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Con lo cual, adelantamos la fecha de la primera datación de *esturión* en nuestra lengua en unos 90 años. Y la datación de *sollo* se adelanta en 243 años.

Pero todavía queda otro *sollo*, el *sollo* 'soplo' 'golpe de viento' que no registra ni el DRAE [19], ni el DCELC, ni el DEEH, a pesar de que los tres recogen *sollar* 'soplar'. Tampoco lo recoge el REW, que también registra *sollar*.

Sollo 'soplo' es un deverbativo de *sollar* y es también una palabra venerable perteneciente a la más decantada solera de nuestro idioma. El testimonio más antiguo que poseo es de 1280 y se encuentra en la *General estoria*:

> e estando alli el uino un *sollo* de uiento, et mouio aquellas cannaveras.
> salio adesora un tan grand *sollo* de uiento cierço que todos los metio so ell agua
> (*Gen. estoria*, 1280) [67].

Otro testimonio hay en la *Biblia medieval romanceada*:

> E fue tu adalid en el desierto grand e espauentable, do era la sierpe, que quemaua con el *sollo*, e el escorpion e la dipsa.
> (*Bibl. rom. Pent.*: *Deut.*, s. XIII) [68].

Y hasta aquí estas notas, escritas con amistosa cordialidad para un amigo.

Málaga

(67) Alfonso El Sabio, *General estoria*, Edición de J. Antonio G. Solalinde, Madrid, 1930, I, 161b y 195b.

(68) *Biblia medieval romanceada, I. Pentateuco*, Buenos Aires, 1927, 249a, c. VIII v.[15].

The strategy of linguistics

by Jan W. F. MULDER

When building a theory, or even when judging or criticising an existing theory (perhaps one's own) a number of arbitrary, though thoroughly motivated, choices are involved. These choices range from the very general (i.e. epistemological) to the extremely particular. Now, even epistemology cannot escape being, to a certain extent, historically determined. That is, we are more or less forced to regard as scientific what historically has come to be regarded as being worthy of the epithet «scientific». One does not, nowadays, generally regard the methodology used in, say, theology, or in politics, as «scientific». As to theology, there may have been a time (frequently called «the dark ages»), when all science used speculative reasoning, and when dogmatics was as respectable in science as it still is in politics, but those days have gone. For theology, speculative reasoning does not only seem to be a necessity, but it has become so refined that one cannot help admiring it. I for one would, therefore, not dare to state categorically that speculative reasoning is — humanly speaking, at least — inferior to scientific reasoning. But I would object to calling «scientific» anything that is speculative, or has anything speculative in it. Is for this reason in the first place, that I would reject any linguistics that uses even a partially speculative method. Linguistics is, or, at least in my opinion, should be, a *scientific* discipline. I reject, therefore, most approaches that are connected, directly or indirectly, with the so-called Chomskyan revolution. I, furthermore, reject all other approaches that have pledged their allegiance to the so-called «black box» approach. These include exponents of the, otherwise respectable, schools of Tagmemics and Stratificational Grammar. For instance W. A. Cook (Cook 1969), a tagmemicist, quoting J. G. Keméný (Keméný 1959), says «Suppose you have a locked box, which cannot be opened without destroying its contents. One can observe how the box works, and predict how it will act. We can form theory as to what kind of machinery is in the box, and we will maintain that theory as long as our predictions concerning the box are verified». He goes on to say that the linguist «constructs a formal grammar, a stament of *competence*, and maintains his theory as long as it continues to predict accurately the *performance* of native speakers» (my italics). Cook adds — thereby giving his staments at least the appearance of scientific respectability — that «this formal grammar has only a relation of equivalence, not a relation of identity, with the fundamental grammar in the mind of the speaker». But so had, has, and will have any description of any consequence, as far as its scope goes. Even the story of the tower of Babel could be rated an acceptable linguistic theory on this account. The trouble is that in the term 'theory' as used by Cook there is a double confusion. The first of these confusions, in fact, implies the second. In the first place 'theory' is here used in the popular sense of pure speculation. Note that also the term «hypothesis» is popularly used in this way. The second error, a common one even among philosophers and scientists, is to confuse 'theory' with 'description', and, implicitly, to regard a theory as a set of hypotheses or hypothetical models. We can, then, read Cook's account of the little black box as follows: «We cannot observe what the contents are, but we guess what they are. We now set up a description of our speculation (called a 'theory' by Cook) and speculate that the object as described is at least equivalent to what is actually in the box». Of course, if 'equivalence' is rendered so trivial as merely to mean that objects are considered equivalent if and only

