking locatives both pre- and post-nominally with the pre-nominal marking being absolutely regular but the post-nominal marking being phonologically conditioned in complex ways. Then, although it remains an empirical question, it seems to me that Slobin's universal might meet an exception. With an 'other things being equal' clause this wouldn't count as an exception but, conceivably, the domain of application of the universal would be reduced to zero.

propose a principle with which the child approaches language learning (p.191):

OPERATING PRINCIPLE A : Pay attention to the ends of words

It is the status of this principle which deserves fairly detailed discussion. First, it appears that the domain of enquiry from which it emerges is something like 'strategies and heuristics which will help in the learning of language' rather than 'strategies and heuristics for the perception of particular utterances', i.e., such strategies have, as their rationale, a theory of the language being acquired and not an understanding of a particular utterance, although they will inevitably contribute to such understanding. This interpretation seems to be confirmed by Slobin's reference to the "information-processing devices used and developed by children to understand speech and to construct grammars" (p.187. my emphasis - RMA). Presumably, as the child develops, the set of operating principles he has available changes and, when the language system is acquired fully (if it ever is), operating principles will cease to function. In this light they are best seen as part of a mechanism, M, fundamental to understanding the child's progression towards language mastery (see Chapters 1 and 7 for inadequate discussion).

Can any close relative of Operating Principle A be seen at work in other cognitive domains? Obviously, as it is

worded, A is^a purely linguistic device referring as it does to "the ends of words"; but it is perhaps possible to see it as an instance of a more general cognitive principle which we could phrase as follows:

In any temporally ordered sequence of events pay attention to the most recent ones

and this could be seen as related to a 'last in - first-out' view of short-term memory. That Slobin wishes the principle to be extended within the linguistic domain is apparent from some of the examples he uses which require attention not to the ends of words but to the ends of sentences.¹⁹⁴

Whether or not there is support for the general cognitive principle in the form suggested above or in some related form is not what concerns me here. The important point is that the question can be raised in an intelligible form and we can begin to get a grip on the sort of evidence which would argue for or against it. Slobin seems to assume that A itself is cognitive but such an assumption buries the interesting questions.

Operating Principle A presupposes that the phonological

194. Of course, this may raise a difficulty for the principle as, within any linguistic utterance, there are a number of domains of temporally ordered sequences of events, e.g., syllables, morphemes, words, phrases and sentences. To see these domains being manipulated in an experimental context with adults, see Savin and Bever (1970), Foss and Swinney (1973), McNeill and Lindig (1974).

form of words can be modified and Slobin offers some evidence that children approach language learning equipped with (p.192):

OPERATING PRINCIPLE B : the phonological forms of words can be systematically modified.

Just like A, B, as it stands, is restricted to linguistic phenomena, but the issue arises as to whether it can be regarded as an instance of a general cognitive principle, and what suggests itself is the principle that invariants will tolerate a certain amount of variation, a principle which can be seen as underlying all instances of generalisation and as involved in the constancies of visual perception. It is far from clear that the matter can be interestingly pursued at this level of generality but, again, I am content merely to raise it.

Slobin introduces a section of his paper entitled 'Constraints on Linguistic Performance' with (p.195):

By and large, the language processing variables to be discussed below are determined by the fact that human language is produced and received in rapid temporal sequence ... The sorts of processing variables considered here are therefore closely linked to general perceptual and performance-programming principles.

This makes the subject-matter sound very much like that discussed by Bever, but Slobin goes on (p.195):

The constraints on linguistic performance are both short-term and long-term. The short-term have to do with the ongoing use of speech, and the long-term

with the storage and organization of the linguistic system.

and the second category here sounds more like what we have already been considering - heuristics which the child brings to language learning - than it does like heuristics for sentence perception. The extent to which this distinction is clear in Slobin's own presentation is something to which I shall return below. As far as short-term constraints are concerned, he makes his position clear when he says (p.196):

... the short-term limitations under which children operate ... are universal human limitations on sentence processing, and they are based on general perceptual and information-processing principles.

From this general standpoint , another operating principle is introduced (p.197):

OPERATING PRINCIPLE C: Pay attention to the order of words and morphemes

Is this a short-term strategy or a long-term heuristic vital to the child's eventual grasp of the language? Slobin's discussion fails to provide an answer, but the answer required appears to be 'both', i.e., it will clearly facilitate the child's learning of (most) languages if he assumes that the order of the morphemes in the utterances he hears is significant and it will also facilitate his comprehension of particular utterances. However it should be taken, can we make sense of the view that it is based on "general perceptual and information-

processing principles"? Obviously, as it stands, it is restricted in its application to linguistic materials, mentioning such linguistic constructs as "words" and "morphemes" but it would be possible to see it as an instance of a general principle of the form:

Pay attention to the order of events or to the order of items which are presented serially and regard it as significant

If a serious argument were to be constructed along these lines, we would need to see this general strategy being accommodated to a cognitive theory and appearing in cognitive development before it showed up as a linguistic strategy and, while such a situation seems plausible, it is apparent that much more discussion will be necessary before the matter can be regarded as closed.

The next operating principle we are asked to consider is one which is admitted by Slobin to have ambiguous status with regard to the short-term - long-term distinction.

(p.199).

OPERATING PRINCIPLE D : Avoid interruption or re-arrangement of linguistic units

Slobin claims that (p.199):

A number of strategies can be related to this principle - both strategies for speech perception [i.e., short-term constraints - RMA] and strategies for the formation and use of rules of production [i.e., long-term constraints - RMA].

A considerable amount of evidence concerning inversion

and permutation is cited in support of the strategy but its empirical status is not what concerns me. As stated, D is restricted to linguistic phenomena as it mentions "linguistic units" but it can be seen as an instance of a general principle used most productively by Gestalt psychologists and, interestingly, cited by Fodor and Bever (1965) in their first experiments involving the location of a click superimposed on linguistic materials:

Perceptual units will tend to resist interruption¹⁹⁵

The obvious next step is to investigate the ontogenesis of the general principle in contrast to D and, if it is prior to it, we shall have a situation which is consistent with the linguistic principle being determined by the general cognitive principle.

A fifth operating principle also appears to have ambiguous status (p.202):

OPERATING PRINCIPLE E : Underlying semantic relations
should be marked overtly and
clearly

In a sense, this could be interpreted as a heuristic which the child brings to language learning: assume that there will be some clear correlation between semantic relations and their realisation in syntactic form. In another sense, it can be seen as the source of predictions

195. This principle has an immediate corollary:

Any perceptual event which interrupts a perceptual unit will contribute to the complexity of the perception in a disproportionate manner.

of difficulty in sentence processing: to the extent that semantic relations are not clearly marked, a sentence will be difficult to understand. That Slobin either has both these interpretations in mind or is unclear of the distinction between them is apparent from the evidence he cites in support of the principle. This includes evidence for (p.202):

Universal E1: A child will begin to mark a semantic notion earlier if its morphological realisation is more salient perceptually (*ceteris paribus*)

which is most straightforwardly interpreted as the result of a strategy which the child brings to language learning as well as evidence for (p.203):

Universal E5: It is easier to understand a complex sentence in which optionally deletable material appears in its full form

which is best interpreted in terms of strategies for sentence-perception.

Whatever the possibilities of confusion here, we can again ask whether E is an instance of a more general cognitive principle and, in this case, it is difficult to begin to conceptualise such a strategy. As it stands, the reference to "underlying semantic relations" and "marking" involves linguistic terms and there are no obvious non-linguistic cognitive correlates for them. Of the operating principles we have considered so far, then, E would appear to be the strongest candidate for being autonomous of any general cognitive factors.¹⁹⁶

196. It is to be emphasised that Slobin provides no discussion in this connection.

The phenomenon of overregularization is the topic addressed by the next operating principle (p.205):

OPERATING PRINCIPLE F : Avoid exceptions

This is unambiguously involved in language learning rather than in sentence perception and, so far as I know, the possibility of exceptions and irregularities contributing to the difficulty of sentence perception has not been explored. F could be an instance of a general cognitive principle of the form:

Make the widest possible generalisations avoiding peculiarities

but one of the problems with a principle such as this is that it doesn't have a great deal of evidence in its favour. Most relevant in this respect is perhaps the work of Brown (1958) and Rosch et al. (1976) discussed in some detail in Chapter 2 and which can be seen as indicating that maximisation is not the sole determinant of semantic categories. It would be hazardous to speculate further at the moment until we have a much tighter formulation of the principle itself and any possible related cognitive strategy.¹⁹⁷

The final operating principle discussed by Slobin takes

197. It is also unclear how such a blanket strategy can deal with the learning of irregular forms before regular forms as discussed in Chapter 1 unless F itself is to be fitted into a developmental sequence of strategies of which an earlier member is:

Treat everything as an exception

account of the fact that "errors within choice of functor are always within the given functor class" (p.206), and the "numerous examples in the cross-linguistic data of the principle that rules relating to semantically defined classes take precedence over rules relating to formally defined classes, and that purely arbitrary rules are exceptionally difficult to master" (ibid).

OPERATING PRINCIPLE G : the use of grammatical markers
should make semantic sense

Again, G looks more likely to be of use in learning the language than in understanding sentences and I am aware of no studies indicating that arbitrariness in grammatical form contributes to perceptual complexity in a disproportionate way.¹⁹⁸ But the relationship of this strategy to cognition is difficult to be clear on. As phrased, it refers to "grammatical markers" and "semantic sense" which are linguistic terms and, although, if we consider the introduction of arbitrary distractors or variable realisations of properties into concept formation tasks or problem solving situations, we would find them contributing to task complexity, this is not the same thing as saying that we can identify a cognitive strategy

198. It is easy enough to imagine how such studies might commence. For example, we could contemplate investigating, for a language with non-natural gender, whether noun phrases with arbitrary gender contributed to difficulties of perception, memory, etc. more than noun phrases with natural gender.

in development of which G is an instance.¹⁹⁹

In summary of Slobin's often unclear position, it seems to me fair to make the following points:

- (1) He appears, at times, to take a definitional, and, therefore (for me), uninteresting stance on the relationship between linguistic and cognitive development.
- (2) At other times he indicates that systems of linguistic expression can sometimes be seen as autonomous of cognitive development.
- (3) Regarding content of linguistic utterances, Slobin appears to fall into the trap of believing that there is a substantive issue concerning the existence of corresponding concepts. The issue only becomes substantive when we have a cognitive theory and a linguistic theory which are explicitly related and, perhaps, intermeshed or order-isomorphic. For the examples Slobin cites, these theories are conspicuously absent.

199. Notice that if we were to suggest something like:

Variation is significant
with its corollary

Non-significant variation contributes to complexity this would be in conflict with some of the suggestions I have made earlier in connection with strategies for dealing with constancies. To try to avoid this conflict by suggesting that the child has some general strategy of the form:

Some variation is significant
is to merely restate the problem. What we are interested in is exactly which variation is significant and how the child knows or learns that it is.

(4) For systems of expression (mappings from content to form), Slobin says nothing of the formal properties which appear to give rise to complexity nor of the possibility of identifying these same formal properties in non-linguistic cognitive domains, i.e., the interesting questions are simply not raised.

(5) There are some problems concerning the precise status of the operating principles: are they heuristics for language learning or for sentence perception or both? Slobin is sometimes not clear on this issue perhaps not seeing the distinction as a clear one which needs to be drawn.

(6) As far as the relationships between the operating principles and general cognitive strategies is concerned, Slobin fails to provide the theoretical account we are entitled to expect. This could be because he feels that the relationship is self-evident but, as I hope to have demonstrated, there are certain cases where this is not so. Where it is possible to speculate intelligently about the identity of general cognitive strategies, it seems that Slobin might be correct but the matter deserves to be investigated in more detail than has been possible here.

(7) No ordering relationships are established between the operating principles and their cognitive analogues when these latter exist.

4.5 Cromer on autonomous linguistic development

In 4.2.1 I briefly discussed Cromer's use of Brown's views on the relationship between sensori-motor intelligence and Stage I speech to support the Cognition Hypothesis. Picking up a similar discussion in Brown (1970), Cromer says (1974, p.236):

... the possession of sensori-motor intelligence would still not explain the expression of that intelligence in language. That early grammar expresses the meanings which sensori-motor intelligence makes possible does not in itself solve the mystery of how these meanings are conveyed by a grammar.

This is absolutely true and raises a clear issue. If we have a theory of the meanings available to the Stage I child and we also have a way of representing the form of Stage I speech, then we ought to be able to work out a mapping between the two.²⁰⁰ However we conceive of this mapping, it will have, one would assume, substantive and formal aspects and we can examine what we know about the development of the child in other cognitive domains searching for translations of the substantive terms and identical formal properties. It seems to me that, at this stage, there is virtually nothing we can say with regard to this problem. On the one hand, the theory relating the set of meanings to the child's forms is, as far as precise formulation is concerned, virtually non-existent (see Schlesinger (1971, 1974) for proposals which indicate

200. I don't wish to commit myself to this being a grammar in the Chomskyan sense.

the validity of this claim). On the other hand, our knowledge of the formal properties and relations with which it is necessary to credit the child in order to explain his developing abilities in other cognitive domains is rudimentary, to say the least. In particular, it seems nonsensical to restrict ourselves to the Piagetian theory of cognitive development at the expense of, say, theories of visual or auditory perception.

Cromer assumes an answer to the question of the linguistic independence of the system of expression without even raising the issue of exactly what other domains of child behaviour and child knowledge should be considered relevant. The fact that organisms other than humans also possess the set of meanings associated with sensori-motor intelligence has no bearing on this argument. Brown and Cromer point out that, although the chimpanzee Washoe shows evidence to suggest that she manipulates these meanings, she does not appear to control a system for their consistent expression. But this could be related to differences in other aspects of Washoe's information-processing capacities when compared to a human infant. Just as she lacks the system of expression with its properties, P_1, P_2, \dots, P_n , so also she may lack the system X which also has the properties, P_1, P_2, \dots, P_n . The human infant controls both a system of expression and the system X and the fact that X is a non-linguistic system indicates that the development of the linguistic system of expression need not be autonomous of development

in another cognitive domain (we would be tempted to believe that it was not if X showed up before the linguistic system of expression).²⁰¹ Therefore, nothing can be concluded from the case of Washoe on these issues. Of course, the system X is hypothetical and perhaps the best course is to assume that the system of expression is autonomous until evidence comes along to show that it is not - this seems to be the position Chomsky has preferred throughout his writings (but cf. fn. 200).²⁰²

Cromer considers a number of additional arguments for the independent development of language, none of them very convincing. The first concerns negation and the central fact on which the argument hangs is that, given Bloom's (1968) three-way distinction of Non-existence, Rejection and Denial, the syntactic expression of each of these categories changes, becoming more complex, as the child develops. Cromer says (p.239):

If to express the meaning of non-existence, the child comes to use more and more complex techniques over time, these new structures cannot be being acquired due to advances in 'meaning'.

201. We could infer on this basis that the set of properties, P_1, P_2, \dots, P_n , are characteristic of cognitive systems which are unique to man, but, of course, this is an independent issue from the autonomy of any aspect of language development. For the former we need to make cross-species comparisons but, for the latter, we can restrict our attention to humans.

202. see p. 398a.

202. Two further remarks can be made in this connection. The first is that it seems to me that, if the sort of expression-system which has been envisaged by child language theorists for the early stages of language development is made precise, then the sorts of formal properties they manifest will be identifiable in non-linguistic information processing systems. The second point is that the sort of property Chomsky seems to have in mind when he discusses this sort of argument has become more and more abstract throughout his writings (cf. structure dependence of rules with, say, the specified subject constraint (Chomsky (1973)) and, certainly in the case of some of the properties he has investigated recently it is difficult to imagine them having any role to play outside the linguistic system. Further to this second point we should note again that it is not clear that Chomsky is concerned with systems of expression in the sense in which this phrase is being used here, but with grammars which are theories of the native speaker-hearer's competence and which only relate to the expression system in an indirect way. It would be quite possible that the system of grammar, as envisaged by Chomsky and his associates, should have properties which make it independent of any non-linguistic cognitive system while the system of expression should share all its properties with such systems. Whether child language theorists have been concerned with studying the development of Chomskyan grammars or of expression systems is of course a difficult question, briefly mentioned in Chapter 3.

But the assumption here that there is no change in meaning is surely to take the status of Non-existence, Rejection and Denial as semantic categories too literally. To see this, compare a selection of Eric's utterances, categorised as expressing Non-existence, from his Stage II with a selection of his utterances expressing the 'same' semantic category from Stage V.

Stage II: no more juice, no more noise, no more

Stage V : no more ball, no more bridge, oh no fire engine,
you no bring choo choo train, I no reach it,
I didn't do it, I didn't crying

In the second list there are some sentences which are identical in structure to those on the first list but, of course, they are not relevant to an evaluation of Cromer's claim. Of the other sentences on the second list, how is it possible to say that, as far as their negativity is concerned, they do not differ in meaning from the sentences on the first list? It is not, and this amounts to a criticism of Bloom's semantic categories which are no more than broad notional sets, and fairly misleading ones at that. So it is not the case, contrary to Cromer's assertion, that there is a clear syntactic development in the expression of a single unchanging semantic category. But suppose for a moment that he is correct. Nothing would follow from this until the 'new' formal principles (and, perhaps, substantive terms) involved in the more complex system of expression are spelled out in detail. Then we

would be in a position to investigate whether these formal principles are already used in some non-linguistic cognitive domain. As things stand, Cromer's assertion has no more support than any of the imaginable alternatives.

A similar argument draws on the work of Bellugi and concerns the development of the child's abilities to refer to himself (see Bellugi-Klima (1969), Bellugi (1971)).

Cromer's summary is admirably clear (pp.241 - 2):

At the earliest stage, the child used his own name in all positions ... During this stage he was making utterances like:

Adam home

Adam go hill

Like Adam book shelf

Pick Adam up

In the second stage, he began to substitute the pronoun 'I' for his name if it occurred in the first position in the sentence, and occasionally produced both together:

I like drink it

I making coffee

I Adam driving

I Adam do that

And at the same time he was substituting 'me' for his name if it occurred other than in the first position in the sentence (with the exception of vocatives and greetings). This strategy produced some adult-like sentences:

One for me

Wake me up

Why laughing at me?

But it also produced a regular pattern of errors:

Why me spilled it?

What me doing?

At the third stage, the rules changed and the child now produced 'I' if his name served a nominative function in the sentence, and he produced 'me' if it served an object function in the sentence:

That what I do

Can I put them on when I go outside?

You watch me be busy

You want me help you?

That is, the child's pronouns no longer depended on sentence position but on grammatical function.

With regard to this sequence, Cromer continues (p.242):

...the developments are not solely based on meaning or reference. Throughout, the meaning remained the same-reference to self.

But this is fallacious. Certainly when the child says That what I do he is referring to himself and is using the pronoun, I, to do so. But this is not to say that I, in the child's semantic system, has the same meaning as the child's own name at that stage (or at an earlier stage). This is to confuse meaning with reference among other things and, indeed, an identical argument could be constructed from adult usage with absurd consequences. Putting this aside, assume that, for the forms, Adam, I and me, it makes sense to talk about their meaning being some constant 'reference to self'. Then we can suggest that the child must operate with 'rules' at the three stages in question which we can represent in the following way:

Stage 1 (all references to self are Adam)

'reference to self' \longrightarrow Adam

Stage 2 (first position references to self are I or I Adam, all other references to self are me)

If 'reference to self' is in an initial position,
then

'reference to self' \longrightarrow I or I Adam

Otherwise

'reference to self' \longrightarrow me

Stage 3 (nominative reference to self are I, objective references to self are me)

If 'reference to self' is nominative, then

'reference to self' \longrightarrow I

If 'reference to self' is objective, then

'reference to self' \longrightarrow me

These 'rules' are not intended to be taken very seriously but they do begin to make apparent the development of the child's system in formal terms. So, it is clear that the child's rule at Stage 1 is context-free while the rules from Stages 2 and 3 are context-sensitive (only an impressionistic comparison with rewrite systems is intended by the use of these terms). Similarly the context which the rules at Stage 2 use is that provided by the linear structure of the sentence whereas, at Stage 3, it seems that reference must be made to hierarchical structure although the use of the term 'nominative' is not explained. From this we might conclude that the rules in the second stage have something in common with context-sensitive

rewrite rules and that the rules at Stage 3 are reminiscent of transformational rules. All of this could be quite accidental and is anyway incidental to the main point which is that the rules should have some identifiable formal properties which we can then search for in non-linguistic domains of development. If we can identify such properties in a non-linguistic domain X and, furthermore, show that the development in X precedes the development in the system of pronominal reference, then we shall have the beginnings of a reduction where Cromer⁴ is saying there isn't one. Nothing is gained by prejudging the issue.

The final set of arguments presented by Cromer is also used by Slobin and concern the relatively late appearance in some languages of the appropriate linguistic encodings for particular semantic contents when this is compared with the appearance of encodings of the same content in other languages. So, although it has often been reported that children acquiring English ask their first yes/no questions using rising intonation, Finnish children do not ask yes/no questions when they are at an otherwise comparable stage of development (see Bowerman (1973) for details). The explanation offered is that Finnish lacks a simple intonational device for forming yes/no questions (but cf. Sauvageot (1949)) having resort to a relatively complex operation involving a moved question particle. Note that, in this case, since it is claimed that Finnish

children simply do not ask yes/no questions, i.e., there is no direct evidence that they control the relevant meanings, the claim that the late development of yes/no questions in Finnish is explicable in terms of the complexity of the mapping from meanings to forms is on somewhat weaker ground than in other examples. A second example concerns the acquisition of plurals in Arabic and the vital observation is that children's acquisition of the Arabic plural system is not complete by the age of fifteen. This is put down to the severe irregularities which are encountered in this system.²⁰³ The final, and best-known, example comes from Slobin's interpretation of Mikeš's work (see 4.4.2 for a brief description).

In each of these three cases Cromer is claiming that there is something peculiarly linguistic about the development but, again, we can only conclude that he is prejudging the issue in a clumsy fashion. So long as it is only the semantic system which can interact with the cognitive system he would seem to be correct. But no plausible reason is advanced for restricting the enquiry in this way and it is much more interesting not to do so. So, for the case of the Finnish yes/no question, it would be

203. Presumably, in this study (Omar (1970)) which I have not seen there was evidence that the children were intending to communicate about collections of objects, etc. even when they did not get the right form and, to this extent, we have a slightly different case to the Finnish yes/no questions.

necessary, before the relevant issue can be precisely formulated, to have a detailed statement as to exactly what formal properties are central to this tiny area of Finnish grammar. Given such a statement, we would be in a position to investigate other cognitive domains looking for just these properties. If we can identify them in such a domain and if, furthermore, it transpires that they are manifest in that domain before they appear in the linguistic domain, then we would have the beginnings of a reduction. Similar remarks can be made for each of the other two cases mentioned above. One can imagine the discovery of a collapsed sequence, (C_1, L_1, C_2, L_2) , where C_1 is a cognitive theory with formal properties, P_1, P_2, \dots, P_m , L_1 is an expression system (or grammar) with P_1, P_2, \dots, P_m and that the properties, P_1, P_2, \dots, P_m are not sufficient to handle the complexities of, say, the Serbo-Croatian locative system, C_2 is a cognitive theory with formal properties, $P_1, \dots, P_m, \dots, P_n$ and L_2 is an expression system with $P_1, \dots, P_m, \dots, P_n$, and $P_1, \dots, P_m, \dots, P_n$ are sufficient to handle the complexities of the Serbo-Croatian locative system. Such a sequence would be intermeshed and provide us with the strongest reason for believing that the system of linguistic expression depends crucially on an antecedently developed cognitive system. It is, of course, a purely hypothetical example and awaits the formulation of the various linguistic properties to merit serious investigation. It is, however,

easy enough to imagine and it seems to me that something which is so easy to imagine cannot be ruled out on a priori grounds. If Cromer has thought about these issues at all this is exactly what he appears to do.

Cromer concludes (p.245):

We can see then, that cognitive development and linguistic development do not necessarily proceed together ... cognition can make certain understandings available, but there may be linguistic constraints.

The arguments which he has presented on these issues are either self-evidently valid or fallacious. Other authors considered in this chapter do somewhat better but the overall impression is that so far there has been a remarkable lack of awareness of the complexities involved in demonstrating dependence or independence of linguistic development when compared to cognitive development.

CHAPTER 5 : PHONOLOGICAL DEVELOPMENT

In this chapter I return to the procedure followed in Chapters 2 and 3 of evaluating proposals against Conditions 1 - 4 of Chapter 1. The domain under investigation is the child's ability to produce the forms of words and, because there has been a good deal of unclarity with regard to methodology in this area, I wish to leave open the question as to whether this involves confusing a number of abilities or not. The suggestion that it might arise if one takes the view that the child's ability to repeat word forms is not identical with his ability to produce such forms spontaneously in his natural speech. One of the studies to be discussed below (Smith (1973)) had resort to techniques which could lead to confusion of these abilities and, from what we know about the child's ability to imitate syntactically structured strings when compared with his spontaneous production of such strings, we might expect serious problems to arise here (see, for example, Fraser, Bellugi and Brown (1963), Slobin and Welsh (1973)).²⁰⁴

Somewhat neglected during the 1960's, phonological

204. In connection with this, Smith says (p.9):

... I do not think that this elicitation is in any way misleading. Typically, a sound or contrast he [the child under study - RMA] was able to repeat after me, he would be able to produce spontaneously a few days or at most a few weeks later.

For a more pessimistic view, see Kornfeld (1971).

development has received a great deal of attention in the last few years. However, a great many of the studies which have been conducted in this period have either been limited in scope, focussing on the acquisition of a restricted set of sound segments (e.g., Macken (1975)), or atheoretical in emphasis concentrating on detailed description of the 'facts' of phonological development (see, particularly, Ferguson and Farwell (1975), Ferguson (1976)). Without wishing to deny the interest and value of such studies it should be apparent that they do not lend themselves to the sort of analysis I am undertaking here to the same extent as do the global theoretical treatments such as Jakobson (1968) and Smith (1973). Largely for this reason this chapter has only two sections with 5.1 being devoted to a discussion of the status of Jakobson's theory of phonological development and 5.2 examining aspects of Smith's detailed study of his own son. It is worth pointing out that the general mode of theorising which each of these scholars employs can be seen as representative of a large number of other works. So, in the case of Jakobson, we can consider the studies of, e.g., Velton (1943), Shvachkin (1948), Garnica (1971, 1973), as falling in the same tradition and, although these authors were led to substantively different conclusions to Jakobson, I think that it is fair to say that, with regard to explaining and justifying their conclusions,

they were accepting a similar framework.²⁰⁵ Similarly for Smith, we can consider such works as Ingram (1971), Moskowitz (1970), Stampe (1969) and Berman (1977) as having similar emphases and adopting similar frameworks for the resolution of problems but Smith's discussion is by far the most detailed with which I am familiar. It is not clear that Smith as phonologist should be aligned with the natural phonology of Stampe but there do appear to be obvious correspondences between their views on phonological development and, in this context, Smith cites Stampe approvingly in a number of places.²⁰⁶

5.1 Jakobson and the laws of universal solidarity

Jakobson (1968) conceives of phonological development in terms of the acquisition of a succession of oppositions later viewed in terms of the acquisition of distinctive features (see Jakobson and Halle (1956)). The first such opposition to be acquired is that between a consonantal sound and a vocalic sound, this contrast providing the foundation for the syllable. It is important to realise
 205. Of course, as far as the last two studies mentioned here are concerned, they were concerned with the child's phonological perception rather than production and therefore identity of domain of investigation throughout this set of studies cannot be assumed. The relationship between production and perception has been the subject of some discussion recently but will not concern me in this chapter (see especially Kornfeld (1971), Edwards (1974)).

206. see p. 409a.

206. In a useful review article Ferguson and Garnica (1975) discuss behaviourist theories of phonological development owing most to Mowrer (1960) and theories emphasising the role of prosodic features in development most ably represented by Waterson (1970, 1971) as well as the views analysed in this chapter. I have chosen to neglect both of these positions on the grounds that there is no formulation of them which leads to interesting discussion within my framework. It is worth mentioning that in an introductory discussion of 'requirements for a theory of phonological development' Ferguson and Garnica say (p.155):

[a theory of phonological development] must account for the development of all the characteristics of an adult phonology as specified by a given phonological theory. It must account for known facts of phonological development that are not included in the characterization of adult phonology by existing theories. It must be consistent with a broader theory of language development and be relatable to theories of other aspects of child development. It should make principled predictions that can be empirically verified.

The relationship between some of these conditions and some of mine is obvious but, disappointingly, Ferguson and Garnica fail to systematically evaluate the theories they review.

that it is oppositions which are being acquired and not sounds as such. So, although Jakobson suggests that the first vocalic sound is usually a wide vowel and the first consonantal sound usually a bilabial, nothing follows against his position if these suggestions are not confirmed as it is merely the contrast which is at issue. With the first syntagmatic opposition established Jakobson takes up the development of paradigmatic oppositions within the consonantal system saying (p.48):

The first consonantal opposition is that of nasal¹ and oral stop (e.g., mama - papa), which is followed by the opposition of labials and dentals (e.g., papa - tata and mama - nana). These two oppositions form the minimal consonantal system of the languages of the world.

