

**LINGUISTIC META-THEORY: THE FORMAL AND EMPIRICAL  
CONDITIONS OF ACCEPTABILITY**

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**A Thesis Submitted for the Degree of PhD  
at the  
University of St. Andrews**

**1984**

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LINGUISTIC META-THEORY

THE FORMAL AND EMPIRICAL CONDITIONS OF

ACCEPTABILITY

OF LINGUISTIC THEORIES AND DESCRIPTIONS

BY

P. R. RASTALL



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LINGUISTIC META-THEORY - THE FORMAL AND EMPIRICAL  
CONDITIONS OF ACCEPTABILITY OF LINGUISTIC THEORIES  
AND DESCRIPTIONS.

By P. R. Rastall

Abstract

Most linguists acknowledge, explicitly or implicitly, the relevance of epistemological questions in linguistics but relatively few have given more than a cursory, ad hoc or incomplete consideration to them. The work of one of those few, Jan Mulder, forms the starting point for much of the present discussion. Epistemological considerations arise in many contexts in linguistics and in many guises. It is an epistemological matter whenever we test the adequacy of a description or the acceptability of a theory. Epistemological considerations are latent whenever we discuss the form or the content of linguistic theories and descriptions or their interrelations. The comparison of different approaches to linguistics inevitably raises epistemological questions concerning our approach to linguistics or our presuppositions about it. These questions are of a general nature and transcend questions about particular linguistic theories and descriptions. These epistemological questions force us to consider what we take linguistics to be. In considering

questions of the type mentioned we are forced, for example, to analyse what we mean by a "linguistic theory", a "linguistic description" and what phenomena we are aiming to understand. We are, furthermore, forced to analyse the constraints which a scientific attitude places upon linguistic theorising and description-building. It is these questions concerning the acceptability of linguistic theories and descriptions which we call linguistic meta-theory.

This thesis falls into five main parts. Firstly, in Chapter One, we consider the nature and scope of linguistic meta-theory. Secondly, in Chapter Two, we look at a number of previous approaches to the subject. Other important contributions are discussed as they arise in the text. Thirdly, in Chapters Three and Four, we consider in detail the major meta-theoretical distinctions in linguistics and their consequences. In particular, we distinguish linguistic theories from linguistic descriptions and discuss the nature of linguistic phenomena. The view is put forward that linguistics is a scientific subject. The meaning of this assertion is analysed and the interrelations of linguistic theories, descriptions and phenomena are considered in the light of this analysis. The main epistemological requirement that is put forward and defended is that of the empiricism of linguistics. Certain changes in our view of the philosophy of science and in our view of the form of linguistic theo-



ries and descriptions follow from the conjunction of these major meta-theoretical positions.

Fourthly, we consider the main meta-theoretical considerations concerning theories (Chapter Five) and reject a widespread view of linguistic theory as a non-empirical study (Chapter Six) and we consider the main meta-theoretical conditions relating to linguistic descriptions and some practical examples of description-building consonant with the general positions adopted in Chapter Seven. In Chapter Eight, we look at a concrete example of theory-building in the light of the meta-theoretical conditions of acceptability previously set up. We are especially concerned to show how a theory can meet the condition of being "applicable" or "indirectly scientific" through the establishment of acceptable empirical descriptions consonant with the meta-theoretical conditions on descriptions considered earlier.

The view that linguistics is a science implies that we must be concerned with the empirical testing of descriptions and, so, the fifth part of the work is devoted to methodology. In Chapter Nine, we defend the role and necessity of methodology in linguistics, and set up the logical framework of relations between the methodology and theory descriptions and phenomena.



In Chapter Ten, we examine two of the known types of empirical testing and their shortcomings. Finally, in Chapter Eleven, we give an example of the successful and correct application of a methodology in order to bring out the nature of empirical testing and to demonstrate its feasibility within a scientific linguistics of the sort we imagine.

## ACKNOWLEDGEMENTS

I would like to express my sincere thanks to my supervisors at St. Andrews University Dr. S. G. J. Hervey, Dr. D. Roberts and, especially, Professor J. W. F. Mulder for their patient encouragement and constructive criticism throughout the long preparation of this work. My thanks also go to Dr. A. M. Spector of Sunderland Polytechnic for his encouragement and Mrs C. Steadman of Specialist Language Services, York, Ltd. for her material help in the later stages of the work. I am grateful to the University of St. Andrews for the Research Scholarship which funded the early years of the research. Finally, I am greatly indebted to those scholars who have studied Linguistic Metatheory before me and from whose ideas I have profitted, notably Karl Bühler, Louis Hjelmslev, Sebastian Shaumjan and Jan Mulder.

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CHAPTER ONE

PRELIMINARIES TO LINGUISTIC META-THEORY

a. The function of linguistic theories and descriptions

In any subject or thesis there will probably be found one or more "fundamental questions" which it is the aim of the study to investigate. The value of any study will be largely determined, therefore, by the exactness and usefulness of the formulation of these questions. In linguistics the fundamental questions have often been formulated as "what is language?", "what is communication?" or some such. These questions will lead nowhere, if "language" or "communication" are considered as given objects of investigation or if they remain no more than intuitive concepts to be rendered comprehensible by "explicating" them. Rather, one should be concerned to develop a conception of, say, language as a consequence of a self-contained theory. One can then test the applicability of the theory and of the theory of language contained therein. We explicitly assert that language is a conceptual model which we build; it is neither an intuitive concept to "explicate" nor a pre-existent thing to investigate.

Chomsky; by contrast, has maintained that, in setting up a grammar of English, "we assume intuitive knowledge of the grammatical sentences of English"<sup>1\*</sup> and that "we thus face a familiar task of explication

\*The footnotes are placed at the end of each chapter.

of some intuitive concept - in this case the concept "grammatical in English" and, more generally, the concept "grammatical"<sup>1</sup>. It would seem, on this showing, that linguistics, as Chomsky sees it, is concerned with the "explication" of certain familiar and intuitive concepts. However, it is simply a non-sequitur to suggest that the assumption of an intuitive knowledge of the set of grammatical sentences of a language (if, indeed, this can be assumed) implies that one is aiming to explicate an intuitive concept. Of course, one might wish to attempt some explanation of the linguistic intuitions which speakers seem to possess, but it does not follow from the fact that one uses certain data drawn from intuition that one is necessarily concerned with the intuitions involved, rather than, say, the development of a grammatical model of the sentences about which intuitions are held. There is no reason to assume that the development of such a model would be identical with the development of the explication of speakers' intuitions. In fact, the unlikelihood of this identity places the burden of proof onto those who claim that a linguistic model is both a linguistic and a psychological model. Nor is there any reason to assume that the existence of grammatical intuitions implies the existence of some concept of "the grammatical" other than that which is created by the linguist to explain not intuitions about sentences but the sentences themselves. That is, there is no necessary



correlation between a grammatical model and linguistic intuitions.

From my point of view the explication of "familiar concepts" is of no interest to any scientific subject, i.e., in general, one concerned with the acquisition of knowledge about the world that may be shared with others and which is empirically testable in confrontation with intersubjectively agreed phenomena. Although it may have a place in conceptual analysis, the approach apparently advocated by Chomsky has no place in the epistemology of the sciences (including linguistics), as I see it.<sup>2</sup>

Generally speaking, we may assert that science recognises an explanandum only when there is general consent about the existence and certain properties of (some set of) observables. One can only observe that "intuitive concepts" scarcely constitute observables and that, although intuitive reports about sentences may serve as data for something, there is no reason to think that they serve as data about familiar concepts of the grammatical. In fact, there is nothing to suggest that the explication of such a concept would be of value to linguistics. (by contrast with the explication of a theory of grammar, i.e. the establishment of a concept of grammar, which would be of value).



The exploration of familiar concepts is then no part of linguistics as I conceive it, although it may have a place in certain branches of psychology or philosophy. The situation is even more confused when we approach the "familiar concept" of language. The existence of familiar concepts of language or "the grammatical" is as irrelevant and as harmful to linguistics as the existence of certain familiar prejudices about language and grammar. (In fact, it is sometimes difficult, to say the least, to tell the familiar concepts from the familiar prejudices - e.g. in the contention that each "sentence" must be analysed in terms of a "subject" and a "predicate".) Unless one considers it to be the task of linguistics to explore "familiar concepts", there is no reason to think that some familiar concept of, say, language has anything to do with linguistics at all. One may as well maintain that the familiar concept of Heat must be treated as one of the explananda of physics. The mere existence of familiar concepts does not imply either their validity or their usefulness. The explication of familiar concepts is often nothing more than the gross hypostatization of the intuitive, for nothing is easier than the establishment of one's intuitions as reflections of reality.

Slightly different is the contention of Shaumjan that,

"Although the definitions of concepts are in them-

selves arbitrary, we can regard any definition of a concept as a statement which possesses an explanatory function. In this case, the the definition of the concept answers the question, "what is the nature of x ?".....since the definitions of concepts which are based on the formula, "what is the nature of x ?", require on the part of the scholar a deep penetration into some sphere of reality and are at the same time formulated not in the form of single, isolated statements but in the form of an entire system of statements, such definitions can be called "theories" as well."<sup>3</sup>

Shaumjan goes on to take as an example the definition of the phoneme. In the case of the phoneme, the theoretician is required, according to Shaumjan, to find "which definition of the phoneme based on the formula, "what is the nature of x ?", i.e. which phoneme theory, reflects most closely the linguistic reality."<sup>4</sup> It is clear that this position is somewhat confused. One would agree that, if the definitions of concepts within a theory are arbitrary, then the stated definitions constitute explanations of the meaning or use of the terms ("concepts") in question. In this sense only can one speak meaningfully in the sciences, of "explicating a concept", i.e. by defining it (and by deriving the consequences of defining it in the way chosen). However, it is not at all clear that such definitions do answer the question, "what is the



nature of x ?". Alternatively, if the definitions of concepts do answer questions of this form, it is not at all clear why one should call the definitions "arbitrary", since in such a case one would be defining objects and not concepts.

Still more controversial is the claim that the theoretician must find that definition of a concept which most closely resembles "linguistic reality". In the first place, one should point out that, if the definition of a concept, e.g. the definition of the concept "phoneme", does give an arbitrary answer to the question, "what is the nature of x ?", then the definition in question supplies us with an explanation of the nature of the concept (as it is used in the theory) ; it cannot be said to constitute a definition of some external reality, an object, unless, of course, it is maintained that the concept in question, e.g. "phoneme", corresponds to, or is the concept of, some external reality. If this last condition were met, one could indeed require the linguist to find the "best definition" by means of some "deep penetration" into "linguistic reality". The trouble is, however, that there is no reason to think that this "linguistic reality" exists outside the linguist's own skull ; that is, the phoneme and other theoretical objects of linguistics belong to our conceptual world.

One should be careful to note that our argument does not imply that no linguistic reality exists. Our argument implies that, if the linguist wishes to "penetrate" linguistic reality, he must first show us what this reality is. This a linguist cannot do by retreating into artificial systems of definitions.

The difficulty with Shaumjan's position is that much more evident when we remember that it is precisely with theoretical terms that he is concerned. Whilst the ontological status of particular phonemes might be a matter of dispute, it would seem beyond serious question that the intension of the class of phonemes ("the phoneme concept") is a theoretical abstraction.<sup>5</sup> That is to say that the search for the nature of some external reality by means of the definition of a theoretical concept is an inherently pointless exercise, unless one has reason to believe in the existence of the "reality" in question as an entity outside one's theoretical conception. The reason why such a course would be pointless is that, unless the above condition is met, one is not defining a reality at all and, hence, one is not supplying a definition of the nature of x, because there is no x, the nature of which can be defined. The step of giving reasons for a belief in the existence of "the phoneme" outside the theory is precisely what is missing from Shaumjan's account. The confusion is



particularly clear in Shaumjan's case because he explicitly asserts that the definitions of theoretical concepts are arbitrary. Since a theoretical concept (i.e. presumably a name of a concept for Shaumjan) stands in a relation of equivalence to its definition (a definition of x must state the necessary and sufficient conditions of x), it follows that the theoretical concept is arbitrary also. If this is so, then it is self-contradictory to maintain that the concept in question is the concept of a linguistic reality, since this would imply that the concept in question was both arbitrarily selected as a theoretical function appropriate for a certain descriptive task and that it was not arbitrary, since the concept reflects linguistic reality.

If the above condition were met and a given theoretical concept were to correspond to a given linguistic reality, it would be entirely clear that the definition of the concept could be achieved through a study of a given set of phenomena ; it would be difficult to see under such circumstances how the definition could be arbitrary. In any case, it is dubious whether such a case could occur, without gross hypostatization, when we take into consideration the nature of theoretical concepts as inherently abstract entities (intensions of classes, types of relations, etc.). The main point is that the explanation of a theoretical concept is not the

same thing as the explanation of a reality, except insofar as a concept can itself be considered as a reality worthy of scientific investigation. Rather, the theoretical concept may be applied in the explanation of some sphere of observable reality. That is to say that a scientific linguistics should distinguish between its conceptual apparatus, in terms of which one attempts to comprehend phenomena, and the phenomena themselves.

It would be a mistake to think that the conceptual apparatus embodied in a theory gave us an account of the nature of empirical objects, such as the "phoneme" or the "sign" or of empirical relations, e.g. "is in construction with", etc., existing independently of the theory. The phenomena of speech may be modelled in terms of such objects and relations. Claims about the, e.g. psychological, existence of particular objects or relations in particular languages (e.g. /b/, the sign "weather", the constructional relation "prenominal - noun" in "hatstand", all in English) require additional, independent and different (i.e. psychological) support. However, even if such support were forthcoming, it would not allow us to say that theoretical objects and relations correspond to empirical realities, although one could say that such theoretical objects and relations were the intensions of (sets of)



real objects and relations. As we shall see later, it is important to maintain a distinction between theories and the applications or instantiations of theories in the description of particular sets of phenomena.

Related to this distinction is another concerning the phenomena. The phenomena with which the linguistic sciences are principally concerned, i.e. the phenomena of speech communication,<sup>6</sup> must be capable of being considered both as entities existing in space and time independently of any theoretical apparatus and as independently existing entities capable of satisfying the arbitrary functions of some theory and in their capacity of satisfying those functions. That is, we distinguish phenomena per se from phenomena as modelled. In other words, we distinguish simple, relatively unclassified observables (e.g. the sum total of all phonetically recordable speech events in English) from observables classified with respect to a given theoretical function (e.g. the sets of phonetic events which are classified as exponents of phonemes, distinctive features, phonotagms, etc.). Subclasses of observables, when classified, constitute the empirical interpretations of descriptive objects, such as the phoneme /b/ or the distinctive feature /labial/ in English, provided that the observables in question meet the conditions of the theoretical functions ("is a phoneme", "is a distinctive feature", etc.) concerned. The fact is that descriptive objects such as /b/ and /labial/ in English have large doses of arbitrary theory in them and the

fact that descriptive objects, such as /b/ and /labial/, may be assigned an empirical interpretation or may correspond to sets of phenomena as modelled does not imply the existence of any real object outside the classification. This is why independent evidence would be required to demonstrate the existence of the psychological reality of phonemes (or other objects). One would need to demonstrate the existence of psychological correlates for linguistic descriptive objects. Linguistic evidence alone is not sufficient to demonstrate the psychological reality of linguistic objects. It should now be clear that the production of such evidence would not affect the status of theoretical objects such as the "phoneme", which remain arbitrarily selected theoretical functions with descriptive (classificatory) application.

The distinction between phenomena per se and phenomena as modelled (see below for further discussion) is introduced in order that, in the first place, linguistics be not vacuous, i.e. it must deal with existing intersubjectively agreed phenomena and, in the second place, in order that the subclass of phenomena relevant to linguistics may be distinguished from the class of all phenomena. In the third place, clearly, empirical models must be empirically interpretable. It is important that linguistic theories be applicable in the description of



phenomena which are linguistically describable. That is, the set of phenomena per se relevant to linguistics should be independently chosen and should not be confused with that set of phenomena which happens to satisfy or be modelled by some particular theory. This is a question of determining the scope of relevance of theories of linguistic communication<sup>7</sup> on the one hand and of determining what sort of facts any linguistic theory might be expected to account for on the other hand. Each theory determines its own scope of relevance. In "functionalist" approaches to linguistics, for instance, the criteria of relevance are the "functional principle"<sup>8</sup> and the "double articulation" doctrine. However, one should not be misled into thinking that a theory can afford to ignore phenomena which fall outside its determined scope. Functionalist theories have been extended to cope with communicational phenomena falling outside the double articulation and the functional principle. Notably, it has been necessary to provide components of functionalist theory concerned with prosodic phenomena, facts of variance and semantic phenomena. This would be a case where agreement over sets of phenomena per se has led to extensions and revisions of an existing theory - that is, changes in the scope of the theory. Modern functionalist theories are now capable of coping with, at least, phenomena of variance and prosody, i.e.

the set of phenomena capable of being modelled has increased. In fact, changes in the theory were necessary in order to deal with, for example, variance. As is fairly well known, it is not possible to regard the second articulation as operating directly on the signifiants of signs, if the signifiant is itself a class of allomorphs. This leads to a more radical distinction between the first and second articulations.

The point of this discussion is, then, that there may be a conflict between the scope of relevance which a theory determines for itself and the set of phenomena which the theory might be expected to account for. In any such conflict the theory-determined scope of relevance must always be the loser, if there is sufficient evidence that (a) the theory should be capable of describing the phenomena in question and (b) that the theory currently cannot describe those phenomena. One such area confronting many varieties of functionalist linguistics is the question of the factors involved in relating observed sentences in texts to the grammatical analogues of sentences set up in the grammar. Sentences in English such as, for example,

- (1) They never leave, do they ?
- (2) You are, aren't you ?
- (3) You want to marry my daughter, do you ?
- (4) John won't, won't he ?

are all well attested and are incapable of being related



to many known grammatical models in any obvious way. The factors determining the form and usage of such sentences are as yet unexplained in many functionalist accounts (and other treatments) because there is no theoretically justified way of accommodating the observed features of these sentences while relating them to grammatical models. The outstanding features are the following. Firstly, the sentences fall into two categories of question from the point of view of usage : those where the speaker seeks to confirm or to gain acceptance of his own supposition (1 and 2) and those where the speaker (rhetorically) seeks confirmation of new information from a third party or his interlocutor (3 and 4). In the first case (1 and 2) the occurrence of a negative "tag" clause depends on the occurrence of a positive main clause and, conversely, the occurrence of a positive "tag" clause depends on the occurrence of a negative main clause. In the second case (3 and 4) both the main clause and the tag are positive or both are negative. The second outstanding feature is that in both of the categories considered the auxiliary verb of the tag (whether a "do" pro-verb, part of an aspectual verb or a modal) must be identical with the first verb (main or auxiliary) of the main clause. There must also be identity of subject between the two clauses (the tag clause subject being normally

a pronoun). These remarks hold true both when the main clause is non-elliptical (1 and 3) and when it is elliptical (2 and 4).

It is dubious whether many approaches can handle these facts. The question of the usage of this type of sentence is not a purely grammatical matter. Nor is it a purely semantic matter. Bühler would have said the difference in usage depended upon the communicative role of the speaker. Sentences 1 and 2 would be used by the speaker when his role was predominantly one of appeal to the speaker („Appellfunktion“) and 3 and 4 would be used when the expressive function was dominant („Ausdrucksfunktion“). However that may be, most approaches have no theoretically justified means of explaining the usage of these sentences.

The question of the form of the tag clause is, however, a purely grammatical matter. Since most approaches do not have a theoretically justified mechanism for determining the first verb in a construction, it is dubious whether the above grammatical observations can be handled either. One must remember also that there is usually no theoretically justified way of handling grammatical relations between such constructionally distant objects as subordinate auxiliaries and auxiliaries or pro-verbs in different clauses or the



subjects of different clauses. Obviously, any rigidly or uniquely immediate-constituent approach could not handle these data but it is also probable that most dependency approaches would have difficulty as well.

Even in a dependency approach there would be serious difficulties involved in setting up dependency relations (presumably occurrence dependency relations) between objects that are often very loosely grammatically related as when a modal or aspectual verb in a tag clause is occurrence dependent on a subordinate modal or aspectual verb in a main clause. Even worse would be the case where the occurrence of a negative "not" is occurrence dependent on the non-occurrence of a subordinate negative in the main clause. Incidentally, the negation may not even come in the nucleus of the main clause, as in, for example,

(5) They will give you no credit at that bank, will they ?

Here the occurrence of "no", which is a subordinate element of the direct object (itself subordinate to the verbal nucleus), excludes the possibility of a negative tag.

Here is not the place to go into all the details of this problem or to offer a solution in the case of a particular linguistic theory. Suffice it to say that even a number of dependency approaches would need modification in order to handle the sort of phenomena

we have been discussing. Any such modification, probably involving the notion of dependency and the scope of dependency relations, would arise as a result of an effort to enrich a theory which was previously too poor to accommodate the relevant facts. The main point here is that in such a case as this the scope of the theory must be enlarged because of a pressing need to accommodate bona fide observations about speech phenomena. The established scope of the theory cannot lead us to ignore or exclude speech phenomena.

However this may be, no theory can be said to have any scope at all, unless there exist explicit means of modelling phenomena using the functions of the theory. That is, we can never arrive at phenomena as modelled, unless the functions of the theory are explicit and capable of being satisfied by the phenomena under consideration. One way of achieving this is to stipulate that all linguistic descriptions constructed using a certain theory be set up in such a way that all statements within them be related directly or ultimately to classes of "protocol statements"<sup>9</sup>. Such "protocol statements" simply record the occurrence of particular events from a particular point of view. For instance, the occurrence of a speech event may be recorded and given an initial phonetic classification, e.g. "[p]<sup>i</sup> occurred in speech phenomena



S at time T in place P". Protocol statements connect a description to observed events and provide a way of giving a description an empirical interpretation. Such a protocol of a speech phenomenon is the record of an independently existing event in its capacity of satisfying some arbitrarily selected function of the theory so long as the theory in question contains the notion "phonetic form" as a means of classifying speech events by means of phonetic description<sup>10</sup> (this is the force of the square brackets). Protocol statements are, of course, a highly idealised way of establishing intersubjective agreement about phenomena, although a transcript of a field recording is in fact just such a (set of) protocol statements.

It is highly important to note the "theory-laden" nature of the apparently simple notion of phenomena ; without the connection with some appropriate theoretical notion in a protocol (or otherwise implicit), it would be impossible to show that the recorded event was in any way relevant to any linguistic description or theory. The notion of phenomena as modelled is clearly heavily theory-laden. But it is also true that what we recognise as independently existing phenomena per se is not established without theoretical considerations. Firstly, phenomena per se must be relevant to the investigation at hand (ultimately to some theory) and, secondly, they must be recordable or quantifiable (in a way which



allows them to be connected to theoretical considerations. (Often, incidentally, there is great difficulty in finding genuinely independent phenomena in support of a linguistic argument constructed with reference to data.)<sup>11</sup>

From the present point of view one can think of the function of a linguistic theory as being to enable the linguist to establish conceptual models which will account for independent, empirical phenomena. That is, one may view linguistic theory as a self-contained analytical basis which, in Hjelmslevian terms, "a pour but d'élaborer un procédé au moyen duquel on puisse décrire....des objets donnés d'une nature supposée".<sup>12</sup> Such an analytical basis will necessarily adopt a particular point of view<sup>13</sup> with regard to the description of phenomena through its "rigid selection of functions as necessary and sufficient for unambiguous description",<sup>14</sup> as Uldall puts it. Self-evidently, a homogeneous theory of the sort described will be developed in response to some "fundamental question". In order to avoid the difficulties outlined above, in linguistics/semiotics this question may be formulated as follows. "What are the both sufficient and necessary conditions for communicational systems ?".<sup>15</sup> One should note that, if this formulation is accepted, then linguistics would fall within the wider study of semiotics, as was foreseen by Saussure, when he says, "la tâche du linguiste est

de définir ce qui fait de la langue un système spécial dans l'ensemble des faits sémiologiques".<sup>16</sup> Axiomatic Functionalism and Glossematics offer solutions to this problem. On the Hjelmslevian view, which we shall adopt here, Shaumjan (above), in attempting to set up an arbitrary but appropriate linguistic theory with an "existence postulate", is merely trying to have his theoretical cake and eat it, i.e. have a "glossematic" view of linguistic theory and yet maintain the position, impossible under the glossematic approach, that linguistic theory and the functions which it contains correspond to some supposed linguistic reality.

The question which we have suggested as the starting point for the development of a linguistic theory is prior to the question, "how is communication achieved in language, L?". If L is a communication system, then it must satisfy the conditions proposed in answer to the above ("fundamental") question. The development of a general theory of communication systems is, therefore, logically prior to linguistic description, i.e. the description of a set of conventions as necessary for communication in some particular L (a description of how communication is achieved in L). On the other hand, any proposed theoretical construction will be adequate, only



if the proposed theory allows us to answer the question, "how is communication achieved in L or any other communication system?". That is to say that the theory will not be accepted unless applicable in linguistic description, i.e. the adequate description of particular semiotic systems as a necessary condition of the acceptability of a theory containing proposed conditions as necessary and sufficient for communication. The possibility of the adequate description of any particular semiotic system is, however, a both sufficient and necessary condition of the applicability of such a theory.<sup>17</sup> (We consider the conditions under which a description will be considered adequate below). It follows that the acceptability of a linguistic theory implies the applicability of that theory in the construction of linguistic descriptions. I.e. if the acceptability of a theory, T, implies that the adequate descriptions, D<sup>1</sup>, . . . . ., D<sup>n</sup>, may be constructed in accordance with that theory and the adequacy of D<sup>1</sup>, . . . , D<sup>n</sup> is equivalent to the applicability of T (the adequacy of D<sup>1</sup>, . . . . ., D<sup>n</sup> is equivalent to showing that T is suited to its purpose), then the acceptability of T implies the applicability of T (applicability is a necessary condition of the acceptability of a theory.) As we shall see the acceptability of a theory involves many other factors as well. In this sense linguistic theory



entails the potential for adequate linguistic description ; the development of a linguistic theory as an analytical basis implies the possibility of the adequate description of particular communicational phenomena by making use of that theory (whereas the existence of a linguistic description implies the existence of some theory of linguistic structure).

It is in this way that the theoretical and empirical interests of linguistics are, in the first place, bound together, viz. through the notions of acceptability and applicability. Linguistic meta-theory, as it is understood here, is mainly concerned with the investigation of what it is for linguistic theories and descriptions to be acceptable and for linguistic theories to be applicable. The importance of linguistic meta-theory is then that it raises such questions as the nature and structure of linguistic theories and descriptions, their mutual relations, the nature of "adequacy" and empiricism in linguistics, the relations of theories and descriptions to phenomena, their place in the conceptual world and the relation of linguistics to the philosophy of science.

One should note that we speak here of "acceptability" and not of "validity" in the context of an arbitrary theory which is introduced in order to allow the adequate and appropriate description of phenomena. We could never speak of the "truth" or "validity" (other

than in a purely formal sense) of such a system, because it is arbitrarily introduced. We can find only necessary conditions for the acceptability of a theory as a useful tool in description and, hence, as a theory of language ; we could never find a sufficient condition of the validity of the theory. From this point of view there is a serious lacuna in Hjelmslev's exposition of the epistemological foundations of linguistics. He writes, "il semble légitime en tous cas de poser a priori l'hypothèse qu'à tout processus répond un système qui permette de l'analyser et de le décrire au moyen d'un nombre restreint de prémisses". Additionally, he writes, "le but de la théorie du langage est de vérifier la thèse de l'existence d'un système sous-jacent au processus, et celle d'une constance qui sous-tende les fluctuations et d'appliquer ce système à un objet qui semble tout particulièrement s'y prêter".<sup>18</sup>

There is some confusion here over whether Hjelmslev is speaking of a theory to be applied in analysis or of a particular application of a theory in describing some set of speech phenomena or both. However, the main point is that Hjelmslev seems to be making it a premise (a priori) of linguistic theory that some systematic constancy underlies the process of speech. Of course, any approach to linguistics presupposes that linguistic phenomena may be accounted for by means of rational models (regularities or constancies). However, Hjelmslev is suggesting also that this premise ("hypothesis")



is subject to empirical test, i.e. that, for at least one premise of linguistic theory, there is a test of empirical validity. This is directly in opposition to the view we are adopting here.

The fact that no sane man would doubt that speech is organised in some way does not imply that the premise of a linguistic constancy can be empirically tested or that it can be anything more than an assumption underlying our attempts to give a rational account of speech communication. Similarly, the premise that "the shortest distance between two points is a straight line" remains an assumption. It could not be proved without circularity, because it is a definition. So also, in the case of Hjelmslev's hypothesis, one could not prove the existence of a system underlying the process of speech from the fact that it is possible to establish such a system, since the establishment of the system already presupposes the premise that for each process there is an underlying system. Furthermore, Hjelmslev's view leads to paradoxes. Firstly, Hjelmslev maintains that linguistic theory contains no "existence postulate".<sup>19</sup> If this view is correct, then Hjelmslev cannot maintain that it is possible to "vérifier l'existence d'un système sous-jacent au processus", since no objective existence is claimed for the said system. Alternatively, if one can always set up a priori the hypothesis that such a system exists, then it is false to say that the theory contains no existence postulate. However, if Hjelmslev's

<sup>19</sup> "The theory can be advanced a priori, then no evidence



of any sort could ever, even in principle, be capable of refuting it. On the other hand, to suggest that one can test for the existence of some postulated system implies that the hypothesis might be false. This is paradoxical - we must have either an irrefutable hypothesis or a refutable a priori. It seems clear that the assumption of a system underlying the process of speech is intended to be accepted as true by definition and not by empirical test. The difficulty stems directly from the mistake of viewing a premise of the theory as some sort of empirical hypothesis. The moment we drop the idea that theoretical statements are or can be hypotheses, the paradoxes disappear. However, then we could say only that a linguistic theory might be applicable and acceptable and not that it might be verified and empirically valid - and that is the position which we adopt here. One might add that Hjelmslev's difficulties are only a special case of the general confusion in the hypothetico-deductive approaches to linguistics over what is accepted a priori and what is subject to empirical test, i.e. the problem of distinguishing the "hypothetical" from the "deductive" elements of those approaches (see below for more extensive treatment of this question).

Our present formulation is designed to imply a minimum commitment to any particular view of language or any particular theory of language. Insofar as there

is a commitment here, it is to what is perhaps inappropriately called a "fictionalist" position. The fictionalist position in linguistics, it seems to me, is capable of being held by supporters of any linguistic theory. It is the minimal theoretical position in linguistics in that it involves no commitment to any extra-linguistic (psychological or sociological or philosophical or other) position, while leaving open the possibility that collateral information may lead to an interdisciplinary approach. The fictionalist approach is preferable also in that it will save us from the hypostatization of such entities as Language, etc., since each such entity will be a purely theoretical construct and subject to the restriction that linguistic theory contains no existence postulate (see below).<sup>20</sup>

In this approach descriptive constructs are not assigned any ontological status other than that of scientific construct until such time as collateral non-linguistic evidence is available. The approach certainly does not involve any less commitment to empiricism. "Communication" may be left as an undefinable. We shall treat it extensionally as the class of communication events, the class of actual or potential signals capable of being transmitted by means of a fixed physical medium by a source to at least one receiver, where a signal is a physical indication of something other than itself.



This explanation is offered largely for the sake of completeness. It is not intended to restrict the study of communication to any particular type of signal or to any particular type of property of signals. We merely wish to point out some common area of phenomena with which semiotic/linguistic theories can be expected to be concerned. One should note that "communication" is not defined or explained as a concept. Each theory might be expected to provide an account of some set of properties of the phenomena we are calling "communication events". A theory might, for instance, provide the intension of the class of communications' systems, a typology thereof and a typology of communicational objects. This is what we find in Axiomatic Functionalism.<sup>21</sup> In such an approach the theory aims to provide the intension of the class of communication events.<sup>22</sup> Linguistic theory will be concerned with the subset of communications' systems which involve vocal communicational phenomena. A linguistic description, then, provides an account of specified properties of communication events by reference to an applicable and appropriate theory by modelling those events and their properties.

#### b. The function of linguistic meta-theory

In the previous section we attempted to describe linguistic theories and linguistic descriptions by



considering what such theories and descriptions do (or, perhaps, what we consider they ought to do). We also looked at various ways of considering the ontological nature of linguistic theories and descriptions. Discussions of this sort are not uncommon in linguistics. These two questions are both "meta-theoretical" in being "about linguistics" rather than being particular proposals within linguistics. A new theory within linguistics might be a new theory of distinctive features or of morphological structure. A description within linguistics might be, for example, a phonology of Russian. However, whenever a theory or description is proposed within linguistics, precisely "meta-theoretical" questions arise about that theory or description, i.e. we must ask whether the new theory meets the kind of conditions which we would demand of any theory or description. The conditions which any linguistic theory or description might be required to meet are clearly "meta-theoretical" and are themselves subject to meta-theoretical debate. Clearly, our views about the conditions to be met by any theory or description can only be kept apart from the questions of the function and nature of linguistic theories and descriptions with considerable artificiality. These three questions are inevitably interconnected. To maintain that a linguistic theory is a logical system

(e.g. a deductive system) implies that linguistic theories meet the conditions of logical systems. To establish the condition that linguistic descriptions be psychologically realistic implies the view that a theory of language is properly an investigation of a psychological nature. All such questions we might assign to what we could call "the philosophy" of the subject. In this thesis we shall concentrate on the particular problem of judging the acceptability of linguistic theories and descriptions. As will already have been seen, this problem leads to the three aforementioned questions concerning the function, nature and requirements of linguistic theories and descriptions. It is this restricted area which we will provisionally call "linguistic meta-theory".

The philosophy one adopts notoriously affects one's view of linguistic theory. We have already come to some decisions regarding the nature and function of linguistic theories and descriptions in the previous section. It is inevitable that one takes up some position in these matters and, of course, any position will be unacceptable to somebody. I think it would be naive to suppose that one's opinions of linguistic theory did not affect one's meta-theoretical pronouncements also. The generality introduced by the term



"meta-" is to some extent illusory. The force of this pessimistic caveat can, I think, be minimised if our version of linguistic meta-theory can be justified on generally acceptable grounds. Otherwise, our linguistic meta-theory will be of relevance to only a restricted number of approaches to linguistics (although this in itself would be a worthwhile exercise). We do not say that the meta-theory selects a particular sort of linguistic theory or description. Since the adoption of a particular position in linguistics or one's general philosophical position both affect one's view of linguistic meta-theory, one would run the danger of circularity or vacuity in setting up linguistic meta-theory as an "evaluation procedure". The danger, exemplified by the case of Chomsky (below) is that one may select those criteria which are favorable to one's own version of linguistics.

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Wittgenstein has rightly pointed out that, in attempting to answer the question, "what is the meaning of a word ?", one should first enquire what would constitute a satisfactory answer to the question, i.e. what would a satisfactory explanation of the meaning of a word look like ?<sup>23</sup> Wittgenstein's point is of general application. It would be absurd to attempt an answer to any question without knowing what kind



of answer is required. One should not, then, attempt to set up a linguistic theory in response to, e.g. our "fundamental question" or any other such starting point, without having some idea of the requirements that such a theory should meet. The same goes, mutatis mutandis, for descriptions of semiotic systems. Obviously, not every answer or theory or description is an acceptable answer, theory or description. We have already seen some of the restrictions that must be placed on linguistic theories and descriptions. The theory must, for instance, avoid hypostatization. Also, it must lead to adequate descriptions of phenomena in order to be considered "applicable". An inapplicable theory will be considered vacuous. The phenomena with which linguistics is concerned are, on the other hand, required to be intersubjectively agreed and capable of being recorded in such a way that they are describable in linguistic/semiotic terms. It is the aim of this discussion to determine the conditions under which linguistic theories and descriptions will be considered "acceptable" ; in other words, rather than being concerned with the construction of a linguistic theory answering our initial question, I shall be dealing with what the nature and structure of such a theory and the descriptions which can be constructed using that theory would look like. Much of the discussion will centre around

the problem of knowing when a theory is "applicable" and when a description is "empirically adequate" . We show afterwards how given theories and descriptions might meet the proposed conditions.

It is important to point out that we are not concerned to establish the content of a particular theory (other than for illustrative purposes). It is also important to realise that, although it should be possible to agree on the kind of phenomena with which linguistics can be expected to cope, it is not possible to determine the content of a linguistic theory or description directly from a consideration of the phenomena. We have already said that it is the function of linguistic theories to permit the construction of conceptual models and the function of linguistic descriptions to be conceptual models accounting for observable phenomena. The content of linguistic theories and descriptions clearly belongs to the conceptual world and is determined in a very large measure by the way we choose to approach linguistic problems. It could not, without circularity, be claimed that our choice of an analytical basis was determined by the phenomena themselves, since such an attitude would presuppose an analysis of the properties of the phenomena under consideration ; it is, however, the function of linguistic theories and descriptions to present precisely such an analysis.



The problem of what may be considered an acceptable linguistic theory or description has been relatively little discussed and, often, its treatment has been of either an ad hoc or cursory nature. The present investigation considers the conditions under which an answer to the theoretical problem (the construction of a linguistic theory) and to the associated descriptive problem (viz. "what constitutes an acceptable account of how communication is achieved in L ?", "what is an acceptable linguistic description of L ?") will be considered acceptable. This may be called the "meta-theoretical problem". Accordingly, we will place the study of these conditions within linguistic meta-theory.

Clearly, the considerations involved transcend particular questions of linguistic theory or linguistic description (hence, "meta-"). Questions of linguistic meta-theory will extend, as we have suggested, not merely to linguistic theories but to linguistic descriptions also. One might be inclined to call those considerations of acceptability which are appropriate to linguistic description "linguistic meta-description", or some such. We have seen already, however, that linguistic theories and descriptions are inextricably linked through the notions of applicability and "modelling in accordance with". If



we take it to be the first principle of linguistic meta-theory that no theory shall be considered acceptable, unless it permits the construction of adequate linguistic descriptions, then it is clear that the acceptability of a linguistic theory depends in part on the acceptability of the linguistic descriptions which can be made in applying that theory to the speech phenomena. If, in addition, linguistic meta-theory tells us that the acceptability of a linguistic description depends in part on the acceptability of the general linguistic theory upon which it relies, then we can say that the conditions of acceptability attaching to linguistic descriptions have a bearing on those pertaining to linguistic theories and vice versa. The meta-theoretical conditions of acceptability applying to theories and descriptions can only arbitrarily and unnaturally be sundered, i.e. what we are saying is, in effect, "no description without good theory and no theory without good description".

We will take it as a principle that linguistic meta-theory involves both the conditions for good theories and for good descriptions and that good theory and good description are inextricably linked. Clearly, no acceptable linguistic description can be made on the basis of an unacceptable linguistic theory.

An inconsistent theory inevitably leads to confusion in description. Martinet's double articulation theory, to take a solid example, contains the view that the elements of the second articulation are determined by means of analysis of the signifiants of signs. Thus, if we compare the signifiant of "cat" with that of "pat", we find that the signifiants are differentiated by two units, which both recur elsewhere, (in "pin" and "kill") and which are not items of the first articulation - they are not signs. However, a moment's reflection will convince us that the signifiant of a sign is a set of elements in free or complementary distribution, as is the case with the sign "be" in English, where we have the variants "am", "are", "is", etc.. If we accept this, then it is impossible to maintain that the second articulation is a product of the analysis of signifiants. The signifiant is a set. It is not the set which is analysed but the members of the set by comparison with the members of other sets. But this outlet is simply not catered for in the theory. Strictly speaking, the inconsistency involved makes a description of the second articulation of a language impossible, since the theory leaves nothing which can be compared in the desired fashion.

The reverse problem arises when insuperable difficulties in description lead us to reject a theory. A



case in point is the immediate constituent analysis of the 1940's and 50's. In I.C.A. two meta-theoretical criteria were proposed. These were that the analysis should be exhaustive and that the analysis should lead to determinate results. Clearly, any theory which, when applied in the analysis of speech phenomena, could not achieve these goals, would be rejected. Now, the theory provided various criteria for 'cutting' strings. One of these criteria was the principle of binary cuts, which asserted that, for any grammatical  $n$ -tuple,  $X$ , where  $n > 1$ ,  $X$  is a pair,  $\langle A, B \rangle$ , of grammatical objects. That is, every string is cut in two parts. So, in English, the string,

(6) John died.

is analysed

John / died

and the string

(7) The cat went home.

is analysed

The cat / went home

The methodology for the analysis was a criterion of substitutability in a fixed context. John died is clearly a pair (in syntax). The item, the cat, is substitutable for John (but not the or cat singly) in the context, '\_\_\_\_\_ died' ; and went home (but not home) is substitutable for died. That is, all substitutions leave us with wellformed grammatical strings. Thus we have,



(8) The cat died.

and

(9) John went home.

but not

(10) \*the died.

(11) \*cat died.

or

(12) \*John home.

Of course we have,

(13) John went.

But in that case we are left with a pair again.

Now, the criterion of substitutability or "like-patterning" works quite well in a number of cases. However, it proved impossible, in a number of cases, to arrive at a binary cut. In those cases there was a principle of multiple cuts. The French, bleu-blanc-rouge, was one example. Another was multiple apposition, as in

(14) John / the man from London / the man who read the  
paper // went home.

This was, of course, the poverty of the method. There was a principle of binary cuts except only when binary cuts were impossible.

Quite apart from these examples, in a range of cases, only arbitrary decision could determine the order of cuts. For example, in

by comparison with John / died or

John runs / fast

by comparison with

swim / fast.

In the case of

(16) The King of England's hat

we might have the King of England's/ hat by comparison with a / hat or the / King of England's hat by comparison with the / man. A lot of rather doubtful solutions were proposed to deal with these examples. All this is very well known. The point I wish to make is that I.C.A. was a theory which was rejected principally because it was incapable of leading to acceptable linguistic descriptions. It failed the meta-theoretical requirements which specified that a theory should allow the determinate and exhaustive analysis of speech phenomena. That is, we have a case where a theory is rejected on the grounds that it fails to give good descriptions. (It was also unacceptable on other grounds, no doubt).

The example of Martinet's double articulation theory shows there is no good description without good theory and the I.C.A. example shows there is no good theory without good description. The implication for linguistic meta-theory is that there can be no arbitrary division between the conditions of acceptability for theories and those for descriptions.

From a different point of view, as we shall see, any linguistic description implies the existence of some linguistic theory in terms of which the linguistic description was formulated : we want to avoid the terminologically confusing conclusion that linguistic theory is also "linguistic meta-description" (albeit in a different way). To the extent to which a linguistic description implies some theory, any linguistic description will be dependent on the acceptability of the theory applied in constructing that description (the consistency, etc., of that theory). Hence, we may say that the conditions of acceptability of linguistic theories are relevant to the acceptability of linguistic descriptions and vice versa. That is, since the acceptability of descriptions is a necessary condition of the acceptability of linguistic theories, the conditions of acceptability of descriptions are relevant to the acceptability of theories. The I.C.A. example and the Martinet example illustrate these last points.

This is not all, however. Certain meta-theoretical conditions, most notably - consistency, can be interpreted as concerning also the relation between a linguistic theory and a linguistic description. We require, for example, that a linguistic description be consistent with some linguistic theory. If a theory



specifies that all communicational entities are 'functional' (in the functionalist sense) , then no communicational entity can be postulated in a description using that theory which can be shown to be not functional. A case in point might be whether or not than is a sign in English in such expressions as,

(17) He is bigger than John.

(18) My car is newer than John's.

The functional principle requires that at each point in the chain the speaker is considered to make a choice. This implies that than must be chosen from a set containing at least one other item (possibly  $\emptyset$ ). Consequently, to set up a sign than would make the description inconsistent with the theory, because than is not replaceable by any other item - it is obligatory in the comparative construction (it is not even replaceable by  $\emptyset$ ). Than can be treated as a discontinuous part of the comparative. The fact that conditions of acceptability may apply, as in this case, to the relation between theory and description affords us another reason for subsuming the conditions of acceptability applying to theories and those applying to descriptions under one rubric - linguistic meta-theory.

One objection which could appropriately be raised at this point in connection with the study of linguistic meta-theory is that the present formulation leaves us

open to a potentially infinite and pointless regression. That is to say, one could not only study the meta-theoretical conditions of acceptability of linguistic theories and descriptions, but one could go on to ask, "under what conditions are the proposed meta-theoretical conditions themselves acceptable and under what conditions are these meta-meta-theoretical conditions acceptable (and so on ad infinitum) ?".

To avoid this infinite regression and the potential horrors of linguistic meta<sup>n</sup>- meta<sup>n-1</sup>-....meta<sup>1</sup> - theory, I introduce in this context a priori Hjelmslev's notion of the arbitrary and the appropriate.<sup>25</sup> In the context of linguistic theory, this notion is explained as stating that linguistic theory is,

"un système déductif pur, en ce sens que c'est la théorie à elle seule qui, à partir des prémisses qu'elle énonce, permet le calcul des possibilités qui en résultent".<sup>25</sup>

The theory, according to this view, does not imply the prior knowledge of existing phenomena or of the nature of those phenomena (thus avoiding a potential circularity) and is, thus, "arbitrary" in that the calculus of theoretical possibilities depends on the selection of the initial set of premises - each of which



is arbitrarily selected (i.e. might have been different).  
On the other hand,

"Le théoricien sait par expérience que certaines prémisses énoncées dans la théorie remplissent les conditions nécessaires pour que celle-ci soit applicable à certaines données de l'expérience".<sup>25</sup>

That is to say that a linguistic theory must, as we have pointed out, avoid vacuity by being applicable in the description of speech phenomena. In order to achieve this, it must be capable of enumerating all necessary possibilities for the consistent and adequate description of fields of phenomena. A theory which is capable of achieving this will be "appropriate".

In the context of linguistic meta-theory, we will say that the conditions to be fulfilled by a linguistic theory and those to be satisfied by linguistic descriptions are required to be (demonstrably) appropriate conditions of the acceptability of linguistic theories and descriptions. A meta-theoretical condition will be "arbitrary" in that it is introduced, as a condition, without implying prior knowledge either of linguistic theories or of linguistic descriptions (i.e. the conditions might have been different). A condition will be

"appropriate", if it is such that its non-observance in a linguistic theory or description would lead to self-contradiction, absurdity, inadequacy or circularity of argument. Stated positively, we will say that certain requirements are necessary for the acceptability of any theory or description. Meta-theoretical conditions of acceptability will be appropriate (useful) insofar as their observance is necessary for meeting these conditions. We will, say, arbitrarily, that the demonstration of the appropriateness of any given condition of acceptability belongs to linguistic meta-theory. The demonstration in question will constitute the justification for the acceptance or rejection of any meta-theoretical condition proposed. It is also part of the justification for the acceptance of any meta-theoretical condition of acceptability that the condition meet certain general, epistemological conditions of, for instance, consistency (i.e. with other meta-theoretical conditions). One cannot, for instance, require both that a linguistic theory abstain from containing an existence postulate and that it be empirically valid also (as does Shaumjan, above). Similarly, one cannot set up a "general linguistic theory" without an existence postulate and then require that empirical linguistic descriptions be "deducible" from that theory (as does Uldall, see below).

It might be argued that conditions on conditions  
(st eory.



However, it is more appropriate, in view of the very general nature of these epistemological considerations, to treat them as part of the justification of a given condition of acceptability and hence as part of linguistic meta-theory. The reason for this is that considerations of consistency, etc., in a set of meta-theoretical conditions may be regarded as merely the application of general principles of epistemology rather than specifically linguistic meta-meta-theory. One does not have to reinvent epistemology in order to discuss the acceptability of linguistic theories and descriptions (although we may have to remind ourselves of it).

It should be clear from the foregoing that I shall not be principally concerned with the construction of actual linguistic theories and descriptions but I shall deal with the conditions which particular theories and descriptions or descriptive methods will ideally<sup>26</sup> be required to meet and, to a lesser extent, with illustrative examples and the success or failure of existing theories and descriptions in matching up to these conditions.

The conditions involved in the acceptability of theories and descriptions may be usefully, if somewhat crudely, divided into the following categories.

(a) The 'general' and the 'specific' depending upon whether the condition in question is of a general, epistemological nature and applicable to any scientific discipline or a condition peculiar to linguistics.

Consistency is a general condition but the condition that all features in communicational or linguistic systems possess physical realisations, i.e. there is no feature that is not physically transmitted, is clearly a condition peculiar to linguistics/semiotics.

(b) The 'formal' and the 'empirical'. A 'formal' condition tests the theory or description with respect to its logical structure. An 'empirical' condition is a test of the theory or description which involves the confrontation of the theory or description with some set of phenomena. If we want to know whether the conclusions of a theory or the implications of a description are valid, as deductions from the theory or description, we are concerned with 'formal' conditions.

We might wish to check, for instance, whether there is a minimum entity in phonotactics or a maximum entity in phonematics in Axiomatic Functionalist theory.

This would be a purely formal matter involving the postulates, definitions and permissible inferences of that theory. The question of the recognition of a morphology/syntax distinction revolves around the definition of terms and what is established elsewhere

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in a given approach, bank-clerk and men in English are morphological or syntactic complexes, we have to do with an empirical matter concerning the structure of English which cannot be resolved within the confines of any theory or description but which can be resolved only through a consideration of the relevant data (and the construction of appropriate models and tests).

(c) The 'internal' and the 'external'. An 'internal' condition tests a linguistic theory or description by means of some criterion lying within linguistics. An 'external' condition is a criterion which tests a linguistic theory or description by some standard not intrinsically concerned with linguistics. It is an 'internal' (but not 'specific') condition that all linguistic theories be applicable to all fields of speech phenomena. (The same condition applies mutatis mutandis to phonetic theories.) An 'external' condition might be that any linguistic theory provide a psychologically realistic account of language acquisition and that each description be usable as a model for language learning.

To take other examples ; it would be a general condition of scientific systems that they be 'exhaustive' in their treatment of the data under consideration but

it would be a requirement peculiar to linguistics to require that certain of the categories of linguistic theory be translatable into phonetic terms or that linguistic descriptive constructs be interpretable as observable as phonetic forms. The requirement of consistency would be regarded as a formal condition of acceptability, whereas the requirement that a linguistic description enumerate all and only the finite strings of a language is a clearly empirical condition. Whether a linguistic theory should account for "paraphrase relations" or whether a linguistic description can adequately describe the affricates in Russian are matters internal to linguistics. However, the condition that a linguistic theory provide an account of language acquisition or be interpretable as a behavioural account of language use can only be regarded as external criteria of acceptability.<sup>27</sup> (Note that we do not comment at this point on the merits of these particular conditions - they are examples only.) These six categories are not intended to be exhaustive or mutually exclusive. Consistency is a general, formal and external condition, for example. The advantage of the classification is that it helps us to know what kind of condition we are dealing with in any given case. We can also see, incidentally, the wide range of types of condition that are involved in the acceptability of linguistic



theories and descriptions.

As we have said, we will be concerned not only with the types of meta-theoretical condition but the justification of their selection and formulation also. This justification is a matter of demonstrating the consistency and appropriateness of the conditions proposed. In order to be able to apply the conditions of acceptability, it will be necessary for us to consider the areas of linguistic theories and descriptions to which any given meta-theoretical condition will, or will not, be applicable.

As I take it, any scientific discipline is ultimately concerned with this kind of meta-theoretical consideration. The point where linguistics, or any other science, becomes self-consciously aware of the conditions to be satisfied by its own theories and descriptions is more or less the overlap between that science and the philosophy of science.

The need for an awareness of the epistemological constraints on linguistic theories and descriptions has been noted by only a small number of writers on linguistics. The most notable (and outspoken) of these was probably Hjelmslev, who writes, "aucune théorie

scientifique ne peut être construite sans une collaboration active avec l'épistémologie".<sup>28</sup> Whilst this particular statement may be considered something of an overstatement if taken too literally, one would certainly be inclined to agree that any approach in science requires an implicit or explicit foundation in some philosophical attitude towards science in general. This is especially so when we consider a scientific theory in terms of its logical structure. A concern with the philosophy of science seems to be particularly important in linguistics which deals with an area of reality which is the traditional concern of the Humanities.<sup>29</sup> One should be careful to note, however, that the fact that some theory or description is in accord with some conception of the philosophy of science cannot serve as a justification of that theory or description (although the converse might hold, i.e. a successful scientific theory might lend support to a philosophical conception of science). As Shaumjan has pointed out,

"The question of the adequacy of some or other scientific and, in particular, linguistic theory must be decided using the resources of that science within the framework of which a given theory is proposed. Philosophy cannot serve as a weapon for the justification of a scientific theory. The justification of



a scientific theory is to be sought in those facts which are explained or predicted by that theory.<sup>30/31</sup>

Shaumjan's view clearly implies the position (which we will adopt) that external conditions of acceptability of the sort we have mentioned must be regarded as taking second place to the other types of condition (i.e. the epistemological and, especially, the empirical). Nevertheless, as Lyons<sup>32</sup> has pointed out, it is part of the "material adequacy" of a linguistic theory ("saving the appearances") that the theory in question (or description in question) should not conflict with relevant information from other disciplines or other areas of linguistic study.

It is clearly the case that "an active collaboration with epistemology" is intended by Hjelmslev to be a necessary but not sufficient condition of the acceptability of linguistic theories. This being so, the satisfaction of that condition cannot serve as any form of justification for the theory. Furthermore, any given attitude towards the philosophy of science may itself be in error, contain contradictions or may prove inappropriate to linguistic theory on rigorous application or, indeed, it may lead to self-contradiction within the linguistic theory in question.<sup>33</sup> For these

reasons it is necessary to speak of the meta-theoretical conditions or constraints on, or to be met by, linguistic theories or descriptions. The satisfaction of these conditions does not constitute the justification of any theory or description to which the conditions may be applied but it does show that the theory or description is not to be rejected on epistemological grounds ; more positively, conditions of acceptability show us how we might go about rejecting a linguistic theory or description in a principled way.

There is a sense, as we have seen, in which we must avoid the dangers of an excessive commitment to any particular attitude in the philosophy of science prevailing or otherwise in setting up the meta-theoretical conditions to be satisfied by linguistic theories and descriptions. The difficulty in this respect is exactly the same as that encountered in psychological approaches to linguistics (as opposed to psycholinguistics), which notoriously run the risk of becoming quickly outmoded or of relying on notions which are either too naively reproduced or which are already discarded or ~~ob~~solescent.<sup>34</sup> A certain independence is advisable, then, but it would be churlish not to recognise that the philosophy of science necessarily plays a considerable role in setting up meta-theoretical conditions on the acceptability of theories and



descriptions in linguistics.

One should emphasise again that no approach to the philosophy of science is to be accepted uncritically in linguistic meta-theory. On the other hand, we must be wary of the dangers of an eclectic approach to the philosophy of science. The position adopted here is that linguistic meta-theory looks both towards the philosophy of science and towards linguistics. There is no doubt that no view of the philosophy of science has been constructed with linguistics in mind ; equally, there are reasons to think that no view of the philosophy of science adequately represents the nature of linguistics. Also, there are questions in linguistic meta-theory, as we have seen, which have relevance only to linguistics. For these reasons we set up linguistic meta-theory as a separate field of research different from the philosophy of science.

NOTES TO CHAPTER 1

1. N. Chomsky, Syntactic Structures, p. 13.
2. Intuitions about unacceptable, "asterisked", forms serving as the basis for (sometimes) highly theoretical treatments illustrate the difficulty of the approach. Often the reason for the asterisk is quite opaque and one suspects that the theoretician's intuitions are all too convenient for his theoretical purpose or that the theoretician is not presenting independent phenomena but phenomena that have been extensively modelled already (see below). Examples of forms asterisked for opaque reasons are not difficult to find. Consider, for instance, the expressions asterisked by Chomsky in his "Remarks on Nominalisation" which serve as the basis of discussion ;
  - \*John's easiness to please (p. 18)
  - \*His criticism of the book before he read it (p.27)
  - \*Don't be tall (p. 27)

And compare such counter-examples as,  
John's easiness to please is well known.

I didn't like his criticism of the book before he read it.

Don't be tall when you grow up - I don't like tall people.



The point is that it would be absurd for each linguist to give a different account of "the grammatical" because his intuitions about what is and what is not acceptable in English lead him to accept or reject different sets of data from the next linguist. The obvious possibility of bickering over data drawn from intuitions is very clear in this case.

3. S.K. Shaumjan, Problems of Theoretical Phonology, pp. pp. 15 - 16.
4. S. K. Shaumjan, ibid., p. 16.
5. The view that the theory provides the intension of the class of descriptions is due to J. Mulder, "The Strategy of Linguistics".
6. See J. Mulder, "Linguistic Theory, Linguistic Descriptions and the Speech Phenomena".
7. This problem has been highlighted by R. Harris, Synonymy and Linguistic Analysis, pp. 158 - 9.
8. As expressed, for instance, in the dictum of A. V. Martinet that, "function is the criterion of linguistic reality"; A Functional View of Language, p.5.
9. According to I.M. Bochenski, "protocol statements" are "Aussagen, welche das Vorkommen von Phänomenen feststellen" (Die Zeitgenössischen Denkmethoden, p. 105) and are of the form, "X hat zum Zeitpunkt T

das Phänomen P auf der Stelle Z beobachtet"  
(Europäische Philosophie der Gegenwart, p. 71).

In linguistics the first writer to have regarded recorded phenomena as "protocols" seems to have been K. Bühler. He writes, "An konkreten Sprechereignissen macht der Sprachforscher seine spezifischen Beobachtungen und fixiert ihr Ergebnis in Protokollsätzen" („Die Axiomatik der Sprachwissenschaften", p. 19). See also J. Mulder, "Linguistic Theory, . . .", p. 92.