if they can be imagined to have the same effect (note that in that case all linguistic theories are equivalent in so far as their scopes coincide), then Cook is still right in what he says. Nevertheless, I call the whole procedure 'speculation' and recognise in it the normal procedure of theology. It is, of course, possible that some sciences, including some of the scarcely very precise social sciences, notably psychology, have a need to avail themselves, at least for the solution of *some* of their problems, of methods of this kind. But I should like to maintain that such disciplines are non-scientific to the extent that they do use these methods. D. G. Lockwood (Lockwood 1972) argues along similar lines as Cook does, and invokes Astronomy as an example of a science that works in the way he advocates. Unfortunately his analogy has several flaws. Astronomy deals with a particular sub-section of the world of physical phenomena. It is an application of a theory of physics in the description of those phenomena, and, strictly speaking, it is, therefore, a branch of physics. What gives it an air of independence and separateness is the fact that it deals with macro-phenomena, which makes it *look* rather different from other descriptions that are made in terms of physics. Also, due to the particular type of phenomena it deals with, and due to the particularities of interest this creates, astronomy may occupy itself with certain side-issues that are, perhaps, not strictly speaking within the realm of physics. But this is no less true in other branches of descriptive physics. Physics itself, including astronomy, avails itself of a *'point de vue'* (see below), which makes it especially interested in quantitative measurement and mathematical calculus. For this it relies havily on several branches of mathematics. Granted that direct empirical observation is in most cases impossible, much use is made of indirect observation, i.e. observation that involves calculus. Thus, on the one hand instruments are used, of wich the mathematical properties are known, and on the other hand 'models' are established, of which, too, the mathematical properties are known. Moreover these 'models' are for the most part 'simulation models', i.e. simplified scale-copies of observed phenomena. Predictions are made with the aid of these models, and by quantitative calculus, and the predictions are tested by observation in wich instruments are used. Another corollary of the 'point of view' in theories of physics is the particular interest physics, including astronomy, takes in causal explanation. It goes without saying that its methodology is geared to this area of interest. This is why 'prediction' plays such an important role. But the important point is that every minute stage between cause and effect is carefully reconstructed by both calculus and (often indirect) observation. No stage between two interstages is furnished by speculation, but is only hypothesised if it is the result of calculus and/or observation. In those cases where speculation (i.e. imagination or guess-work) is brought in — but many astronomers refrain from doing this at all — astronomy ceases to be scientific. But even in such cases calculus is used to the utmost and most astronomers would regard the resulting models, e.g. the notion 'black hole', as purely formal and mathematical, rather than scientific-descriptive. It would be an awful insult to astronomers to treat their way of working as analogous with some recent developments in linguistics, especially the 'little black box' epistomology which, indeed, bears more resemblance in that respect to astrology than to astronomy. In the case of «the black box», there is a totally hidden mechanism, and some caricature that can be imagined to produce the same output is to be regarded as being equivalent — and what is meant is actally «structurally' equivalent — to the thing in the box. Lockwood says: «Underlying this method of investigation is the assumption that the more closely the behavior of a model approximates that of the unobservable system under investigation, the closer the internal workings of the model can be expected to correspond to the internal structure of the actual system. Luckily he adds: «The possibility of equally workable alternate models is not to be excluded, however». But even so, this is an extremely naive assumption indeed, and moreover falsified many times over by all sorts of 'substitution' devices we know and which can scarcely be called «models». Why, if such an assumption were correct, do not the *structures* of robots bear more resemblance to the *structure* of human beings than they actually do, or why do not pocket calculators bear more *structural* resemblance to the relevant portions of the human brain

than they actually do? For all we can observe, it even seems to be the case that important differences in hardware between model and modelled make profound differences and structural inequivalences imperative. What greater difference in hardware could there be than there is between a particular «grammar» and the «competence» of a native speaker. Unless 'equivalence' is taken in the most trivial possible and thereby wholly superfluous, sense, this assumption remains not just naive, but extremely unlikely to have any chance of being correct. On the contrary, it has almost every chance of being false. The only aspect of it that is logically true — and this is, therefore, the most one can say of it — is that a model that cannot do — in an abstract sense of 'doing' — what the object does, e.g. it does not account for existing linguistic data, or produce new data, cannot be considered to be a structurally correct simulation model of such an object.