In fact Jakobson is more explicit than his own position demands here, in that there is no necessity that the first consonantal opposition should be an opposition of stop consonants - any contrast between a nasal and oral consonant will serve to carry the basic opposition.

From a developmental point of view the picture is abundantly clear. We have three stages which we can represent, concentrating exclusively on consonantal development and indulging in some transparent notation, as in Figure 72.²⁰⁷

207. As far as possible I have attempted to confine myself to the content of Jakobson (1968) in what follows and no systematic way of representing the various oppositions is put forward there. My use of feature notation should be seen merely as an attempt to encode the intuitive statements Jakobson provides and not as being derivative on any general theory. (cf. below for further discussion).

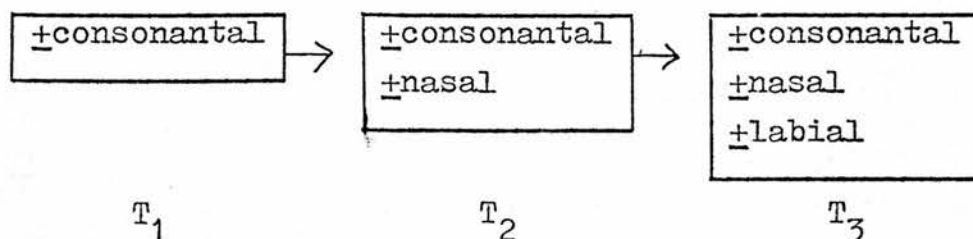


Figure 72

A similar course of development is suggested in the vocalic system with the first stages involving the acquisition of a width contrast followed by either a place contrast restricted to narrow vowels or a further width contrast representable as in Figure 73.

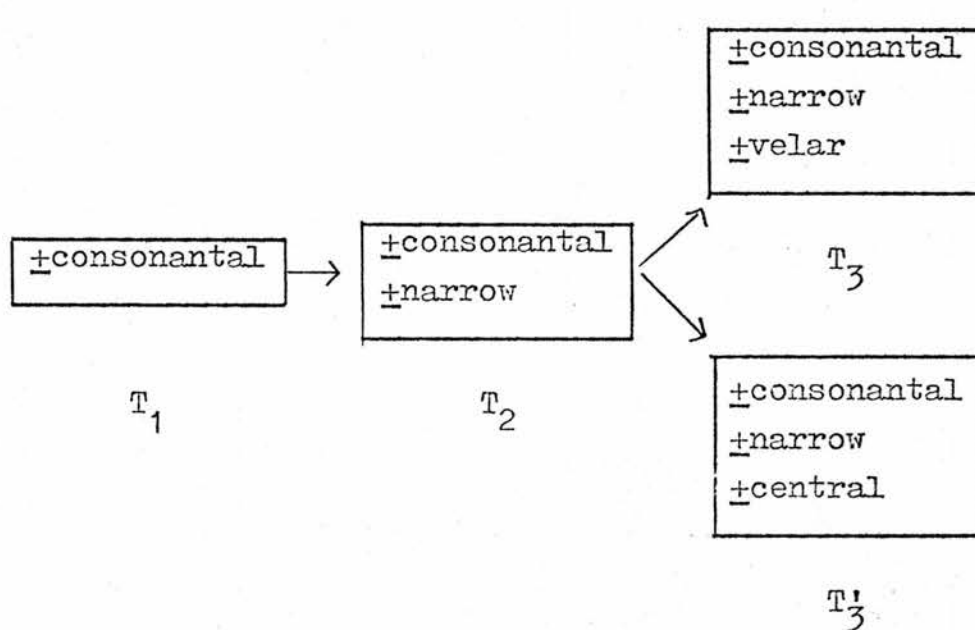


Figure 73

In connection with the content of Figure 73, Jakobson says (p.49):

Each of these two processes leads to a system of three vowels, which is the minimal vocalic system presented by the languages of the world.

and subsequent development in both the consonantal and vocalic systems is related to the Laws of Solidarity which are characteristic of synchronic descriptions of the phonological structures of the world's languages. Thus we find (p.51):

The acquisition of fricatives presupposes the acquisition of stops in child language; and in the linguistic systems of the world the former cannot exist unless the latter exist as well.

and (p.53):

The acquisition of back consonants presupposes in the linguistic development of the child the acquisition of front consonants, i.e., labials and dentals ... The existence of back consonants in the languages of the world presupposes accordingly the existence of front consonants.

Both of the above 'solidarities' are irreversible, i.e., the presence of stops in a language does not require the presence of fricatives nor does the presence of front consonants require the presence of back consonants. As a third example Jakobson suggests (pp. 55-6):

A so-called half-stop consonant (or affricate) which functions as an opposition to the corresponding stop consonant in phonemic systems, is acquired by the child only after the fricative of the same series ... Similarly, the opposition of a stop and an affricate in the languages of the world implies the presence of a fricative of the same series ... The number of such affricates in a phonemic system is therefore never greater, and is generally less, than the number of fricatives.

Similar claims are made concerning further developments in the vocalic system (pp. 56 - 7):

An opposition of two vowels of the same degree of aperture is not acquired by the child as long as a corresponding vocalic opposition of a narrower degree of aperture is lacking ... A differentiation of rounded vowels according to degree of aperture cannot arise in child language as long as the same opposition is lacking for the unrounded vowels ... Rounded palatal vowels ... arise in child language only after the corresponding primary vowels...

all of these facts being related to the distribution of vocalic oppositions in the world's languages.

According to this view then, we can conceive of phonological development in terms of successive stages where each stage recognises a set of oppositions. Concentrating on the consonantal system, we can represent this theory as an ordered sequence of sets of oppositions

$(S_1, S_2, S_3, \dots, S_{n-1}, S_n)$ where S_1, S_2 and S_3 are as in Figure 72 and various constraints are imposed on the rest of the sequence by the facts cited by Jakobson.

Thus assume that S_i contains the necessary oppositions to enable definition of the category 'Stop' and that S_j contains the necessary oppositions to enable definition of the category 'Fricative'.²⁰⁸ Then Jakobson is claiming that $i < j$. Similarly, assume that S_k contains the necessary oppositions for the category of 'Front consonant' to be

208. It is not clear from Jakobson (1968) exactly what oppositions are involved in such definitions.

defined and that S_1 contains the necessary oppositions for the category of 'Back consonant' to be defined. The claim is that $k < 1$.²⁰⁹

We are now in a position to begin to evaluate Jakobson's proposals against Conditions 1 - 4 of Chapter 1. For Condition 1 we are in a familiar position. Jakobson makes a number of remarks concerning the typical 'substitutions' found in child language before relevant oppositions are learned (see p.52, p.54 and other places) but does not propose a detailed theory of such substitutions. Without such a theory we cannot formulate conditionals of the form 'If the child intends to say X, then he says Y' where X denotes a word-form and Y denotes the child's version of this form. We should note further that even such conditionals, involving reference to

209. Jakobson, of course, extends his considerations to accommodate relatively late acquisitions saying (pp.58-9):

Oppositions which occur in the languages of the world comparatively rarely are among the latest phonological acquisitions of the child. Thus the geographical distribution of nasal vowels is relatively limited, and accordingly, these phonemes appear, in French and Polish children only after all the remaining vowels have been acquired ... The Czech ř, a sibilant opposition to r, is one of the rarest phonemes that occur in language, and hardly any other phoneme of their native language presents such major and persistent difficulties to Czech children.

So we also have constraints of the same general sort imposed on the mature end of the sequence.

intention in their antecedents, fall far short of predicting behaviour.

Condition 2 requires that each of the sets of oppositions be constructed in accordance with some general theory. This general theory will, presumably, consist of the specification of some finite set of phonological oppositions out of which each language (and each child) selects a subset and that there do exist theories of just this nature can only lend credence to this aspect of the proposal.²¹⁰ This leads me to the conclusion that Condition 2 is satisfied (cf. in this connection the situation here with that discussed in Chapter 2 where we were very much in the dark as far as general theories of semantic features were concerned.

Condition 3 is obviously satisfied in a very straightforward fashion. As the child develops he simply adds to his inventory of oppositions and so each stage of development will be additively more complex than the preceding one.

It is when we turn to Condition 4 that the most interesting

210. Here the delay in translating Kindersprache (Jakobson (1941)) takes on some significance and it is difficult to read Jakobson (1968) without taking ^{account} of Jakobson, Fant and Halle (1952) and Jakobson and Halle (1956). Thus, while the text under consideration does not contain a general theory of the required type, such a theory was not far from being developed.

aspects of Jakobson's proposals become apparent. Recall that what Condition 4 demands in a situation in which Condition 3 is satisfied is an answer to the following question : given that we have a sequence of theories (T_1, T_2) satisfying Conditions 1 - 3 why do we find the development schematised as $X \rightarrow X + Y$ rather than the development schematised as $Y \rightarrow X + Y$? In the case of the theory under consideration we can see X and Y as denoting sets of oppositions and we can test Condition 4 against a number of transitions.

Jakobson's views impose only a partial ordering on the full sequence of sets of contrasts and analysis of the whole sequence would be extremely complicated. I shall therefore restrict attention to the consonantal sub-sequence of Figure 74.

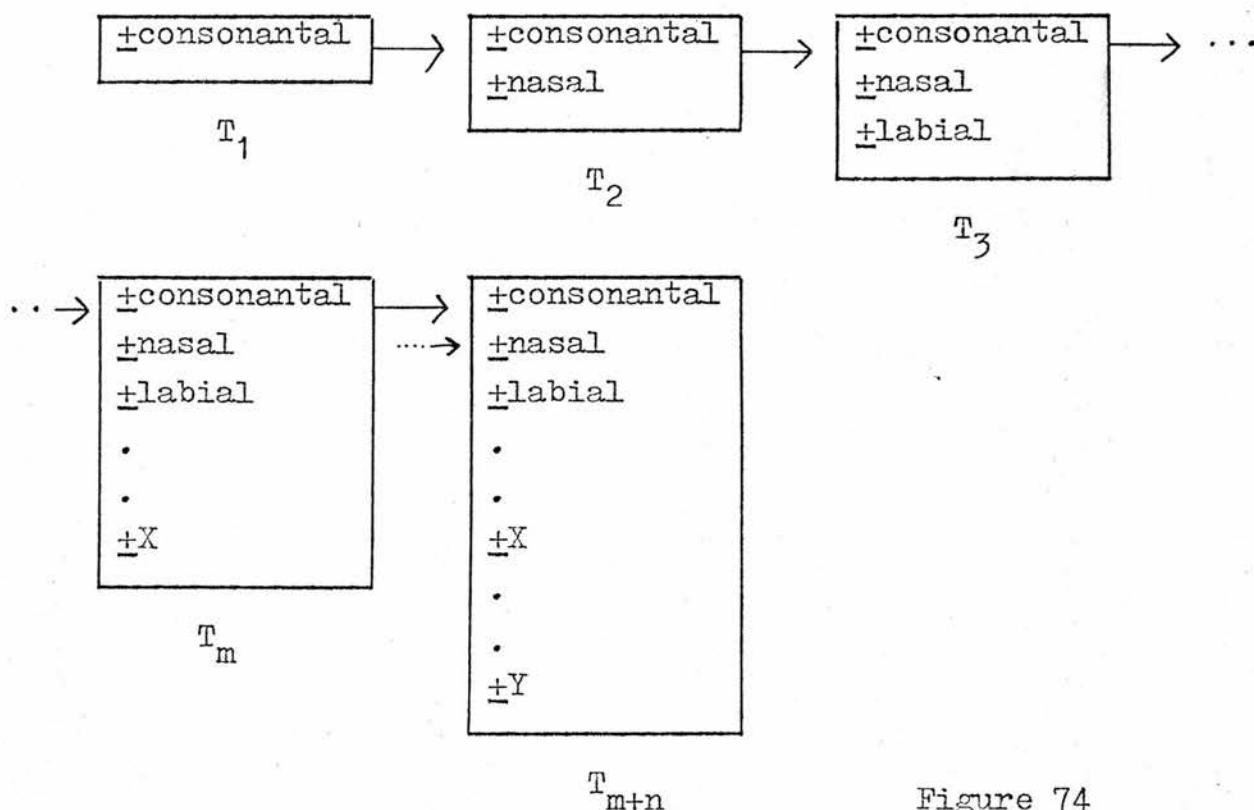
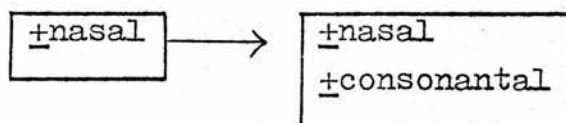


Figure 74

In Figure 74 T_1 , T_2 and T_3 are as in Figure 72, T_m recognises just those oppositions which allow the category 'Stop' to be defined and T_{m+n} does the same thing for the category 'Fricative'. Obviously further sequences could be considered for other aspects of consonantal and vocalic development described by Jakobson.

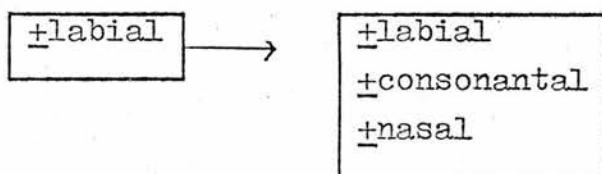
Consider then the transition from T_1 to T_2 . Condition 4 asks why we find (T_1 , T_2) rather than the sequence:



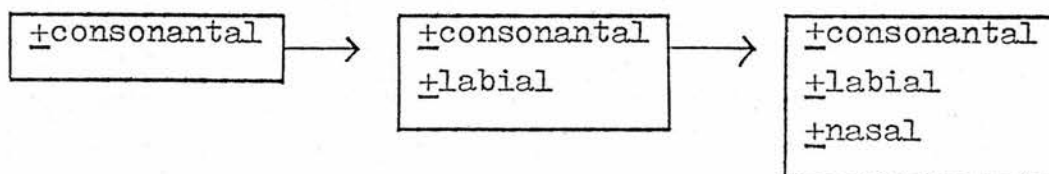
where such a sequence might be proposed if the child first produced a systematic distinction between nasal and non-nasal sounds and only later distinguished consonants from vowels. The problem is somewhat clouded here by the special role played by the syllable in Jakobson's theory and it could well be that he would wish to deny the possibility of a nasal/non-nasal opposition without prior recognition of the syllable in which the opposition could be realised. However, there are no immediately compelling reasons for going along with this and it seems to me that the fairest conclusion is that this aspect of the development is not logically grounded. Furthermore, at this stage in the argument, there is no reason to believe that we could relate the development to some sequence in a more basic theory, say a theory of perception or a theory of cognition (but cf. below) and so the second possibility of grounding the development doesn't

appear to be very promising. Given the earlier discussion we might expect to find an adequate grounding for the developmental facts in generalisations about the world's languages but, in this case, the expectation is confounded precisely because all the world's languages are asserted to utilise the two oppositions in question, +nasal and +consonantal. So it makes no sense to talk about the distributional facts assigning some sort of priority to +consonantal and it appears that, with regard to this transition, Condition 4 is not satisfied.

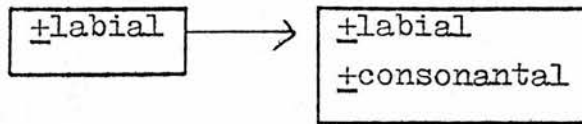
Almost identical considerations apply to the transition from T_2 to T_3 . Condition 4 requires that we consider the sequence:



and produce a reason for why we do not find it. We can suggest that +labial necessarily follows +consonantal because of what these theoretical terms are assumed to denote but no similar considerations can be brought to bear on the priority of +nasal over +labial. It would make perfect sense in terms of the theory so far discussed to discover the developmental sequence:

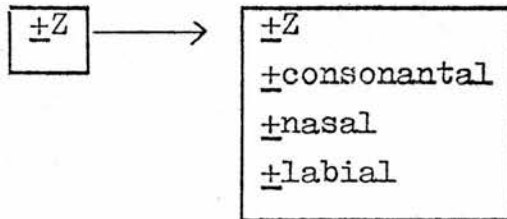


although the discovery of the sequence:



would be ruled out on logical theory-internal grounds. We have a partial logical grounding but again no suggestion of a more basic theory in which to ground the development and, because all the languages of the world are assumed to use all three contrasts, no possibility of grounding the development in distributional facts.

Imagine now a fourth member of the sequence, T_4 , where the next opposition appears; call it +Z. Do we have an explanation for why we find the sequence we do rather than the sequence:



We would expect to find at least a partial logical grounding for this hypothetical transition from T_3 to T_4 but, this time, we are also able to ground the sequence in distributional facts about the world's languages. At least this is the impression given by Jakobson when he talks about minimal consonantal systems (p.48):

These two oppositions (nasal v. oral and labial v. dental) form the minimal consonantal system of the languages of the world. These are the only oppositions that cannot be lacking anywhere, provided that there

is no mechanical deformity of the speech apparatus. Thus the claim, to be distinguished from the claim that there exists a language with just the minimal set of consonantal oppositions, is that, whatever the identity of Z, there will be some language which does not utilise Z in its phonological structure. This can be phrased as a unilateral implicational universal:

If L utilises phonological opposition $\pm Z$ then it also utilises the set of contrasts, \pm consonantal, \pm nasal and \pm labial.

The existence of a language using this set of contrasts but not the opposition $\pm Z$ is necessary if the universal is not to be strengthened to a bilateral one but, unfortunately, Jakobson neither tells us what Z is nor does he provide the sort of data necessary to substantiate the claims of universality and non-universality.

Similar reasoning can be applied to the transitions involving the introduction of $\pm X$ and $\pm Y$ in Figure 74 and, if we take it that reference to $\pm X$ is essential to the definition of 'Stop' and reference to $\pm Y$ essential to the definition of 'Fricative', then we have, as a consequence of the irreversible Law of Solidarity relating stops and fricatives, the unilateral implicational universal:

If L utilises the phonological opposition $\pm Y$ then it will also utilise the phonological opposition $\pm X$ although not necessarily vice versa.

Again the identity of X and Y is not explicit in Jakobson's system but if we concentrate on the defined categories

of 'Stop' and 'Fricative' some evidence is provided for the relevant Law of Solidarity (pp.52 - 3):

Hence there are no languages without stops, whereas P.Schmidt cites a number of Australian, Tasmanian, Melonesian, Polynesian, African and South American languages in which fricatives are completely unknown, In Kara-Kalpak and in Tamil, to cite additional examples from another continent, there is no autonomous category of fricatives; stops and fricatives appear as combinatory variants of the same phoneme - the first as basic variants, the others as variants conditioned by the environment. In Tamil, e.g., stops become fricatives after a vowel.

Thus, returning to our original sequence in Figure 74, we can see the extent to which it satisfies Condition 4 and it is easy to see how similar sequences could be discussed in the same way. The parallelism between this sort of explanation and that met in Chapter 2 in connection with Heider's work on the acquisition of colour vocabulary should also be apparent and, moreover, just as there we seemed to have the beginnings of a deeper explanation in terms of human visual physiology, so here Jakobson is not content to leave the explanation at the level we have now reached but feels the need to go on and produce a more fundamental explanation - an explanation which will tell us why the facts of language acquisition and the Laws of Solidarity are the way they are.²¹¹ He says:

211. I refer back to Chapter 1 for some discussion as to why I believe that even at this stage Jakobson has achieved a certain level of explanation.

These laws can be explained, however, only by considering and demonstrating their inner necessity. All attempts at atomistic interpretation, which necessarily explain only one aspect or a single phenomenon and are therefore never comprehensive, are clearly inadequate. Thus, the phonological laws of child language are not^{to} be mechanically separated from the corresponding evidence of the languages of the world and of aphasia, and the appearance of single sounds must not be treated in isolated fashion without regard for their place in the sound system.

Taking this unity of a number of fields as a basis for rejecting certain traditional notions on the ontogeny of early speech sounds utilising either the phenomenon of sucking or that of visual prominence, Jakobson goes on to search for a demonstration of 'inner necessity'. The principle he invokes is the principle of maximal contrast and this principle is applied to the successive oppositions mastered by the child. Taking first the opposition between consonant and vowel, Jakobson points out (p.69)

From the motor point of view, these two fundamental classes of speech sounds are contrasted with each other as closure and opening. The optimal opening is achieved in the wide a - vowel, while among the stop consonants it is the labial sounds which obstruct the entire oral cavity. One might postulate that just this most simple and maximal contrast is qualified to inaugurate the distinction between the vocalic and consonantal systems at the threshold of child language, and in fact this hypothesis is confirmed by experience.

There are two points being made here. The first is that the contrast between consonant and vowel is the optimal

application of the principle of maximal contrast and the second is that it follows from that principle that the 'core member' of the class of consonants will be a bilabial.²¹²

For the acquisition of the next contrast in the consonant system, ±nasal, we must consider the general configurations of the oral and nasal cavities. The distinction between an oral and nasal consonant can be represented as a distinction between a single obstructed cavity and a single obstructed cavity together with an open subsidiary cavity. Jakobson says (p.71): "this synthesis follows naturally the contrast consonant-vowel." What he seems to have in mind is that a contrast between an entity which we can represent as X and another entity which we can represent as X+Y where Y differs from X along significant dimensions is a particularly obvious one and will be picked up by the principle of maximal contrast. To

212. Two further points could be made here. The first is that reference to a general principle of maximal contrast cannot readily be accommodated to the models of explanation so far considered and this is because such principles find their proper place in the specifications of mechanisms for moving from one theory to another rather than in the substance of the theories themselves although, of course, such mechanisms might have implications of a substantial nature. Compare in this regard the discussions in 2.5 and 4.4 and Chapter 7. The second point concerns the obvious similarity between the notion of 'core member' and prototype in 2.4 - 2.5. In this connection, see the discussion of canonical phonetic forms in, e.g., Daniloff and Hammarberg (1973).

appreciate some of the force behind this argument we need to take account of Jakobson's discussion of the relatively late appearance of nasalised vowels. On this matter he says (p.71):

... a nasal vowel, which opposes a double open cavity to the simple open cavity of the oral vowel and thereby simply increases the vowel quality, is a much more complicated and much less opposing entity.

Here an entity which we can represent as X is opposed to an entity which we can represent as X+X' where X and X' do not differ along certain significant dimensions. The contrast between X and Y is seen as more apparent than the contrast between X and X' and, therefore, the opposition +nasal in the consonantal system is a better representative of the principle of maximum contrast than is the same opposition in the vocalic system. Although the intuitive force of the argument is clear enough it is difficult to avoid the feeling that there is a lack of precise specification of what is being contrasted in the operation of the principle.²¹³

213. To the same point it should be added that Jakobson makes the further observation that ejective and glottalized consonants involving double obstructions are learned relatively late just like nasal vowels, i.e., within the class of consonants we have a contrast between an entity we can represent as Y and another entity we can represent as Y+Y' where Y and Y' do not differ along certain significant dimensions. The contrast between Y and Y' is not as apparent as the contrast between X and Y leading to the prediction that it will be learned late. Where additional precision is necessary is clearly in the reference to 'significant dimensions'.

Some perspective on the above discussion is provided by Jakobson's discussion of the next consonantal opposition, that between labial and dental consonants. He introduces the idea of two psychophysical dimensions which are referred to as chromatism and light-dark and claims that among vowels, a is the most chromatic and least affected by the light-dark axis. Vowels generally have chromatism as their "specific phenomenal feature" whereas consonants, which are not susceptible to analysis in terms of chromatism, have light-dark as their primary axis of classification. Along this axis labials are dark when compared to dentals, and, according to the psychophysical principles Jakobson was following, the direction of darkness is the direction of increase along the light-dark axis just like the direction towards chromatism is the direction of increase along the chromatic axis. Therefore, just as a is the optimal vowel representing a maximum along one of the two fundamental dimensions, so the bilabial consonants are the optimal consonants representing a maximum along the other fundamental dimension.

There is much that is obscure in this account but the general direction seems clear enough. There are two primary acoustic dimensions along which speech sounds can be classified and the child's first achievement is to make the corresponding opposition. This is related to the view that (p.75):

The first opposition to appear - that of the two basic phonological classes - could therefore be

justified on the grounds that it is a more elementary problem to perceive the distinctions between one class of perceptions and another than to perceive those within one and the same class of perceptions.

A general principle of discrimination learning is invoked to explain the identity of the first phonological opposition. Then, given a classification in terms of the two primary dimensions, it is assumed that distinctions between entities which occupy the end points of the dimensions will be acquired before distinctions between entities which occupy medial points.²¹⁴ Thus, within the class of consonants, a distinction is made between labial consonants (maximal darkness) and dental consonants (minimal darkness) and within the class of vowels between wide vowels (maximum chromatism) and narrow vowels (minimum chromatism)²¹⁵

Further speculations concern the role of palatovelars in the consonantal system. Among consonants they possess minimum achromatism and therefore they (p.80) "Hold a

214. The relationship between this suggestion and the phenomenon of transfer along a continuum in discrimination learning is clear here, (see, e.g., Lawrence (1952)).

What Jakobson is achieving is some measure of explanatory adequacy within the universal core of phonological oppositions (cf. above) although, in order to explain the ontogenetic primacy of the oral-nasal opposition, further assumptions are necessary.

215. These distinctions are, of course, later discussed in terms of the features grave-acute and compact -diffuse (Jakobson, Fant and Halle (1952)).

place in the consonant system similar to that of the wide vowels within the vocalic system". Two facts follow from this : 1) that palatovelars do not represent an end point on any primary classificatory dimension and, therefore, by the principles discussed above, we might expect them to be acquired after labials and dentals; 2) having vocalic qualities they are less susceptible to analysis along the light-dark axis and, therefore, we might expect languages which have both a class of palatal consonants and a class of velar consonants to be relatively rare. Both of these expectations are confirmed.

It seems to me that one cannot but be impressed at the degree of concern shown by Jakobson with problems of explanation. While it cannot be claimed that he has given the principle of maximal contrast sufficient content to answer all our queries, it obviously has intuitive appeal and enables Jakobson, albeit tentatively, to relate phonological development to what he takes to be a fundamental axiom of human learning. Such sophistication becomes all the more impressive when we contrast it with the lack of concern we have already come across in numerous cases and when we also take account of the original date of Jakobson's work.

5.2 Smith and phonological rule systems

Smith (1973) is one of the first studies to systematically use the theoretical apparatus of generative phonology in

a developmental context. Although it is difficult to find any statement in Jakobson's work to this effect it would appear that, on his view, the relationship between the adult phonological system and the child's system could be represented by a set of simple substitution rules of the form $X \longrightarrow Y$ where X and Y represent speech sounds and where Y requires oppositions only available to the child whereas X may go beyond this set (cf. brief discussion of Condition 1 in 5.1). We can see such substitution rules merely as descriptive of the relationship between the two systems but, more interestingly, we can credit the child with some acquaintance with the rule, a move which allows the child access to the adult phonological form. Smith's favoured theoretical position assumes that the child does have access to the adult form and goes on to claim that the sorts of rules which must mediate between such forms and what the child actually produces are a good deal more complicated than simple substitutions. The assumption itself has been repeatedly questioned by e.g., Kornfeld (1971), Kornfeld and Goehl (1974), Waterson (1971) and it will not be my purpose to review this dispute. Suffice it to say that the evidence on both sides is far from compelling and hardly makes sufficient reference to perception phenomena of the type investigated by Shvachkin (1948), Garnica (1970, 1973) which itself is subject to a number of methodological considerations (see, e.g., Dodd (1975), Barton (1976)). Smith's own evidence in favour of adopting the assumption amounts to informal

observations of his child's comprehension and the fact that developments in the child's phonological system appear more intelligible if the assumption is made (see also Stampe (1969, p.447)).

Smith considers two views on the child's developing phonological system and, in fact, only the first of these assumes that the child always controls an adult phonological representation of a word. This view additionally has it that a set of realisation rules operates on this phonological representation converting it into a form identical (up to allophonic variation) to what the child produces. The realisation rules are cast in the framework of generative phonology as developed in Chomsky and Halle (1968). The second approach assumes that the child's knowledge of sound structure is more or less directly revealed by his own productions and treats his phonological system as an independent construct with no direct dependence on the adult system. The theory based on this approach consists of a traditional phoneme inventory and a set of morpheme-structure conditions following Stanley (1967). These two alternatives will be discussed in 5.2.1 and 5.2.2 respectively.

For theories of both types Smith considers a total of no fewer than 29 developmental stages which are defined in terms of there having been a significant change in the system from one stage to the next. Obviously this means that we are provided with two sequences of theories

each containing 29 theories and, in principle, we could investigate each of the transitions in the manner of earlier chapters. This would be a lengthy and often tedious process and so I shall confine myself to discussion of just the first two theories in each sequence and make some general remarks with respect to the remainder of the sequences.

5.2.1 A sequence which assumes that the child controls the adult form

We consider the sequence (T_1^1, T_2^1) where T_1^1 contains a phonological representation of each of the words in the child's vocabulary at Stage 1 plus the set of ordered realization rules as in Figure 75. (see p.431ff.)