10 This programme is carried out in the "Postulats de la Linguistique Fonctionnelle Axiomatique" by J. Mulder and in the "Postulats de la Sémantique Axiomatique" by S. Hervey, where the notion, phonetic form, is integrated into the theory via the notion, phonological form. In Axiomatic Functionalism a phonological form is defined as a "particular maximum class of one or more phonetic forms, f, each member, f, in its capacity of standing in a relation with a particular distinctive function, d" (df. 23) and a phonetic form is a "generalised model for a class of impressionistically similar phenomena that may correspond to one or more figurae in a natural language" (dfs. 22 and 22a).

11 Chomsky's argument in favour of transformational grammars over "taxonomic grammars" is a case in point. It is discussed in the following chapter.

12 L. Hjelmslev, Prolegomènes à Une Théorie du Langage,



- 13 This is clearly related to the dictum of Saussure that, "bien loin que l'objet précède le point de vue, on dirait que c'est le point de vue qui crée l'objet", Cours, p. 23 ; for a discussion of this idea, see J. Mulder, "Linguistic Theory,....".
- 14 H. Uldall, Outline of Glossematics, p. 11.
- 15 The present position implies that a semiotic theory provide a typology of semiotic systems of which language is but one possibility. This is, in fact, the position in Axiomatic Functionalism (see J. Mulder and S. Hervey, Theory of the Linguistic Sign) and in Glossematics (see L. Hjelmslev, Résumé of a Theory of Language).
- 16 F. de Saussure, Cours, p. 33.
- 17 See J. Mulder, "Linguistic Theory,....", p.96, where he writes, "the purpose of a linguistic theory is... to render an unlimited number of good...linguistic descriptions possible and this gives us the criterion for judging the adequacy of linguistic theories". I prefer to reserve the term "adequacy", for another purpose but the same condition is retained.
- 18 L. Hjelmslev, Prolégomènes, pp. 16 - 17.
- 19 L. Hjelmslev, ibid., p. 24.
- 20 See note 14 above.
- 21 See J. Mulder, "Les Postulats....", especially the Introduction, pp. 19 - 21.
- 22 See note 5 above.
- 23 The reference is to L. Wittgenstein, The Blue

and Brown Books, p. 1, where he says,

"What is the meaning of a word ?".....Let us attack this question by asking first, what is an explanation of the meaning of a word : what does the explanation of the meaning of a word look like .....to understand the meaning of "meaning" you ought to understand the meaning of "explanation of meaning!".".

24 See below, ch. 3.

25 L. Hjelmslev, Prolégomènes, pp. 24 - 5.

26 When Popper says, "I suggest that it is the task of the logic of scientific discovery ..... to give a logical analysis of this procedure : that is, to analyse the method of the empirical sciences" (The Logic of Scientific Discovery, p.27), it is clear that he has in mind the ideal method of the the empirical sciences. A similar position is adopted here with respect to linguistic theories and descriptions.

It might be argued that no "ideal meta-theory" could be set up for linguistic theories and descriptions on the grounds that each theory (a) imposes its own point of view and (b) thereby, in a sense, creates its own set of phenomena (i.e. those which satisfy the conditions for analysis by that theory as opposed to any other theory). Thus, it might be held, no set of meta-theoretical conditions could be of relevance to all linguistic theories and descriptions (each theory would have its own meta-theory). Such a point of view fails to distinguish between the agreed range of the theory (the set of



phenomena existing independently and agreed upon as explicanda (phenomæna per se) and the scope of a description (the set of phenomena which actually satisfy and are considered in their capacity of satisfying the functions of the theory or description (phenomena as modelled). It is agreement on the range of phenomena that is required ; that is, we require an agreement on an independent selection of phenomena. Furthermore, to create different meta-theories for each approach to linguistics would destroy the initial motivation for setting up meta-theoretical conditions, viz. the need to establish an independent set of criteria of acceptability.

27 Of course, there is a certain amount of overlap between these categories, which are intended as no more than broadly illustrative.

28 L. Hjelmslev, Prolégomènes, p. 25.

29 See in this connection, H. Uldall, Outline, ch. 1.

30 S.K. Shaumjan, Filosofskije Problemy Teoretičeskoj Lingvistiki (Philosophical Problems of Theoretical Linguistics), trans. P.R.R., p. 4.

31 The requirement that a theory be tested for empirical adequacy in leading to the understanding of given phenomena is itself a meta-theoretical condition.

32 See J. Lyons, Structural Semantics, p. 9

33 Examples of these hazards are illustrated below.

34 One will recall in this context the obsolete and naive pseudo-psychologism of Saussure and Bloomfield.

CHAPTER TWO

A SURVEY OF SOME PREVIOUS APPROACHES



In general, we can say that only very few linguistic theoreticians have turned their attention to the problem which is considered here. The treatment of the conditions of acceptability of linguistic theories and descriptions has been cursory or has been simply ignored as a matter of self-evidence or epistemology transcending linguistics proper. While it is not the present task to enumerate the valid forms of argument which are presupposed in all rational discussion and while epistemology must lurk somewhere in the near background, so that one may say that there is some degree of self-evidence and epistemology in linguistic meta-theory, it is not true to say that all the problems of the acceptability of linguistic theories and descriptions can be solved by an easy reference to well known principles of argument or universally accepted views on the epistemology or philosophy of science. In the first place, it is necessary to have firm principles in judging the acceptability of linguistic theories and descriptions and, in the second, it is far from obvious that there is any prevailing view of the philosophy of science to which one can make unambiguous and universally acceptable appeal. It would seem to be the case, furthermore, that linguistics, as a discipline, contains a structure of components that may be unique to it. The relations between the various components of linguistics, as we shall see,

determine, in a large measure, not only the kind of conditions to be met by linguistic theories and descriptions but also their precise formulation and the way in which they are to be satisfied. Moreover, less specific conditions of acceptability deriving from the general notion of a science and the need for a "calculus" aspect of, say, linguistic description must be formulated specifically with respect to linguistics in such a way that the "empirical" and "formal" aspects of sciences are recognisably retained. Through a consideration of the problems associated with these questions we are led to an investigation of various notions of acceptability of linguistic theories and descriptions, the precise formulation of conditions of acceptability and a consideration of the points at which such conditions are pertinent and how given linguistic theories and descriptions may satisfy the conditions in question.

One area of linguistic meta-theory which has received considerable attention of late is the question of "explanation" in linguistics. As should be obvious from the present remarks, the "explanatory" aspect of linguistics is, from my point of view, only one of the areas to be considered by linguistic meta-theory. In particular, insofar as any explanation may be said to rely on a principled and epistemologically sound description of phenomena, in linguistics necessarily involving an epistemologically sound theory, one can see that the requirement that (aspects of) linguistics



be "explanatory" already presupposes the satisfaction of a whole set of other logically prior conditions of acceptability on the part of linguistic theories and descriptions. It is for this reason that we are not greatly concerned in this thesis with the notion "explanation".

In the main, we can say that there have been five major contributors to linguistic meta-theory. They are ; Hjelmslev,<sup>1</sup> Chomsky,<sup>2</sup> Lyons,<sup>3</sup> Shaumjan<sup>4</sup> and Mulder.<sup>5</sup> Contributions of a more cursory nature have come from many sources. The most noteworthy of these is undoubtedly Bloomfield according to whom, "the only useful generalisations about language are inductive generalisations".<sup>6</sup> The principle that linguistics should be characterised by its inductive approach may be said to recur in virtually all British and American works on linguistics between 1930 and 1960. European linguistics since Saussure (and probably influenced by neo-grammarians tradition) has been either implicitly or explicitly characterised by its deductive or hypothetico-deductive approach. The adoption of an inductive approach to linguistics as a necessary condition of its scientific nature was, no doubt, influenced by the logical positivism of the Vienna Circle<sup>7</sup> of logicians for most of whom science was to be characterised by its inductive nature. It may fairly be said that, since the early sixties, most

philosophers of science and most linguists have **considered** that science and linguistic science in particular are to be conceived of as "deductive" or "hypothetico-deductive" or "nomological-deductive" in character. In the present work it is assumed throughout that linguistic theories and descriptions will be of an implicitly or explicitly deductive nature.

Inductivism is rejected in the present work for several reasons. In the first place, it is not at all clear that inductivism is of any particular relevance to linguistics. Even those who have explicitly defended the inductivist cause have found it expedient to introduce arbitrary but appropriate sets of theoretically defined and justified functions (e.g. Bloomfield's definitions of the phoneme and the word, etc.) in advance of the analysis of texts. To the extent that the functions do not emerge from the inductive comparison of data but are part of a preconceived language model, one must say that the position of inductivist linguists, such as Bloomfield, is at best ambiguous in that there always seems to be assumed some non-inductive element which is used as a criterion of relevance. Inductivism is irrelevant to linguistics also in that nearly all modern paradigms in the philosophy of science adopt the "hypothetico-deductive" or "nomological-deductive" approach in characterising



scientific systems and the majority of linguists follow that lead. It is certainly the case that all generative approaches to linguistics are nomological-deductive in character (some quite explicitly so) and one can regard the inductive approach as inherently opposed to structuralism in linguistics insofar as structuralist theories of language involve an explicitly defined descriptive framework interrelated through a well defined ontology with explicit criteria of relevance. One can easily see that the idea of the applicability of such a theory conflicts with the notion of the inductive comparison of phenomena with a view to revealing the supposed inherent structure of those phenomena. In the one case, the structure describing the phenomena is due in part to the theory adopted and, in the other, such structure as is found in the description is intended to be entirely the product of the investigation of phenomena. Clearly, a "preconceived" structure will be anathema to the genuine inductivist, whether or not the structure in question is non-redundant and applicable. The structuralist, however, will maintain that the only part of linguistic description which is remotely inductive is the determination of the extension of previously defined sets.<sup>8</sup> This "additive" induction scarcely qualifies as true inductive methodology.

Although lip-service to inductivism was duly paid by many linguists, including Bloomfield, there is very little in the works in question which genuinely corresponds to an inductive procedure or methodology. In the case of Bloomfield it is quite clear that he has in mind a (part) theory of language, including his explanation of the act of speech and the set of linguistic categories (phoneme, morpheme, etc.) which he introduces by means of examples, but which has in fact been set up quite independently of any inductive cycle of data-gathering, generalisation, etc... The work of Zellig Harris has more of the appearance of an approach governed by an inductive procedure. Harris claims that his "schedule of procedures.....is designed to begin with the raw data of speech and end with a statement of grammatical structure"<sup>9</sup>. One might imagine from this that Harris starts with observations about speech, continues with experiments on the data and ends with general statements which are the product of classification, comparison and generalisation. However, it quickly becomes obvious that a "schedule of procedures" is at best a way of organising research. Harris himself points out elsewhere (with some contradiction perhaps) that "the chief usefulness of the procedures... is...as a reminder in the course of original research and as a form for checking or presenting the results,



where it may be desirable to make sure that all the information called for in these procedures has been validly obtained".<sup>10</sup> It follows that the procedures "do not constitute a necessary laboratory schedule in the sense that each procedure should be completed before the next is entered upon".<sup>11</sup> These remarks seem to take away the force of Harris' view that the procedures he advocates involve a stepwise progression from data to general statements. What is more, there is an enormous amount of linguistic theory underlying Harris' work that is not, and cannot be, justified by any form of induction. Every procedure requires, in fact, a theoretical justification which cannot, without circularity, depend upon the inductive observation of data. For example, the division of linguistic study into phonology and grammar and the establishment of the distributional criterion of relevance and the notion of "context" are the most striking of the theoretical ideas introduced a priori.

Clearly, Harris could not set up a phonology/grammar distinction or a distributional criterion of relevance as inductive generalisations resulting from the application of his procedures. This would be circular since his procedures already involve the phonology/morphology distinction and the the distributional criterion of relevance. Harris justifies

his conceptual approach in terms of its inherent usefulness and, since a priori reasoning is excluded from the approach and inductive reasoning leads to circularity, this is the only possible justification. But, if usefulness is the only justification for the procedures advocated and, hence, for the theoretical approach underlying those procedures, then the theory can be rejected simply because it fails to give us a justification for its most important statements, i.e. the highest level of general theoretical statements about language. The approach is theoretically bankrupt.

We could say, then, that it simply does not matter (methodologically speaking) whether one chooses to set up hypotheses and then to test them using Harris' procedures or whether one makes a painstaking and probably impossible collation of data and application of the procedures in order to produce a hypothesis as a result. But in that case we are left with a free choice between a viable hypothetico-deductivism (the first alternative) and an unrealistic inductivism (the second). If this is the case, however, one simply fails to understand Harris' insistence on an inductive methodology and an ordered schedule of procedures, since there seems to be no virtue in the approach whatever (and Harris himself does not seem to claim any virtue for it).



The idea of an inductive methodology as the hallmark of a science undoubtedly held an ingenuous fascination over linguists such as a Harris. The problem with Harris' defence of inductivism in linguistics is, however, fundamental. The problem is that of moving from the charm of inductivism to the formulation of a mechanism of reasoning (in linguistics or anywhere else) which could be acceptably justified and applied in scientific activity. Harris produces a set of procedures but he fails to find any serious justification for them. It is not even clear that the procedures can be applied rigorously (as Harris admits (above)). In fact, much of the justification of inductive methodology in linguistics relies on the assumption that the so-called "problem of induction" can be solved and that an acceptable inductive logic can be set up which would justify our undoubted use of inductive arguments based on experience. We have to take a certain amount on trust, although no real reason for our doing so is given. This is not the place to discuss the wider question of the "justification of induction" but we can remark that the Canons of Induction proposed by Mill and which were associated with the "scientific method" by many linguists and philosophers of science alike (in fact, they were what was meant by "inductivism") must be rejected. As Harre<sup>12</sup> has pointed out, the canons of induction frankly do

not stand up. The "principle of accumulation" by which a science is deemed to consist in the accumulation of new facts and to grow by adding new facts to some supposed store of knowledge is simply false. All sciences involve theory building and our theories not only determine the way we interpret the facts but also play a large role in determining what we consider the facts to be. The "principle of inductive inference" will not stand up because it cannot help us to decide between competing solutions. It is always backed up by a debatable "principle of simplicity". Finally, the "principle of instance confirmation" by which our belief in a law increases in proportion with the number of phenomena conforming to the law is, perhaps, the weakest point of all. Most obviously, this principle cannot account for the fact that theories can be overthrown no matter how often their 'laws' have been seen to be instanced. Furthermore, the value of a theory is not determined by simply adding up the number of times its laws hold good. The law does not exist independently. The theory must constitute a coherent system of ideas, perhaps with surprising or unforeseen consequences. It is this system as a whole which must be tested and, if we are not to be blinkered by our own laws, the theory must be tested against new types of phenomena.



The most serious difficulty with naive inductivism is that, as we have hinted above, those who advocate the outlook do not put it into practice. Neither linguists nor any other scientists are actually observed to behave in the manner we would expect if the canons of induction were a faithful picture of scientific activity. In particular, no scientist is especially interested in the simple accumulation of facts ; his interest is in the theories which explain those and, he hopes, other facts. The attraction of deductivist views of the philosophy of science is that they do reflect the interests of scientists. Deductivist views of the philosophy of science lay the emphasis on the right place ; viz. on systems of ideas and not on the facts which those systems of ideas attempt to explain. As Hempel has said, naive induction "could never get off the ground".<sup>13</sup>

One can argue that induction has a very important place in the development of different (alternative) forms of logical analysis,<sup>14</sup> i.e. non-deductive logics which do not utilise the notion of logical necessity in their inference patterns. However, for the non-logician at least, there remains the debate over the so-called "justification of induction" and the apparent failure of attempts to overcome the "problem of induction".<sup>15</sup> These difficulties added to the irrelevance of inductive techniques in most modern philosophical approaches to

science are quite sufficient to demonstrate the reasonableness of concentrating on deductive approaches to linguistics to the exclusion of non-deductive ones.

We now turn to some of the major contributions to linguistic meta-theory. We consider those of Hjelmslev, Chomsky and Lyons. The ideas of Shaumjan are considered as they arise in extenso in other parts of the text along with more minor contributions. It will be obvious that the present work takes Mulder's contributions as a starting point and his ideas are discussed in their appropriate places.

#### a. Hjelmslev

In establishing the "prolegomena" to a science of language, Hjelmslev was, undoubtedly, many years ahead of his time in taking seriously the epistemological considerations involved in linguistic theorising.

Although we are in disagreement with a number of Hjelmslev's positions, we do not wish to minimise the great value of Hjelmslev's contributions to the epistemology of linguistics or the fact that Hjelmslev was the first to discuss the matter in any depth. One should also note that there are a number of points in the present text where we are in perfect agreement with Hjelmslev's views. We agree, for instance, that a



linguistic theory, although appropriate with respect to the speech data, is arbitrary with respect to them also and we agree that a linguistic theory contains no "existence postulate" and that a linguistic theory is a conceptual model of (descriptive) models with descriptions as models of phenomena. It is fairly self-evident that one cannot judge the acceptability of a linguistic theory or linguistic description without having a clear conception of the conditions which will decide whether a given theory or description is acceptable or not. Herein lies the importance of epistemological considerations in linguistics. Hjelmslev's approach to this problem is summed up in his Principle of Empiricism and his Principle of Simplicity. Both of these principles are capable of criticism.

Hjelmslev introduces his topic by remarking that,

"Une théorie, pour être la plus simple possible, ne doit rien supposer qui ne soit strictement requis par son objet. En outre, pour rester fidèle à son but, elle doit, dans ses applications, conduire à des résultats conformes aux "données de l'expérience" réelles ou présumées telles".<sup>16</sup>

One may glean from these comments that Hjelmslev envisages that there must be some ultimate level of testing lin-

guistic descriptions against observable experience. Such an assertion would seem indisputable. What one may wonder, however, is how Hjelmslev could believe, as he apparently did, that the satisfaction of his Principle of Empiricism would lead to the fulfillment of this condition. Hjelmslev's Principle states ;

"La description doit être non-contradictoire, exhaustive et aussi simple que possible. L'exigence de non-contradiction l'emporte sur celle de description exhaustive et l'exigence de description exhaustive l'emporte sur celle de simplicité."<sup>17</sup>

This principle applies both to linguistic theory and to linguistic description, as Uldall points out<sup>18</sup>, and following on from this Hjelmslev defines his Principle of Simplicity, as follows ;

"Si le calcul permet d'établir plusieurs procédés possibles conduisant toutes à une description non-contradictoire et exhaustive d'un texte et d'une langue quelconques, on doit choisir parmi ces procédures celle qui assure la description la plus simple. Si plusieurs procédures permettent des descriptions dont les résultats ont le même degré de simplicité, on doit choisir celle qui emploie la voie la plus simple."<sup>19</sup>

We will take the Principle of Empiricism first. There are three major objections to be made against it.



The first and, in some ways, the most important objection to this principle is that it is scarcely an empirical requirement at all (at least in the normal sense of the word "empirical"). Various writers have objected to Hjelmslev's principle on the grounds that it is oddly named.<sup>20</sup> This typically esoteric piece of Hjelmslevian terminology is certainly oddly named in that, although, supposedly, a Principle of Empiricism, it makes no reference to the empirical testing of descriptions, at least in the usual sense of the term "empirical", i.e. with respect to observable phenomena. One would normally be inclined to say that a description was "empirical", if its validity depended not merely on the definitions of, and permissible operations on, the terms involved but also on other external factors. The other factors involved are usually deemed to be the experience and observation of contingently existing phenomena. Thus the validity of an empirical statement would normally be held to depend on the relation between the content of a statement and the behaviour of existing observable objects, and the same goes mutatis mutandis for (empirical) descriptions. It would, therefore, seem reasonable to expect of a principle of empiricism that it require of linguistic descriptions empirical validity. That is to say that the condition should somewhere be embodied in an empirical principle that a linguistic description shall not be deemed

acceptable, unless it be valid with respect to the phenomena which it purports to describe. The only part of Hjelmslev's requirement which approximates to such a condition is the condition of "exhaustiveness". However, it cannot be maintained that a linguistic description will be empirically valid, in the appropriate sense, solely because it is exhaustive. Nor are the other conditions of "consistency" and "simplicity" of any assistance here, since no conjunction of these three conditions will ever constitute a sufficient test of the empirical validity of a linguistic description, at least in the usual and, to my mind, perfectly reasonable sense of the word. The point is that neither ("formal" or "internal") consistency, nor "exhaustiveness", nor a conjunction of these two, will ensure that a linguistic description is either empirically interpretable or, when empirically interpreted, consistent with respect to the set of describienda.

What makes the situation even odder is, of course, that Hjelmslev, as we have seen, explicitly requires that linguistic descriptions be empirically interpretable in terms of "les données de l'expérience". However, the situation is by no means as simple as it may appear at first blush. The point is that, in Glossematics, the Principle of Empiricism applies on



several levels including the level of "theory" or "general descriptive apparatus"<sup>21</sup> and, also, that linguistic descriptions are to be derived from<sup>22</sup> (deduced from) the general linguistic theory.

Hjelmslev would be on perfectly solid ground in applying his Principle at the level of theory, since, as he points out;

"La théorie elle-même ne dépend pas de l'expérience. Rien en elle n'indique si elle aura des applications en rapport avec les données de l'expérience ou non. Elle n'implique en elle-même aucun postulat d'existence. Elle constitue ce que l'on a appelé un système déductif pur."<sup>23</sup>

If this is so, and if the "empirical principle" applies to the theory, then, clearly, it would be self-contradictory to formulate the "empirical principle" in such a way that the theory was subject to empirical testing of the usual sort with respect to phenomena. Equally, if particular linguistic descriptions are to be derived from the theory, and if these descriptions were to be subjected to empirical testing, then the theory would itself be indirectly made subject to empirical testing and this would, of course, lead to exactly the same contradiction. Putting the matter another way we can say that Hjelmslev conceives

linguistic theory as a purely a priori construction, the validity of the statements of which depends solely on the definitions of terms and the permissible inferences within the theory. To introduce a necessary condition of empirical testing with respect to observable phenomena would be self-contradictory within the confines of such a theory. It follows that Hjelmslev's Empirical Principle would be, as a purely formal condition, entirely applicable to the level of theory. It is precisely a set of formal conditions which are enunciated in the Principle of Empiricism. The most important of these conditions are those of consistency and exhaustiveness. These two conditions assert, respectively, that the theory shall be deemed inconsistent, if it contains any two statements, or if any two statements can be derived from it, such that the one statement asserts or implies the negation of the other (i.e. if the theory asserts both p and - p) ; and the theory shall be considered exhaustive, if it is sufficient for the deduction of all necessary theorems required for linguistic description.

If it is correct to assert that the Principle of Empiricism is a formal condition of acceptability applicable to the level of theory, then one can see that the principle is all the more oddly named. On the other hand, one must observe that the principle is intended, in the first place, to be applicable to the level of description. If so, then, as we have



noted, the principle signally fails to be an "empirical" condition. Although one can and must be able to apply the formal conditions embodied in the Principle of Empiricism to descriptions as well, what distinguishes linguistic descriptions from linguistic theories is precisely the additional constraint on linguistic descriptions that they be not merely formally acceptable but also subject to empirical testing with respect to the phenomena. In failing to see this, Hjelmslev is committing a cardinal error and this brings us to our second objection against the Principle of Empiricism. This is that Hjelmslev fails to distinguish linguistic theory from linguistic description and this leads him to subject both to the same conditions of acceptability, whereas formal conditions alone are insufficient for the testing of linguistic descriptions. If, however, linguistic descriptions are to be deduced from linguistic theory, then it becomes impossible within a glossematic framework, as we have said, to apply without inconsistency tests of the empirical validity of linguistic descriptions with respect to phenomena. This would mean that the absence of such empirical testing in glossematics was a fault in principle due to the failure to draw the distinction between linguistic theory and linguistic description. That Hjelmslev has failed to draw this distinction is particularly evident, when he remarks that,

"On peut donc décider de la valeur de la théorie du langage et de ses applications en vérifiant si le

résultat, tout en répondant aux exigences de non-contradiction et d'exhaustivité, est en même temps le plus simple possible.

"C'est donc seulement par rapport au principe d'empirisme' qu'elle a énoncé que la théorie du langage doit être jugée."<sup>24</sup>

The assertion that linguistic theory and its applications (= descriptions) are equally subject to the Principle of Empiricism without the further qualification that the descriptions of particular texts must be subject to empirical testing is typical of the glossematic blindspot with respect to the empirical aspect of linguistics and shows particularly acutely the absence of a clear distinction between theory and description.

We can make the notion of subjecting linguistic descriptions to empirical testing in confrontation with phenomena clearer through a consideration of the hypothetico-deductivism of an epistemologist like Popper. This brings us to the third point concerning Hjelmslev's empiricism. Whereas Hjelmslev is typically concerned with the testing of linguistic descriptions, Popper is concerned with the attempt to refute them. The point is that when we are concerned with the



description of infinite classes and infinite sub-classes of phenomena, as is the case in linguistics, it is clearly the case that no absolute verification of any given linguistic description of those phenomena is even in principle possible. However, if we can show that a universal, empirical hypothesis referring to a potentially infinite class of speech phenomena is false in at least one valid instance or set of instances, then this will be sufficient to establish the empirical invalidity of the description in question. As Popper says,

"It must be possible for an empirical system to be refuted by experience."<sup>25</sup>

The criticism which one would have to make of Hjelmslev's Principle of Empiricism is that this principle, being essentially formal in nature, does not give us any possible way of achieving the empirical refutation of a glossematic linguistic description and, consequently, one would have to say that the principle is simply insufficient as a condition of the acceptability of linguistic descriptions. In order to make it sufficient for this purpose it would be necessary to introduce a method of potential empirical refutation.

Given these faults in the Principle of Empiricism, it is quite clear that the Principle of Simplicity, as a subsidiary principle, cannot save the position, since it is not an empirical condition of the sort

required. In fact, the Principle of Simplicity comes down to a head-count of descriptive objects and operations. Unfortunately, a reduction in the number of objects in a description generally leads to an increase in the number of operations, as Fischer-Jørgensen has pointed out, and thus "simplicity" in one area of the description, e.g. in the number of taxemes, may be offset by complications in other areas, e.g. realisation statements. In any case, such a strong use of the Principle of Simplicity as we find in Glossematics has the danger that considerations of convenience may override empirical testing and this seems to have been what was at stake in the discussion of "underlying elements" in glossematics. The "underlying element", /n/, was set up in French in a post-vocalic, syntagmatic position in the description, where it did not always correspond to a separate choice in the chain. The phoneme, /n/, combined with a preceding vowel was to be realised as (part of) a single nasalised phoneme. Thus, [bõ] was rendered phonemically as /bon/. This leads to a reduction in the number of taxemes and is useful in relating such allomorphs as "bon" and "bonne", for example, which were phonemically /bon/ and /bonə/ respectively, where in the feminine /n/ is prevocalic and so does not combine with /o/.<sup>26</sup> As we can see, phonological realisation statements are complicated by this solution. However, the analysis is convenient. Nevertheless, the questions remain whether it is consistent with the theory (can two syntagmatic elements



correspond to a single choice in the chain ?) and what empirical tests are available to examine the solution of convenience ? It is precisely this kind of question that Hjelmslev seemed unable to answer and which the Principle of Simplicity obscures.

As is well known, Hjelmslev's approach was highly abstract. He often takes the view that a linguistic description should be a kind of abstract algebra of linguistic entities. In this he seems to be thinking of the abstract calculi of logic and, one might suggest, he appears to adopt in his Principle of Empiricism a version of the "coherence theory" of truth from the philosophy of logic. This theory of truth holds that the truth of a logical system depends on its consistency and completeness<sup>27</sup> (exhaustiveness). This theory of truth is not normally held for empirical theories, where a "correspondence theory" of truth seems more applicable. A "correspondence theory" of truth, such as we adopt for linguistic descriptions here, requires that the truth of an empirical system depends at least in part on the observation of phenomena and their relation to the statements of the description. As we have seen, it is precisely this sort of requirement which is absent from the Principle of Empiricism. We have also seen that there was some confusion in glossematics between the general theory

and the empirical description (which was only rarely seen as distinct from the general theory). It may be that Ejelmslev applied the coherence theory of truth to both general linguistic theory (which was reasonable) and to empirical descriptions (which was not reasonable). The Principle of Simplicity was then invoked simply to overcome the deficiencies caused by the lack of a genuine empirical criterion.

b. Chomsky's views on "adequacy"

Chomsky's main contribution to linguistic meta-theory comes in a general argument in favour of a theory of generative grammars and against what he calls "taxonomic" models in linguistics and falls into four main parts, in which he describes ;

(a) the central task of linguistics as he conceives it.

(b) the nature of taxonomic and generative models in linguistics.

(c) the "levels of success" to be achieved by grammars.

(d) the application of evaluation criteria in specific instances.

Chomsky considers that ;

"The central fact to which any significant



linguistic theory must address itself is this : a mature speaker can produce a new sentence of his language on the appropriate occasion, and other speakers can understand it immediately, although it is equally new to them....normal mastery of a language involves not only the ability to understand immediately an indefinite number of entirely new sentences, but also the ability to identify deviant sentences and, on occasion, to impose an interpretation on them".<sup>28</sup>

This leads Chomsky to conclude that, "a theory of natural language that neglects this "creative" aspect of language is of only marginal interest".<sup>29</sup> Chomsky means by "linguistic theory", "systems of hypotheses concerning the ~~general~~ features of human language put forth in an attempt to account for a certain range of linguistic phenomena".<sup>30</sup> The range of linguistic phenomena which must, in the first instance, be accounted for is this "creative aspect" of language. Consequently, a linguistic theory, from Chomsky's point of view, may be regarded as (principally) a system of hypotheses accounting for the creative aspect of language. As this "creative aspect" of language is explained (above), it clearly refers to the capacity of an individual speaking a particular language, since Chomsky defines this "creative aspect" as part of "normal mastery of a language". It would seem that, on this account, it is not human language which is creative but particular

languages. Now, Chomsky tells us that linguistic theory is concerned with the general features of human language. It follows that the problem of the infinite creativity of particular languages (which may be taken as a characteristic of languages in general) may be posed in two opposed ways. Either we may say that, since each language is infinitely creative, each particular language may be regarded as an instantiation of the general features of human language ; or, we may say that each language is infinitely creative and the means by which this infinite extension is attained, are language specific. The first of these two alternatives involves an inference which is clearly not a valid deduction ; the mere fact of the infinite extension of all natural languages does not imply that this infinite extension is achieved as a reflection of any general features at all. The second alternative is not a valid deduction either, since the mere fact that one cannot assume or deduce the validity of the notion of "the general features of human language" does not imply that the means by which this infinite extension is achieved are language specific. Since neither of these formulations is acceptable, one must conclude that we are left with the trivial observation that all natural languages are of infinite extension, unless further evidence can be given to support one position or the other. The point has been made by Hjelmslev and B<sup>u</sup>hler (among others) and leads directly to what Martinet calls the notion of "linguistic economy".



The importance of linguistic economy lies in the observation that the infinite diversity of natural languages is attained not in an ad hoc manner nor by a simple infinite list of all the signs of a language but by the use of a finite inventory of items which are capable of being manifested in infinitely many constructions in combination with other entities. Martinet would, no doubt, go on to argue that a natural language requires complexity of this sort on two ontological levels of phonology and grammar in order to achieve the maximum "economy". It can be argued, at any rate, that some principled method must be found by means of which the infinite extension of any natural language may be accounted for in terms of a finite set of statements. Such a principled method will constitute a theory for the description of linguistic phenomena in a uniform manner. The uniformity in question does not imply any assumed universalism but is a necessary condition of the comparison of particular linguistic descriptions. One must be careful to distinguish those aspects of a description which are imposed by the theory and those which are peculiar to the linguistic phenomena under consideration. Any theoretical category, such as "phoneme" or "transformational rule" will, of necessity, be universally instanced, if the descriptive framework of the theory requires that any description contain such categories. On the other hand, the particular set of phonemes or transformational rules within a linguistic description will depend on the phenomena under description, if we are not to beg the question. ~~The descriptive method is implied by~~

a desire to compare linguistic structures.

This point, however, brings us back to one made earlier, that only grammars of the same type may be evaluated by comparison one with another. It is this point that forces Chomsky to attempt to reconcile two incompatible interests. On the one hand, Chomsky must produce two inherently different models of linguistic description for comparison, viz. the taxonomic and the transformational-generative. On the other hand, no evaluation of grammars is possible, unless these grammars are of inherently the same type, since the criteria of evaluation may be applicable to one type of grammar but not the other.

The second section of Chomsky's argument involves a characterisation of the two models and the attempt to show that the taxonomic model may be formulated as a generative grammar. In this way the models are supposed to be rendered comparable in that each contains a generative phrase-structure component. It seems to be assumed that the only grammars worth considering must be generative grammars, since only generative grammars, so the argument goes, account for the infinite creativity of natural languages.

Now, it has already been argued that "generativity" is a trivial consequence of a scientific structural grammar, i.e. one which aims to make predictions about the describiendum that are empirically testable. Viewed



from a different point of view, then, a structural description may be considered generative. Some structural approaches, e.g. glossematics, contain an explicit and specific framework leading from the highest hierarchical entities to the "phonetic-noetic" consequent hypotheses implied by them. This descriptive framework is set up, in the case of glossematics, as a structure of contextually selected classes. It is not a set of algorithmic processes. What is objectionable, then, about Chomsky's argument is not his contention that structural grammars may be viewed as generative grammars but the impoverished way in which he describes the generative aspect of those grammars.

Chomsky remarks that, "it should be noted that modern grammars are typically not conceived as generative grammars, but as descriptive statements about a given corpus (text). Hence, the taxonomic model.... is no more than an attempt to formulate a generative grammar, which is in the spirit of modern procedural and descriptive approaches"<sup>31</sup> If it is true that structural grammars or taxonomic grammars are not generative grammars, then there is no basis for the evaluation of structural and transformational grammars in any way, let alone the way Chomsky suggests, since Chomsky's evaluation procedures concerning levels of adequacy explicitly presuppose that one is dealing

only with generative grammars. This would mean, however, that structural and transformational grammars could not be compared and that, therefore, Chomsky's argument in favour of transformational grammars collapsed. The evaluation procedure would simply not be an independent test. It would say, in effect, that a certain grammar was preferable to another if it was a generative grammar while the other was not. The real question is why anyone should accept such a test. However, it is clear from the above that all structural grammars can be viewed as generative grammars (although they might not take the form Chomsky suggests). However, if it is the case that,

"It is clear that the question of explanatory adequacy can be seriously raised only when we are presented with an explicit theory of generative grammar, that specifies the form of grammars and suggests a mechanism for selecting among them (i.e. an evaluation procedure for grammars of a certain form",<sup>32</sup>

then Chomsky's criteria for comparing levels of adequacy of structural and transformational grammars will simply lapse. It is no reflection on either the structural or transformational model that they cannot be compared in terms of Chomsky's levels of adequacy. But it is a reflection on the criterion in question, if it demands the fulfilment of conditions which cannot in principle be met.



In fact, then, it is simply not the case that structural grammars are not trivially generative. Secondly, some are explicitly generative. Thirdly, the whole burden of Chomsky's argument in favour of transformational grammars against structural grammars falls upon the acceptance of Chomsky's formulation of the taxonomic grammar and the acceptance that the taxonomic grammar is the inevitable form of any (generativised) structural grammar. If the taxonomic model can be shown to be the inevitable form of any structural grammar when it is viewed as a constructional-generative device, then, and only then, will Chomsky's argument hold. The reason for this is quite simply that, unless Chomsky's formulation of taxonomic grammar is accepted as the generative version of some or all structural grammars, no comparison between the two types of grammar (in terms of Chomsky's evaluation procedures) can be considered in any way meaningful.

However, it is not clear why structural grammars, when turned into generative grammars, must take the form of the taxonomic model. It may be, for instance, that a structural grammar could generate all and only the strings of a language through the application of set theory or the propositional calculus, let alone all the more recently developed techniques of systemic grammar, stratificational grammar and others in which

some of the logical methods mentioned are actually used.

Chomsky characterises the taxonomic model in the following way,

"The taxonomic model is simpler, more "concrete" and more "atomistic" than the transformational model. We can characterise it in the following way. Each rule is of the form : element A has the member (variant, realisation) X in the context Z \_ W. Let us call such a rule a rewriting rule.. The syntactic component consists of an unordered set of rewriting rules, each of which states the membership of some phrase category or formative category in some context. The structural description it provides can be regarded as a labelled bracketting of the string of formatives, indicating the category of each sub-string, which is a constituent. Let us call such a labelled bracketting, obtainable automatically from a single derivation, a phrase-marker of this string of formatives. The phonological component consists of two distinct sets of rewriting rules. The first set (morpho-phonemic rules) states the phonemic constitution of morphophonemes or formatives with respect to stated contexts. The second set (phonetic rules) states the phonetic constitution of phonemes with respect to stated contexts. Each of these sets is unordered".<sup>33</sup>

This formulation is implausible from several points of



view. Firstly, and most significantly, Chomsky is dealing in a mammoth generalisation here. He clearly has no particular theory or grammar in mind when setting up this taxonomic model. It seems, therefore, to refer to every and no structural grammar in particular. The first point in this respect is that very few linguists would be interested in a description which did not go beyond the confines of some "corpus", as Chomsky puts it. In fact, such a grammar could not account for the infinite extension of a language by definition. Secondly, Chomsky gives no particular reason why a taxonomic model must be of this form. In fact, it can be argued that this form of algorithmic approach is inadequate. It contains, for instance, no semantic component, no statement of the types of syntactic and phonological distributional structure, which determine the structure of particular phrase-markers. (Yet this is the very point of structural linguistics.) It contains, furthermore, no specification of the structure of phonological complexes in terms of distinctive features or statement of intonational structure and it contains no inventories of the units entities which, at any particular level, may enter the syntactic or phonological structures defined for the language in question (there are no paradigms to go with the structures). In short, the syntagmatic and paradigmatic aspects of the description which are the core of any structural grammar are simply ignored.

We can see that structural linguistics, on Chomsky's account, when reduced to the taxonomic model, adds up to no more than statements of realisational variance in grammar and phonology. The average structuralist might well be forgiven for thinking that his theory was a good deal more subtle than Chomsky's taxonomic model.

Another very important point which is, in fact, the corollary of the above criticism is that the phrase-structure grammar, presented as the taxonomic model, is largely confined to statements of realisation on the syntactic, morphophonemic and phonetic levels. Although such statements of realisational variance are necessary in relating hypothetical entities to classes of observed phenomena, the explicit formalisation of realisational statements is of only marginal interest to the structural grammar. It is required that any descriptive hypothesis be ultimately testable in confrontation with some observable data via its realisational statements ; it is not required, or even very interesting, that this can or should be done by means of some formal explicitly generative apparatus, although a true descriptive framework, as one finds in glossematics, will indeed lead from the most abstract descriptive hypothesis to the statements of realisation. The main point is, however, that the taxonomic model



is greatly impoverished, if it is to be concerned solely with statements of contextual variance, which, in any case, presuppose the structural descriptions involving distribution, syntactic and phonological structures, etc..

It must be pointed out, furthermore, that it is not necessarily the case that a structural description will be interested in "labelled bracketting" (it may be a dependency grammar) or "morphophonemic rules" (it may not contain the notion "morphophoneme"). Lastly, many structural approaches require an ontological distinction between the levels of grammar and phonology, which is simply not catered for in Chomsky's formulation.

There are, then, many reasons why Chomsky's formulation of the taxonomic model is unacceptable and it is hard to accept that a generative view of structural models leads inevitably to the taxonomic model. It is scarcely surprising, then, that, on the one hand, Chomsky finds the model inadequate or that, on the other, he can find no-one who actually maintains such a position. Further pursuit of this question must be, in view of this, strictly speaking otiose. However, the widespread conviction that this argument and arguments of its type demonstrate the inadequacy of structural approaches to linguistics requires us to go a little further into the question.

Chomsky introduces three levels of "success" for grammars. These are the levels of "observational", "descriptive" and "explanatory" adequacy. "Observational adequacy" is the lowest level of success and "is achieved if the grammar presents the observed primary data correctly".<sup>34</sup> "Descriptive adequacy" is achieved "when the grammar gives a correct account of the linguistic intuition of the native speaker and specifies the observed data (in particular) in terms of significant generalisations that express underlying regularities in the language".<sup>34</sup> Finally, "explanatory adequacy" is achieved "when the associated linguistic theory provides a general basis for selecting a grammar that achieves the second level of success over other grammars consistent with the relevant observed data that do not achieve this level of success".<sup>34</sup> In such a case, "we can say that the linguistic theory in question suggests an explanation for the linguistic intuition of the native speaker. It can be interpreted as asserting that data of the observed kind will enable a speaker, whose intrinsic capacities are as represented in this general theory to construct for himself a grammar that characterises exactly this linguistic intuition".<sup>34</sup>

Now, supposing anyone were foolhardy enough to defend the taxonomic model, is there any reason why he should accept these "levels of adequacy" as relevant tests? Certainly, Chomsky suggests none and they each call for considerable comment.



In the first place, it is far from clear what is meant by presenting the observed primary data "correctly". Would "observational adequacy" be achieved by, for instance, a tape-recording? Or, perhaps, a phonetic protocol? In any case, it seems that the "achievement" of observational adequacy could be attained by something that was not even a grammar, let alone a generative grammar, purporting to account for the infinite extension of some language. Equally, it is not clear what is meant by "a correct account of the linguistic intuition of the native speaker". How, one may ask, is this correctness judged? Whilst one might test whether a specific hypothesis in a grammar corresponds to observed utterances or generally agreed utterance types, one could not conclude from that that the native speaker's intuitions were "correctly" described. One would be dealing in different orders of data. One could, of course, simply require native speakers to intuit in order to test hypotheses for correctness but, whilst a native speaker's judgment may be relied on when it comes to testing particular sentences for acceptability as surface structures (as particular utterance types), the claim of descriptive adequacy could not be tested in this way.

One should also make the point that there is no particular reason for the apparent assumption that expressing significant generalisations about a language has anything at all in common with describing the intuitions of the native speaker. Only under the

previous idealisation could this position be maintained but then, of course, this would be tantamount to defining a language as the intuitions of the idealised native speaker of that language. This is not only circular. It suggests the redundancy of the notion of the "idealised native speaker".

The conclusion of this argument is, therefore, that descriptive adequacy and, hence, explanatory adequacy are not attainable even in principle, since, in order to be descriptively adequate, a grammar must give "a correct account of the linguistic intuition of the native speaker" and this correctness not only falls outside the scope of empirical linguistic methods, it can in no way be attained. It follows that the tests of adequacy proposed by Chomsky would be unacceptable to the defender of the taxonomic model, or anyone else.

The fourth stage of Chomsky's argument involves both a demonstration of how the levels of adequacy are put into operation as tests and a proof of the superiority of transformational grammars over taxonomic grammars with respect to these levels of adequacy. Both sides of this stage of the argument rely on the treatment of specific examples. It is clear, however, for anyone who maintains the distinctions between theory, descriptions and phenomena, that arguments invoked directly from the treatment of particular phenomena in particular descriptions presuppose the application of the theory



to the data in question. Such arguments cannot without circularity be used to support the theory which is presupposed. Furthermore, the validity of a description (grammar or particular descriptive solution) can be only a necessary condition of the validity of the theory (see below). It can never be a sufficient condition of the validity of the theory that some description made using that theory as a basis of analysis "correctly" accounts for data (i.e. the description is not refuted). If this is the case then one may draw two consequences. In the first place, only descriptions made under the same theory can be compared directly for relative adequacy and, in the second place, theories cannot be compared indirectly via descriptions made under them. The reasons for these two assertions are quite simple. Given that two solutions employing different theories are equally consistent with both the data and their respective theories, the adequacy of each description is a necessary condition of the validity of its respective theory but the comparative adequacy of one description with respect to another is neither sufficient nor necessary for the validity of either theory. One theory's failure is not another theory's success. At best, comparative adequacy could be a measure of the relative elegance of the solutions and this might give a higher evaluation to one theory over the other (or it might mean that one linguist had simply done a neater job than another). However, it must be borne in mind that each description implies the theory - since each des-

cription is proposed as an application of a theory to certain data. Consequently, since each descriptive statement in each description is justified at least in part by a set of theoretical statements, which is disjunct from each other set of theoretical statements (theories), which separately justify other descriptions (perhaps of the same data), it follows that particular (sets of) descriptive statements under different theories are non-comparable in this respect. So, theories cannot be compared indirectly via their respective applications. On the other hand, it is a necessary condition of the validity of any description that it be the most adequate under the theory in question with respect to the data in question. Furthermore, competing solutions under a single theory are justified with respect to the same set of theoretical statements. It follows that competing descriptions under the same theory contain comparable statements. It follows from this that only descriptions under the same theory can be compared for relative adequacy, since only such descriptions are theoretically comparable. It also gives us another reason for saying that theories cannot be compared indirectly via their applications, since the descriptions involved imply different theories. We maintain that theories may be compared directly and that they must be compared with respect to fields of phenomena. (see above).

It has been assumed throughout that we are dealing



different theories (i.e. disjunct or partially disjunct (overlapping) theories), or the same theory. There remains the case of the proper inclusion of one theory within another (i.e. the class of theoretical statements in the one theory is properly included within the class of theoretical statements of another). Given that the two theories are equally consistent, the theory with the larger extension will be preferable, if it is totally applicable to some aspect of the data, to which the theory with lesser extension is in principle incapable of application, provided that the data which the theory is extended to cover constitute valid primary observations and provided that the additional scope of the theory implies no contradiction with the properly included class of theoretical statements. (If this last condition is not met, either the theory as a whole is inconsistent or the including theory must be modified. If the theory is inconsistent, criteria of adequacy are irrelevant and, if the theory is changed, we may speak of the original and new theory as disjunct.

It is clearly proper inclusion that Chomsky has in mind when he compares taxonomic and transformational grammars. The transformational component may be viewed as additional to a phrase-structure generative grammar. The transformational component gives greater scope and elegance to grammatical description and the transformational-generative model properly includes the generative taxonomic model. Insofar as no-one

actually defends or is required to defend the taxonomic model, the argument seems to be against a cardboard opponent. However, it is important to see that even the arguments in favour of a "transformational component" are fallacious and that the levels of adequacy that the transformational model satisfies are such that

- a) only a transformational grammar could meet them
- b) they are irrelevant to most structural grammars and
- c) they rest on unsupported assumptions of an unacceptable nature.

These points are particularly evident when we come to consider Chomsky's syntactic examples. For instance, Chomsky asks us to consider the sentences,

(3) John is easy to please.

(4) John is eager to please.

(Chomsky's numbering). Chomsky tells us that these sentences are wellformed and goes on to claim :

"A grammar that achieves only the level of observational adequacy would merely....note this fact in one way or another (e.g. by setting up appropriate lists). To achieve the level of descriptive adequacy, however, a grammar would have to assign structural descriptions indicating that John in (3) is the direct object of please (the words are grammatically related as in "this pleases John"), while in (4) it is the logical subject of please (as in "John pleases someone"). A



theory of grammar that does not allow structural descriptions of this sort cannot achieve the level of descriptive adequacy, since information of this kind cannot be represented in the phrase-marker that it provides as the full structural description on the syntactic level."<sup>35</sup>

The implication is clearly that a transformational-generative model can achieve this level of adequacy, whereas a taxonomic model of the phrase-structure sort cannot. Whilst this is no doubt true, it is rather trivial, since no-one actually defends the taxonomic model. This is far from all, however.

In the first place, one might well wonder why observational adequacy could be achieved by a grammar that "set up appropriate lists". Such a description could be on many levels, phonetic, phonemic, morphemic, etc., and Chomsky does not make clear which. Such a description would be infinite and, as Chomsky himself maintains, "one requirement that a grammar must certainly meet is that it be finite...hence the grammar cannot be simply a list of all morpheme (or, word) sequences, since there are infinitely many of these".<sup>36</sup> It seems that a description that simply set up "appropriate lists" would not be a grammar at all. On the other hand, it is difficult to imagine anyone so lunatic as to go around making lists of utterances and calling himself a grammarian. The criterion of observational

adequacy is, on this showing, quite irrelevant.

As far as the question of descriptive adequacy is concerned, as it is outlined here, one can only say that it begs various questions. In the first place, it is not clear why it is a necessary question of descriptive adequacy that a grammar should assign structural descriptions indicating that John is the direct object of please in (3) and the logical subject of it in (4). Since we are given no definition of either direct object or logical subject, the allocation of John to these categories must be taken on trust. Furthermore, the criterion of descriptive adequacy, which utilises this notion, clearly presupposes that any grammar should represent this information. What Chomsky never tells us is why this should be so. One would like to enquire why data about grammatical relations should be treated simultaneously and on the same level as data about logical form. The contention, that any grammar which failed this criterion of descriptive adequacy by failing to represent the desired information in the phrase-marker must be rejected, merely reinforces the unacceptably a priori assertion that this information should be represented. Any taxonomic linguist who rejects the simple a priorism of unjustified and undefined categories such as those borrowed wholesale from traditional grammar by Chomsky will simply ignore the criterion of descriptive adequacy on the grounds that it is irrelevant



and involves an unjustified assumption. The non-transformationalist could go on to argue that the transformationalist, by dealing in non-linguistic data, was simply not doing linguistics. On the other hand, it would be possible for Chomsky to maintain that taxonomic grammars are reducible to phrase-structure grammars and, since phrase structure grammars utilise these categories of description, the objection still holds. Unfortunately, Chomsky never makes this "reduction" clear and any such "reduction" would lead to further problems. If taxonomic grammars are now taken to be equivalent to traditional grammars (by some process of reduction), which is a condition apparently required by Chomsky's argument, then the only bone of contention between the structuralist and Chomsky involves the scope of linguistic descriptions. (Actually, this must be so, if the transformational model properly includes the taxonomic model.) The difficulty is, of course, that of finding some reason why we should think that taxonomic grammars constructed under structuralist theories were equivalent to traditional grammars either in descriptive scope or in the set of theoretical statements of each (insofar as traditional grammar is a theory at all). On the other hand, if the taxonomic model is equivalent to the traditional grammar model, then Chomsky contradicts himself to the extent that he contrasts the two. As far as "explanatory adequacy"

is concerned, Chomsky tells us ;

"To achieve this level, the theory must provide for the selection of a descriptively adequate grammar, given such data as (3), (4), "John's eagerness (\*easiness) to please...", "to please John is easy (\*eager)", "John is an easy (\*eager) fellow to please", "it pleases John", "John pleases everyone", "John is easy (\*eager) for us to please", "it is easy (\*eager) to please John", "John is a person who (it) is easy to please...and many other similar and related structures.

"The general theory would have to make possible the formulation of the underlying generalisations that account for this arrangement of empirical data...."<sup>37</sup>

Chomsky would no doubt claim that only transformational-generative grammars are capable of achieving the explanatory level of adequacy and that this shows the superiority of the transformational approach over the taxonomic. Unfortunately, it is quite clear that, if the criterion of explanatory adequacy requires the formulation of grammars with underlying generalisations about the data of the sort quoted, then the proof is circular. The reason for this is that only a transformational grammar would attempt to relate these disparate structures and only a transformational approach would countenance evidence from syntactically totally unrelated structures as decisive in the description



of some string. It follows that Chomsky claims the superiority of transformational grammars on the grounds that they are explanatorily adequate ; unfortunately, it is clear that the criterion of explanatory adequacy states, in effect, that a grammar achieves explanatory adequacy, if it is a transformational grammar.

In fact, the only reason for choosing to treat all this data under one rubric stems from the a priori analysis of it into the categories of traditional grammar, for only then can one determine that the various instances of John or any other item perform analogous roles within the sentences in question. It follows that transformational analysis without the presumed categories of traditional grammar cannot exist (as one would expect, since the transformational model properly includes the formalisation of traditional grammar that one finds in the phrase structure model). The point is, however, that, in this set-up, no transformational grammar can refer directly to primary data, since the data relied on by a transformational grammar includes a traditional grammatical analysis. (Otherwise there is no justification for treating the various data above under the same or related structures.) In other words transformational grammars refer to quite different aspects of data from the data referred to by structural grammars. Secondly, the scope of the trans-

formational grammar is acceptable only, if one is willing to accept the vague and intuitive classifications of traditional grammar. Whereas a transformational grammar may properly include a formalised traditional grammar, it certainly does not properly include any structural grammar, since structural grammars typically refer directly to classes of "phonetic-noetic" complexes specifically without the mediation of intuitive notions of schoolbook grammar. The notions which mediate in a structural grammar are those theoretical notions which are applied in constructing empirical descriptions drawn from the structural theory in question. Chomsky's appeal to independent empirical evidence is illusory since the evidence has already been modelled in a way which favours transformational grammars. Structural and transformational grammars are, therefore, disjunct and non-comparable, except through the comparison of structural and transformational theories. Just what will not wash is the idea that structural grammars can be simply reduced to taxonomic grammars and be dismissed as inadequate. Even if such a reduction were possible, the additional scope of the transformational grammar is acquired at the price of making the grammar unacceptable.

c. Lyons' descriptive adequacy

Chomsky's conditions of adequacy may be rejected



as little more than an attempt to justify transformational grammar at the expense of other approaches. One cannot dispense with a criterion of adequacy altogether, however. Clearly, adequacy in some sense is a necessary condition of the validity of theories and descriptions. In general, an appropriate definition of adequacy must take into account both the theoretical and empirical issues involved.

This appears to be what Lyons means when he sets up his conditions of "operational" and "material" adequacy. Lyons' actual formulation of conditions of adequacy may be regarded, in some respects, as somewhat unfortunate but it is a step in the right direction. Lyons tells us that a linguistic theory (in his case a semantic theory within linguistics) "must employ concepts that are operationally definable in terms of empirical techniques"<sup>38</sup>. This is operational adequacy. Lyons' condition is unexceptionable, if it is taken to mean that descriptions under the theory in question must refer to observables or be reducible to observables and that the descriptions must be empirically testable by clear-cut and unambiguous means. However, it could not be maintained that the concepts of the theory could be defined in terms of empirical techniques, although the theory must contain a methodology which is consistent with the concepts of the theory and applicable to empirical phenomena. Moreover, a linguistic theory

may be regarded, from a Hjelmslevian point of view, as appropriate but arbitrary. It follows that the concepts of the theory are arbitrary in having no empirical content although having empirical application as a product of their appropriateness. The concepts of the theory would not, then, be empirically definable by any means. What may be maintained is that, given some theoretical category, it must be the case that there is available for that category both some clear definition of it and some co-ordinated methodology such that the category in question takes its place in the theory and may be applied to data in such a way as to produce descriptive solutions testable via empirical means (see below).

In fact, without some method of testing descriptions against observables for the purpose of the attempted refutation of the descriptions, the mere reference or reduction to observables is an insufficient condition of operational adequacy.

Unfortunately, Lyons' account of material adequacy may be criticised on two counts of irrelevancy. Neither the condition that a linguistic theory "is to that degree more adequate, if, based on operationally definable concepts, it gives results which are in significant agreement with the native speaker's feelings about his language",<sup>39</sup> nor the condition of "saving the



appearances" (i.e. "compatibility with the known or apparent facts of language learning and language use")<sup>40</sup> can be regarded as a sufficient or necessary condition on the validity of theories. Of course, any linguistic description must be compatible with the known facts of language use in a given community but, then, it is these facts that a linguistic description purports to describe (i.e. it boils down to operational adequacy). While the facts of language learning seem to be simply irrelevant to a linguistic description of a language state, the requirement of agreement with the feelings of the native speaker seems bizarre. It is comprehensible if it merely means that no description should be contrary to fact in a way which is recognised as predicting utterances which are unacceptable to native speakers - but this is operational adequacy in a another form. However, if the description is required to capture in some way the feelings of the native speaker about his language (whatever these may be), Lyons seems to be moving out of the realm of the empirically observable. One may as well require that a physical theory be in significant agreement with the native earth-dwellers' feelings about his universe.

NOTES TO CHAPTER 2

1. See especially L. Hjelmslev, Prolegomenes.
2. See N. Chomsky, "Current Issues...", Syntactic Structures and Aspects of the Theory of Syntax.
3. See J. Lyons, Structural Semantics.
4. See S. K. Shaumjan, Principles and Filosofskije Problemy.
5. See J. Mulder, "Linguistic Theory,...", Sets and Relations, etc..
6. L. Bloomfield, Language, p. 20.
7. The influence of the Vienna Circle on Bloomfield can be seen clearly in his paper, "Language or Ideas ?" and through the work of his colleague A. P. Weiss; see Weiss' paper, "Linguistics and Psychology".
8. This seems to be what Hjelmslev meant by "induction", see Prolegomenes, pp. 46 - 7.
9. Z. S. Harris, Methods, p. 6.
10. Z. S. Harris, ibid., pp. 1 - 2.
11. Z. S. Harris, ibid., p. 1.
12. R. Harre, The Philosophies of Science, pp. 42 - 8.
13. C. Hempel, The Philosophy of Science, p. 11.
14. There are far too many works on this subject to name them all or even a representative sample but one might note, for instance, L. J. Cohen, The Implications of Induction and W. C. Kneale, Probability and Induction.
15. Again one can only select a couple from the many important contributions to this subject, such as



- G. H. von Wright, The Logical Problem of Induction and  
K. R. Popper, The Logic of Scientific Discovery.
16. L. Hjelmslev, Prolégomènes, p. 19.
17. L. Hjelmslev, ibid., p. 19.
18. H. J. Uldall, Outline, p. 21.
19. L. Hjelmslev, Prolégomènes, p. 29.
20. See, for example, A. Martinet, "Au Sujet des Fondements de la Théorie Linguistique de L. Hjelmslev", p. 36, E. Fischer-Jørgensen, "Review of L. Hjelmslev", p. 84, A. Nehring, "Die Glossematik", and B. Siertsma, A Study of Glossematics, pp. 38 - 9.
21. H. J. Uldall, Outline, p. 21.
22. H. J. Uldall, ibid., pp. 22 - 3.
23. L. Hjelmslev, Prolégomènes, p. 24.
24. L. Hjelmslev, ibid., p. 29.
25. K. R. Popper, The Logic of Scientific Discovery, p.
26. E. Fischer-Jørgensen, "Remarques", pp. 225 - 230.
27. Consider, for instance, Hjelmslev's claim that, "la théorie du langage ne peut être ni vérifiée ni infirmée par le recours aux textes...elle n'admet qu'un contrôle : la non-contradiction et l'exhaustivité du calcul", Prolégomènes, p. 29. This may be true for the theory, but is it true for descriptions ?
28. N. Chomsky, "Current Issues....", p. 50.
29. N. Chomsky, ibid., p. 51.
30. N. Chomsky, ibid., p.50.
31. N. Chomsky, ibid., p.53.
32. N. Chomsky, ibid., p. 64.
33. N. Chomsky ibid., p. 53.