It should be said that the above opinion of Cook and Lockwood are not essential to Tagmemics and Stratificational Grammar respectively. The above criticism is, therefore, not levelled against those two doctrines as such. With respect to Transformationalism, however, matters are different, as the whole movement stands or falls with the acceptance or rejection of the above thesis of 'equivalence' between «grammar» and «competence». This thesis is implicitly the sole justification for the 'generative', rather than 'analytic' approach. There are, of course, other arguments adduced, such as the fact that a native speaker is able to produce completely novel sentences, and that, therefore, the mere analysis of a corpus does not lead to a satisfactory description of data. Such arguments do not, however, impress those linguists who know more than Transformational Grammer alone. Only an approach which limited itself, to an unacceptable degree, to corpus-analysis would produce descriptions that *could not* 'generate' data beyond the original corpus as such. Arguments like those adduced by transformationalists against the so-called taxonomic approach, perhaps impress the dilettante who naively equates description with merely *recording* wat is directly there in the data, without any generalisation or other administrative procedures whatsoever, but such arguments could scarcely cut any ice with more mature linguists.

All this does not mean that the speculative approach is denied validity as a source of gaining knowledge. But such knowledge cannot be considered *scientific* knowledge. Moreover, such knowledge, being less 'exact' than scientific knowledge, along with the speculative method employed in obtaining it, should be abandoned if scientific methods covering the same scope are available. There are, of course, objects and problems that cannot be reached in a scientific way. Pondering about them is to ponder the imponderable, and this involves speculation. One of those imponderables is, perhaps, the very existence of the Universe itself, and philosophers and theologians alike are fully justified in speculating about this. It is possible that there are also some areas within the sphere of psychology where speculation has to play an importante role. Also, if some linguists take an interest in the intricacies of the human mind, and begin to ponder about innate competence, language-acquisition, and the intuition of native speakers, they are perfectly entitled to take refuge in speculation, because one can only speculate about such matters. What I do object to is, however, the presentation of this procedure as «science», the new paradigm of science which replaces all the previous ones. It is further objectionable that this procedure should be considered, therefore, a revolution, 'the Chomskyan revolution'. Far from being a revolution, Chomsky's achievement was the revival of virtually obsolete (at least in the respectable sciences) interests and methods. As transformationalism coincides with other forms of structural linguistics neither in subject-matter nor in methodology, it is hard to say in what sense it could be said to replace or to revolutionise linguistics. It is even regrettable that the same term 'linguistics' was applied to two such disparate disciplines, in fact almost disparate enough to exist side by side. No formalisation, let alone formulisation, can whitewash the fact that transformationalism sanctions the revival of an obsolete method and nor can any act of making such a method explic. As R. Jolivet (olivet 1976) has nise linguistics. It is even regrettable that the same term 'linguis- of making such a method explict. As R. Jolivet (Jolivet 1976) has

so convincingly argued: «la formalisation n'est ni le seul, ni sans doute le principal aspect qu'il faille considérer dans l'évaluation d'une théorie linguistique». Elsewhere in the same paper Jolivet says: «on voudrait simplement montrer ici qu'en tout état de cause la formalisation ne saurait donc garantir, en elle-même, l'adéquation, l'excellence ou la «vérité» d'une théorie linguistique». Transformationalists use the term «theory» in a rather odd sense anyway, a fact wich has also been noted by Jolivet, who adds that formalisation of a theory, up to now, has merely meant formalisation of descriptions. It should be noted, however, that what Jolivet calls *'formalisation'* is what I have referred to as 'formulisation' and he does not regard the result of an *«axiomatisation matérielle»* (as for instance in Axiomatic Functionalism) of a theory, as a *formalised* theory.