These rules accept as input the surface phonological form of any word and as output specify the child's production of that word up to allophonic variation. They account for 97% of the child's vocabulary at Stage 1.

In T_2^1 there are some changes in the set of rules as well as some additions to the phonological forms which the child has available. Several of the rules survive in unmodified form. These are 1, 4, 6, 7, 8, 9, 11, 13, 15, 16, 19, 22, 25 and 26 from Figure 75. Of the remainder only one, 5, disappears in T_2^1 and of this Smith says (p.56):

[Rule 5], nasalising a continuant consonant after a nasal in the same syllable had apparently disappeared by Stage 2, but as it was of such marginal status originally it is hard to be sure if the forms occurring at stage 1 were not just remnants.

- (1) [+nasal] → ∅ / ___ [-voiced]
- (2) [+cons] → ∅ / [+nasal] [_____] [+voiced]
- (3) [+coronal] → [-coronal] / ___ ([+syllabic] [-stress]) [+lateral]
[-del rel] → [-anterior]
- (4) [+syllabic] → [+high] / [_____] [+lateral] ≠
[+back] / [-stress]
[-tense]
- (5) [+coronal] opt → [+nasal] / [+nasal] [+syllabic] ___
[+continuant]
- (6) [+lateral] → ∅ / ___ { ≠
[+consonantal] }
- (7) [+coronal] → ∅ / ___ [-syllabic]
[+anterior]
[+continuant]
[+strident]
[-voiced]
- (8) [+consonantal] →
[-coronal] / [+cons] [-cons] [+syll] ___
[+anterior] [+ant]
- (9) [+coronal] opt →
[+continuant] ∅ / ≠ ___ [+syllabic] [+anterior]
[+strident]
[+anterior]
- (10) [+coronal] opt →
[+continuant] ∅ / ≠ ___ [+syllabic] [-coronal]
[+strident]
[-anterior]
- (11) [+coronal] → ∅ / ___ ≠
[+continuant]
[+strident]
[+anterior]
[+voiced]

cont.

- (12) $[+nasal] \rightarrow$
 $\left[\begin{array}{l} +coronal \\ +anterior \end{array} \right] / X \left[\begin{array}{l} +syllabic \\ -stress \end{array} \right] \text{ --- Where } X \neq /g/$
- (13) $\left[\begin{array}{l} -syllabic \\ -consonantal \\ -sonorant \end{array} \right] \rightarrow \emptyset$
- (14) $[+syllabic] \rightarrow \emptyset / \neq (C) \left[\begin{array}{l} -stress \\ \text{---} \end{array} \right] C \left[\begin{array}{l} +syllabic \\ +stress \end{array} \right]$
- (15) $\left[\begin{array}{l} +coronal \\ +anterior \\ -continuant \end{array} \right] \xrightarrow{opt} \emptyset / \neq \text{ --- } \left[\begin{array}{l} +coronal \\ +continuant \end{array} \right]$
- (16) $[+sonorant] \rightarrow \emptyset / [+consonantal] \text{ ---}$
- (17) $\left[\begin{array}{l} +coronal \\ -nasal \end{array} \right] \rightarrow$
 $\left[\begin{array}{l} -coronal \\ -anterior \end{array} \right] / \left[\begin{array}{l} -coronal \\ -anterior \end{array} \right] \left[+syllabic \right] \text{ ---}$
- (18) $\left[\begin{array}{l} +coronal \\ +continuant \\ +sonorant \end{array} \right] \rightarrow$
- $$\left\{ \begin{array}{l} +lateral / \left(\left(\left[\begin{array}{l} +cor \\ +cont \\ +son \end{array} \right] \right) \left[+syll \right] \text{ --- } \left[+syll \right] \right) \\ \text{---} \left[+syll \right] \left[\begin{array}{l} +cor \\ +cont \\ +son \end{array} \right] \left[+syll \right] \\ \left(\left[\begin{array}{l} -cons \\ -cor \\ -ant \end{array} \right] \right) / \left[+syll \right] \text{ --- } \left[+syll \right] \\ \emptyset \\ \left[-son \right] \end{array} \right\}$$
- (19) $[+coronal] \rightarrow$
 $\left[\begin{array}{l} -coronal \\ -anterior \end{array} \right] / \text{ --- } \left[+syllabic \right] \left[\begin{array}{l} -coronal \\ -anterior \end{array} \right]$

cont.

- (20) $\begin{bmatrix} -\text{coronal} \\ +\text{anterior} \\ +\text{continuant} \\ -\text{lateral} \end{bmatrix} \rightarrow [+sonorant] / \text{---} [+syllabic]$
- (21) $[+coronal] \rightarrow \emptyset / [+consonantal] \text{---}$
- (22) $[+coronal] \xrightarrow{\text{opt}} \emptyset / \text{---} \neq$
- (23) $[+coronal] \rightarrow [+anterior]$
- (24) $[-sonorant] \rightarrow \begin{bmatrix} -\text{del rel} \\ -\text{strident} \\ -\text{continuant} \\ -\text{lateral} \end{bmatrix}$
- (25) $[+segment] \rightarrow [+voiced]$
- (26) $[-syllabic] \rightarrow [+consonantal]$

Figure 75. (adapted from Smith (1973), pp. 22 - 31)

So, putting rule 5 aside, we can assume that no rule completely disappears in the transition from T_1^1 to T_2^2 .

I now consider in turn each of the remaining rules and the changes they underwent. For rule 2 two things happened. It became restricted to apply only in final environments and it became optional. So in T_2^1 we have:

$$(2') \quad [+cons] \xrightarrow{opt} \emptyset / [+nasal] \left[\begin{array}{c} \text{---} \\ +voiced \end{array} \right] \neq$$

and Smith describes this change as rule 2 being "complicated to apply only in final position at stage 2" ((p.54) - my emphasis - RMA).

For rule 3 a change was necessitated entirely by the change in rule 2 and amounted to treating rule 3 as a transformational rule as in:

$$(3') \quad \left(\begin{array}{c} [+cor] \\ [+nas] \end{array} \right)_1 \left[\begin{array}{c} +cor \\ -del \text{ rel} \end{array} \right]_2 \longrightarrow$$

$$\left(\begin{array}{c} [-cor] \\ [-ant] \end{array} \right)_1 \left[\begin{array}{c} -cor \\ -ant \end{array} \right]_2 / \text{---} \left(\begin{array}{c} [+syll] \\ [-stress] \end{array} \right) [+lat]$$

and Smith says (p.55): "I accordingly complicate R3 to become the transformational rule $\lfloor 3' \rfloor$ " (my emphasis - RMA)

I have already mentioned the possible disappearance of rule 5 and the next rule to be affected in the transition is rule 10 which, in fact, is added to by a new rule deleting /j/ before a velar consonant preceded by a round vowel. This rule can be represented as in 10A:

$$(10A) \begin{bmatrix} +\text{cor} \\ -\text{cons} \end{bmatrix} \rightarrow \emptyset / \text{---} \begin{bmatrix} +\text{round} \\ +\text{syll} \end{bmatrix} \begin{bmatrix} -\text{cor} \\ -\text{ant} \end{bmatrix}$$

Although Smith makes no statement on this it is difficult to see the introduction of a new rule as anything other than a complication.

Consider now rule 12, which in T_1^1 has the function of making a nasal occurring in an unstressed syllable alveolar. In T_2^1 it only applies to velar nasals and no longer to /m/. Thus we require the modified rule:

$$(12') \begin{bmatrix} +\text{nasal} \\ -\text{anterior} \end{bmatrix} \rightarrow \begin{bmatrix} +\text{coronal} \\ +\text{anterior} \end{bmatrix} / X \begin{bmatrix} +\text{syllabic} \\ -\text{stress} \end{bmatrix} \text{---}$$

Where $X \neq /j/$

Referring back to the discussion of changes in rule 2 it would seem that, in order to be consistent, Smith must treat this modification of rule 12 as a complication as it involves a more precise specification of one of the terms although, again, he makes no comment in this respect.

Rule 14 deleting unstressed initial syllables has become optional in T_2^1 and at this point there is nothing to say about it.

Rule 17 "harmonising a post-vocalic non-nasal coronal consonant to a preceding velar was restricted at Stage 2 to apply only to continuants" (p.74). Thus the new version of rule 17 has the form:

$$(17') \begin{bmatrix} +\text{cor} \\ -\text{nas} \end{bmatrix} \rightarrow \begin{bmatrix} -\text{cor} \\ -\text{ant} \end{bmatrix} / \begin{bmatrix} -\text{cor} \\ -\text{ant} \end{bmatrix} \begin{bmatrix} +\text{syll} \end{bmatrix} \begin{bmatrix} \text{---} \\ +\text{cont} \end{bmatrix}$$

which again, by the same token as has been applied to

rule 2 and rule 12, will have to be regarded as a complication, as the last part of the conditioning environment is more precisely specified than in 17.

The third part of the horrendous rule 18 becomes optional in T_2^1 and, because of the fragmentary nature of the data leading to this conclusion, I shall say no more about this rule.

The discussion of rule 20 is interesting (pp. 85 - 6):

[Rule 20], converting labial continuants to $|w|$, underwent a putative simplification at Stage 2 which generalised the process to $/l/$ as well.

Thus, while we have:

slipper \rightarrow $bibə$ (*wibə)

at stage 1, we have:

sleep \rightarrow $wi:p$ (*bi:p)

at stage 2. In fact, the data are too limited at this early period for one to ^{be} confident that this reflects accurately what is going on. It seems probable that stage 1 slipper and rubber-band (\rightarrow [$b^h b \bar{e} b \bar{x} n$]) are remnants of a 'proto-stage' before the appearance of $|w|$ at all; and accordingly that slipper should be characterised as a restructured exception, and rule 20 have the form it has at stage 2 (20') ab initio:

20' $\left[\begin{array}{l} -\text{coronal} \\ +\text{anterior} \\ +\text{continuant} \end{array} \right] \rightarrow [+sonorant] / \text{---} [+syllabic]$

What appears to be a simplification involving the dropping of part of the specification of the input to the rule is argued against by the author himself.

Rule 21 which deletes post-consonantal alveolar consonants

must be modified to:

$$(21') \quad [+coronal] \longrightarrow \emptyset / \begin{bmatrix} +cons \\ -nasal \end{bmatrix} \text{ —}$$

involving a more precise specification of the conditioning environment. In addition the rule becomes optional after non-nasals. To the first of these modifications Smith, consistently with earlier remarks, says that rule 21 "must be complicated at Stage 2" (p.87).

Similar remarks apply to the development of rule 23 which at stage 2 must be complicated to have the form:

$$(23') \quad [+coronal] \langle \text{opt} \rangle [+anterior] \left[\begin{array}{c} \text{—} \\ \langle +son \\ +cons \rangle \end{array} \right]$$

and rule 24 actually splits into two rules at Stage 2 to take account of the appearance for the first time of $[\emptyset]$ and $[f]$:

$$(24i) \quad [-sonorant] \langle \text{opt} \rangle \begin{bmatrix} -del \text{ rel} \\ +cont \end{bmatrix} / \left[\begin{array}{c} \text{—} \\ \langle +cont \\ -cor \\ +ant \rangle \end{array} \right]$$

$$(24ii) \quad [+segment] \longrightarrow [-strident]$$

Smith says (p.89): "The development of Rule R24, making non-sonorants also non-strident, non-continuant, non-affricated and non-lateral was first complicated by the appearance of $[\emptyset]$ and $[f]$..."

Finally, and following from the already discussed development of rule 2, it is necessary to state restrictions on nasal-consonant clusters once they appear and this is

achieved by another new rule having the form:

$$(27) \quad [+cons] \longrightarrow [+coronal] / \begin{bmatrix} +anterior \\ +nasal \end{bmatrix} \text{ ---}$$

and again, the most natural alternative is to see the introduction of a completely new rule as involving a complication of the overall system.

Consider now the sequence (T_1^1, T_2^1) from the point of view of Conditions 1 - 4. Smith's approach with its explicit emphasis on processes of realisation fares better than Jakobson's against Condition 1. It does not, of itself, predict behaviour in the specified domain but, for any word form, the theory makes clear predictions as to the child's pronunciations of that word form at various stages in his development should he decide to use it.

Condition 2 appears to be satisfied in a fairly straightforward manner, Chomsky and Halle (1968) providing the framework which Smith follows conscientiously throughout.²¹⁶

One serious point which should be raised here is not concerned with the use of the formalism but with the domain of the theory and what appears to be a remarkable coincidence in the appropriate theories for distinct domains.

216: Smith appears to use the formalism in a rather cavalier and unconstrained fashion at times, resorting to optionality, conditions on rules, transformational rules, etc. as and when it suits him. I am aware of no detailed discussion of constraints on rules within the SPE framework to compare with such discussion in syntax.

In Chomsky and Halle (1968) we are presented with a model of the adult speaker-hearer's phonological competence which includes underlying phonological forms, surface phonological forms and rules, in the format employed by Smith, for relating forms at the two levels. Most of these rules are motivated by consideration of alternations in vocabulary which a child of the age of the child in Smith's study does not produce. For Smith, it is only his postulated underlying forms (= the adult surface forms) which comprise the child's phonological competence and the realisation rules are best interpreted as constraints on the child's performance. There is no underlying phonological level in the sense of Chomsky and Halle for Smith's child and, therefore, no need for rules, as part of the child's phonological competence, relating the two levels of phonological representation. That this is a fair summary of Smith's position is revealed by his statement that (p.133):

It is of fundamental importance to know whether the adult surface forms represent the competence of the child in any real sense, or whether the child's output mirrors his perception of the adult system, and the realisation rules are thus merely an artifact ... (my emphasis - RMA)

and (ibid):

... it seems clear that the first position is correct: namely the child's competence* is a close reflection of the adult form he hears and that his deviant output is the result of the operation of a set of psychologically valid realisation rules.

The footnote to this passage indicated by '*' reads:

At least in so far as the lexical representation of items is concerned. I am not claiming that the child shares all the adult phonological rules as well.

Indeed, Smith is not claiming this because he is not claiming that the child possesses any phonological rules in the sense of Chomsky and Halle at this stage in his development. So now we can clearly see a peculiar asymmetry between the model of the child and the model of the adult emerging from these considerations. We can represent it diagrammatically as in Figure 76.

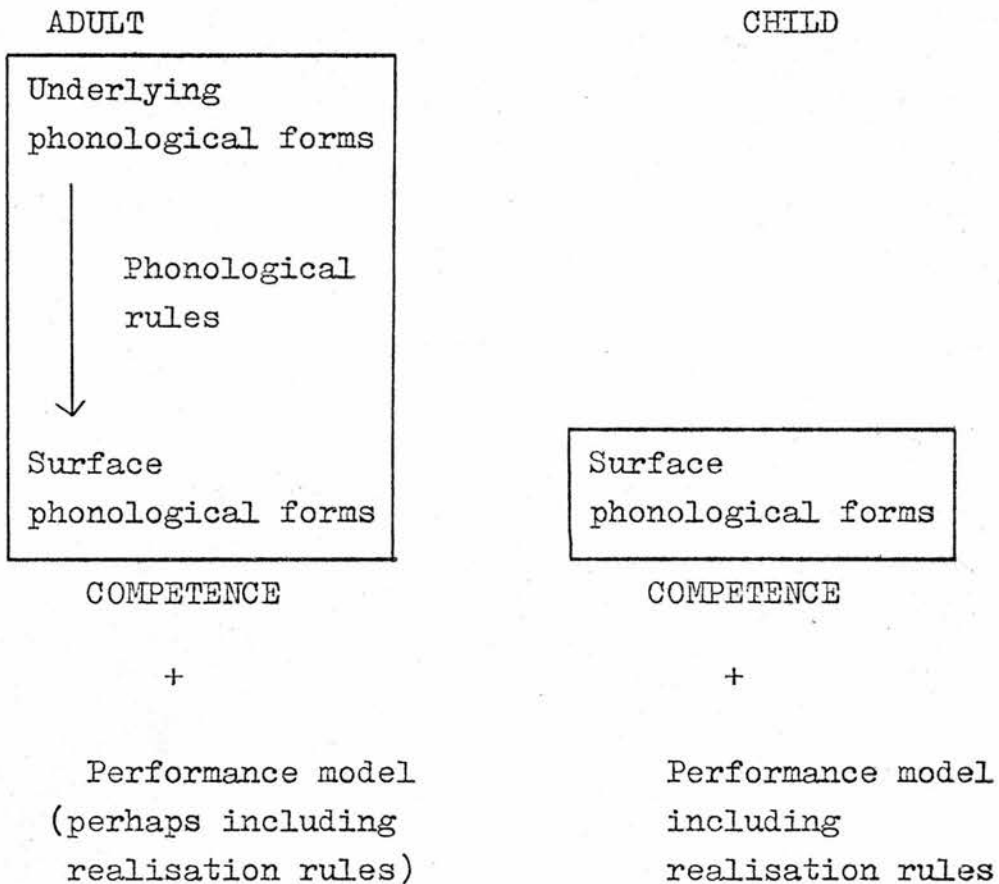


Figure 76

The problem that immediately arises concerns the identification of the formal theory of realisation rules with the formal theory of phonological rules. It is apparent that these two rule-types have quite distinct domains, the latter being part of a competence theory and the former part of a performance theory, and there is no a priori reason for believing that the same formal properties should characterise both of them. Of course, there is no reason to believe that they must be distinct either but it might not be thought remarkable if this turned out to be the case.²¹⁷

Moving on to Condition 3, as we have seen above, Smith appears to see the transition from T_1^1 to T_2^1 as one involving complication of the theory. There are at least three senses of 'complexity' which might be worth investigating here. The first simply involves reference to the number of phonological forms available to the child at a particular stage and would claim that this number is larger at Stage 2 than at Stage 1. It is not clear that such a

217. We would appear to have a somewhat analogous situation in syntax/semantics with regard to the interplay between perceptual strategies and 'grammatical' rules, the important difference being that there is no suggestion that perceptual strategies are theoretical constructs of the same type as are linguistic rules (for an attempt to identify the two, see Lakoff and Thompson (1975)). This preliminary conclusion holds despite the fact that perceptual strategies have so far not been stated with sufficient precision for their formal comparison to linguistic rules to be particularly meaningful.

notion is appropriately investigated within a theory of phonological development, and Smith provides no detailed statement on lexicon sizes but examination of the appendices to his book indicates that new items were recorded for the first time at Stage 2. In a sense this comparison is not very meaningful as, in principle, Smith's theory assumes that the child is capable of attempting to produce any form and makes a prediction as to what such an attempt will sound like. This qualification also applies to the second sense in which T_2^1 may be deemed more complex than T_1^1 which involves reference to the sets of features utilised in the theories. One might expect that certain features which the child begins to manipulate at T_2^1 are not part of his phonological competence at Stage 1 but this possibility is precluded by the above consideration which makes the full set of adult features available to the child and also by reference to the data Smith collected according to which there are no features absent at Stage 1 which begin to be used at Stage 2.

The third sense of complexity is by far the most interesting and concerns the complexity of the rule systems. Of the changes in the realisation rules between T_1^1 and T_2^1 we have seen that only one appears to lead to simplification and Smith himself argues that this is probably due to inadequate sampling. All of the others involve either the further specification of the domain of a rule, the introduction of new rules or a change whereby an

originally obligatory rule becomes optional. Although it is not the case that we can talk about all these in terms of a simple notion of additive complexity whereby new rules are introduced at T_2^1 leaving everything in T_1^1 intact, there nevertheless appears to be a complication of statement involved in every case and this appears to govern the author's use of 'complication'. Some support for the view that the transition from T_1^1 to T_2^1 so conceived is in line with Smith's preconceptions is to be found in the opening paragraphs of his book where he says (p.1):

What I expected to find was a constantly developing and interacting competence and performance unique to the child, moving steadily from a more idiosyncratic and simple system to one which was more complex and more closely isomorphic with the system of the adult language....

All of this appears to be perfectly consistent and could lead us to conclude that the transition from T_1^1 to T_2^1 satisfies Condition 3, albeit only in an intuitive fashion, but it is at this point that we must take a wider view and introduce consideration of later transitions in the sequence ($T_1^1, T_2^1, \dots, T_{29}^1$). At Stage 29 only a handful of realisation rules have survived at all. In fact, of the original set of 26 rules only rule 3 (in modified form), rule 12, rule 23 (in modified form) and rule 24 (in modified form) remain at the final stage. They have been joined by four new rules introduced at various points between Stage 1 and Stage 29 but each of these rules has perished by the final stage and so we can

see that it is only by the arbitrary decision to concentrate on the first transition in the sequence that we succeed in satisfying Condition 3. It looks as if virtually any other of the transitions will fail in this respect. This conclusion is anticipated by Smith despite the predisposition already referred to when he says (p.52):

These data were then analysed as changes in the sets of rules characterised above [the set of realisation rules at Stage 1 and the set of rules credited to the child at Stage 1 on the assumption that he has his own system]: the conditions and rules of his own system becoming, in general, more complex; the set of realisation rules becoming in general more simple.

and again in fn.2 to p.133:

P.Seuren (personal communication) has suggested the name of 'incompetence rules' for the realisation rules ... as they constitute a kind of filtering device for the child's competence, and have gradually to be unlearned as the child approximates more and more closely to the adult language (my emphasis - RMA).

This provides us with a new and not totally unexpected slant on how theories should meet Condition 3. This is going to depend entirely on how the theory is interpreted - as an ability or, in general, something positive, or as a constraint on an ability. So far I have assumed that Smith is attempting to explain the genesis of a positive ability but it now becomes apparent that he is characterising constraints on that ability, constraints which we might expect to become fewer and more simply

described as the child gets older.²¹⁸

We can now formulate a different version of Condition 3 to take account of theories which are to be interpreted in this negative fashion.

CONDITION 3'

Given a theory T in the domain of language development D where T is to be interpreted in terms of constraints on knowledge or ability in D and T amounts to a sequence of theories (T_1, T_2, \dots, T_n) , then T is an explanatory theory in D only if T_{i+1} is simpler than T_i ($1 \leq i \leq n-1$)

The various problems surrounding the notion of simplicity when used in this way as raised in Chapter 1 will, of course, appear again here.

In general the sequence $(T_1^1, T_2^1, \dots, T_{29}^1)$ will satisfy Condition 3' in an intuitive way. It will only do so in general because of the exceptions like the transition from T_1^1 to T_2^1 which, as we have seen, if anything satisfied Condition 3. It will only do so in an intuitive way because no simple notion of additive complexity is applicable to the sets of rules Smith is discussing except where

218. It is at this point that Smith makes contact with Stampe's views on natural processes. The latter assumes that such processes constitute part of the child's innate linguistic endowment and may be modified by suppression (that is, unlearned), restriction or ordering all of which have the result of reducing the domain of applicability of a process.

rules actually disappear.

What then of Condition 4? Obviously the version we have in Chapter 1 is not going to be applicable as that version was geared to Condition 3. Furthermore, because we don't have any clear notion of complexity it is going to be rather messy to apply in whatever version we formulate. Nevertheless, if only for the sake of completeness, it is worth formulating a condition to go with Condition 3' which is analogous to Condition 4. So:

CONDITION 4'

Given a theory $T (= (T_1, T_2, \dots, T_n))$ in the domain of language development D where T is to be interpreted in terms of constraints on knowledge or ability in D , then T is an explanatory theory in D only if Conditions 1, 2 and 3' are satisfied and, in addition, for all i , $1 \leq i \leq n-1$, the relationship of simplicity between T_i and T_{i+1} can be related to either

- (i) logical relationships in the form of the theories T_i and T_{i+1} . In this case we shall say that the theory is logically grounded. Or
- (ii) a theory of development in some other sphere of the organism's activity, T' , which is also interpreted in terms of constraints. In this case we shall say that the theory is grounded in T' . Or
- (iii) a theory of generalisations concerning the world's languages. In this case we shall say that the theory is linguistically grounded.

Examples of proposals to which these considerations apply are difficult to come by but one loosely formulated one concerns the relationship between sentence structure and constraints on the capacity of short-term memory. It is appropriate to construe the use of reduction transformations in this light (see the discussion in Chapter 3) where we can relate, albeit informally, an aspect of a sentence production model to general constraints on information processing which are relaxed as the child gets older. Bever's views on the development of perceptual and probabalistic strategies (Bever (1970), Maratsos (1974), Strohner and Nelson (1974)) can also be interpreted in this light although the picture is complicated by the claim that such strategies are not totally abandoned in adulthood. An example closer to the theme of this chapter would be Jakobson's remarks on the dissolution of language in patients who have suffered some brain trauma, where a simplification in the language system can be interpreted by reference to the Laws of Solidarity and ultimately to the psychophysical dimensions discussed in 5.1.

It is easy enough to imagine a phonological theory of constraints of the type Smith puts forward which would satisfy Condition 4'. So, for example, we could consider the function of constraints, as Smith subsequently does in his book, in terms of the achievement of cluster simplification, consonant harmony, systemic simplification,

etc. and we could attempt to discover the distribution of phonological structures which are marked with respect to these features throughout the world's languages, i.e., those which permit consonant clusters of different degrees of complexity, those which tolerate high levels of consonant disharmony, etc. A successful correlation between the distribution of the structures in the world's languages and the order of relaxation of the relevant constraints would amount to grounding Smith's proposals linguistically in much the same way as Jakobson's were grounded. Similarly, one might hope to be able to discover physiological properties of the developing organism which could be related to the order of relaxation of the constraints leading to the theory being grounded in a more basic theory of motor development (see Kent (1976) for recent work on the child's control of the temporal aspects of speech production). Of course, none of this has been done but, in principle, the way ahead is clear.

As for the theory under discussion, we are led to the conclusion that Smith's proposals, while satisfying some of our conditions, notably Condition 2 and, to some extent, Condition 3', fails to satisfy Condition 4' and, for this reason, fails to achieve explanatory adequacy.

5.2.2 A sequence which assumes that the child's phonology is idiosyncratic

The alternative considered by Smith comprises theories

which have two components: a phoneme inventory and a set of morpheme-structure conditions (as well as low-level phonetic rules to take account of allophonic variation which were also necessary in the first approach). The consonantal phoneme inventory from T_1^2 is as in Figure 77.

Matrix of A's consonant phonemes at Stage 1

	b	d	g	m	n	ŋ	w	l
consonantal	+	+	+	+	+	+	+	+
syllabic	-	-	-	-	-	-	-	-
coronal	-	+	-	-	+	-	-	+
anterior	+	+	-	+	+	-	+	+
nasal	-	-	-	+	+	+	-	-
continuant	-	-	-	-	-	-	+	+

Figure 77 (from Smith (1973, p.45))

In addition, T_1^2 contains an inventory of vocalic phonemes and a set of 10 unordered morpheme-structure conditions as in Figure 78. In connection with these, Smith says(p.47):

Conditions MS1 and MS2 are canonical or 'positive' conditions (see Stanley (1967)); MS3 - MS7 are sequential; and conditions MS8 - MS10 ... are segmental; i.e., they merely specify redundancies in the matrix.

For this approach I shall not consider any particular transition in the sequence ($T_1^2, T_2^2, \dots, T_{29}^2$) in detail

All morphemes must be of the form characterised by MS1 or MS2:

MS1 \neq ([-syllabic]) [+syllabic] ([+syllabic]) ([-syllabic]
[+syllabic]) * ([-syllabic]) \neq

or, equivalently, \neq (C)V(V)(CV)*(C) \neq

where C = $\begin{bmatrix} \text{-syllabic} \\ \text{+consonantal} \end{bmatrix}$ and V = $\begin{bmatrix} \text{+syllabic} \\ \text{-consonantal} \end{bmatrix}$

MS2 \neq $\left(\begin{bmatrix} \text{+consonantal} \\ \alpha \text{coronal} \\ \beta \text{continuant} \end{bmatrix} \right) V \begin{bmatrix} \text{+consonantal} \\ \gamma \text{coronal} \\ \delta \text{continuant} \end{bmatrix} V \dots \neq$

where either: $\alpha = \beta = \gamma = \delta$

or: $(\alpha \neq \beta)$ and $(\gamma \neq \delta)$

MS3 C \longrightarrow [-continuant] / $______ \neq$

MS4 C \longrightarrow [-coronal] / $______ V \begin{bmatrix} \text{-coronal} \\ \text{-anterior} \end{bmatrix}$

MS4a C \longrightarrow [-coronal] / $\begin{bmatrix} \text{-coronal} \\ \text{-anterior} \end{bmatrix} V \begin{bmatrix} \text{---} \\ \text{-nasal} \end{bmatrix}$

MS5 C \longrightarrow [-coronal] / $\begin{bmatrix} \text{---} \\ \text{+nasal} \end{bmatrix} V \text{[-coronal]}$

MS6 C \longrightarrow [+anterior] / $\begin{bmatrix} \text{---} \\ \text{+nasal} \end{bmatrix} V \text{[+anterior]}$

MS7 [+nasal] \longrightarrow $\begin{bmatrix} \text{+coronal} \\ \text{+anterior} \end{bmatrix} / X \begin{bmatrix} \text{+syllabic} \\ \text{-stress} \end{bmatrix} \text{---}$

Where X \neq / η /

MS8 [+coronal] \longrightarrow [+anterior]

MS9 [+nasal] \longrightarrow [-continuant]

MS10 [+continuant] \longrightarrow [+anterior]

Figure 78. From Smith (1973, pp.47 - 50)

but simply restrict myself to some general remarks. In the development from T_1^2 to T_{29}^2 there is scope for complications of a sort which were impossible under the first approach: an increase in the phonemes controlled by the child and a concomitant increase in the set of distinctive features. So we find, for example, that by Stage 8 the child has added to his phoneme inventory the phonemes /p, t, k, f, r/ and is using the distinctive feature \pm voiced. By Stage 20 he has further added the phonemes /dz, ts, m, n, l, h/ and is using the feature \pm strident. As far as the rules are concerned, there is also a tendency towards complication, although it is impossible to quantify this in any satisfactory way. By Stage 29 of the original 10 morpheme structure conditions only MS1 (in modified form), MS7 (in modified form), MS8 (in modified form), MS9 (in modified form) and MS10 (in modified form having actually disappeared at one stage) still survive. This is countered by the emergence, between Stage 1 and Stage 29, of 12 new morpheme structure conditions of which 9 are still operative at Stage 29. As we have already seen Smith regards the development, when viewed in this light, as moving from the simple to the complex but the notion of complexity we can invoke remains at an intuitive level.