34. N. Chomsky, "Current Issues, . . .", p. 66.
35. N. Chomsky, ibid., p.
36. N. Chomsky, ibid., pp. 66 - 7.
37. N. Chomsky, ibid., p.
38. J. Lyons, Structural Semantics, p. 1.
39. J. Lyons, ibid., pp 6 - 7.
40. J. Lyons, ibid., p. 7.



CHAPTER THREE

THEORIES, DESCRIPTIONS AND PHENOMENA

In the foregoing we have assumed throughout that one can legitimately maintain a distinction between "linguistic theories" and "linguistic descriptions". Such a distinction may seem intuitively self-evident. We cannot, however, rely on this intuitive self-evidence. It is the aim of this chapter to demonstrate the nature and necessity of this distinction in a precise fashion. The reason for this is that the distinction between linguistic theories and linguistic descriptions is of vital importance to the present discussion, since, without it, linguistic theory and linguistic description would collapse into a single entity and one would not be able to maintain a distinction between meta-theoretical conditions to be satisfied by theories and those to be met by descriptions, i.e. one would not be able to specify to what a particular condition applies. Also, one would not be able to distinguish the theoretical conditions to be met by descriptions from the meta-theoretical conditions to be met by descriptions. In view of the difficulties encountered by "two-level" approaches to linguistics (such as that of Shaumjan and others, see below), these consequences would be highly undesirable. As we shall see, a two-level linguistics, according to Shaumjan, is one which recognises a distinction between a level of scientific linguistic constructs and a level of linguistic phenomena, where the phenomena are modelled by the linguistic constructs. We shall argue that such a view (which is implicitly adopted by the majority of theoreticians)



is inadequate and that it is necessary to recognise a further distinction within the class of constructs between a level of theoretical constructs and a level of descriptive linguistic constructs. We call this latter view (adopted explicitly by axiomatic functionalists and implicitly by some others) a "three-level" linguistics. It is important to note that the distinction between linguistic theory and linguistic description is itself an important meta-theoretical matter.

a. Previous approaches to the distinction

The distinction between linguistic theory and linguistic description has been latent in the Prague School (and Neo-Prague School) and in the glossematic approach to linguistics without, however, the distinction having been made entirely clear and explicit until recently. Casting a brief glance over the history of linguistics, one can see that the attempt to set up a homogeneous and coherent theory which could be applied in the description of any set of speech phenomena was first made explicitly by the Prague School linguists in their famous "Projet de Terminologie Phonologique Standardisée"<sup>1</sup>. The improvements to this projected set of definitions and their corresponding notions suggested by Vachek<sup>2</sup> take the form of an implicitly deductive set of definitions. This theoretical work culminated in Trubetskoy's Principles of Phonology, in which, however, the systematically deductive

aspect of phonological theory is, perhaps, not so evident. The notion of linguistic theory as a deductive or axiomatic-deductive system of definitions seems to have been introduced to the Prague School by Bühler's Sprachtheorie<sup>3</sup> and his „Axiomatik der Sprachwissenschaften“<sup>4</sup>. One should not forget Bloomfield's ill-fated "Set of Postulates for the Science of Language"<sup>5</sup> in this respect. (A deductive system of definitions with application to linguistic description is, of course, not the same as a (hypothetico-)deductive description. Hypothetico-deductive linguistic descriptions can be said to begin, in an explicit manner, with Saussure's Mémoire).

A later Praguian, Martinet, implicitly drew the distinction between linguistic theory and linguistic description, when he wrote,

"Décrire une langue c'est proprement indiquer ce en quoi cette langue diffère de de toutes les autres langues connues, existantes ou possibles. Restera hors de la description tout ce que cette langue a nécessairement en commun avec toutes les autres langues."<sup>6</sup>

Clearly, to maintain that linguistic description is concerned with the properties which distinguish a given language from all others and that it excludes the properties necessarily common to all languages implies that we have some linguistic theory whose role



is to state the properties necessarily held in common by all languages. That is, one would distinguish between a level of descriptive constructs and a level of theoretical concepts. Such a theory would state the necessary and sufficient conditions for membership in the class of languages and would constitute a theory of language. One cannot set about describing languages, deductivist linguists of Martinet's ilk would maintain, unless one can recognise individual languages. A theory of language such as is implied (but never developed) by Martinet would allow us to recognise individual languages and would be logically distinct from particular descriptions of entities satisfying the necessary and sufficient conditions for membership in the class of languages. This position is adopted, albeit implicitly and sporadically, by the glossematicians. Uldall, for instance, writes,

"The Principle of Empiricism applies on several levels.....because the term "description" can be interpreted as referring to either a particular description of a particular object or the general descriptive apparatus".<sup>7</sup>

If we look at glossematic theory in these terms, we can see that this difference of interpretation corresponds to a potential distinction between linguistic theory (the general descriptive apparatus) and particular linguistic descriptions (of particular sets of

phenomena). It must be said immediately, however, that this distinction was never put to any use by the glossematians nor was it particularly well understood. Uldall goes on to say, for instance, that glossematic algebra "is a general description from which all particular descriptions, actual or potential, must be deducible"<sup>8</sup>. It is, in particular, not clear why the "general descriptive apparatus" should be sufficient to allow the deduction of all particular descriptions from it. It would seem more reasonable to suggest that the application of the descriptive functions contained in the general descriptive apparatus, to some field of phenomena should lead to (facilitate) particular descriptions, since one does not see how an arbitrary general theory (introduced a priori) can be expected to imply (materially ?) a set of possible (?!) empirical descriptions. Uldall might have meant that one can deduce abstract models from the theory, some of which are actually instantiated (although it is not clear what he did mean). This latter view is the one advanced by Hjelmslev<sup>9</sup>. Under this view a particular description would be a model of the general theory, rather than logically deducible from the theory. However this may be, one should note that neither Martinet nor the glossematians make a distinction between linguistic theory and linguistic descriptions in any consistent or regular manner, nor do they make out detailed arguments for such a distinction.



Their reasons remain of an implicit nature. They do not, furthermore, draw any of the consequences of adopting such a position.

b. The theory/description distinction in Axiomatic Functionalism

It has been left to Mulder<sup>10/11</sup> to give an explicit explanation of the distinction between linguistic theory and linguistic description. This distinction is closely related to the question of the status of linguistics as a science.

It would be generally agreed that linguistics may be defined globally as the scientific study of language.<sup>12/13</sup> This is the definition given by, for example, Lyons, Martinet and many others. In general, as we have said, the adoption of a "scientific" approach to linguistics implies the attempt to gain knowledge about given intersubjectively agreed phenomena. In order that this knowledge transcend mere "natural history" or random observation, one requires that the investigation of given phenomena be carried out on a principled basis, i.e. in accordance with some theory.<sup>14</sup> A theory, as we have suggested, may be regarded as providing us with a coherent and sufficient set of functions which constitute an analytical basis (as in axiomatic functionalism, standard functionalism, glossematics, (less formally) tagmemics and others). Now, we can call the presentation of acquired knowledge

about a given set of phenomena a "description".<sup>15</sup> Since one of the aims of linguistics as a discipline is here taken to be the acquisition of knowledge about given fields of speech phenomena<sup>16</sup> that is intersubjectively testable,<sup>17</sup> we may assert, along with Mulder, that it is an aim of linguistics as a discipline "to make possible the scientific description of any chosen field of speech phenomena".<sup>18</sup> In order to achieve this, as Mulder points out, "linguists have to provide a theory as a device...without such a device this aim cannot be achieved".<sup>19</sup>

It is a reasonable and a reasonably self-evident reflection that, in order to make a description of the sort in question, one requires both a set of phenomena and a principled way of describing those phenomena. It is for this reason that Mulder asserts that "any description presupposes both a theory and a field of phenomena".<sup>20</sup> The field of phenomena, the describiendum, in the case of linguistics can be agreed upon as an arbitrarily selected set of speech phenomena. The description may be regarded as "the application of a particular linguistic theory to a selected field of linguistic phenomena".<sup>21</sup> Now, it is clearly the case that there are as many fields of phenomena to which one can apply a given theory as one cares to delimit,<sup>22</sup> i.e. the selected fields of phenomena are virtually infinite in number. Since the theory may be applied to any of these fields of phenomena, or all of them



separately, it follows that there are correspondingly many descriptions in potentiality. We may say, then, that there is a one-one relation between a given field of phenomena and the corresponding description of that field of phenomena (as an application of the theory in question) ; however, there is a one-many relation between, on the one hand, the theory and the descriptions and, on the other, the theory and the fields of phenomena to which the theory may be applied. One may distinguish the notion "linguistic theory" from the notion "linguistic description" in the same way that one distinguishes a set of statements purporting to describe a given object from the terms in which one chooses to make the set of statements in question. Clearly, the general descriptive apparatus of a given theory may be applied to each set of phenomena separately and, thus, a given descriptive method (set of analytical functions) would be distinguished from the descriptions of particular fields of phenomena using those functions. "Is a phoneme", "is a moneme", etc. are analytical (propositional) functions of a theory. "/p/ is a phoneme" and "plural is a moneme" are descriptive statements about English, where it is stated that /p/ and plural meet respectively the conditions of the analytical functions "is a phoneme" and "is a moneme" of some particular theory (and /p/ and plural have specific empirical interpretations).

One important consequence of a rigorous application

of this distinction is that it leads to a clear-cut dichotomy between theoretical and descriptive statements. As Martinet suggests (above) it is the job of linguistic descriptions to state the particular properties of given fields of phenomena. The necessary properties of languages are excluded. Thus, if the double articulation is a necessary property of language then it will not be a descriptive term appearing in the description of any language - any given language will possess double articulation by definition. On the other hand, if /p/ is a phoneme of English, this will be a property of English and a peculiarity of that language not present by definition - thus, the statement, "/p/" is a phoneme of English", will be relevant in the description of that language but will not be a statement of linguistic theory. (In fact, the properties of /p/ depend on the particular features of English, which determine that it has the distinctive features, /labial, voiceless, stop/, as opposed to the phoneme /p/ in Russian which has the features /labial, stop, voiceless, unaspirated/.) No descriptive statement appears in the theory, because there can be no identity between the terms employed in the descriptive statements about different fields of phenomena - except the theoretical functions involved - as will be clear from the two descriptively different phonemes, /p/, in English and Russian. This position implies, for instance, that there can be no meaningful universal definition of, say, "aspect",<sup>23</sup> since what



we call "aspect" in English and, say, Russian is a quite different phenomenon, grammatically and semantically, in the two languages. ("Aspect" in English (in an axiomatic functionalist description) is a syntactic entity whereas Russian "aspect" is morphological.) This makes clear the view implicit in Martinet's statement (which we accept) that linguistic descriptions are concerned with those features which vary (or may vary) from language to language, whereas linguistic theory is concerned with the defining properties of language and those analytical functions which are required for linguistic description (leaving aside the question whether Martinet actually adopts this view). An important consequence of adopting this position is that it excludes the possibility of empirical linguistic universals.

The necessity of making this distinction in linguistics may be seen when we consider that it is related to the fact that there is a one-many relation between the theory and the fields of phenomena. Since the theory may be used as a device for, or is applicable in, the description of potentially an infinite number of fields of speech phenomena - and the fields of phenomena may be regarded as discrete from one another - it follows that no "theory" could be a theory or description of all fields of phenomena simultaneously. In popperian terms, the refutation class of such a theory would be "non-homotopic".<sup>24</sup> Consequently, it is necessary to set up a level of "description" distinct

from a level of "theory", such that each description is related to a single, homogeneous refutation class of "potential falsifiers". It should be clear from this that the notion of description, as used here, is similar to the notion of "empirical theory" as used by Popper. The major difference between the position adopted here and the position adopted by Popper is that, in the present "three-level" approach (involving separate levels of "theory", "descriptions" and "speech phenomena"), the level of empirical constructs (linguistic description) implies some further level of non-empirical constructs (linguistic theory) since a linguistic description is an application of some linguistic theory to a field of speech phenomena. Any linguistic description requires a conceptual apparatus in terms of which it is constructed. No such separate level of "conceptual apparatus" is implied in Popper's use of the term theory.

c. Comparison with the "two-level" view

Popper, along with any other defender of a "two-level" approach to the philosophy of science (involving two levels of "observable objects" and "empirical constructs"), would probably object to the use of the term "theory" as it is understood in the "three-level" approach advocated here on the grounds that a "scientific theory" (for Popper and others) should be confrontable with empirical data in order to test its empirical



validity or falsehood. Such a course is barred to us, if we set up the "theory" in a three-level approach in such a way that it is a purely logical construction incapable of experiential refutation or confirmation (following Mulder and Hjelmslev).<sup>25</sup> Mulder and Hjelmslev would be on strong grounds, however, in refusing to admit that theories introduced a priori could have empirical validity and in insisting that the construction of such theories is unavoidable (at least in linguistics). One should be careful not to confuse the essentially trivial terminological matter of the use of the term "theory" with the important question of whether the philosophy of science recognises one level of constructs or two levels, one of which is not an empirical level. There is no dispute between the parties that scientific systems require at least one level of empirical constructs. Defenders of the two-level view will have to be convinced that a level of non-empirical constructs is necessary and practicable. If such a level is necessary, we will require a considerable modification of the view of at least one science on the part of many people, viz. linguistics).

The above argument for a "three-level" approach to linguistics relies on the view that no linguistic description will be acceptable, unless it is theoretically justified, i.e. all such descriptions involve such defined predicates as "is a phoneme", "is a sign", "is a grammatical relation", "is a phonotagm", etc., none of which contains the name of an empirical object.

These expressions should form a coherent system (they should not be ad hoc). It seems to be the case that, in fact, we always assume a level of non-empirical objects (phoneme, sign, grammatical relation, phono-tagm, etc.) in setting up a linguistic description and the names of these objects appear in the theoretical predicates mentioned. This argument seems irresistible. It may be pointed out, in part explanation of the absence of the distinction in the philosophy of science, that the philosophy of science has been typically concerned with the logical and empirical sciences which deal with a single universe of phenomena. As Popper says,

"the system called "empirical science" is intended to represent only one real world : the 'real world' or the 'world of our experience'",<sup>26</sup>

whereas linguists have not been typically concerned with a single universe of phenomena. The argument for a "three-level" view of science is not based on any argument involving the notion of "multiplicity of logically possible worlds". The point is, rather, that Popper seems to have overlooked the possibility that a single science may be concerned with a potentially infinite number of real worlds of experience or, at very least, real universes of discourse, each of which requires discrete and separate descriptive treatment.



Such, as we have argued, is the position in linguistics, i.e. one is concerned with a potentially infinite set of possible fields of speech phenomena, which one aims to describe from a single, homogeneous point of view (using a single general descriptive apparatus). As Mulder has pointed out,

"Though, perhaps, in the natural sciences, it is unnecessary to distinguish sharply between theory and description, in linguistics it is imperative to do so. This is because linguistics is not concerned with the description of ONE universe - all speech phenomena (taken as a whole) - but with a virtually unlimited number of PARALLEL universes..."<sup>27</sup>

The importance of the present considerations lies in the fact that the case of linguistics is one where a "two-level" approach to the philosophy of science is inadequate and where it is necessary to distinguish between the business of empirical description and the task of constructing a theory to be applied in describing given fields of phenomena.<sup>28</sup> In general, as with Popper (above), philosophers of science have been concerned with disciplines dealing with one field of phenomena and so, as Mulder has said,

"Because any given natural science only deals with one universe, there is no overwhelming need to keep theory and description strictly apart."<sup>29</sup>

That is, the meta-theoretical distinction between theory and description is not of vital importance when dealing with a single universe of experience. One may wonder, however, whether this is entirely correct. In general, the investigation of some part of the universe is carried out in accordance with some, more general (perhaps, all-embracing) theory (e.g. quantum theory) or model. One may usefully distinguish between, say, the general theory of molecular structure, on the one hand, and particular investigations into the structure and properties of, for example, particular proteins on the other, where such particular descriptions presuppose the more general theory. One should also note that it is imperative to distinguish a theory and a model of a theory (as Popper<sup>30</sup> now does), where an abstract calculus (e.g. Lobachevskian geometry) receives an empirical interpretation or modelling (as in astrophysics). In such a case, the theory is not per se empirical, although the models of the theory - which show the applicability of that theory - must be. That is to say that, even in the case of the natural sciences, there seems to be good reason to distinguish a non-empirical level of constructs.

A similar position is adopted by Carnap, who abstracts "theoretical" predicates from "empirical" "observational predicates" (see below).<sup>31</sup> The distinction between a theory and a model of a theory (see below) is of great importance in overcoming the confusion



between the "deductive" and the "hypothetical" aspects of "hypotetico-deductive" approaches. For this reason also it is necessary to distinguish a level of abstract scientific constructs (see below for further discussion).

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What we have been saying is that linguistics differs from, at least, some other sciences in that it is concerned with more than one universe of phenomena. This is one of the facts that make it imperative to distinguish linguistic theory from linguistic description. We now look at some possible objections to this view. One way of attacking our position would be to maintain that the field of linguistic phenomena constitute a homogeneous set of explananda and that this set cannot be arbitrarily divided into discrete, separate fields of speech phenomena. This argument is irresistible if we regard speech phenomena in their entirety and ignore the fact that linguistics imposes its own point of view in selecting certain aspects of the phenomena as relevant. In particular, linguistics is concerned with speech phenomena as communicational tokens and with determining those conventions which "govern" communication in a given speech community. Given that this is the case, we maintain that the alternative position of viewing all speech phenomena as an undifferentiated set from the point of view of linguistics is unreasonable. That is, to maintain that the field of linguistic phenomena was a single universe of speech

phenomena would imply that the speech phenomena found in London and Peking, on the Left Bank in Paris and in Lagos, etc., constituted a single, homogeneous class of phenomena. This is clearly not a reasonable position to adopt, unless it is maintained that linguistic communication can be achieved by any two individuals each using any set of communicational tokens ; i.e. that a knowledge of the conventions of either Chinese or English is sufficient for a non-bilingual English speaker to achieve linguistic communication with a unilingual Chinese speaker. Although some form of communication might take place, it is obviously absurd to think that linguistic communication could be achieved. It would, in principle but absurdly, be possible to construct a simultaneous description of the speech phenomena of Chinese and English (although one fails to see the point of such an exercise, where communication is not achieved). It is self-evidently more reasonable and more useful to treat very different fields of phenomena separately. This is quite different from the situation prevailing in many other sciences, especially those with which the philosophy of science is principally concerned, although even in those sciences, perhaps, there may be room for a "three-level" view.

It may be maintained, of course, that not all fields of communicational speech phenomena are as



grossly different as are those of English and Chinese. There are undoubtedly marginal cases, where any division would be to some extent arbitrary. In describing the dialect of the Leeds-Bradford conurbation, does one include or exclude the speech phenomena of Pudsey? The difficulty of such a question is entirely illusory. Because the decisions as to the exact "range" of speech phenomena to be taken into account are inevitably arbitrary, one may say that it is equally possible to describe the communicational phenomena of Leeds-Bradford with or without the inclusion of Pudsey. In the deductive method of scientific description a linguistic description is projected onto a field of phenomena and may, thus, be empirically valid for that area of speech phenomena but not valid for a slightly larger area of speech phenomena. Thus, a description of the speech phenomena of the Leeds-Bradford area without Pudsey might well have to be changed in order to account for Pudsey speech phenomena. One cannot escape the conclusion, however, that the selection of one's field of phenomena is ultimately an arbitrary matter, although it is justifiably so, if we wish to account for grossly dissimilar fields of phenomena separately and avoid a cline of likeness from English to Chinese.

It is of vital importance to remember that this conclusion can be reached only if we allow linguistic theory to determine that linguistics is concerned, in the first place, with the communicational aspect of

speech phenomena. Consequently, we must revise our previous view that the "third level" of a three-level approach to linguistics is the speech phenomena, in order to say, somewhat pedantically perhaps, that it is instead the communicational speech phenomena. Otherwise, the distinction between linguistic theory and linguistic description will collapse, because speech phenomena per se undoubtedly can be treated as a single universe of phenomena. This requirement of specifying the aspect of speech phenomena with which linguistics is concerned may be satisfied, for example, by the choice of a theory which incorporates the notion of "distinctive function with respect to communication" as in all varieties of functionalist linguistics and glossematics. The important point is that the argument in favour of a single universe of phenomena in linguistics cannot be sustained.

Another line of attack could be as follows. It might not be clear to everyone why one should want to adopt a single theory for the construction of different linguistic descriptions of a virtually infinite set of different fields of phenomena. After all, it might be argued, each field of phenomena calls for a separate theory as an explanatory construct with respect to the phenomena in question.<sup>32</sup> Such a view would apparently require us to maintain the two-level view of linguistics ; i.e. for each set of phenomena there would be one set of constructs. Such a situation would



apparently be more akin to the situation in the natural sciences. There are three points to made in this connexion.

Firstly, no one actually adopts this attitude (at least, in this extreme form), since, necessarily, one could never arrive at any general understanding of Language that is the same for each field of phenomena described, whereas it would be generally accepted that it is the business of linguistics to develop such a conception. That is to say ; a general theory would have to arise from descriptions in which either there were no common methods, analytical types, relations, etc. or where all the theoretical terms (or some of them) were identical but differently defined in each case. For instance, we could not take a generative phonological description in English and a functionalist description of the phonology of French and hope to come out with a general theory of phonological structure (even in the unlikely event of our wishing to do so). Equally, many theoretical terms are common to Bloomfield and Martinet (phoneme, morpheme, etc.)but it is perfectly obvious that we could not arrive at a general linguistic theory by comparing descriptions of a Bloomfieldian type with those of a Martinet type. These two cases are, in fact, the same except that in the second case we have the additional confusion introduced by the use of the same terms defined differently in different linguistic theories.

But this merely takes us back to the main point I wish to make ; viz. that there is an important theoretical level of linguistic debate distinct from that of empirical linguistic description and presupposed by empirical description. This theoretical debate requires some framework for the comparison and discussion of theories (meta-theory).

Secondly, if a separate theory were set up for each set of phenomena, it would be either the case that all descriptions of given fields of phenomena were constructed using the same theoretical terms (and definitions), in which case one is left with precisely the task of setting up a general descriptive apparatus of the sort we are calling "theory", or that one set up a new theoretical apparatus for each new description. In this latter case, however, one is still left with the problem of setting up a theoretical apparatus distinct from the description of phenomena ; otherwise , the "description" would be entirely ad hoc (in the absence of any theoretical justification one would scarcely want to speak of "description" at all). If we were to set up a different theoretical apparatus for each new description of a field of phenomena, this would be not only highly uneconomical, it would not be possible to draw even inductive generalisations about the nature of Language. This is due to the fact that the employment of different analytical bases would render the descriptions in question non-comparable



except (absurdly) via the different theories of language involved. (This is the only way we can compare, for instance, a functionalist description of English with a description based, say, on the Aspects model - even the extensions ("scopes") of the two models involve different classifications (descriptive claims about) of the "range" of phenomena.)

The third point concerns matters of convenience. As we have said, the phenomena may be looked upon in two different ways. Regarded simply as describienda, arbitrarily selected speech phenomena constitute the "range" of the description, i.e. that set of phenomena to which the linguistic theory may be meaningfully applied and of which a particular linguistic description is a descriptive model. As such, speech phenomena constitute the initial motivation ("explananda") for the setting up of linguistic theories and descriptions. On the other hand, phenomena may be regarded as the set of observable entities, each of which satisfies the conditions of the description for analysis. In this latter case, the communicational speech phenomena constitute the "scope" or "extension" of the description. As pure describienda (range), speech phenomena (communicational or otherwise) are simply contingently existing events; but the entity which is constituted by a phenomenon in its capacity of satisfying an analytical function is itself a function, the function of the event and the analytical or descriptive function.

In this second sense, each such phenomenon is part of the extension or scope of the description and may be described by what Whitehead and Russell call an unambiguous (singular) description.<sup>33</sup> A singular description has the general formula, "f (x) ( $\phi$ x)",<sup>33</sup> which is the function of some particular entity in its capacity of satisfying the descriptive function, ( $\hat{\phi}$ x). Now, a set of entities is described by that function which each member satisfies. It follows that it is necessary to select a set of descriptive functions for the description of the describienda (range). This selection of functions will constitute what we call a "theory".<sup>34</sup> If the theory selected for, say, the description of Chinese contains the set of functions, ( $\hat{\phi}^a$ x), ( $\hat{\phi}^b$ x), ( $\hat{\phi}^c$ x), . . . . . , ( $\hat{\phi}^n$ x) , and the theory selected for the description of the speech phenomena of English contains the set of functions, ( $\hat{\psi}^a$ x), ( $\hat{\psi}^b$ x), ( $\hat{\psi}^c$ x), . . . . . , ( $\hat{\psi}^n$ x) , such that - ( $\exists \psi$ ) (  $\psi = \phi$  ), then it is clearly the case that there is no basis for the inductive comparison of the descriptions of the two different sets of phenomena. That is,

$$\text{The description of Chinese} = f( x ( \hat{\phi}^a x ) \vee ( \hat{\phi}^b x ) \vee ( \hat{\phi}^c x ) \vee \dots \vee ( \hat{\phi}^n x ) )$$

and

$$\text{The description of English} = f( x ( \hat{\psi}^a x ) \vee ( \hat{\psi}^b x ) \vee ( \hat{\psi}^c x ) \vee \dots \vee ( \hat{\psi}^n x ) ) .$$

One can see that only the above theories can be compared

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and the theories determine the differences between the descriptions (assuming that the range of phenomena is selected in the same way in both cases). Put more simply the two theories select different aspects of the different fields of phenomena: It follows, furthermore, that the respective scopes of the descriptions are different and, hence, it is not possible to arrive at inductive conclusions about Language on the basis of descriptions made using different theories and concerning different phenomena ranges. If we set up a different theory for each set of phenomena, the aim of making general statements about the two sets of phenomena conjointly could not be achieved. (Also, if general statements could be made, they would be meta-descriptive and so a third level of linguistic constructs would automatically be implied.)

The same points could be made, if we were to apply different theories in the description of the same range of phenomena, e.g. the speech phenomena of Chinese. Since the analytical functions would be different in the two cases, it follows that the respective scopes of the two descriptions for the same range of phenomena would be different and, thus, incapable of anything other than theoretical or trivial comparison. (This would be the case, for instance, where one set of analytical functions contained the notion morphophoneme

but not archiphoneme and the other contained archi-  
phoneme but not morphophoneme, as in the contrast between  
Bloomfieldian and functionalist approaches to phonology.)  
In other words, the "bi-uniqueness" of linguistic  
descriptions implies a three-level linguistics.

One may assert, then, that it is advantageous to  
adopt the same theory for the description of different  
and parallel fields of phenomena in linguistics.<sup>35/36</sup>  
Again, the "two-level" view of linguistics cannot be  
maintained. The comparison of theories is clearly  
a different and meta-theoretical matter.

#### d. Radical universalism

It might be possible to maintain a "two-level"  
approach to linguistics, if one were prepared to take  
the view that all of the information required for  
each linguistic description were to be contained in  
the theory. One could then maintain that the theory  
would be directly refutable in confrontation with all  
and any set of phenomena. This would be the position  
of the radical universalist. That is, one who holds  
that the differences between languages are entirely  
superficial and that all languages share a common  
structure which the linguist attempts to model in his  
theory. This common linguistic structure would then  
be related to each set of speech phenomena by rules



of interpretation. Whereas the rules of interpretation might be different for each particular language, since they would account for the allegedly superficial differences between languages, the theory of linguistic structure would be invariant. In principle, such a view seems to be adopted by many transformationalists.<sup>37</sup> In effect, this point of view denies a level of description separate for each language except insofar as linguistic description could be equated with establishing rules of interpretation for the theory with respect to sets of speech phenomena.

Now, this is a very radical proposal. It seems to underlie the view that a child learning a language is disposed to select a particular type of grammar of a narrowly defined sort on the basis of its linguistic experience and its innate capacities.<sup>37</sup> It is not clear, however, that this proposal disposes of a three-level view of linguistics quite as neatly as might appear. The main point is that one can never dispose entirely of the sort of theoretical predicates of which we have already spoken. In transformational-generative grammar it becomes necessary to introduce and to define concepts which are not empirical but theoretical. Thus, for instance, concepts such as that of tree and tree-structure or the notion of transformation as a kind of mapping are theoretical and not empirical in nature. Even in

transformational-generative grammar it is necessary to define the types of unit over which operations take place. That is, we require definitions of terms such as morpheme, verb, noun phrase, etc.. To admit these things, however, is merely to admit a level of theoretical debate and modelling different from that of the modelling of empirical data.

There is another point. This is that the position of the radical universalist, viz. that the underlying structure of each language is the same and that languages differ only superficially, is itself a matter of contention. The argument between the universalist and his opponent is not to be resolved in any empirical manner. It is only on the theoretical plane that the dispute can be decided, if at all. The reason for this is quite straightforward. Any empirical deficiency in the universalist's theory can be remedied in one of two ways. Firstly, the universalist can introduce a new set of interpretation rules for the set of data under consideration. Since any two objects in the universe can be related, there must exist for any pair,  $\langle$ theory, set of data $\rangle$ , some set of rules connecting the members of this pair. Secondly, in extremis, the universalist can change his theory, i.e. his actual claim as to what the universals of language are. But, as we can see, the universalist is not required on the basis of empirical evidence to abandon his initial



position, since changes in either way leave him free to maintain still that languages differ only superficially and not in their underlying structure. The dispute between the proponent of universalism and his adversary thus cannot be resolved on empirical grounds. The universalist's initial position is adopted as an axiom. Conversely, it is not possible to prove the universalist's case to his opponent by empirical arguments. Even if it can be shown that some theory with appropriate interpretation rules can account for all known sets of speech phenomena, the non-universalist always has two counter-arguments. Firstly, he can attribute the success of the universalist theory to the arbitrariness of its interpretation rules and, secondly, again in extremis, he can point out that there are in principle any number of theories which can account for a single set of speech phenomena and that, in principle, there must be an infinite set of theories which can account for all sets of speech phenomena granted appropriate interpretation rules. The non-universalist can say, then, either that the universalist has not made out his case for having established a genuine linguistic theory or that he has not made out his case for having established the unique, empirically valid theory intended. Thus, it is abundantly clear that the dispute cannot be resolved on the empirical plane. The proof which we have given is, incidentally, meta-theoretical. Only by discussing theories can any position be taken in this

dispute. But to say that implies a distinction between empirical and theoretical concerns in linguistics, i.e. it implies a three-level linguistics.

e. Phenomena and the "three-level" approach

One should note that it is in respect of the comparison of descriptions ... using the same analytical basis that the distinction between independently existing phenomena and independently existing linguistically describable phenomena becomes important. Whereas the range of phenomena is merely independently existing, the scope of the description is determined by the limits of applicability of the linguistic theory used in constructing the description. Most phonological theories exclude phonostylistic phenomena, for example. It is in this sense that a theory determines its own scope, i.e. the set of phenomena (and the aspect of those phenomena) which will be considered as relevant to linguistic description under that theory. This sameness of theoretically determined scope (through the set of analytical functions)

ensures the comparability of descriptions. In terms of the above symbolization for one theory we obtain,

$$\text{The description of Chinese} = f( x (\phi^a_x) V (\phi^b_x) V (\phi^c_x) V \dots V (\phi^n_x) )$$

and

$$\text{The description of English} = f( y (\phi^a_y) V (\phi^b_y) V (\phi^c_y) V \dots V (\phi^n_y) )$$

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where  $(\forall \phi^i) (\phi^i = \phi^i)$ .

A second important point about the scope of phenomena determined by a linguistic theory is that it is the notion of scope of linguistic theory which is crucial to the idea of a three-level linguistics. We have already said that the range of speech phenomena may be considered per se as a single universe of phenomena (and it is sometimes so regarded by phoneticians). It is only when we determine the scope of the linguistically describable phenomena by introducing, for example, a specifically communicationally oriented linguistic theory that we can speak of 'communicational speech phenomena'. Furthermore, it is only when we take the aspect of communication into account that we are justified in taking the position that a linguistic theory may reasonably be regarded as standing in a one-many relation with a virtually infinite set of fields of communicational speech phenomena. Hence, the notion of scope and its correlated determining factor in the linguistic theory must be distinguished from the notion of range of speech phenomena. Without a criterion of relevance to determine the scope of a theory, such as the functionalist principle that only objects separately relevant to communication are members of semiotic sets, it is difficult to see how even grossly dissimilar fields of phenomena could be kept distinct. Without the

distinction between fields of phenomena the distinction between theory and description would collapse and we should arrive at an absurd position ; the set of speech phenomena would be totally undifferentiated. The distinction between theory and description would come into question, under such circumstances, because we would no longer be dealing with a multiplicity of universes of phenomena.

A compromise suggestion has sometimes been made that, given various 'paradigms' for linguistic description (e.g. "item and arrangement", "word and paradigm" and "item and process" morphology) it should be possible to determine, for each set of data, the best or most appropriate paradigm.<sup>38</sup>

This attitude implies that, given the possibility of establishing a description in terms of each of the descriptive paradigms concerned for each field of phenomena, it is possible to determine by empirical means which is the "best" or "correct" one. However, if our previous argument was correct, no conclusive empirical argument can be adduced to prefer one theory over another even for particular sets of phenomena. If the different descriptions are equally adequate from both the formal and empirical points of view (and one would not wish to compare descriptions unless they were equally adequate in both these regards), there remains



no method of empirical evaluation apart from considerations of elegance or simplicity, most of which are naturally of a subjective nature and none of which is very satisfying. What one can do is to apply a set of meta-theoretical conditions to each description and theory in turn. Of course, we would prefer one description and its concomitant theory, if it survived, where others failed, tests of formal and empirical adequacy. In such a case, however, it is not so much the comparison of descriptions which gives a result from empirical data as the application of independent conditions to each description in turn. One should note that this method does not lead to a cline of language types ranging from WP through IA to IP. Rather, one comes to reject one or more of the proposed theories. In any case, it would be very difficult to compare descriptions, since, in this case, the scopes of the respective theories are different. So, we are left with the process of eliminating theories of language.

The conclusion that we should aim to establish meta-theoretical conditions to be met by theories and which can serve as a method of determining the adequacy of theories should not be too surprising. The main argument against the proposal of a range of language types classified according to theory is that this proposal simply hides the central theoretical conflict between theories. WP, IA and IP, for example, simply

have nothing in common. They are different concepts of language (or part of it). They are in competition with respect to all languages.

One great difficulty with comparing different theories through their respective descriptions of specified fields of phenomena is how to determine when a given theory has actually failed to lead to an empirically adequate solution. There is always the possibility that the comparer has simply failed to apply each of the conflicting theories equally well. Linguistic description is, after all, partly an art form requiring ingenuity. A case in point, as we have seen, is Chomsky's comparison of the "phrase-structure model" with the "transformational model", where Chomsky's transformational model is presented with far more inventiveness than the clearly unacceptable and very primitive phrase-structure model. As many writers have since pointed out, with sufficient inventiveness, the phrase-structure model can be set up so as to perform the tasks Chomsky requires of it.<sup>39</sup>

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In the comparison of linguistic theories, there is only one case where we can arrive at a straightforward decision and that is the case where one theory properly includes the other. In the case of two disjunct theories,



one would require some sort of external criterion of evaluation, which may or may not be generally acceptable, or one might invoke a simplicity measure. Since, however, theories determine their own scopes, it is not at all clear how a simplicity measure might be applied in evaluating different, equally applicable, linguistic theories. In the case of disjunct theories, for the same reason, one could not speak of one theory being "more inclusive" than another with respect to phenomena.

From the above arguments one will conclude that the best course to adopt involves the establishment or consideration of a single theory at a time for the separate description of many fields of phenomena and the development of a set of meta-theoretical conditions of acceptability so that each linguistic theory and description may be judged in terms of the same standards.

f. The "two-level" approach revisited.

The best known defender of the "two-level" approach in linguistics has been Shaumjan, who writes,

"Contemporary logic of science demands that in each theoretical discipline two levels of abstraction be strictly distinguished ; the level of observation and the level of constructs. On the level of observation we deal with directly observable objects, properties and

relations, which are usually called elementary. Constructs are objects, properties and relations which are not directly observable."<sup>40</sup>

On this interpretation, linguistics is required to set up explanatory models on the level of constructs in order to account for the set of empirical data on the level of observation. In this way the empirical adequacy of a system of constructs is tested. As Shaumjan says,

"Each abstract model of natural languages presents itself as nothing other than an artificial system of symbols, an artificial language imitating natural languages."<sup>41</sup> (trans. P. R. R.)

Furthermore, "constructs are linked with the level of observation by means of so-called rules of correspondence".<sup>42</sup> Shaumjan goes on to distinguish the "static" and "dynamic" forms of linguistic investigation ;

"Statics includes the network of taxonomic relations in language (by "taxonomic" I mean the relations between the elements of language which can be investigated by "taxonomic", i.e. "classificatory", methods) ; and dynamics is the network of inner relations in language, connected with the laws for generating linguistic units of all ranks from the simplest primitive elements



of language."<sup>43</sup>

According to Shaumjan this distinction corresponds to the distinction between the "axiomatic" and the "genetic/constructional" methods of constructing scientific systems. Thus, "a generative device is a mathematical system based on the genetic method",<sup>44</sup> whereas the "axiomatic method" is concerned with "classification"/"taxonomy". Shaumjan describes a generative device as consisting of three components as follows,

"(1) a set of elementary grammatical objects from which complex grammatical objects are generated.

(2) a set of operations which apply to the elementary grammatical objects and serve to generate the complex grammatical objects.

(3) a set of structural specifications which are given to each complex grammatical object generated (in this way a hierarchy of generated, complex grammatical objects is set up)."<sup>45</sup>

One can see that it is not possible to maintain a "two-level" approach to linguistics on the basis of this outline. An axiomatic, "taxonomic" theory for the description of observable speech phenomena (e.g., presumably, glossematics) must clearly contain both an analytical basis distinct from a particular taxonomy and, on Shaumjan's account, the proposed classification

of objects ; otherwise, the descriptive classification can be disregarded as ad hoc. One cannot treat the analytical basis on the same level as an application of it - the two entities are of different orders of abstraction. If, however, there is an analytical basis distinct from a particular taxonomy of the speech phenomena of a particular group of speakers, then one must recognise a three-level linguistics, since the need arises to ensure that the analytical basis is itself adequate and not self-contradictory.<sup>46</sup> Only if the analytical basis satisfies such conditions of acceptability can one accept the linguistic descriptions which are based on it.

On the other hand, a constructional/generative device of the sort described must either presuppose some taxonomy of initial objects (i.e. one must have at least the theoretical predicate, "is an elementary grammatical object"), in which case the device in question clearly presupposes the theory used to make the classification in question ; or, alternatively, it may be argued that the set of elementary grammatical objects requires no further justification than an empirical adequation via the rules of correspondence with the class of observables. If this is maintained, however, then it will be impossible to set up a relation of correspondence between any grammatical object and any



set of observables, unless it is possible to determine the nature of the grammatical object in question, i.e. it must be a member of a defined grammatical category in such a way that the class of observables, which satisfy the conditions of the grammatical object, constitute the correspondence class of the grammatical object in question. It is presumably for this reason that Shaumjan requires that each complex grammatical object be given a structural specification. However, it will not be possible to determine the correspondence class of any grammatical object which is not an instance of a theoretical object, i.e. there must be clearly defined conditions to be satisfied by putative members of the correspondence class of a given instance of a theoretical object (otherwise, the rules of correspondence will be simply arbitrary). For instance, if X is a syntactic complex in L, then we will need to know the definition of the theoretical predicate, "is a syntactic complex" before the correspondence class, C, of observables can be determined for X. Of course, we will need to know many other factors in the description of L, e.g. the constituents of X, but such information simply increases the number of theoretical terms needed (e.g. "is a constituent of").<sup>47</sup> This level of theoretical definition is clearly absent in the two-level approach and, where there is such definition to be found, one clearly has an implicit three-level approach. Indeed, to say that structural specifications must be assigned to

complex grammatical objects implies that it is possible to justify both the grammatical methods used to determine these structural specifications and that there is some general theory of the type and form of the generative processes to be adopted. That is to say that the notions "structural specification"<sup>48</sup> and "generative operation" and the definitions of them are to be distinguished as theoretical objects from any particular instantiation of them and from the empirical justification of any such instantiation. These notions are exactly the sort of theoretical concepts which, Shaumjan himself tells us, require definition.<sup>49</sup> It follows that, even here, a two-level approach is untenable in linguistics. Of course, the untenability of the two-level approach is yet more evident, if it is decided that the set of observables is not undifferentiated, i.e. that a single set of grammatical constructs is insufficient to describe all fields of speech phenomena. The effect of this decision would be to impose a distinction between the general theoretical model (in Shaumjanian terms "genotype") of natural language and the set of its instantiations as descriptions of particular natural languages ("phenotype" languages). One can regard this "genotype/phenotype" distinction as an implicit reliance on a three-level approach to linguistics. Shaumjan's insistence on the "two-level" approach may be seen as an attempt to squeeze linguistics into an epistemological mould to which it is not suited



and which was not created with linguistics in mind.

g. Conclusion

I shall assume, in view of the present arguments, that any approach to linguistics will be inadequate, unless it recognises a distinction between linguistic theory and linguistic descriptions as well as the already well founded distinction between the level of observation and the level of empirical constructs. We shall call the level of empirical constructs "linguistic description", although this usage conflicts with Popper's use of the term "theory". The point of view adopted here is that the term "theory" may be best reserved for the most abstract and general level (that of non-empirical constructs), whereas the term "description" is most appropriate to name particular empirical constructs related to given fields of phenomena. The main point is that a three-level linguistics is required, as opposed to the two-level approach advocated in most treatments of the philosophy of science and linguistics.

We now append diagrams to provide a visual representation of the relations between theory, descriptions and phenomena in the case of (a) a single theory and in the case (b) of the comparison of a multiplicity of theories.

Case a

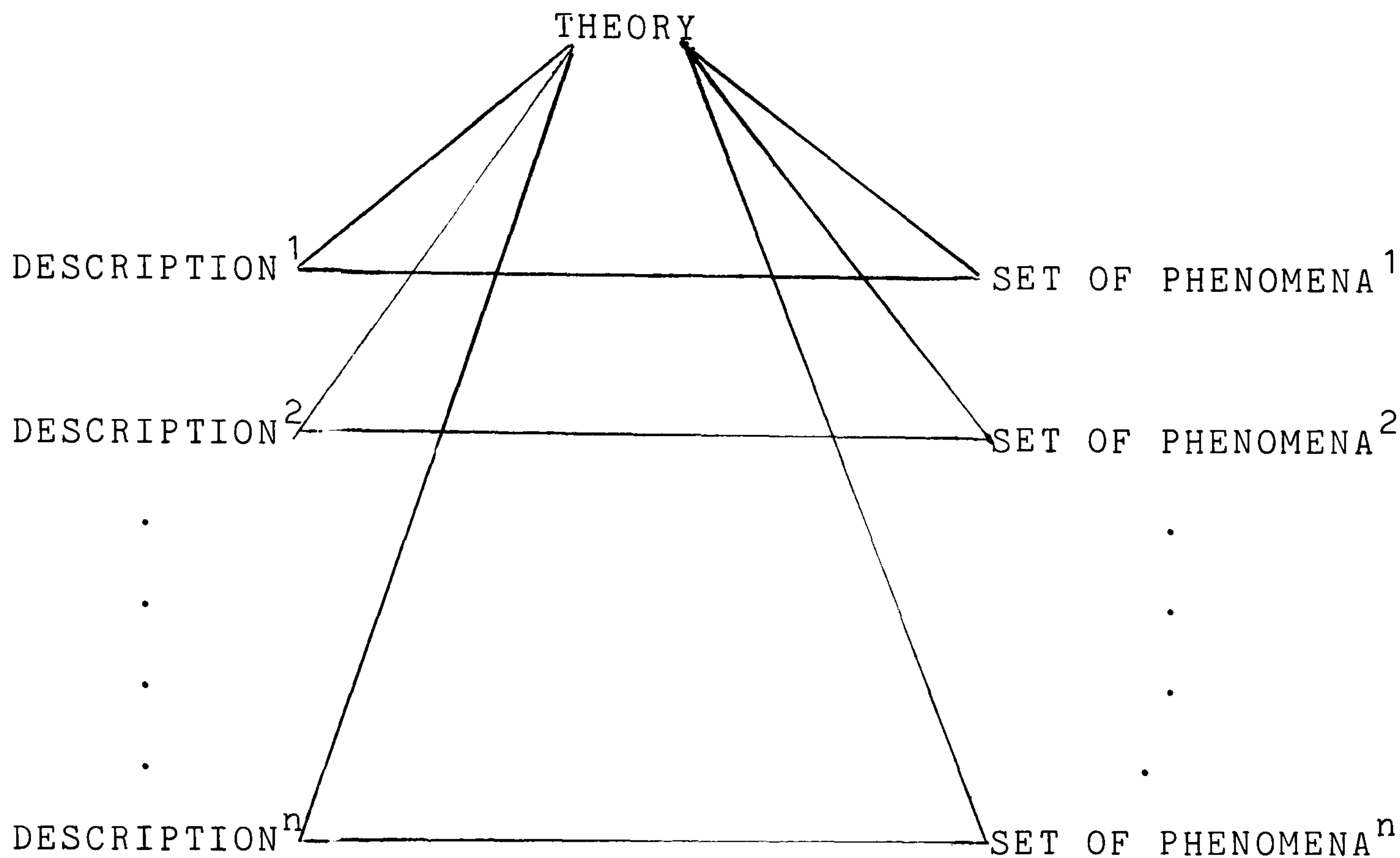


diagram 1

Diagram to show the one-many relation of theory and description, the one-many relation of theory and phenomena and the one-one relation of description and set of phenomena (under a single theoretical approach). Under a single theoretical approach there is ideally a one-one relation between the description ("empirical theory") and the phenomena ("class of homotypic falsifiers").

If we consider a plurality of theories we obtain a far more complicated picture, as follows,



Case b.

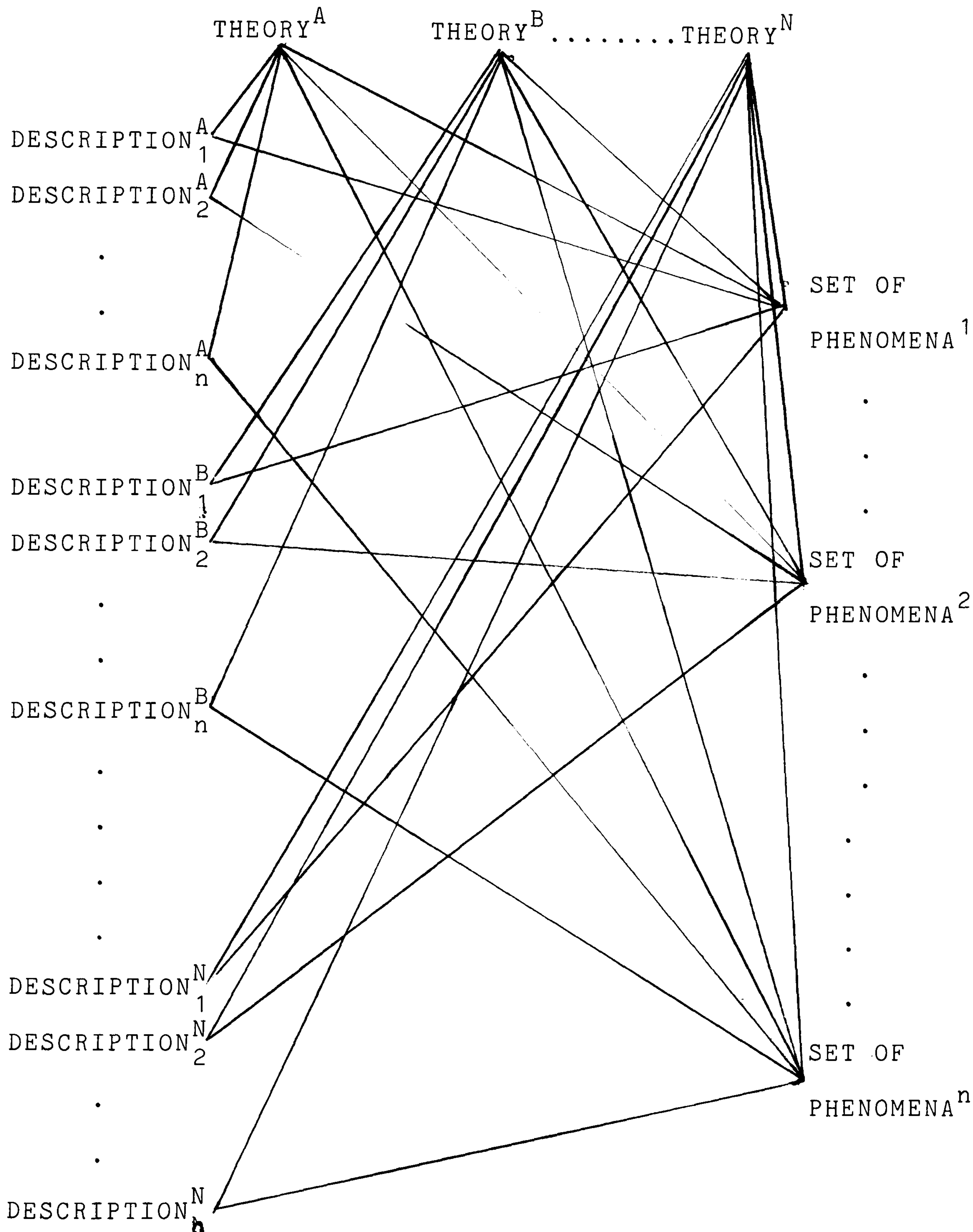


diagram 2

In diagram 2 we see that there is a one-many relation between descriptions and phenomena when we take n theories into account. This is the non-uniqueness of linguistic

descriptions. The multiplicity of descriptive solutions springs from the multiplicity of theoretical approaches.



NOTES TO CHAPTER 3

1. See the "Réunion Phonologique Internationale tenue à Prague (1930)".
2. See J. Vachek, "Phonemes and Phonological Units".
3. K. Bühler, Sprachtheorie.
4. „Im logischen Aufbau, im Begründungsgefüge der Sätze einer empirischen Einzelwissenschaft gibt es stets zwei Grenzen, an deren einer Protokollsätze und an deren anderer Axiome stehen...", K. Bühler, „Die Axiomatik der Sprachwissenschaften", p. 19.
5. L. Bloomfield, "A Set of Postulates for the Science of Language".
6. A. Martinet, La Description Phonologique, p. 11.
7. H. Uldall, Outline, p. 22.
8. H. Uldall, ibid., p. 22.
9. L. Hjelmslev, Prolégomènes, pp. 24 - 30.
10. See, in particular, J. Mulder, "Linguistic Theory,....".
11. It is interesting to note that a similar distinction is to be found in an absorbing, but virtually unknown, book, Was ist Sprache?, by H. Dempe. He writes, „'Sprache' ....bezeichnet : (1) Sprache als unwirkliches Wesen, als Idee, z.B. in der sokratisch gemeinten Frage ; „Was ist Sprache ?" ; (2) Sprache als lebendiger Sprechakt....; (3) Sprache als Ergebnis des Sprechaktes, als Ausspruch, Satz, Rede,...; (4) Sprache als Voraussetzung des Sprechaktes, als allgemeine, zusammenfassende Regeleinheit des von Menschen einer bestimmten Kulturgemeinschaft Gesprochenen.....Die Einheit des Gegenstandes der

Sprachwissenschaft ist aber in der Idee der Sprache zu suchen, die, da sie Sprechakt, Rede und Allgemesprache gleichermaßen ermöglicht, das einheitliche Bezugszentrum aller der Merkmale darstellt, die diesen drei Momenten wesentlich sind, womit zugleich ausgedrückt ist, dass Sprechen, Ausspruch und Grammatik nur Besonderungen eines einheitlichen Phänomens sind, das die Wissenschaft planmässig und in bestimmter Richtung zu bearbeiten hat", p. 26.

This position clearly involves distinctions between the theoretical study of „Sprache als Idee" and the descriptive study of „Sprache als zusammenfassende Regeleinheit des von Menschen einer bestimmten Kulturgemeinschaft Gesprochenen" and „Sprache als Sprechakt", the particular speech acts of individuals which form the phenomena of linguistic analysis.

12. J. Lyons, Introduction to Theoretical Linguistics, p. 1, and A. Martinet, Elements of General Linguistics, p. 15.
13. E. Itkonen, for one, would disagree ; for discussion of his position, see below.
14. This sort of position is presumably what Lyons has in mind when he says, "by the scientific study of language is meant its investigation by means of controlled and empirically verifiable observations and with reference to some general theory of language structure", Introduction, p. 1 (for discussion, see below).
15. This is the position adopted by Hjelmslev when he



says, "nous arrivons à l'intelligence ou à la connaissance d'une langue par le même chemin qui mène à l'intelligence des autres objets, à savoir par une description", Le Langage, p. 29, and "on peut donc dire qu'une théorie, au sens où nous entendons ce terme, a pour but d'élaborer un procédé au moyen duquel on puisse décrire non-contradictoirement et exhaustivement des objets données d'une nature supposée. Une telle description permet ce que l'on a l'habitude d'appeler reconnaissance ou compréhension de l'objet en question ; aussi pouvons nous...dire que la théorie a pour but d'indiquer une méthode de reconnaissance ou de compréhension d'un objet donné", Prolégomènes, p. 26.

16. See J. Mulder, "Linguistic Theory,...". Mulder has remarked that, "most sciences, for example physics or chemistry, aim at a DESCRIPTION of phenomena", "From Sound to Denotation", p. 170. A similar position is taken by M. Bunge in his Meta-Scientific Queries, p. 36. This is not all, however, since most sciences aim to be "explanatory" also, as Bunge says ; "scientists are not satisfied with detailed descriptions ; besides inquiring how things are, scientists try to answer 'whys'", op. cit., p. 51. Of course, an explanation implies an adequate description of how things are in a given field and one should not take too restrictive a view of what it is to be a "description". Linguistic descriptions may be "explanatory", for example,

when viewed in conjunction with the general theory or when viewed as a calculus or when related to a particular communicational explanandum.

17. For "intersubjective testability". see K. Popper, The Logic of Scientific Discovery, pp. 44 ff.

One might note that Uldall has pointed out that one advantage of a scientific approach is that "the selection of functions to the exclusion of all other aspects of the universe has made it possible to give one comparatively simple explanation of an enormous mass of details that would have otherwise appeared unconnected", Outline, p. 14. The implication is that the mass of details in observable speech phenomena require similar treatment, if one is to achieve understanding of these phenomena.

18. J. Mulder, "Linguistic Theory,....", p. 89.

19. J. Mulder, ibid., p. 89.

20. J. Mulder, ibid., p. 92.

21. J. Mulder, ibid., p. 92.

22. The selection of fields of phenomena is discussed at length by J. Mulder in, "Linguistic Theory,....".

23. See, for instance, M. Golian, "La Definition de l' Aspect" and ensuing discussion.

24. K. Popper, The Logic of Scientific Discovery, pp. 89 - 90.

25. See J. Mulder, "Linguistic Theory,....", p. 95, where he says, "unlike in descriptions - the statements in a theory are not hypotheses, i.e. they are not immediately testable in confrontation with facts" and L. Hjelmslev, Prolégomènes, p. 24, where he says,



- "les données de l'expérience ne peuvent jamais ni confirmer ni infirmer la validité de la théorie".
26. K. Popper, The Logic of Scientific Discovery, p. 39.
27. J. Mulder, "From Sound to Denotation", p. 170.
28. Perhaps this is an instance of what Hjelmslev meant when he wrote, in rather outspoken fashion, that "c'est par sa contribution à l'épistémologie générale que la linguistique révélera incontestablement son importance", Prolégomènes, p. 12.
29. J. Mulder, "From Sound to Denotation", p. 170.
30. K. Popper, The Logic of Scientific Discovery, 1972, p. 74, fn. \* 2, see below.
31. See R. Carnap, "Observation Language and Theoretical Language" (and below for further discussion).
32. In such a case each linguistic description would be a separate theory of a single universe of experience. Of course, this is, in a sense, the function of a linguistic description, but it would be terminologically confusing to employ the term "theory" in two distinct senses and aslo it is difficult to see why the empirical description of a set of speech phenomena should be called "theoretical".
33. See A. Whitehead and B. Russell, Principia Mathematica, pp. 69 - 70, p. 173 et passim.
34. A. Juilland and H.H. Lieb have described linguistic description as equivalent to a "classification" (K) of (speech) phenomena (B) according to a set of properties („Eigenschaften"; E), which are the classificatory criteria for the description of phenomena. Any such classification containing

an equivalence relation (R) with the describienda implies a set of criteria (or "theory" in our sense). They write, „Ist K eine strenge Klassifikation von B nach R, so gibt es ein E, dass K eine strenge Klassifikation von B nach E ist", 'Klasse' und 'Klassifikation' in der Sprachwissenschaft, p. 36.