There is another important arbitrary, but motivated, choice which a theory-builder has to make. This choice is still of a philosophical nature, but it belongs to methodology, rather than epistemology. In practice this boils down to choosing between a hypothetico-deductive, and an inductive methodology. In addition the scientist must choose whether he allows himself — in the case of descriptions, rather than theories — to make such statements, or construct models, that have no known or even testable relation to the data at all. I have said that this can only apply to descriptions, as a theory, by its very nature, as L. Hjelmslev (Hjelmslev 1953) has put it, does not contain any existential postulate. We shall see, however, that inductivism, even in its mildest forms, is conducive to confusion between 'theory' and 'descriptions'. Typically, instrumentalism tends to go hand in hand with inductivism. It is often said that transformationalism, which can be characterised as being speculative-inductive, or, perhaps, rather speculative-intuitive (its self-styled 'hypothetico-deductivism' confuses 'hypothetical' with 'speculative', and uses the term 'deductive' in a very narrow and, in fact, odd, sense), is a paradigm example of instrumentalism, but this is missing the point. The difference between 'instrumentalist' and 'speculative' (note that both refer to descriptions or descriptive methodologies, not to theories proper) is that existential assumptions are attached to the second but not to the first. The «black hole» approach in astronomy is instrumentalist, and so is the notion «morpho-phoneme» in Bloomfieldian linguistics, unless one assumes that their objects really exist, in wich case they are speculative. But there may be certain notions in varieties of transformationalism, e.g. perhaps, 'lexical decomposition', or, even, the recognition of the auxiliary «to do» in every underlying sentence structure, wich are rather instrumentalist than speculative. In transformationalist approaches it is difficult to distinguish between the two attitudes, especially as speculation is not *a priori* declared inadmissible. What makes transformational grammar even more difficult to pin down on this point is that it appears that the whole of an adequate grammar has the character of a speculation (i.e. existential import is claimed for this), but individual components or mechanisms may have no more than a purely instrumental character. The only criticism one can offer here is that this is logically absurd. The abovementioned inductive element comes in especially where so-called «substantive universals» are assigned a place in the model.

In inductivism, of which early Bloomfieldianism and also later Bloomfieldianism, especially Harris, are paradigm examples, the linguist is supposed to arrive at a description by «discovering» essential recurring patterns in the facts. I shall not in this article go into the question of whether this can be done at all without at least some primitive theoretical notions, or «subjective bias», nor into other problems of induction. On the whole, inductivists are probably right in stressing that an observational knowledge of the objects in question plays an important part in the theoretical or descriptive hunches (hypotheses) that are launched by the investigator. On the other hand, deductivists of the logico-positivist kind are right in stressing that the exact source of those hunches cannot be ascertaind, and, therefore, any real proof of correctness must be based on formal procedures that take distance from the probable source of these hunches, — e.g. from «observation». The hypothetico-deductivists are even more right in concluding from all this that a hypothesis cannot be verified, only refuted in case it can be shown to conflict with the data. But theirs too is a weak foundation as long as they stick to the general confusion