As far as satisfaction of Conditions 1 - 4 is concerned, we can tentatively draw the following conclusions. Condition 1 raises more problems than it did for the first approach as viewing the child's development as

idiosyncratic does not lead to detailed predictions of his rendition of completely new adult forms. Condition 2 is satisfied by reference to a traditional phonemic theory coupled with a theory of distinctive features and a general account of morpheme structure conditions. It is with respect to Condition 3 that the two approaches most clearly diverge, with the first approach requiring a formulation of Condition 3' but this second approach apparently satisfying Condition 3 at an intuitive level. With regard to phoneme inventories and distinctive features the position is a little less opaque with simple additive notions being applicable; the more the sequence progresses the more phonemes we find and the more distinctive features are necessary to distinguish them. One unfortunate consequence of this is that the child appears, at some stages, to make phonemic distinctions not used by the adult (see Smith, pp.120 - 22 for details) and, therefore, he will have to 'unlearn' such distinctions at some later stage leading to an overall simplification in the phonemic inventory (this seems to be in the spirit of the proposals of Kornfeld (1971)) although such a stage does not appear in the development charted by Smith. Clearly, further argument would be necessary to make such a position attractive. Such a problem does not arise for the set of distinctive features and, for this set, we could look to the satisfaction of Condition 4. Unfortunately the question is not raised by Smith and there is little point

in building further on the speculative nature of much of this discussion.

Summarising the main points of this chapter, it seems to me important to emphasise the contributions of Jakobson and the extent to which he showed a sophistication with regard to questions of explanatoriness which is quite uncharacteristic of workers in language development. Smith's work is stimulating and has led to some clarification concerning the positive and negative interpretations of theories and the corresponding differences in conditions which they might be expected to meet. At the same time, its meticulous detail means that it is an extremely long job to analyse it in depth, and, most importantly, no attention is given to the satisfaction of a modified version of Condition 4.²¹⁹ I might add that Smith himself sees virtues in the approach of 5.2.1 to capture

219. In general, in developmental phonology we might expect the 'more basic' theories of Condition 4 to be theories of motor development or perceptual development rather than conceptual development. One recent study suggesting that this need not always be the case is Schwartz and Folger (1977) where it is argued that the reduction in variability of forms noted after the child passes the '50-word stage' can be attributed to his abilities to represent stable rule systems, a representation which is only possible at the end of the sensori-motor period. This is an interesting suggestion but, as the authors point out, there are alternative explanations which must be seen as equally plausible at the moment.

generalisations in development which are highly significant and which I have not touched on here at all. These virtues are much more closely related to the linguist's notion of explanation and its relationship to 'significant generalisations' (see Hurford, (1977) for extensive analysis) and, it seems to me, can be considered independently of the conditions discussed here.²²⁰

220. I would not like to be interpreted as insisting that such considerations are necessarily raised in an independent framework but they raise so many problems within linguistics itself that it would have complicated the current work enormously to attempt to accommodate them.

CHAPTER 6 : THE ACQUISITION OF SPEECH ACTS AND CONVERSATIONSKILLS

In this chapter I shall deal with a somewhat disparate set of topics which have in common that they focus not on formal and structural aspects of language development but rather aspects which are rooted in the fact that language has, as one of its primary functions, the property of being used to communicate in a shared context. We shall also meet the additional point that the formal and structural aspects of language can be understood by paying attention to its communicative and interactive properties.

As problems in the domain of formal syntax have become more formidable (cf. Chapter 3 for some discussion) this 'functional' area has received an increasing amount of attention in the last few years, yet no common theoretical framework has emerged and this contributes to the uneven subject matter of the chapter. In particular, I feel that everything remains to be demonstrated as far as finding a basis for syntax in communication is concerned and that most proponents of this view have grossly underestimated the complexity of the structures which need to be provided with a communicative base,²²¹ taking the fact that the functions which language serves appear to be continuous from a non-structured 'holophrastic' period

221. Cf. similar remarks in connection with a cognitive basis for structures in 4.3. See Ervin-Tripp (1977) for a recent review of conversation and syntax with very tentative conclusions.

to a structured syntactic period as demonstrative of the correctness of the position. Clearly it is not. It is perfectly consistent to recognise continuity in the development of a functionally defined system without insisting that the functions play any explanatory role for the genesis of structures which eventually come to serve them; consistent but not necessarily correct and it seems to me that stronger arguments than have been put forward so far will be necessary before this issue can be resolved.

The chapter falls into six sections each one discussing work which is roughly within the area just delimited and submitting it to the sort of analysis with which we are by now familiar.²²² 6.1 is devoted to the work of Dore (1974, 1975) manipulating the concept of 'primitive speech act' and subscribing to the view that the study of such acts is a necessary precursor to an understanding of the genesis of structure. 6.2 considers the less far-ranging work of Gruber (1975) which, nevertheless, has some interesting properties and which can be related to some of my own work (Atkinson (1974, forthcoming)) which is examined in 6.3. To some extent extending my work and developing an independent framework is the work of Keenan and her associates (see, particularly, Keenan and Schieffelin (1976)) and a discussion of this is the subject matter of 6.4. Bruner is the scholar who has

222. One very notable omission is Bates (1976) to which I did not have access in time to include here.

attempted to identify the basis of a theory of speech-acts and language structure in pre-linguistic routines of interaction and play and this attempt will be scrutinised in detail in 6.5. The final section, 6.6, will be devoted to a discussion of the very influential work of Halliday (1973, 1975) which, taking a functional stance on meanings, attempts to provide an explanation for the development of structure which is grounded in facts which are outside the individual psychology of the child; facts which reside in the socio-cultural framework in which the language being learned by the child is embedded.

6.1 Dore and primitive speech acts.

Dore (1974, 1975) argues for the value of the notion of 'primitive speech act' in understanding aspects of early language development. Beginning from the idea of 'speech act' emerging from the work of Searle (1969, 1975a, 1975b) but having its origins in the insights of Austin (1962), Dore makes the reasonable enough claim that a child eventually acquires a repertoire of speech acts and that, therefore, "it is appropriate to ask how he acquires this repertoire" (1974, p.344). Our expectations following a remark like this are that we shall be presented with a developmental theory of speech acts and to some extent this expectation is fulfilled. In his 1974 paper, while not presenting his conclusions in a developmental context, it is possible to interpret some of Dore's findings as embodying a developmental hypothesis and in his 1975

paper he does talk at length about the relationship between early aspects of speech act development and subsequent sentential structures. I shall take these aspects of his work in turn in 6.1.1 and 6.1.2 below.

6.1.1 The first speech acts

The adult notion of 'speech act' is not the appropriate tool for studying early child language. The reason advanced for this is that the typical speech act studied by Searle has, as subcomponents, acts of referring and predicating and these involve at least two linguistic forms. The child at the beginning of language learning, while a producer of speech acts, does not produce utterances more than one word in length and, therefore, we are never in a position to identify acts of referring and predicating in the same child utterance.²²³ To solve this problem Dore coins the notion of primitive speech act which he introduces in the following terms (1974, p345):

A primitive speech act ... is defined as an utterance, consisting formally of a single word or a single prosodic pattern, which [here the text has 'with' - RMA] functions to convey the child's intention before he acquires sentences. The single word is either a rudimentary referring expression such as the names of people, objects or events, or a specifically expressive work like "hi", "by-bye",

223. It is not clear that anything in Searle's framework precludes the possibility of illocutionary acts without associated propositional acts (see 6.3 below for relevant discussion).

or "nighty-night". The utterance of a prosodic pattern counts as a PSA if (1) it contains a consistent prosodic feature produced without the segmental phonemes of a word, and (2) it communicates the child's intention. Prosodic patterns, with or without lexical content, convey the primitive force of the PSA.

Dore then proposes four observational criteria which are to be taken account of in deciding the identity of PSA's. These are: (1) the child's utterances; (2) his nonlinguistic behaviour, e.g., gestures and facial expressions; (3) the adult's response, both verbal and non-verbal; and (4) the relevant, salient aspects of the context of utterance, such as objects attended to, location of objects and people. Applying these criteria to the data he had from two children yielded, for Dore, eight PSA's which he relates to the four observational criteria as in Figure 78 overleaf.

The first thing to note in connection with this tabulation is that Dore is not applying his own definition of a PSA consistently. On the list in Figure 78 there are three entries which seem to be quite neutral as far as the child's intentions are concerned (cf. the role of intention in Searle's theorising adapted largely from Grice (1957) and Strawson (1964)). Thus, for the PSA of Labelling the child apparently has no audience-directed intention and so, as far as Searle's theory of speech acts and Dore's definition of PSA are concerned, Labelling would not count as a speech act, primitive or otherwise. Griffiths

Primitive speech act	Child's utterance	Child's nonlinguistic behaviour	Adult's response	Relevant contextual features
Labelling	Word	Attends to object or event; does not address adult; does not await response	Most often none; occasional repetition of child's utterance	Salient feature focussed on by child; no change in situation
Repeating	Word or prosodic pattern	Attends to adult utterance before his utterance; may not address adult; does not await response	Most often none; occasional repetition of child's utterance	Utterance focussed on; no change in situation
Answering	Word	Attends to adult utterances before his utterance; addresses adult	Awaits child response; after child utterance most often acknowledges response; may then perform action	Utterance focussed on; no change in situation unless child's response prompts adult reaction
Requesting (action)	Word or marked prosodic pattern	Attends to object or event; addresses adult; awaits response; most often performs signalling gesture	Performs action	Salient feature focussed on by child and adult; change in condition of object or child

cont.

Primitive speech act	Child's utterance	Child's nonlinguistic behaviour	Adult's response	Relevant contextual features
Requesting (answer)	Word	Addresses adult; awaits response; may make gesture regarding object	Utters a response	No change in situation
Calling	Word (with marked prosodic contour)	Addresses adult by uttering adult's name loudly; awaits response	Responds by attending to child or answering child	Before child's utterance adult is some distance away; adult's orientation typically changes
Greeting	Word	Attends to adult or object	Returns a greeting utterance	Speech event is initiated or terminated
Protesting	Word or marked prosodic contour	Attends to adult; addresses adult; resists or denies adult's action	Adult initiates speech event by performing an action the child does not like	Adult's action is completed or child prevents action
Practicing	Word or prosodic pattern	Attends to object or event; does not address adult; nor await response	No response	No apparent aspect of context is relevant to utterance

Figure 79. From Dore (1974, p 346).

(forthcoming) makes a similar point when he notes that Dore fails to distinguish between communicative acts and informative acts where only the former presupposes an audience-directed intention on the part of the performer of the act. Thus, the constituency of the set of PSA's is not on the firmest foundation and Dore admits another possible source of indeterminacy when he says (p.347):

The set is not meant to be exhaustive - a study of other children might well yield PSA types which our children did not perform. Also, in a finer analysis, one might wish to distinguish between, say, different kinds of labelling (for example, labelling an action vs. labelling an object) in which case a different set of PSA's would emerge.

To construe Dore as endorsing a developmental hypothesis we have to indulge in a certain amount of speculation.

We fix D as the domain of speech acts the child is capable of performing and from what we have seen so far we can safely conclude that Dore sees theories in D as inventories of PSA's. Figure 79 is based on the development of two children and we refer to the set of speech acts in this inventory as S_i .²²⁴ Now it surely makes sense to assume that a developmental sequence exists and that S_i is just one among several inventories of speech acts which,

224. The two children in Dore's study were not quite identical with regard to their speech act inventories; the child M did not produce any instances of Calling and the child J did not provide any instances of Protesting except by using non-conventional forms. Dore does not suggest that any developmental sequence is involved here.

when considered together, constitute a sequence, $(S_1, S_2, \dots, S_i, \dots, S_n)$. Clearly this much is true to the extent that the child eventually becomes an adult and then has available the full set of adult speech acts which, incidentally, will include most, if not all, of the PSA's from S_i (note such explicit performatives as I request that, I repeat that, etc.). This indicates that we have at least a sequence of the form (S_i, S_A) where S_A refers to the full inventory of adult speech acts. How does such a sequence match up to conditions 1-4?

The detailed prediction of behaviour hardly seems to be an issue here as the sets of speech acts are closely tied to the child's intentions rather than to the form of his utterances and so, unless we have a paradigm of psychological explanation which embraces intentions, there seems little else to say. Of course, this is not a new conclusion but it is particularly apparent in this case.

Condition 2 requires that the inventories of speech acts in the sequence be constructed in accordance with some general theoretical considerations. The only sensible interpretation for this is that there should be a finite set of possible speech acts from which, at each stage in his development, the child is using a subset. The difficulties is specifying such a set were first made clear by Austin who claimed to have found several hundred verbs which had a performative use and each of which could be seen as entailing the existence of a distinct speech

act. As different languages and different cultures are considered such a list would need to be extended and it seems unlikely that a once and for all exhaustive list could be produced. It might, nevertheless, be possible to develop a theory of speech act types suggesting general properties each set of which will characterise large classes of speech-acts and which will be independent of cultural differences. Searle (1975a, 1975b) can be seen as making some preliminary suggestions along these lines but nothing similar is considered by Dore. We have seen that he considers the set of PSA's abstracted from his data as somewhat indeterminate and certainly not exhaustive. A tighter coding procedure with regard to the four observational criteria or, possibly, the employment of different criteria could lead to different inventories and no guide-lines are provided for choosing between such alternatives.

Condition 3 can only be considered in a hypothetical way since we are not provided with a developmental sequence but, as already noted, the existence of an adult stage can guarantee us a minimal developmental theory. It seems fair to say, with regard to the sequence (S_i , S_A) that, with the exception of Practicing, which anyway does not properly count as a speech-act, all the child PSA's listed by Dore survive, as speech acts in the adult model. In addition a large number of speech acts not included in S_i appear in S_A , e.g. promising, threatening,

advising, and so it seems safe to conclude that there is a simple additive relationship between S_i and S_A . For the sake of argument let us assume that Condition 3 is satisfied in this straightforward sense for the whole sequence $(S_1, S_2, \dots, S_n=S_A)$. Then Condition 4 would require an answer to the question: given two inventories, S_j and S_{j+1} , such that $S_{j+1} = S_j + X$, why do we find the sequence schematised as $S_j \longrightarrow S_j + X$ rather than the sequence schematised as $X \longrightarrow S_j + X$?

The hopes of providing a logical grounding for the sequence are remote, as Dore's PSA's are unanalysed units with no logical relationships holding between them (cf. in this connection the proposals of Gruber, Atkinson and Keenan analysed in detail below). Reference to languages of the world would appear to be a promising route to investigate. Although, as pointed out above, there is almost certainly a good deal of variation in the languages of the world as far as their total inventories of speech acts are concerned, it seems equally likely that there is a functional core to the world's languages consisting of a set of speech acts which can be performed in any language (cf. Jakobson's minimal consonantal and vocalic systems described in 5.1). In particular, it is difficult to imagine a language which does not enable its speakers to assert, to question and to command whereas it does not stretch the imagination to consider a language in which it is impossible to sentence, to find (guilty or not

guilty), etc.²²⁵ An immediate prediction from such a state of affairs would be that the functional core will be learned first and we might even hope to discover principles akin to the Laws of Solidarity operating outside the functional core. Again it would be possible to interpret Searle's recent work as taking the first steps along this direction where he is not only producing definitions of categories of speech acts but also giving some indication of what the paradigmatic member of each category would be. Unfortunately, from Dore's point of view, all of this is speculation. Even if it could be realised in a satisfactory way we would still be left with the problem - the same one as Jakobson had - of predicting order of development within the functional core, but the nearest Dore comes to even raising such possibilities is when he reproduces some claims of Slobin (1971) that "Everywhere language consists of utterances performing a universal set of communicative functions (such as asserting, denying, requesting, ordering and so forth) ..." (p.302) but it should be clear by now that

225. Bennett (1976) makes exactly the same point in a critique of Chomsky's view that the 'primary' function of language is not communication. It seems necessary to distinguish between a particular linguistic act and a language in this dispute and, while Chomsky has argued that communicative intention is not a necessary concomitant of every linguistic act, this does not show that such a notion is not central in the definition of a language.

much more than this is needed if we are to follow this road to explanatory adequacy. Nothing in Dore's work suggests that the third possibility, that of grounding the theory of speech acts in a more basic theory, is a realistic one. Indeed, there is some indication that, at least in one regard, he would consider such a reduction impossible. To appreciate his argument, we can consider the view that a theory of speech acts could be reduced to a theory of non-linguistic communication with the various conditions of Chapter 4 being satisfied, but against this Dore makes the point that (1975, p.37):

Certainly, some forms of communicative intentions exist before language emerges, but linguistically expressed intentions are not isomorphic with pre-linguistic intentions and the former need not be derived from the latter. (It is difficult to imagine, for example, what would count as a pre-linguistic 'asserting' of a proposition.)

Thus, even if we had an explicit developmental theory of non-linguistic communication to put alongside an explicit theory of the development of speech acts, there would be at least one substantive term in the latter, namely 'assertion', which could not be translated into a substantive term of the former.²²⁶ One of the necessary

226. Dore's difficulties of imagination are not necessarily insurmountable. It is certainly not incoherent to consider an act of pointing as corresponding to assertion. Cf. also Grice's (1957) discussion of the differences between showing photographs and drawing pictures where he seems to want to say that the latter can be functionally equivalent to asserting. It is patently non-linguistic and it is easy to imagine it as pre-linguistic even if it is, empirically, extremely unlikely.

conditions on reductions described in Chapter 4 would not be satisfied.

It seems, then, that there is no plausible route for Dore's proposals to approach Condition 4. This predicament is made even worse when we recall that we are only discussing Condition 4 on the counterfactual assumptions that Conditions 2 and 3 are satisfied. In short, Dore's suggestions can only realistically be seen as a preliminary coding of data into a technical vocabulary. They cannot be seen as constituting any sort of explanation.

6.1.2 Primitive speech acts and syntax

In Dore (1975) the author puts forward some ideas concerning the relationship between speech acts and syntax which can be interpreted in terms of my framework. He is at pains to argue that his approach through PSA's yields a more valuable treatment of the 'holophrastic stage' in child speech and he contrasts his own view where "the PSA is formalized" (p.34) as in Figure 80:

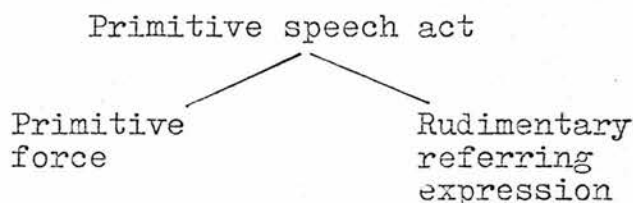


Figure 80 From Dore (1975, p.34). Note that in this paper the PSA does not admit intonational contours alone.

with that of McNeill whose "formalization of the holophrase" (p.23) is as in Figure 81.

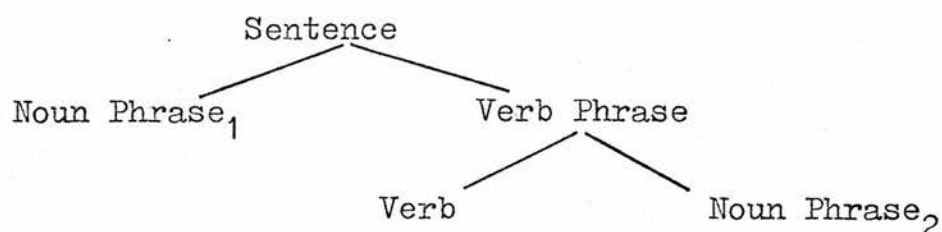


Figure 81 From Dore (1975, p.23)

"where only one constituent (but not NP₁) is expressed in any given one-word utterance" (ibid), that of Ingram (1971) whose "version of a case grammar approach is formalized as" (p.25) in Figure 82:

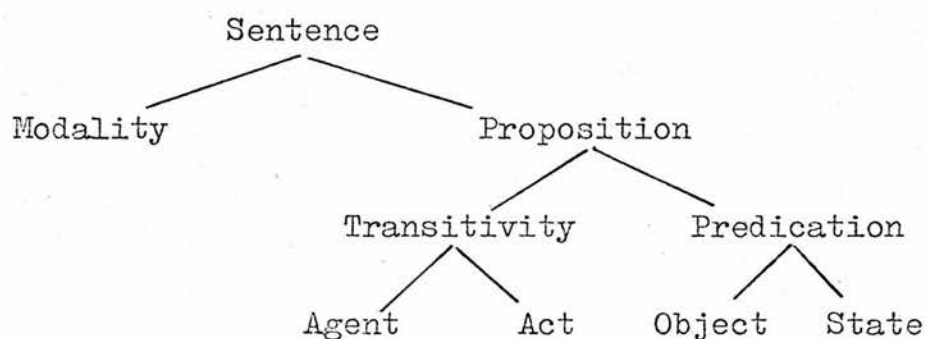


Figure 82 From Dore (1975, p.25).

and that of Greenfield, Smith and Laufer²²⁷ whose "version of a case grammar analysis would presumably be formalized as" (p.25) in Figure 83:

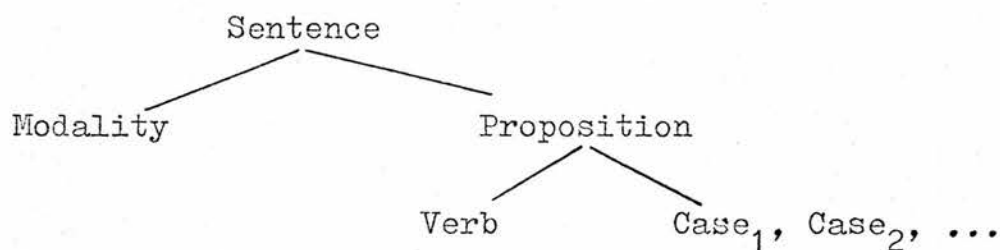


Figure 83 From Dore (1975, p.26).

227. The published version is Greenfield and Smith (1976), already discussed in Chapter 4.

It is not my purpose here to evaluate Dore's criticisms of these alternative accounts. Suffice it to say that I largely agree that taking the sentence, a structured entity, as the pivotal notion in treatments of early child language has led to a great deal of obfuscation and that the arguments advanced against the positions documented in Figures 81 - 83 are generally telling ones. It is Dore's own analysis which is of interest to me. He says (p.34):

The child's transition to structural meaning is of course the crucial issue of his acquiring syntax. The central question for speech act development is: how do the PSA constituents become grammaticalised?

Note that here the question is quite distinct from that discussed in 6.1.1. The domain of investigation is now something like (it is difficult to be precise) the interaction of a theory of linguistic communication and formal principles of organisation of linguistic structures. It is not appropriate to regard the "formalization" of Figure 81 as arising from a phrase structure grammar as linear order is not at issue and a most neutral interpretation is to view the downward direction in the tree as representing some notion like "is realised as". Dore next notes that Figure 81 (p.34):

... is clearly inadequate as a representation of the child's knowledge after the one-word stage...
(my emphasis - RMA)

and proposes the schema of Figure 84 as a representation

of the sequence of development into the next stage:

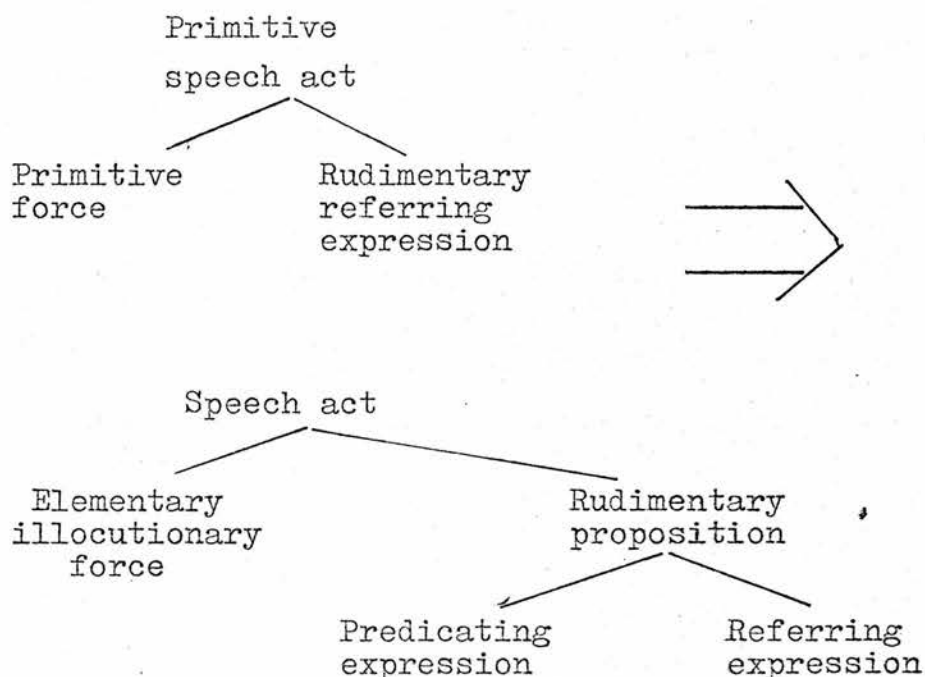


Figure 84 From Dore (1975, p.35)

and on the interpretation of the right hand side of this figure Dore says (ibid):

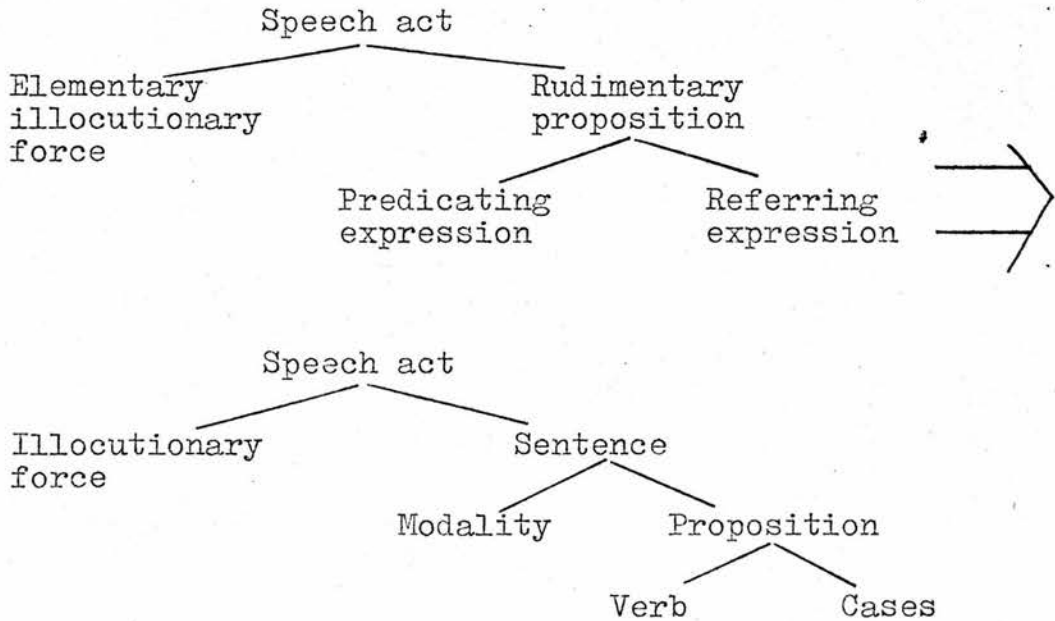
In this figure a predicating expression is introduced and it combines with a referring expression to form a rudimentary proposition. The force component begins to be expressed by elementary kinds of illocutionary force indicators.