35. This has been maintained by Mulder, see "Linguistic Theory,...".
36. The terms "range" and "scope" as used here are similar to the terms "range" and "scope" used by Whitehead and Russell in Principia Mathematica, pp. 15 - 17.
37. This is the position apparently adopted by Chomsky and many others.
38. See P.H. Matthews, "Recent Developments in Morphology".
39. See, for instance, R. Hudson, English Complex Sentences, etc..
40. S. K. Shaumjan, Principles of Structural Linguistics, p. 14.
41. S. K. Shaumjan, Philosophical Problems of Theoretical Linguistics, p. 10.
42. S. K. Shaumjan, Principles of Structural Linguistics, p. 14.
43. S. K. Shaumjan, ibid., p. 16.
44. S. K. Shaumjan, ibid., p. 18.
45. S. K. Shaumjan, ibid., p. 18.
46. See A. Whitehead and B. Russell, Principia, pp. 12 - 13, who say, "the proof of a logical system is its adequacy and coherence".



47. See P. R. Rastall, "A Note on Formalisation in Transformational Grammar".
48. Shaumjan suggests, for instance, "let us adopt the hypothesis that subject and predicate should be considered fundamental linguistic universals" (P. R. R.), Philosophical Problems..., p. 15, It would seem that this hypothesis leads to the possibility of setting up structural specifications in terms of "subject" and "predicate". This "hypothesis" is established a priori and seems to be axiomatic. However, since no definition of these terms is provided, they are inapplicable. In any case, the mere establishment of such a hypothesis, as opposed to its application in the description of particular languages, implies a covert (axiomatic) theoretical level.
49. See above, Introduction, pp. 1 - 6.

CHAPTER FOUR

LINGUISTICS - "THE SCIENTIFIC STUDY OF LANGUAGE"



a. Preliminary

It is one of the claimed benefits of adopting a "three-level" linguistics that the establishment of a general linguistic theory with no "existence postulate", i.e. one which is an abstract conception, avoids the hypostatisation of such entities as Language. Such entities are regarded from the three-level point of view as mere conceptions, the applicability of which may be tested in the description of phenomena. The adoption of a three-level approach in linguistics is, as we have noted, also related to the idea that linguistics may be regarded as a "science". These questions are interrelated through the problem of the relations between the range of speech phenomena and the abstract conception of such theoretical models as "language" and instances of these models found at the level of description. The problem may be stated as one of formulating the conditions under which the models on the level of theory may be considered "applicable"<sup>1</sup> in the construction of empirical descriptions, while maintaining the abstract nature of the level of theory and certain aspects of linguistic description (see below).

In general, we can exclude any meta-theoretical condition on linguistic theories which demands that a theory depend on prior experience of linguistic

phenomena. The reason is that the fulfilment of this condition would lead to an ultimate circularity. Equally, the suggestion made in some glossematic texts, discussed earlier<sup>2</sup>, that the general descriptive apparatus should be sufficient to entail any linguistic description may be dismissed as either introducing a "synthetic a priori" theory<sup>3</sup> (which may be rejected as untenable) or for relying on the fallacious notion that an abstract theory may have empirical consequences, i.e. ones which are directly testable in confrontation with the experience of observable phenomena. A more reasonable proposal, which has often been suggested<sup>4</sup>, is that linguistics, qua discipline, be required to be "scientific". We shall enquire what such a requirement would entail, to which facets of linguistics it might meaningfully be applied and whether (and to what extent) the condition may be satisfied by, or is consistent with, a three-level linguistics.

We may approach these problems via a consideration of what may be regarded as "external linguistic reality". Hypostatisation is a particularly inviting trap for the wary and unwary alike. Shaumjan, for instance, as we have seen, maintains that, "constructs are objects, properties and relations which are not directly observable"<sup>5</sup> and that "any abstract model of natural languages presents itself as nothing other than an artificial system of symbols imitating natural languages"<sup>6</sup>. However,



if this is the case, it is not possible to claim that, "in abstract linguistic models a considerable role is played by hypothetical concepts, which one may call linguistic constructs"<sup>7</sup>. The reason is quite simply that linguistic models cannot both contain linguistic constructs (non-observable objects, properties and relations) and also imitate them,<sup>8</sup> i.e., if constructs are postulated unobservable entities in reality, they are not parts of an abstract model and, if they are parts of an abstract model, then they are not postulated unobservable entities in reality (although they may be models of such entities). In the light of this we can see that, although Shaumjan is undoubtedly right to raise the "problem of the ontological status of linguistic objects described by abstract linguistic models",<sup>9</sup> his particular formulation of the problem is self-contradictory. The point is that, if it is asserted that a linguistic model is an abstract system of symbols which is formally sufficient to generate analogues of given phenomena, then one cannot claim that there is necessarily any linguistic entity in reality with which the model is in correspondence. Although a model may generate analogues of phenomena and, we may believe, unobservable processes are involved in the creation of those phenomena, these reasons do not permit us to conclude that the constructs of the model correspond to the unobservable processes or objects one believes to exist.

The type of position held by Shaumjan can be defended

in two ways. On the one hand, it may be claimed that the model is a descriptive construct or that it contains descriptive constructs, which imitate or attempt to imitate unobservable linguistic linguistic realities. (In this case it becomes absurd to speak of an "abstract" linguistic model, since the relation between the model and the unobservable system is plainly intended to be empirical, although, apparently, incapable of direct empirical testing ; i.e. one should speak of an empirical linguistic model.) On the other hand, one may say, even more strongly, that the model is isomorphic with some system of unobservable linguistic objects, properties and relations in reality. In fact, Shaumjan adopts this latter course.<sup>10</sup> In either case, however, it is necessary to make some hypostatisation. In the first case, one is forced to assume the existence of a coherent system of linguistic objects (existing separately and, presumably, identically in every mind) to be imitated and, in the other case, one is forced to invent an external reality with just the properties of the model in order that the model be isomorphic with something. In neither case can we arrive at a sufficient condition of the empirical validity of the model. Extensional empirical validity relative to the speech phenomena is a necessary but not sufficient condition for the empirical validity of the model as a model of an unobservable system. It is important to note that we do not deny that, for example, linguistic knowledge is a real but unobservable entity - what we are saying is that, unless direct empirical tests can



be made available, no linguistic model can be used to make conclusions about the nature of that unobservable knowledge without untestable and unwarrantable existential presuppositions.

Clearly, Shaumjan has fallen for the fallacy of attributing the properties of his theories to reality. Equally clearly, no linguistics which depended on this form of assumption or hypostatisation could be considered scientific or acceptable. As we shall see, the definition of linguistics as a scientific study depends on its ability to establish models with an observable and empirically testable relation to observable realities on the level of phenomena. For example, both Lyons and Martinet (amongst many others), as we have said, define linguistics explicitly as the "scientific study of language" and stress this empirical relation. This would seem to be a reasonable approach considering its global nature. The definition clearly turns on what is to be understood by the words "scientific" and "language". The explanation of these terms requires some care.

Whereas particular theories are, of course, concerned to explain what is meant by the term "language", the explanation of the word "scientific" is often brushed aside. This is, perhaps, understandable in the rush to "get on with linguistics" rather than be held up by preliminary problems. However, the definition is important to the extent that the require-

ment that linguistics be, in some sense, "scientific" is a condition of the acceptability of a given linguistic theory or description. Chomsky, by contrast, asserts that the question of whether linguistics is to be considered "scientific" or not is a purely "terminological" matter and that the answer to this question "seems to have no bearing at all on any serious issue".<sup>11</sup> There is no doubt that the question whether linguistics may be called a science or whether it is to be considered scientific is a "terminological" matter in the sense that what is and what is not to be considered a science or scientific is a matter of convention, i.e. the convention which is the definition of the term "science" or the term "scientific".<sup>12</sup> Chomsky's objection is beside the point, however, since the "terminological", or other, nature of the classification of linguistics (which is no doubt a trivial matter) cannot affect the question of what it would mean for a linguistic theory or description to meet the conditions of a scientific theory or description (which is not a trivial matter). The adjective "scientific" or the predicate "is a science" cannot be regarded as honorifics awarded for academic conduct above and beyond the call of scholarly duty. The "serious issue" involved is, clearly enough, that of deciding what conditions a given linguistic theory or description must meet, if we are to take it seriously in the pronouncements it makes concerning the phenomena it purports to be dealing with. By convention, we can call any theory or description which satisfies these



conditions a "scientific" theory or description. That is to say that such conditions will constitute some of the requirements of acceptability on theories and descriptions in linguistics.

Shaumjan has repeatedly pointed out that, in order to be considered "scientific", a theory must be constructed with due regard for some specific attitude in the philosophy of science. A more cautious attitude is adopted here. We require the theory or description to meet certain meta-theoretical requirements (to be stated), some of which will be drawn from the philosophy of science. There is no serious shortage of proposals concerning the nature of scientific explanation and the criteria distinguishing scientific from non-scientific theories and descriptions.

All the more surprising it is, then, when those who do explicitly define linguistics as a "scientific study" do not state precisely what is to be understood by the term "scientific", or do so only inadequately. Lyons, to take a well known example, remarks that,

"..by the scientific study of language is meant its investigation by means of controlled and empirically verifiable observations and with reference to some general theory of language structure".<sup>13</sup>

Martinet, on the other hand, tells us,

"A study is said to be scientific when it is

founded on the observation of facts and refrains from picking and choosing among the facts in the light of certain aesthetic or moral principles. Thus, "scientific" is opposed to "prescriptive"<sup>14</sup>.

It turns out that the phenomena in question are the phenomena of speech in the case of linguistics.

Both of these statements are innocuous enough and both seem to require a measure of empiricism. It is to be realised, however, that Martinet's remarks will not take us very far along the road to understanding the distinction between "science" and "non-science" and Lyons' account contains elements which are confusing. Martinet's statement seems to imply that any scientific approach must begin with "the observation of facts". This appears to indicate that Martinet favours an inductive approach to linguistics which is not found in his clearly non-inductive (and probably deductive) exposition. While it is true that there must initially be some intersubjective agreement on the delimitation of some range of explananda, if we are to set up non-vacuous theories and descriptions (and, perhaps, this is all Martinet wants to say), it is by no means the case that hypothetico-deductive theories and descriptions imply or rely on any logically or actually prior (observed) phenomena.<sup>15</sup> (Indeed, one risks circularity by setting up observations as logically or factually prior to theory or description.) What makes hypothetico-deductive theories and descriptions "scientific" is their test-



ability in confrontation with phenomena.<sup>15</sup>

Lyons, although advocating what is apparently a form of hypothetico-deductivism, gives a confusing account of the nature of the approach. It is not, for instance, the observations which are "controlled and empirically verifiable" (although experiments with phenomena might be), but the hypotheses within a certain description or theory which are controlled and empirically tested with respect to phenomena. Observations themselves cannot be either verified or refuted but are simply constataions of experience. (Of course, not all observations are equally acceptable - this is the force of the requirement that intersubjective agreement be reached on the range of phenomena. As the term "phenomena" is already, to an extent, a "theory-laden" concept, dispute over observations may relate to data-gathering techniques or to the aspect of phenomena which is under consideration in a given theory or description.) Not all observational data are relevant to a given hypothesis and, perhaps, this is all Lyons means by "controlled observations".

Another difficulty with Lyons explanation of the term "scientific" concerns his requirement that linguistics investigate "language" by means of empirical observations making reference to some general theory of "language-structure". Without wanting to be too nit-picking, one must wonder what the relation is between "language as an object under investigation" and "language-structure as

a theoretical entity". Clearly, language and language-structure are either the same or different. If the theoretical language-structure is a hypothesis about some entity language, i.e. if the two are distinguished, then language-structure may be regarded as a device for studying or modelling the entity language. In this case, we are left in the dark about language, which appears to be a simple hypostatization, since we have no reason to believe that any such entity exists. Alternatively, language may be regarded as a pure conception. In this latter case, however, one cannot distinguish the conception language from the general theory of language-structure, since a theoretical language-structure is a conception of language (unless, somewhat absurdly, language-structure is a conception of a conception language or a model of itself), i.e. in this case the notions are not differentiated. On the one hand, there are no observations, empirical or otherwise, which could be relevant to the investigation of a hypostatization and, on the other, a theoretical conception would be a purely a priori matter and, hence, its validity would depend solely on formal conditions; that is, one cannot imagine how the investigation of the conception language or language-structure could be "empirical" in this case. Since such a conception would have no empirical content, no amount of empirical observations could be at all relevant to the testing of it. The empiricism of linguistics, its hallmark as a science, according to Lyons' account, would be lost



on either interpretation of the relation between language and language-structure.

Clearly enough, if the definition of the term "scientific" involves a measure of empiricism, as it surely does, one would conclude that, if "the investigation of language" is a non-empirical matter, then it is self-contradictory to maintain that linguistics is a scientific study (at least on the definitions given so far). What one could do, of course, is to investigate the applicability of a theoretical conception language (= theory of language structure) in the description of given fields of phenomena, as we have already suggested. In this case, one could legitimately speak of investigating languages by means of the application of a given theory of language-structure to empirical observanda. (It is possible that this is what Lyons has in mind, although his ambiguous use of the term language to mean both a set of phenomena and a system of constructs is confusing.) In this way, particular linguistic descriptions satisfying the conditions of the meta-model language could be called particular language-models. Thus, the investigation of languages, but not the investigation of language, would turn out to be an empirical matter, since the relation between a given language-model and a specific set of observables could be subject to empirical tests in ways determined by the application of the theory of language to observables. It is, therefore, through the notion of applicability that

linguistics may be said to have empirical interests and, indeed, to be an empirical science.<sup>16</sup> Now, it should be obvious from this that, if we require of a study that its descriptive constructs be empirically testable in order for it to count as a science, then we can say that the study of languages is (or ought to be) a scientific enterprise, whereas the study of language as a theoretical entity is not. Thus, if we accept the convention of demarcation as set up by Popper that,

"It must be possible for a scientific system to be refuted by experience",<sup>17</sup>

then the empirical, scientific interests of linguistics will be confined to linguistic descriptions.<sup>18</sup>

It now remains to be seen whether Popper's criterion of demarcation can be unreservedly accepted and whether there is any sense in which we can reasonably maintain that on the level of linguistic theory we are also concerned with a type of scientific activity.

#### b. The study of language and the criterion of demarcation

The distinction between the conception of language as a theorematic possibility of a given theoretical calculus and languages as descriptions of given fields of phenomena (normally speech phenomena) was introduced by Mulder. Languages in this sense, are descriptive and



explanatory models of the speech phenomena. For this reason we called them "language-models" above. The distinction between language and languages or language-models runs parallel to the distinction between theory and descriptions. In fact, it is simply part of that distinction. The speech phenomena, or rather arbitrarily selected fields of such phenomena, may be regarded in two ways. We can think of the phenomena simply as sets of contingent describienda<sup>20</sup> or we can think of them as a set of functions of particular events, each in its capacity of satisfying some descriptive function. The distinction depends broadly on how we are considering the phenomena. If we think of the phenomena as simply an independent set of events,  $\underline{e}^1, \underline{e}^2, \dots, \underline{e}^n$ , about which we know only that it is a set of communicational events in which we are interested, then we have the range of phenomena. The range of phenomena is particularly important for the testing of linguistic hypotheses, because it is independent of descriptive functions. The scope of phenomena is important, if we want to deal with the empirical interpretation of a descriptive function or hypothesis. The scope of phenomena allows us to think of a communicational speech event as an instance of a descriptive function. For instance, if the function,  $\underline{f}$ , classifies phonetic events as voiceless, aspirated, bilabial stops and  $\underline{e}^i$  meets these conditions, then  $(\underline{f} \underline{e}^i)$  is an event in its capacity of satisfying a descriptive function. If voiceless, aspirated, bilabial stops are allophones of a given phoneme, say /p/ in English, then  $(\underline{f} \underline{e}^i)$  is an inter-

pretation of the phoneme /p/ ; it is one of the communicational events classified by /p/ and, by implication, its behaviour is predicted by the phoneme model /p/.

A language, or language-model, is here regarded as a principled structure, each descriptive model in which stands directly or indirectly in a relation of correspondence with a potentially infinite class of speech phenomena, such that each phenomenon separately satisfies the conditions for description by that model. The phenomena are in this case regarded as the scope of the description. Broadly, we may say that ideally the scope and range of a description totally overlap and that any asymmetry or discrepancy between the range of describienda and the scope of a description will constitute an empirical refutation<sup>21</sup> of the adequacy of the description. To pursue the analysis of the previous paragraph, if the description predicts that, for example, (e) (f e  $\rightarrow$  g e), then we have a test of the empirical adequacy of the description simply by inspecting all cases in the range, e<sup>1</sup>, e<sup>2</sup>, . . . . ., e<sup>n</sup>. Those members of the range which satisfy f should also satisfy g. The description will be refuted, if we find that (E e<sup>i</sup>) (f e<sup>i</sup>) & - (g e<sup>i</sup>). If g means "is in complementary distribution with all voiceless, unaspirated, bilabial stops", then the adequacy of a phoneme-model /p/ in English could be refuted, if we find a case of commutation between [p] and [p<sup>h</sup>]. That is, in such a case, there would be a discrepancy between the interpretation, or



scope, of a model and the range of phenomena. (We turn to more complicated, concrete examples of empirical testing in later chapters).

Language may be regarded as a sub-type of the class of theoretically possible semiotic systems. It is a conceived and purely theoretical entity, which constitutes the theoretical meta-model containing the set of conditions to be satisfied by any description purporting to be constructed in accordance with the theory in question, i.e. to be satisfied by any purported language-model constructed using that theory. Language is, in this sense, the intension of the class of languages (= language-models), the set of descriptions which satisfy the conditions of being languages. It is a necessary condition of the acceptability of any conception, language, that empirically adequate language-models may be constructed which indeed satisfy those conditions. The construction of empirically adequate language models is, however, neither a sufficient nor a necessary condition of the empirical validity of a conception of language, or theory of language, since, as we have seen, there can be no claim to empirical validity on the level of theory (which is where the conception, language, is found), unless the meaning of "empirical" is shifted and by "the empirical validity of a theory" is meant the adequacy of the theory as a means to establishing empirically testable descriptions, while the theory itself remains not empirically testable by any direct means.

As we have seen, Mulder distinguishes concomitantly between the levels of theory, descriptions and phenomena. A principled description, as noted above, implies both its describienda and a means of making the description. This latter is the theory for making descriptions. Such a theory stands in a one-many relation with the descriptions justified by the theory. The theory implies, on this account, neither the descriptions which may be constructed in applying the theory nor the phenomena which the descriptions purport to describe, although it would be pointless to set up a theory which had no application to phenomena or for which there was no initial motivation in the form of a set of explananda, i.e. such a theory would be inapplicable and, hence, useless as an instrument for gaining knowledge about fields of phenomena. (We consider below the conditions under which such a theory is considered applicable.)

On this view, language is a theoretical entity - a calculated possibility within a semiotic theory. It is not an objectively existing entity outside the theory. Language must be regarded as a pure conception in this sense. A language obviously belongs to the level of description and contracts a one-one relation of correspondence with a select field of speech phenomena ; as Juilland and Lieb<sup>22</sup> (above) say, such an object is a classification of a certain set (range) of phenomena. Again, there is nothing in what we may call "objective reality" which is a language corresponding to the structural description of the phenomena. (The set of



classificatory functions are, after all, arbitrarily selected and not, in any sense, "natural"). The alternative to this position is to maintain that "languages actually exist". Such an assertion would involve the assumption of the existence of a mysterious and unobservable entity, never involved in the empirical testing of descriptions, but which is conveniently considered to be isomorphic with the description (i.e. Shaumjan's position, above). Such an entity clearly calls for the application of Ockham's razor. What do exist are, of course, the speech phenomena as the range of the description, which one claims to describe using a theoretically justified set of analytical functions. From this point of view, the scope of a description, i.e. the set of phenomena in their capacity of satisfying the conditions for analysis under the description has existence only in virtue of the selected set of functions.

Of course, one does not want to deny that the linguistic knowledge of particular individuals exists. Even so, it is dubious whether linguistics is really concerned with the linguistic knowledge of individuals, although describing the linguistic knowledge of sets of individuals might be of interest. However, the linguistic knowledge of sets of individuals is even more inaccessible than the knowledge of particular individuals. We have no direct access to the linguistic knowledge of individuals and the linguistic knowledge of a set of individuals is an abstraction from the know-

ledge of particular individuals. In any case, to claim that a description which successfully describes the speech phenomena of a community a fortiori constitutes a model of the linguistic knowledge of the speakers of that community is clearly unwarrantable. The notion of a correspondence between a successful linguistic description of speech phenomena and the linguistic knowledge of an individual or of a group of individuals should be abandoned partly because of the absence of direct, empirical testing procedures and partly because the existence of one linguistic description which successfully models the phenomena does not preclude the possibility or even the existence of other successful descriptions. The absence of alternative, non-linguistic checking procedures means that we have no external evidence for preferring one or other competing solution. What a linguistic description does do is to describe and explain the means by which communication is achieved in a particular speech community ; and this is no slight task.

Let us now see in what sense this conception of a three-level approach can be said to be "scientific". The conditions to be met by theories and descriptions which purport to be scientific generally fall into two categories ; the formal (concerning the purely logical properties of the system) and the empirical (concerning the relation between the system and that area of experience with which the system in question is



concerned).<sup>23</sup> We have noted that the demarcation between science and non-science (meta-physics, as Popper calls it) involves the notion of empiricism. Formal conditions of acceptability (e.g. consistency, axiomatisability, etc.), however, apply to scientific and non-scientific systems alike. Let us say, provisionally, that, in order to be considered "scientific", a linguistic theory or description must be testable for empirical validity. The notion, empirical validity, clearly involves the testing of hypotheses in confrontation with observable phenomena. That is to say that the validity of a universal, empirical statement, or hypothesis, depends on the relation between the empirical claim of the statement or hypothesis and the observed behaviour of the phenomena (which may be reported by singular, observational statements) which the statement or hypothesis purports to describe. Since the statements in a linguistic theory or description are universal statements referring ultimately to potentially infinite classes, we may adopt Popper's notion of falsifiability as the necessary condition of empiricism. That is, although no absolute verification of descriptive statements is possible (when dealing with potentially or actually infinite classes), we shall require the statements in question to be, in principle, empirically falsifiable in confrontation with the phenomena (in order to be considered "empirical statements") ; no system would be considered "scientific", unless it

contained such statements. Empirical statements will be empirically adequate statements, if, although falsifiable, they are not, in fact, falsified after testing.<sup>24</sup>

The above initial formulation follows the kind of conditions laid upon scientific systems by Popper. The question is now whether we should, in fact, accept Popper's criterion of falsifiability as also the criterion of demarcation between science and non-science in quite this form. Clearly, as we said above, if we adopt this convention, then linguistic descriptions, but not linguistic theories, in a three-level approach will be considered "scientific". That is to say that, if we maintain that entities at the level of theory, such as language, cannot without hypostatization or absurdity be regarded as more than "abstract conceptions", then it is clearly the case that there can be no phenomena which can be used for the empirical testing of these conceptions. A theory of language will, then, be non-scientific - it will be a meta-physical system. Furthermore, there is a sense in which linguistic descriptions are also "abstract conceptions" - viz. when we set up deductive or predictive systems in order to enumerate all and only the well formed entities in a language. Without a level of empirical interpretation such systems remain merely abstract calculi. Secondly, as we have seen, a linguistic description has an existence postulate only with respect to a set of phenomena - communicational



events. There is no existence postulate with respect to any purported unobservable "system to be described". It is only with reference to the set of phenomena that a linguistic description is "empirical". There are important respects, then, in which it is reasonable to say that even linguistic descriptions are "abstract"; i.e. they involve abstract calculus and abstract objects (classes and classes of classes) and operations on these objects, although each of these classes and operations has an extensional, empirical interpretation. (This may be what Shaumjan had in mind when he wrote of abstract linguistic models.)

We can say, then, that linguistic descriptions are extensionally empirical and thus satisfy Popper's criterion of demarcation. It is not the case that linguistic descriptions are empirical in every point of detail or in every respect, although linguistic descriptions, viewed as a totality, will be empirical systems. From the popperian point of view, a linguistic description of the sort imagined would be a scientific system (given the above provisos) and a linguistic theory would be a meta-physical system.

This would be an embarrassing solution when we take into consideration the intimate relation between linguistic theory and linguistic description. We would have to say arbitrarily that one bit of linguistics is scientific and the other is not - despite their close connection. Furthermore, insofar as linguistic theories

are required to be applicable and insofar as this implies that linguistic theories must be material systems,<sup>25</sup> linguistic theories are not merely logical calculi and are intended to have (and must have) a purpose in linguistic description.

Moreover, if we admit Popper's criterion of demarcation into linguistic meta-theory as it stands and apply it to the three-level approach, we are led to at least an apparent clash of meta-theoretical conditions. That is, theories are necessarily "meta-physical" in Popper's sense but, by another meta-theoretical condition, linguistics is required to be "scientific" and, hence, empirical. There appear to be two possible attitudes to this problem. On the one hand, one can minimise the difficulty and admit linguistic theories as "meta-physical" and descriptions as "scientific", i.e. accept the criterion of demarcation as it stands along with its consequences. On the other hand, we can argue that Popper's criterion is inadequate at least in the context of linguistics and attempt to replace or revise it.

I shall argue that, although the criterion of falsifiability is indispensable as a necessary condition of empiricism, a broader conception of science is needed than is allowed for by Popper's criterion. Our broader conception of science will include Popper's criterion of falsifiability ; i.e. we will try to revise the criterion of demarcation. The first reason for this is that Popper's criterion of demarcation is intended to operate in the



context of a two-level approach to the philosophy of science. We have argued that, at least in linguistics, this approach is untenable. We would conclude that the criterion of demarcation must be amended to take into account the three-level view of the philosophy of science.

It would be my view that the two-level approach must be regarded as something of an oversimplification of the state of affairs obtaining in any science. One would not wish to call, for instance, atomic theory anything other than a scientific theory, yet it bears the same relations of being presupposed and of abstract modelling to particular descriptions constructed in accordance with it as linguistic theories bear to descriptions, viz. one-many relations. One observes a similar division of labour into abstract theoretical modelling and practical description in astronomical and biochemical investigations.

However this may be, in linguistics (as in other sciences) linguistic descriptions presuppose and are justified by a given linguistic theory. Consequently, we can say (in the second place) that, although linguistic descriptions have an important empirical content, they also depend to a large extent on the linguistic theory employed in their construction, i.e. at least part of their justification is non-empirical and non-formal - some of the justification of linguistic descriptions is "theoretical". That is, a linguistic description must meet the conditions of some general

theory of language. If it fails to do so, then it will be in some large measure ad hoc and, perhaps, (strictly speaking) incomprehensible. In a functionalist description of, say, language L the phonological component of the description will have theoretical justification to the extent that all of the descriptive models (e.g. /p/, /bilabial/, /P/, etc.) meet the conditions of theoretical predicates of some specified theory of language (e.g. "is a phoneme", "is a distinctive feature", "is an archiphoneme") etc ) and no descriptive model fails to satisfy such predicates (e.g. "p" → /p/ /\_\_\_ X and "p" → /b/ /\_\_\_ Y, as morphophonemic rules, would find no theoretical justification in a functionalist description : they would be theoretically unjustified).

As a result of this, one can assert that linguistic descriptions in the three-level approach are not exactly analogous to 'theories' in the popperian sense, which do not involve any additional theoretical and, hence, meta-physical justification. For this reason also the tempting conclusion that Popper's criterion of demarcation can be reconciled with a three-level approach as it stands is to be resisted.

Another important point in this respect is that a linguistic description cannot be regarded as an undifferentiated whole. We have already seen that a linguistic description contains empirical models, empirical statements and a predictive calculus. No predictive calculus can



be adequate, unless it is a formally sufficient, deductive device. The difficulty is, as we shall see below, that, if a description is an undifferentiated totality or single system of statements, then it becomes impossible to distinguish what is "hypothetical", "nomological" or "empirical" from what is "deductive". This, in turn, leads to serious epistemological difficulties. The two-level view treats scientific theories as single systems of statements.

Although Popper distinguishes, as we have seen, between the formal and the empirical conditions to be placed on what he calls 'theories', he fails to distinguish (at least in the earlier versions of his views)<sup>26</sup> between what is formally deductive about scientific theories and what is empirical and hypothetical about them. (The same goes for Hempel's "nomological deductivism, although it must be pointed out that both of these scholars now accept that a distinction must be made between 'theories' (in their sense) and 'models of theories'. One should add that, in the first place most followers of these thinkers (and others like Curry who put forward similar views of the philosophy of science) have failed to see the importance of this distinction<sup>27</sup> and, in the second place, in the case of Popper, it is not clear how, or if, his criterion of demarcation would have to be amended in the light of the 'theory - model' distinction. Only Carnap, to the best of my knowledge, has attempted a revised picture

of the philosophy of science in the light of the 'theory - model' distinction - see below.)

The failure to draw the distinction between a formal calculus and its empirical interpretation has some important consequences. Broadly (and we return to this topic later), we can say that, when we test a scientific system for its formal properties, such as consistency and sufficiency, we are not interested in the empirical claims of the system. In fact, it is important that the premises of the system be a priori, i.e. first principles or knowledge derived from first principles. Without such premises we could not arrive deductively at the consequence class of the system, the set of theorems of the system. Here the empirical claims of the system are irrelevant. On the other hand, the empirical testing of the consequence class of the system involves the presumption of the formal consistency and sufficiency of the system. (If we cannot generate a known consequence class, then the question of the empirical validity of the claims about reality made by the statements in that class cannot even be raised.) In empirical testing we are past the stage of formal testing. We can distinguish quite clearly the points at which formal and empirical tests respectively apply. Just what we cannot do, however, is to regard a scientific system as an undifferentiated totality which is both an empirical system and a formal calculus at one and the same time. The problem is that, to do so would involve us in saying that all the theorems in the



system were tautologically true (within the system), i.e. true in virtue of the premises, definitions and rules of inference involved, but yet, at the same time, their truth was dependent on experience, i.e. that the statements in a scientific theory were both analytic and empirical at the same time. Since "empirical" here means "capable of refutation with respect to experience" and the truth of analytic statements by definition does not depend on experience, such a conclusion would be unacceptable. Furthermore, if all statements in the theory must be empirical, as Popper requires, then the axioms or premises of the theory must be both a priori and empirical, which is also unacceptable (see below).

What we are saying is that the criterion of demarcation distinguishes an empirical theory from a non-empirical theory but it is too crude, if it is considered as the dividing line between science and non-science. This is especially true when we consider the complications in the view of a scientific theory introduced by the 'theory - model' distinction, which, as we have said, requires us to separate the formal, deductive calculus of the theory from the empirical interpretation or model of it. In linguistics, we certainly find that a linguistic description contains not only empirically interpretable models (e.g. the set of phonemes in English, the set of distinctive features, the properties of the distinctive features

and phonemes and the operations relating these classes) but also a calculus which allows us to formalise our descriptive models and to make predictions or to derive the consequences of the description. Clearly, descriptive models do not have consequences nor do they contain or allow us to make predictions. Descriptive models are a certain sort of abstract object. If we wish to make predictions or derive consequences, we must deal with statements or systems of statements. The statements of the calculus cannot contain descriptive models. The statements contain the names of descriptive models and the names of their properties and relations. If the statements are fully organised into a system, then we have a calculus. This calculus will be a purely deductive system. That is, the class of statements derived as consequences of the calculus, the consequence class, will be determined by purely deductive means (by deductive inference patterns). We can arrive at an empirical interpretation for the calculus via descriptive models or "structures". But this does not mean that every statement of the calculus will be empirically interpretable. There is no doubt that no such calculus could work without at least some premises drawn from the theory. The statements of the theory are, as we have seen, all non-empirical. Furthermore, any formal calculus will inevitably contain abstract objects (variables, logical constants, operators, formation rules, inference rules, "grammatical" symbols, definitions, etc.). Notably, descriptive constants, predicate and relation



names (e.g. "is a phoneme", "is the nucleus of"), come from the theory and are defined in the theory ; they are not defined empirically, although they have empirical applications in the description of given fields of phenomena.

Let us give a simple illustration of these points. Imagine a model in which there is a set of phonemes and a set of distinctive features for language, L. We can set up a calculus to describe the connection between phonemes and distinctive features in L. In such a calculus we will require the premises, drawn from the theory, that "all phonemes are equivalent to simultaneous n-tuples of distinctive features, where  $n \geq 1$ ". Now, suppose that there is a phoneme in the model, which we name "/p/". Then we can form the existential statement, "/p/ is a phoneme in L", where the predicate, "is a phoneme", comes from the theory and is defined in the theory. These two statements clearly imply that there is some n-tuple of distinctive features in L equivalent to /p/. That is, "there is an n-tuple of distinctive features equivalent to /p/ in L" is a member of the consequence class of the calculus. Of course, this argument is not fully formalised, although we can see how such a system might be constructed. Also, one can express this argument in a more abstract form; viz.

$$x (\underline{f} x) \rightarrow (E y) (\underline{g} y) \& (y \leftrightarrow x)$$
$$E x^i (\underline{f} x^i). x^i = /p/.$$
$$\therefore E y^i (\underline{g} y^i) \& (y^i \leftrightarrow /p/)$$

wh. ~~the following~~ letters, f and g, stand for "is a

phoneme" and "is a simultaneous n-tuple of distinctive features" respectively. This abstract way of expressing the argument brings out some of the abstract symbols necessary for the calculus and it also shows the deductive nature of the argument.

By interpreting the name "/p/" as the descriptive construct /p/ in our model we can arrive at the scope of /p/ (using the methods indicated above). Then it becomes possible to make an empirical test of our conclusion. If there is no n-tuple of distinctive features equivalent to /p/, then clearly there is a disparity between the consequence class of the calculus (and the contents of the model) and the phenomena. We would then have an empirical refutation of the hypothesis that /p/ is a phoneme of L. We would have to conclude that one or other of the premises (or both) is false because of the purely deductive inference pattern, modus tollens, which says in outline ;

$A \rightarrow B \ \& \ - B \ \therefore \ - A.$

Here A consists of our two premises and B is our consequence and the consequence, B, is false. However, since the major premise (above) comes from the theory and is true by definition (we could change the premise only by changing the theory), we reject the minor premise above, viz. that there is a phoneme /p/ in L.



In this case, /p/ disappears from the model and our calculus is revised accordingly. (Alternatively, if possible, the distinctive feature analysis might be changed.) If there is an n-tuple of distinctive features equivalent to /p/, then the consequence and the model provisionally stand.

The imaginary case we have discussed is a simple but not untypical case in linguistics. While we can see from it that empiricism, in the sense of the refutability of statements and systems through their confrontation with phenomena, is central to the method, we can also see that empirical falsifiability is too crude a tool to differentiate between science and non-science. The reason for this is that many of the steps and components which we have described as vital to an empirical linguistic description would have to be regarded as non-science. Most notably, linguistic theory, which plays an indispensable role, would have to be regarded as "non-science". But this would be a very arbitrary dividing line. It is not at all clear why certain parts of the process described above (the model or structure and the empirical statements of the description) should be described as "science" but not the non-empirical parts of the description or the theory employed in making the description.

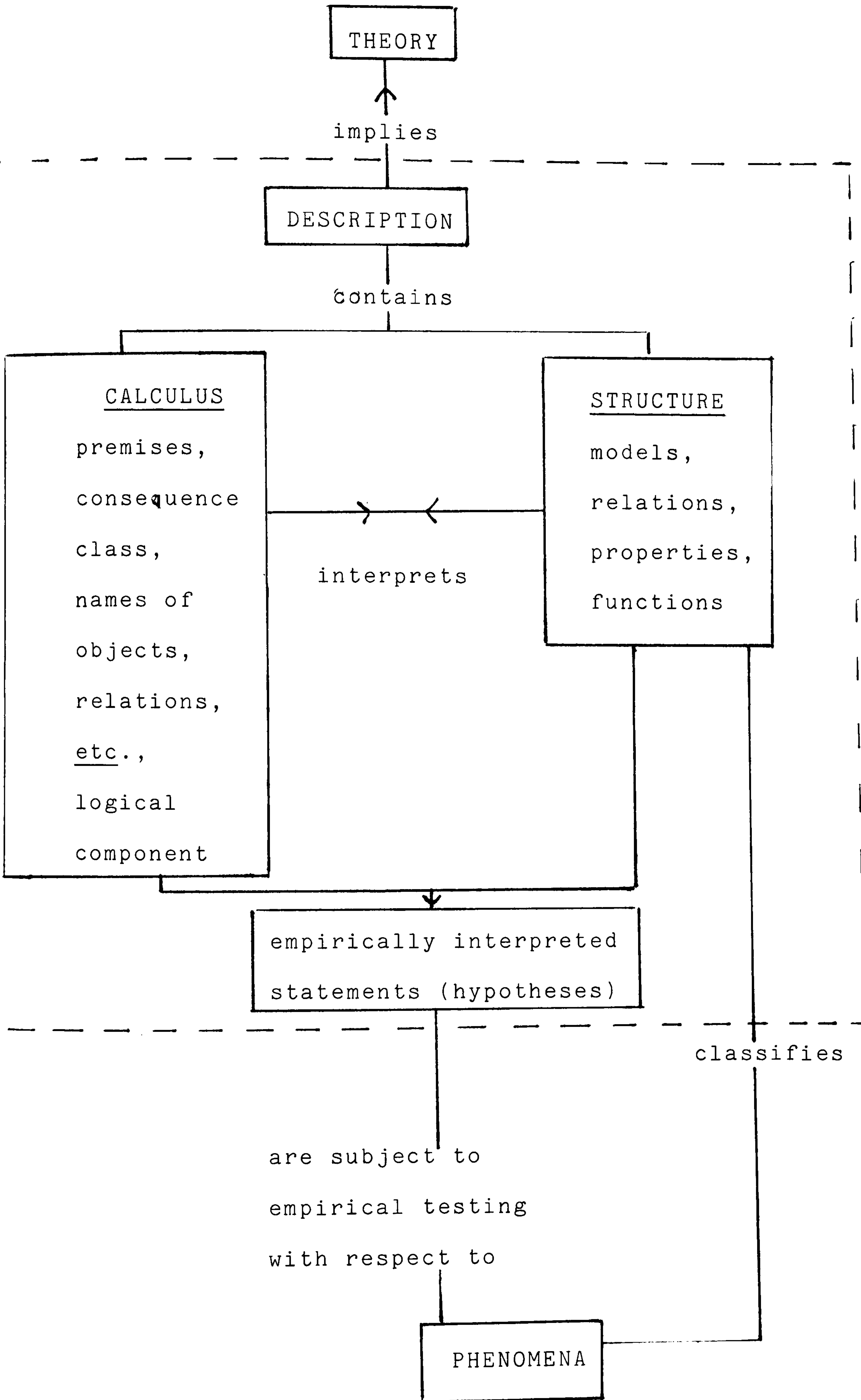
We distinguish between two main parts of a description. A linguistic description, D, is a pair consisting of a calculus, C, and a model or structure, S,

which interprets that calculus, i.e.,

$$\underline{D} = \langle \underline{C} , \underline{S} \rangle$$

A description is not just a structure, since a structure is merely a system of abstract objects and relations with an empirical interpretation in the field of phenomena under consideration (or, conceivably, even a potential system of objects without actual interpretation). We need a calculus, if we are to arrive at statements about the phenomena, to predict the behaviour of the phenomena modelled by the structure and to test the structure (by means of the consequences of the description). A description is not just a calculus either, since we need a structure to give an empirical interpretation to the statements of the calculus. A linguistic description is both a calculus and a structure. The following diagram gives some idea of the factors involved.





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In this diagram we can see that the calculus consists of four main parts, the premises, the names of (structural) objects, relations, etc. (non-logical constants), the logical component and the consequence class. The premises are initial statements, admitted a priori, of the system. These statements may or may not come from the theory, although some will undoubtedly come from the theory. The logical component will provide the rules of formation, inference rules, logical constants, variables and improper symbols as usual in a calculus. The consequence class will be the set of statements which are logically implied in the system. The consequence class contains statements in which appear the names of objects, properties and relations (including functions) found in the structure. These names, which are non-logical constants, are built into the calculus through the premises. The type-names of the non-logical constants clearly come from the theory. Consider, for example, such type-names as, phoneme, syntactic complex, etc.. Language specific names, the names of objects and relations in the structure clearly do not come from the theory ; they are description specific. Examples might be the relation "article-noun" in English or the "aspect-verbal root" relation in Russian. The structure interprets the statements of the calculus (hence, indirectly, the theory). The structure is a class of abstract objects and the properties and relations (including functions) defined in



that class. These objects and relations, properties and functions are all and only those named in the calculus by non-logical constants. The structure constitutes, in effect, as Lieb (quoted earlier) has said, a classification of the speech phenomena on a principled basis. The objects of the structure allow us to interpret the statements of the calculus in terms of speech phenomena. Empirically interpreted statements make a claim about the phenomena which can be empirically tested.

In this way we can distinguish (at least conceptually) between the two important parts of linguistic description - the deductive calculus and the structure of objects and relations. We can, furthermore, distinguish the respective domains of formal, theoretical and empirical testing. Formal testing concerns the logical properties of the calculus. Theoretical testing concerns the establishment of structural models, i.e. their conformity to theoretically specified standards. Empirical testing concerns empirically interpreted descriptive statements (and, hence, the description as a whole). It is important to note, however, that empirical testing does not concern the objects and relations in the structure directly, since one cannot determine the empirical truth value of an object or relation. One can only speak of the truth of a statement. Instead, one might speak of the adequacy of a structural object or relation, although, obviously, the testing of empirical statements and the theoretical

adequation of structures go hand in hand and may not in practice be clearly distinguished. What we are concerned with here is, of course, how the testing of linguistic models works in principle. See below for practical examples.

If we now turn back to the question of Popper's criterion of demarcation, we can make a final point about the notion 'theory' in a three-level approach. It will be remembered that we introduced the condition that a linguistic theory would not be considered acceptable, unless it was applicable in the description of fields of speech phenomena. Any non-applicable theory would be considered vacuous and, hence, unacceptable. Indeed, any theory yielding inadequate linguistic descriptions would be unacceptable. It was pointed out that it was via the notion of applicability that the theoretical and the empirical interests of linguistics were bound together. If a linguistic theory is to be applicable, then it must be an interpreted theory (it must contain a semantics, or verbal interpretation or material aspect).<sup>28</sup> Empirical interpretations of the theory are provided, rather indirectly, by the descriptive structures established. An applicable theory will also contain a methodology and an ontology (see below). It follows that, although a linguistic theory (in our sense) may be meta-physical (from a popperian point of view) in that it cannot be falsified from experience, it is not, however, a purely



formal theory. Although a theory meeting the condition of applicability would still fail Popper's criterion; nevertheless it is a possibility not considered by Popper that a meta-physical theory might still be indirectly scientific via its applicability and indispensability in the construction of descriptions which are in principle empirically falsifiable and, hence, scientific in Popper's sense. We have linguistic descriptions meeting the criterion of demarcation but the indispensable theory would be non-scientific on a strict popperian interpretation. Accordingly, we revise the criterion of demarcation as follows,

"directly scientific" : A system will be said to be directly scientific, if it satisfies all formal, theoretical and meta-theoretical conditions of acceptability including the condition that its empirical interpretation be falsifiable via the demonstration that the empirical interpretation is false for a given range of phenomena (does not fit the facts).

"indirectly scientific" : A system will be said to be indirectly scientific, if it satisfies all formal and meta-theoretical conditions of acceptability, except the condition of empirical falsifiability, but such that the system in question is applicable in the construction of directly scientific systems and if it is necessary for the construction of those systems.

## Conclusion

In the present chapter we have examined the meta-theoretical condition that a linguistic theory or description be scientific. In order to set up such a condition of the acceptability of linguistic theories and descriptions, it has been necessary to formulate a definition of the notion "scientific" which can serve as a criterion of demarcation. It has been necessary to revise the notion of "science" put forward by Popper and to construct a criterion which is consonant with the three-level approach to linguistics and with the 'theory - model' or, rather, 'calculus - structure' distinction drawn at the level of description (directly scientific or empirical system). Popper's criterion was reformulated so as to apply to systems of empirically interpreted statements which are the products of calculi and structures. This criterion will determine, in effect, the systems which are directly scientific. In addition, however, we have found it necessary to set up a further notion "indirectly scientific system" for those theories which are not empirically falsifiable but which are applicable. The present formulation avoids all possibility of hypostatization and of the other epistemological dangers alluded to. In the course of the chapter we have found it necessary to determine which system will be required to satisfy which criteria. Linguistic theories will be required to be "indirectly scientific" and, hence, applicable and linguistic des-



criptions will be required to be "directly scientific" and, hence, empirical as a condition of their acceptability. In developing these conditions it has been necessary to distinguish between the formal calculus of linguistic theories and their semantic interpretation. The semantic interpretation of a theory is a necessary condition of its applicability and the formal calculus ensures its sufficiency. We have also distinguished the formal calculi of linguistic descriptions from the structures which supply their empirical interpretation. We thus distinguish between valid propositions of the descriptive calculus and the empirical interpretation of these propositions, hypotheses. We can then set up tests of the empirical validity for the hypotheses in question. It is a necessary condition of empiricism and, hence, of the acceptability of linguistic descriptions as scientific systems that they be in principle empirically falsifiable, i.e. it is at the point where we speak of hypotheses that Popper's criterion applies and, thus, his criterion is included in the notion "scientific" as it applies in a three-level approach to linguistics. Finally, we have seen that a linguistic description is related to a linguistic theory via the applicability of that theory and that this applicability depends on the incorporation of a semantics, a methodology and an ontology into the theory. The methodology of the theory will play an

important role in the justification of a linguistic description. We leave to later chapters the further discussion of the notions "applicability of a theory" and "empiricism of a description".<sup>29</sup>



NOTES TO CHAPTER 4

1. See Chapter 1.
2. See Chapter 3.
3. For discussion of arguments which are synthetic a priori (in linguistics and in general) see below.
4. See references in the previous chapter to Lyons and Martinet.
5. S. K. Shaumjan, Principles, p. 14.
6. S. K. Shaumjan, Philosophical Problems..., p. 10 (trans. P. R. R.).
7. S. K. Shaumjan, Philosophical Problems...., p. 3, (trans. P. R. R.).
8. Unless, of course, the model imitates itself, which is absurd.
9. S. K. Shaumjan, Philosophical Problems..., p. 3, (trans. P. R. R.).
10. S. K. Shaumjan, Philosophical Problems..., pp. 3 - 17.
11. N. Chomsky, Aspects, p. 20.
12. Popper has asserted, for instance, that his criterion of demarcation between science and meta-physics "will have to be regarded as a proposal for agreement, or convention", Logic of Scientific Discovery, p. 37.
13. J. Lyons, Introduction, p. 1.
14. A. Martinet, Elements, p. 1.
15. As Popper has said, "a hypothesis can only be empirically tested and only after it has been advanced", Logic of Scientific Discovery, p. 30. Phenomena serve as an initial motivation for

theory building ; they do not determine the premises of the theory.

16. See Chapter 1.
17. K. R. Popper, Logic of Scientific Discovery, p. 41.
18. See below.
19. See J. Mulder, "Linguistic Theory,...".
20. In this first sense the notion of speech phenomena is well established. Note, for instance Bühler, who writes, „das konkrete Sprechereignis...ist wie jeder Blitz und Donner und Cäsar's Ueberschreiten des Rubikon, ein Geschehen hic et nunc, das seinen bestimmten Platz im geographischen Raum und im gregorianischen Kalendar hat....Dasz die Linguistik aufs Beobachten angewiesen ist, bedarf keiner Erörterung ; ihr Ruf als wohlbegründete Wissenschaft hängt zum guten Teil an der Zuverlässigkeit und Exaktheit ihrer Beobachtungsmethoden", „Axiomatik der Sprachwissenschaften", p. 19.
21. These are, of course, preliminary formulations - for a fuller account, see below.
22. See Chapter 3, fn. 34.
23. K. R. Popper, Logic of Scientific Discovery, p. 32 et passim.
24. K. R. Popper, Logic of Scientific Discovery, pp. 1 - 70.
25. See my introduction to J. Mulder's, "Postulats...", where it is argued that linguistic theories are material systems.
26. Avid readers of footnotes will know that Popper has accepted the 'theory - model' distinction, see Logic of Scientific Discovery, 1972, p. 74, fn.



\*2.

27. For example, followers of Hempel such as R. Botha, The Methodological Status of Grammatical Argumentation, or of Curry such as S. K. Shaumjan, opera cit., or of Popper such as G. Sampson, "One Fact Needs One Explanation", or J. Mulder, Sets and Relations, or R. Lass, English Phonology and Phonological Theory.
28. The term "verbal interpretation" is due to M. Przelecki, The Logic of Empirical Theories, p. 24.
29. The relation between linguistic theory and empirical testing is explained in the appendix, "Empirical Testing and Linguistic Theory".

CHAPTER FIVE

THE META-THEORETICAL CONDITIONS

ON LINGUISTIC THEORIES



In the previous chapter we concluded that a linguistic theory should be regarded as an indirectly scientific system. We said that, although a linguistic theory could not itself be regarded as an empirical scientific theory, it bore a special relationship to empirical linguistic descriptions and we investigated that relationship. In this chapter we will consider the logical structure of linguistic theories and what meta-theoretical conditions are to be satisfied by linguistic theories. Such meta-theoretical conditions will determine the acceptability of specific linguistic theories.

The epistemological conditions relating to linguistic theories are of two sorts. A linguistic theory will be required to meet formal conditions of acceptability and conditions of applicability. The formal conditions of acceptability are concerned with the logical properties of the form of the theory. The conditions of applicability are introduced in order to ensure that a given theory will be useful in leading to linguistic descriptions. That is to say that the conditions of applicability imposed on linguistic theories are conditions, the satisfaction of which will render a linguistic theory an indirectly scientific system.

a. Formal conditions of acceptability

The major formal epistemological requirements on

linguistic theories are fairly self-evident and have been amply discussed.<sup>1</sup> These are the conditions of consistency, sufficiency (sometimes called adequacy or exhaustiveness) and relative simplicity. These conditions were introduced long ago by Hjelmslev<sup>2</sup> and we will adopt Hjelmslev's conditions with certain modifications.

A theory will be consistent, if it neither contains nor implies any contradiction (the statements that both p and - p) and if the theory is homogeneous (not ad hoc). This latter condition will be met, if, for all statements contained in or implied by the theory, every statement is related to each other in such a way that any two statements may be tested for their freedom from self-contradiction. This latter condition was not foreseen by Hjelmslev but it is clearly necessary as a condition requiring the "integratedness" of linguistic theories, since a theory may be free of contradiction by virtue of its eclecticism (as Mulder<sup>3</sup> has pointed out). A linguistic theory will be sufficient, if it contains or implies all necessary functions for the adequate description of any given range of speech phenomena consistent with the dimensions of relevance of the theory. Again, the latter condition expressed here (underlined) is not mentioned by Hjelmslev, although the condition is, strictly speaking, necessary. The point is that any range of describienda possesses an infinite variety of properties only some of which will be linguistically describable. The fact that some



properties may not be describable under a given theory does not automatically imply that the the theory in question is inadequate, e.g. if the phenomena involved are irrelevant with respect to the dimensions of relevance of the theory involved (and if the theory would as a result have to be made inconsistent in order to account for them) or if the properties are incapable of rational formulation or, finally, if the properties are merely random. More positively stated, it will be necessary to introduce a theoretical function into a given theory only if it can be shown that the phenomena to be described fall within the the dimensions of relevance of the theory in question (i.e. if it would be inconsistent to ignore them).

As an example, we can say that it would be inconsistent to introduce the notion "transformation" into functionalist linguistics. Whereas one must recognise a "second articulation" in functionalist linguistics, since the level of figurae clearly increase the communicational economy of the system, it is not at all clear that one can regard transformational relations as having any bearing on communication (the transmission of information) whatsoever. If this is the case, then the observation of transformational relations holding between linguistic entities (if valid and statable at all) would simply be irrelevant to a semiotic study of Language. Certainly, one could not say that transformational relations were either signs, figurae or relations between linguistic entities in constructions. Rather, transformations are

mapping relations between linguistic entities. It is difficult to see how such relations could be of relevance to communication without accepting, in advance, a transformational account of Language ; i.e. if we assert that transformational relations hold between deep and surface structures. It is a transformational account of Language, of course, which the functionalist does not accept and which is inconsistent with a functionalist account of Language. The incorporation of transformations into a functionalist account of Language would be inconsistent, because any linguistic entity in a functionalist account has identity at a given level of analysis (e.g. at the level of signs). To say that a transformation holds between sign x and sign x' implies the separate identity of x and x' but to maintain that a transformational relation holds between x and x' also implies that x and x' (for example, pairs of active and passive sentences in English) are merely variants of one sign (i.e. they have the same identity at some level of analysis). This would mean either that signs could both have separate identities and the same identity ( which would be inconsistent ) or that there is an unspecified meta-level of signs not provided for or justified in the theory. If, however, we say that transformational relations hold between allomorphs, then we merely assert the existence of unnecessary and trivial operations over items in free or complementary distribution (as with stylistic transformations). Such operations are



unnecessary since the variation is already captured by allomorphy statements and they would be as trivial as "concord" variation (which we do not make a fuss about). But it is not at all clear that the notion of "transformational relation" can even be consistently formulated within functionalist linguistics, since one would not be able to say what the relata of the relation would be. In transformational grammar, the relata are "forms of sentences", or some such, but this naive and undefined conception, springing from a mixture of logic and traditional grammar, is simply not recognised in functionalist linguistics. This is why it is absurd for Ruwet<sup>4</sup> and Postal<sup>5</sup> to criticise Martinet for his phrase-structure view of Language. Martinet does not have a phrase-structure account of Language because he simply does not recognise the level of sentence, as it is understood in the "phrase-structure model". In any case, the "sentences" related by transformations (by transformationalists) would be (for functionalists) either lexically different or syntactically different or both. If lexically different, they would not meet the conditions for transformations. If syntactically different (or both syntactically and lexically different), one has no need to relate the sentences in formal grammar at all. If the sentences are lexically the same but grammatically different, then one is concerned with a semantic relation, viz. synonymy. However one looks at the matter, one is confronted with a problem that cannot be formulated without inconsistency or obfuscation. Under such

circumstances one could hardly expect functionalists to take the problem of transformational relations seriously. Even if the problem were statable in functionalist terms, it would, as we have seen, fall outside the dimensions of relevance of the theory and lead to contradiction. Thus, we can say that the absence of the notion "transformational relation" in functionalist linguistics does not automatically imply the inadequacy of functionalist linguistics and illustrates the necessity of the above condition of sufficiency, viz. that of consistency within the dimensions of relevance of the theory. (It is possible that it is this kind of consideration that Hjelmslev would have covered through the criterion of appropriateness. One should not forget also that the condition of consistency is, as Hjelmslev says, logically prior to the condition of sufficiency. It can be argued that these two factors imply that sufficiency should be taken to mean "sufficiency within a particular domain", i.e. within the dimensions of relevance of the theory.)

We turn finally to the most notional of the above formal conditions, relative simplicity. There is no doubt that any theory should aim for simplicity as a kind of elegance. It is not clear, however, that anything more than a notional definition of simplicity for a theory can be given, but we will say that a theory will be relatively simple, if it achieves its tasks, consistently and sufficiently, without redundancy and



in the fewest possible number of operations<sup>6</sup> and if the theory achieves maximum generality (within its dimensions of relevance)<sup>7</sup>. In popperian terms, maximum generality relates to the largest possible class of "potential falsifiers". We do not accept that linguistic theories are empirical and so we interpret maximum generality to mean "applicability in the greatest possible number of descriptions".

It may be argued, in addition, that a linguistic theory should be, preferably, an axiomatic or axiomatised theory<sup>8</sup>. The reasons for this are numerous. Axiomatic systems possess greater rigour (than, for example, lists of definitions), greater simplicity and greater transparency. They possess a resistance to circularity which is the product of the introduction of axioms (a priori) ; a system of definitions, such as that proposed by Hjelmslev, is potentially limitless and this involves either infinite regress or circularity<sup>8</sup>. Finally, an axiomatic system reduces the number of primitive notions to an explicit minimum. If a theory is an axiomatic theory, however, it must (clearly) meet the conditions of axiomatic systems over and above those already mentioned. These are, principally, that the axioms of the system should be non-interderivable and that there should be no superfluous axiom (no redundancy)<sup>9</sup>.

Insofar as these conditions touch the purely formal

mechanism or calculus of the theory, it is evident that we are concerned with a system which contains a set of axioms which introduce theoretical predicates and relations as propositional functions which are constants within the system (see below). In order that the calculus may work, we will require also a logical meta-language for the theory containing variables, logical connectors, grammatical symbols, rules of formation and rules of inference ( and any other necessary logical apparatus).