in much of existing philosophy of science between 'theory' on the one hand and 'description' on the other. In fact, as long as philosophy of science tends to overlook this distinction between theory and description, people will continue to fight out these problems, as they have done for centuries. I cannot go into the details of the distinction here, as I have done this elswhere (Mulder 1975). Suffice it to say that, according to the Axiomatic Functionalist philosophy of science, which is hypothetico-deductive, but with a difference ...... call it theoretico-deductive, or theoretico-hypothetical, a *structural* description of any set of data, if it is to be taken as scientifically serious, presupposes a theory as an instrument. Without such a theory a description would not even be meaningful, in a formal sense. The theory is to be axiomatic-deductively organised, i.e. it contains, as statements, axioms and definitions, which lead to theorems, and, as terms, it contains primitive and defined terms. The theory may also contain so-called «models», or «models» can be derived from some of its statements. The borderline, in the case of definitions, between 'statements' and 'models' is a slim one inedeed. The task of definitions is to clarify terms of the theory. This it does in anticipation of the possible use of those terms in descriptive statements, but it does not imply any existential import for the notions referred to by those terms. Indeed, the theory itself — in Hjelmslev's conception (Hjelmslev, 1953) — does not contain any existential postulate. If it did, one would be sure to introduce circularity in descriptive statements on the basis of that theory. A subsidiary, but no less important, task of definitions is to introduce notions of the theory. From some of the definitions which do this — e.g. the definitions of «phoneme», «syntagm», «distributional unit», etc. — «models» can be derived, so one can say that the terms in question refer to «models», and that some definitions introduce «models». «Models» are «notions» of a particular kind, i.e. they are expected to be able to be brought in a certain relation of isomorphism to models in the description, the later standing in a relation of isomorphism to phenomena. The expectation of «realisability» is part of the meta-hypothesis of *adequacy* that accompanies any lauching of a statement or model in the theory. It is the meta-hypothesis of adequacy that, via the adequacy of description, links the theory to the phenomena within its scope and makes it deserve the epithet «empirical», but in a quite different sense from the inductivist use of this term. It should, namely, by now be clear that such an epithet cannot be taken to imply existential import for anything in the theory, be it «model» or anthing else. Statements and models carry with them also the meta-hypothesis of «consistency within the theory», and the, at least intuitive, assumption of «maximally possible simplicity». These meta-hypotheses *are* proper hypotheses, not just theoretical *a priori's* or calculables, nor speculations, as they have a) *reference to observable data* (i.e. the statements of the theory as «objects» in themselves), and b)*they can in principle be refuted by counter-evidence.* I have called them *meta*-hypotheses, as they are *about* the theory, not *in* the theory itself. The theory does not *contain* hypotheses.

A description, on the other hand, does not contain axioms, nor definitions of the kind found in the theory, but primarily it contains *hypotheses.* It may, in addition, contain *quasi*-definitions (i.e. definition-like statements introducing generalising *labels*) for purely descriptive notions, e.g. «consonant», «semi-vowel», «verb» «adjective», «subject», «direct object», and so on. Such labels are language-specific, i.e. they have to be established for each language separately. They are mere *«étiquettes»* that are stuck on to generalising classes of descriptive objects. The device of labelling is mainly of a simplificatory and administrative nature, and it is *internal* to the description. As a language is a system *«où tout se tient»* (Saussure, *Cours*), one should be extremely careful in proceeding from labelling to inter-linguistic generalisation, let alone to the establishment of descriptive linguistic universals.

A hypothesis is refuted if it conflicts with the data. If it conflicts with other statements about the data under the same theory it is also invalidated («refutation» implies «invalidation» but not *vice versa*), but it is not the hypothesis itself, but a meta-hypothesis, i.e. a hypothesis about the hypothesis, that is refuted. It is, namely, the case that also every *descriptive* statement carries with it the meta-hypothesis of its «consistency» (within the description), its «adequacy», and the assumption of not violating the principle of «simplicity». If any of the former two are refuted, the description as it stands is invalidated, and has to be revised. However, any refutation of the meta-hypothesis of «adequacy» is simultaneous with, and a direct corollary of the refutation of the statement, as a hypothesis, in question. Materially speaking, refuting the meta-hypothesis of «adequacy», implicitly accompanying a descriptive hypothesis, and refuting that descriptive hypothesis, are one and the same act. It is only formally speaking that they are different, but even so they are formally *equivalent* by material implication.