It is unclear whether, at this stage, the child is supposed to control word-order as a grammatical device²²⁸, and

228. At this stage no category 'sentence' has been introduced and Dore says (p.35): "The notion of sentence is introduced here to account for the child's increasing control over word-order, grammatical morphemes, paraphrase, detection of anomaly, and so on, in multiple word utterances" (my emphasis - RMA).

so it is probably legitimate to interpret the right hand side in the same way as the left hand side with the leaves of the tree not being linearly ordered. However (pp.34 - 5):

... since children soon begin to combine words in a relatively non-random manner, [Figure 84] must soon develop into something like



Here the child begins to form more complete propositions which, furthermore, begin to become marked syntactically and semantically according to the conventions of the language he is learning (my emphasis - RMA).

The right hand side of the figure in this passage must, apparently, be interpreted in hybrid fashion with a notion like 'realization' obtaining between the Speech act and its 'components' and a more traditional interpretation corresponding to what we have under the Sentence node. While some may not see the resolution of these issues as vital, I submit that they have some importance, particularly

if we take seriously Dore's use of "formalization" in this context.²²⁹

There are some attractive features to this account, not least the attempt to integrate the formal aspects of language structure with structures representing communicative functions without suggesting that the former can somehow be reduced to the latter. However, there is a great deal which is confusing and which casts doubt on the status of the theory. There are, for example, several unexplained notational modifications such as the change from Primitive Force to Elementary Illocutionary Force to Illocutionary Force and we are given no indication as to what the distinguishing characteristics of these categories are beyond the fact that they appear to co-occur with either rudimentary referring expressions, rudimentary propositions or sentences. But these latter could be seen as equally in need of elucidation.

Because of factors like this it is difficult to contemplate rigourously testing the proposals against Conditions 1 - 4, but a brief word is perhaps in order. Putting Condition 1 aside, the difficulties immediately emerge with respect to Condition 2. There is vagueness as to

229. Actually the issue is extremely confused as Dore suggests that the child's intention will become grammaticalized in the modality component of the sentence but this would insist on too close a correspondence between intention and grammar and would lead no clear role for the node labelled 'illocutionary force'.

the exact nature of the domain under investigation and, therefore, little wonder that no general theory of that domain exists to which Dore can refer his proposals. As for Condition 3, there appear to be some rather obvious discontinuities in the development and such terms as 'Primitive Force' and 'Rudimentary referring expression' disappear at later stages but, being charitable, we might suggest that a more careful set of definitions would see them being absorbed into categories which are used at later stages. Any discussion of Condition 4 must await more precise formulation of the theory.

In summary, Dore's ideas to integrate functional and structural development are very much to the point in the present research climate. However, without a clearer articulation of his proposals, it is difficult to see him progressing beyond a position where "this view is highly speculative" (p.35), and analysis reveals that it may be not merely speculative but incoherent.

6.2 Gruber on performatives and constatives

Gruber (1975) begins from Austin's distinction between constatives (utterances which admit of judgements in terms of truth and falsity) and performatives (utterances which do not admit of such judgements) and argues that, in the earliest two-word utterances of one child studied between the ages of 1.24 years and 1.42 years, all items are interpretable as performatives with constatives only

appearing at the end of the period. As the whole argument proceeds from what I consider to be a fundamental confusion between utterances and sentences and between the theory of communication and the theory of sentence structure, I shall not be concerned to evaluate the details of the analysis in what follows. Rather I shall be concerned with the logical structure of the proposal and Gruber's explanation which backs it up. Consideration of the latter will necessitate some reference to the sort of syntactic structures suggested by Gruber as underlying the earliest sentences used by the child.

For simplicity, I assume that the domain under investigation is the set of speech acts available to the child and reconstrue Gruber's position as claiming that the speech act of Stating only appears after certain other speech acts.²³⁰ Putting forward the main claim of the paper, Gruber says (p.517):

There is evidence from the behavioural context that all of Dory's utterances for the first nine weeks were performatives. The complement see, for example, always appears to be accompanied by parallel behavior consistent with the performative meaning 'I indicate to you'. Whenever see was uttered as the complement of some referent, Dory was either

230. It is probably more accurate to see this domain as the interaction between communication and formal sentence structure as in 6.1.2 but, as Gruber himself makes no distinctions along these lines, it is difficult to know whether he had anything like it in mind.

reaching for the object named or described by the referent, pointing to it, or showing it to her mother.

Thus a large class from the child's earliest two-word utterances are what we might refer to as Indicators, a sort of performative where the utterance of the sentence counts as an act of indication, on the assumption that certain conditions not discussed by Gruber are satisfied (cf. Austin (1962) on whether saying so can make it so). Other performatives observed during this period are what I shall call Desideratives and, in connection with these, Gruber says (p.518):

From the behavioral context it was also clear in the case of the performative 'I demand of you' that it was indeed the performative that was intended and not the constative expressed by the adult form 'I want' or 'I am demanding'. For the performative it is necessarily the case that the communication itself is for the purpose of satisfying the desire. A performative utterance cannot lie. ... Dory's bodily activity accompanying these utterances, such as reaching or beckoning for an object was consistent with the utterance being for this purpose of satisfying the desire.

Although Gruber does not commit himself on this, it appears that Indicators and Desideratives are the only classes of performatives used by the child in this period.²³¹

231. There is nothing in the data he presents to indicate that this is not so, although there are examples which will embarrass his principle that (p.519): "in general
cont.

The change taking place at the end of this period is described in the following terms (p.519):

Performatives are still produced ... But in addition there are utterances that cannot be simply performatives ... Here Dory is describing things in her environment. The complements now express predications of the referent remote from the speech act itself. Bodily activity signifying indication or demand does not accompany the communication.

Examples cited include Kathleen coming, powder all gone, shoe on floor and see the baby looking the last of which appears to have both performative and constative characteristics although Gruber does not explicitly suggest this.

Let us agree that we have three speech acts, Indicating, Desiring and Stating of which the first two count, for

231. (cont.) a word chosen to represent holophrastically an underlying structure in child language corresponds to a word of adult language whose content approximates the categories or structures of part of that underlying structure". I refer to such instances as hi lamb where, presumably, hi is expressing 'I indicate to you' and thank you mama where, if this is to be construed as a desiderative, thank you must be seen as originating in something like 'I demand of you'. To save the principle Gruber could resort to some notion of association where hi is somehow associated with seeing and indicating and thank you associated with getting and demanding, but it is unclear how far this path can be trodden without reducing the principle to vacuity.

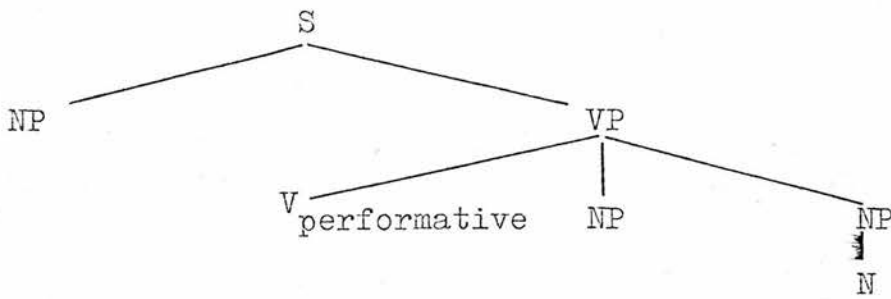
Gruber, as performatives and the last one as constative, and examine the status of his proposal as a developmental theory. Casting it in terms which are amenable to analysis, we can see that what we have, if the domain of enquiry is fixed as those speech acts which the child is capable of performing, is a two-member developmental sequence, (S_1, S_2) where $S_1 = \{\text{Indicating, Desiring}\}$ and $S_2 = \{\text{Indicating, Desiring, Stating}\}$. By inference there will be later stages in the sequence as the child adds to his inventory but we can usefully restrict our attention to the sequence studied by Gruber.

Passing over repetitive discussion of Condition 1, we might suggest that Condition 2 is satisfied at least to the extent that any general theory of speech acts will contain analogues of these three early types. While this is highly debatable for Desiring, I shall assume that it is so for the sake of further discussion. Condition 3 is then satisfied in a straightforward sense: the items from the earlier stage persist into the later stage and one additional item, Stating, shows up giving us a situation which can be schematised as $X \longrightarrow X + Y$ where X denotes the two-member set of speech acts $\{\text{Indicating, Desiring}\}$ and Y denotes the singleton set $\{\text{Stating}\}$. Why should we find this development rather than the alternative, $Y \longrightarrow X + Y$? Gruber discusses this point and can therefore be seen as looking for satisfaction of Condition 4.

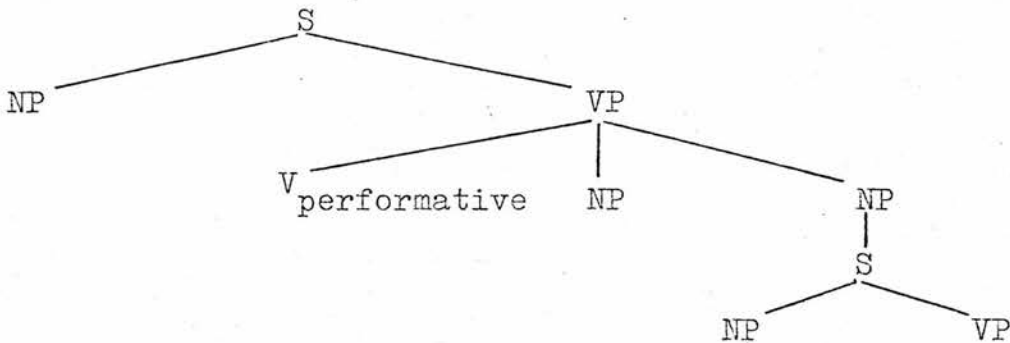
Accepting Ross's (1970) thesis that every declarative sentence has, as part of its underlying structure, a performative verb of saying with a first person subject, second person indirect object and an embedded clause as direct object corresponding approximately to the surface form of the sentence, and the similar arguments for non-declarative sentences found in, e.g., Lakoff (1972), Sadock (1974), Gruber concludes (p.521):

Given that every utterance is obligatorily dominated by a performative sentence underlyingly, the simplest possible sentence structure would be one which consists of a performative sentence only. In such a sentence the direct object (referent) (sic) of the performative is a simple noun phrase as in 'I indicate to you the book', as has been described. This would be the underlying structure of the majority of the child's utterances during the earliest period ... It is not possible to claim, however, that during this period the underlying structure of the child's utterances were of this simplest type only. As has been pointed out, 'see broke' and similar utterances appear to involve an underlying relative clause as in 'I indicate to you the thing which is broken'; also, we do not exclude the possibility of sentences embedded as the direct object of the performative (my emphasis throughout to reinforce the opinion expressed at the top of page 474 above - RMA)

What we have here is a putative explanation which is weakened and subsequently abandoned by the author himself. The claim is that structures of the type shown in Figure 85

Figure 85

are simpler than those of the type shown in Figure 86

Figure 86

where, in the former, the direct object of the performative verb is a simple N (corresponding to the fact that objects can be indicated and desired) and in the latter the direct object of the performative verb is an embedded sentence (corresponding to the fact that one cannot state or assert objects but only propositions). This has a certain intuitive appeal, but Gruber argues against it by his treatment of see broke as involving a relative clause and his insistence that the performative-like verb demand be allowed to take sentential direct objects from the earliest stages of language development. Ultimately this line of speculation evaporates in the view that all the underlying structures are innately supplied anyway, and

therefore questions of developmental sequence do not arise in connection with them (p.522):

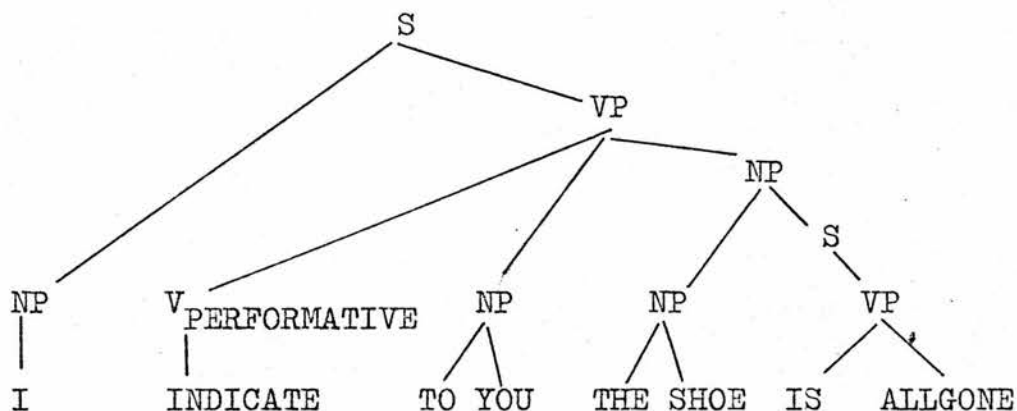
... these considerations are in accord with the hypothesis of the universality of the base among the languages of the world, which would be a matter of innate competence ...

The developmental sequence remains unexplained but there is another avenue and Gruber explores it. It involves differences in lexicalisation. At the earlier performative stage the child is seen as lexicalising the performative aspect of a sentence together with its direct object if it is a nominal or a lexical item representing an embedded sentence if this is what we have in direct object position. At the constative stage, however, as well as these patterns, we also find (p.523): "the advent of the ability to lexicalise analytically an embedded sentence direct object so that both the underlying subject and predicate of the embedded sentence have separate lexical representations". The explanatory principle is then presented as follows (ibid):

In regard to the two clear stages of development observed here differentiation in lexicalization competence consists in the acquisition of subdivisions of the base into potentially lexicalizable constituents proceeding successively down the underlying base tree. During the performative stage complex utterances reflect a subdivision of the base between the performative and its direct object ... During the constative stage a further subdivision of the base is acquired lower down the tree between the subject

and predicate of the sentence embedded as direct object of the performative.

To illustrate this principle Gruber presents the example of Figure 87 with lexicalisations from his two stages.



1.

Perf-
ormative
stage

see

allgone

2.

Cons-
tative
stage

see

shoe

allgone

Figure 87

Two glaring weaknesses in the supposed explanation are immediately apparent. The first is that the subdivision governing lexicalisation at the performative stage does not honour constituent structure in the tree and, therefore, there is no reason to expect the split which Gruber is suggesting between the performative aspect of the structure and the rest of the tree rather than, say, a split between the subject of the performative verb and the rest of the structure or any other conceivable split.

More importantly, we do not even have the beginnings of an explanation for the absence of constatives from the earliest two-word utterances. To see this we only have to consider the putative structure of Figure 88 together with some possible lexicalisations from the performative stage. The principles we have been presented with do nothing to rule out a structure of this sort. In particular, since the underlying structures are innately supplied, there is no question of this structure not being available at the performative stage. Figure 88 shows the claimed but inconsistent split between the performative aspect of

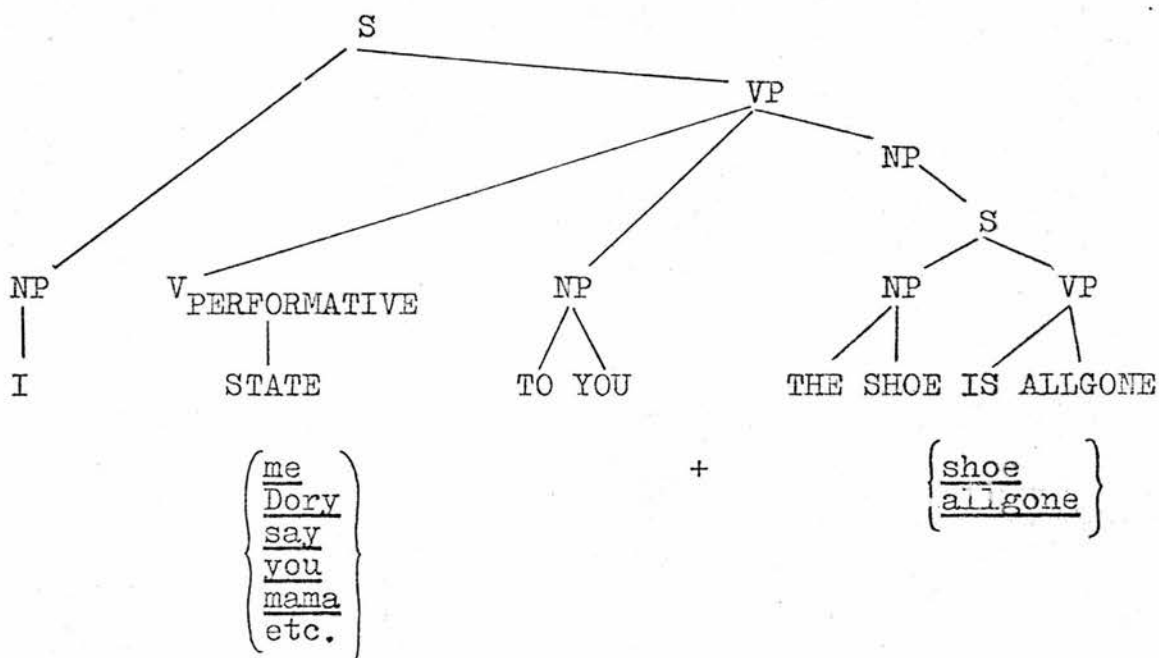


Figure 88

the structure and the remainder as far as lexicalisation is concerned and Gruber's principles would lead us to expect such utterances as me shoe meaning 'the shoe something or other' and say allgone meaning 'something

or other is all gone'. The fact that we do not find such utterances argues strongly against his explanatory principles.²³² We must conclude that Condition 4 remains unsatisfied.²³³

6.3 Attention drawing and reference

In a number of recent papers (Atkinson (1974, forthcoming), Griffiths (1974, forthcoming), Lyons (1975, 1977a), Keenan (1974, 1975), Keenan and Schieffelin (1976)) the importance of routines, verbal and non-verbal, for manipulating the attention of an addressee has been emphasised (for some observations on the development of this ability from well before the start of linguistic development, see Bruner (1975b), Bruner and Scaife (1975)). Lyons discusses this topic in terms of his notion of quasi-reference and in Atkinson (1974, forthcoming) I argue that the ability to refer to objects or sets of objects presupposes, in a large number of cases, a previous act of attention-drawing.

232. Gruber could resort to a principle that aspects of statemental performatives are not lexicalised at the performative stage but this would be blatantly ad hoc.

233. If Gruber's arguments were more cogent we would, of course, have an argument in which the more basic theory of Condition 4 is a theory of linguistic structure which is being used to ground a theory of communicative development. This runs quite counter to the trends examined in this chapter and probably reflects the fact that the first version of Gruber's paper was written in 1967.

Briefly, linguistic reference to a particular entity assumes that the addressee can identify the entity under the linguistic description used by the speaker. For the adult there are devices such as restrictive relative clauses, superlative adjectival forms, phrases like the first, etc. which enable him to realise this assumption without recourse to non-linguistic means. The child, however, at the beginnings of language development does not have access to this sort of construction and usually refers to objects simply by using a nominal (either a common noun or proper name) often with a schwa vowel in the article position (cf. Dore, Franklin, Miller and Ramer (1976)). Of course, he cannot usually expect to succeed in referring using such limited linguistic devices unless he performs some additional act to reduce somehow the possible range of referents. This act may take the form of pointing, which is interpreted as directing the addressee's attention along a particular line of regard which includes only one entity fitting the simple linguistic description used by the child. Further possibilities, however, are that the child may resort to linguistic means to direct his addressee's attention and a considerable portion of the argument in Atkinson (1974) is devoted to producing evidence for this possibility. Certain forms such as see, look, there (cf. now Gopnik (1978)), here and this (in the speech of one child studied in the project reported in Griffiths, Atkinson and Huxley (1974)) were

most naturally interpreted as attention-manipulators and this was seen as the beginnings of an explanation for why we find such forms being used so commonly early in language acquisition. Taking the argument somewhat further, I proposed that some uses of nominal forms were best interpreted as having the same function, i.e., that some uses of, e.g., doggy should be viewed not as holophrastic encodings of something like 'That's a doggy' but as attempts to get the addressee attending to a particular dog often, but not always, with the intention of going on to produce a statement about it. This possibility was exploited to explain aspects of such recalcitrant phenomena as replacement sequences (Braine (1971), Bowerman (1973)) and, more generally, repetition. As an additional suggestion I speculated that instances of apparent questions on the part of the child could be similarly interpreted as attention-drawers and related this to the peculiar phenomenon, found for a time in the development of a fairly large proportion of children, of the child apparently asking a question and then answering it himself immediately (see Keenan, Schieffelin and Platt (1976) for further speculation, argument and data along these lines). Several of these lines of research are admirably summarised and expanded upon by Griffiths (forthcoming).²³⁴

234. Lyons in the works cited above argues that quasi-reference is to be distinguished from reference as the latter is a bed-fellow of predication and the reader is
(cont.)

As usual it is of little concern to me in this context whether the various points made above are entirely justified. What is of interest is the status of the theorising and in order to approach that I need to spell out one or two additional concepts. In Atkinson and Griffiths (1973) it was argued, within a quasi-logical framework the details of which are irrelevant here, that an appropriacy condition on being able to refer to an entity by the + X where X is a simple common noun was that the entity should be the unique object in the addressee's attention such that he believes it to be an X. Lifting this condition into the context of language development is straightforward and we can now consider the speech act of making a statement (or, for that matter, asking a question or issuing a command). This involves, among other things, in the framework of Searle (1969), propositional acts of referring and predicating and we can say that it is a necessary condition on the performance of an act of making a statement that acts of referring and predicating are successfully performed. But the act of

234. (cont.) referred to these works for a more extensive discussion of the theoretical foundations of the distinction and some programmatic remarks on its significance for the study of child language. It is interesting to note that precursors to the distinction can be found in the categories of judgements embraced in the Port-Royal Grammar and Logic and in the metaphysics of Brentano (cf. the distinction between categorical andthetic judgements and its exploration in an interesting way in the context of Japanese syntax by Kuroda (1972)).

referring itself, it has just been argued, has as a necessary condition on its successful performance, the fact that the addressee's attention is appropriately directed, which can be achieved by non-verbal pointing or by verbal means. It follows then that, while we cannot conclude that verbal acts of attention directing are a necessary condition for referring (as the same effect can be achieved without them), we might expect such acts to appear in the repertoire of the child before he produces statements. Atkinson (1974, forthcoming) can thus be seen as amounting to a developmental theory of inventories of speech acts with just two stages, (S_1, S_2) , such that S_1 denotes the set of speech acts with only one member, {Attention-drawing} and S_2 denotes the set of speech acts, {Attention-drawing, Stating}. Whether this sequence can be embedded in a more extended one is a question to which I shall return briefly in 6.4.

Again putting Condition 1 aside we can see that the theory is in a similar position to Gruber's with regard to Condition 2. No one, to my knowledge, working within a speech act framework has discussed the act of manipulating an addressee's attention but, as already indicated, it has been considered within rather different sets of assumptions by Atkinson and Griffiths (1973) and Lyons (1975). It certainly appears that a case can be made for the primary function of English sentences of the form

There's ... and Here's ... being that of attention-directing.²³⁵ It seems fairly safe to conclude, therefore, that a full inventory of speech acts will include reference to such an act. With the reference to appropriacy conditions which we find in the above, contact is made with the Austin-Searle tradition which adopts some similar notion as central and, on these grounds, it is tempting to say that the proposal is on somewhat stronger ground than those of Dore and Gruber both of which treated the speech act as an unanalysable atomic construct.

Just as for Gruber, Condition 3 is satisfied in an additive fashion with S_2 containing everything in S_1 plus something new and so we can schematise the development as $X \rightarrow X + Y$ and ask, in accordance with Condition 4, why we find this rather than $Y \rightarrow X + Y$ where X denotes the set {Attention-drawing} and Y denotes the set {Stating}.

It seems unlikely that we are going to find an answer to this by reference to the languages of the world all of which might be expected to make available vehicles for making statements and for manipulating attention.²³⁶ No readily available cognitive or perceptual reduction suggests itself and so we are left with the possibility that the theory might be logically grounded and enough has been said already to indicate that a case can be made in this

235. For more extensive and enlightening discussion, see Isard (1975) and for a formal framework in which speech acts can be seen as changing the context, Apostel (1972).

236. see p. 489.

direction. According to the theoretical framework adopted, the act of making a statement depends upon the satisfaction of certain appropriacy conditions to do with reference. These conditions may be, although they need not be, established verbally and, to this extent, we can see that it is built into the structure of the theory that attention-drawing (although not necessarily verbal attention-drawing) precedes the making of statements. The alternative sequence of development raised by Condition 4 is incoherent in this framework: one cannot refer using simple expressions until one has done some additional work.

Atkinson's suggestions (1974, forthcoming) do somewhat better than the earlier proposals in this chapter and that is hardly surprising, since it was in writing those papers that I became aware of the need for the sort of evaluation criteria advanced here. The proposal is very restricted in scope and we can now move on and consider

236. If we were to go beyond the bounds of human language and consider non-human systems of communication, we could speculate that we might find systems in which it was possible to manipulate attention but which provided no analogue of 'making a statement'. Within the immediate spatio-temporal environment the making of statements is something of a luxury. Once the attention of an addressee has been directed in an appropriate fashion it is possible for such an addressee to directly perceive whatever 'facts' may be of relevance without the speaker supplying those facts within the communication system.

how Griffiths and Keenan have modified and extended the above suggestions.

6.4 Vocatives and attention

Griffiths (1974), having described some of the ideas discussed in 6.3, says (p.8):

I shall also try to show that drawing attention to something can be decomposed into first getting someone's attention and then, having got it, putting something into it. The act also appears to presuppose one having noticed the something oneself.

The obvious linguistic candidate for securing the attention of someone is a vocative utterance. There are also non-linguistic devices having the same function such as waving and it is easy to see that an argument analogous to that of the previous section can be constructed. Just as the addressee's attention to a restricted set of objects is a necessary condition on a speaker successfully referring, so having an addressee attending to oneself is a necessary condition on directing the addressee's attention in a particular direction. Now, of course, it is perfectly possible that one single-word utterance could perform the dual function of both getting the addressee to attend to the speaker and directing the attention of the addressee in the direction of the speaker's interest. Similarly, it is the case that statements can, in the absence of an explicitly attention directing utterance, serve to adjust the addressee's attention in such a way

as to make the statement intelligible. But clearly there are utterances which exclusively serve the 'attend to speaker' function (cf. Dore's category 'Calling' in 6.1.1) and, most importantly, Griffiths finds instances of these in his data before he finds instances which can be plausibly interpreted as directing the addressee's attention. Although Griffiths does not develop his analysis along these lines, it seems profitable to view what is being proposed as a three-stage theory in the development of speech acts, (S_1, S_2, S_3) where S_1 denotes the set {Calling} (to borrow Dore's term), S_2 denotes the set {Calling, Attention-drawing} and S_3 denotes the set {Calling, Attention-drawing, Stating}. For such a theory a discussion of Conditions 1 - 4 could now proceed in identical fashion to that in 6.3 and the direction of the development from S_1 to S_2 would be explained as grounded in the theory which assumes that a successful act of Attention-drawing presupposes an act of Calling.

In addition to raising the question of vocatives, Griffiths also discusses what, following Dore again, we can call, Noticing. He rightly points out that the speaker noticing an entity is a pre-condition on his directing his addressee's attention to it, although not, of course, on his directing attention to himself with a vocative, and we can infer that, if we wish to regard Noticing as a

speech act,²³⁷ then a similar argument to those above can be constructed to explain the fact that it appears very early and, in particular, that it predates attention-drawing. That is, we would have a partial ordering on this set of speech acts representable as in Figure 89.

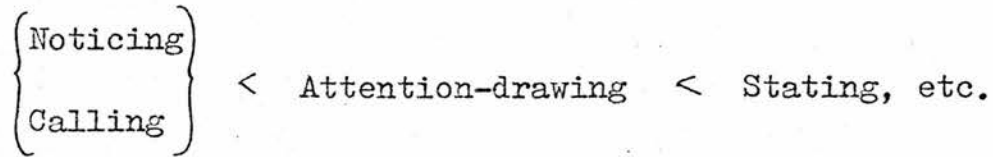


Figure 89

This partial ordering is explainable by reference to necessary conditions on the performance of particular speech acts.

Keenan and Schieffelin (1976) in the context of a discussion of the notion 'discourse topic' attempt to bring together a good deal of the above discussion. They present their central ideas as in Figure 90 (p.493).

There are two remarks to make in connection with this schema. First the notion of discourse topic is not directly relatable to anything which has been discussed

237. Given the earlier remarks on the lack of any audience-directed intention in cases of Noticing, it is almost certainly incorrect to regard it as a speech act assimilable to any of the current frameworks. We could weaken the idea of audience-directed intention to allow the audience to be the speaker but, as Chomsky (1976) has pointed out, this seems to destroy most of what is distinctive in the communication-intention approach.