The propositional functions of the theory will not be mere variables ; they will be non-logical constants, because each such function will be determinate within the theory and will be assigned a determinate semantic interpretation. The reason for this will be evident. The propositional functions of the theory will be such theoretical predicates as "is a phoneme", "is a moneme", etc. and the theoretical relations will be such as "contracts a syntactic relation with" or "commutes with", etc. Clearly, these theoretical terms will be quite inapplicable if left without interpretation. Without interpretation we would have a meaningless calculus containing a set of empty propositional functions,  $\underline{p} \hat{x}$ ,  $\underline{q} \hat{x}$ ,  $\underline{r} \hat{x}$ , etc., and relations  $(\underline{f} \hat{x}, \hat{y})$ ,  $(\underline{g} \hat{x}, \hat{y})$ , etc. Labels such as these would be of no assistance in the practical task of linguistic analysis, bearing in mind that it is the



avowed aim of a linguistic theory to make possible the acquisition of knowledge about speech phenomena. (It is in virtue of this that the linguistic theory is an indirectly scientific system). It follows that the kind of linguistic theory we have outlined would be an interpreted or material axiomatic system. That is to say that the propositional functions of such a system would be defined and, hence, constants of the system. This should come as no surprise when we consider that a label such as "specific gravity" or "resistance" has no meaning in physics until defined. These seem to be the kind of considerations involved when Tarski wrote that,

"The constants with which we have to deal in every scientific theory may be divided into two large groups. The first group consists of terms which are specific for a given theory. In the case of arithmetic, for instance, they are terms denoting either individual numbers or whole classes of numbers, relations between numbers, operations between numbers, etc..... On the other hand, there are terms of a much more general character occurring in most of the statements of arithmetic, terms which are met constantly both in considerations of everyday life and in every possible field of science and which represent an indispensable means for conveying human thoughts in any field whatsoever ; such words as "not", "and", "is", "every", "some" and many others belong here."<sup>10</sup>

The implication is clearly that in the sciences, although not in formal logic, it is not the so-called logical constants alone which are to be defined but one must define also the non-logical constants of the scientific theory in question. This is not to deny the importance of the operators and logical constants which we will find in any scientific system. What we do deny is that linguistics is concerned with the study of the properties of these (logical) constants, variables and operations. Linguistics is principally concerned to make use of logical methods in theory-construction. The emphasis in any scientific theory will be on the constants specific to the theory. In linguistics, as we have said, we will be concerned with such propositional **functions as** "is a phoneme", "is a distinctive feature", etc.. When interpreted semantically, these determinate functions constitute theoretical meta-models to be applied in linguistic description.

#### b. Theories and Models

If we ignore, for the moment, the semantic interpretation of theoretical terms, we can see that it is tempting to think of descriptive models as interpretations of theoretical models (or "meta-models" as they are sometimes called). Under this view, a descriptive model would be a model of a theory in the usual sense.



That is, the theory,  $\underline{T}$ , contains a calculus,  $\underline{K}$ , consisting of a meta-language (rules of formation, rules of inference, etc.) and a language (axioms, logical and non-logical constants, variables, etc.). A descriptive model would then be a pair consisting of the set of objects,  $\underline{O}$ , and the relations defined in the set,  $\underline{R}^1, \underline{R}^2, \dots, \underline{R}^n$ . The descriptive model is a model of the theory, if, for every relation in the model, each relation is put in correspondence with the name of that relation in the theory and the variables of the theory are explained as members of the set,  $\underline{O}$ , all formulae of the theory are true.<sup>11</sup> Thus, for example, if the theory says that, under specified conditions, a pair of signs in construction contract a relation of subordination and we find that, in English, "men" and "run" are signs and are in construction and meet those conditions, then "men" is subordinate to "run" in that construction and we have a model of the theory.

This way of viewing the interpretation of a theory is not entirely indefensible. But it is not entirely correct either. The point is that a linguistic theory does not provide all the conceivable descriptive classifications, possible descriptive solutions, relations, entities, etc. All such objects and relations must be justified by reference to the theory, but not necessarily contained in the theory. Thus, for example, the theory does not contain the paradigms, "noun" and "verb", but the "noun" and "verb" (e.g. in English)

must satisfy the conditions of paradigms specified in the theory. In the above example, "men" would belong to the paradigm "noun" and "run" to "verb". This means that in description we require a separate language-specific calculus. As we have said, a description is a pair consisting of a calculus and a model. The model of the description interprets the calculus of the description directly and interprets the theory only indirectly. For this reason, we say that a descriptive model is an application of the theory and not a direct interpretation of it. (Of course, if we look at description as a process of describing rather than as a state, then the description is a product of the application of the theory to the phenomena).

Now, it is possible to use the theory so as to generate potential models which may or may not be instantiated by actual descriptive models. For instance, we may have a theory of phonotactic structure which contains the notions of "paradigm", "well-formed phonotagm" and the relation of "subordination" and which operates on the set of phonemes. It would then be possible to set up a potential model in which the set of phonemes was represented by the variables,  $x, y, z, \dots$ , the notion "well-formed phonotagm" by the predicate letter,  $\underline{W}$ , and the relation of "subordination" by  $\underline{R}^{\rightarrow}$  for a pre-nuclear relation and  $\underline{R}^{\leftarrow}$  for a post-nuclear relation. We could suppose that in a potential language three paradigms,  $\underline{A}$ ,  $\underline{B}$ , and  $\underline{C}$  could be set up,



where the members of B and only B were nuclear and the following phonotagm types were possible,

B

AB

BC

ABC.

That is,

$(\underline{A}) \rightarrow \underline{B} \leftarrow (\underline{C})$

We could then set up a sub-theory, as follows,

$x (A \ x \ V \ B \ x \ V \ C \ x)$

$x \overset{R}{\rightarrow} y \rightarrow A \ x \ \& \ B \ y$

$y \overset{R}{\leftarrow} z \rightarrow B \ y \ \& \ C \ z$

$Wx, \dots, z \leftrightarrow B \ x \ V \ x \overset{R}{\rightarrow} y \ V \ y \overset{R}{\leftarrow} z$

and we could set up a model as follows,

$\langle x (x \text{ is a phoneme in } L) ; \underline{W}, \underline{A}, \underline{B}, \underline{C}, \underline{R}, \underline{R} \rangle$

and where we explain the members of the set of phonemes as interpreting the variables of the sub-theory and the relations W, A, B, C, R, R, of the model are explained as interpreting the corresponding relation names, W, A, B, C, R, R, of the theory.

The generation of potential models of this sort may have interesting uses in determining the limits of a theory. However, we must distinguish the potential model of a theory from the real model of a description. The model in the description satisfies the descriptive calculus, although, naturally, it must be possible to establish a potential theoretical model which corresponds to the descriptive model. It is this possibility which

shows, ultimately, the theoretical justification of the description and the applicability of the theory.

c. Applicability conditions

We must turn now to a consideration of how a theory may be constructed in order to be indirectly scientific or applicable. The main problems are those of knowing what a theory is about and how a theory can lead us to knowledge of the desired sort. The first problem is a question of the interpretation of the theory and the second is a point about the components of the theory. The point about the interpretation of a theory brings us back to a previous position, viz. that a linguistic theory must be a material system and that a theory must contain a semantics. The second point about the components of the theory will lead us to recognise components of methodology and ontology as parts of the theory distinct from the calculus and semantics of the theory.

Logicians have long been aware of the problem of interpretation for scientific theories. As we have seen, Tarski was explicitly concerned with the problem of scientific definition and Russell remarks,

"The question of interpretation has been unduly neglected. So long as we remain in the region of mathematical formulae, everything appears precise, but



when we seek to interpret them, it turns out that the precision is partly illusory. Until this matter has been cleared up, we cannot tell with any exactitude what any science is asserting."<sup>12</sup>

As Russell implies, there are two aspects to the problem of semantic interpretation in scientific theories. The one is to determine what it is to be an interpretation (a philosophical problem) and the other is to determine what is the semantic interpretation of a specific scientific theory. This latter point is where the sciences go their several ways and, hence, we say that the formal relations contracted by linguistic theories and linguistic descriptions are merely the starting point in descriptive modelling and theory-building. This is why, when speaking of applied theories (as in linguistics) it is possible, though not strictly speaking desirable, to dispense with an exact formulation of the meta-language of the theory. The reason is that, unless a special form of logic is to be applied in the theory (as in glossematics), one may make use of, say, ordinary propositional calculus or naive set theory.<sup>13</sup> Linguists, like other scientists, are interested only in applying a logical framework not in the investigation of its logical properties. That the framework for linguistic analysis must be (equivalent to or translatable into) a logical framework is, however, a necessity.

In accordance with our three-level conception of

linguistic analysis, we must distinguish at this point between the semantic interpretation, which is a matter of the definition of theoretical terms, and empirical interpretation, which is involved in the empirical adequation of descriptions. Semantic interpretation, which we call definition, is a necessary condition of the applicability of theoretical terms, whereas empirical interpretation is the determination of the empirical extension or scope of a descriptive model (the determination of the set of entities satisfying the conditions of the descriptive model in the field of phenomena under consideration). It is clear that the notion of empirical interpretation presupposes definition, since empirical interpretation implies the applicability of some theoretical terms. Roughly, we can say that the distinction is one between definition as the establishment of analytical conditions and empirical interpretation as the establishment of a set of objects. Thus, we define the notion "phoneme" but we give an empirical interpretation to the phoneme /p/ in English.

We should not forget, however, that a linguistic theory must be applicable in the analysis of speech phenomena. A theory of the sort envisaged contains a calculus which, except for the axioms and meta-language (and the definitions, if they are included in the calculus), is a product of a priori reasoning. The axioms are introduced a priori and the set of theorems depends ultimately entirely on the axioms, the meta-language and the definitions. In an axiomatic



(formal) system, the axioms have no claim to empirical truth ; in fact, they are not tautologies either. They are "accepted as true without in any way establishing their validity",<sup>14</sup> as Russell puts it. It follows that the theorems of the system cannot simultaneously be synthetic, since this would imply that these statements (and, in fact, the axioms) were both analytic, i.e. that their validity depended solely on the use of symbols, and that the self-same statements were simultaneously dependent for their validity on external, empirical factors; i.e. they would be both accepted as true and potentially falsifiable, which is absurd. It follows that the theorems of a linguistic theory would be tautologies, as argued above. Furthermore, since propositional functions do not refer to empirical objects (they are inherently "classificatory", e.g. "is a phoneme", "is a syntactic relation", etc.), it is evident that no empirical evidence can, even in principle, be brought to bear in testing the theory. This, I assume, is what is meant by saying that linguistic theory contains no "existence postulate",<sup>15</sup> i.e. there is no empirical world in which the theory can be empirically tested. In popperian terms, the class of potential falsifiers is always zero.<sup>16</sup> If this argument is correct, then any linguistic theory will be quite vacuous, i.e. it will remain a mere calculus, unless it is applicable. That is to say that, in order for a linguistic theory to be applicable (to be "indirectly scientific"), then it must be possible to construct descriptions of phenomena in terms of and

✓ justified<sup>ly</sup> (satisfying the conditions of or being a model of) the framework of theoretical terms, such that the descriptions in question are not tautological, but empirical.

As we have seen, a linguistic theory, in order to meet the conditions of applicability, must contain both a semantics and a methodology. We will require, in addition, that the theory contain an ontology. This last condition is necessary because of the degree of abstractness involved in theory-construction and the necessity of providing some "routes" for the interpretation of descriptive models. In particular, we will find that entities will differ in their communicational dimensions. The expression of a sign, for instance, may be regarded as a class of phonological forms, {p}, each of which contracts the same distinctive function in grammar (R s); they all have a "grammatical dimension". One should note, however, that phonological forms are classes of phonetic forms, {f}, with a "phonological dimension" (distinctive function in phonology, R d).<sup>17</sup> In order to relate the more abstract entities, such as signs, to observable entities we will require an intervening ontology which relates first-order functions (those which can be satisfied by observables) to n-th order functions. This can be done, as we have seen, by adding or compounding dimensions of communicational relevance).



We saw above the formal conditions pertaining to the acceptability of linguistic theories and we saw how these conditions could be satisfied by setting up the "calculus" of the theory in a certain way. We are now in a position to sum up the factors which ensure the applicability of the theory.

Firstly, as we have noted, the calculus of the theory can not be uninterpreted but the theory must contain a semantics. The semantics of the theory may be regarded either as part of the calculus,  $\underline{K}$ , of the theory,  $\underline{T}$ , or it may be regarded as a necessary condition of the applicability of the theory on a par with the methodology and ontology. In fact, of course, it is both of these. We require the set of definitions contained in the semantics in order to generate the set of theorems but we require them also in applying any theoretical predicate **or relation** in linguistic analysis. In order to convey this "in-between" status we will represent the semantics as a set of definitions or "verbal" interpretations,  $\underline{V}$ , conjoined with the calculus,  $\underline{K}$ . Thus,  $\underline{T}$  contains ( $\underline{K}$  &  $\underline{V}$ ). The calculus,  $\underline{K}$ , contains the set of non-logical constants,  $\{\underline{P}\}$ . Not all members of the set,  $\{\underline{P}\}$ , will be defined. In all systems of the type outlined there is a small number of primitive or undefined terms,  $\underline{U}$ , such that  $\underline{U} \subset \underline{P}$ . As a matter of policy, the role of the primitive terms will be kept to a minimum and undefined terms will be replaced by defined terms wherever possible. The fact remains, however, that a number of primitive terms will

enter the theory (for which one might only be able to give some notional explanation). As far as the defined terms are concerned, each such term will enter an equivalence relation with at least one semantic interpretation,  $\underline{V}^i$ . The equivalence between the definiendum and the definiens must be such that the definiens is a propositional function which contains only defined or primitive (immediately obvious) constants and which does not contain the definiendum (otherwise, we shall have a circularity). Thus, for each defined theoretical term,  $\underline{P}^i$ , there is at least one definition,  $\underline{V}^j$ , such that  $\underline{V}^j$  states the necessary and sufficient conditions for the satisfaction of  $\underline{P}^i$ ;  $\underline{P}^i \leftrightarrow \underline{V}^j$ . (Obviously, the set of definitions falls under the conditions of consistency above.) If the definitions are to be comprehensible and applicable, it follows that they may not be purely formal, i.e. all formal terms must (ultimately) receive an ordinary language interpretation, even if there are instructions concerning the technical use of particular expressions - this is the interpretability requirement.<sup>18</sup>

The ontological requirement helps to ensure the applicability of the theory by demanding that (a) all theoretical terms (non-logical constants) be related in an ontological framework and (b) there is at least one first-order function, i.e. the functions of at least one ontological level must be interpretable in terms of



or translatable into a set of functions which are observably satisfiable predicates. This may be achieved in linguistics by incorporating a level of "phonetic form", such that all observably satisfiable predicates drawn from phonetics constitute the set of phonetic forms, e.g. "is bilabial", "is a voiced, apical fricative", etc.. In semantics, the first order will consist of concrete messages. In this way we ensure that all functions of the theory are related to the level of phonetic form and thus render them ultimately translatable into empirically satisfiable predicates. Thus, any application of the theoretical functions would be directly or indirectly operationally related to observables in their capacity of satisfying particular first-order functions. The relation is operational because the observables are related by specific operations to more abstract entities. This ontological factor builds in the possibility of empirical interpretation in any application of the theory (in any model of the theory). It is important to note that the possibility of translating theoretical predicates into observably satisfiable predicates does not render the theory "empirical" by a back-door route. The satisfaction of the phonetic predicate is necessary but not sufficient for the interpretation of the theory in terms of phenomena. The reason is that the satisfaction of the phonetic predicate (being a phonetic form) does not imply the satisfaction of any criterion of linguistic analysis and, hence, does not imply member-

ship of any phonological form. Even when linguistic criteria are met, any phonetic form will be a member of the extension of a particular phonological form, i.e. it will belong to a model or application of the theory, a particular language (such as English) and not to the theory itself. Consequently, the ontological requirement allows for the empirical applicability of the theory ; it does not allow for its empiricism. It does so by relating the theory to predicates which determine the potential operands of the theory (ranges of phenomena). The position outlined here is in accord with that of Saussure, who wrote ;

"Les rapports de la linguistique avec la physiologie ne sont pas....difficiles à débrouiller ; la relation est unilaterale, en ce sens que l'étude des langues demande des éclaircissements à la physiologie des sons, mais ne lui en fournit aucun."<sup>19</sup>

Although translatability into the predicates of phonetics is an obvious point of connection between linguistic theory and speech phenomena, one must be prepared for the possibility that, particularly in semantics, one will require a number of other first-order predicates. We now turn to an example of how an ontological framework relates theoretical predicates to one another and to phenomena.

Let a sign,  $S^i$ , be a particular set of phonological



forms,  $\{\underline{p}\}^i$ , each in its capacity of satisfying a particular distinctive function in grammar,  $\underline{s}^i$ ; so,  $\underline{s}^i \leftrightarrow \{\underline{p}\}^i R \underline{s}^j \leftrightarrow \{(p^1 R \underline{s}^j), (p^2 R \underline{s}^j), \dots (p^n R \underline{s}^j)\}$ . Now, let a phonological form,  $\underline{p}^i$ , be a particular set of phonetic forms,  $\{\underline{f}\}^i$ , each in its capacity of satisfying a particular distinctive function in phonology,  $\underline{d}^k$ ; so,  $\underline{p}^i \leftrightarrow \{\underline{f}\}^i R \underline{d}^k \leftrightarrow \{(f^1 R \underline{d}^k), (f^2 R \underline{d}^k), \dots (f^n R \underline{d}^k)\}$ , where each phonetic form is a generalised model for the set of particular models of specific realisations in speech, the set  $\{\underline{i}\}$  of images, such that for all realisations there is an  $\underline{i}$  which is a member of  $\underline{f}$ ,  $\underline{i} \in \underline{f}$ . That is,  $\underline{f}$  is empirically satisfiable. So, by substitution, we obtain ;

$$\underline{s}^i \leftrightarrow \{(\{(f^1 R \underline{d}^1), (f^2 R \underline{d}^1), \dots (f^n R \underline{d}^1)\} R \underline{s}^j), \\ (\{(f^1 R \underline{d}^2), (f^2 R \underline{d}^2), \dots (f^n R \underline{d}^2)\} R \underline{s}^j), \dots \\ (\{(f^1 R \underline{d}^n), (f^2 R \underline{d}^n), \dots (f^n R \underline{d}^n)\} R \underline{s}^j)\}.$$

It follows that, in this way, any sign is related to a set of observably satisfiable predicates, but not vice versa. Consider the sign "to be" in English.

$$\text{"to be"} \leftrightarrow \{(\{([bij] R \underline{d}^k), \dots\} R \underline{s}^j), (\{([iz] R \underline{d}^1), \dots\} \\ R \underline{s}^j), \dots (\{([bijin] R \underline{d}^n), \dots\} R \underline{s}^j)\} .^{20}$$

The relations,  $R \underline{s}$  and  $R \underline{d}$ , can be viewed as grouping or classificatory operations over the phenomena.

The third condition of applicability is that of "containing a methodology". In addition to the above two conditions, the applicability of a linguistic theory depends on its terms being related to operational procedures operating on observables in such a way that

the satisfaction of the conditions of these methodological operations is equivalent to, or implies, the satisfaction of the semantically interpreted theoretical term associated with those methodological conditions. The set of operations satisfying these conditions will constitute the methodology of the theory and, for each defined theoretical term or non-logical constant, there must be a determinate subset of methodological operations meeting these conditions. Clearly, the methodology is closely related to the semantic interpretation of the theoretical terms in that it is via the methodology that the conditions of the definition of the theoretical term are satisfied. The incorporation of the methodology is of prime importance in testing the empirical applicability of the theory and in the empirical testing of linguistic descriptions as applications of the theory.

We are now in a position to express the logical structure of a linguistic theory,  $\underline{T}$ , as a triple ;  
$$\underline{T} = \langle \underline{K} \ \& \ \{ \underline{V} \} , \underline{O}, \underline{M} \rangle.$$
Here  $\underline{K}$  is an axiomatic-deductive calculus with the above-mentioned properties (i.e. it contains a 'meta-language' with the rules of formation and inference and a 'language' with the logical and theoretical constants (predicates and relations), the variables and the axioms of the theory).  $\underline{K}$  will contain all and only the non-logical constants (predicates and relations,  $\{ \underline{P} \}$ , of the theory. Each member of the set,  $\{ \underline{P} \}$ , is



defined with the exception of a minimum of undefined terms. Each defined term is equivalent to at least one definition from the set,  $\{\underline{V}\}$ , i.e.  $(\forall P) (E V^j) (\underline{P}^i \leftrightarrow \underline{V}^j)$ . The semantics,  $\{\underline{V}\}$ , is conjoined to the calculus,  $\underline{K}$ .

The ontology,  $\underline{O}$ , is such that (allowing subscripts to represent ontological levels) ;  
 $(\forall P) (\underline{P}^i_{\gamma} R^0 \underline{P}^j_{\delta}) \quad \& \quad (\forall P) (\underline{P}^i_{\gamma} R^0 \underline{P}^1_{\alpha})$ ,  
 where  $\alpha$  represents the first order and  $\gamma$  and  $\delta$  represent higher orders and the relation  $R^0$  is the relation "is ontologically related to". That is, all theoretical terms are ontologically related and all contract an ontological relation with at least one first order function (directly or indirectly).

Finally,  $\underline{M}$  is the methodology. It is a set of (subsets of) conditions, such that the satisfaction of each subset of conditions implies or is equivalent to a particular non-logical constant. Formally, allowing  $(m^1, m^2, \dots, m^n)$  to be a subset of methodological conditions, we can write;

$$(\forall P) (E (m^1, m^2, \dots, m^n) ((m^1, m^2, \dots, m^n)_x \rightarrow (\underline{P}_x^i) \vee (m^1, m^2, \dots, m^n)_x \leftrightarrow (\underline{P}_x^i))).$$

That is, for all non-logical constants of the theory which are defined, there is a subset of the set of methodological conditions, such that the satisfaction of those conditions by any x implies the satisfaction of a particular non-logical constant by that x, or is equivalent to the satisfaction of that constant

by x.

We turn to a much more detailed consideration of methodology in later chapters, where methodology will be seen to be of vital importance in the question of empiricism in linguistics.



NOTES TO CHAPTER 5

1. See, for instance, L. Hjelmslev, Prolégomènes and Résumé, H.J. Uldall, Outline, H. Spang-Hanssen, Probability and Structural Classification in Language Description and J. Mulder, "Linguistic Theory, . . .", amongst many others.
2. L. Hjelmslev, Prolégomènes, pp. 19 - 24.
3. Mulder has called this "a-consistency" ; see his "Linguistic Theory, . . .", p. 93.
4. N. Ruwet, Introduction à la Grammaire Générative.
5. P. Postal, "Review of Martinet".
6. The notion of "simplicity" is, of course, hard to pin down. The conditions of "non-redundancy" and "minimum number of operations" are drawn respectively from J. Mulder, "Linguistic Theory, . . . ." and L. Hjelmslev, Prolégomènes.
7. It would seem to be the case that simplicity involves "maximum generality" ; in popperian terms, the greater the number of "potential falsifiers", the greater the simplicity of a system or statement. See K. R. Popper, Logic of Scientific Discovery, pp. 71 - 2.
8. A. Whitehead and B. Russell, Principia Mathematica, pp. 91 - 2.
9. K. R. Popper, Logic of Scientific Discovery, pp. 71 - 2.
10. A. Tarski, Introduction to Logic, p. 18.
11. Ju. A. Shrejder, "Mathematical Models of Language", pp. 243 - 5.
12. Ju. A. Shrejder, ibid., p. 242.

13. Ju. A. Shrejder, ibid., pp. 243 - 4.
14. B. Russell, Human Knowledge ; its scope and limits, pp. 255 - 6. (quoted by I. I. Revzin, Models of Language, p. 4.).
15. Kiefer has rightly pointed out the virtual vacuity of some highly abstract mathematical models of language for the purposes of linguistic analysis. This is why I emphasise the need for applicability in linguistic theories and say that the development of formal models, although necessary for the acceptability of linguistic theories, does not constitute the primary task of linguistic theorising. See F. Kiefer, Mathematical Linguistics in Eastern Europe, pp. 1 - 9.
16. A. Tarski, Introduction to Logic, p. 118.
17. For the definitions given here see J. Mulder, "Les Postulats ....".
18. L. Hjelmslev, Prolégomènes, p. 24.
19. F. de Saussure, Cours, p. 21.
20. The view of definition adopted here is that of Tarski, Introduction to Logic, p. 35, where he says, "**every definition may assume the form of an equivalence**". See also Whitehead and Russell, Principia, pp. 11 - 12.



CHAPTER SIX

IS LINGUISTICS A "RATIONAL META-PHYSICS" ?

It has sometimes been maintained, contrary to the view adopted by us in previous chapters, that the statements in a linguistic description are of a purely tautological character and that no empirical considerations enter linguistic descriptions at any point. Lass<sup>1</sup>, for example, has maintained that transformational-generative theory would be counted as "meta-physics", i.e. excluded from the domain of science, by a strict application of Popper's criterion of demarcation. Itkonen<sup>2</sup> has claimed that a transformational-generative grammar, viewed as an axiomatic-deductive device, is irrefutable, because it is constructed in such a way that it defines the "norms" of a language, i.e. the well-formed strings, against which it is to be empirically tested. For example, according to Itkonen, the string "the man" is defined as well-formed by the grammar of English and this involves the rule that a definite article always precedes the noun with which it is in construction. Now, this being the case, according to Itkonen, any instance of "man the" would thus be defined as not well-formed and, hence, not a valid refutation of the rule, "article precedes noun". However, this would mean that this rule was in principle irrefutable.<sup>3</sup> If all rules of grammar were of this nature, no grammar could be considered as "empirical" or "scientific".

It would seem to me that both of these arguments are simply exaggerated. In the first place, the mere fact that linguistic theory is, in one respect, purely



tautologous does not imply that linguistics is purely a form of "rational meta-physics". As we have seen, empirical considerations may enter at many points. For example, we may say that the requirement that linguistic theories be semantically interpreted theories and that they lead to acceptable empirical descriptions relates the level of "abstract" theory to the level of description via the notion of applicability. Any theory which is not interpreted cannot be said to be a linguistic theory at all, but it is merely a meaningless calculus. If we admit the possibility of linguistic descriptions of speech phenomena, where each description is constructed using an interpreted linguistic theory, then it is implied that particular grammars are of an empirical nature. In transformational-generative grammar of the Chomskyan sort ( Aspects and after), this will mean that the generated strings of a grammar, in order to describe any given language, must be empirically interpretable in terms of observed speech events. This empirical interpretation implies the possibility of the empirical falsehood of the grammar, its refutation by modus tollens through having empirically false consequences. Alternatively, the grammar may be empirically inadequate, if it is incapable of generating all possible strings exhaustively. That is to say that any validly generated string that did not correspond to any sub-class of the class of well-formed utterances would be sufficient to refute the grammar ; i.e. it would have generated a non-string and, hence, since the falsification of

any derived statement in an axiomatic-deductive system is sufficient to falsify the whole system, the possibility of empirical falsification is not excluded from the description. To accept this, however, is to accept that linguistic description is empirical in Popper's sense. On the other hand, Itkonen appears to deny the possibility of refutation by modus tollens in linguistic description. If it were the case that Itkonen were looking at linguistic descriptions as purely formal objects in abstraction from their empirical interpretation, then, of course, it would be possible to deny the refutation by modus tollens and, since the grammar would define the strings of the language, the question of exhaustiveness would not arise. However, linguistic descriptions, interpreted in this very narrow way, are merely formal calculi and cannot be said to be grammars of anything at all, i.e. they are simply (algorithmic) logical systems. It is only when there is some kind of empirical interpretation of a formal system in terms of observable utterances that we can speak of a grammar of a language. But, if we must have empirical interpretation for there to be a linguistic description at all, then we must allow the possibility that either the grammar is not exhaustive with respect to the phenomena to be described or that the grammar can be refuted by modus tollens as Popper advocates for scientific systems. A grammar will be empirically inadequate, if it does not generate all the well-formed



strings of a language. For example, a grammar of English will fail to be exhaustive, if it cannot generate, for whatever reason, relative clauses. Of course, Itkonen could retort that, since, for instance, the string, "the man who came to tea was stupid", is not defined by his grammar that, ipso facto this is not a string of English. However, this would be a particularly perverse position to adopt in linguistics which, after all, is concerned to provide a description of all the well-formed utterances of a language. On the other hand, a grammar of English would be empirically refuted, if it generated the string, "the who blue was here is". Only if the grammar of English were claimed to be necessarily correct ; i.e. if it defined all the strings and no non-strings, could we fly in the face of facts and claim this to be a genuine string of English, i.e. because the grammar so defines it. Again this would be a strange way of doing linguistics. Two other points must be made. In the first place, the fact that we must carry out a different interpretation of any formal system for each particular language implies that linguistics is not purely tautologous, but is interested in the empirical description of different fields of phenomena. Secondly, the mere fact that Itkonen's rule of "definite article precedes noun" has not been validly refuted, does not imply that it could not be refuted. Furthermore, we know how we might try to refute this rule by empirical means. Itkonen has simply chosen an easy and convenient example for himself. If the rule is not refuted, then, to use Popper's term, it

is "corroborated".

There are areas of linguistics, notably in linguistic theory and in using (the names of) descriptive constructs in deductive arguments to form predictions about the phenomena where we are concerned with purely deductive calculi. This does not imply that there are no empirical considerations in linguistics or that linguistic descriptions are "meta-physical" in the popperian sense. The reason is quite simply that there are clear methods of empirical testing for linguistic descriptions. The predictions which one can make about speech phenomena are, in fact, quite empty, if there is no empirical control on them through testing against phenomena. We reject, therefore, the view that linguistics is a form of "rational meta-physics" in the popperian sense.



NOTES TO CHAPTER 6

1. R. Lass, English Phonology and Phonological Theory, pp. 215 ff..
2. E. Itkonen, "The Use and Misuse of Axiomatics in Linguistics".
3. E. Itkonen, ibid., p. 187.

CHAPTER SEVEN

STRUCTURE AND THEORY IN LINGUISTICS



We have said that "structures" are a necessary part of linguistic descriptions. In general, linguistics is said to be a "structuralist" science. In this chapter we consider what is meant by "structuralism" and the place of "structures" in linguistics.

Linguistics has been characterised by its concern with structures since Saussure. This concern is shared with many other sciences, including chemistry, psychology and physics, e.g. in the theory of "physical structures". Despite the wide variety of disciplines which can be called "structuralist" (and some differences in the interpretation of the term "structure"), there is sufficient similarity in structuralist approaches to science for us to say that structuralism makes a specific, philosophical claim.

The essence of this claim can be explained as the view that the properties and behaviour of homogeneous collections of observables may be accounted for consistently, exhaustively, simply and in an empirically testable manner by reference to abstract, self-contained and autonomous systems or totalities.<sup>1</sup> The identity of such systems is determined by the external dependencies which they contract. A totality of this sort is presented by means of an analysis which postulates the component parts of the system and establishes the functional relations which organise these component parts.

The limits of the system, its component parts and the internal relations of the system are determined by the common function or purpose of the whole. Abstract objects of this sort are set up in such a way that the observable characteristics of the collection of phenomena under consideration are explained as a function of the external dependencies of the system, its component parts and the relations which the components contract. This is achieved by specifying "rules of correspondence" between the objects and relations of the abstract system (on the level of constructs) and the observable objects, relations and processes (on the level of phenomena).

A structure in linguistics is a class of those constructs and those relations described in that class<sup>2</sup> which are necessary to account for the communicational speech phenomena (or some subset thereof) of some specified speech community. Such a class is identified, in the first place, by its external relations, e.g. with other linguistic structures, with non-linguistic structures and with the set of phenomena, the communicational conventions of which it purports to describe. Linguistic structures contain constructs, such as phonemes, signs, syntagms, etc., and linguistic relations, such as relations of opposition, substitutability in a given context, syntactic relations, dependency relations, etc., which organise the constructs. Every construct and relation (in extension) is related to specific sets of observables (and relations between observables) ultimately by specifying the phonetic forms of constructs.



Each construct and relation possesses the characteristic, communicational function of the whole. Often, in linguistics, we find constructs, such as phonemes or syntagms which are themselves analysable as structures. Thus, the phoneme /p/ in English is analysable as a structure consisting of the set of distinctive features, { /labial/, /voiceless/, /stop/ }, and the unordered, constructional relation described in this set,  $\underline{R}^u$ ; i.e. /p/ is equivalent to the structure, or model,  $\langle \text{/labial/, /voiceless/, /stop/ ; } \underline{R}^u \rangle$ , where  $\underline{R}^u$  is an unordered, binary relation true for all pairs in the set. As a construct, however, /p/ contracts relations of opposition, mutual substitutability and constructional relations with other phonemes in other structures. To take another example, a syntagm construct such as "in the garden" is a prepositional phrase entering such relations as NP, Prep. Phr., and copulative, Prep. phr. and contracts relations of opposition with other prepositional phrases ("on the table", "in Gibraltar", etc.) and it is itself analysable as a structure of the components "in" and "the garden" and the ordered, constructional relation, preposition, NP, where "the garden" is again a construct analysable as a structure (although "in" is not further analysable in grammar).

Linguistic structures are often hierarchies of connected structures. Structures of distinctive features are not hierarchical but they enter a (phonological) hierarchy. (Had Hjelmslev accepted distinctive

features as linguistic units, he would have regarded them as the ultimate resultants of an analysis.) The limits of a structure are reached when we arrive at the minimum units with communicational function. Distinctive feature systems form such limits.

Complex constructs, such as /p/ in English or "in the garden" can be considered analytically (as above) or synthetically as a function of their parts. So, the complex "in the garden" can also be viewed as a function of the grammatically simple item "in" and the complex "the garden" and the relation preposition, noun phrase (NP). In complex structures such as these, different constructs may contract the same constructional relations and, hence, be mutually substitutable in the same context(s). On this basis we group such constructs into paradigms. Paradigms are, in fact, just a different type of structure. The items in the set defined by the property of "standing in the counter-domain of the relation copulative, complement in English" (the items commuting in that position), e.g. "the man", "in the garden", "good", "broken", etc. in the context "it is \_\_\_\_\_", contract the binary relation of mutual substitutability in this context. That is, for any pair in that set, the relation "x is mutually substitutable with y" holds in the stated context, where x and y represent constructs. Thus, representing the relation as Mut., we obtain the model,  $\langle x \text{ (predicative complement } x) ; \text{ Mut. } \rangle$ , where items such as "a man", "in the garden", "good", "broken", etc., form the



extension of the set. So, we see that the paradigm is a sort of structure or model..The elements of this set are also further analysable into subsets or sub-paradigms on the basis of their different external relations (distribution). For instance, the class of predicative complements is analysable into such subsets as noun phrase, prepositional phrase, adjective (A), past participle (P. Pt.), etc... This gives us a cross-classification of the paradigm using unary (classificatory) relations described in the set of predicative complements. Since all pairs in that set continue to contract the relation of mutual substitutability, we can simply extend the above model, as follows ;

$\langle x \text{ (predicative complement } x) ; \underline{\text{Mut.}}, \underline{\text{NP}}, \underline{\text{Prep. Phr.}}, \underline{\text{A}}, \underline{\text{P. Pt.}} = \rangle$  . (The identity relation appears in all models.)

Of course, some of these sub-paradigms are themselves structures or models. We can see from this the hierarchical nature of linguistic structures.

It is on the basis of characteristic relations that any linguistic structure can be analysed into substructures. The set of all linguistic objects in a system is organised into phonological and grammatical substructures by the unary relations, is a phonological form and is a sign. These substructures are further organised by other relations, such as is a complex object

and the n-ary constructional relations found in the language in question.

A structuralist approach to science is one in which observable regularities and irregularities are accounted for by an organised system of constructs. Each such system will be finite and will assist the comprehension of real objects and their behaviour by forming a rational and controllable model of observables and the forces which fashion their behaviour. A system of this sort is accepted only insofar as it is capable of explaining observable phenomena. Clearly, every component and relation established must be necessary for an adequate account of the phenomena and have its own empirical justification.

It is important that each structure or substructure is postulated as an autonomous entity. In its strongest form, e.g. in chemistry, psychology and in certain approaches to linguistics, structures are deemed to mirror exactly, or to attempt to mirror exactly, unobservable, postulated entities (e.g. of the mind or of the sub-atomic world). According to a weaker claim (in other versions of linguistics such as glossematics or axiomatic functionalism) structures are not models of unobservable realities but the scientific conceptions by means of which we may understand phenomena and explain and predict their properties. The common, philosophical



claim of structuralism can be summed up as the view that observable phenomena are to be accounted for by the establishment of structures,<sup>4/5</sup> where structures are objects endowed with at least the above mentioned characteristics.

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The conception of structure which we have outlined is widely held by linguists and others. In the physical sciences, there is a similar, though more literal, notion of structure, i.e. identifiable objects with a specific mechanical purpose. Bridges, for example, in the theory of structures are structures. They are composed of parts organised into a functional whole by specifiable mechanical and geometrical relations in order to meet this purpose. Bridges bear a family resemblance to linguistic structures. Bridges, however, exist in space and time ; they consist of real parts and relations. Linguistic structures, by contrast, consist of postulated constructs and relations. Linguistic structures, at least as established by the linguist, have no physical existence ; they belong to the conceptual world (whether taken to mirror unobservable reality or not). They have a closer resemblance to the mathematical specification or "blueprints" of bridges. However, there is a one-many relation between linguistic constructs and observables which does not normally hold between a blueprint

and a bridge (although it might hold between a blueprint and a pump or a boiler). Again, by contrast, the constructs and relations of a linguistic structure are not determinable by direct inspection of the object under consideration. The constructs and relations of a linguistic structure are conjectural. They are for the linguist to establish through analysis. The "unknowns" of physical science are different. They are mensurable properties of a given object with given parts under given quantifiable circumstances (e.g. loading conditions or behaviour in wind). The linguist deals with qualities rather than quantities. His description, as Mulder has called it, establishes the constructs and relations of the structure as conjectural entities. These conjectural entities meet the conditions of some principled theory of communication systems. Qualities, such as "is a phoneme", "is a syntactic relation", etc. , are examples (names) of such conditions and are defined in a mulderian theory.<sup>6</sup> The linguist's conjectural entities are not rendered haphazard or inexact by dint of being qualities any more than a logical theory is haphazard or inexact. Linguistic conjectural entities are controlled by the theory which provides the methods of testing of each proposed construct or relation and the observable phenomena of speech.

Again, by contrast with the bridge-specification



analogy, the linguistic structure established by the linguist has no "strategic" relation with the production of linguistic phenomena, whereas a bridge blueprint specifies and directs the production and properties of an actual or potential physical object. Linguistic structures are established as conjectural, abstract and autonomous, explanatory entities which are tested by reference to observables (although they may have some application in a pedagogic grammar).

In order for testing to take place, two conditions must be met. First, on the theoretical level, we must know the theoretical conditions involved (e.g. what it is to "be a phoneme"). Secondly, we must determine the empirical claim of a structure and subject it to empirical testing. This means that we must move away from structures and their parts and set up empirical statements, e.g. "/p/ is a phoneme of English". Structures are conjectural entities but they are not conjectures. Conjectures are statements. Statements, but not abstract objects such as structures, can be said to be empirically true or false. Structures are not statements, although they may be used to interpret the names of objects and relations which appear in statements. It would, for example, be absurd to say that a hypothetical structure "in at" was empirically false in English, just as a bridge or a blueprint could not be empirically false. It would be correct to say, however, that the statement, "there is a sign construction "in at" in English", is empirically false. Equally,

it would be nonsense to say that /p/ is empirically true in English, whereas it would be reasonable to say that the statement, "there is a phoneme /p/ in English" is true (or, at least, not falsified). Similarly, a bridge or a blueprint cannot be empirically true or false, although a claim such as "this bridge (actual or as specified in a blueprint) can carry four thousand vehicles per hour" can be empirically tested and shown to be empirically true or false.

Philosophers of science have not generally distinguished clearly between the role of a non-empirical theory (such as Mulder or Hjelmslev<sup>7</sup> describe and have set up) and conjectural objects such as structures existing in what Mulder calls descriptions. The distinction involves the difference between, for instance, the purely factual matter that the phonemes /b/, /i/ and /n/ form the ordered group /bin/ in English and the purely non-empirical matter that, by definition, phonemes are the minimum analytical units in phonotactics.<sup>8</sup> We must be careful to note that the term "theory" in the philosophy of science has, until recently, been applied to a variety of objects, most of them being a coalescence of empirical and non-empirical factors.

It will be remembered from what we have said above that we should distinguish between a non-empirical level of theory in linguistics and an empirical level of



description. "Structures" clearly belong to the level of description. Accordingly, we have defined a description as a pair,  $\langle \underline{C}, \underline{S} \rangle$ , where  $\underline{C}$  is a calculus and  $\underline{S}$  is a structure. Description, in this usage, is broadly comparable to the term theory as normally used in the philosophy of science. (A description might be thought of as an empirical theory of a particular language or part-language.) It is most unfortunate that there is a confusing proliferation of terminology in model theory and that much of the terminology coincides with terminology in linguistics and the philosophy of science. In model theory the terms "theory" and "calculus" are usually synonymous as are the terms "model" and "structure". In what follows, we look at the notion description in terms of model theory and relate the notion "theory" or, rather, "calculus" in model theory to the notion "calculus" (above) and "model" or, rather, "structure" in model theory to the notion "structure" (above).

It should be clear that linguistic science is not exhausted by the investigation of structures. Firstly, there is a need for some general theory of communication systems and, secondly, there is the need to relate structures to empirical statements. In other words, a purely structuralist account of (linguistic or other) science would fail to explain either the meaning of theoretical terms or how empirical testing can take place. Empirical testing implies the establishment of statements with a claim about reality. Nevertheless,

one should not underestimate the importance of the philosophical claim of structuralism. It remains important that scientists account for the behaviour of real objects and processes by setting up structures, systems of abstract objects and relations. What is not clear in structuralist or philosophical accounts of science is the way in which theory, structures and empirical statements are related. Most scientists have (naturally) neglected the philosophical import of structuralism. This neglect of the philosophical claim of structuralism has led to the neglect of structuralism by philosophers. This absence of dialogue has given rise to an apparent disparity between the scientist's view of science and that of the philosopher of science.

The dominant view of the philosophy of science has, in the last fifty years, generally characterised science in terms or systems of statements.<sup>9</sup> Secondly, the philosophy of science has been principally concerned with the logic of such systems of statements.<sup>10</sup> Scientific theories are usually described as organised bodies of statements. The statements contained in scientific theories are restricted to those which make a claim about some sphere of reality, the truth of which depends on the nature of observable phenomena. Scientific statements must be capable of empirical testing ; they may be upheld or refuted by reference to experience. Such statements are usually called empirical statements or hypotheses. Hypotheses are normally deemed to form



part of a theory and have meaning only in the context of that theory. The scientific theory stands or falls with the hypotheses it contains. The organisation of the theory is a matter of the logical relations holding between the statements of the theory. Scientific theories are usually considered to take the form of axiomatic-deductive calculi (explicitly or implicitly) in the most rigorous ideal of science (which has sometimes been instantiated). Such calculi are normally mathematical/quantitative in character, although the possibility is not excluded that theories of a qualitative or classificatory nature dealing with non-mathematical objects may exist (various linguistic approaches are, in fact, concrete examples - notably, glossematics, axiomatic functionalism and tagmemics, although numerous approaches have the same tendency). Qualitative theories naturally remind one of the formalisations of set-theory or the propositional calculus and concepts from these areas are often applied in linguistics. According to the usual version of the philosophy of science, scientific theories make use of deductive logic and are conjectural in character : they are, thus, hypothetico-deductive systems.

The hypotheses in this familiar version of hypothetico-deductivism are of two main sorts : those which are introduced as premises of the theory and those which are the consequences , theorems or "derived hypotheses" of the theory. Theorems are logically or mathematically necessary within the theory, but their ultimate justi-

fication (and that of the theory as a whole) is a matter of empirical testing.<sup>12</sup> As Bochenski<sup>13</sup> has pointed out, theorems may be deduced progressively as "predictions" of the theory with the aid of the deductive inference pattern called modus ponens (schematically,  $(A \ \& \ A \rightarrow B) \rightarrow B$ ) or regressively, using the same inference pattern, where we wish to prove that some statement, B, based on observables or experimental evidence is a member of the consequence class of a general theory. Regressive proof is usually known as "explanation".<sup>13</sup>

The testing of a scientific theory, according to Popper, involves the deductive inference pattern known as modus tollens. His opinion is based on the view that no degree of inductive support for a hypothesis would be logically sufficient to verify that hypothesis (although a scientific hypothesis could be (indefinitely) upheld or corroborated by surviving appropriate tests), whereas a single, genuine demonstration that a hypothesis of a theory was false would be sufficient to refute the theory which contains that hypothesis.<sup>14</sup> This does not, of course, mean that the theory could not be revised so as to exclude the embarrassing hypothesis. Scientific advancement is thus held to involve the formulation of hypotheses (within specific theories) and the attempt to refute those hypotheses. Refuted hypotheses lead to the revision of theories and the formulation of new theories not containing erroneous hypotheses and so on.



Schematically, if the premises  $P^1, P^2, \dots, P^n$  imply the theorem,  $T$ , so,  $P^1 \ \& \ P^2 \ \& \ \dots \ \& \ P^n \longrightarrow T$ , and  $T$  is false, i.e.  $\neg T$  is true, then the conjunction  $P^1 \ \& \ P^2 \ \& \ \dots \ \& \ P^n$  is false. The theory containing these statements is thus refuted, since it contains false consequences. This is simply the inference pattern, modus tollens, referred to above.<sup>15</sup>

Important as the concerns of the philosophy of science undoubtedly are, the reader may well feel that, in comparing the positions and interests of the structuralist and the philosopher of science, there is an enormous difference in their respective views of science. Is a science to be described in terms of theories or structures, or both, and, if of both, then how are the two related ?

We should remind ourselves of two points. Firstly, it is necessary to distinguish the deductive or "calculus" element of a scientific theory from its empirical element. This is often stated as a distinction between a "theory" or "calculus" and the "structure" or "model" which interprets that "theory" or "calculus".<sup>16</sup> Secondly, there is a clear disparity between the structuralist, who sees scientific activity in terms of the establishment of structures, i.e. systems of abstract objects and relations, and the philosopher of science, who typically views science in terms of theories, i.e. bodies of statements and their (logical) relations.

Now, this disparity is considerable. Scientific constructs are not statements. Statements have a truth-value ; constructs do not. The scientist's overriding interest is in empirical objects and relations and not in logical objects and relations. If we say, for example, that "in English, /labial/ contracts the unordered constructional relation,  $\underline{R}^u$ , with /nasal/", we do not present the distinctive features, /labial/ and /nasal/, along with the relation,  $\underline{R}^u$ . We present the names of these constructs. The model,  $\langle /labial/, /nasal/ ; \underline{R}^u \rangle$ , with its known properties and its relation to observables interprets this statement.

What we are saying is that one obvious way of relating theories and structures is to incorporate structures into the truth conditions of empirical statements. This would have the advantage that statements would be interpreted in terms of structures and that the empirical element of scientific theories would be introduced by a structural conception of reality. The "theory - model" or (terminologically better) the "calculus - model" distinction is one which is intended to overcome the problem of distinguishing the deductive and the empirical elements of scientific theories and which does so by relating calculi and structures (where structures are understood as models) in the way suggested. It would be useful to apply these ideas in linguistics.



When dealing with a deductive calculus, we can determine the logical truth of theorems by purely formal means. If we take as a simple example the argument, "all metals expand when heated ; iron is a metal : so, iron expands when heated", we can see that the conclusion, "iron expands when heated", is a logical consequence of the premises : it is analytic. As such, the argument contains no more information than that contained in the argument schema,  $x (Px) \longrightarrow (Qx) \quad \& \quad (Ex^i) (Px^i) \longrightarrow (Qx^i)$ . The only difference is that the above argument is expressed in ordinary English and the names of real objects and processes have been inserted, viz. "iron", "metals" and "expand when heated". Of course, we accept the argument schema as a means for the logical deduction of consequences but the specific inference, "if x is a metal, x expands when heated", is clearly not a law of logic. What lends importance to the argument is its place in our understanding of the universe (in physical theory) and, therefore, the interpretation of the names in the argument become important. In a scientific theory, the interpretation of statements is of at least as much importance as the logical form of the theory, the "naming" or "nomological" function is of vital importance for empirical interpretation. The conclusion, "iron expands when heated", becomes a claim about reality (an empirical statement) only when we know the interpretation of the names involved. We reach the same conclusion, if we look at a linguistic argument. For example, "In English, all verbs select the -ing form

in the construction, preposition, verb, "run" is a verb, so, "run" takes the -ing form in the construction, preposition, verb". To become a claim about reality, this argument must be interpreted and that means knowing the interpretation of the names in it, i.e. "run", -ing form, preposition, and verb. The problem in the hypothetico-deductive version of scientific theories is that, as Winnie<sup>17</sup> has said,

"To hold that a statement of the theory is analytic is surely to consider that statement as devoid of factual content ; but then we must deny that statement any nomological function in the theory..."<sup>17</sup>

This clearly conflicts with the above recognition that in a scientific theory the nomological function is of vital importance. We are thus led to distinguish the purely formal calculus of a theory from the empirical interpretation of that theory. It is in virtue of this empirical interpretation that we can formulate claims about reality and which, we maintain, should be a structure in linguistics.

We are now in a position to see how the two points about the philosophy of science are related. To introduce a distinction between a calculus and the interpretation of that calculus is to remove some of the



disparity between a view of science dominated by systems of statements and one dominated by structures. We can assign the logical element of scientific theories (descriptions in axiomatic functionalist terminology or in glossematics) to calculi and the empirical element can be introduced through a structural interpretation of the statements of a calculus. We arrive at empirical statements, or hypotheses, by determining a fixed interpretation for each statement of the calculus.<sup>18</sup>

Model theory gives us a precise way of relating calculi to structures. In model theory, a calculus (or "theory", as it is often called) is related to the world of observables through the intermediary of a "model", "relational system" or "structure". A calculus,  $\underline{C}$ , as understood in model theory,

"..is a series of names of relations,  $\underline{R}^1, \underline{R}^2, \dots, \underline{R}^n$ , the names of variables,  $x, y, z, \dots$ , and the formulae,  $\underline{F}^1, \underline{F}^2, \dots, \underline{F}^n$ , expressed with these names".<sup>19</sup>

To give an exact definition of "calculus", we would need to specify also axioms, logical constants, rules of formation, rules of inference, etc., although, in practice, most of this information is drawn from branches of logic, e.g. the propositional calculus. A "model", "relational system" or "structure", as it is variously called,  $\underline{M}$ , can then be defined as,

"a certain set,  $M$ , with a set of relations,  $R^1$ ,  $R^2, \dots, R^n$ , described in it".<sup>19</sup>

$\underline{M}$  is a model of calculus,  $\underline{C}$ , if each relation of  $\underline{M}$  is compared with every name of that relation in the calculus in such a way that, if the variables of the calculus are explained as elements of the set,  $M$ , all formulae of the calculus are true. The calculus is said to be true for the interpretation in question. Naturally, the question remains open, whether the theory or "description" validly corresponds to the phenomena in question. Empirical testing would then be a matter of comparing the fixed interpretation of statements with observables and tests on observables.

Model theory was originally devised for the interpretation of mathematical theories, although its applicability in other areas seems clear. In particular, the notion, "model", corresponds to the notion, "self-contained set", used by Mulder.<sup>20</sup>

Let us illustrate these ideas with a restricted example. If we take the set of substantives (including possessive pronouns) in Russian as the set,  $M$ ; so,  $M = x (S(\text{ubstantive}) x)$ , we can classify the members of this set by two unary relations,  $D(\text{eterminer})$  and  $N(\text{oun})$ , and we can establish two binary relations in the set, the constructional relation,  $R^C$ , and the dependency relation,  $R^{\rightarrow}$ . Ignoring allomorphy, we would obtain a model as follows ;



$\langle \text{"/etAT/" ("this/that"), "/v'eS'/" ("all"), "/moI/" ("my"), "/xArošI/" ("good"), "/z'I1'onI/" ("green"), "/Stol/" ("table"), "/p'Iro/" ("pen"), \dots ; D, N, R^C, R^{\rightarrow} \rangle .$

We can now set up a calculus for this model. For instance, we will require the following premises.

- a. All substantives are either determiners or nouns.
- b. A pair of substantives are in direct, constructional relation, only if one of the substantives is a noun.
- c. In any construction, the determiner is grammatically dependent on the noun (i.e. determiner  $\rightarrow$  noun).

Using variables and underlining letters for the names of relations, we can express these premises more formally ;

- a.  $x - (\underline{N}x \ \& \ \underline{D}x)$
- b.  $x, y (x \ \underline{R}^C \ y) \longrightarrow (\underline{N}x \ \vee \ \underline{N}y)$
- c.  $x \ \underline{R}^C \ y \ \& \ - (\underline{N}x) \longrightarrow x \ \underline{R}^{\rightarrow} \ y.$

A simple and rapid inspection of this subset of signs in Russian will show that this account is broadly correct. By establishing subsets of determiners and nouns and substituting for variables, we will obtain a large class of well-formed constructions (after allowing for allomorphic variance). Also, by immediate inference,

we can see that no determiner, according to this theory, can be the head of a noun phrase and no noun can be subordinate in a noun phrase and there is no direct constructional relation between substantives, unless a noun is in the construction.<sup>21</sup> We could also establish that the above calculus leaves open the possibility that two nouns might be in direct construction, which is a case actually found in Russian (although fairly infrequently). To allow for that possibility, one would have to add a premise and one would also have to introduce a further binary relation of interordination,  $R^{\leftrightarrow}$ , as there is no unilateral subordination between nouns in construction in Russian, such as "/aFtAr sAt'ir'IK/" ("author-satirist"), etc., viz.,

d.  $x \underline{R}^C y \ \& \ (\underline{N}x \ \& \ \underline{N}y) \ \rightarrow \ x \underline{R}^{\leftrightarrow} y.$

As it stands, the description we have established describes a small section of the Russian syntactic system and illustrates the role of the model, the role of the calculus and how we can form and test hypotheses. We have also seen how the calculus and model can be revised.

In an exact description of Russian, one would wish to distinguish between "articles" and "adjectives"; that is, one would introduce two new unary relations into the model, Art. and Adj., and remove the unary relation, D, and make the corresponding changes in the calculus (i.e. one would make an exact subdivision



of the rather coarse class of determiners). One would also want to take care of the rare possibility that a pair of interordinated articles may form a noun phrase, e.g. "/Fs'o etA/" ("all that"). We also note that a pair of adjectives may not be interordinated to form a well-formed noun phrase or substantival phrase. We note also that some substantives belong both to the class of nouns and to the class of adjectives (and this is the only possibility of overlap between classes). Any description which did not allow these possibilities would be easily empirically refuted. One would need a considerable revision not only of the above model but also of the calculus to accommodate these possibilities. The model would be as follows ;

$\langle$  "/etAT/ ", "/v'eS'/" , "/moI/" , "/xArošI/" , "zIl'onI/" ,  
"/Stol/" , "/p'Iro/" , "/aFtAr/" , "/sAt'ir'IK/" , ..... ;  
Art. , Adj. , N ,  $R^C$  ,  $R^{\rightarrow}$  ,  $R^{\leftrightarrow}$  , =  $\rangle$

Our calculus must allow for the following constructions and dependencies : article  $\leftrightarrow$  article , article  $\rightarrow$  noun , adjective  $\rightarrow$  noun , noun  $\leftrightarrow$  noun , article  $\rightarrow$  (noun  $\leftrightarrow$  noun) , adjective  $\rightarrow$  (noun  $\leftrightarrow$  noun) . We must exclude , however , \* adjective  $\leftrightarrow$  adjective , \* adjective  $\rightarrow$  (article  $\leftrightarrow$  article) , and \* article  $\rightarrow$  (article  $\leftrightarrow$  article) .

Such a calculus may be set up as follows for the above model .