What I have been advocating here is a very strict use of the terms «hypothesis» and «theory». I am, of course, aware of the fact that there may be sciences, e.g. those concerned with causation, and those in which taxonomies play an overriding role, for which a more lenient attitude may be appropriate. But *non -speculative* linguistics as we know it (i.e. excluding transformationalism and its cognates) is a structuralist science in the first place. Causation does not play a part in it, and taxonomies are merely economising or other administrative (ultimately simplifying) devices *within the description.* It is in taxonomies that inductive processes can be tolerated, but even here it has to be recognised that these need the backing of some sort of, at least implicit, theory to have any strict validity or meaning. It even remains to be seen whether a taxonomy, without the backing of a theory (however crude) would be at all possible. We may agree here with Chomsky that to treat taxonomies as the ultimate in linguistics is highly unsatisfactory. Taxonomies may *facilitate* explanation, but, by themselves, they explain nothing. They are mere simplificatory devices in describing the data. What Chomsky did not realise, in his obvious ignorance, was the fact that his indignation was only justified in the narrow context — and then only partly — of Bloomfieldian linguistics. He was perefctly entitled to bring considerations of causation into linguistics, but for this he had to alter the scope, and he did not succeed in doing this in such a way that speculation was kept down to a tolerable level. Chomskyanism could, therefore, not *replace* Bloomfieldianism, let alone non-inductivist Saussurian types of linguistics, for two reasons, a) transformationalism was not the same discipline, and b) to be speculative implies, scientifically speaking, working on a lower plane. Here I may, perhaps, invoke the constraints of history: methods of enquiry tend to develop from being speculative, *via* being inductive and taxonomic, to being deductive and exact. Even theologians would agree that speculation is to be abandoned wherever and whenever observation is possible, and inductive methods can be developed; and even the most stubborn inductivist would not deny that, say, Pythagoras' theorem states a more exact truth than, say, the statement that «to like» cannot occur without an «object», in English.

This brings us back to the question of what we have to understand by the term «hypothesis». There is no doubt that last type of statement is a true hypothesis in the sense I have used that term, even though use of the term «object» implies that taxonomic devices have already been applied. But this only means that the term «object» is used as a generalising *class label*, and hence as a symbol for a variable of some kind. But inductivists would probably — I know of several who definitely would — call Pythagoras' theorem a hypothesis too. Some might even call Pythagoras' theorem a hypothesis, while calling the statement about the compulsory transitivity of «to like» an observational truth. Even those non-inductivists who do not make a sharp distinction between «theory» and «description», would have the greatest difficulty in not regarding both as hypotheses. But Pythagoras' theorem lacks at least the second of the conditions for being a hypothesis, i.e. that of being in principle refutable. If we take the latter in conjunction with the first criterion for being a proper hypothesis, «refutable» means here «refutable by confrontation with the data». The reason for its irrefutability is that the data, i.e. triangles, etc., are purely theoretical objects, created, as defined, by the theoretician, and of which, therefore, *all* the properties are known. Any apparent refutation would tautologically imply that the conflicting object was not an object of the premised kind in the first place. In fact, Pythagoras' theorem would be true, irrespective of whether there *existed* triangles in the world external to the

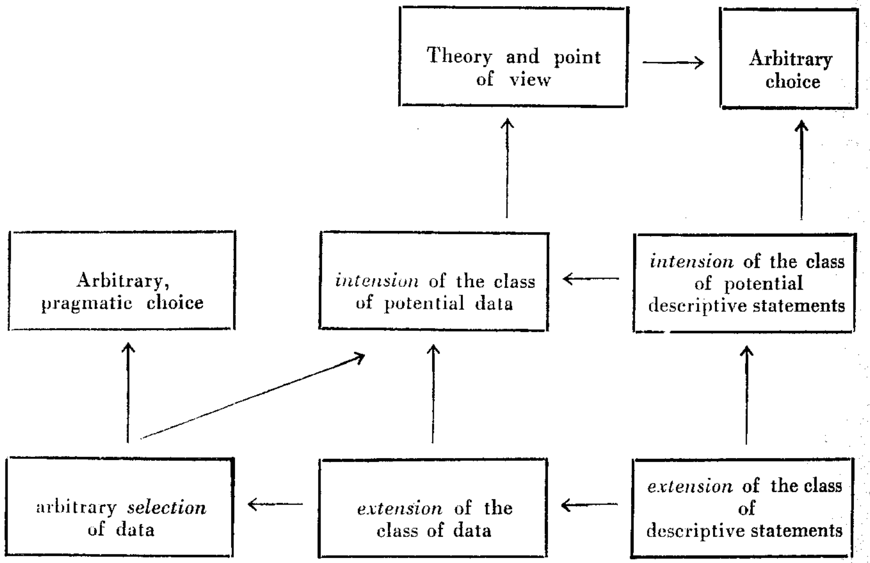
theory of Geometry in question. In that theory, the notion «triangle» has no existential import. Pythagoras' theorem is, therefore, a theoretical statement, not a descriptive one. It can however be used in structural descriptions of the Universe in terms of Geometry. The above example at the same time illustrates the ideal relation between theory and description in a structuralist, as opposed to a causal, or to a mainly taxonomic, science. A scientist who would advocate replacing the application of Geometry, in relevant descriptions, by inductive taxonomic procedures, has nowadays very little chance of being taken seriously. And if he proposed to replace it by speculation, people would only shrug their shoulders. Yet, the analogue of this is what Chomsky, and linguists influenced by him, have endeavoured to do in linguistics. We should not lose sight of the fact that, in spite of bringing «causation» into the picture, transformationalism, and even its variety called «generative semantics», have remained structuralist to a large extent. It is in this area too that — by substituting mere «intuition» for «discovery procedures» — speculation has replaced the inductive methods of the Bloomfieldians. Every step in a generative derivation is in essence axiomatic, or rather «dogmatic», and because, at least ultimately, existential import is claimed, the whole of a grammar is, therefore, speculative. The latter implies such existential claims. The Bloomfieldians are, compared with this, only minor offenders against proper scientific procedure. They may only be blamed for not being sufficiently aware of the fact that, especially in Europe, methods had been developed which should have rendered inductivism obsolete in respect of linguistics as a structuralist science. I have already argued that, whenever more exact procedures are feasible and available, they ought to replace inductivist procedures.