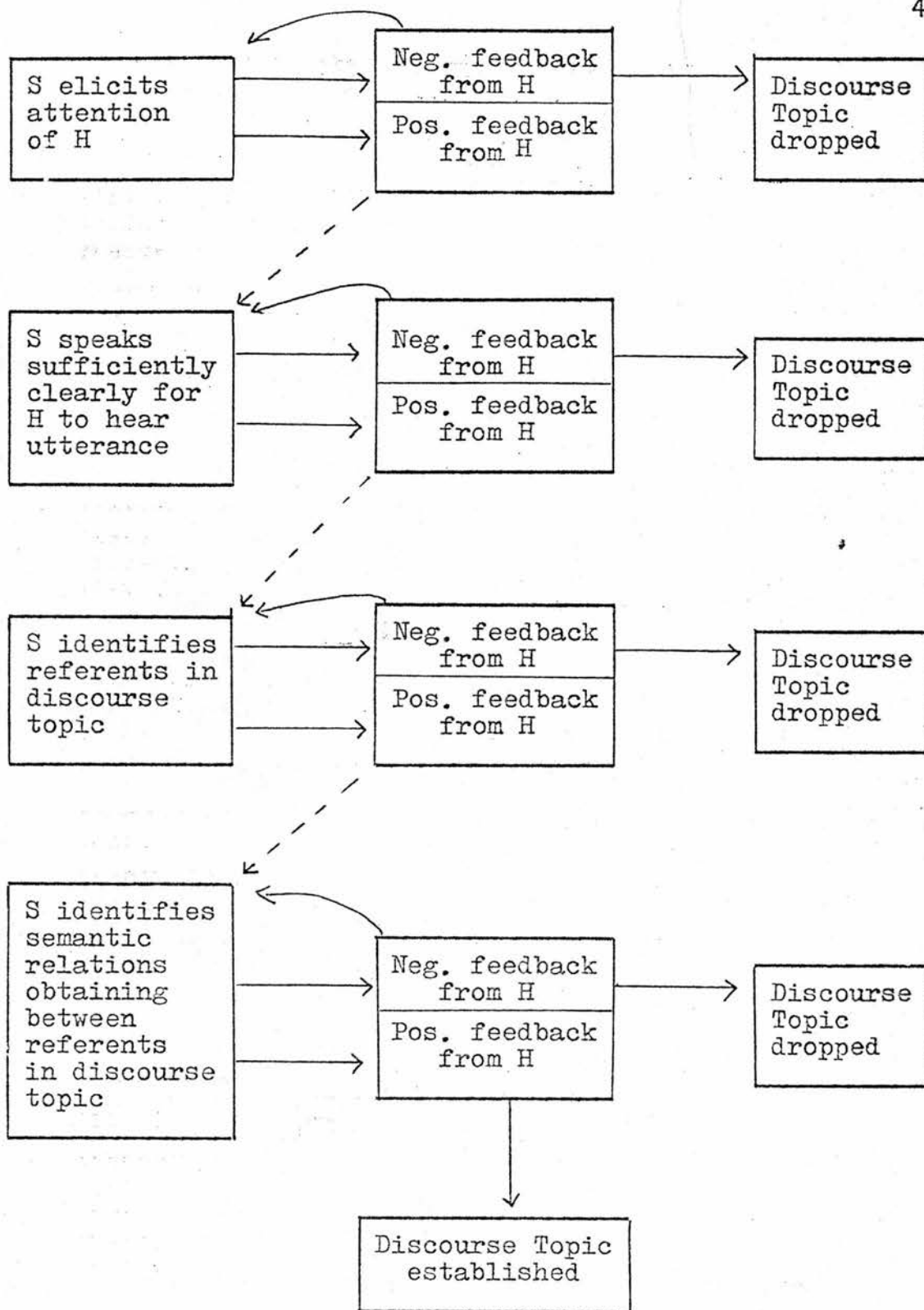


Figure 90 From Keenan and Schieffelin (1976, p.353)

above since, for Keenan and Schieffelin, a discourse topic is a proposition rather than a single object or set of objects: a discourse topic (p.343) "expresses a concern (or set of concerns) the speaker is addressing". Second the above figure is not presented as a developmental hypothesis but rather as a "dynamic model for establishing a discourse topic" (p.353). Nevertheless, it is possible to see the model as encompassing most of the content of the ideas of Atkinson and Griffiths. So, corresponding to the first line of the Keenan and Schieffelin model where the speaker elicits the addressee's attention we have the Calling function. In a conversation no progress can be made until the addressee is attending to the speaker and, in the language development of the child, he must develop devices for focussing attention of addressees on himself before he can go any further. There is no correlate for the second line of Figure 90 in the Atkinson and Griffiths proposals and there does not appear to be any good reason to expect such a correlate within a developmental theory of speech acts.²³⁸ For the third line we have the correlate of the child manipulating the addressee's attention already discussed at length

238. What is involved here would be treated by Searle under 'input and output conditions' (see Searle (1969,p.57) but, unlike some of the other conditions discussed in the text, it does not seem plausible to regard such input and output conditions as being establishable by an antecedent speech act. Because of this we should not anticipate them fitting into any ordered sequence of speech acts.

and, for the fourth line, again no correlate in what has been discussed so far. This is because the ideas put forward by Atkinson and Griffiths were all relevant to what we might refer to as the 'pre-statement stage' of language development and the identification of semantic relations, which Keenan and Schieffelin have as the fourth stage of their model, involves the encoding of statements - a process they see as necessary to the successful communication of further statements which, in one sense of the term, presuppose the earlier statement content. There is clearly no contradiction here so long as we realise that Keenan and Schieffelin are operating with their own definition of discourse topic which is not identical to the traditional notion of topic (see Lyons (1968) for a summary of traditional views). So, to all intents and purposes, we can see that the Keenan and Schieffelin model accords well with the Atkinson and Griffiths suggestions to some extent unifying them in a coherent conversational framework.²³⁹

6.5 Bruner on the ontogenesis of speech acts

The exact subject matter of Bruner (1975a) is not easy to assess. Although the title of the paper is 'The ontogenesis of speech acts', there is little in it directly concerned with speech acts except for an emphasis

239. Keenan, Schieffelin and Platt (1976) further develop the above model without adding anything of interest to the current discussion.

on language use and some rather confusing discussion of the standard philosophical literature.²⁴⁰ Some of what Bruner says indicates that he is interested in perlocutionary force and its relationship to the intentions of the speaker but it is difficult to glean anything systematic from the paper on this issue and it is perhaps fairest to say that the real content of the work lies in a domain having nothing to do with speech acts except in a most peripheral way.²⁴¹

There appear to be three aspects of linguistic structure (broadly interpreted) approached by Bruner. The first is to do with sequencing in conversation and the breaking down of conversation into units and, as such, it has nothing

240. To this point I note the following passage (p.3): "The relation between the instrumental or illocutionary function of an utterance and its grammatical structure is, I shall argue later, crucial to language acquisition" where, assuming that "instrumental or illocutionary function" is to be interpreted as providing alternative labels for the same concept, Bruner displays a lack of awareness of the distinction between illocutionary and perlocutionary acts.

241. What the argument of the paper boils down to is that there are correspondences between aspects of pre-linguistic behaviour and the structures which come to encode information about the environment. In many ways the analysis of his arguments would fall most naturally into the format of Chapter 4 but, since Bruner makes a great deal of the integration of linguistic and non-linguistic communicative behaviour emphasising the role of context, it seemed to me that it could be appropriately included here.

to do with sentential structure. The second is concerned with case relationships and their realisation in linguistic structures. And the third, most closely related to the title of the paper, discusses subject-predicate formations in language and their relationship to topic-comment structures. I shall consider each of these in turn.

The relevant data on the imposition of unitising structure on conversations is provided by the study of interactions between mother and child. The claim is advanced that many of these interactions, before the child has any language, are broken down by the mother into 'segments' (p.12):

In the case of intention-oriented interactions, the principal form of signalling is MARKING THE SEGMENTS OF ACTION. Most usually it begins by the use of terminal marking, the use of what might be called a COMPLETIVE. The child takes a mouthful of newly introduced food from a spoon; the mother exclaims, Good boy! with distinctive intonation. Or he offers back an object handed to him, and the mother exclaims There! Or he removes a ring from a peg-and-ring toy, and the mother cries Aboom! It may well be that completion marking of this kind serves as an initial step in primitive semantic segmentation, the forming of units.

The suggestion seems to be that the child's primitive conception of ongoing interaction can be seen as a continuum which we may represent as a straight line:

As far as the adult is concerned, this continuum is broken down into units each of which corresponds to a 'turn' in the interaction:

_____ / _____ / _____ / _____ / _____

and the child is 'taught' this structuring by the mother's use of completives (there is no suggestion that this is the only device the mother has available for signalling the end of a 'turn'). Once this amount of structuring is achieved the child is in a position to individualise items in the interaction sequence, to repeat items, to vary items, to substitute some items for others, etc.

(p.13):

Segments of action are, in effect, positions occupied in a sequence by varying or substitutable acts. It is in this sense that we conceive of them as representing privileges of occurrence for classes of acts and, consequently, a particularly important form of psycholinguistic learning.

But exactly what relevance has this to psycholinguistic learning? The structure of the argument seems to be that actions in interactional sequences can end up with the sort of unitised structure referred to above and that this is of relevance for the developing language of the child. An attempt is being made to identify some aspect of language structure with the structure of actions in early interactions. But what aspect of language structure? Certainly not sentence structure because the completives typically terminate an action which would subsequently be encoded by a whole sentence; they do not terminate

pieces of action which can be related to sentence-internal units.²⁴² Therefore, it must be some level of structure above the sentence and, undoubtedly, there are principles governing the construction of well-formed conversation although these have not been well-articulated. It appears that this is the most likely domain for identifying structures which are isomorphic to the action sequences Bruner discusses. But, even in this domain,²⁴³ remarkably little is shown by the above argument. In conversation we have the phenomenon of a question followed by an answer or some response to indicate why an answer is not forthcoming. To my knowledge Bruner has not indicated any sequences in non-verbal interaction which can be interpreted in this way. Further we have the notion of a speaker meaning something distinct from the literal meaning of his words, this speaker's meaning being determined from the literal meaning of the words by poorly understood principles which almost certainly make reference to the conversation in which the relevant utterance is embedded (see Grice (1968, 1975) for the beginnings of a theory in

242. It might be suggested that Bruner's concern could be related to the sorts of procedures put forward by a number of N.American structuralist linguists which depended crucially on the notion of substitution (see, particularly, Harris (1951)) but there is no indication that he has this sort of affiliation in mind.

243. One of Bruner's aims is to attempt to produce functional explanations for phenomena which have given rise to innateness claims. It is worth pointing out that the domain of turn-taking in conversation is not one which has led to such claims.

this area). Exactly where are the correlates of this complex facet of conversation in non-verbal interaction? In short, it seems that Bruner has succeeded in identifying only the crudest and least interesting aspects of conversational structure in non-verbal interaction and this, coupled with the fact that the structural properties under analysis are at a level above the sentence, makes any interpretation of these proposals as an attack on linguistic nativism quite beside the point.

Moving on to the second set of arguments, these are of more relevance to sentence structure. The first observation is in support of a general proposition that "The facts of language acquisition could not be as they are unless fundamental concepts about action and attention are available to children at the beginning of learning".(p.6) This observation follows from a collation of the evidence, much of which has already been discussed, from Bowerman, Brown and Schlesinger on the semantic characteristics of the child's first one and two-morpheme utterances. Summarising this evidence, Bruner says (p.7):

These various sets of data suggest that the child, in using language initially, is very much oriented towards pursuing (or commenting upon) action being undertaken jointly by himself and another.

As a general description this is unexceptionable, but examination of later stages in the argument reveals that Bruner probably intends something more than description.

Recall that the structure of a sequence of actions which was claimed to emerge via the mother's use of completives was represented as:

_____ / _____ / _____ / _____ / _____

Now, going inside each unit in the sequence, we note that it is claimed that mothers, from an initial stage where the child focusses his attention on the agent of actions, go through a process of "dramatizing or idealizing the act itself with some kind of serial marking" (p.13). This is described in terms of the mother making sounds to accompany the action, the end result being a distinction between the Agent and the Action within a single act in the sequence. That is, we have a structure which we can represent as:

Agent-Action___/Agent-Action___/Agent-Action___/...

and further speculation shows how this can be elaborated to allow reference to a Recipient and, presumably, an Object although there is no discussion of this latter category. It is necessary then to assume, for the argument which follows, that the output of this structuring of actions are action units, each of which can be seen as having a structure of the form:

Agent - Action - Object - Recipient

and this corresponds, in linear order, with the most normal word order in an English sentence encoding each of these notions, a correspondence which Bruner grasps and uses

when he says (p.17):

The argument has been that the structures of action and attention provide bench-marks for interpreting the order-rules in initial grammar: that a concept of agent-action-object-recipient at the pre-linguistic level aids the child in grasping the linguistic meaning of appropriately ordered utterances involving such case categories as agentive, action, object, indirect object and so forth.

But this is a poor argument at two levels. The first is concerned with the facts concerning word-order in individual languages. Not all languages have Agent-Action-Object-Recipient as their canonical word order and, therefore, the action sequences described by Bruner could not have a facilitating effect on the acquisition of such languages. Important in this regard is the finding that children do not universally adopt SVO as their first dominant word-order and do not necessarily follow the dominant order in the adult language (for detailed treatment see Bowerman (1973) and 4.3.2 above). The only way to save the hypothesis at this level is to suggest that the strategies mothers and children employ in breaking down action sequences differ according to the language being acquired, but I have seen no evidence cited in support of this suggestion and, indeed, it seems highly implausible. At the second level, let us assume, for the sake of argument, that the first objection can be answered and that, for all languages, relationships can be observed between canonical word-order and segmentation of action sequences.

Still some serious issues would remain unresolved. For example, no progress will have been made on the question of how non-canonical word-orders are learned and it will also remain to explain the source of the formal relatedness between sentences with different structures. In short, I believe that Bruner seriously under-estimates the structural complexity of human languages and that, even if his attempt to reduce certain superficial facts to a non-linguistic domain worked, it would only produce a chink in the armour of the Chomskyan position on innateness.²⁴⁴

The third set of arguments concerns the relationship between aspects of attention and the topic-comment and subject-predicate organisation of linguistic utterances. The hypothesis being explored is that (p.4):

... early language, to be acquired, must reflect the nature of the cognitive processes whose output it encodes. One instance has to do with the isomorphism between a central linguistic form, predication, and the nature of human attention processing.

and further (ibid):

Concerning predication, I refer to the topic-comment structure of utterances, reflected formally in such devices as subject-predicate in grammar or as function and arguments in logical analysis. Topic-comment structure reflects an underlying feature of attention ... and its realisation in language by the use

244. I must emphasise that this is not to say that the Chomskyan position is correct but merely to point out the magnitude of the task of showing that it is not in all respects.

of subject-predicate rules is sufficiently akin to this characteristic of attention to make these rules easily accessible to a language learner.²⁴⁵

The sort of evidence cited in favour of the above claims comes from work on visual attention which Bruner describes, following Neisser, as "positing wholes (topics) to which parts or features or properties may be related and from which the new wholes may be constructed" (p.4). This can best be represented in a format analogous to that used above in connection with the segmentation of action sequences where the child's perceptual experience can initially be represented as a continuum:

subsequently broken down into 'items' of experience:

_____ / _____ / _____ / _____ / _____

which themselves are internally structured by attentional mechanisms giving:

Topic-Comment / Topic-Comment / Topic-Comment/ ...

where each Topic corresponds to the child hypothesising a particular 'object' of perception and each Comment consists of the child filling in details of that object. The claim appears to be that the Topic-Comment structure

245. Although a reference to Chomsky (1965) follows, it is unclear, in this context, what the reference of "subject-predicate rules" in the cited passage is supposed to be, since such functionally defined notions are not explicitly mentioned in the grammars Bruner appears to be trying to make contact with.

emerging from this sort of analysis is formally analogous to the topic-comment structure of utterances.

While the proposal is somewhat vague, it is difficult to see it as suggesting anything very profound. First, although it may make sense to see the topic-comment distinction as one of the organising factors of utterances and discourse-structure, it cannot be identified with the subject-predicate distinction, an organising principle in the theory of grammar (for some discussion of this, see Atkinson (forthcoming)). Second, without giving the reader some clearer idea of what the "subject-predicate rules" are supposed to be, it is impossible to evaluate the substance of Bruner's claim. Third, even if the child can get access to some aspects of grammatical organisation via the structure of attentional routines, again this would only represent the most primitive beginnings of the acquisition of language structure. It seems to me much more productive at the moment to view routines manipulating attention as geared to the establishment of topics in discourse, and I hope to have demonstrated the fecundity of this approach for a theory of the development of speech acts in earlier sections of this chapter. Bruner's suggestions leave us rather in the dark as to what is being explained; if it is the high frequency of attention-drawing devices in early language, then the considerations of 6.3 and 6.4 seem more intelligible; if it is the acquisition of formal

structure, then there is much further to go than Bruner appears to envisage.

Bruner (1975b) contains amplification of some of the points discussed above but does not succeed in providing enough detailed argumentation to make his positions convincing. Strengthening confidence in my interpretation of the first set of issues, we find (p.28):

Let me suggest that the development of the exchange mode marks the beginning of privileges of occurrence in discourse, the emergence of rule-bound exchanges that operate on a wide variety of objects, gestures and calls which, so to speak, become tokens in a standardised transaction.

and (p.29):

In such exchanges, the child is learning to deal practically with such relational concepts as Agent, Action, Possession, Instrument, Recipient of Action, and so forth ... Is it unreasonable to suppose that mastery of the concept of a reciprocal task may provide the basis for later interpretation of sentences?

The topic-comment organisation of visual processing and pre-linguistic play is explored at some length again with a view to seeing the germs of predication in it, but what I find an extremely confusing discussion of the properties of predication can only conclude that "full transition from these early components to full subject-predicate organization in language is as obscure as ever it was" (p.35).

Rather more argument is provided on the relationship between the order of events and the order in which these events are encoded in an utterance and the existence of non-canonical word orders is recognised along with the problems arising from them. That Bruner is making a strong claim with regard to serial order in events and sentence structure is reflected by his references to the work of Cromer and E. Clark discussed in earlier chapters and his citing of data from a child Matthew who, at twenty months (p.39):

... sees an airplane approaching overhead, points and says "Airplane", follows the plane across the sky with his eyes, and comments finally "All-gone", followed a moment later by a connected utterance "airplane allgone". The serial intercalation of comments and context is appropriate, well timed, and natural. The order of events provides the serial order of the utterances.²⁴⁵

Once again though we must note the existence of languages which would not be readily learned in terms of such strategies as well as the existence of non-canonical orders in English. Bruner raises this second point when he says (p.40):

For adult grammar, of course, event order is a weak and rigid rule. Adult grammar obviously is ripe with inversion rules, as in the interrogative and passive.

Obviously the mastery of such non-canonical orders cannot be located in the normal order of events and Bruner's

245. For identical observations, see Menn (1973).

solution is to argue for a "pragmatic approach" to the problem. I find his reasoning on this issue difficult to follow. To locate a source of non-canonical word-order in pre-linguistic behaviour it would appear necessary to demonstrate at least the following:

- 1) There is a non-linguistic behaviour preceding the development of non-canonical word orders in which order is a vital consideration.
- 2) There is a notion of canonical order definable for this behaviour.
- 3) Non-canonical orders are identifiable in this behaviour before the emergence of non-canonical word orders.
- 4) The relationship between sequences manifesting the canonical and non-canonical orders in the non-linguistic domain should be relatable to the relationship (formal and semantic) between canonical and non-canonical word orders.

Much of this needs further analysis but it seems to me that one argument offered by Bruner comes close to satisfying these conditions. He says (p.42):

There is also order violation in play. Reynolds (1972) uses the expression "simulative mode" to emphasise the fact that play bears a close resemblance to "real action", yet departs from it and need not achieve the useful results of real action. Means and ends are uncoupled and conventional or adaptive order loosened. Once conventional or natural order is no longer the sole determinant of the order of acts, new principles of ordering emerge. One such is the principle of emphasis. It is a principle used in adult speech as well. In ordinary adult

discourse the order of a sentence is often chosen for emphasis.

I have not seen Reynold's paper but certainly 1), 2) and 3) above seem to be satisfied and Bruner has begun to grapple with 4) via his reference to emphasis.

The only other suggestion in Bruner (1975b), which is not examined in detail in Bruner (1975a) concerns the relationship between the emergence of a competence to use deictic expressions and the source of this competence in pre-linguistic routines. What the proposal amounts to is that the appropriate use of deictic expressions presupposes, on the part of the speaker, some acquaintance with his addressee's point of view (surely correct) and that various pre-linguistic routines to do with the direction of gaze as well as some early linguistic phenomena such as the use of here you are and thank you in giving and taking games can be seen as indicating just such an acquaintance. At this level of generality the suggestion is correct but does not have an exciting amount of content and so I shall not pursue it.²⁴⁶

6.6 Halliday's functional model

Halliday (1975) represents an ambitious attempt to build a model of language development on a foundation consisting

246. To examine this question meaningfully would require a specification of what aspects of the addressee's cognitions must be available to the speaker in connection with each deictic form and this information must then be seen as important in the behaviours Bruner draws our attention to.

of a functional semantics. The model is concerned, on the one hand, with the development of functionally defined semantic notions and, on the other, with the development of a level of lexicogrammar consisting of realisation rules mapping functional meanings onto forms. In what follows I shall be principally concerned with the former, both for reasons of space and also because Halliday himself devotes more time to this aspect of his theorising in his monograph. The study used one child, Halliday's own, and it was found necessary to divide the total course of development into three phases. In Phase I the child's system²⁴⁷ owes no direct allegiance to the adult system although plausible adult models are suggested for some of the child's forms. The system comprises, as well as a small set of functional meanings and a small set of forms, a simple one-one mapping between them so that each form serves exactly one meaning and each meaning is realised by exactly one form. Halliday sometimes

247. There is scope for discussion as to whether the system is appropriately called a language at this stage or whether it is better regarded as a primitive communication system - discussion which Halliday provides to some extent, producing criteria according to which it is a language. It would, however, be possible to consider alternative criteria which the system fails to meet as Halliday points out. I agree with him that it makes little difference what we call the system at this stage but feel that there is room for disagreement on whether we emphasise the continuity of functional development, as he does, or the discontinuity of other properties.

refers to the system at this stage as a 'Protolanguage'. Phase II is a transitional stage where the child makes generalisations within the set of functional meanings which move him towards the abstract notion of function which Halliday locates in the adult system. Also at this stage he begins to learn a system of lexicogrammar which can be seen as a complication of the mapping between meanings and forms designed particularly to make available the possibility of more than one meaning being served by one form. Additionally, Phase II is characterised by the child's learning of dialogue which makes available a new function, that of imparting information, and we shall return to this below. By the end of Phase II the child has left behind the protolanguage and is ready to embark on the task of learning the language he hears around him, a task which he begins to pursue in Phase III.

As already mentioned, Halliday sees the essential characteristics of the overall development in terms of continuity of function, the way in which the set of functions available to the child changes and, indeed, the way in which the very notion of 'function' changes. In Phase I 'function' is co-extensive with 'use' but subsequently takes on a more abstract sense which can be identified only in the linguistic system whereas 'use' becomes identifiable only through some notion of 'social context'. It would be possible to investigate the macro-structure of the development paying attention to these ideas but

I have decided against this for two reasons. The first is the simple one of space and the second is that I find this aspect of Halliday's theorising extraordinarily opaque. In order to interpret it in such a way as to make my framework applicable to it I would have to make a great many assumptions as a result of which Halliday's position might have little to do with the final product which I matched against Conditions 1 - 4. As far as I am concerned, it takes an act of faith to go along with the transitions between the phases which Halliday describes. This act I cannot perform with sufficient enthusiasm to believe that subsequent analysis will yield a justification for it. I have therefore decided to concentrate my analysis on Phase I with the exception of some discussion of the Informative function and its relationship to the development of dialogue - a phenomenon which occurs in Phase II.

Phase I is divided into six stages, the last of these being transitional between Phase I and Phase II. Six week intervals divide the stages and Halliday provides justification for this (p.12):

If I had chosen a longer period, then certain significant steps in the development would have been left out; whereas if I had chosen a shorter period I would have been at the mercy of random non-occurrences, items which simply had not been observed over the period but which should have been recognised as present in the system.

For each of the six stages Halliday provides a representation of the content systems for this stage along with the expressions which realise each function. These latter will not concern me in what follows and will generally be omitted. For Stage 1 or, as Halliday has it, NL1, the content systems are as in Figure 91.

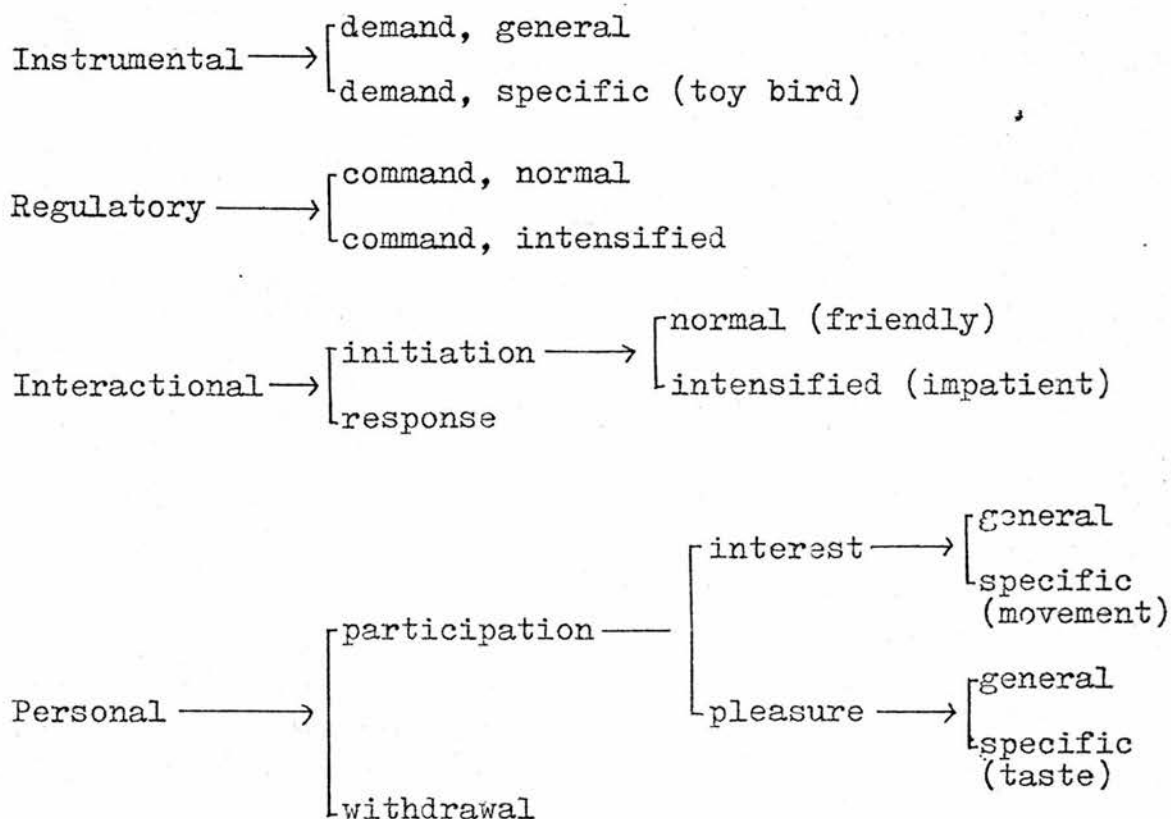


Figure 91. Adapted from Halliday (1975, p148)²⁴⁸

248. For the reader unfamiliar with Halliday's writings Figure 91 embodies the claim that the child has functional meanings in four categories at NL1. These are Instrumental, Regulatory, Interactional and Personal. Within the Instrumental function he is capable of making either a general demand for any object or a specific demand for
(cont.)

In this format Halliday presents a sequence of theories, (NL1, ..., NL6), for his child's development in D where D is fixed as the child's semantic system and the mapping from this semantic system to the level of expression. Since it is one of the characteristics of Phase I that the latter undergoes no development we can concentrate on the former.

To begin with I wish to assume that the theory is a good deal simpler than it is and concentrate on the major functions (i.e., what we find instances of down the left hand side of Figure 91). For these functions we have a developmental theory which we can represent as in Figure 92. (see p.515)

Halliday himself does consider predictions of the order of the emergence of the functions at this level of generality and this provides some sort of justification for the simplifying procedure. As far as Conditions 1 - 4

248. (cont.) his toy bird. This distinction is carried by a distinction at the level of expression between the two forms nā... and bφ. Similarly within the Regulatory function there are just two expressions, ē and mng encoding a normal command and an intensified command respectively and, in general, for every function which is not developed to the right in Figure 91 there is one and only one form regularly correlated with it. This is the property which leads Halliday to insist that at this stage the child does not possess a lexicogrammar but a simple mapping between the two levels of representation with no intervening level of organisation.