- a.  $x - (\underline{\text{Art.}}x \ \& \ (\underline{\text{Adj.}}x \ V \ \underline{N}x))$   
 b.  $x,y \ (x \ \underline{R}^C \ y) \longrightarrow - (\underline{\text{Adj.}}x \ \& \ \underline{\text{Adj.}}y)$   
 c.  $(x \ \underline{R}^C \ y) \ \& \ ( - \ \underline{N}x \ \& \ \underline{N}y) \longrightarrow x \ \underline{R}^{\rightarrow} \ y$   
 d.  $(x \ \underline{R}^C \ y) \ \& \ - \ (x \ \underline{R} \ y) \longrightarrow x \ \underline{R}^{\leftrightarrow} \ y$   
 e.  $x,y,z \ (x \ \underline{R}^C \ (y,z)) \longrightarrow ( - \underline{N}x \ \& \ \underline{N}y \ \& \ \underline{N}z) \ \& \ (y \ \underline{R}^{\rightarrow} \ z)$   
 f.  $x \ \underline{R}^C \ (y,z) \longrightarrow x \ \underline{R}^{\rightarrow} \ (y,z)$

These formulae state : (a) that nothing is both an article and either an adjective or a noun (although it may be both a noun and an adjective, e.g. "/bIlloI/", "past" or "the past") ; (b) that, if two items are in construction, then it is not the case that they are both adjectives (although they may be both adjectives or both nouns) ; (c) if two items are in construction and only one of them is a noun, then the noun is superordinate to the other item ; (d) if two items are in construction and (c) is not satisfied, then the items are interordinated (i.e. either a pair of articles or a pair of nouns) ; (e) in a triple, a pair of interordinated nouns may be in construction only with a non-noun (adjective or article) and (f) if (e) is satisfied, then the adjective or article is subordinate to the interordinated pair of nouns. The application of these rules will give us a more exact picture of the system of Russian substantives than either of those previously given and the description applies to a very large class of Russian constructions. Nevertheless, it is quite clear that any full-scale description of Russian would



have to take into consideration many other factors; for example, numerals and the many types of relative clause. The attempt to model such phenomena would lead to the refutation of the present description and a new calculus and model would replace those given here (although this is beyond our present illustrative purposes). However, we can see how the kind of description which we envisage can be refuted and changed, while remaining within the same overall framework of description. The present description contains no ill-formed constructions but allows such well-formed constructions as ; "/Fs'o eTA/" ("all that", article ↔ article), "/moI Stol/" ("my table", article → noun), "/z'Ill'onI Stol/" ("the green table", adjective → noun), "/aFtAr sAt'ir'IK/" ("author-satirist", noun ↔ noun), "/etAT aFtAr sAt'ir'IK/" ("that author-satirist", article → (noun ↔ noun)), "/xArošI aFtAr sAt'ir'IK/" ("the good author-satirist", adjective → (noun ↔ noun)). Clearly, an article and an adjective may be subordinate to the same noun or pair of inter-ordinated nouns. This possibility allows us such constructions as "/etAT z'Ill'onI Stol/" ("that green table") and "/etAT xArošI aFtAr sAt'ir'IK/" ("that good author-satirist").

The acceptance of the distinction between calculus and model leads to adjustments in the hypothetico-deductive version of the nature of science. Notably, model theory introduces a distinction between a deductive

calculus in which there are no empirical statements and systems of objects and their relations, which interpret calculi. We arrive at empirical statements via the fixed interpretation of a specific model. It is only by comparing our models with reality that empirical claims can be tested. Thus, using the above description of Russian, we could predict that for instance, "/xArošI Stol/" ("a good table") or "/Fs'o p'Iro/" ("all the pen") are well-formed constructions and that "/xArošI/" and "Fs'o/" are grammatically dependent and these claims can be tested. Equally, we could test a claim that "/z'I1'onI etAT/" ("a green that") is not well-formed. This is quite a different conception from the usual version of hypothetico-deductivism, in which a theory is a system of hypotheses, and the usual version of structuralism, in which scientific description is the establishment of a structure.

What we are proposing is that the notion "linguistic description" be explained by reference to the notion "scientific theory" as developed here with the help of model theory. We have suggested that a linguistic description (an empirical "theory" of a language or part-language - hence a "scientific theory") is a pair,  $\langle \underline{C}, \underline{S} \rangle$ . We can give more substance to this by relating  $\underline{C}$  to the notion "calculus" in model theory and  $\underline{S}$  to the notion "model" in model theory. Scientific linguistic hypotheses would then be a function of  $\underline{C}$  and  $\underline{S}$ .



Since a general linguistic theory would play a considerable role in C (see above, for instance, in giving substance to the notions, constructional relation and dependency relation), we can begin to see the sense in which a description is theoretically motivated and how a description always involves considerable, theoretical (general) presuppositions.

There are numerous advantages to this view. First, this conception of empirical, linguistic description makes clear the components of calculus and structure and their relations in a way which is compatible with modern views of the philosophy of science. Secondly, we can arrive at a precise characterisation of the notions, calculus and structure. By using the relation of "being a model of a calculus" we can, furthermore, give a precise understanding of the hitherto intuitive notion of establishing a linguistic structure in accordance with a general theory, since the calculus draws upon and must satisfy the general theory. In fact, many premises of any description will come from the theory, e.g. that ordered grammatical relations are syntactic or that phonemes are the minimum units of phonotactics, and many classificatory predicates will be defined in the theory, e.g. "is a phoneme", "contract dependency relations", "is a moneme", etc.. Conversely, no theoretical predicates can come from outside the theory and no premises can be set up in a calculus which contradict the theory, although there may be description-specific premises and predicates. Finally, any descrip-

tion-specific predicates or premises are restricted by the requirement that all calculus statements be interpretable through the model and that they be theoretically justifiable. On the other hand, we can arrive at testable empirical statements by interpreting the variables and relations of the calculus using the structures established by our models.

This outline of the structure of scientific linguistic descriptions preserves the deductive and empirical elements found in the hypothetico-deductive version of science, maintains the role of structures and allows us to remove the disparity between structuralist and philosophical versions of science, provided that we accept the conclusion that an empirical theory or description must contain both a calculus and a model-interpretation (structure). In linguistics, this would mean that a linguistic description cannot be regarded solely as a calculus (or hypothetico-deductive theory).<sup>22</sup> nor can it be regarded as solely a structure.<sup>23</sup> To regard linguistic description as solely a calculus sacrifices the conception of language as a "structure sui generis" and the view of linguistic description as concerned solely with structures fails to explain how we can arrive at empirical statements<sup>24</sup> (and, indeed, how we can form predictions about phenomena, as is necessary when dealing with infinitely large sets.



Summary of the conditions of acceptability of linguistic descriptions

It will be obvious from the above that linguistic descriptions must be characterised, from the present point of view, as possessing a logical structure and meeting meta-theoretical conditions different in at least one important respect from those of linguistic theories. Linguistic descriptions must be characterised as being "empirical" in nature, i.e. they must be "directly scientific systems".

The principal points to bear in mind with respect to linguistic descriptions are that any description must be both theoretically justified (be constructed in accordance with some theory) and an empirical system. We sum up these requirements by saying that a linguistic description is an application of the theory to the phenomena. The first of these requirements will be met, if all the theoretical expressions in a description ("is a phoneme", "is subordinate to", etc.) are found in or are deducible from the theory and no theoretical expression applied in the description is not drawn from the theory. (We shall mean by "theoretical expression" a defined theoretical predicate or relation finding its place within the ontology and related in the appropriate ways to the methodology.) The description is then not ad hoc. We will require, in addition, that the description be consistent with respect to the theory, i.e. any

descriptive model must satisfy the conditions of the theoretical expression of which it is an application. We will say that a description is consistent with the theory, if, for all descriptive models in the description, there is no descriptive model which does not satisfy all the conditions (stated by definition) of the theoretical expression applied in constructing that model and if all necessary models appear in the description. If we say, for instance, that /p/ is a phoneme of English in an axiomatic functionalist description, then it must be the case that the descriptive model, /p/, is a phonological form, a minimum syntagmatic unit and a bundle of distinctive features. To say that /p/ is a bundle of (non-syntagmatic) distinctive features implies, among other things, that the description of English contain (somewhere) the distinctive features contained by /p/. (This is what is meant by saying that all necessary models must appear in the description.)<sup>25</sup>

On the other hand, we require any description to be an empirical system. A system will be "empirical", if the judgement of its validity depends ultimately on the experience of externally existing, observable phenomena, i.e. if its class of potential falsifiers is non-zero. In particular, we require, along with Popper, that the descriptive system and the descriptive models in it be capable of refutation with respect to the phenomena. Thus, any description must be consistent with the phenomena it purports to describe and it must be interpretable in terms of those phenomena - we call



this requirement the condition of "empirical adequacy".

The capacity to satisfy the condition of empirical adequacy is what principally distinguishes linguistic descriptions from linguistic theories. As far as the "internal structure" of descriptions is concerned, the conditions to be met by descriptions are similar to those to be met by theories, i.e. descriptions must also be consistent, sufficient (for describing the range of phenomena in question) and relatively simple. As we have seen, the logical structure of descriptions is not the same as that of theories. The description will not contain a semantics, an ontology or a methodology, but it will be a pair, consisting of a calculus and a structure via which empirical interpretation may take place. We require, finally, of course that descriptive calculi meet the conditions of axiomatic-deductive systems (mentioned in connection with theories - above).

NOTES TO CHAPTER 7

1. This formulation is quite close to Hjelmslev's (e.g. in Prolégomènes) but differs in including the notion of empirical validation.
2. Translated "ensemble autonome" in Mulder's "Postulats...", see defs. 1b and 1b<sup>1</sup>, pp. 23 - 4.
3. J. Mulder, Sets and Relations, pp. 9 - 12.
4. For a fuller discussion of structuralist views of science, see R. Harre, The Philosophies of Science.
5. The most notable linguist who has defended this view is, of course, Hjelmslev (see Prolégomènes or Résumé).
6. See J. Mulder, "Linguistic Theory,...." and L. Hjelmslev, Prolégomènes.
7. See Mulder, ibid. and Hjelmslev, ibid..
8. See J. Mulder, Sets and Relations, or J. Mulder and H. Hurren, "The English Vowel Phonemes".
9. Harre says, in the Philosophies of Science, that "science is a collection of well-attested theories which explain the patterns and regularities and irregularities among carefully studied phenomena", p. 62.
10. See P. Nidditch in his "Introduction" to The Philosophy of Science, who says, "broadly speaking, philosophers of science have, since the end of World War II, been chiefly occupied with the logic of science. They interpret science as a body of deductive or quasi-deductive systems of assertions..",



11. See K. R. Popper, The Logic of Scientific Discovery and many other places.
12. K. R. Popper, Logic of Scientific Discovery, pp. 40 - 42.
13. I. M. Bochenski, Europäische Philosophie der Gegenwart, p. 101.
14. K. R. Popper, Logic of Scientific Discovery, p. 41.
15. K. R. Popper, ibid., pp. 89 - 114.
16. The distinction is now accepted by Popper ; see The Logic of Scientific Discovery, 6<sup>th</sup> (Revised) Edition, 1972, p. 74, fn. \*2.
17. J. Winnie, "Theoretical Analyticity", p. 145.
18. It is important to see that the interpretation of a calculus is normally indirect, i.e. we reach observables via an abstract structure. This is obvious in physics, where sub-atomic objects are set up as constructs to account for observables (e.g. in cloud chambers). Similarly, abstract objects in linguistics, such as phonemes and syntagms, are set up to account for communicational observables.
19. See Ju. A. Shrejder, "Mathematical Models....", pp. 242 - 5, and J. Bridge, Beginning Model Theory.
20. See fn. 2 above.
21. It is quite clear that thousands of constructions meet the same conditions apart from the examples cited.
22. That is, as one conform with the usual version of hypothetico-deductivism as suggested by, e.g., G. Sampson, "One Fact Needs One Explanation", and many others.

23. As seems to be suggested by, for example, Hjelmslev, opera citanda, and Shaumjan, Principles.
24. The importance of empirical statements in linguistic description can not be underestimated ; see J. Mulder, "Linguistic Theory,....", p.92.
25. For the conditions on phonemes, see J. Mulder, Sets and Relations, and many other places.



CHAPTER EIGHT

PHONEME TABLES

AND DISTINCTIVE FEATURE COMBINATIONS

In previous chapters, we have considered the meta-theoretical conditions of acceptability applying to both linguistic theories and descriptions. In the last chapter we looked at examples of linguistic description in the light of these ideas. We have also specified the forms of linguistic theories and descriptions. In this chapter, we turn to a concrete example of theory building which is intended to illustrate how new theoretical apparatus can be set up in accordance with the principles previously established. To be precise, we develop some supplementary theoretical apparatus which allows us to model well-formed complexes of distinctive features on the basis of the mutual compatibility or exclusiveness of distinctive features. The apparatus helps us to see distinctive feature structure in a new light. In concentrating on the possible combinations and mutual exclusiveness of distinctive features in determining the well-formed complexes of distinctive features, the new apparatus is intended to supplement existing phonological theory which is concerned with the identity of phonological elements and their complexity. In particular, we are not concerned with the analysis of complex objects into their components ; we are concerned with the reverse of that analysis ; viz. the establishment of complex objects as a function of simple components. For the purposes of illustrating theory construction, we will be especially



concerned with showing how descriptions can be set up using the new apparatus, how the apparatus satisfies the demands placed upon it by the meta-theory and how it fits in with existing phonological theory.

a. Phoneme tables

Linguists are accustomed to the idea of analysing phonemes into their component distinctive features. Phoneme tables, or distinctive feature matrices, present information about the identity of distinctive features and about the structure of phonemes in terms of distinctive features. If we think of the table as presenting information about the structure of phonemes, it is clear that we presuppose the establishment of the identity of the proposed component distinctive features. We can think of the structure of the phoneme in two ways (when we consider a phoneme table). Broadly, we can think of the phoneme as analysed into component distinctive features or we can think of the phoneme as the product of (or function of) distinctive features. These two ways are merely different aspects of the same thing but the theoretical apparatus we require for the analysis of the complexity of phonemes is quite different from the apparatus we require in generating complexes of distinctive features.

Let us look at the types of table available. It was Jakobson<sup>1</sup> who first presented a type of matrix in

which the features are placed in the leftmost column and the phonemes are listed on the top row. The table is then plotted, as Jakobson explains,

"Let us exemplify the results of dissolving phonemes into bundles of distinctive features. The inventory of Standard Serbocroatian totals 29 qualitatively distinct phonemes and, if we add the phonemes distinguished by prosodic features, the amount of phonemes swells to 47. The whole pattern is based on eight dichotomous properties ; among them six inherent (or qualitative) features concerning the axis of simultaneity only (vocality, nasality, saturation, gravity, continuousness and voicing) and two prosodic features involving also the axis of successiveness (length and high tone). We mark by a plus sign only the presence of the feature in question ; the absence (as its opposite) is indicated by a minus sign only there, where no plus sign occurs at all. A complex combining both opposite terms is represented by the ± sign. To avoid longer comments, the current spelling form is used for denoting the Serbocroatian phonemes." <sup>1</sup>

There have been various developments of the Jakobsonian table but it remains clearly a two-dimensional table of this type. We give the example of Serbocroatian following Jakobson.



	t	d	c	s	z	p	b	f	v	ć	č	š	ž	k	g	x	n	m	ń	r	l	l'	i	u	e	o	a	
Vocality.....																												
Nasality.....																												
Saturation...																												
Gravity.....																												
Continuoussness																												
Voicing.....	-	+																										

TABLE 1

Qualitatively distinguished phonemes in Serbocroatian following Jakobson<sup>1</sup> and an analysis of their distinctive features.

The type of table presented by, for example, Martinet<sup>2</sup> is also a two-dimensional table. In the Martinet-style of matrix, there is one set of distinctive features in the column on the left and another (different) set of features on the top row. Each phoneme is then plotted as a function of these two dimensions, i.e. each phoneme in the table is equivalent to a pair of distinctive features. A typical Martinet-style of matrix might be that given by Mulder<sup>3</sup> for Pekingese (see Table 2). Another way of looking at this is to say that a phoneme, as a simultaneous bundle of distinctive features, is equivalent to a pair,  $\langle x,y \rangle$ , of distinctive features and the set of pairs is the product of the cartesian multiplication of the two discrete and disjunct sets or dimensions of distinctive features ( see Diagram 1).

	unaspirated	aspirated	fricative	nasal
Labial	b	p	f	m
Apical	d	t		n
Hissing	z	c	s	
Hushing	ʒ	č	š	
Dorsal	g	k	h	ŋ

TABLE 2

Two-dimensional table of distinctive features for Pekingese following Mulder<sup>3</sup>. (To complete the picture for "consonantal" phonemes, one would have to add the single feature phonemes, /l/, /r/ and /r̄/.)



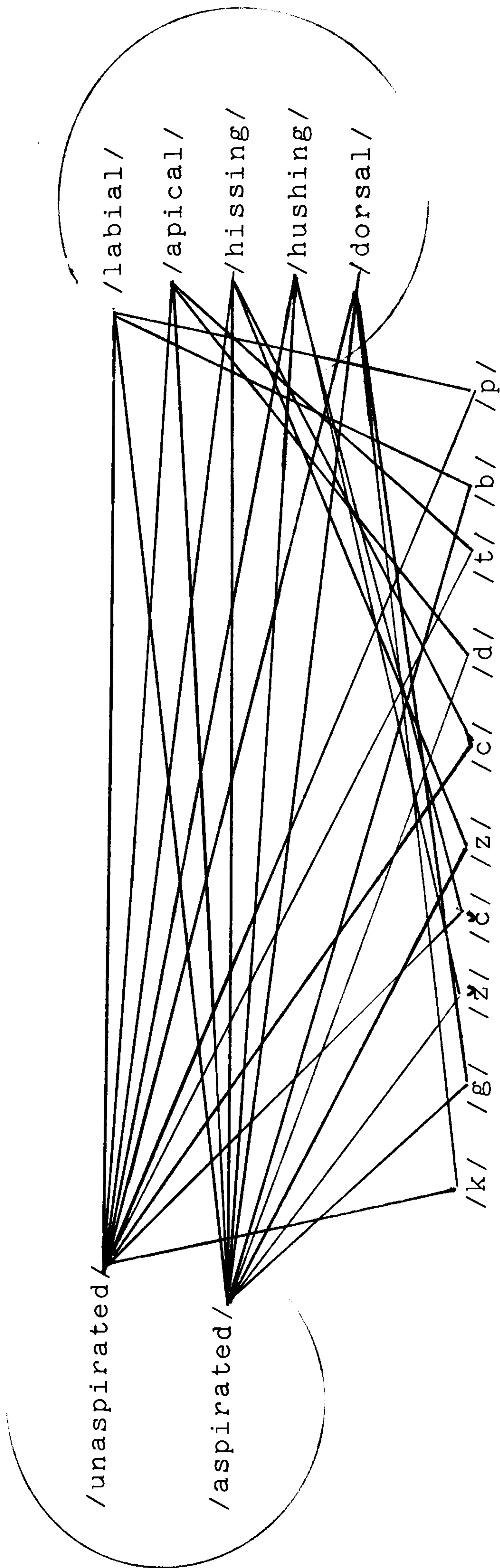


DIAGRAM 1

Partial Cartesian multiplication of distinctive feature dimensions for Pekingese and mapping onto the set of phonemes.

As Mulder<sup>4</sup> and Hill<sup>5</sup> have pointed out, however, there is no reason to think that linguistic systems are restricted to two-dimensional systems of distinctive features. There might be three sets of distinctive features involved, as in English (see Table 3 following Mulder<sup>4</sup>) or there might be four or more dimensions involved.

	Stop		Fricative		Nasal
	Voiced	Unvoiced	Voiced	Unvoiced	
Labial	b	p	v	f	m
Apical	d	t	ð	θ	n
Dorsal	g	k			ŋ

TABLE 3A

3-dimensional Cartesian table for part of the English consonantal system (following Mulder<sup>4</sup>). Of course, the nasal phonemes are 2-dimensional. The analysis is completed by setting up the following 2-dimensional table and by specifying the features /l-ness/ and /h-ness/ for the single feature phonemes /l/ and /h/. The case of the marginal phoneme, /x/, in Scottish English is ignored.



	Voiced	Unvoiced
Hissing	z	s
Hushing	ʒ	ʃ

TABLE 3B

Russian is an example of a language with four sets or dimensions of distinctive features (see Table 4A).<sup>6</sup> Of course, the increase in the number of sets involved does not affect our view of phonemes as combinations of distinctive features. We should note merely that we must regard phonemes as n-tuples of distinctive features rather than as simply pairs. Similarly, we will think of cartesian multiplication as, an operation on more than two sets, if necessary. Also, as Mulder has pointed out, there may be occasions when  $n = 1$ , i.e. where we have phonemes such as /l/ and /h/ in English which possess only one distinctive feature. In these cases, clearly, no cartesian multiplication is involved. As we said, above, we can equally well look at phonemes analytically. That is, we can think of the distinctive features and their organisation as an analysis of the minimum entities contracting ordering relations (i.e. phonemes are the minimum entities forming phonotactic or non-simultaneous constructions).

		Stop		Fricative		Nasal
		Voiced	Unvoiced	Voiced	Unvoiced	
Labial	Palatalised	b'	p'	v'	f'	m'
	Unpalatalised	b	p	v	f	m
Apical	Palatalised	d'	t'	z'	s'	n'
	Unpalatalised	d	t	z	s	n
Dorsal	Palatalised	g	k'		x	
	Unpalatalised		k			

TABLE 4A

4-dimensional cartesian table for part of the Russian consonantal system. The analysis is completed by setting up the following two 2-dimensional tables.



	Voiced	Unvoiced
Hushing	ʒ	ʒ̥

TABLE 4B

	Palatalised	Unpalatalised
l-ness	l'	l
r-ness	r'	r

TABLE 4C

(These two subsystems connect with the main cartesian matrix through the dimensions, /voiced/ - /unvoiced/ and /palatalised/ - /unpalatalised/, respectively).

One disconcerting problem here is that it is not really very clear what the sets of distinctive features or dimensions of distinctive features are. For Martinet there are two dimensions for consonants, series and order, which correspond to the phonetic distinction between 'point of articulation' and 'manner of articulation'. Distinctive features are grouped into classes on the basis of their mutual exclusion with respect to these two criteria. Thus, we have a series of points of articulation from labial to pharyngeal. Phonemes are then further analysable by manner of articulation, e.g. presence of voice, nasality, obstruction, etc.. Where we are dealing with a system in which there are three or more dimensions, we might have two or more dimensions corresponding to 'manner of articulation'. In Russian, furthermore, a dimension of 'manner of articulation', /palatalised/ - /unpalatalised/, is combined with the features in the series or 'points of articulation' (see Table 4A, above). In other words, a simple correspondence between series and point of articulation and between order and manner of articulation is no longer possible. In particular, we end up in the dark about the definition of dimension. Martinet's dimensions are, in any case, a preliminary form of analysis and look much more like the phonetic exponents of phono-



logical dimensions than they, perhaps, should. (It is this, no doubt, that led Hjelmslev to regard phonemes as the minimum units of phonology and distinctive feature analysis as part of phonetics.) We require a phonological definition of dimension.

In order to clarify the notion of dimension, we can use the ideas of mutual exclusion and mutual combinability. A dimension can then be understood as a set of mutually opposed distinctive features in which the identity of each feature is defined by reference to the oppositions it enters within the set. The identity of each feature is then defined negatively, relationally and oppositionally and is the negation of the sum of features which commute with it in an identical context. Distinctive features of different dimensions are cartesian multiplied to form complexes of distinctive features. In some cases two dimensions are involved and in other cases there are more than two dimensions. As Mulder has pointed out, however, the definition of the identity of distinctive features is not as straightforward a business as it might appear at first sight. Since identity is defined by reference to mutual exclusiveness in a given context, we may find, as Mulder says, that a single item in the table refers to two or more distinct identities. In the case of the phoneme table

for Pekingese (above), for example, the identity of "labial" in the complex, /labial, aspirated/, is not the same as the identity of "labial" in /labial, nasal/, since, as inspection of the table will show, the set of oppositions entered by "labial" in the context of /aspirated/ is different from the set of oppositions "labial" enters in the context of /nasal/. In the article in question, Mulder overcomes this problem by introducing a modified Jakobsonian distinctive feature matrix which makes plain the differences in the functional identity of a feature in different contexts. As Mulder explains,

"If we mark "+" for a feature and we note "-" for an opposition into which it enters and "0" for a gap, we get the following scheme. The number and place of the items "-" in any one dimension determine the value of the item "+" in that dimension. I use a thick line to separate the two dimensions.

"This table speaks for itself. Though we may still say that, for instance, /b/ has the distinctive feature {labial, unaspirated}, and, for instance, /m/ has the distinctive features, {labial, nasal}, it is clear from the scheme that the label "labial" refers to items of different identity and, hence, to not exactly the same items, functionally speaking, in the two cases."<sup>7</sup>



	b	p	f	m	d	t	n	z	c	s	ʃ	ʒ	ʁ	ʕ	g	k	ŋ
Labial	+	+	+	+	-	-	-	-	-	-	-	-	-	-	-	-	-
Apical	-	-	0	-	+	+	-	-	0	-	-	-	-	0	-	-	-
Hissing	-	-	-	0	-	0	+	+	+	+	-	-	-	-	-	-	0
Hushing	-	-	-	0	-	0	-	-	-	+	+	+	+	-	-	-	0
Dorsal	-	-	-	-	-	-	-	-	-	-	-	-	-	-	+	+	+
Unaspirated	+	-	-	-	+	-	-	+	-	-	-	+	-	-	+	-	-
Aspirated	-	+	-	-	-	+	-	-	+	-	-	+	-	-	-	+	-
Fricative	-	-	+	-	0	0	0	-	+	+	+	-	-	-	-	+	-
Nasal	-	-	-	+	-	+	0	0	0	0	0	0	0	0	-	-	+

TABLE 5

Jakobsonian-type table for Pekingese consonants following Mulder.<sup>7</sup>

Quite clearly, when we interpret the notion "dimension" in this way and incorporate the Jakobsonian-type table into the description of the identity of distinctive features, we are a long way from any phonetic definition of the notion "dimension" and we avoid the difficulties referred to above. As Mulder goes on to show, however, there are still problems of the identity of distinctive features concerned with gaps in phoneme tables. Where gaps occur, due to the absence of a dimension, no serious problems arise, because we can simply describe which dimensions are involved in which cases. In other cases, however, such as that of the phoneme, /x/, in Russian (see Table 4A above), there is a phoneme which we would expect to be represented by the features, /dorsal, unpalatalised, fricative, unvoiced/. However, the feature /unvoiced/, is not opposed to the feature, /voiced/ in the context of /dorsal, fricative/, although there is opposition between /voiced/ and /unvoiced/ in the context of /dorsal, stop/ and /dorsal, fricative/. In terms of the system, we expect, as it were, an opposition, /voiced/ - /unvoiced/, in this case and it is this opposition which links the three- and four-dimensional systems to the two-dimensional system (see Table 4B). Following Mulder<sup>7</sup>, we will say, the phoneme /x/ represents both /dorsal, fricative, unvoiced/ and /dorsal, fricative, voiced/. It is a hyperphoneme.



In this case, there is no further feature with which /voiced/ and /unvoiced/ are in opposition, so the distinctive feature specification for /x/ will be simply /dorsal, fricative/. The phoneme, /g/ in Russian, is also a hyperphoneme. Although there is a connection between the dorsals and the rest of the system, because /k/ - /k'/ displays the opposition between /palatalised/ and /unpalatalised/, this opposition is not present for the voiced, dorsal stop. ([g] and [g'] are in complementary distribution, as are [x] and [x']).

If there had been a third feature involved, we might have set up a hyperfeature, /voiced / unvoiced/, as Mulder does for Kamali Arabic in the article cited.<sup>8</sup> In that case, the opposition between two features, /voiced/ and /pharyngal/ is suspended in the distinctive feature system in the case of the phoneme, /ʒ/, but a third feature, /unvoiced/ , is opposed to the suspension of features in the same dimension. This factor makes the suspension of features , or hyperfeature, functional in that dimension - it has identity.

	Occlusive			Fricative			Nasal
	Unv'd	Voiced	Pharyngal	Unv'd	Voiced	Phar'gal	
Labial		b			f		m
Apical	t	d	ṭ	θ	ð	ð̣	n
Hushing	ç		ç̣		ç		
Dorsal	k	g	q	x		χ	
Glottal	ʔ		ʕ	h		ħ	

TABLE 6

Phoneme Table of Kamali Arabic consonants quoted by Mulder.<sup>8</sup>

As we have said, in looking at phonemes as complexes of distinctive features or in generating complexes of distinctive features to map onto phonemes, we presuppose the identity of distinctive features and we will clearly have to take into account the problems raised by hyperphonemes and hyperfeatures. If we turn now to the question of generating complexes of distinctive features, we will find that the Martinet-style table (whether two or more dimensional) has drawbacks in this respect also.

It is not the case, for instance, that all the features in one dimension of a phoneme table combine with all the features in another dimension. That is,



there are restrictions on the cartesian multiplication of sets of distinctive features. Thus, in English, /š/ is a two-dimensional phoneme equivalent to /hushing, voiced/, whereas /p/ is three-dimensional and equivalent to /labial, unvoiced, stop/. Therefore, /labial/ but not /hushing/ combines with /stop/, even though /labial/ and /hushing/ appear in the same dimension. To take matters from another point of view, the features, /nasal/ and /dorsal/, belong to two different dimensions of distinctive features in Russian (see Table 4A above), where the sets of features in question can be cartesian multiplied to produce well-formed pairs of distinctive features. However, there is no phoneme in Russian which contains or is equivalent to the pair /dorsal, nasal/. That is, cartesian multiplication of the series set with the order set would produce a non-pair. We have already seen some of the problems of identity concerning accidental gaps. Such gaps also cause problems in determining the set of well-formed complexes of distinctive features in the usual Martinet-style matrix. Cartesian multiplication of the dimensions of a phoneme table (such as in Table 4A) for Russian would generate as well-formed the complex, \*/dorsal, voiced, fricative/ to which no phoneme corresponds. Careful analysis of the complexity of the phoneme, /x/, and its categorisation as a hyperphoneme helps us to overcome the problem but, as a consequence, we see the

weakness of the phoneme table. It does not give a very accurate picture of the combinatory possibilities of distinctive features. In the case of the above table for Russian consonants at least two non-pairs of distinctive features are generated. The phoneme table also rather glosses over the fact that the feature, /dorsal/, can only be combined in Russian with either of the features, /palatalised/ or /unpalatalised/, in the case of an unvoiced stop. Also, in the table 4B (above), we have detached the two-dimensional phonemes, /ž/ and /š/, from the phoneme table. Clearly, they are connected to the major system through the opposition, /voiced/ - /unvoiced/, just as the phonemes, /l/, /l'/, /r/ and /r'/, are connected through the opposition, /palatalised/ - /unpalatalised/, to the main system (Table 4C). Of course, it is natural to find that there is no pair, /hushing, nasal/, and one would not expect to find an opposition between voiced and unvoiced "l" phonemes and "r" phonemes (although they might occur). The point is, however, that the phoneme tables present combinatory restrictions as a fait accompli without explaining their nature. It is also the case that in many presentations we find the phonemes of tables, 4A, 4B and 4C, lumped together into one phoneme table with large numbers of gaps which could not, even in principle, be filled precisely because of the combinatory restrictions operative but unexplained in the language. That is, we have already



been rather more subtle than is normal with our phoneme tables.

b. A new type of distinctive feature matrix

In order to show the structure of distinctive feature systems from the point of view of the possibilities of combination and mutual exclusion of distinctive features in forming well-formed complexes, we will present a new sort of distinctive feature matrix which overcomes the deficiencies of the older types of matrix. On the basis of this new type of matrix, we will set up classes of mutually exclusive distinctive features. Because these classes are not identical with dimensions of opposed distinctive features, we will call them categories of distinctive features in order to avoid any terminological confusion.

Let us say that any pair of distinctive features which may be combined contract a simultaneous constructional relation and we will denote this relation by 'S'. Thus, if x and y are distinctive features and x and y may be combined, then  $x S y$ . The features, x and y, may be combined, if and only if there is a phoneme, / $\alpha$ /, such that  $/\alpha/ \equiv x, y$  or  $/\alpha/ \equiv \langle \dots x, y, \dots \rangle$ , i.e. if and only if there is a phoneme which is equivalent to that pair or which is equivalent to an n-tuple containing that pair. Thus, we have in English /labial/ S /voiced/ and /unvoiced/ S /stop/ because of the phonemes, /b/ (equivalent to  $\langle$ voiced, labial, stop $\rangle$ ) and /t/

(equivalent to <unvoiced, apical, stop>). However, we do not find the construction /nasal/ S /hushing/, i.e. - /nasal/ S /hushing/.

We now proceed to set up a new type of two-dimensional matrix for Russian which will express the possibilities of combination of distinctive features.

We can set up the matrix in the following way. Let the distinctive features be placed in any arbitrary order in the lefthand column, 1...n. The features are then placed in the top row in the same order, 1...n. The value,  $\beta$ , for the  $i^{\text{th}}$  feature in the column and the  $j^{\text{th}}$  feature in the row will be  $\beta_{i,j}$ . The value,  $\beta_{i,j}$  will express the combinatory possibilities of the features  $i$  and  $j$ . If  $i$  and  $j$  may be combined, i.e. if  $i$  S  $j$ , then we will fill in "1", if not, i.e. if -  $i$  S  $j$ , then we will fill in "0". This is the normal sense of the word "matrix" as used in mathematics. We have simply applied the notion to linguistics. Thus, we will have,

$$\beta_{i,j} = \begin{cases} 1 & \text{iff } i \text{ S } j \\ 0 & \text{iff } - i \text{ S } j \end{cases}$$

The table for Russian will look as follows;



	Labial	Apical	Dorsal	Hushing	l-ness	r-ness	Stop	Fricative	Nasal	Voiced	Unvoiced	Palatalised	Unpalatalised
Labial	0	0	0	0	0	0	1	1	1	1	1	1	1
Apical	0	0	0	0	0	0	1	1	1	1	1	1	1
Dorsal	0	0	0	0	0	0	1	1	0	1	1	1	1
Hushing	0	0	0	0	0	0	0	0	0	1	1	0	0
l-ness	0	0	0	0	0	0	0	0	0	0	0	1	1
r-ness	0	0	0	0	0	0	0	0	0	0	0	1	1
Stop	1	1	1	0	0	0	0	0	0	1	1	1	1
Fricative	1	1	1	0	0	0	0	0	0	1	1	1	1
Nasal	1	1	0	0	0	0	0	0	0	0	0	1	1
Voiced	1	1	1	1	0	0	1	1	0	0	0	1	1
Unvoiced	1	1	1	1	0	0	1	1	0	0	0	1	1
Palatalised	1	1	1	0	1	1	1	1	1	1	1	0	0
Unpalatalised	1	1	1	0	1	1	1	1	1	1	1	0	0

TABLE 7

New-style cartesian matrix for the Russian consonantal system.

This table expresses quite clearly the combinatory possibilities and restrictions for all pairs of consonantal features.<sup>9</sup> The table tells us, for instance, whether /labial/ combines with /stop/. It does not tell us whether the combination, /labial, voiced, palatal, stop/, is a well-formed complex. Also, we have not yet arrived at categories of distinctive features; these will emerge from the matrix.

First, however, if we look at table 7, we will observe a number of points. In the first place, since no feature ever combines with itself, we can see that the matrix forms a Kronecker Delta. That is, a matrix in which the diagonal values from top left to bottom right are all the same (in this case "0"). Secondly, the table is, of course, symmetrical in that the triangle above the diagonal exactly matches the triangle below it. More importantly, we observe that some features are identical with respect to the relation "S". Other features are partially alike and others have little or nothing in common. For instance, /labial/ and /apical/, /voiced/ and /unvoiced/ and /palatalised/ and /unpalatalised/ are three pairs which are identical with respect to mutual exclusion and combinability with other features. That is, these three pairs of features are mutually exclusive and combinable with exactly the same sets of features. If we compare /labial/ or /apical/ with /dorsal/, however, we will find that the values are only partly alike, since /dorsal/, but not /labial/ or /apical/, is mutually



exclusive with /nasal/. Similarly, /nasal/ is mutually exclusive with both /voiced/ and /unvoiced/ and with /stop/ and /fricative/, whereas /stop/ and /fricative/ are combinable with /voiced/ and /unvoiced/. If we compare the values for /labial/ and /voiced/, for example, we will find still less in common.

Now, let us propose the following definition ;  
a category of distinctive features is a set of distinctive features which are mutually exclusive with respect to the relation "S". That is, a set of distinctive features such that no pair in the set contract the relation of simultaneity. If we let "C" denote category, we get the following formal definition ;

$$C = \left\{ \text{distinctive features, } d^1, \dots, d^n : - (E d^i, d^j) \right. \\ \left. (d^i S d^j) \right\}.$$

It is worth pointing out here that the relation of mutual opposition, which is used to define identity, does not wholly coincide with the definition of mutual exclusion. Mutual exclusion, as defined here, is the absence of combinability for a given pair of features. Mutual exclusion will often coincide with mutual commutability in a given context. It is commutability which is the test of opposition. However, mutual exclusion does not always involve commutability. The feature, /dorsal/, in Russian is, for example, mutually exclusive with the feature, /nasal/. There is no complex, \*/dorsal,nasal/. However, there is also

no case where /nasal/ and /dorsal/ commute. This is not unusual. The features, /hushing/ and /palatalised/, neither combine nor commute, etc.. Very occasionally, we may find the reverse of this situation. That is, cases where distinctive features which enter the same dimension (i.e. the same set of mutually opposed distinctive features) can nevertheless form combinations. The most obvious cases of this phenomenon are those of the "semi-cluster". A semi-cluster, such as /dʒ/, in English, is a pair of phonemes which, in certain contexts, contract an ordering relation but which, in other contexts, function as a single phoneme. When functioning as a single choice in the chain a semi-cluster is described as possessing the totality of the distinctive features of the constituent phonemes. Thus, /dʒ/ has the features /apical, hushing, occlusive, voiced/, where /apical/ and /hushing/ enter the same dimension. The case of the semi-cluster is admittedly rather marginal.<sup>10</sup> However, the case of the phoneme, /pf/, in German, is, as Mulder points out, less so.<sup>11</sup> That phoneme is described as /labial, occlusive-fricative/, where /occlusive/ and /fricative/ enter the same dimension but are equally represented by the phoneme in question.

If we now return to the above definition of category and apply it in the case of Russian, we find that some features fall into two or more categories.



Thus, for example, /nasal/ is mutually exclusive with /voiced/ and /unvoiced/ and is also mutually exclusive with /stop/ and /fricative/. We can represent this diagrammatically as follows,

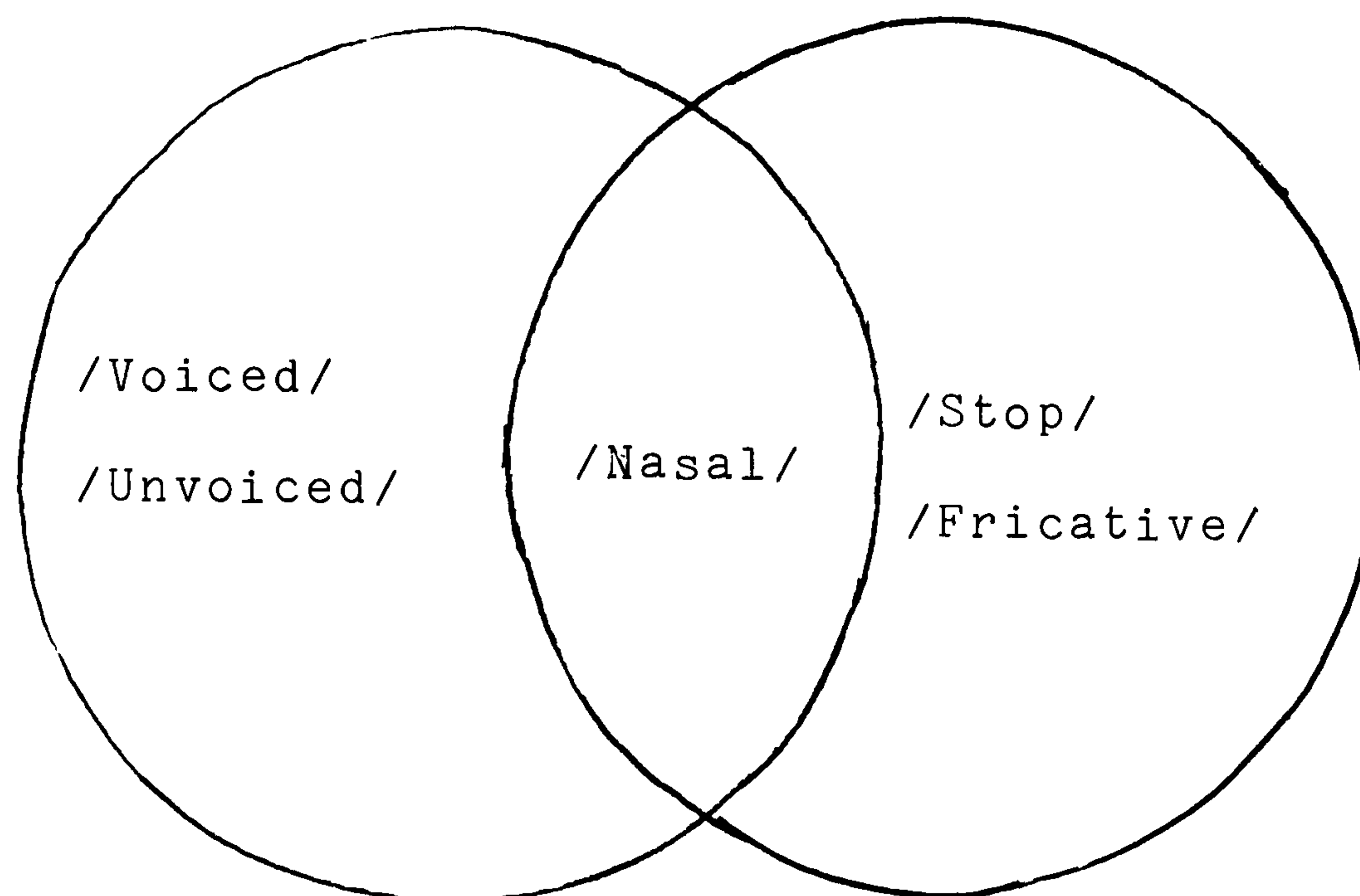


DIAGRAM 2

Venn diagram showing /nasal/ at the intersection of two categories (in Russian).

Here the circles represent sets of mutually exclusive features, i.e. sets of features which cannot be combined. Whereas we can have the combinations,  $\langle /voiced/, /fricative/ \rangle$ ,  $\langle /voiced/, /stop/ \rangle$ ,  $\langle /unvoiced/, /fricative/ \rangle$ ,  $\langle /unvoiced/, /stop/ \rangle$ , we can never have  $\langle /nasal/, /stop/ \rangle$ ,  $\langle /nasal/, /fricative/ \rangle$ ,  $\langle /nasal/, /voiced/ \rangle$ , or  $\langle /nasal/, /unvoiced/ \rangle$ . Using the above table 7, we can read off these sets of mutually exclusive features and their intersections. They can be represented in the following Venn diagram.

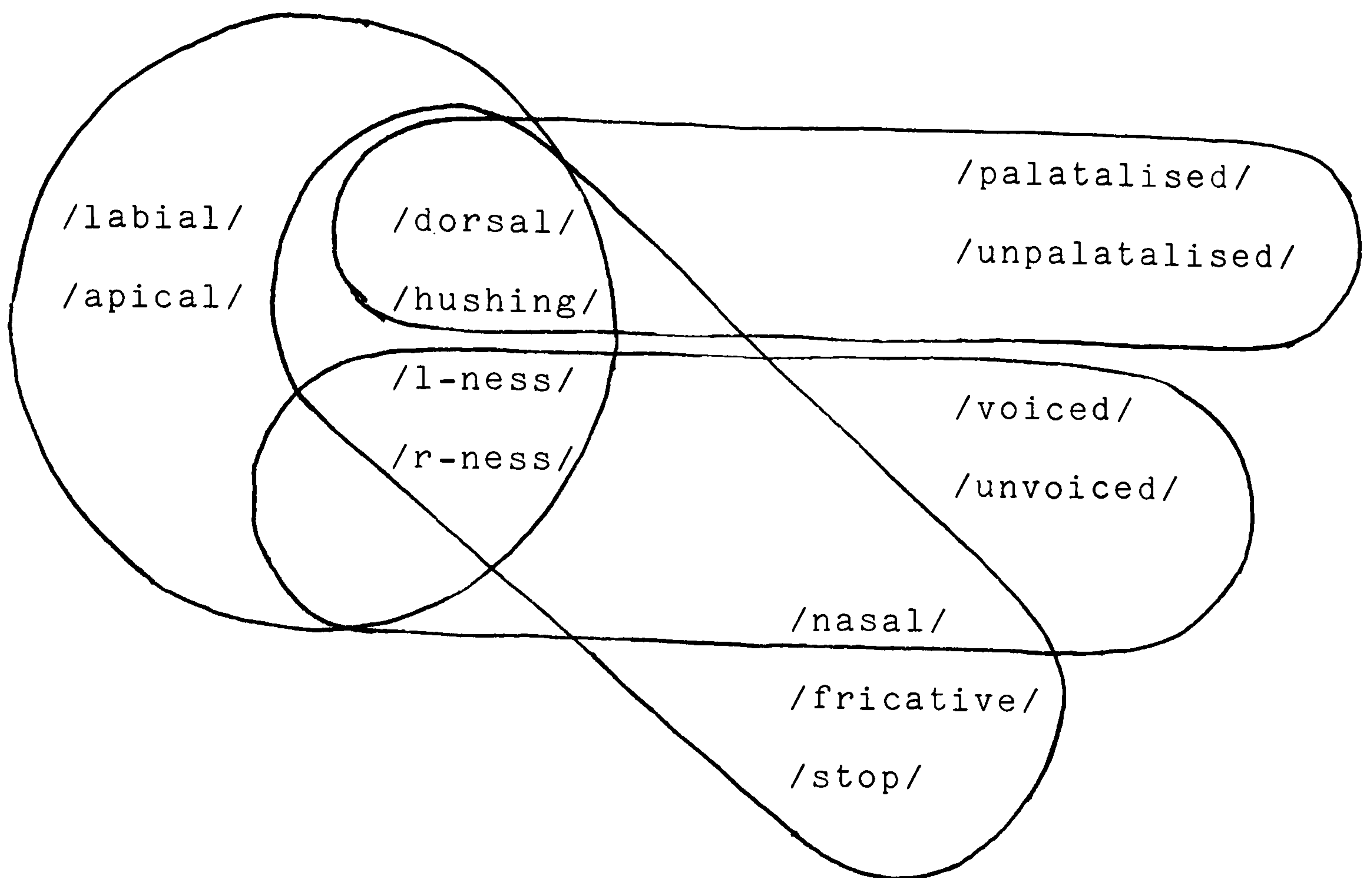


DIAGRAM 3

Venn diagram for Russian distinctive feature categories (consonantal).

This definition has some unusual consequences. The most unusual is that not only is a feature like /nasal/ found at the intersection of categories but we also find a feature such as /dorsal/ in the same category as /nasal/. Similarly, /hushing/ is mutually exclusive with the series /labial/, /apical/ and /dorsal/, as we might expect, but it is also mutually exclusive with the orders /palatalised/, /unpalatalised/ and with the order feature /nasal/. We find that the features, /l-ness/ and /r-ness/, are mutually exclusive with all other features except /palatalised/ and /unpalatalised/. We can list the categories of Russian, as follows ;



Category 1	Category 2	Category 3	Category 4
labial	palatalised	voiced	stop
apical	un- palatalised	unvoiced	fricative
dorsal		l-ness	nasal
hushing	hushing	r-ness	hushing
l-ness		nasal	l-ness
r-ness			r-ness

TABLE 8

Categories of Russian distinctive features based on Table 7.

If we set up this kind of matrix for English, as below, a number of interesting points emerge.

	labial	apical	dorsal	hissing	hushing	l-ness	h-ness	stop	fricative	nasal	voiced	unvoiced
labial	0	0	0	0	0	0	0	1	1	1	1	1
apical	0	0	0	0	0	0	0	1	1	1	1	1
dorsal	0	0	0	0	0	0	0	1	0	1	1	1
hissing	0	0	0	0	0	0	0	0	0	0	1	1
hushing	0	0	0	0	0	0	0	0	0	0	1	1
l-ness	0	0	0	0	0	0	0	0	0	0	0	0
h-ness	0	0	0	0	0	0	0	0	0	0	0	0
stop	1	1	1	0	0	0	0	0	0	0	1	1
fricative	1	1	0	0	0	0	0	0	0	0	1	1
nasal	1	1	1	0	0	0	0	0	0	0	0	0
voiced	1	1	1	1	1	0	0	1	1	0	0	0
unvoiced	1	1	1	1	1	0	0	1	1	0	0	0

TABLE 9

New-style cartesian matrix for English consonants.

Firstly, we find the following categories ;



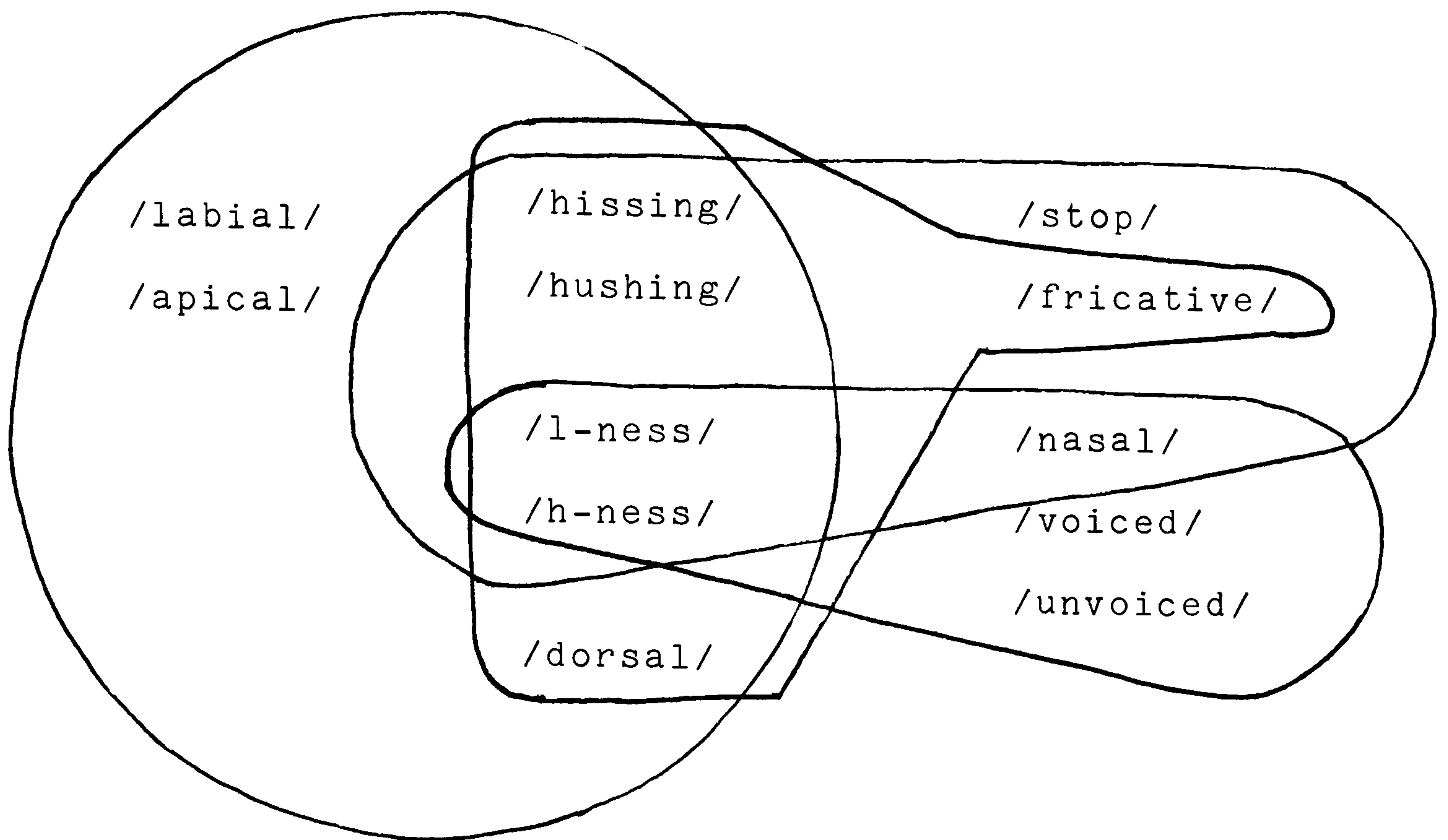


DIAGRAM 4

Distinctive feature categories of English Consonantal phonemes as a Venn diagram (see tables 3A and 3B). Note that /-ness/ and %h-ness/ stand at the intersection of all sets.

Here we find not three categories, as we might expect from an inspection of the three dimensions of table 3A, but we find that, on our present definition, we must set up four categories - one of which cuts across two others. If we look at Mulder's analysis of Pekingese (table 2), we can set up the following matrix :

	labial	apical	hissing	hushing	dorsal	unaspirated	aspirated	fricative	nasal	l-ness	r-ness	r̄-ness
labial	0	0	0	0	0	1	1	1	1	0	0	0
apical	0	0	0	0	0	1	1	0	1	0	0	0
hissing	0	0	0	0	0	1	1	1	0	0	0	0
hushing	0	0	0	0	0	1	1	1	0	0	0	0
dorsal	0	0	0	0	0	1	1	1	1	0	0	0
unaspirated	1	1	1	1	1	0	0	0	0	0	0	0
aspirated	1	1	1	1	1	0	0	0	0	0	0	0
fricative	1	0	1	1	1	0	0	0	0	0	0	0
nasal	1	1	0	0	1	0	0	0	0	0	0	0
l-ness	0	0	0	0	0	0	0	0	0	0	0	0
r-ness	0	0	0	0	0	0	0	0	0	0	0	0
r̄-ness	0	0	0	0	0	0	0	0	0	0	0	0

TABLE 10

New-style matrix for Pekingese consonants following Mulder (see Table 2 above).

Converting this to a Venn diagram, we again obtain more categories than dimensions - in this case four categories as opposed to two dimensions (see Table 2 above).



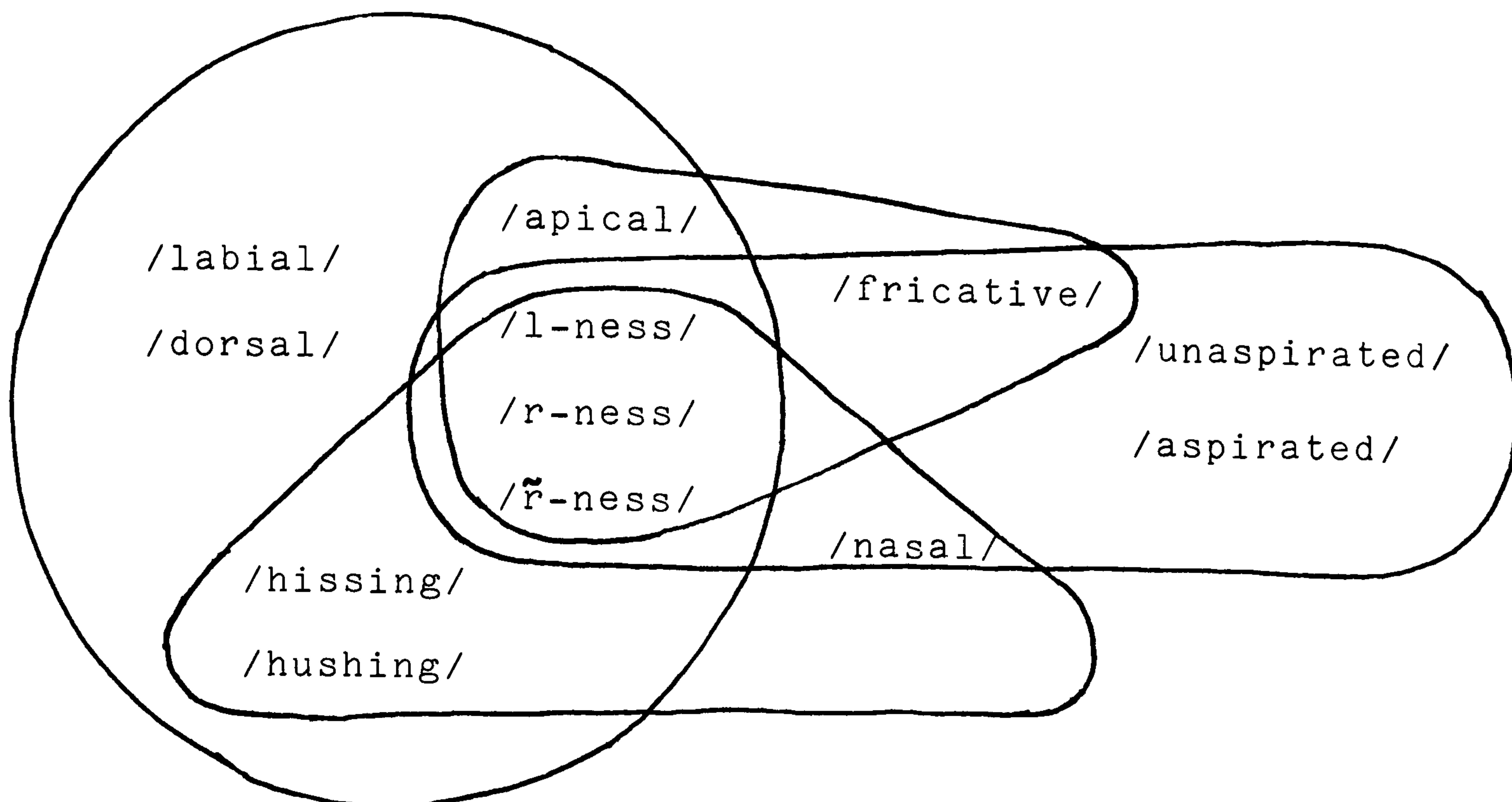


DIAGRAM 5

We can see that an interesting feature of Pekingese is that the distinctive features, /l-ness/, /r-ness/ and /r̃-ness/, are mutually exclusive with all other features and so they stand at the intersection of all categories. The same is true of the features, /l-ness/ and /h-ness/, in English.