Before I leave the topic of inductivism, I should also like to say that inductivism leads to circularity and even Procrustean beds by logical necessity, Typically, inductivists relegate taxonomies, i.e. generalisations, together with generalising statements, i.e. «laws», especially when these are regarded as language-universal, to the realm of theory. But no matter how much one generalises on the basis of particulars, and how much one classifies recurring patterns under one heading, this does not change their ontological status. Consequently, laws, — or call them «rules» — and language-universals alike, are, or belong to the realms of *descriptive* statements, i.e. they are parts of descriptions, albeit sometimes of a more generalised nature than those pertaining to the description of a particular language. They are, then, at the most, *descriptive* statements about *several* or even — speculatively — about *all* languages. If one now relegates such a statement to the theory, as is done by Bloomfieldians, transformationalists and, in fact, most non-Saussurian linguists, such a theory has become automatically invalidated for use as an *instrument* in the description of languages. If it is so used, circularity, of course, results, or worse, observation of the data is prejudiced in a Procrustean fashion. The fact that such an invalid theory can still be used for prediction or for the generation of new data is not only unimportant, but it confirms what I have just said. In a proper scientific set-up, prediction or generation is done by *descriptive* generalisations on the basis of the theory, not by the theory itself. The role of the theory in this respect is that from it the form or matrix for descriptive statements can be derived, and it is, again, the theory that makes any descriptive statement scientifically meaningful. It is also the theory that allows us to ask meaningful questions about the data, and to formulate our observations of the data in a meaningful way.

I should like to mention one other, albeit very minor reason why a consciously inductivist way of setting up a description is methodologically inferior to adopting a more theoretical deductive approach. Even though it is, theoretically speaking, a minor reason, it canbe, from the point of view of actual practice, very important. It is, namely, the case that within inductive procedures it is virtually impossible not to run into inconsistencies. Of course the criterion of consistency between statements is, or ought to be, as important to an inductivist as it is to a deductivist. Only the latter, however, can maintain consistency without great difficulty. If one has to start by making observational statements, it is, perhaps, when, say, making statement number ten, still sufficiently easy to be sure that it is consistent with all previous statements. But, lacking structure between statements amassed additively, this