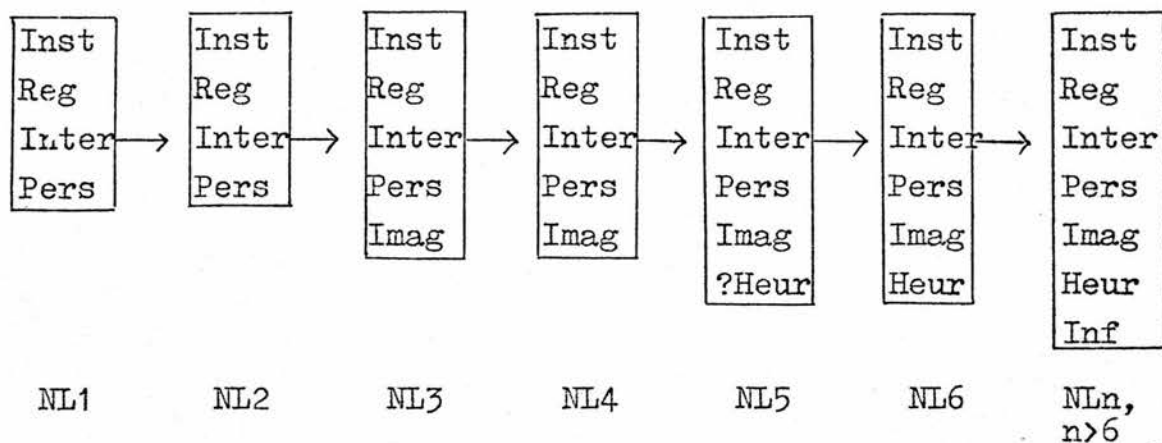


Figure 92 (Inst = Instrumental, Reg = Regulatory, Inter = Interactional, Pers = Personal, Imag = Imaginative, Heur = Heuristic, Inf = Informative)

are concerned and moving directly on to Condition 2, it is to Halliday's credit that he provides some relevant discussion. On the one hand, with respect to the form of his theoretical vocabulary he has this to say (p.13):

In general we cannot represent the content of the child's system at this stage in terms of the words and structures of the adult language. We cannot match the child's meanings with the elements of the adult semantic system, which are again too specific. What is needed ... is a kind of postural notation for the content. What does this mean in fact? It means some form of functional representation of meaning. The content in other words, has to be specified in relation to the functions of language.

Why a functional representation should be more 'postural' (= less specific) than a non-functional representation is not something Halliday elaborates on but, more importantly from my point of view, Halliday directly addresses the

question of constraining the set of functions countenanced by the theory, i.e., he tries to delimit a general theory of functions. He sees the functions necessary for understanding Phase I as having three sources (p.15):

The question then is: what are the functions that we can recognise as determining the child's semantic system at this stage, and how do we arrive at them? Here we must try to keep things in proportion shunting between sensible observations on the one hand and imaginative but at the same time goal-directed theory on the other (my emphasis - RMA)

Thus the first source is observation. When a child utters, parents impute certain intentions to him in a fairly consistent way. Whatever set of functions emerges from such observations can be used in conjunction with theoretical considerations to arrive at a final set. These theoretical considerations involve two sources. The first is theories of language structure which take functional notions as central and the second is theories of social structure which assign an important role to language and its functions. In connection with the former, Halliday turns, not surprisingly, to his own work, e.g., Halliday (1967, 1970) and, for the latter, to the work of Bernstein (1971) on 'critical socializing contexts'. From Hallidayan linguistics come three functions: the ideational function, the interpersonal function and the textual function, and Bernstein provides another four: the regulative, the instructional, the imaginative or innovative

and the interpersonal. These seven functions, along with whatever functions are the product of our observations of early child speech are then submitted to some sort of selection procedure with the result (pp.18 - 21):

Taking these factors into account I had suggested a set of functions which would serve for the interpretation of the language of a very young child; that is as an initial hypothesis for some kind of functional or sociolinguistic approach to early language development. The postulated set of functions was as follows:

- (1) Instrumental
- (2) Regulatory
- (3) Interactional
- (4) Personal
- (5) Heuristic
- (6) Imaginative

... Later on there is in fact a seventh to be added to the list ... This is the one we can call the informative function of language ...

But this procedure is uncomfortable. What appears to be going on can be represented as in Figure 93:

Functions:

X	+	Ideational	+	Regulative	Selection
		Interpersonal		Instructional	
		Textual		Imaginative	→
				Interpersonal	

Source:

Observation	Hallidayan	Bernstein's	Instrumental
	Linguistics	Sociology	Regulatory
			Interactional
			Personal
			Heuristic
			Imaginative
			Informative

Figure 93

but we are told nothing about the constituents of X, nothing about the procedure for identifying functions across disciplines and nothing about the procedure of selection. It is remarkable that Halliday makes such a bad job of justifying his choice of functions, given his realisation that a mere inventory of functions not related to any general theory would be unsatisfactory. Condition 2 is not satisfied in a convincing way. Condition 3 appears to be on much firmer ground as there is no development in the set of general functions from NL1 to NL2, from NL3 to NL4 and from NL5 to NL6 while there is a straightforward additive complication between NL2 and NL3, between NL4 and NL5 and between NL6 and NLn.

Consider now the transition from NL2 to NL3 from the point of view of Condition 4. This condition raises the question as to why the Imaginative function appears after the four found at NL2 rather than the other way round. Similarly, for the transition from NL4 to NL5 we ask why the Heuristic function should only appear after the five functions found in NL4. and, for the transition from NL6 to NLn, why the Informative function should appear last of all. Halliday is not uninterested in these questions and, as we shall see, he provides an attempt to answer the last one. As far as the first two are concerned, he says, with reference to the functions listed as in the passage cited on the previous page, (p.37):

The hypothesis was that these functions would appear, approximately in the order listed, and in any case with the 'informative' significantly last ...

But, as we have already seen (p.40):

... in one important respect the hypothesis fails ... there is no sign of a developmental progression within the first four functions ... Furthermore, the imaginative function seems to appear before the heuristic.

Had the functions emerged in the predicted order it is not clear what principles he would have invoked as explanatory: the obvious generalisation is that the pragmatic functions appear first and, to use Halliday's term, the mathetic functions later but, in itself, this is merely to set up superordinate categories and not to approach explanatory adequacy.²⁴⁹ We must conclude that Condition 4 is not satisfied by the transitions from NL2 to NL3 and from NL4 to NL5. Predictions made by the author on a basis he never makes clear are unfulfilled and he makes no attempt to ground the development he finds.

There is more to say in connection with the transition from NL6 to NLn. Halliday invokes the fact that, of the seven functions he is concerned with, the Informative is the only one which depends upon language for its fulfilment, (p.31):

249. Cf. in this respect the position of Bühler alluded to briefly in Chapter 1.

The use of language to inform is a very late stage in the linguistic development of the child, because it is a function which depends on the recognition that there are functions of language which are solely defined by language itself. All the other functions in the list are extrinsic to language. They are served by and realised through language, but they are not defined by language. They represent the use of language in contexts which exist independently of the linguistic system. But the informative function has no existence independent of language itself. It is an intrinsic function that the child cannot begin to master until he has grasped the principle of dialogue, which means until he has grasped the fundamental nature of the communication process.

There is much that I find difficult in this passage but what is clear is that it is an argument for the Informative function coming late (cf. fn. 226 above in connection with the view that the informative function can only be served linguistically). If Halliday is correct and this function is defined only by reference to language itself, then it clearly must follow some language and, if language is construed functionally, then it must follow some function or functions. But this does not show that it necessarily follows all functions, i.e., comes last in the set of seven. So, the notion of dialogue to which Halliday refers becomes crucial. Mastery of dialogue is the sine qua non for the emergence of the informative function and if an explanatory statement can be produced demonstrating why dialogue follows the six functions preceding the

Informative we would be justified in optimism. Of dialogue Halliday says (p.30):

Dialogue is, for [the child], a very new concept. Dialogue involves the adoption of roles which are social roles of a new and special kind, namely those which are defined by language itself.

But this fails to distinguish dialogue, in the required sense, from the Informative function. There is little illumination in being told that the Informative function is to be understood by reference to dialogue which is explicated in terms of new social roles where these latter are those characterised by instances of the Informative function. Furthermore, unless we see language as characterised by the full set of six functions rather than any subset of them, no convincing reason has been offered for why, for example, at NL2, the child should not develop dialogue and the Informative function (before the Imaginative and Heuristic functions) on the basis of his language at NL1 which includes the four early functions. The transition from NL6 to NLn seems to go the way of the earlier transitions without some independent characterisation of the 'new' social roles which make the development of dialogue possible. Condition 4 is nowhere satisfied in Halliday's model of functional semantic development.

I would like to consider briefly, and without going into much detail, some of the finer points of Halliday's

content systems. We can compare Figure 91 above with Figure 94 which is from NL2.

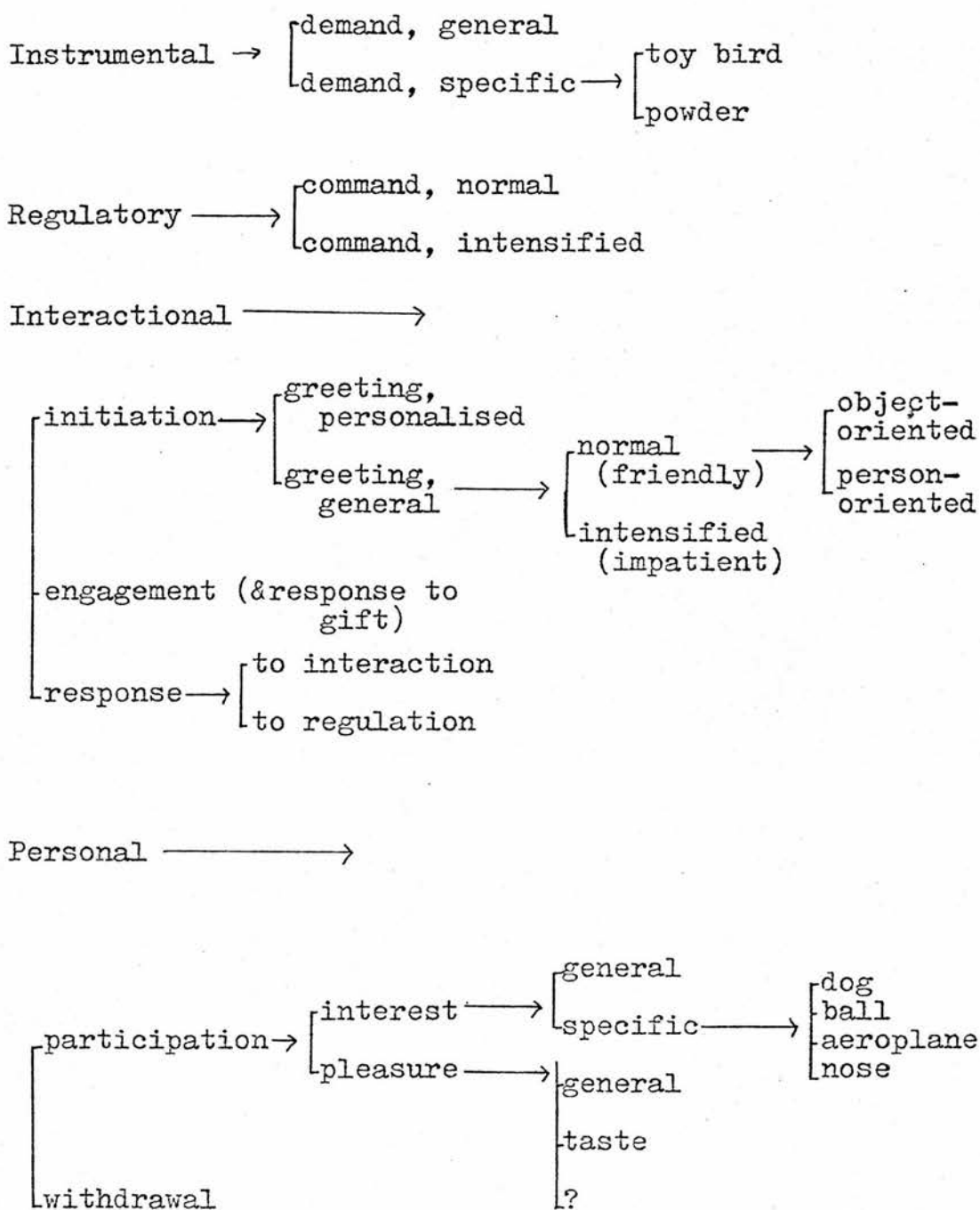


Figure 94 Adapted from Halliday (1975, p.149)

What interests me about Figure 94 is whether there are any explanatory principles governing its development from Figure 91. In certain cases this question may not make a lot of sense. So, consider the development within the Instrumental function from NL1 to NL2. The only change at NL2 is that the additional option of demanding powder has emerged as a choice within the 'specific demand' option. There is no reason to believe that, within Halliday's framework, there is an interesting answer to the question as to why this option only appeared after the option of demanding a toy bird.²⁵⁰ But now consider the development within the Interactional function. At NL2 we have the appearance of the function of Engagement subsequent to the emergence of Initiation and Response and the introduction of a function of General Greetings to include Normal Greetings and Intensified Greetings which were treated as instances of Initiations at NL1. It seems to me that in the case of these fairly gross functional categories it is possible and desirable to demand an explanation for order of development of the sort Halliday himself tries to provide for the most general functions. The need for this is particularly acute when we do not

250. Of course, at the right hand side of one of Halliday's systems functional and 'referential' meaning are conflated and the sorts of considerations pursued in Chapter 2 might be of relevance. I see no reason why the proposals considered there could not be combined with a theory in the Halliday mould but such speculation will not be pursued here.

have a straightforward general trend towards more and more specific functional categories but instances of general categories being introduced to 'dominate' two or more specific categories, exactly the situation we have with the introduction of General Greetings. Halliday provides no discussion of these issues and I have no ready answers but, if his proposals are to be treated seriously as an adequate developmental theory, they must ultimately be approached.

In this chapter, perhaps more than in any other, I have felt the lack of general theoretical concepts in the work of the scholars discussed. There is a sophisticated literature on speech acts in the linguistic and philosophical journals, but it appears that the field of child language has not availed itself of it in a very meaningful way. The result has been a good deal of ad hocness and ill-motivated categorisations which fail to excite the imagination and can only delight the critical faculties. This is not to suggest that theorists of the adult language have solved most of the important issues nor that studies of child language cannot enrich our understanding of speech acts in general. Both of these propositions are, I believe, false, but we can demand a more sophisticated approach than has been evident in the above and it is only with the advent of such a non-taxonomic approach that progress will be achieved in this area.

CHAPTER 7 : CONCLUSIONS

My discussion has been lengthy but my conclusions can be brief. It is my belief that the above analyses are valuable in at least two ways. The first is that they enable one to see the inadequacies and strong points of theories within one coherent framework. One is often struck, when reading research in child language, by feelings of unease which it is difficult to be precise about: in many cases, this feeling has been uncovered and explained in the preceding chapters. The second is quite independent of the particular methodological conditions I have put forward and investigated at such length. It is concerned with a general level of consciousness within the discipline. If this work has the function of making theorists in this area more conscious of the need for evaluation procedures and stricter argumentation, then it will have fulfilled its primary function. This could be the case even though the methodological conditions I have suggested fall by the wayside in later developments.

In several places I might be accused of having done violence to an author's intentions by reformulation of his theory in a way which makes my conditions applicable to it. It seems to me that all the conditions demand is an answer to 'What is this theory saying?' and that, where it has been necessary to guess what an author's intentions might have been or to remove a vagueness, this has always

resulted in a more precise and conceptually clear theory. If it is thereby exposed as an inadequate theory this can only be for the better and the onus is on the proponent of the theory to put forward a precise interpretation differing from mine which more adequately represents the intended view.

The conditions have been interpreted as constituting necessary conditions on the adequacy of developmental theories and it remains to say something about why, taken conjunctively, they may not be regarded as sufficient conditions. As I have already pointed out in Chapter 1, there is an additional component in such theories to which Conditions 1 - 4 have no relevance. It may be that, so far as consideration of the sequence of theories in the developmental theory is concerned, there is little more to say, but there is every reason to believe that the mechanism, M of Condition 5 would repay a full and extensive study. I shall discuss only some of the problems it raises and then only in the barest outline.

We can assume that the child is equipped with a single mechanism for learning in a particular domain of language and we can construe this mechanism as a function which accepts theories and data as arguments and has theories as values. So, if we have a sequence of theories in D, (T_1, T_2, \dots, T_n) , our task is to produce a mechanism M such that:

$$M (T_1, D_1) = T_2$$

$$M (T_2, D_2) = T_3$$

.....

.....

$$M (T_{n-1}, D_{n-1}) = T_n$$

where T_1 is the child's initial 'uninformed' theory and D_1, D_2, \dots, D_{n-1} are the relevant data to which the child is exposed and which he uses between t_1 and t_2 , between t_2 and t_3 , ... and between t_{n-1} and t_n , respectively.

The notion of 'relevant data' in this formulation leads to the major research problem of the exact nature of this data in any domain of language development. One could cite the early work of Brown and Bellugi (1964) on the role of parental expansions in the acquisition of syntax and of Brown and Hanlon (1970) on correction for truth-value rather than syntactic form as preliminary attempts to investigate this problem. An area which is receiving intensive investigation at the moment concerns the general characteristics of the mother's speech to the child (see, particularly, papers in Snow and Ferguson (1977)) and I might also point to recent work on the role of imitation by children in language learning (see, for example, Bloom, Lightbown and Hood (1974)) although this is of somewhat ambivalent status as between data and mechanism.

Taking the discussion one step further, there is no need

to assume that the child is equipped with a constant mechanism for developing theories in D . A priori, equally plausible is the view that the child's mechanisms themselves develop such that, for the sequence of theories, (T_1, T_2, \dots, T_n) , we have a sequence of mechanisms, (M_1, M_2, \dots, M_r) such that:

$$\begin{array}{l}
 M_1(T_1, D_1) = T_2 \\
 M_1(T_2, D_2) = T_3 \\
 \dots\dots\dots \\
 \dots\dots\dots \\
 M_1(T_i, D_i) = T_{i+1}
 \end{array}
 \left. \vphantom{\begin{array}{l} M_1(T_1, D_1) = T_2 \\ M_1(T_2, D_2) = T_3 \\ \dots\dots\dots \\ \dots\dots\dots \\ M_1(T_i, D_i) = T_{i+1} \end{array}} \right\} \text{the scope of } M_1$$

$$\begin{array}{l}
 M_2(T_{i+1}, D_{i+1}) = T_{i+2} \\
 \dots\dots\dots \\
 \dots\dots\dots
 \end{array}
 \left. \vphantom{\begin{array}{l} M_2(T_{i+1}, D_{i+1}) = T_{i+2} \\ \dots\dots\dots \\ \dots\dots\dots \end{array}} \right\} \text{the scope of } M_2$$

$$\begin{array}{l}
 M_r(T_{n-1}, D_{n-1}) = T_n
 \end{array}
 \left. \vphantom{M_r(T_{n-1}, D_{n-1}) = T_n} \right\} \text{the scope of } M_r$$

where D_1, D_2, \dots, D_{n-1} are the relevant data to which the child is exposed between t_1 and t_2 , t_2 and t_3, \dots, t_{n-1} and t_n respectively.

The speculative nature of a framework such as this is hardly in need of emphasis and, of course, the sequence, (M_1, M_2, \dots, M_r) will itself be open to investigation in exactly the terms I have been advocating here. That is, (M_1, M_2, \dots, M_r) can be seen as a developmental theory in D where D concerns the child's mechanisms for learning in D' .

Examples from previous pages which can be treated in these terms are not extensive but the operating principles of Slobin discussed in 4.4 constitute one obvious instance. Furthermore, this is an instance which would require the more complex version developed above. Slightly less obvious is the reference to the 'maximisation of cue validity' in the work of Rosch et al on the learning of semantic categories discussed in 2.5 and the maximisation of acoustic distinctness adopted by Jakobson and considered in 5.1. The parallels between these last two examples deserves remarking upon. It is interesting to note that an attempt to reduce a language learning mechanism to a more general cognitive facility does not fit easily into the scheme outlined in Chapter 4. Rather, we seem to need resort to the view that the language learning mechanism can be seen as a particular instance of a general mechanism (cf. the discussion of Slobin's views in 4.4).

The reader might at this point object that my formulation runs the risk of infinite regress as an additional component of the theory including the sequence (M_1, M_2, \dots, M_r) will be a meta-mechanism which will explain how the child moves from M_{i-1} to M_i ($1 \leq i \leq r$). There are two responses to this. The first is that at some point in the regress we might reach a non-developmental mechanism and no meta-mechanism would be necessary to explain its development. Thus someone who maintained that all learning

proceeded by inductive data-analytic techniques would not be faced with such a regress. The second possibility, and I am not clear that this is realistic, is to suggest that at some point in the regress we will come across a sequence the development of which can be viewed in entirely maturational terms with no reference to an interaction between mechanisms and data. The possibility of infinite regress appears sufficiently remote for me not to be too concerned by it at this stage.

Such formulation of general questions and general responses to them are, of course, quite programmatic but it is my belief that formulation in these terms can only facilitate future research even if this involves rejection of these formulations. I hope that the previous pages have gone some way towards convincing the reader that this is the case.

BIBLIOGRAPHY

- Adams, K.L. & Conklin, N.F. (1973). 'Towards a theory of natural classification'. In Papers from the Ninth Regional Meeting of the Chicago Linguistic Society. 1-10
- Antinucci, F. & Miller, R. (1976). 'How children talk about what happened.' Journal of Child Language, 3, 167-189.
- Antinucci, F. & Parisi, D. (1973). 'Early language acquisition: a model and some data'. In C.A. Ferguson & D.I. Slobin (eds)
- _____ (1975). 'Early semantic development in child language'. In E. & E.E. Lenneberg (eds)
- Apostel, L. (1972). 'Illocutionary forces and the logic of change'. Mind, 81, 208-224
- Atherton, M. & Schwartz, R. (1974). 'Linguistic innateness and its evidence'. Journal of Philosophy, 71, 155-168.
- Atkinson, R.M. (1974). 'Prerequisites for reference'. Paper presented to BAAL seminar on First Language Acquisition. University of Newcastle-upon-Tyne.
- _____ (1975). Review of Bowerman (1973). Journal of Linguistics, 11, 87-101.
- _____ (forthcoming). 'Prerequisites for reference'. In E.O. Keenan (ed.) Studies in developmental pragmatics New York: Academic Press
- Atkinson, R.M. & Griffiths, P.D. (1973). 'Here's here's there's here and there'. Edinburgh Working Papers in Linguistics, 2, 29-73.
- Austin, J.L. (1962). How to do things with words. Oxford: OUP.
- Bartlett, E.J. (1976). 'Sizing things up: the acquisition of the meaning of dimensional adjectives'. Journal of Child Language, 3, 208-219.
- Barton, D. (1976). 'Phonemic discrimination and the knowledge of words in children under 3 years'. Papers and Reports on Child Language Development (Stanford University), 11, 61-68.
- Bates, E. (1976). Language and context: the acquisition of pragmatics. New York: Academic Press.
- Bellugi, U. (1967). 'The development of negation'. Unpublished doctoral dissertation, Harvard University.
- _____ (1968). 'Linguistic mechanisms underlying child speech'. In H. Zale (ed.) Proceedings of the Conference on Language and Language Behaviour. New York: Appleton Century-Crofts.

- Bellugi, U. (1971). 'Simplification in children's language'. In R. Huxley & E. Ingram (eds.) Language acquisition: models and methods. London: Academic Press
- Bellugi-Klima, U. (1969) 'Language acquisition'. Unpublished ms.
- Bennett, J. (1976). Linguistic behaviour. Cambridge: Cambridge University Press.
- Berlin, B., Breedlove, D.E. & Raven, P.H. (1973). 'General principles of classification and nomenclature in folk biology'. American Anthropologist, 75, 214-242.
- Berlin, B. & Kay, P. (1969). Basic color terms. Berkeley: University of California Press.
- Berman, R.A. (1977). 'Natural phonological processes at the one word stage'. Lingua, 43, 1-21.
- Bernstein, B. (1971). Class, codes and control I: theoretical studies towards a sociology of language. London: Routledge & Kegan Paul.
- Bever, T.G. (1970). 'The cognitive basis of linguistic structures'. In J.R. Hayes (ed.)
- _____ (1974). 'The ascent of the specious or there's a lot we don't know about mirrors'. In D. Cohen (ed.) Explaining Linguistic Phenomena. Washington: Hemisphere Publishing Corp.
- _____ (1975). 'Functional explanations require independently motivated functional theories'. Papers from the Parasession on Functionalism. Chicago Linguistic Society, 580-609.
- Bierwisch, M. (1967). 'Some semantic universals of German adjectivals'. Foundations of Language, 3, 1-36.
- _____ (1970). 'Semantics'. In J. Lyons (ed.) New Horizons in Linguistics. Harmondsworth: Penguin.
- Bloom, L. (1968). 'Language development: form and function in emerging grammars'. Columbia University doctoral dissertation. University Micro-films.
- _____ (1970). Language development: form and function in emerging grammars. Cambridge, Mass: MIT Press.
- _____ (1973). One word at a time. The Hague: Mouton.
- Bloom, L., Lightbown, P. & Hood, L. (1974). 'Imitation in language development: if, when and why'. Cognitive Psychology, 6, 380-420.

- Botha, R. (1970). On the status of argumentation in generative phonology. The Hague: Mouton.
- _____ (1973). The justification of linguistic hypotheses. The Hague: Mouton.
- Bowerman, M. (1973). Early syntactic development: a cross linguistic study with special reference to Finnish. Cambridge: CUP.
- _____ (1975). 'The acquisition of word-meaning'. Paper presented at the Third International Child Language Symposium, SOAS, London, September 3rd - 5th.
- _____ (1978). 'The acquisition of word-meaning: an investigation into some current conflicts'. In C. Snow & N. Waterson (eds.) The development of communication. New York: Wiley.
- Braine, M.D.S. (1963). 'The ontogeny of English phrase structure: the first phase'. Language, 39, 1-13.
- _____ (1971). 'The acquisition of language in infant and child'. In C. Reed (ed.) The learning of language. New York: Appleton-Century-Crofts.
- Brown, R. (1958). Words and things. Glencoe, Ill.: Free Press.
- _____ (1970). Psycholinguistics. New York: Free Press.
- _____ (1973). A first language: the early stages. London: Allen & Unwin.
- Brown, R. & Bellugi, U. (1964). 'Three processes in the acquisition of syntax'. Harvard Educational Review, 34, 133-151.
- Brown, R. & Fraser, C. (1963). 'The acquisition of syntax'. In C. N. Cofer & B. S. Musgrave (eds.) Verbal behavior and Learning. New York: McGraw Hill.
- Brown, R. & Hanlon, C. (1970). 'Derivational complexity and order of acquisition in child speech'. In J. R. Hayes (ed.)
- Bruner, J. (1975a). 'The ontogenesis of speech-acts'. Journal of Child Language, 2, 1-19.
- _____ (1975b). 'From communication to language: a psychological perspective'. (ms.).
- Bruner, J. & Scaife, M. (1975). 'The capacity for joint visual attention in the infant'. Nature, 253, 265-266.
- Brush, L. R. (1976). 'Children's meaning of 'more''. Journal of Child Language, 3, 287-289.