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So far, we have been concerned only with pairs of features, i.e. those pairs which contract the relation, "S" and those which do not contract this relation. In many languages, including English and Russian, there are, of course, n-tuples of features, where n is greater than two. In such cases, we must find a way of specifying the well-formed n-tuples. We can do this in two stages. Firstly, we construct a lattice representing the combinatory possibilities of all distinctive





Here are some other lattices for Russian ;

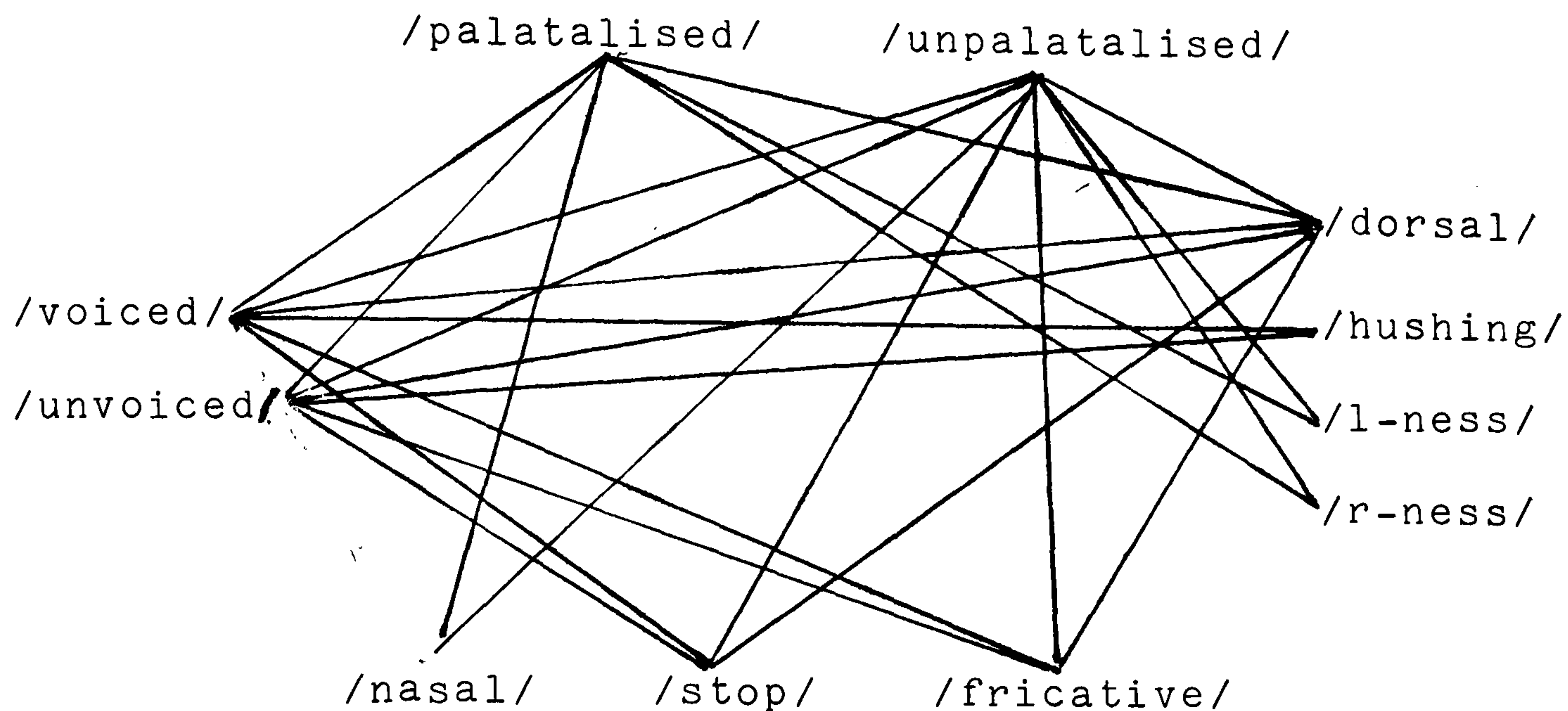


DIAGRAM 8

Combined 2, 3 and 4 category lattice for Russian.

Our function will specify all the maximum well-formed n-tuples of distinctive features. It also systematically excludes impossible combinations. The combination, /nasal, dorsal/, is excluded because the features, /nasal/ and /dorsal/, are never connected by — . Similarly, we never find n-tuples containing both /hushing/ and /palatalised/ or /unpalatalised/ and we never find /l-ness/ or /r-ness/ in combination with either /voiced/ or /unvoiced/, since all of these features are in the same category. A systematically excluded phoneme is one for which, in principle, there could

never be a corresponding, maximum, well-formed n-tuple of distinctive features.

On the other hand, we can see from the above lattices that the combinations, /voiced, dorsal, palatalised, stop/ and /voiced, dorsal, unpalatalised, stop/, are defined as well-formed. This is because all of those features are connected by a line; viz. /dorsal/ and /voiced/, /dorsal/ and /palatalised/, /dorsal/ and /unpalatalised/, /dorsal/ and /stop/, /voiced/ and /palatalised/, /voiced/ and /unpalatalised/, /voiced/ and /stop/, /palatalised/ and /stop/ and /unpalatalised/ and /stop/. All these pairs separately contract the relation "S",

We have already seen, however, that there is no correlation, \*/g/ - /g'/, in Russian. Although palatalised and unpalatalised, voiced, dorsal stops occur, they are in complementary distribution. The phoneme, /g/, is then a hyperphoneme with the features, /voiced, dorsal, stop/, which also represents<sup>12</sup> the features, /palatalised/ and /unpalatalised/. In a case of this sort, we require an additional convention, viz. that mutually exclusive features (features in the same category) may be connected by a line, if they are represented by the same hyperphoneme. The line would be in force only in a fixed context. For instance, /palatalised/ and /unpalatalised/ would be joined only in the case where both of these features are connected to all of /stop/, /dorsal/ and /voiced/. This is not



a case where we find the relation, /palatalised/ S /unpalatalised/, and consequently we join the features in question by a broken line, ----- . We will have to amend the conditions for a well-formed lattice to include the broken line. We will say simply that a broken line completes the lattice in the case of a hyperphoneme and thus all features joined in the graph are represented by the hyperphoneme in question. In the case of /g/ in Russian, we obtain the following lattice,

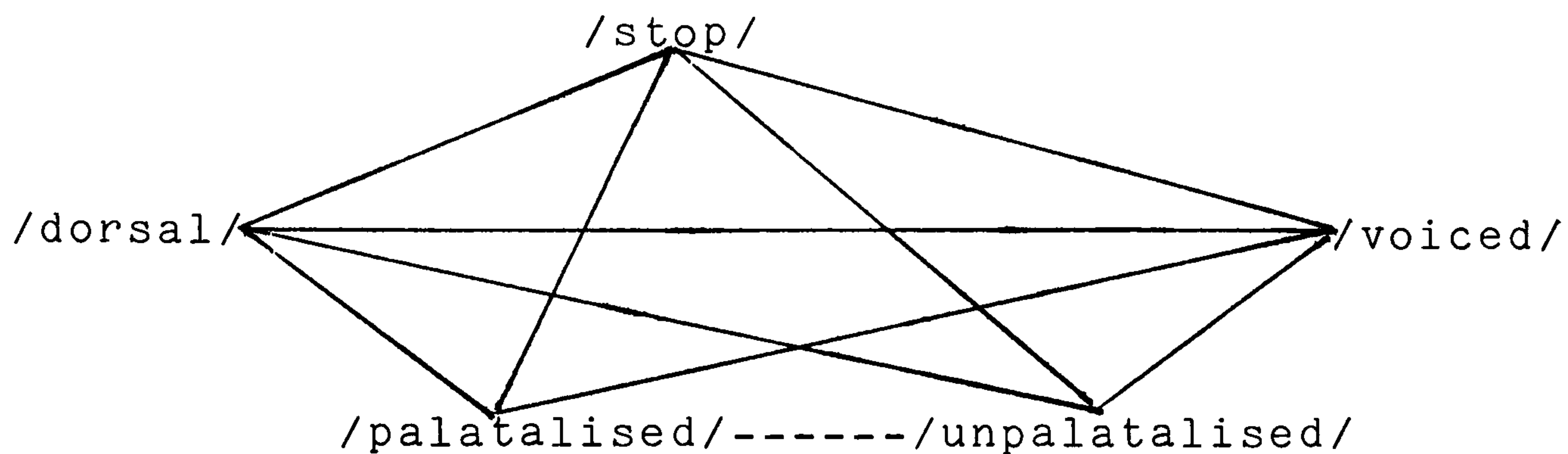


DIAGRAM 9

The phoneme /x/ in Russian represents a slightly more complicated case, since /x/ has the features, /dorsal, fricative/, but represents the features, /dorsal, fricative, voiced, unvoiced, palatalised, unpalatalised/. The reason for this is, as we can see from the lattice, both /dorsal/ and /fricative/ contract the relation "S" with /voiced/, /unvoiced/, /palatalised/ and /unpalatalised/. With broken lines, where appropriate, we obtain the following lattice,

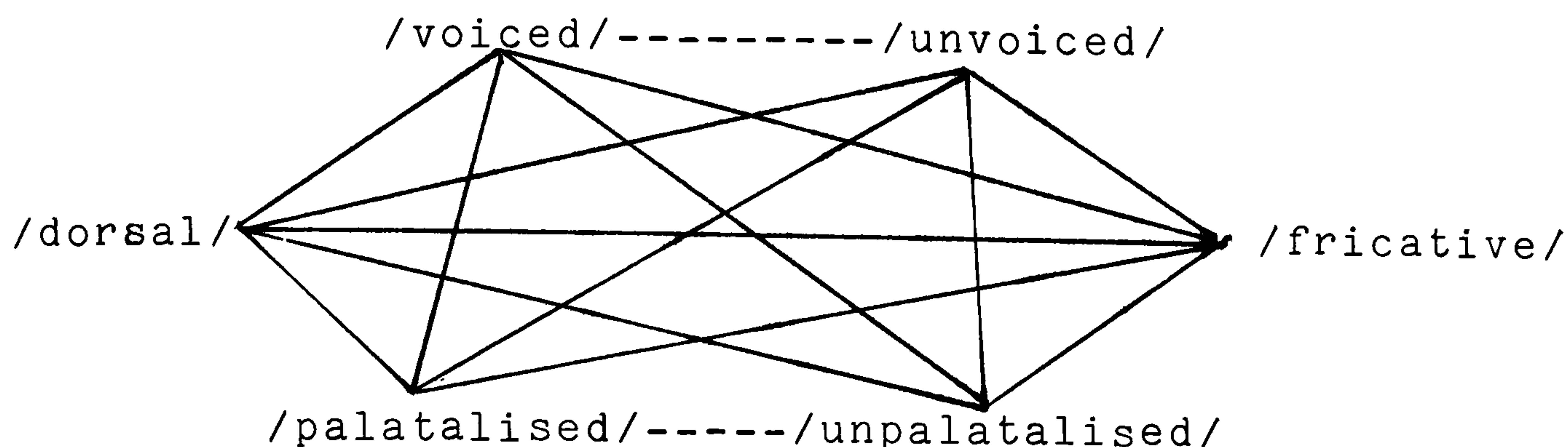


DIAGRAM 10

The "broken line convention" helps us to overcome one of the problems of the phoneme table, viz. the treatment of distinctive feature combinations in the case of hyperphonemes. It also helps us to see that the idea of a phoneme representing more features than it is functionally equivalent to arises because of a disparity between the identification of distinctive features on the basis of their oppositions and the combinatory possibilities of distinctive features.

Another case where the broken line may be useful is the case of the archiphoneme. An archiphoneme such as, /š̌/, in Russian possesses all the features of the phonemes involved in the suspension of opposition. /š̌/ in Russian possesses the features, /hushing/, /voiced/ and /unvoiced/. It is formed by the suspension of opposition, mainly in post-nuclear contexts, of /š̌/ and /ž̌/. Of course, the features, /voiced/ and /unvoiced/,





"S", i.e. those pairs of features for which  $- i S j$  is true. In setting up lattices we are, in effect, establishing the pairs of features which do contract the relation "S", i.e. the pairs for which  $i S j$  is true. We have seen that a number of contextual considerations may override the cases where we find  $- i S j$ ; they can be described as, perhaps,  $- - i S j$ . The well-formedness condition(s) determine the well-formed combinations of distinctive features in a given system.

In both cases (i.e. setting up categories and lattices), we are working principally from the function which establishes our new type of distinctive feature matrix. A Venn diagram representation of the distinctive features which contract "S", i.e. their organisation into sets, is possible but very clumsy, as the reader can quickly demonstrate for himself simply by drawing the sets of distinctive features for which the relation,  $S_{i,j}$ , holds.

\*\*\*\*\*

So far, we have been concerned solely with consonantal features. However, it is quite clear that there is no reason why we should not specify a new style distinctive feature matrix for vocalic features as well. The following table gives a conventional



(Martinet-style) distinctive feature analysis for the vowels of Russian.

	Front	Back
High	i	u
Mid	e	o
'a-ness'	a	

TABLE 11

Martinet-style distinctive feature matrix for Russian vowels.

The following table, 12, uses our function to specify a new style matrix on the basis of this information.

	high	mid	'a-ness'	front	back
high	0	0	0	1	1
mid	0	0	0	1	1
'a-ness'	0	0	0	0	0
front	1	1	0	0	0
back	1	1	0	0	0

TABLE 12

New style matrix for Russian vowels.

This is the lattice of combinatory possibilities involved,

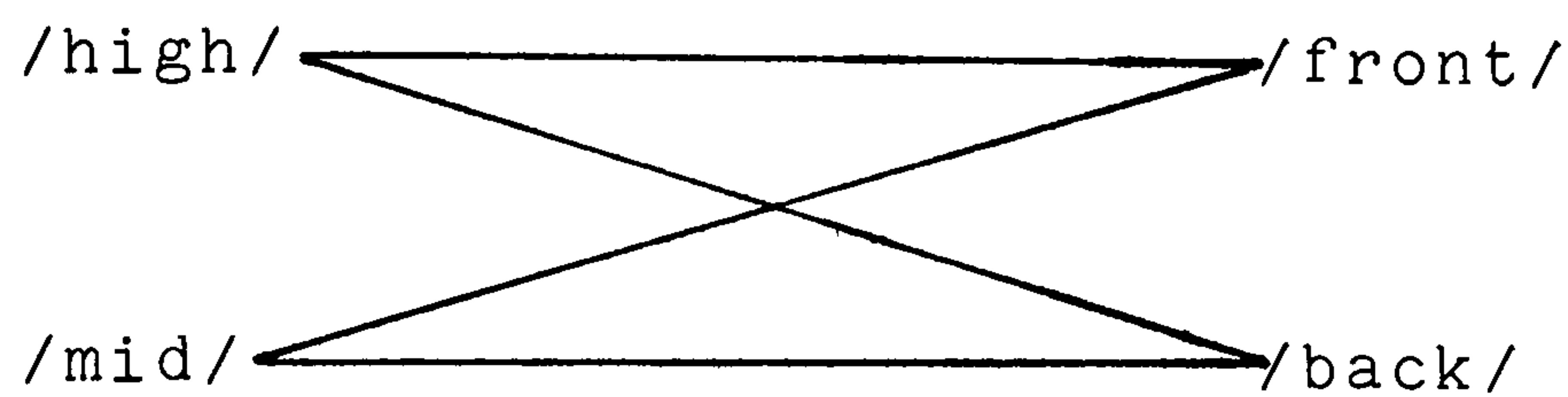


DIAGRAM 12

Lattice for Russian vocalic distinctive features. (Only the features, /high/, /mid/, /front/ and /back/, are considered since /a-ness/ has no combinatory possibilities.)

The matrices for Russian consonants and vowels can be combined to give the following overall matrix for the Russian distinctive feature system.



	labial	apical	hushing	dorsal	pal'sed	unpal'sed	stop	fricative	nasal	voiced	unvoiced	front	back	high	mid	'a-ness'
labial	0	0	0	0	1	1	1	1	1	1	1	0	0	0	0	0
apical	0	0	0	0	1	1	1	1	1	1	1	0	0	0	0	0
hushing	0	0	0	0	0	0	0	0	0	1	1	0	0	0	0	0
dorsal	0	0	0	0	1	1	1	1	0	1	1	0	0	0	0	0
pal'sed	1	1	0	1	0	0	1	1	1	1	1	0	0	0	0	0
unpal'sed	1	1	0	1	0	0	1	1	1	1	1	0	0	0	0	0
stop	1	1	0	1	1	1	0	0	0	1	1	0	0	0	0	0
fric've	1	1	0	1	1	1	0	0	0	1	1	0	0	0	0	0
nasal	1	1	0	0	1	1	0	0	0	0	0	0	0	0	0	0
voiced	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0
unvoiced	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0
front	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1	0
back	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1	0
high	0	0	0	0	0	0	0	0	0	0	0	1	1	0	0	0
mid	0	0	0	0	0	0	0	0	0	0	0	1	1	0	0	0
'a-ness'	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0

TABLE 13

Combined matrix for Russian consonants and vowels.

If we look at the lattice in diagram 12, then we see that there are no common terms with the previous consonantal lattices. Similarly, if we look at table 13, we see that the vowel features never combine with consonantal distinctive features. This is obvious on the matrix as two symmetrical blocks of '0's. If we look at the vowel features (with the exception of /a-ness/, which is the sole feature of the phoneme /a/), then we find they are intercombinable. The same is true of the consonantal distinctive features. Now, this lack of combinability between the two sets of features is clearly one of the reasons for speaking of two subsystems of distinctive features, "vocalic" and "consonantal", in a language. Of course, it is not necessarily the case that there is no combinability between the features of "vocalic" or "nuclear" phonemes and "consonantal" or "peripheral" phonemes. The possibilities of independent and connected subsystems of distinctive features present an interesting typological classification. In fact, we will normally find two major subsystems, where the features of each subsystem are not combinable and not interconnected in any way. That is, we expect to find a "vocalic" and a "consonantal" subsystem. It is interesting to note that in English and in Russian, however, there are minor classes of those features that do not combine with any other feature. In Russian, there is the feature, /a-ness/, and in English the features, /l-ness/ and /h-ness/, which



behave in this manner. In Russian, we should naturally associate /a-ness/ with the vocalic subsystem and /l-ness/ and /h-ness/ in English with the consonantal subsystem. /a-ness/ is phonetically [low] and is obviously part of a series with [high] and [mid] and is the sole feature of the nuclear phoneme /a/. In English, /l-ness/ and /h-ness/ are the sole features of the phonemes, /l/ and /h/, respectively, where, of course, /l/ and /h/ are consonantal or peripheral in their distribution. From the present point of view, however, this sort of classification is misleading,

to be continued

We should be quite clear that, from the point of view of the new type of matrix, /l-ness/ and /h-ness/ form a special subsystem of distinctive features of their own in English. That is, they are the set of features which are never joined by \_\_\_\_\_ to any other feature. This set is clearly different from the English consonantal subsystem containing /labial/, /apical/, /dorsal/, /hissing/, /hushing/, /occlusive/, /fricative/, /nasal/, /voiced/ and /unvoiced/. It is also different from the nuclear subsystem containing /neutral/, /spread/, /rounded/, /vocalic/ and /semi-vocalic/ (as in the matrix set up for English by Mulder and Hurren<sup>13</sup> - see the following table). We will call such a sub-system a "zero-combination" sub-system.

	spread	neutral	rounded
semi-vocalic	i	r	u
vocalic	e	a	o

TABLE 14

Martinet-style matrix for English vowels following Mulder and Hurren.<sup>13</sup>

In a similar way, the feature, /a-ness/, in Russian can be considered "vocalic" only if we look at it from the point of view of the Martinet style of matrix and distinctive feature analysis. From the present point of view, /a-ness/ forms a subsystem of its own. What we are saying is that the terms "vocalic" and "consonantal" may be misleading, as may the terms "nuclear" and "peripheral", in distinctive feature analysis (although not in phonotactics), since apparently consonantal features may not belong to the "consonantal" subsystem and apparently vocalic features may not belong to the "vocalic" subsystem, as in the cases of English /l-ness/ and /h-ness/ and Russian /a-ness/ respectively. In fact, we may have three or more distinctive feature subsystems. These subsystems emerge naturally from the new type of matrix.



There are some other points which are important in this respect. It is significant, for instance, that there exists the possibility that no absolute division between the "consonantal" and the "vocalic" subsystems appears in a given language, i.e. it is possible to have a single lattice and not two lattices - one overall system not two subsystems. Such a case arises when one or more features combine with both "consonantal" and "vocalic" features. Such a feature might be, for example, /nasal/ in Old Church Slavonic or French. If we take the phoneme tables for Old Church Slavonic to be as follows,

	stop		fricative		nasal
	voiced	unvoiced	voiced	unvoiced	
labial	b	p	v	f	m
apical	d	t	z	s	n
hushing			ž	š	
dorsal	g	k		x	

TABLE 15

Martinet style distinctive feature matrix for Old Church Slavonic consonants. (Assuming that /ts/, /tš/, /dz/, /dž/, /ri/, /li/ and /ni/ are phonemic complexes and ignoring marginal "foreign" (borrowed) phonemes).

	front	back
high	i	u
reduced	ɨ	ʉ
mid	e	o
nasal	ɛ̃	ɔ̃
low	ɛ	a

TABLE 16

Martinet style matrix for Old Church Slavonic vowels.

We are now in a position to set up the following new style matrix for Old Church Slavonic consonants and vowels combined.



	labial	apical	hushing	dorsal	stop	fricative	nasal	voiced	unvoiced	front	back	high	reduced	mid	low	l-ness	r-ness
labial	0	0	0	0	1	1	1	1	1	0	0	0	0	0	0	0	0
apical	0	0	0	0	1	1	1	1	1	0	0	0	0	0	0	0	0
hush'g	0	0	0	0	0	0	0	1	1	0	0	0	0	0	0	0	0
dorsal	0	0	0	0	1	1	0	1	1	0	0	0	0	0	0	0	0
stop	1	1	0	1	0	0	0	1	1	0	0	0	0	0	0	0	0
fric'e	1	1	0	1	0	0	0	1	1	0	0	0	0	0	0	0	0
nasal	1	1	0	0	0	0	0	0	0	1	1	0	0	0	0	0	0
voiced	1	1	1	1	1	1	0	0	0	0	0	0	0	0	0	0	0
unv'd	1	1	1	1	1	1	0	0	0	0	0	0	0	0	0	0	0
front	0	0	0	0	0	0	1	0	0	0	0	1	1	1	1	0	0
back	0	0	0	0	0	0	1	0	0	0	0	1	1	1	1	0	0
high	0	0	0	0	0	0	0	0	0	1	1	0	0	0	0	0	0
red'ed	0	0	0	0	0	0	0	0	0	1	1	0	0	0	0	0	0
mid	0	0	0	0	0	0	0	0	0	1	1	0	0	0	0	0	0
low	0	0	0	0	0	0	0	0	0	1	1	0	0	0	0	0	0
l-ness	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
r-ness	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0

TABLE 17

New style matrix for all Old Church Slavonic distinctive features (including the features /l-ness/ and /r-ness/ of the single feature phonemes /l/ and /r/ respectively).

We can now set up the following lattice for Old Church Slavonic in which the "consonantal" and "vocalic" features are connected. (We leave aside the minor "zero-combination" system of /l-ness/ and /r-ness/.)

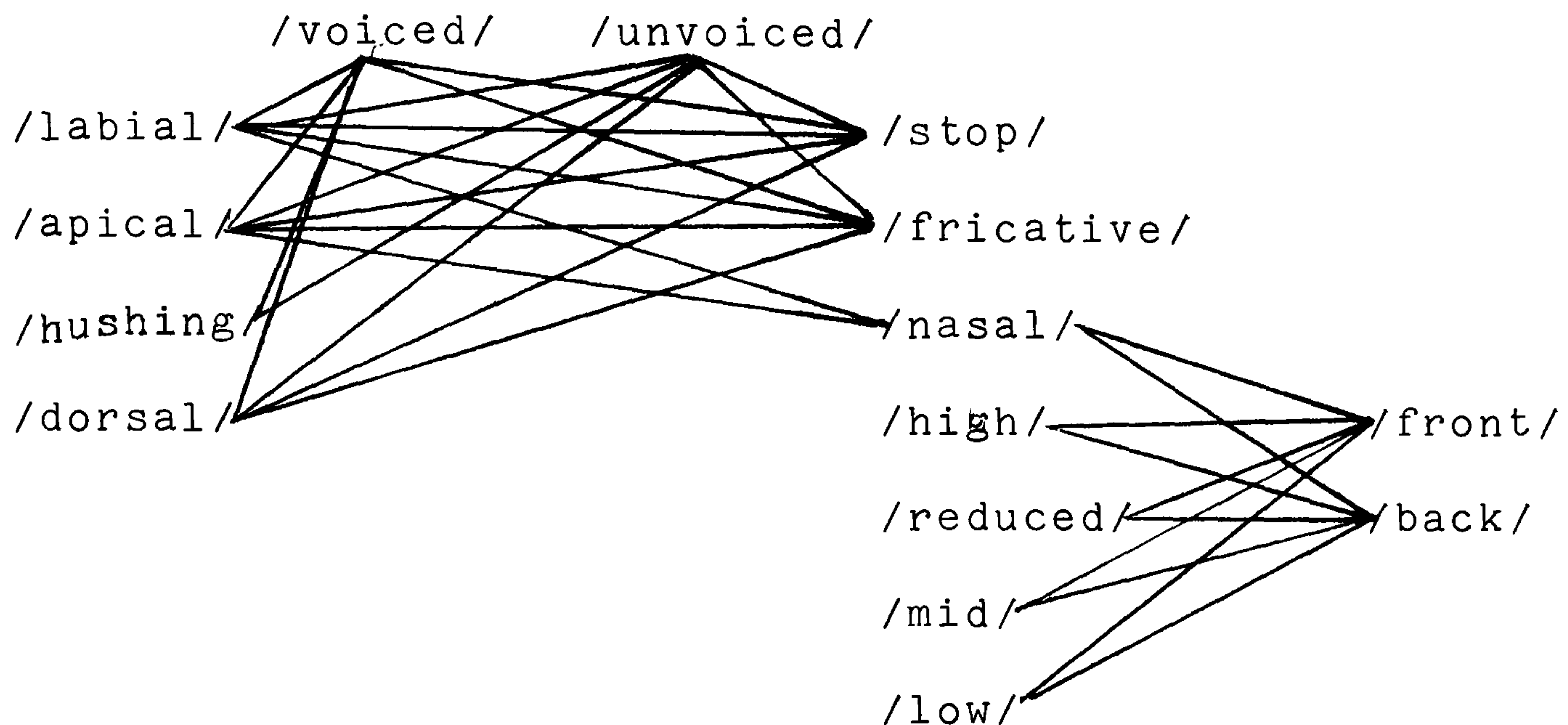


DIAGRAM 13

Distinctive feature lattice for Old Church Slavonic.

In Russian, but not in Old Church Slavonic, we can set up a clear distinction between two major subsystems of distinctive features on the phonological grounds of mutual exclusion and combinability. As we have said, the "consonantal - vocalic" distinction is not present in Old Church Slavonic.

In English, we find two major subsystems, consonantal and vocalic, and a minor "zero-combination"



subsystem. The vocalic features are not connected to the consonantal features or to /l-ness/ and /h-ness/ (of course). However, the vocalic phonemes, /i/, /r/ and /u/, are distributed in both nuclear and peripheral positions. The same is true of the phonemes, /i/ and /u/, in Russian and the phoneme /i/ in Old Church Slavonic. That is, there may be a disparity between the behaviour of the distinctive features and the behaviour of the phonemes containing those distinctive features.

To round off this discussion let us say that, although we have chosen to set up the new style of matrix via the familiar Martinet style of matrix, this is by no means necessary. We can go direct to the new style of matrix from an analysis of phonemes into their distinctive features. However, the new type of matrix is intended to supplement existing styles of matrix (which have other virtues) and not to replace them. The new type of matrix allows us to look at the structure of distinctive feature systems in a new and interesting way. The possibility of setting up Venn diagrams of mutually exclusive features and lattice diagrams of mutually combinable features provides a useful and novel view of distinctive features and helps us to overcome problems of multidimensional representation. The function specifying well-formed, maximum n-tuples of distinctive features, which grows directly out of the new matrix, allows us to distinguish

impossible feature combinations as well as possible, but unrealised, combinations quite clearly and to define them in phonological terms. The establishment of phonologically determined subsystems of distinctive features also follows from this function and allows a new view of the behaviour of distinctive features. By applying the convention on broken lines, we can overcome any disparity between the economy of the distinctive feature system and the actual stock of phonemes (as with the hyperphoneme problem) or concerning contextual considerations in distinctive feature combinations (as in the case of archiphonemes).

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Let us now try to look at these ideas in a more formal manner. We have used a number of ideas from the existing theory of distinctive features. Notably, we have used the notions, phoneme and distinctive feature, and the relation, unordered constructional relation in phonology. We have attempted to analyse the premises that, all phonemes are maximum n-tuples of distinctive features and that all pairs of distinctive features in a phoneme contract the unordered constructional relation. We have added the notions, category, C, and subsystem of distinctive features,  $\Sigma$ .

The unordered constructional relation in phonology, S, has been interpreted and defined, as follows,



def. 1.      $S = \{ \langle x, y \rangle : \langle x, y \rangle = / \alpha / \text{ or } \langle \dots x, y, \dots \rangle = / \alpha / \}$

where  $/\alpha/$  is a phoneme and  $x$  and  $y$  are distinctive features. That is, the relation,  $S$ , is extensionally the set of all pairs of distinctive features which combine either in such a way as to be equivalent to a phoneme are members of a larger  $n$ -tuple of distinctive features which is equivalent to a phoneme. For example, the features,  $/\text{hushing}/$  and  $/\text{voiced}/$ , combine in English to form the pair  $/\text{hushing, voiced}/$  which is equivalent to the phoneme,  $/z/$ , whereas  $/\text{labial}/$  and  $/\text{voiced}/$  combine only in larger constructions, such as  $/\text{labial, voiced, occlusive}/$ , equivalent to  $/b/$ .

A (distribution) category,  $C$ , can now be defined for distinctive features, as follows,

def. 2.      $C = \{ x, y, z \dots : - (E \langle x, y \rangle) (x S y) \}$

That is, a category is a set of distinctive features, such that no pair of features in the set contract the relation,  $S$ . In English, there are four consonantal categories, as follows ;

$C^1 = \text{labial, apical, dorsal, hissing, hushing, l-ness, h-ness}$

$C^2 = \text{occlusive, fricative, nasal, hissing, hushing, l-ness, h-ness}$

$C^3 = \text{nasal, voiced, unvoiced, l-ness, h-ness}$

$C^4$  = hissing, hushing, l-ness, h-ness, fricative,  
dorsal

and two nuclear categories

$C^5$  = spread, neutral, rounded

$C^6$  = vocalic, semi-vocalic.

A subsystem of distinctive features,  $\Sigma$ , is defined as follows,

def. 3a.  $\Sigma = \{x : (\exists y) (x S y)\}$

That is, a subsystem of distinctive features is a set of features, each of which contracts the relation, S, with at least one other feature in the set. A zero-combination subsystem will be almost the opposite of this, i.e. a set of features, none of which contracts the relation, S, with any other feature in the language. That is, a zero-combination subsystem,  $\Sigma^0$ , is defined ;

def. 3b.  $\Sigma^0 = \{x : - (\exists y) (x S y)\}$

In English there are three subsystems,

$\Sigma^1$  = labial, apical, hushing, hissing, dorsal, voiced,  
unvoiced, nasal, occlusive, fricative

$\Sigma^2$  = spread, rounded, neutral, vocalic, semi-vocalic

$\Sigma^3$  = l-ness, h-ness

$\Sigma^3$  is the zero-combination subsystem.

Finally, we can analyse the two premises stated



above. An n-tuple of distinctive features is well-formed, W, if it is maximum and all pairs in it contract "S" and there is no phoneme which is not equivalent to such a well-formed n-tuple (where  $n \geq 1$ ).

def. 4.  $W = \{ \langle x, y, \dots, z \rangle : x S y \ \& \ x S z \ \& \dots$   
 $\ \& \ y S z \ \& \ (- (Ei) i S x \ \& \ i S y \ \& \dots$   
 $\ \& \ i S z) \}$ .

This definition determines which n-tuples of distinctive features are maximum and well-formed when  $n \geq 2$ . Clearly, if a feature belongs to the set of zero-combination features, then it is a well-formed n-tuple, where  $n = 1$ . Now, we require a further definition which states that there is no phoneme which is not equivalent to a well-formed n-tuple of distinctive features, where  $n \geq 1$ .

def. 5.  $- (E\alpha). \alpha \neq W_{\langle x, y, \dots, z \rangle}$  (where, as before  
 $\alpha$  is a phoneme).

Given the specification of all the pairs that contract S and the information that, for example,  $\{ /labial/ S /occlusive/ \ \& \ /labial/ S /voiced/ \ \& \ /occlusive/ S /voiced/ \}$  is well-formed, then the n-tuple can be mapped into the set of phonemes. Since the triple,  $\langle labial, occlusive, voiced \rangle$ , satisfies def. 4., we can write  $W_{\langle labial, occlusive, voiced \rangle}$ . That is, all the features in the triple contract the relation, S, and there is no quadruple or greater combination

including these features and satisfying def. 4..  
 Finally, def. 5. is also satisfied because there is a corresponding phoneme, /b/, in English for this triple (equivalent to the triple). Given the above conventions on lattices (and broken lines, where necessary), one can say simply that a complete lattice of distinctive features is a well-formed maximum n-tuple of distinctive features.

What we have described for English using the above definitions is a model of the distinctive feature structure of English. The model contains the distinctive features of English, nine unary relations,<sup>14</sup> which classify the distinctive features (viz. the six categories,  $C^1, \dots, C^6$ , and the three subsystems,  $\Sigma^1, \Sigma^2$  and  $\Sigma^3$ ), the binary relation, S, the n-ary well-formedness relation, W, and the equivalence relation, =, which is found in all models. That is, we have a model or structure for English distinctive features in the sense of "structure" defined in previous chapters; i.e.

$\langle \{ \text{labial, apical, hissing, hushing, dorsal, voiced, unvoiced, nasal, fricative, occlusive, l-ness, h-ness, vocalic, semi-vocalic, spread, neutral, rounded} \}; C^1, C^2, C^3, C^4, C^5, C^6, \Sigma^1, \Sigma^2, \Sigma^3, S, W, = \rangle$

In accordance with our view of linguistic description stated above, we can now give the following calculus for the model. The calculus has the following



premises ;

1. If a distinctive feature belongs to any of the categories,  $C^1, \dots, C^4$ , then it is a member of subsystem,  $\Sigma^1$  or  $\Sigma^3$ .
2. If a feature belongs to either  $C^5$  or  $C^6$ , then it is a member of  $\Sigma^2$ .
3. If a pair of features belonging to  $\Sigma^2$  also belongs to different categories, then that pair is a well-formed maximum combination (they contract S) and there is a corresponding phoneme to that pair.
4. If a feature belongs to the zero-combination subsystem,  $\Sigma^3$ , then it is a well-formed n-tuple, where  $n = 1$ , and a phoneme corresponds to it.
5. An n-tuple of features from  $\Sigma^1$  is well-formed and maximum, if and only if none of the features are members of the same category and the combination corresponds to a maximum lattice (all pairs contract S and the n-tuple is maximum). There is then a corresponding phoneme.

These rules apply only to English, of course, and specify the interpretation of the relations in the above model and ultimately the well-formed n-tuples of distinctive features in English. Clearly, the terms, category, distinctive feature, subsystem, the relation "S", and the notion "well-formedness" must be interpreted in the light of the existing theory and of the new

theoretical apparatus given above. Trivially, of course, a more formalised, or rather, more formularised account of these rules could be given.

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In earlier chapters, we defined a linguistic theory as a triple,

$$\underline{T} = \langle \underline{K} \ \& \ V, \underline{M}, \underline{O} \rangle$$

and linguistic description as a pair,

$$\underline{D} = \langle \underline{C}, \underline{S} \rangle.$$

We can see that the new apparatus given here is a calculus, K, in which a number of new theoretical, propositional functions are introduced and inter-related. They are also related in obvious ways to existing phonological theory (distinctive feature theory), since they operate on notions from that theory, subject them to analysis and are consistent with existing theory. The new apparatus supplements and, to some extent, develops distinctive feature theory. In particular, the apparatus helps us to relate the unordered constructional relation to the notion of maximum, well-formed n-tuple in phonematics. Furthermore, each of the new terms is defined verbally and formally. That



is, we have a system of definitions, V. The definitions lead on to clear methodological tests, M, notably consistency with lattices and the analysis of phonemes into their distinctive features. The ontology, O, of the apparatus is quite clear, since it sets up operations over, and sets of, distinctive features, which are objects of known ontological status. The main requirement of the ontology, that all statements in a description be capable of empirical test through an observable connection with first-order objects, is thus satisfied.

Any proposed linguistic theory, or part theory, must, as we have said, be applicable in the description of speech phenomena. Descriptions based on the proposed theory, or part theory, must be subject to empirical test and, so, to empirical refutation. A linguistic theory or addition to a linguistic theory (part theory) which is applicable in this way is deemed to be indirectly scientific in the sense given above. We have interpreted the notion "description" to mean the combination of a calculus and a structure (or model). A description, considered as a system of empirical hypotheses, is a function of the calculus and the structure. It follows that, in order to be indirectly scientific, a new theory or part theory must be applicable in the construction of descriptions of the required sort. One can see that our new apparatus meets this condition and that it is thus indirectly scientific.

Since our new apparatus meets both the formal requirements imposed on linguistic theories and part theories and it is also indirectly scientific, we can conclude that the new theoretical apparatus appears to be acceptable. We do not claim, of course, that it is correct. It may, for instance, be shown, at some point in the future, that our new apparatus is untenable. What is important is that the formulation of new theoretical apparatus be sufficiently rigorous and transparent for it to be inspected for acceptability and that the considerations involved in inspecting a theory, or part theory, for its acceptability be themselves clear and defensible.



NOTES TO CHAPTER 8

1. R. Jakobson, "On the Identification of Phonemic Identities", pp. 208 - 9.
2. A. Martinet, Elements..., p. 84.
3. J. Mulder, Sets and Relations, p. 107.
4. J. Mulder, "Phoneme Tables and the Functional Principle", pp. 6 - 11.
5. A.A. Hill, Introduction to Linguistic Structures, p. 205.
6. J. Mulder, "Phoneme Tables...", pp. 6 - 11.
7. J. Mulder, "Phoneme Tables...", p. 13.
8. J. Mulder, "Phoneme Tables...", pp. 14 - 16.
9. A similar mechanism is used for a quite different purpose by Mulder in Sets and Relations, i.e. the establishment of opposition classes, pp. 130 -135.
10. For the notion "semi-cluster", see J. Mulder, Sets and Relations, pp. 199 - 201.
11. J. Mulder, "Phoneme Tables...", pp. 14 - 17.
12. J. Mulder, "Phoneme Tables...", pp. 12 - 13.
13. J. Mulder and H. Hurren, "The English Vowel Phonemes...", p. 54.
14. For the notion "one-term" or "unary" relation, see, for example, C. Kilmister, Language, Logic and Mathematics, pp. 38 - 9.

CHAPTER NINE

THE NEED FOR METHODOLOGY



We have argued that it is necessary to incorporate a methodology into linguistic theory and that the methodology should be logically related to the theoretical models, relations and statements of the calculus and semantic components in such a way that proposed descriptive models, relations and statements relying on these theoretical "genotypes" may be definitively tested. We have interpreted this to mean that the methodology should supply appropriate tests giving clear results, which lead us either to uphold the descriptive model tested or the relation or statement tested or to reject it. Thus, if we choose to incorporate the notion, "phoneme", into the theory, we must be able to define the term, e.g. as a simultaneous bundle of distinctive features in phonology and a minimum unit in phonotactics and maximum unit in phonematics. Clearly, there must be tests of maximum size in phonematics (e.g. the well-formedness condition - above) and of simultaneity (e.g. the permutation test). These tests in combination may be applied whenever a phoneme-model is proposed for a given language. By applying the tests to the appropriate range of data, we can judge the validity of the model. We have argued that this is possible and give an example in Chapter 11.

From a meta-theoretical point of view, we regard a methodology of this sort as necessary for all theoretical models, relations and statements. Not all linguists

have been in agreement.

a. The feasibility and necessity of a methodology in linguistics

The main argument against the feasibility and necessity of a methodology in linguistics has been advanced by Chomsky in Syntactic Structures. Chomsky argues that it is,

"unreasonable to demand of linguistic theory that it provide anything more than a practical evaluation procedure for grammars."<sup>1</sup>

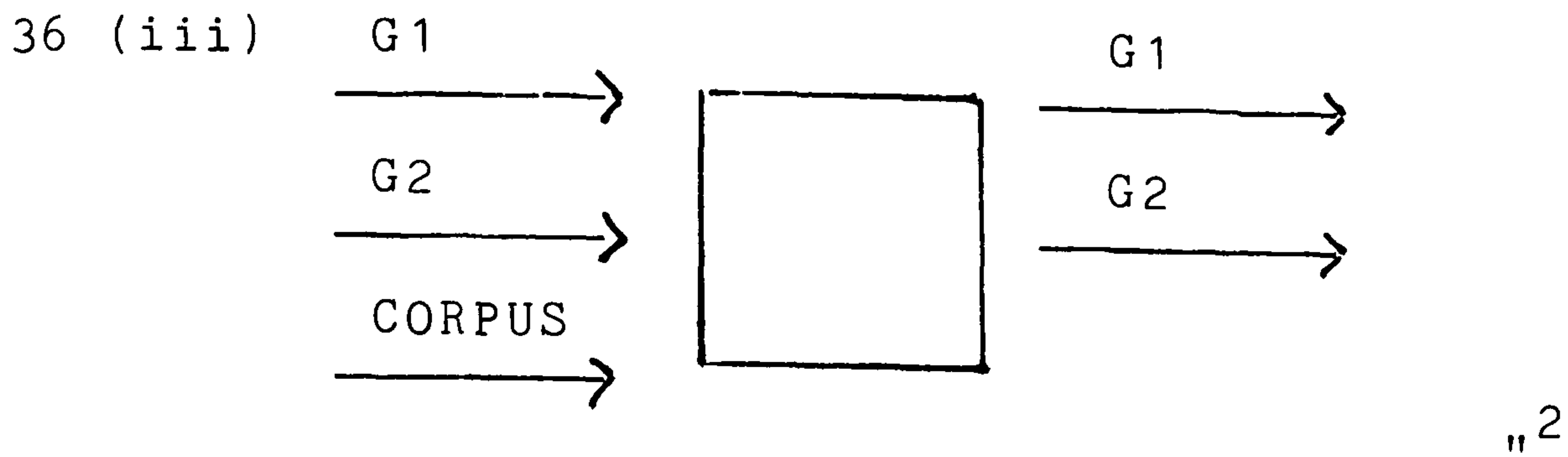
In doing so, he suggests that it is neither feasible nor necessary for linguistic theory to develop a rigorous methodology. As will emerge (below), this position is both self-contradictory and confused.

In this connection, Chomsky begins by considering the question, "what is the relation between the general theory and the particular grammars that follow from it ?"<sup>2</sup> He finds three answers to this question distinguishing between "discovery", "decision" and "evaluation" procedures. He says ;

"The strongest requirement that could be placed on the relation between a theory of linguistic structure and particular grammars is that the theory must provide a practical and mechanical method for actually construc-







Chomsky comments ;

"36 (i) represents a theory conceived as a machine with a corpus as its input and a grammar as its output ; hence, a theory that provides a discovery procedure. 36 (ii) is a device with a grammar and a corpus as its inputs and the answers "yes" or "no" as its outputs, as the grammar is, or is not, the correct one ; hence, it represents a theory that provides a decision procedure for grammars. 36 (iii) represents a theory with grammars G1 and G2 and a corpus as its inputs and the more preferable (sic !) of G1 and G2 as its output ; hence, a theory that provides an evaluation procedure for grammars".<sup>3</sup>

From these remarks it will be self-evident that, when Chomsky rejects both discovery and decision procedures as unnecessary and not feasible, he rules out of linguistics the kind of methodology that would be capable of either corroborating or refuting the adequacy of linguistic descriptions. The "methodology" for which he opts merely determines the better, or best,



of proposed grammars. At no point in such an evaluation procedure could there be any question of determining the validity or non-validity of a given grammar with respect to the behaviour of empirical phenomena. Chomsky's suggestion is, therefore, simply inadequate, since it fails the meta-theoretical condition that a methodology be capable of testing the empirical adequacy of descriptions with respect to the phenomena which those descriptions purport to describe. That is, according to Chomsky, there can be no methodology which can determine the empirical truth or falsehood of any descriptive statement. This would be a strange type of science.

Chomsky is no doubt correct, however, when he rejects "discovery procedures", although it is not certain that he rejects them for the right reason. If Chomsky means by "a practical and mechanical method for actually constructing a grammar" the kind of inductive cycle adopted by Zellig Harris and others, then it is unnecessary to bring the charge that such a methodology is unfeasible. Equally, the feasibility of any approach which demands that a descriptive hypothesis be justified by some mechanical discovery procedure is beside the point. Whether it is actually possible to devise such procedures or not, the reason for their rejection is simply that "discovery procedures" are neither necessary nor sufficient for either the launching or the testing of a descriptive hypothesis and they

bear no relation to the question of the validity or otherwise of a given hypothesis. At best such discovery procedures may give a certain "motivation" (rather than "justification") for the launching of a hypothesis.<sup>4</sup>

We may say, then, that discovery procedures are irrelevant to, and evaluation procedures inadequate for, the testing of the empirical claim of linguistic descriptions (grammars). The remaining "decision procedures", as adumbrated by Chomsky, are similar to the kind of methodology which would satisfy the meta-theoretical conditions of empiricism which we have set up in that the ultimate test of the validity or otherwise of a given empirical statement (or set of statements) in a given description lies in the confrontation of that statement (or those statements) with empirical phenomena (the "corpus"). The descriptive statement(s) will be either refuted or, if not refuted, upheld (a simple "yes/no" answer given). Two curious points emerge in this connection. The first is that at no point in his discussion does Chomsky demonstrate, or attempt to demonstrate, the "unfeasibility" of decision procedures, although he criticises discovery procedures at length in this connection. Decision procedures are simply asserted to be not feasible and an unreasonable requirement. Perhaps, Chomsky feels that they are tarred with the same brush as discovery procedures. Again, however, an attack on decision procedures concerning



their feasibility, were it forthcoming, would not affect their necessity.

The second curious point here is that Chomsky asserts the following in the course of the same argument,

"Clearly, every grammar will have to meet certain external conditions of adequacy ; e.g. the sentence generated will have to be acceptable to the native speaker....In addition, we pose a condition of generality on grammars ; we require that the grammar of a given language be constructed in accordance with a specific theory of linguistic structure in which such terms as "phoneme" and "phrase" are defined independently of any particular language."<sup>5</sup>

It will be fairly obvious that "external conditions of adequacy" of the sort Chomsky has in mind involve testing the validity or non-validity of descriptive statements with respect to certain empirical judgements. In the case cited by Chomsky one test of the empirical validity of a grammar is whether or not the set of sentences generated by that grammar are acceptable to the native speaker.<sup>6</sup> Such a test will involve answers of a "yes/no" sort (leaving uncertainty aside ; the problem of uncertainty merely shows the inapplicability of the test). External conditions of adequacy require, then,

some sort of decision procedure. It is therefore self-contradictory for Chomsky to assert either that decision procedures are unreasonable demands (since he himself requires them) or that they are unnecessary (since he himself argues de facto for them) or that they are not feasible (since he himself gives an example of one). If we take Chomsky's "condition of generality" also into account, then one can envisage that the actual methodology (as part of theory as opposed to meta-theory) will be incorporated, or can be incorporated, into the general theory (as we have argued). Thus, it would be necessary to replace evaluation procedures with decision procedures in transformational theory or to drop the pretence that evaluation procedures are anything other than disguised decision procedures.

From another point of view, we may say that it is simply absurd to attempt to evaluate grammars that do not meet the conditions, suggested by Chomsky himself, of external adequacy and generality (which are not dissimilar to the kind of conditions laid down by Hjelmslev and many others). It would seem reasonable to maintain that the evaluation of two grammars presupposes, at least, that both the grammars in question are both formally and empirically adequate. (Other conditions would be that they cover the same "range" of describienda and that they have the same scope (are constructed under the same theory and utilise the same parts of that theory - otherwise they are not in competition (if made using different theories, they are not theoretically comparable)



or there must be a relation of total inclusion between the descriptions - in which case the description with wider scope is preferable). Evaluation loses its point, unless one is evaluating equally (empirically) adequate descriptions ; it would remain possible that one was evaluating two equally false grammars without decision procedures. If evaluation is to have any point, then, it must presuppose decision procedures, just as considerations of simplicity presuppose conditions of empiricism (of the decision procedure type), as was noted by Hjelmslev long ago. A method which ignores decision procedures contains no methodology for the testing of empirical hypotheses ; it follows, therefore, that no empirical hypothesis associated with any description made under that theory refers directly or indirectly to any refutation, or verification, class and this renders such descriptions a-scientific from the point of view of any attitude in the philosophy of science.

We may add that the view of a linguistic theory as an evaluation procedure is presented in a deliberately naive way. As we have indicated elsewhere, there are many connections between a theory and the grammar of a given language (constructed in accordance with that theory). This is the implication, incidentally, of Chomsky's "condition of generality". As we have also pointed out earlier, a theory as an evaluation procedure can not be impartial. It will select grammars which are constructed in accordance with the theory or are

in close harmony with the theory. The theory as an evaluation procedure merely turns out to be a way of preferring one sort of grammar over others.

The attitude that linguistics must do without decision procedures (hence, without a sufficient methodology) implies that linguistics must be a-scientific in the sense given to this word by Popper or in the modified sense we have given to it. Having disposed of the the arguments against the feasibility and necessity of decision procedures, we can now turn to an independent proof of their necessity.

Popper has pointed out the question of the need for a methodology is closely connected with the criterion of demarcation. As Popper says,

"The theory of method, insofar as it goes beyond the purely logical analysis of the relations between scientific statements, is concerned with the choice of methods - with the way in which scientific statements are to be dealt with."<sup>7</sup>

In general, Popper argues, the methods chosen should be such as to allow empirical statements to be revised, i.e. it should be possible to reject given statements in favour of new ones in the light of new observations which refute the old statements. In this way one relates



methodology to the criterion of demarcation between science and non-science, since any methodology which allowed the revision of descriptive statements in the light of further empirical information would include the notion of unilateral testability, i.e. falsifiability. That is to say that descriptions subject to such a methodology would be capable of rejection on the grounds of the empirical refutation of the hypotheses they contain. Since empirical testability can only be unilateral (according to Popper), it follows that such a methodology must include the notion of "falsifiability". A scientific theory in Popper's sense or in our sense is distinguished not so much by its formal properties as by its methods, since, as Popper says,

"If...we characterize empirical science merely by the formal or logical structure of its statements, we shall not be able to exclude from it that prevalent form of meta-physics which results from elevating an obsolete scientific theory into an incontrovertible truth."<sup>8</sup>

Thus, from our point of view, a linguistic description is distinguished as scientific by its subjection to the condition of empirical adequacy and a linguistic theory is (indirectly) scientific in virtue of containing a methodology capable of testing the empirical validity of proposed descriptions constructed using that theory. It is for this reason that an empirical methodology

(decision procedure) is indispensable to linguistics, as to any discipline which claims empirical content for any of its statements, i.e. without some form of methodology it is impossible to relate the supposed empirical statements of a science to the phenomena which those statements seek to describe and, hence, any body of statements lacking this requirement can be put forward (vacuously) as scientifically true in virtue of their factual incontrovertibility (impossibility of confrontation with potential falsifiers). One may conclude that there can be no scientific linguistics without some form of empirical methodology involving a decision procedure.

It might be felt by some that this conclusion has been reached rather "from above" by an appeal to the philosophy of science. This would be so, were it not for the fact that the same conclusion can be reached from a purely linguistic point of view (working from first principles). The arguments reinforce one another. The linguistic argument is as follows.

Let us assume (as above) that the "theoretical problem" of linguistics is to set up the both necessary and sufficient conditions for communication ("semiotic theory") and the "descriptive problem" is to set up descriptions of selected fields of communicational phenomena through the application of the theory to the phenomena, then we may say that the "meta-theoretical problem" is to set up the conditions for the acceptability



of both particular theories and particular descriptions. Now, it is required (among other things) that a theory be adequate or sufficient and this may be interpreted to mean that a theory must meet the condition that it lead to descriptions which are empirically adequate. It is the aim of the theory to lead to empirical knowledge of given fields of phenomena (descriptions), and no description is acceptable, unless it is descriptively adequate. Thus,

"A necessary condition of the acceptability of theories is that the descriptions which they lead to, through the application of the theory to the phenomena, be empirically adequate."

In order that a description may be considered empirically adequate, it must meet conditions of a formal, a theoretical and an empirical nature. As we have seen above, when we require that a description be formally justified, we state that any description should be logically consistent and sufficient for the derivation of all necessary descriptive statements. This requirement excludes vacuity through self-contradiction or eclecticism or ad-hocness. Thus,

"A necessary condition of the adequacy of a given description is that it be formally justified".

The theoretical condition may be expressed, as follows,

"A necessary condition of the adequacy of descriptions is that they be theoretically justified."

That is, each statement must be consistent with some part of the general theory. We can refine the requirement, as follows,

"A necessary condition of the adequacy of a given description, as an application of a given theory to a select field of phenomena, is that the description in question be justified by that theory."

This refinement restricts a description to the application of one theory to the phenomena and thus helps to avoid eclecticism. Finally, we require that any description be empirically adequate ; that is, following Popper, any description, which, after appropriate tests, is not demonstrated to be empirically false, will be provisionally upheld. Thus,

"A necessary condition of the adequacy of a description is that it is not empirically false."

All of these conditions do not add up to a sufficient condition of adequacy. The adequacy of a description is always open to doubt. Clearly, a methodology will be necessary for all these conditions. Except for



logical tests, which have their own methodology, such a linguistic methodology must come from the theory. This methodology must decide the consistency of a description with respect to the theory and the sufficiency of the description as an application of the theory to a given set of phenomena. It must decide the empirical validity of descriptions as applications of the theory. The methodology must be applicable to the description as a whole and to each point of the description. We conclude, then,

"An appropriate methodology is a necessary condition of a linguistic theory"<sup>9</sup>.

NOTES TO CHAPTER 9

1. N. Chomsky, Syntactic Structures, p. 52.
2. N. Chomsky, ibid., pp. 50 - 51.
3. N. Chomsky, ibid., p. 52.
4. For the distinction between "motivation" and "justification", see J. Mulder, "Linguistic Theory,..", p. 89.
5. N. Chomsky, Syntactic Structures, pp. 49 - 50.
6. This is a "reductive" condition (see below). It is necessary but not sufficient for the validity of the grammar.
7. K. R. Popper, The Logic of Scientific Discovery, p.49.
8. K. R. Popper, ibid., p. 50.
9. See J. Mulder, "Linguistic Theory,..." for a similar position.



CHAPTER TEN

REDUCTION AND DEDUCTION

IN EMPIRICAL LINGUISTICS

It will be obvious from the comments in the previous chapter that the objects and relations that are set up in a linguistic description are to be the product of the application of the methodology of the theory to independent phenomena and that this product is to be arrived at by way of a deductive argument. The satisfaction of the methodology by a given set of phenomena should imply or be equivalent to the satisfaction of the theoretical predicate or relation associated by equivalence or implication with that methodology. It should also be clear that any description will contain a consequence class or set of predictions about the data. Where objects or relations are predicted by the description, it is important that the objects and relations can be independently established by the application of the methodology to the relevant data. We will give an example of this form of testing in the following chapter.

Although we advocate a deductive approach to linguistics, it does not at all follow that all deductive approaches are equally acceptable. In this chapter we will look at some of the unacceptable aspects of empirical testing in deductive approaches to linguistics. We must distinguish first of all between "reductive testing" and "deductive testing". In the case of linguistic descriptions which can be tested only through the application of empirical tests to their consequence classes, we will speak of "reductive testing". Where the objects and relations of a linguistic description are the logical



consequences of applying the methodology to the phenomena, we speak of "deductive testing". In this latter case, objects and relations may or may not have been previously hypothesised. It does not really matter, since, if they have been hypothesised and the product of applying the methodology is equivalent to the hypothesised objects and relations, then the hypothesis stands and, if there is no prior hypothesis, the application of the methodology provides one. Clearly, if a previously hypothesised object or relation is not equivalent to the product of applying the methodology, the hypothesis falls in any case. In the case of reductive testing, however, typically the objects and relations of the description are not open to direct empirical testing. They are confirmed, or corroborated, if the consequence class withstands empirical testing and at least one part of the description is deemed to be false, if the consequence class does not survive empirical testing.

Both of these methods have been employed in deductive approaches to linguistics and reductive testing is very widespread. While deductive testing, in its naive form, has a number of practical difficulties (discussed below), reductive testing has a number of principle (and has been misunderstood). The practical problems of deductive testing can be overcome by a more carefully integrated approach but we take the position

here that reductuctive testing can be no more than a part of empirical testing in linguistics and that it must always be subordinate to deductive testing.

a. Reductive and deductive methodology

In the simplest outline, the "reductive" and "deductive" modes of inference have been described by Bochenski in the following manner ;

"In deduction, we infer the consequent from a conditional statement and its antecedent :

if A, then B,  
A,  
therefore, B.