- Bühler, K. (1934). Sprachtheorie. Jena: Fischer.
- Carter, A. (1975). 'The transformation of sensori-motor morphemes into words: a case study of the development of 'more' and 'mine'. Journal of Child Language, 2, 233-250.
- Chomsky, A.N. (1957). Syntactic structures. The Hague: Mouton
- _____ (1964). Current issues in linguistic theory. The Hague: Mouton.
- _____ (1965). Aspects of the theory of syntax. Cambridge, Mass: MIT Press
- _____ (1973). 'Conditions on transformations'. In S.R. Anderson & P. Kiparsky (eds.) A Festschrift for Morris Halle. New York: Holt, Rinehart & Winston.
- _____ (1974). 'Questions of form and interpretation'. Montreal Papers in Linguistics, 3.
- _____ (1976). Reflections on language. London: Fontana.
- Chomsky, A.N. & Halle, M. (1968). The sound pattern of English. New York: Harper & Row.
- Churma, D.G. (1975). 'Child language acquisition and the justification of linguistic hypotheses'. Papers from the eleventh regional meeting. Chicago Linguistic Society. 92-101
- Clark, E.V. (1970). 'How young children describe events in time'. In G.B. Flores D'Arcais & W.J.M. Levelt (eds.) Advances in Psycholinguistics. Amsterdam: North Holland.
- _____ (1971). 'On the acquisition of the meaning of 'before' and 'after''. Journal of Verbal Learning and Verbal Behavior, 10, 266-275.
- _____ (1972a). 'Some perceptual factors in the acquisition of locative terms by young children'. In Papers from the Eighth Regional Meeting of the Chicago Linguistic Society. 431-439.
- _____ (1972b). 'On the child's acquisition of antonyms in two semantic fields'. Journal of Verbal Learning and Verbal Behavior, 11, 750-758.
- _____ (1973a). 'What's in a word?'. In T.E. Moore (ed.).
- _____ (1973b). 'How children describe time and order'. In C.A. Ferguson & D.I. Slobin (eds.).
- _____ (1974). 'Some aspects of the conceptual basis for first language acquisition'. Papers and Reports on Child Language Development (Stanford University), 7, 23-51

- Clark, E.V. & Garnica, O. (1974). 'Is he coming or going? On the acquisition of deictic verbs'. Papers and Reports on Child Language Development (Stanford University), 7, 1-22.
- Clark, H.H. (1970). 'The primitive nature of children's relational concepts'. In J.R. Hayes (ed.).
- _____ (1973). 'Space, time, semantics and the child'. In T.E. Moore (ed.).
- Clark, H.H. & Clark, E.V. (1968). 'Semantic distinctions and memory for complex sentences'. Quarterly Journal of Experimental Psychology, 20, 129-138.
- _____ (1977). Psychology and Language. New York: Harcourt Brace Jovanovich.
- Clark, R. (1974). 'Performing without competence'. Journal of Child Language, 1, 1-10.
- Clowes, M. (1969). 'Pictorial relationships - a syntactic approach'. In B. Meltzer & D. Michie (eds.) Machine Intelligence, 4, Edinburgh: Edinburgh University Press.
- Collier, G.A. (1973). Review of B. Berlin & P. Kay (1969). Language, 49, 245-248.
- Collier, G.A. et al. (1976). 'Further evidence for universal color categories'. Language, 52, 884-890.
- Cooper, D. (1975). Knowledge of Language. Dorchester: Prism Press.
- Cromer, R. (1968). The development of temporal reference during the acquisition of language. Unpublished doctoral dissertation. Harvard University.
- _____ (1974). 'The development of language and cognition: the cognition hypothesis'. In B. Foss (ed.) New perspectives in child development. Penguin: Harmondsworth
- _____ (1976). 'Developmental strategies for language' In V. Hamilton & M.D. Vernon (eds.). The development of cognitive processes. London & New York: Academic Press.
- Daniloff, R. & Hammarberg, R.G. (1973). 'On defining co-articulation'. Journal of Phonetics, 1, 239-248.
- Derwing, B.L. (1973). Transformational grammar as a theory of language acquisition. London & New York: Cambridge University Press.
- de Valois, R. & Jacobs, G.H. (1968). 'Primate colour vision'. Science, 162, 533-540.

- Dodd, B. (1975). 'Children's understanding of their own phonological forms'. Quarterly Journal of Experimental Psychology, 27, 165-172.
- Donaldson, M. & Balfour, G. (1968). 'Less is more: a study of language comprehension in children'. British Journal of Psychology, 59, 461-472.
- Donaldson, M. & Wales, R.J. (1970). 'On the acquisition of some relational terms'. In J.R. Hayes (ed.).
- Dore, J. (1974). 'A pragmatic description of early language development'. Journal of Psycholinguistic Research, 3, 343-350.
- _____ (1975). 'Holophrases, speech acts and language universals'. Journal of Child Language, 2, 21-40.
- Dore, J., Miller, R.T., Franklin, M.B. & Ramer, A. (1976). 'Transitional phenomena in early language acquisition'. Journal of Child Language, 3, 13-28.
- Durbin, M. (1972). 'Basic terms off-colour'. Review of B. Berlin & P. Kay (1969). Semiotica, 6, 257-277.
- Edwards, M.L. (1974). 'Perception and production in child phonology: the testing of four hypotheses'. Journal of Child Language, 1, 205-219.
- Eilers, R.E., Oller, D.K. & Ellington, J. (1974). 'The acquisition of word meanings for dimensional adjectives: the long and short of it.' Journal of Child Language, 1, 195-204.
- Eimas, P.D., Siqueland, E.R., Jusczyk, P. & Vigorito, J. (1971). 'Speech perception in infants'. Science, 171, 303-306.
- Ervin-Tripp, S. (1977). 'From conversation to syntax'. Papers and Reports on Language Development (Stanford University), 13, K1-K21.
- Ferguson, C.A. (1976). 'Learning to pronounce: the earliest stages of phonological development in the child'. Papers and Reports on Child Language Development, 11, 1-27.
- Ferguson, C.A. & Farwell, C.B. (1975). 'Words and sounds in early language acquisition: English initial consonants in the first fifty words'. Language, 51, 419-439.
- Ferguson, C.A. & Garnica, O.K. (1975). 'Theories of phonological development'. In E. & E.E. Lenneberg (eds.).
- Ferguson, C.A. & Slobin, D.I. (eds.). (1973). Studies of Child Language Development. New York: Holt, Rinehart & Winston.

- Feyerabend, P.K. (1975). Against Method. London: New Left Books.
- Fillmore, C.J. (1968). 'The case for case'. In E. Bach & R.T. Harms (eds.) Universals in Linguistic Theory. New York: Holt, Rinehart & Winston.
- Flavell, J. (1963). The developmental psychology of Jean Piaget. Princeton, N.J.: Van Nostrand Co. Inc
- _____ (1971). 'Stage-related properties of cognitive development'. Cognitive Psychology, 2, 421-453.
- Flavell, J. & Wohlwill. (1969). 'Formal and functional aspects of cognitive development'. In D. Elkind & J. Flavell (eds.) Studies in cognitive development: essays in honor of Jean Piaget. New York: Oxford University Press.
- Fodor, J.A. (1968). Psychological explanation. New York: Random House.
- _____ (1976). The language of thought. New York: T.Y. Crowell & Co.
- Fodor, J.A. & Bever, T.G. (1965). 'The psychological reality of linguistic segments'. Journal of Verbal Learning and Verbal Behavior, 4, 414-420.
- Fodor, J.A., Bever, T. & Garrett, M. (1974). The psychology of language. New York: McGraw Hill.
- Foss, D.J. & Swinney, D.A. (1973). 'On the psychological reality of the phoneme: perception, identification and consciousness'. Journal of Verbal Learning and Verbal Behavior, 12, 246-257.
- Fraser, C., Bellugi, U & Brown, R. (1963). 'Control of grammar in imitation, comprehension and production'. Journal of Verbal Learning and Verbal Behavior, 2, 121-135.
- Fries, C.C. (1952). The structure of English: an introduction to the construction of English sentences. New York: Harcourt Brace & Co.
- Garnica, O.K. (1971). 'The development of the perception of phonemic differences in initial consonants in English speaking children'. Papers and Reports on Child Language Development (Stanford University), 3, 1-29.
- _____ (1973). 'The development of phonemic speech perception'. In T.E. Moore (ed.).
- Gentner, D. (1975). 'Evidence for the psychological reality of semantic components'. In D.A. Norman, D.E. Rumelhart & the LNR Research Group Explorations in Cognition. San Francisco: W.H. Freeman.

- Gibson, E.J. (1969). Principles of perceptual learning. New York: Appleton-Century-Crofts.
- Gopnik, A. (1978). 'The analysis of proto-demonstratives in the one-word period'. Paper presented to Child Language Seminar. University of York.
- Greenberg, J. (ed.) (1966). Universals of language. Cambridge, Mass.: MIT Press.
- Greenfield, P., Nelson, K.E. & Saltzman, E. (1972). 'The development of rule bound strategies for manipulating seriated cups: a parallel between action and grammar'. Cognitive Psychology, 3, 291-311.
- Greenfield, P. & Smith, J. (1976). The structure of communication in early language development. New York: Academic Press.
- Grégoire, A. (1937). L'apprentissage du langage. Paris: Libraire Droz.
- Grice, H.P. (1957). 'Meaning'. Philosophical Review, 66, 377-388.
- _____ (1968). 'Logic and conversation'. William James Lectures, Harvard University. ms.
- _____ (1975). 'Logic and conversation'. In P.Cole & J.L.Morgan (eds.) Speech acts: Syntax and Semantics Vol 3. New York: Academic Press.
- Grieve, R. & Hoogenraad, R. (1977). 'Using language if you don't have much'. In R.J.Wales & E.C.T.Walker (eds.) New approaches to mechanisms in language. Amsterdam: North Holland.
- Griffiths, P.D. (1974). 'That there deixis, I' Unpublished ms. University of Edinburgh.
- _____ (1976). The ontogenetic development of lexical reference. Unpublished doctoral dissertation. University of Edinburgh.
- _____ (forthcoming). 'Speech acts and early sentences'. In P.Fletcher & M.Garman (eds.) Studies in language acquisition. Cambridge: Cambridge University Press
- Griffiths, P.D., Atkinson, R.M. & Huxley, R. (1974). 'Project Report'. Journal of Child Language, 1, 157-158.
- Grosu, A. (1975). 'A plea for greater caution in proposing functional explanations in linguistics'. Papers from the Parasession on Functionalism. Chicago Linguistic Society, 170-208.

- Gruber, J.S. (1975). 'Performative-constative transition in child language'. Foundations of Language, 12, 513-527.
- Guillaume, R. (1927). 'Les débuts de la phrase dans le langage de l'enfant'. Journal de Psychologie, 24, 1-25.
- Halle, M. (1961). 'On the role of simplicity in linguistic description'. In R. Jakobson (ed.) Structure of language and its mathematical aspects. Proceedings of the twelfth symposium in applied mathematics. Providence, R.I.: American Mathematical Society.
- Halliday, M.A.K. (1967). 'Notes on transitivity and theme in English'. Journal of Linguistics, 2, 199-244.
- _____ (1970). 'Functional diversity in language as seen from a consideration of modality and mood in English'. Foundations of Language, 6, 322-361.
- _____ (1973). Explorations in the functions of language. London: Edward Arnold.
- _____ (1975). Learning how to mean: explorations in the development of language. London: Edward Arnold.
- Harris, D.B. (ed.) (1957). The concept of development. Minneapolis: University of Minnesota Press.
- Harris, Z. (1951). Methods in structural linguistics. Chicago: University of Chicago Press.
- Haviland, S.E. & Clark, E.V. (1974). '"This man's father is my father's son": a study of the acquisition of English kin terms'. Journal of Child Language, 1, 23-47.
- Hayes, J.R. (ed.) (1970). Cognition and the development of language. New York: Wiley.
- Heider, E.R. (1971). '"Focal" color areas and the development of color names'. Developmental Psychology, 4, 447-455.
- _____ (1972). 'Universals in color naming and memory'. Journal of Experimental Psychology, 93, 10-20.
- Heider, E.R. & Olivier, D. (1972). 'The structure of the color space in naming and memory for two languages'. Cognitive Psychology, 3, 337-354.
- Hickerson, N. (1971). Review of B. Berlin and P. Kay (1969). International Journal of American Linguistics, 37, 257-270.
- Holland, V.M. & Palermo, D.S. (1975). 'On learning 'less': language and cognitive development'. Child Development, 46, 437-443.

- Hubel, D.H. & Weisel, T.N. (1962). 'Receptive fields, binocular interaction and functional architecture in the cat's visual cortex'. Journal of Physiology, 160, 106-154.
- Hurford, J.R. (1977). 'The significance of linguistic generalisations'. Language, 53, 574-620.
- Huttenlocher, J. (1974). 'The origin of language comprehension'. In R. Solso, (ed.) Theories in Cognitive Psychology. Hillsdale N.J.: Erlbaum.
- Ingram, D. (1971a). 'Transitivity in child language'. Language, 47, 888-910.
- _____ (1971b). 'Phonological rules in young children'. Papers and Reports on Child Language Development (Stanford University), 2, 31-50
- Isard, S. (1975). 'Changing the context'. In E. Keenan (ed.) Formal semantics of natural languages. London & New York: CUP.
- Jacobs, R. & Rosenbaum, P.T. (1968). English transformational grammar. Waltham, Mass.: Blaisdell.
- Jakobson, R. (1968). Child language, aphasia and phonological universals. The Hague: Mouton.
- Jakobson, R., Fant, G. & Halle, M. (1952). Preliminaries to speech analysis. Cambridge, Mass.: MIT Press.
- Jakobson, R. & Halle, M. (1956). Fundamentals of language. The Hague, Mouton.
- Kaplan, E.L. (1969). The role of intonation in the acquisition of language. Unpublished doctoral dissertation, Cornell University.
- Karttunen, L. (1968). 'What do referential indices refer to?' Unpublished ms.
- Katz, J.J. & Postal, P. (1964). An integrated theory of linguistic descriptions. Cambridge, Mass: MIT Press.
- Kay, P. (1975). 'Synchronic variability and diachronic change in basic color terms'. Language in Society, 4, 257-270.
- Keenan, E.O. (1974). 'Conversational competence in children'. Journal of Child Language, 1, 163-183.
- _____ (1975). 'Evolving discourse - the next step'. Papers and Reports on Child Language Development (Stanford University), 10

- Keenan, E.O. & Schieffelin, B. (1976). 'Topic as discourse notion'. In C.Li (ed.) Subject and topic. New York: Academic Press.
- Keenan, E.O., Schieffelin, B. & Platt, M.L. (1976). 'Propositions across utterances and speakers'. Papers and Reports on Child Language Development (Stanford University), 12, 127-144.
- Kent, C. (1976). 'Anatomical and neurophysiological maturation of the speech mechanism: evidence from acoustic studies'. Journal of Speech and Hearing Research, 19, 421-447.
- Klima, E. (1964).. 'Negation in English'. In J.A.Fodor & J.J.Katz (eds.) The structure of language: readings in the philosophy of language. Englewood Cliffs, N.J.: Prentice Hall Inc.
- Klima, E. & Bellugi, U. (1966). 'Syntactic regularities in the speech of children'. In J.Lyons & R.J.Wales (eds.) Psycholinguistics Papers. Edinburgh: Edinburgh University Press.
- Kornfeld, J. (1971). 'Theoretical issues in child phonology'. Papers from the Seventh Regional Meeting. Chicago Linguistic Society, 454-468.
- Kornfeld, J. & Goehl, H. (1974). 'A new twist to an old observation: kids know more than they say'. Papers from the Parasession on Natural Phonology. Chicago Linguistic Society, 210-219.
- Kripke, S. (1972). 'Naming and necessity'. In G.Harman & D.Davidson (eds.) Semantics of Natural Language. Dordrecht: Reidel.
- Kuroda, S.-Y. (1972). 'The categorical and thethetic judgement: evidence from Japanese syntax'. Foundations of Language, 9, 153-185.
- Lakatos, I. (1970). 'Falsification and the methodology of scientific research programmes'. In I.Lakatos & A.E.Musgrave (eds.) Criticism and the growth of knowledge. London & New York : Cambridge University Press.
- Lakoff, G. (1972). 'Linguistics and natural logic'. In G.Harman & D.Davidson (eds.) Semantics of natural languages. Dordrecht: Reidel.
- Lakoff, G. & Ross, J.R. (1967). 'Is deep structure necessary?' Indiana University Linguistics Club.
- Lakoff, G. & Thompson, H. (1975). 'Dative questions in cognitive grammar'. Papers from the Parasession on Functionalism. Chicago Linguistic Society, 337-350.

- Lawrence, D.H. (1952). 'The transfer of a discrimination along a continuum'. Journal of Comparative and Physiological Psychology, 45, 511-516.
- Lenneberg, E. & Lenneberg, E.E. (eds.) Foundations of Language Development. New York: UNESCO.
- Leopold, W. (1939). Speech development of a bilingual child (Vol I) Evanston, Ill.: Northwestern University Press.
- _____ (1949). Speech development of a bilingual child (Vol III) Evanston, Ill.: Northwestern University Press.
- Lewis, M.M. (1936). Infant speech: A study of the beginnings of language. London: Routledge & Kegan Paul.
- Lord, C. (1974). 'Variations in the acquisition of negation' Papers and Reports on Child Language Development, (Stanford University) 8, 78-86.
- Lyons, J. (1966). Discussion of McNeill contribution. In J. Lyons & R.J. Wales (eds.) Psycholinguistics Papers Edinburgh: Edinburgh University Press.
- Lyons, J. (1968). Introduction to theoretical linguistics. London & New York: Cambridge University Press.
- _____ (1975). 'Deixis as the source of reference'. In E. Keenan (ed.) Formal semantics of natural language. London & New York: Cambridge University Press.
- _____ (1977a). 'Deixis and anaphora'. In T. Myers (ed.) The development of conversation and discourse. Edinburgh: Edinburgh University Press.
- _____ (1977b). Semantics I. London & New York: Cambridge University Press.
- Macken, M. (1975). 'The acquisition of intervocalic consonants in Mexican Spanish: a cross sectional study based on imitation data'. Papers and Reports on Child Language Development (Stanford University), 9, 29-42.
- MacNamara, J. (1972). 'The cognitive basis of language learning in infants'. Psychological Review, 79, 1-13.
- McNeill, D. (1966). 'Developmental psycholinguistics'. In F. Smith & G.A. Miller (eds.) The genesis of language: a psycholinguistic approach. Cambridge, Mass.: MIT Press.
- McNeill, D. & Lindig, K. (1973). 'The perceptual reality of phonemes, syllables, words and sentences'. Journal of Verbal Learning and Verbal Behavior, 12, 419-430.

- McNeill, D. & McNeill, N.B. (1970). 'What does a child mean when he say 'no'?' In E.Zale (ed.) Proceedings of the conference on language and language behavior. New York: Appleton-Century-Crofts.
- McNeill, N.B. (1972). 'Colour and colour terminology: review of B.Berlin & P.Kay 'Basic color terms''. Journal of Linguistics, 8, 21-34.
- Macrae, A. (1976). 'Movement and location in the acquisition of deictic verbs'. Journal of Child Language, 3, 191-204.
- Maratsos, M. (1973). 'Decrease in the understanding of the word 'big' in preschool children'. Child Development, 44, 747-752.
- _____ (1974). 'Children who get worse at understanding the passive: a replication of Bever'. Journal of Psycholinguistic Research, 3, 65-74.
- _____ (1976). The use of definite and indefinite reference in young children: an experimental study in semantic acquisition. Cambridge: Cambridge University Press
- Menn, L. (1973). 'On the origin and growth of phonological and syntactic rules'. Papers from the Ninth Regional Meeting. Chicago Linguistic Society, 378-385.
- Mikeš, M. (1967). 'Acquisition des catégories grammaticales dans le langage de l'enfant'. Enfance, 20, 289-298.
- Miller, W. & Ervin, S. (1964). 'The development of grammar in child language'. In U.Bellugi & R.Brown (eds.) The acquisition of language. Monographs of the Society for Research in Child Development, 29, 1, 9-34.
- Mischel, T. (ed.) (1971). Cognitive development and epistemology. New York: Academic Press.
- Moore, T.E. (ed.) (1973). Cognitive development and the acquisition of language. New York: Academic Press.
- Moskowitz, A. (1970). 'The two-year-old stage in the acquisition of English phonology'. Language, 46, 426-441.
- Mowrer, O.H. (1960). Learning theory and symbolic processes. New York: Wiley.
- Nagel, E. 'Determinism and development'. In D.B. Harris (ed.).
- Nelson, K. (1973a). Structure and strategy in learning to talk. Monograph for the Society for Research in Child Development, 38, (1-2, serial no. 149).

- Nelson, K. (1973b). 'Some evidence for the cognitive primacy of categorization and its functional basis'. Merrill-Palmer Quarterly of Behavior and Development, 19, 21-39.
- _____ (1974). 'Concept, word and sentence'. Psychological Review, 81, 267-285.
- Omar, M. (1970). Cited in Slobin (1973).
- Palermo, D.S. (1973). 'More about 'less': a study of language comprehension'. Journal of Verbal Learning and Verbal Behavior, 12, 211-221.
- _____ (1974). 'Still more about the comprehension of 'less''. Developmental Psychology, 10, 827-829.
- Palmer, F. (1965). A linguistic study of the English verb. London: Longmans.
- Parisi, D. & Antinucci, F. (1970). 'Lexical competence'. In G.B. Flores d'Arcais & W.J.M. Levelt (eds.) Advances in Psycholinguistics. Amsterdam: North Holland.
- Park, T-Z. (1970). 'The acquisition of German syntax'. Unpublished ms. University of Munster.
- Peters, F.S. & Ritchie, R.W. (1971). 'on restricting the base component of transformational grammars'. Information and Control, 18, 483-501.
- Piaget, J. & Inhelder, B. (1948). The child's conception of space. London: Routledge & Kegan Paul.
- Postal, P. (1964). Constituent structure: a study of contemporary models of syntactic description. The Hague: Mouton.
- _____ (1966). Review of A. Martinet 'Elements of general linguistics'. Foundations of Language, 2, 151-186.
- Press, M. (1974). 'Semantic features in lexical acquisition'. Papers and Reports on Child Language Development (Stanford University), 8, 129-141.
- Pulman, S.G. (1977). 'Formal grammar and language use - their interaction in the analysis of modals'. Unpublished doctoral dissertation. University of Essex.
- Putnam, H. (1970). 'Is semantics possible?'. In H. Kiefer & M. Munitz (eds.) Language, belief and metaphysics. New York: State University of New York Press.
- _____ (1975). 'The meaning of 'meaning''. In H. Putnam. Mind, language and reality. London and New York: Cambridge University Press.

- Pylyshyn, Z. (1973). 'The role of competence theories in cognitive psychology'. Journal of Psycholinguistic Research, 2, 21-50.
- Quine, W.V.O. (1960). Word and object. Cambridge, Mass.: MIT Press.
- Reich, P.A. (1976). 'The early acquisition of word meanings'. Journal of Child Language, 3, 117-123.
- Reynolds, P. (1972). Cited in Bruner (1975b).
- Ricciuti, H.N. (1963). 'Geometric form and detail as determinants of similarity judgements in young children'. In A basic research program on reading. Final report, Cooperative Research Project No. 639. US Office of Education. 1-48.
- Richards, M.M. (1976). 'Come and go reconsidered: children's use of deictic verbs in contrived situations'. Journal of Verbal Learning and Verbal Behavior, 15, 655-665.
- Rips, L.J., Shoben, E.J. & Smith, E.E. (1973). 'Semantic distance and the verification of semantic relations'. Journal of Verbal Learning and Verbal Behavior, 12, 1-20.
- Rips, L.J. (1975). 'Inductive judgements about natural categories'. Journal of Verbal Learning and Verbal Behavior, 14, 665-668.
- Romney, A.K. & d'Andrade, R.G. (1964). 'Cognitive aspects of English kin terms'. In A.K. Romney & R.G. d'Andrade (eds.) Transcultural Studies in cognition. American Anthropologist, 66, 146-170.
- Rosch, E.H. (1973a). 'Natural categories'. Cognitive Psychology, 4, 328-350.
- _____ (1973b). 'On the internal structure of perceptual and semantic categories'. In T.E. Moore (ed.).
- _____ (1975a). 'Cognitive representations of semantic categories'. Journal of Experimental Psychology: General, 104, 192-233.
- _____ (1975b) 'The nature of mental codes for colour categories'. Journal of Experimental Psychology: Human Perception and Performance, 1, 303-322.
- _____ (1975c) 'Cognitive reference points'. Cognitive Psychology, 7, 532-547.
- Rosch, E.H. & Mervis, J.B. (1975). 'Family resemblances: studies in the internal structure of categories'. Cognitive Psychology, 7, 573-605.

- Rosch, E.H., Mervis, C.B., Gray, W.D., Johnson, D.M. & Boyes-Braem, P. (1976). 'Basic objects in natural categories'. Cognitive Psychology, 8, 382-439.
- Ross, J. (1970). 'On declarative sentences'. In R.A. Jacobs & P.S. Rosenbaum (eds.). Readings in English transformational grammar. Waltham, Mass.: Ginn.
- Ryan, J. (1974). 'Early language development: towards a communicational analysis'. In M.P.M. Richards (ed.) The integration of a child into a social world. London and New York: Cambridge University Press.
- Sadock, J. (1974). Toward a linguistic theory of speech acts. New York: Seminar Press.
- Sag, I. (1976). 'A logical theory of verb phrase deletion' Papers from the twelfth regional meeting. Chicago, Linguistic Society, 533-547.
- Sampson, G. (1973). 'The irrelevance of transformational omnipotence'. Journal of Linguistics, 9, 299-302.
- _____ (1975). The form of language. London: Weidenfeld & Nicholson.
- Sauvageot, A. (1948). Esquisse de la langue finnoise. Paris: Klincksieck.
- Savin, H.B. & Bever, T.G. (1970). 'The non-perceptual reality of the phoneme'. Journal of Verbal Learning and Verbal Behavior, 9, 295-302.
- Schaerlaekens, A. (1973). Review of Bloom (1970). Cognition, 2/3, 371-376.
- Schlesinger, I.M. (1967). 'A note on the relationship between psychological and linguistic theories'. Foundations of Language, 3, 397-402.
- _____ (1971). 'Production of utterances and language acquisition'. In D.I. Slobin (ed.) The ontogenesis of grammar. New York: Academic Press.
- _____ (1974). 'Relational concepts underlying language'. In R. Schiefelbusch & L. Lloyd (eds.) Language perspectives - acquisition, retardation and intervention. Baltimore: University Park Press.
- _____ (1975). 'Grammatical development - the first steps'. In E. & E.E. Lenneberg. (eds.)
- Schwartz, R. (1969). 'On knowing^a grammar'. In S. Hook (ed.) Language and philosophy. New York: New York University Press.

- Schwartz, R.G. & Folger, M.K. (1977). 'Sensori-motor development and descriptions of child phonology: a preliminary view of phonological analysis for Stage I speech. Papers and Reports on Child Language Development (Stanford University), 13, 8-15
- Searle, J. (1969). Speech acts. Cambridge: CUP.
- _____ (1975a). 'A taxonomy of illocutionary acts'. In K.Gunderson (ed) Minnesota studies in the philosophy of language. Minneapolis: University of Minnesota Press.
- _____ (1975b). 'Linguistics and the philosophy of language'. In R.Bartsch & T.Vennemann (eds.) Linguistics and neighbouring disciplines. Amsterdam: North Holland
- Shatz, M. (1977). 'The relationship between cognitive processes and the development of communication skills' (ms.)
- Shipley, E.F., Smith, C.S. & Gleitman, L.R. (1969). 'A study of the acquisition of language: free responses to commands'. Language, 45, 322-342.
- Shvachkin, N.Kh. (1948). 'The development of phonemic speech perception in early childhood'. English translation in Ferguson & Slobin (eds.)
- Sinclair-de-Zwart, H. (1969). 'Developmental Psycholinguistics' In D.Elkind & J.Flavell (eds.) Studies in cognitive development: essays in honor of Jean Piaget. New York: Oxford University Press.
- Sinclair, H. (1971). 'Sensori-motor action patterns as a condition for the acquisition of syntax'. In R.Huxley & E.Ingram (eds.) Language acquisition: models and methods. New York: Academic Press.
- _____ (1975). 'The role of cognitive structures in language acquisition'. In E. & E.E.Lenneberg (eds.)
- Slobin, D.I. (1971). 'Developmental psycholinguistics'. In W.O.Dingwall (ed.) A survey of linguistic science. College Park, Md.: Linguistics Program, University of Maryland.
- _____ (1973). 'Cognitive prerequisites for the development of grammar'. In Ferguson & Slobin (eds.)
- Slobin, D.I. & Welsh, C.A. (1973). 'Elicited imitation as a research tool in developmental psycholinguistics'. In Ferguson & Slobin (eds.)
- Smith, N. (1973). The acquisition of phonology. London: CUP
- Snow, C. & Ferguson, C.A. (eds.) (1977). Talking to children. Cambridge: CUP.

- Sober, E. (1975). Simplicity. Oxford: Oxford University Press.
- Stampe, D. (1969). 'On the acquisition of phonetic representation'. Papers from the Fifth Regional Meeting, Chicago Linguistic Society, 443-454.
- Stanley, R. (1967). 'Redundancy rules in phonology'. Language, 43, 393-436.
- Stich, S.P. (1971). 'What every speaker knows'. Philosophical Review, 80, 480-496.
- _____ (1972). 'Grammar, psychology and indeterminacy'. Journal of Philosophy, 69, 799-818.
- Strohner, H. & Nelson, K.E. (1974). 'The young child's development of sentence comprehension: influence of event probability, nonverbal context, syntactic form and strategies'. Child Development, 45, 567-576.'
- Taylor, C. (1971). 'What is involved in a genetic psychology' In Mischel (ed.).
- Thomson, J. & Chapman, R.S. (1975). 'Who is daddy (revisited)? The status of two-year-olds' overextensions in production and comprehension'. Papers and Reports in Child Language Development (Stanford University), 10,
- Velten, H.V. (1943). 'The growth of phonemic and lexical patterns in infant language'. Language, 19, 281-292.
- Waterson, N. (1970). 'Some speech forms of an English child: a phonological study'. Transactions of the Philological Society, 1-24.
- _____ (1971). 'Child phonology: a prosodic view'. Journal of Linguistics, 7, 179-211.
- Watson, J.B. (1928). Behaviorism. New York: Norton.
- Williams, E.S. (1977). 'Discourse and logical form'. Linguistic Inquiry, 8, 101-139.
- Wittgenstein, L. (1953). Philosophical Investigations. Oxford: Blackwell.