In reduction, on the other hand, we infer the antecedent from a conditional statement and its consequent :

if A, then B,  
B,  
therefore, A."<sup>1</sup>

Although reduction is the dominant form of empirical testing in contemporary linguistics (being advocated more or less clearly by, among others, Shaumjan, Katz, Lockwood and Sampson - see below), it is nevertheless



probable that reduction is the **less familiar** of the two modes of argument. Deductive testing may be regarded as the utilisation of the "classical" (standard) logical mode of inference (modus ponens) in methodology and, as such, it is very familiar.

Clearly, if in a linguistic description there is an implication that, if the methodological conditions (A) are satisfied by such and such phenomena, then (B) those phenomena satisfy a certain theoretical predicate (or A and B are equivalent) and we find that A is the case, then we can deduce B and we will have an instance of deductive testing. For example, the "functional principle" tells us that all linguistic objects must have separate identity and **the commutation test** is part of the methodology which tells us that, if two items commute, then they have separate identity. The objects [p<sup>h</sup>] and [b] commute in English, so they must have separate identity.

If, however, there is an implication that, if the structural analyses and distributional statements of a description (A) are such as to imply (B) that an item, x, belongs to the consequence class of the grammar (is generated by it) and x genuinely corresponds to observed objects in specified way(s) (so B is asserted to be true), then A is taken to be true (A is confirmed), then we have a fairly familiar reductive argument. Bochenski demonstrates two types of reduction, the one "progressive"

and the other "regressive". He explains,

"In both cases the consequent is known to be true, but not the antecedent ; if the reduction is to be done progressively, however, the antecedent - whose truth-value is still unknown - is taken as the starting point, from which the argument proceeds to the known or ascertainable consequent. This progressive reduction is called "verification". Regressive reduction, on the other hand, begins with the known consequent and proceeds to the unknown antecedent. Regressive reduction is called "explanation"."<sup>2</sup>

It is clear that all forms of reduction contain some initial regressive reduction in order to establish an antecedent which may be tested progressively or regressively.

When the progressive type of reduction is called "verification", this is rather misleading. The point is that no absolute confirmation of the validity of the antecedent can be obtained through the testing of its consequents. The reason for this is that a reductive inference is one which is not logically valid. That is to say that the validity of the consequent is a necessary but not sufficient condition of the validity of the antecedent. Consequently, those linguistic theories which involve the requirement that descriptions construc-



ted in accordance with them be tested only reductively can never in principle lead to ultimately verifiable descriptions. On the other hand, if we set up the testing of descriptions in such a way that the satisfaction of the methodological conditions by the predictions of the description (its consequents) is a necessary condition of the validity of the description, it is obvious that, in the absence of any direct testing of the antecedent description, it is impossible (logically speaking) to draw any conclusion about the validity or non-validity of the antecedent. We cannot even say (strictly speaking) that the description is not invalid, since the satisfaction of methodological conditions by the predictions ("derived hypotheses") of a system is insufficient for this purpose. Such confirmation as can be given through the reductive method is of a purely "operational" character. The method can be useful if the system devised to describe a given set of, say, speech phenomena is operationally adequate in every case. Reductive testing may lead to the conclusion that a system (e.g. a transformational-generative grammar) is operationally adequate.

On the other hand, however, as Bochenski points out ;

"Falsification is logically valid, but confirmation .....is never conclusive in the reductive method . In this case.....the inference from consequent to antecedent

does not hold logically ; whereas the inference from the negation of the consequent to the negation of the antecedent is based on a law of logic and is universally valid."<sup>3</sup>

That is, to use the formula given by Popper,

$$((t \rightarrow p) \cdot \bar{p}) \rightarrow \bar{t}.$$

(Read ; "if p is derivable from t and if p is false, then t is also false"<sup>4</sup>.) As Bochenski points out, the reductive method is usually not set up in such a way that a consequent is derived from a single antecedent statement, "but from a conjunction of this statement with others (perhaps some theory or the like), say "T"<sup>5</sup>. The schema is then,

if (A and T), then B,  
not B,  
therefore, not (A & T).

That is to say, going along with Popper<sup>6</sup>, the whole system required for the deduction of B is falsified, if B is false. Bochenski argues that the falsehood of B in such a case leaves "a choice between the rejection of A and the rejection of T"<sup>7</sup>. This may be true but surely involves a misunderstanding on Bochenski's part. The



reason for this is that B is derived from the conjunction of A and T and not either A or T separately. Consequently, it is the conjunction, (A & T), which is the antecedent of B. When B is false, (A & T) is false. The import of this point is that a single false prediction of a system is sufficient to show that the system is false. As far as linguistics is concerned, a single validly generated non-entity of L would be sufficient to falsify that description. (Of course, a very similar theory of L might be set up which avoided the false predictions, but it would be a different theory nonetheless.)

The trouble with adopting Bochenski's point of view and allowing that the falsification of some prediction of a scientific system implies the falsification of either of the conjoined antecedent statements but not both is that the scientific system can, in this manner, always be preserved by laying the fault at the door not of the theory, T, but of the other, obviously less important, antecedent statement, A. This means that the scientific system in question could never in fact be falsified. It would be irrefutable because the falsification can always be explained away as due to some minor fault. In linguistics, rules of interpretation are useful as a form of immunisation of the theory for some linguists - where the rules of interpretation are minor antecedent statements. Where this is the case,

no amount of empirical evidence could ever in principle be used to reject an "immunised" theory or to choose between competing theories, such as generative semantics, case grammar and the extended standard theory. This is for two reasons. On the one hand, as we have said, it is always possible to spirit away some falsification by putting it down to some minor false assumption in the correspondence rules, which can always be corrected by another ad hoc assumption - hence the theory of exceptions in some versions of transformational grammar. On the other hand, since the only constraint imposed by the reductive method is that of operational success, one can imagine the proliferation of any number of descriptions incorporating all sorts of deep structures, each one equally irrefutable.<sup>8</sup>

#### b. Reduction in linguistics

As we have already noted, the reductive method of testing is associated in linguistics with the transformational generative approach. In fact, it is the only method of testing available to those linguists who treat transformational generative grammars as the only form of linguistic description. Other forms of "hypothetico-deductive" testing are available only to those linguists concerned with structural grammars or with the structural and generative aspects of linguistic description as heads and tails of the same coin.



The general strategy of those who adopt the reductive method of testing or, what amounts to the same thing, those who adopt a transformational-generative framework is as follows. Some form of deep structure is set up. This contains a set of structures defining the notion (initial symbol), S. The deep structure also contains a set of items, each of which is accompanied by a distributional statement defining the categories which may be entered by that item. By further defining the set of structures in terms of categories, it is possible to generate strings of items filling the defined set of structural categories. This is done by means of a set of permissible operations or rules. By means of a further set of transformational rules it is possible to determine a set of terminal strings which stand in a hypothetical relation of "correspondence" to the set of observables to be described.

What we have just described takes the form of a sort of deductive argument. Clearly, if we make our definitions properly, then the set of generated entities will be the set of necessary consequences of the categories, distributional statements and permissible operations of the grammar. For instance, to take a simplified example, if S is defined as the structure AB and x is an item which may appear in position A and y is an item which may appear in position B, then, in the absence of any other restrictive rule, the grammar will necessarily

generate the complex  $((x,A),(y,B))$ . Various transformational rules might be devised to operate necessarily or optionally on any part of the complex, but, given that a certain transformational rule is to apply, then the form of the resultant is a necessary consequence of the operation of that rule in a given context. Again, to take a simplified example, the phonetic form of  $x$ , above, might be defined in  $L$  as  $[fis]$  and  $y$  as  $[dop]$ . Now, given a rule that final unvoiced consonants are voiced in the context of an ensuing voiced consonant and no restrictive rule to the contrary, the final phonetic form of our complex will obviously be necessarily  $[fiz dop]$ . Now, if some semantic representation is also provided, we can say that a semantic-phonetic complex is generated as a necessary consequence of the adoption of a particular grammar.

It is clearly the case that no such grammar, however it may actually be formulated (i.e. begging no questions about the arrangement or form of particular deep structure proposals), will be acceptable, unless the generated strings are empirically adequate for the description of observed phenomena. This is, then, a necessary condition of the acceptability of the grammar. The usual method of testing is to set up a hypothetical relation of correspondence between the terminal strings and the set of observables, such that each terminal string corresponds to a non-null sub-class of the set of observables.



Regarded in terms of its logical relation to the class of terminal strings, the grammar can be considered as the antecedent in a logical (deductive) argument, whereas the terminal strings can be regarded as the consequents in such an argument. These consequents are usually known as the predictions or derived hypotheses of the system. From this it is obvious that the validity of the derived hypotheses is a necessary but not sufficient condition of the validity of the grammar, i.e. a transformational-generative grammar makes use of a reductive method of testing, insofar as its validity depends on the extensional correspondence of terminal strings and (idealised) data. The methodology involved is that of testing the hypothetical relation of correspondence between the derived hypotheses and the class of observables.

The reductive framework for the testing of generative descriptions has been described most often and most clearly by Shaumjan,<sup>9</sup> though the method has been described and used by many others also (as we shall see).

For our present purposes, the exposition of the hypothetico-deductive approach combined with a reductive method of testing given by Shaumjan will suffice to show how these techniques have been set up by linguists. Shaumjan begins with a comparison of "the genetic (or constructive)" and the "axiomatic"<sup>10</sup> methods of constructing

scientific systems. He tells us, "a generative device is a mathematical system based on the genetic method".<sup>11</sup> "Axiomatic methods" are said to be typically concerned with "classification" or "taxonomy". Shaumjan goes on to explain this distinction further by quoting Kleene,<sup>12</sup> as follows ;

"'With the axiomatic method the set of objects in relation to which the theory is constructed is not taken as given. What is taken as given is a certain system of statements describing a certain set of objects and a system of logical operations on the statements of the theory.

"'The genetic approach takes as its starting point certain given objects and a certain system of permissible operations on the objects. In the genetic theory the process of reasoning is represented as mental experimentation with objects which are taken as actually given.'"<sup>13</sup>

What Shaumjan presumably has in mind is the distinction between so-called "taxonomic" linguistic descriptions, which typically determine the set of descriptive entities, roughly speaking, by the application of a descriptive theory and transformational-generative descriptions which do not, in principle, presuppose any



prior establishment of the descriptive entities.

Shaumjan tells us ;

"In each generative device the following three components must be distinguished :

(1) A set of elementary grammatical objects from which complex objects are generated.

(2) A set of operations which apply to the elementary grammatical objects and serve to generate the complex grammatical objects.

(3) A set of structural specifications which are given to each complex grammatical object generated (in this way a hierarchy of generated, complex, grammatical objects is set up)."<sup>14</sup>

One can see that this outline of the "genetic" or "generative" approach accords with the general strategy of reductivists given above. It is obvious that, if the initial objects of the descriptive system are given, the only means of testing the adequacy of the description available is to examine the empirical adequacy of the generated complex entities. As we have said above, this is because no direct method of testing the initial inductive class of entities or the set of structural statements is available to us. If this approach is adopted,

then the generated complex entities may be regarded as the necessary consequents of the operation of the system. As we have said, this allows us to look upon the generation of the predictions or derived hypotheses of the system as a kind of deductive argument in which the components (1) - (3) given by Shaumjan are the antecedents and the generated complex entities are the consequents. It follows that the adequacy of the generated entities is a necessary but not sufficient condition of the validity of the system. That is to say that the genetic method of Shaumjan and **many** others is fundamentally reductivist in nature. From a strictly logical point of view, the fact that reductive testing is insufficient to test the validity of descriptive systems is in itself sufficient reason to regard this approach, however widespread it may be, as only a useful way of helping to determine the "projectivity" of the grammar. It does not help us to test the validity of any particular rule or object in the grammar (unless the grammar leads to false predictions and the cause of the falsehood can be traced). Positive tests merely corroborate the grammar.

There are also other difficulties with this method which are concerned more with the way the reductive method has been construed and used (or mis-used) in linguistics.

The first point to be made is that there is often



a confusion of two different points of view when the predictions of the system are considered. One point is that the "predictions" of the grammar, that is simply grammatical objects (n-tuples of linguistic objects), cannot be empirically true or empirically false, since objects do not have a truth-value. It is only by courtesy or as a short-hand that we can consider the predictions of a grammar as empirically true or false. To be precise, we would always have to say that for each member of the set of generated objects there is an empirical claim that the object in question corresponds to, or correctly models, some set of data. This point may be of a rather nit-picking nature but the fact that it is ignored is itself indicative of confused or inaccurate thinking and hides another confusion over the nature of the predictions of the grammar. The predictions are supposed to be the necessary consequences of a deductive argument and at the same time they are supposed to have empirical content. The point is that, as deductions within the descriptive system, the predictions, if validly deduced, are necessarily true. That is to say that the truth of the predictions depends, within the description, on the entities, definitions and operations of that system and on nothing else. However, if we claim that these self-same predictions are, at one and the same time, "empirical hypotheses" and thereby that the truth of the predictions depends on judgments of experience, then we fall into self-contradiction. The reason for this is that this

state of affairs is tantamount to the claim that the predictions and the antecedent statements of the grammar are both a priori true and that they are synthetic, i.e. their truth depends on experience of reality outside the description. Since a priori statements and synthetic statements are held to be mutually exclusive (see above), this position is self-contradictory. As we have argued, moreover, there can be no empirical evidence which is logically sufficient to justify the view that the antecedent statements of the grammar are synthetically true, although, as a system, they may be held a priori. To accept that a grammar is a system of statements which are both a priori and synthetically true would be tantamount to accepting, for instance, that the rules of Chomsky's phrase-structure component (and much else besides) are empirical truths a priori. This is precisely the position adopted by Shaumjan who fails to distinguish the "hypothetical" from the "deductive" in his explanation of the method of testing. He tells us,

"Logical conditions of adequacy for an explanation can be reduced to the following three points.

(1) The explanandum must be a logical consequence of the explanans.

(2) The explanans must contain general laws required for the description of the explanandum.



(3) The explanans must have empirical content, so that it is capable, at least in principle, of verification by experiment or observation." 15

Let us observe that exactly the same position is adopted by Sampson who tells us,

"A paradigm type of scientific explanation....is deductive explanation..., according to which the data are to be explained as logical consequences of premisses which are counted as explaining them. It is standard practice in science to adopt a hypothesis on the grounds that, perhaps in conjunction with "auxiliary hypotheses" ..., which are accepted independently, it entails further propositions which are observed to be true." 16

This type of scientific explanation is none other than Bochenski's reduction and is described in the following schematic form by Hempel ; he tells us that we can construe

"explanation as a deductive argument of this form ;

$C_1, C_2, \dots, C_k$

$L_1, L_2, \dots, L_r$

---

E

"Here  $C_1, C_2, \dots, C_k$  are statements describing the par-

ticular facts invoked ;  $L_1, L_2, \dots, L_r$  are general laws ; jointly these statements will be said to form the explanans. The conclusion E is a statement describing the explanandum-event".<sup>17</sup>

Clearly, it is a necessary condition of the adequacy of the explanans that E be empirically adequate to describe the explanandum-event.

Now, whether this form of explanation is "standard practice" in science, as Sampson suggests, or not, it is evident that the conclusion of the deductive argument is required by all these three writers to be both necessarily true, from the point of view of the descriptive system, and to depend for its truth on the experience of observable phenomena. This is precisely the inconsistency which arises from confusing the two different points of view involved in the deduction of predictions within the system and the testing of the claimed empirical adequacy of the deduced descriptive statements. It is for this reason that we have distinguished between calculus statements and the hypotheses which are formed through the empirical interpretation of them.

A second point, which has not been sufficiently stressed in linguistics, is that, as is obvious from Hempel's remarks, there is a distinction between the explanandum-event, as an observable event, and the conclusion of a deductive argument, which purports to describe that explanandum-event (and others like it).



It is vital to preserve this rather obvious distinction, since, otherwise, we are led into the self-evidently non-sensical claim that a deductive argument entails an observation or set of observations. A deductive argument entails its conclusion, which may or may not stand in a relation of correspondence with and model some observable or set of observables, depending on whether or not the conclusion is empirically adequate for the description of those observables. This may seem very obvious, yet Sampson, for one, clearly fails to appreciate the distinction, when he says in conjunction with the above ;

"An important characteristic of deductive explanation is that any datum needs only one explanation. Thus, let E be an observation or set of observations to be explained and suppose  $H_1$  and  $H_2$  are two hypotheses, such that  $H_1$  entails E and  $H_2$  entails E, but  $H_1$  and  $H_2$  are logically independent of one another : then E provides evidential support for the disjunction  $H_1 \vee H_2$ , but, if we have independent grounds for holding  $H_2$  to be true, then E provides no support for  $H_1$  (and vice versa)".<sup>18</sup>

Ignoring the import of this particular argument, it is clear that Sampson is either using the term E inconsistently to mean both the set of observations and the deduction from  $H_1$  or  $H_2$ , or he believes that data can be explained as logical consequences of premises. In either case the

argument is to be rejected. It is obvious that data, as purely contingent explicanda, are not the logical consequences of anything. Conversely, one simply fails to understand in what sense a hypothesis, as a scientific statement, could be said to entail an observation or set of observations. It is a further danger that it will also be forgotten that  $H_1$  and  $H_2$  are statements in a system, as is the deduction E, and not states of affairs.

A third confusion which has arisen over the use of the reductive method of testing is exemplified in the work of Katz, who does confuse statements of the theory with real states of affairs. The confusion concerns the ontological implications claimed for an explanans whose consequences are unrefuted. Katz makes out his argument in the course of a defence of an ideational theory of semantics. Such a theory, according to Katz, can be empirically testable, but yet have no recourse to introspection. His argument is as follows,

"When we say that thoughts and ideas need not be present in conscious experience and so need not be available to introspective observation, and further, as we would have to, that they are not publicly observable either, we are simply saying that they are unobservable in much the same sense in which physical scientists say that certain micro-entities and micro-processes are unobservable. We are being no more, or



less, meta-physical than they. What gives their theories about such entities and processes empirical content is that their theories connect the postulated existence of such things with certain observable phenomena through a complex chain of deductive relations."<sup>19</sup>

Such a method of justifying the claim of unobservable ideational entities is open to linguistics, Katz claims. The method he adopts is the familiar reductive one.

"The method is that of hypothetically postulating a theory within which some unobservable entity or process is described and related to the observable behaviour of public objects, and of empirically verifying the theory by checking to determine whether what it predicts on the basis of this relation between the observable and the unobservable accords with the data about the behaviour of the relevant public objects."<sup>20</sup>

Katz goes on to conclude that,

"The option of treating a theory of language and linguistic descriptions as hypothetically postulated theories...permits us to formulate the connection between a linguistic construction and an idea in terms of rules that relate the phonetic representation of the linguistic construction with the semantic representation of the idea."<sup>21</sup>

In this way it would, presumably, be claimed that, if

the theory makes adequate predictions, then one would be justified in hypothesising the existence of the unobservables in question. There are two comments to be made in this connection. In the first place, it is not at all clear that such a complex rigmarole is needed to justify the notion that we must hypothesise certain mental "micro-entities" and "micro-processes" relating to semantic knowledge in the brains of speakers. It would be quite sufficient to observe that without the assumption of semantic knowledge on the part of speakers it becomes impossible to explain interpersonal communication. What is at stake is not whether an ideational theory is possible, but whether a given ideational theory is empirically adequate, i.e. whether the claim that a given theory is justified as an empirically correct account of semantic knowledge. Now, as we have said, reductive testing (and Katz is obviously advocating reductive testing) allows only for the satisfaction of the necessary condition of empirical adequacy by the predictions of the system. Since this form of testing is insufficient to verify the validity of the hypothesised micro-entities and micro-processes, it is also insufficient to justify the further ontological claim that these underlying objects and processes correspond to the unobservable mental objects and processes which are assumed to exist in the brains of speakers. Katz' form of testing is simply insufficient for the



claim that the explanans (representation of semantic knowledge) represents or corresponds to the explanandum (assumed mental processes and objects). The most one could claim would be that the consequences of the deductive system observably correspond to the effects of some unobservable causal system. However many times the deductive system proved empirically adequate, the system in question could never be selected as the "correct" representation of the underlying unobservable process. In fact, there might be any number of such empirically adequate systems. One certainly could never claim to reveal the nature of unobservable processes through the inspection of the deductive system, which is what Katz seems to be claiming.

c. Deductive testing

As we have seen, deductive testing, as it has been described above, involves a valid logical inference (modus ponens). It follows that any method employing such a testing procedure could not be attacked on logical grounds. This does not rule out the possibility that such a mode of testing might be inappropriate from a practical point of view or that the form of linguistic analysis employing this form of testing was badly formulated. In this latter case it might be, for instance, that either the various conditions of the methodology lead to contradictory solutions or that the proposed

conditions were not sufficient after all (although they may perhaps be necessary).

As far as I am aware, no-one has explicitly defended this mode of testing in linguistics. It would seem, however, that various linguists have nevertheless made implicit use of it. It may be remarked that, after all, there is no need of an explicit defence of such a well known and familiar form of inference. If there are linguists who use this method of testing, then they are to be found among the ranks of the "Praguians" and "Neo-Praguians" and I.C. Analysts. Most notably, Trubetskoy and Martinet implicitly rely on deductive testing. As we have said, if there are criticisms to be made of the use of this method of testing by linguists such as these, then these criticisms are of a practical and not a logical nature. This is the exact reverse of the situation which we encountered above in connection with reductive testing. In reductive testing the objections raised were mainly of a theoretical and logical nature. One cannot doubt the feasibility of the reductive method of testing.

To explain some of the practical difficulties with deductive testing, let us try to follow Trubetskoy's chain of reasoning concerning phonemic analysis. The exposition of phonemic analysis in Principles of Phonology<sup>22</sup> shows most clearly the implicit reliance on deductive



testing and demonstrate also how this method may lead to contradictory solutions.<sup>23</sup>

Trubetskoy begins by remarking that the phonologist, as opposed to the phonetician,

"needs to consider only that aspect of sound which fulfills a specific function in the system of language."<sup>24</sup>

It is in this way that the phonologist, by adopting a particular function as his dimension of relevance, can arrive at a phonological constancy despite the infinite differences in sound substance which can be observed by the phonetician. As is well known, Trubetskoy adopts as his dimensions of relevance the three functions of language enunciated by Bühler, the most fundamental of which for linguistic analysis is the "representation" or "communicative" function of linguistic entities (linguistic signs). Linguistic signs, for Trubetskoy, have both an expression and a content. The expression side of the sign is made up, for Trubetskoy, of phonological entities. Phonological entities, per se, clearly have no meaning but they serve to distinguish those entities (signs) which do have meaning. Thus, phonological entities have a differentiative function, i.e. "a meaning-differentiating function or distinctive function".<sup>25</sup> Trubetskoy explains the notion of distinctiveness as follows,

"The concept of distinctiveness presupposes the concept of opposition. One thing can be distinguished only from another thing ; it can be distinguished only insofar as it is contrasted or opposed to something else, that is, insofar as a relationship of contrast or opposition exists between the two."<sup>26</sup>

Armed with this notion of distinctiveness, Trubetskoy can distinguish phonological units of various types, in particular those which are either simultaneous or successive in the chain. The division between simultaneous and successive entities allows the following definition ;

"Phonological units that, from the standpoint of a given language, cannot be analyzed into smaller successive distinctive units are phonemes."<sup>27</sup>

Since it is possible to establish distinctive entities which are non-successive but which are always found in conjunction with other such distinctive units ("distinctive features"), the above definition is equivalent to that which states, "the phoneme is the sum of the phonologically relevant properties of sound".<sup>28</sup>

Having stated his theoretical apparatus, Trubetskoy gives "rules for the determination of phonemes".<sup>29</sup> By this he means that ;



"After ascertaining the definition of the phoneme..., we must now give the practical rules by which a phoneme can be distinguished from phonetic variants on the one hand and from combinations of phonemes on the other."<sup>30</sup>

It is clear from this and from the actual formulation of the rules that Trubetskoy intends to determine the set of phonemes for a given language through the application of his rules to the "sounds" of a language. Strictly speaking, perhaps, one should begin with a hypothesis and then test that hypothesis by examining whether or not it is equivalent to a valid deduction from the application of the methodological rules. For all practical purposes this will make no difference, however, and, in any case, in Trubetskoy's day only Hjelmslev could be said to have been concerned with such methodological niceties. The deductive mode of testing is particularly evident in Trubetskoy's first two "paradigmatic" rules for the determination of phonemes. They are ;

"Rule I : Two sounds of a given language are merely optional variants of a given phoneme if they occur in exactly the same environment and are interchangeable without a change in the lexical meaning of the word.

"Rule II :: If two sounds occur in exactly the same position and cannot be interchanged without a change

in the meaning of the words or without rendering the word unrecognisable, the two sounds are phonetic realisations of two different phonemes."<sup>31</sup>

It is fairly clear that these two rules are each other's converse and that they both take the form of a classical deduction ; i.e. using the above argument schema, in the case of, say, rule II the satisfaction of certain methodological conditions (A) implies a certain phonemic solution (B) and so, if the methodological condition is in fact satisfied in a given language ( $\vdash A$ ), then one may conclude that there are two phonemes involved (B). In the case of rule I the argument schema is exactly the same except that the satisfaction of the conditions (A) implies (B) that a single phoneme is involved.

Now, although a commonsense usage of these rules would not allow it, if we stick to the letter of the condition, then it is quite possible to conclude that /gr/, /bl/, /poteit/ and /hir/ are phonemes of English on this basis, since these four commute in the context of /ou/ (orthographically, "grow", "blow", "potato" and "hero").<sup>32</sup> For this reason it is necessary to set up some "syntagmatic criterion" in order to establish the minimum successive units. It follows also, however, that the rules given by Trubetskoy are not really sufficient for the determination of phonemes but are sufficient only for the determination of phonologically distinctive



units. Unfortunately, the method of setting up a deductive testing procedure is inappropriate for establishing syntagmatic criteria for the determination of minimum successive units. The reason for this is that what is required by Trubetskoy is a necessary and not a sufficient condition. It is necessary for the commutation test that it take place in the same minimum relative position in the chain.<sup>33</sup> It follows that none of Trubetskoy's syntagmatic conditions could be appropriate to establishing minimum successive entities. If we leave aside those rules given by Trubetskoy which are of a more or less phonetic nature, this is particularly evident. Rule I (of the syntagmatic criteria) states, for instance, that ;

"Only those combinations of sound whose constituent parts in a given language are not distributed over two syllables are to be regarded as the realisation of single phonemes."<sup>34</sup>

Although this rule may well prove efficacious in many cases, it is nonetheless the case that combinations of sound which are not distributed over more than one syllable are still not necessarily to be considered single phonemes. Obviously, it is a sufficient condition of establishing that a sound represents a combination of two phonemes that a syllabic juncture divides those two sounds. However, one cannot say that the absence of such a juncture implies that we are dealing with a

single phoneme. In this case the supposed implication is false (it "goes the wrong way"). The situation is particularly difficult with affricates which are the usual source of the "un ou deux phonèmes" problem.<sup>35</sup> We find that the application of Trubetskoy's rules leads to contradictory results. Consider Trubetskoy's treatment of Russian [ts]. He says,

"In Russian, Polish, Czech, etc., where both constituents of the sound combination ts always belong to the same syllable, this combination of sounds is interpreted as a single phoneme (c) ."<sup>36</sup>

The determination of /c/ as a single phoneme of Russian depends, for Trubetskoy, on the application of the above syntagmatic rule I. In fact, the above paradigmatic rule II is also satisfied in a number of cases, e.g. [tsok(ʌt')] ("to click") - [tok] ("current") - [sok] ("juice"). On these grounds we would be tempted to adopt three phonemes in Russian, /t/, /s/ and /c/. However, if we apply the commutation test of rule II in other contexts, we find that the result of the previous tests is contradicted; we find, for example, [tsar'] ("czar") - [psar'] ("hunter") and [trok] ("surcingle") - [tsok(ʌt')] ("to click"). These commutations show [t] and [s] to be successively commutable entities in the chain. The fact that we do not find \*[pts] or \*[tsr] groups shows that [p] commutes in the same position as [t] and [r] in the same position as [s] (i.e. the



then [ts] can be analysed into smaller successive distinctive units and, therefore, cannot be the realisation of a single phoneme, /c/. Now, if this is so, we can conclude that not only does the criterion of syllabic juncture lead to contradictory results but that it is insufficient to determine sounds as belonging to a single phoneme in the chain : i.e. here is at least one example where the absence of a syllabic juncture does not imply that we are dealing with a single phoneme. Moreover, the valid deductions from the application of the paradigmatic rule II are in conflict with each other and with the application of syntagmatic rule I. By applying the paradigmatic rule II we arrive at different phonemic solutions by equally valid inferences because the rule is applied to different sets of data in each case. It is important, then, in any deductive methodology to employ a condition of "no contradiction" which would choose one solution over the other. Such a condition might choose the solution with the lower number of phonemes in the inventory or it might require the selection of the solution which involves the most refined analysis. However this may be, deductive methodologies must avoid contradictions in descriptions which arise from the application of them to different data within the same field. Trubetskoy's methodology fails in this respect (and so does immediate constituent analysis - as we saw above).

NOTES TO CHAPTER 10

1. I. M. Bochenski, Contemporary Methods of Thought, p. 92.
2. I. M. Bochenski, ibid., p. 92.
3. I. M. Bochenski, ibid., p. 94.
4. K. R. Popper, The Logic of Scientific Discovery, p. 76.
5. I. M. Bochenski, op. cit., p. 94.
6. K. R. Popper, op. cit., p. 76.
7. I. M. Bochenski, op. cit., p. 94.
8. A similar point is made by S. Hervey in his article, "Is Deep Structure Really Necessary ?".
9. S. K. Shaumjan, Principles, pp. 18 - 45, Problems, pp. 29 - 46 and Philosophical Questions, pp. 10 - 17.
10. S. K. Shaumjan, Principles, p. 18.
11. S. K. Shaumjan, Principles, p. 19.
12. S. Kleene, Introduction to Metamathematics, quoted by S. K. Shaumjan, Principles, pp. 44 - 45.
13. S. K. Shaumjan, Principles, p. 19.
14. S. K. Shaumjan, Principles, p. 18.
15. S. K. Shaumjan, Principles, pp. 44 - 5.
16. G. Sampson, "One Fact Needs One Explanation", pp. 231 - 2.
17. C. G. Hempel, "Explanation in Science and History", p. 55.
18. G. Sampson, op. cit., pp. 231 - 2.
19. J. J. Katz, The Philosophy of Language, p. 181.
20. J. J. Katz, ibid., pp. 181 - 2.
21. J. J. Katz, ibid., p. 183.
22. N. S. Trubetskoy, Principles of Phonology.



23. The criticisms below are not intended to belittle in any way Trubetskoy's fundamental contributions to phonology.
24. N. S. Trubetskoy, Principles of Phonology, p. 11.
25. N. S. Trubetskoy, ibid., p. 11.
26. N. S. Trubetskoy, ibid., p. 31.
27. N. S. Trubetskoy, ibid., p. 32.
28. N. S. Trubetskoy, ibid., p. 36.
29. N. S. Trubetskoy, ibid., p. 46.
30. N. S. Trubetskoy, ibid., p. 46.
31. N. S. Trubetskoy, ibid., p. 48.
32. See J. Vachek, "Phonemes and Phonological Units" for this argument.
33. This criterion was developed rigorously by J. Mulder in his Sets and Relations.
34. N. S. Trubetskoy, op. cit., p. 48.
35. See the article by A. Martinet, "Un ou deux phonèmes".
36. N. S. Trubetskoy, op. cit., p. 48.

CHAPTER ELEVEN

DESCRIPTIVE MODELLING AND EMPIRICAL TESTING

IN PHONOLOGY



In previous chapters we have seen examples of theory-building and description-building. We now turn to the empirical testing of linguistic descriptions. The processes of modelling and testing are considered in great detail in order to bring out the interplay of the calculus and methodology of the theory, descriptive models and statements, data and the application of the methodology.

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Linguistics aims at the construction of linguistic descriptions. Linguistic descriptions are the means to an ulterior goal. This goal is the understanding of the phenomena under description. As Hjelmslev pointed out ;

"Nous arrivons à l'intelligence ou à la connaissance d'une langue par le même chemin qui mène à l'intelligence des autres objets, à savoir par une description."<sup>1</sup>

Functionalists linguists take it to be axiomatic that the description of a language depends not upon an observed structure in the phenomena but on the viewpoint from which one chooses to consider the phenomena.<sup>2</sup> Linguists influenced by Saussure would be agreed that "la langue est une forme et non une substance"<sup>3</sup>. These two

attitudes imply that a linguistic description should provide us with a comprehension of the phenomena by stating the linguistic form which accounts for the communicational aspect of the phenomena under consideration. We can account for the phenomena of speech as communicational phenomena by a process of linguistic descriptive modelling.<sup>4</sup> Since functionalist linguistics does not adopt the naive realist view that the substance is itself structured in such a way that this inherent structure can be directly gleaned from the phenomena by inductive discovery procedures, one must conclude that a linguistic description must select a set of appropriate functions as its analytical basis. Such an analytical basis will necessarily adopt a particular point of view through a "rigid selection of functions as necessary and sufficient for unambiguous description", as Uldall<sup>5</sup> puts it. Any such set of functions<sup>6</sup> will constitute a potential theory.

A theory of the sort in question is required to meet certain conditions. In the first place, the set of analytical functions must be defined by sufficient and necessary conditions<sup>7</sup> in such a way that each analytical function is related to a methodology. The satisfaction of the conditions of the methodology must be equivalent to, or imply, the satisfaction of the conditions of the definition. In this way we can ensure that any descriptive model<sup>8</sup> which satisfies the



conditions of a given theoretical model (i.e. which is theoretically justified, not ad hoc) is directly comparable with the application of the methodology of the theory to the phenomena which the description purports to describe. This gives us a means of empirically testing descriptive models, since any genuine inconsistency between the results of applying the methodology and the descriptive model will be sufficient to show the unacceptability of the model. The theory, on the other hand, will be considered vacuous (inapplicable), if it does not contain any methodology, since without the methodology no application of the theory could be made; no descriptive model could be theoretically justified using the analytical functions of the theory.

Associated with the condition of "containing a methodology" is the requirement that each theoretical function ("meta-model", e.g. "is a phoneme", "is a phonotagm", etc.) be operationally relatable to functions whose conditions may be observably satisfied by empirical phenomena. Furthermore, it is required that any proposed descriptive model satisfying the conditions of a given theoretical function (e.g. /p/ in English under an Axiomatic Functionalist approach) be empirically interpreted using the operations defined in the theory. These two conditions will mean that ; (a) on

the theoretical plane, there must be an ontological framework such that all theoretical functions are inter-related and that at least one function is a "first-order" function (e.g. "is a phonetic form"), i.e. at least one function introduces the descriptive functions of phonetics which may be observably satisfied by empirical individuals observed in the communication substance and, (b) that, on the descriptive level, all functions must be satisfied in such a way that the first-order descriptive function (corresponding to the first-order theoretical function) is observably satisfied by empirical individuals in the phenomena under description.

Condition (a) ensures that linguistic theory is translatable<sup>9</sup> into the terms of another science (phonetics) such that the categories of phonetics are phonetic forms, the conditions of which may be observably satisfied by empirical individuals (e.g. [ b ] , [bilabial] , etc.). This seems to have been the kind of relation between linguistics and phonetics envisaged by Saussure, when he says,

"Les rapports de la linguistique avec la physiologie ne sont pas....difficiles à débrouiller ; la relation est unilatérale, en ce sens que l'étude des langues demande des éclaircissements à la physiologie des sons, mais ne lui en fournit aucun."<sup>10</sup>



The fact that phonetics now includes acoustic and auditory phonetics does not affect the unilaterality of this relation. It is the job of the notion phonetic form in Mulder's sign theory<sup>11</sup> to perform this connection between the world of speech phenomena as classified by phonetics and linguistic theory. A "phonetic form" is a class,  $\{\underline{i}\}$ , of "images", such that each image is the phonetic model of a single speech realisation. Each class of images is defined by a particular phonetic function, or form,  $\underline{f}$ . Linguistic theory now provides the criterion of linguistic relevance, "distinctive function in phonology", so that any phonetic form contracting a given distinctive function (allophone,  $(\underline{f}^i R \underline{d}^j)$ ) is on a different, though operationally related, ontological level. A "phonological form", as a class of phonetic forms each contracting the same distinctive function in phonology,  $\{\underline{f}\}^i R \underline{d}^j$ , is again a more abstract conception. Thus, in axiomatic functionalist linguistics, we find just such an ontological hierarchy related to at least one first-order function as is required. Condition b, above, ensures on the other hand that any phonological descriptive model is related to a set of observables satisfying the conditions of phonetic forms. For instance, the phonological model, /p/, in English is related to the set of phonetic forms,  $\{[p], [p^h] \dots\}$ , each of which is observably satisfiable.

It is further required that the theoretical functions,

their definitions and the methodology be consistent, sufficient and relatively simple<sup>12</sup> (free from redundancy). The requirement of consistency will be met, if the law of excluded middle is satisfied and if the theory is homogeneous (not eclectic). The theory will be sufficient, if it provides all necessary functions for the adequate description of fields of phenomena. If the theory is an axiomatic theory, then the axioms must be non-interderivable and non-redundant (necessary for their purpose). Any phonological description which satisfies the conditions of a theory of this kind will itself be required to meet formal conditions of the sort outlined above.

Most important of all, however, the phonological description must be directly empirical, i.e. it must be capable of empirical refutation with respect to the phenomena, when the said phenomena are considered with respect to the methodology of the theory. That is to say that there may be no inconsistency between the set of phenomena which satisfy the conditions of the description (the "scope" of that description) and the results of subjecting the describienda (the total "range" of phenomena under description) to analysis using the conditions of the methodology. This relation between the descriptive model and the phenomena analysed by the methodology gives us a means for the empirical testing of the model in question with respect to the phenomena.



In this form of testing the satisfaction of the functions of the theory will be equivalent to, or implied by, the satisfaction of the methodology. Consequently, the results of applying the methodology may not be at variance with any proposed descriptive model (for the same phenomena), since such an inconsistency would demonstrate the descriptive model to be inconsistent with the application of the theory to the phenomena.

The way the testing is carried out is as follows. A descriptive model is launched such that the model in question meets the above epistemological conditions, i.e. the model is (a) theoretically justified. (b) formally acceptable and (c) empirically interpreted. It is a necessary condition of the acceptability of the descriptive model that the hypothesis be established that the model is empirically adequate with respect to the phenomena and that this hypothesis be not refuted.<sup>13</sup> The latter condition may be interpreted as follows. The hypothesis of empirical adequacy is maintained as long as the application of the methodology to the phenomena does not yield results which are inconsistent with the validity of the model. If there is a genuine inconsistency<sup>14</sup> between "phenomena R methodology" and "descriptive model R phenomena", then the hypothesis of empirical adequacy is refuted. If so, then it follows that the description is rendered unacceptable by modus tollens, i.e. it fails the necessary condition of empirical adequacy.

This analytical procedure may be followed for all descriptive models provided that all meta-theoretical and epistemological conditions are met. We thus set up a notion of descriptive modelling and empirical testing which relates descriptive models to observable phenomena in such a way that valid models account for the phenomena through a relation of modelling isomorphism.

EXAMPLE

We start with the meta-theoretically acceptable statement, "/ts/ is a phoneme of Russian". We test the empirical validity of this statement through the empirical testing of the model, /ts/, in Russian.

From the theory we know that a phoneme is a phonological form. From the above calculus we know that a phonological form is a self-contained class of allophones and that an allophone is a particular phonetic form in its capacity of contracting a particular distinctive function in phonology. It follows that for each phoneme there is at least one phonetic form contracting the distinctive function of that phoneme. There must be at least one allophone of /ts/ in Russian. This condition is met; [ts] R d/ts/. The model is empirically interpreted.

We now determine the complexity of the [ts] phenomena by applying the appropriate methodology. A phoneme



is a self-contained simultaneous bundle of distinctive features in phonology or, equivalently, a minimum syntagmatic element in phonology. Simultaneous relations and syntagmatic relations<sup>15</sup> are mutually exclusive, since simultaneous relations are symmetrical relations between entities in combinations and syntagmatic relations are asymmetrical relations between entities in combinations. A phoneme is a simultaneous bundle and a minimum syntagmatic element. It follows that a phoneme may contract but not contain asymmetrical relations. Therefore, if /ts/ is a phoneme of Russian, then it cannot be shown that [ts] satisfies the conditions of asymmetrical complexes. If it can be demonstrated that [ts] contains asymmetrical relations, then it will be inconsistent to maintain that "/ts/ is a phoneme of Russian" is empirically valid, since /ts/ is a model of the phonetic events we call [ts] .

#### Methodology.

To demonstrate the phonemic complexity of an entity it is necessary but not sufficient to show that it is a combination of separately commuting entities. /ts/ meets this condition. /toK/ ("current") - /soK/ ("juice") - /tsoK(AT')/ ("to click, clatter"). This condition is insufficient, however, because /ts/, /s/ and /t/ may commute at the same position in the chain, i.e. they may be the voiceless correlates in a  $\pm$  voice correlation. In Russian, this could, in fact be the case. Consider,

/dzoS/ ("bunker") - /doK/ ("dock") - /zoF/ ("call").  
In order to resolve this problem one must introduce a criterion of syntagmatic relations. A sufficient methodological condition of syntagmatic relations in phonology is the reversibility of separately commuting elements in the same positions, contexts and distributional units such that a change in the communicational purport is achieved through the reversing of the elements. Clearly, any hierarchical relation is asymmetrical and, since the relation of greater or lesser degree of peripherality with respect to the phonological nucleus is a hierarchical relation, the condition of reversibility, which shows the separate relevance of degrees of peripherality, will be sufficient to demonstrate the presence of asymmetrical relations. In any simultaneous bundle degrees of peripherality will be irrelevant. Any proposed complex containing asymmetrical relations will be demonstrated to be not a simultaneous bundle. If /ts/ is a single phoneme in Russian, /ts/ occupies a single syntagmatic position in the phonotagm. For the purposes of the example, we establish the distributional unit in Russian as containing, provisionally, three pre-nuclear positions (E(xplosive)<sup>1</sup>, E<sup>2</sup> and E<sup>3</sup>) and two post-nuclear positions (I(mplosive)<sup>1</sup> and I<sup>2</sup>) such that the proposed phoneme may occupy E<sup>2</sup> and I<sup>2</sup> as follows ;



<u>E<sup>1</sup></u>	<u>E<sup>2</sup></u>	<u>E<sup>3</sup></u>	<u>N</u>	<u>I<sup>1</sup></u>	<u>I<sup>2</sup></u>	
F	ts	f'	e	∅	T	("into colour")
∅	g'	∅	e	r	ts	("cycles per second")

We now attempt to show that the relative peripherality of the components of /ts/ is separately relevant to communication. This is quite easily shown in the case of Russian, if we compare the following model with the one above.

<u>E<sup>1</sup></u>	<u>E<sup>2</sup></u>	<u>E<sup>3</sup></u>	<u>N</u>	<u>I<sup>1</sup></u>	<u>I<sup>2</sup></u>	
F	st	f	o	l	∅	("into the trunk")
∅	∅	v'	e	r	st	("versts", gen. pl.)

One must conclude that /ts/ is a complex of two phonemes<sup>16</sup> and that at least two additional positions are required in Russian, E<sup>4</sup> and I<sup>3</sup>. We thus show that /ts/ as a single phoneme is inconsistent with the application of the methodology to the phenomena. These conclusions are borne out by independent evidence.

<u>E<sup>1</sup></u>	<u>E<sup>2</sup></u>	<u>E<sup>3</sup></u>	<u>E<sup>4</sup></u>	<u>N</u>	<u>I<sup>1</sup></u>	<u>I<sup>2</sup></u>	<u>I<sup>3</sup></u>	
F	ʒ	t	r'	e	∅	K	∅	("into the drift")
F	T	ʒ	l'	e	n	∅	∅	("into the member")
F	S	g	l'	a	∅	T	∅	("glance")
∅	∅	∅	v	o	l	x	F	("wizard")
∅	∅	ʒ	∅	e	r	S	T'	("wool")

Furthermore, it is evident that the opposition, /t/ - /d/,

is suspended in the context of a following /s/ or /z/ and so the complex in question must be represented as /T + s/. The model of phenomena in which /ts/ is a single phoneme is inadequate and so the statement that "/ts/ is a phoneme of Russian" is empirically refuted. We set up a new model, /T + s/, which is subject to further testing.

The actual results of our analysis are not the most important thing here. What is important is that the example shows the many complexities of linguistic modelling and empirical testing. In particular, we have seen that a descriptive statement is given meaning, in the first place, through the interpretation of the names of objects and relations it contains by descriptive models which are themselves empirically interpretable. In the second place, the descriptive statement is made meaningful through the interpretation of the theoretical terms it contains by a given linguistic theory. The theory also controls the structure of the descriptive models and, hence, the empirical interpretation of the descriptive statement. Furthermore, it is the theory that provides the relevant tests of descriptive statements through the methodology. In the above example, the first descriptive statement was shown to be definitely false on the application of the relevant tests. To



arrive at descriptive statements that have not been refuted we have to change not only the initial descriptive statement and the descriptive models which interpret it but also numerous other parts of the description. What we arrive at is, in fact, new descriptive models and new descriptive statements. Quite clearly, this is not the end of the process, since the entire cycle of testing and modelling may be started again in the light of any new evidence which contradicts the present description.

NOTES TO CHAPTER 11

1. L. Hjelmslev, Le Langage, p. 29.
2. "Bien loin que l'objet précède le point de vue, on dirait que c'est le point de vue qui crée l'objet", F. de Saussure, Cours, p. 23.
3. F. de Saussure, Cours, p. 169.
4. See, for example, S. K. Shaumjan, Principles, pp. 52 - 68.
5. H-J. Uldall, Outline of Glossematics, p. 11.
6. The functions of linguistic theory are "propositional functions" in the sense of A. Whitehead and B. Russell, Principia Mathematica, pp. 14 - 15.
7. According to Whitehead and Russell, a definition is an equivalence relation. A "definiens" is a sufficient and necessary condition of its "definiendum". See Principia Mathematica, p. 11.
8. The distinction between linguistic theory and linguistic description is as established above.
9. This "translatability" is advantageous to linguistics in allowing an easy and obvious way of relating phonological models to uncontroversial and intersubjectively agreed phenomena.
10. F. de Saussure, Cours, p. 21.
11. See, for instance, J. Mulder, "From Sound to Denotation" or "Postulats de la Linguistique Fonctionnelle Axiomatique".
12. See J. Mulder, "Linguistic Theory,.....".



13. K. R. Popper, *The Logic of Scientific Discovery*, pp. 86 - 92.
14. That is, one which is not attributable to any mistake in the application of the methodology or one which may be otherwise consistently accounted for in some other part of the description.
15. For "syntagmatic relations", see further, J. Mulder, Sets and Relations, pp. 30 - 33.
16. That is, we have adduced a genuine inconsistency between the application of the methodology and the descriptive model.

APPENDIX

EMPIRICAL TESTING AND LINGUISTIC THEORY



We have seen that the relation between linguistic theory and empirical testing is indirect but highly important. To put the matter in a deliberately paradoxical light, we could say that a linguistic theory is not subject to empirical testing but that empirical tests are the most important tests of linguistic theories. Following Hjelmslev and Mulder, we will say that a linguistic theory has no empirical content and, hence, the tests to which a linguistic theory is subject are non-empirical. That is, there are no observations of phenomena which could, even in principle, be relevant to testing the truth or falsehood of a linguistic theory. This is, however, not to say that the adequacy of a linguistic theory cannot be empirically tested indirectly via linguistic descriptions. We explain this as follows.

Apart from logical considerations, we expect a linguistic theory to contain : a. a calculus giving possible linguistic types (the phoneme, the distinctive feature, etc.), possible linguistic relations (dependency, constituency, etc.) and the connections between the types and the relations (distribution, sign theory, etc.) ; b. the definitions of these types, relations and connections ; c. the ontological levels of the theory ; and d. a methodology to test descriptive claims made in accordance with the application of the theory.

By contrast we expect a linguistic description to

be an empirical system. This means that, apart from logical considerations, the description must : a. satisfy the conditions of the theory and b. make claims about a specific set of data. These claims must be capable of empirical test. It is only by means of empirical testing that we can determine whether a given descriptive claim or set of claims is false.

We can sum up by saying that a linguistic description is a model of a theory in the model-theoretical sense. The description is a way of applying the theory to data. It relates a non-empirical theory to the empirical world by making a model of the data which satisfies the theory. A linguistic model is, then, a conceptual intermediary between a linguistic theory and the world - an interpretation of the theory. Let us introduce the following diagram ;

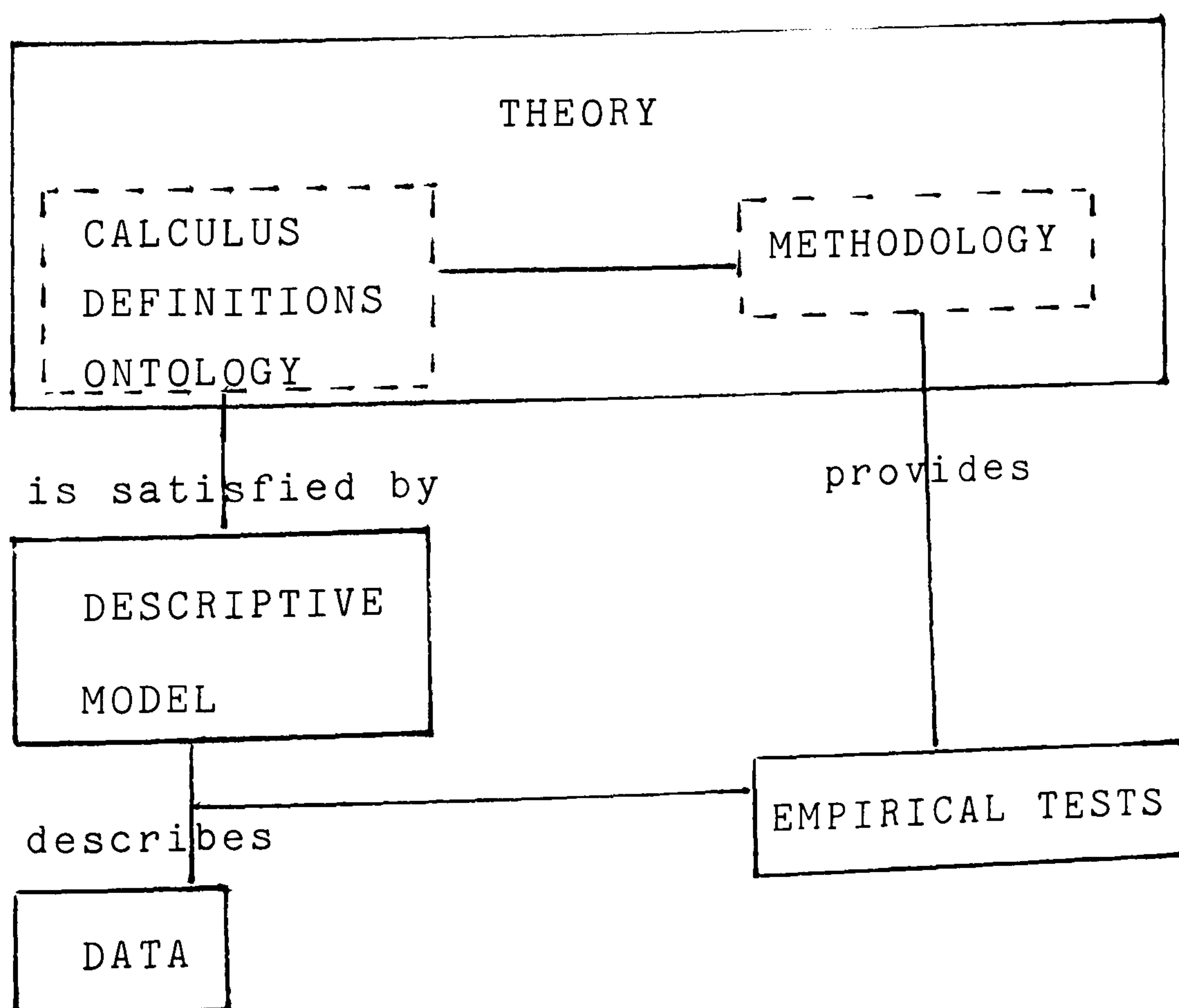


DIAGRAM 1




Following on from what we said above, the diagram adds the ideas that : a. the descriptive model satisfies the 'calculus' part of the theory ; b. the relation between the model and the theory is subject only to logical checking ; c. empirical tests follow from the methodology and the methodology is closely connected to the rest of the theory ; and d. an empirical test is aimed at the relation between the model and the data.

Ideally, we could imagine different stages in theoretical and descriptive activity ; i.e.,

1. CONSTRUCTION OF THEORY

2. APPLICATION OF THEORY

- 
- a. CONSTRUCTION OF MODEL
  - b. THEORETICAL TESTING OF MODEL
  - c. EMPIRICAL TESTING OF MODEL

3. TESTING OF THEORY

The testing of theories and models involves definite meta-theoretical conditions (such as those discussed above). The requirement that a model be empirically testable (and that it withsatnd relevant empirical tests) is itself a meta-theoretical condition. We might have a diagram as follows to show the relation between meta-theoretical conditions and other parts of linguistic activity (arrowed lines) :

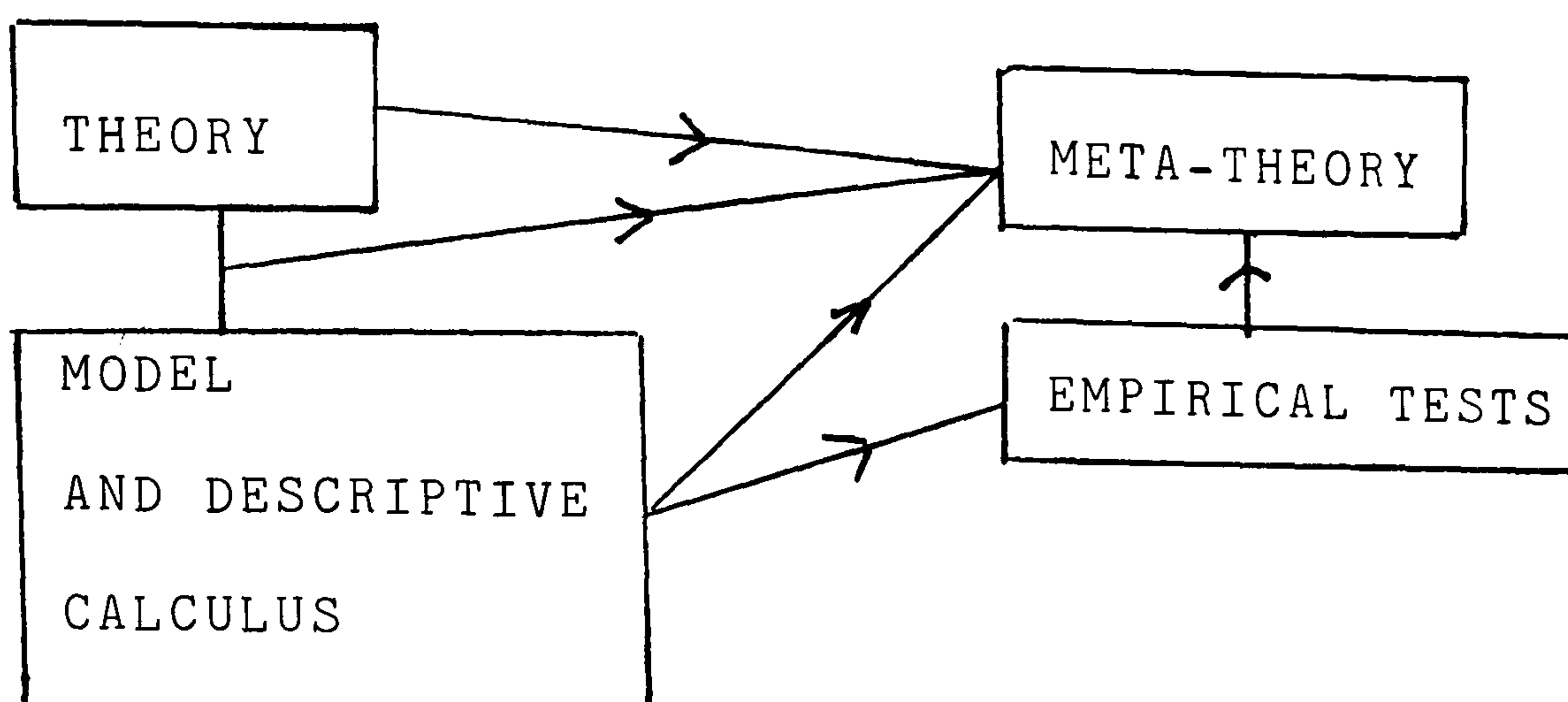


DIAGRAM 2

(The descriptive calculus is subject to meta-theoretical conditions but is only indirectly affected by empirical considerations).

This idealisation presents, of course, only a simplified rationalisation of the factors involved - a framework of ideas which helps us to juggle the many variables involved in linguistic theorising. In fact, the construction, application and testing of theories **proceed simultaneously** with the construction and testing of models. Each application is a test of the theory. But to say this is to claim that a theory must be productive of empirically viable models. This means that we can assert the following maxim ; no theory is acceptable without empirically acceptable models.

This is why a linguistic theory is not directly subject to empirical testing but empirical testing is the most important test of a linguistic theory.



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