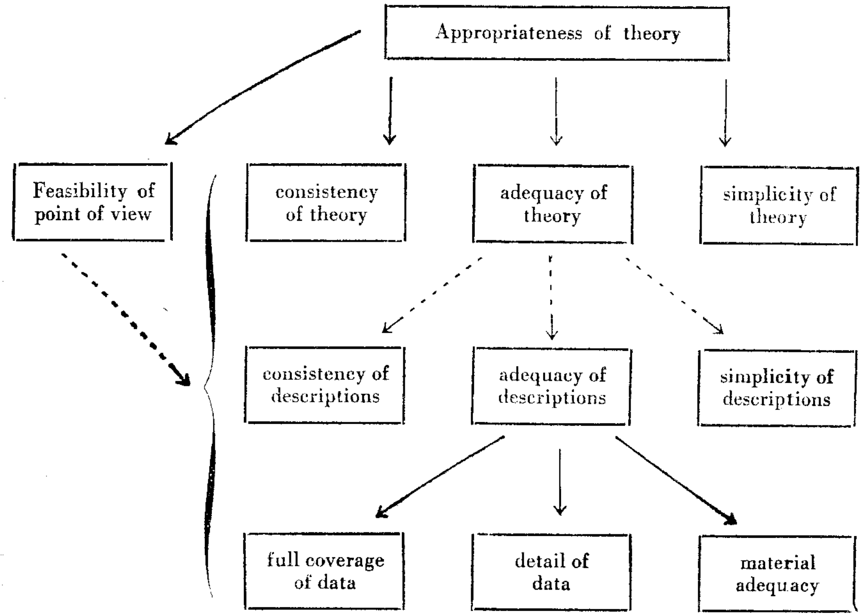
vigilance becomes increasingly more difficult the further the description progresses. It is as if one had a bag full of fleas which one attempted to take out one by one and hold in one's hand. Before long there will be more fleas escaping than those that remain caught.

Having rejected speculativism for a scientific descriptive approach, and having, in conformity with the scope of one's interest, which is structuralist in nature, selected the theoretical and hypothetico-deductive type of approach, rather than the inductive one, the investigator has now to select the point of view that has to be incorporated into the theory he is going to employ. There is an intimate relation between theory and point of view, so intimate, in fact, that actually in practice one may well take them to be one and the same thing, though a theory incorporates more than 'point of view' alone. Nevertheless, there is usually one all-pervading principle that characterises the whole approach. All the other principles are subordinate to it, or, at least, they are interpreted in terms of it. For functionalists this all-pervading principle, i.e. their *primary* point of view, is embodied in A. Martinet's dictum: «Function is the criterion of linguistic reality». (Martinet: *A functional view of language*). As one sees from the wording, such a theoretical «point of view» is a limiting factor in respect of the scope of both data and description. Saussure has said: «*C'est le point de vue qui crée l'objet*». (Saussure, *Cours*). On the theory and point of view depends the *intension* of the class of potential data, i.e. the *type* of data possible and relevant under that theory. Actually the term «language» precisely means that. «Language» is the *intension* of the class of all possible data under a particular linguistic theory of this kind, and the notion «language», therefore, emerges from the theory. In a similar way, those entities that we call «languages», i.e. particular languages, emerge from the relevant *descriptions*. Also the *intension* of the class of potential descriptive statements depends on the theory, but it also depends, of course, on the *intension* of the class of potential data. It is clear that the *extensions* of classes logically depend on their *intensions*, and that the *extension* of the class of actual descriptive statements depends on the *extension* of the class of actual data, the latter also depending on arbitrary, though not unmotivated, selection, which in its turn — this is a limiting constraint on the arbitrariness — depends on the *intension* of the classe of possible data. Part of the motivation for selecting a particular range or field of data, e.g. High German, Southern Standard English, Pekingese Chinese, etc., is of a purely pragmatic nature. The selection of a particular type of data withing such a field, with the exclusion of associated phenomena considerer irrelevant, is ultimately governed by theory and point of view. We can schematise this as follows, the arrows, from tail to point, to be read as «implies a particular» or «depends upon»:



One sees that under this philosophy of science, in an ideal descriptive and structuralist approach, which we assume Axiomatic Functionalism to be, everything — including the, albeit arbitrary (i.e. it could have been different), selection of data, ultimately depends (though of course not exclusively) on the theory and point of view, which are themselves arbitrarily selected. As Hjelmslev (Hjelmslev 1953) has said: the theory is both arbitrary and appropriate. The requirement of appropriateness safeguards theories from running loose, becoming wild, being vacuous, or, in general, being uselss

and being philosophically, especially logically speaking, unsound. It is «appropriateness» that *motivates* the, nevertheless *arbitrary*, selection of the theory in its entire extension. It is, therefore, in itself appropriate to state very clearly what I mean by this important requirement. It can be schematised as follows — the arrow to be read as «implies», the broken arrow as «implies potentially»:



One sees that the feasibility of the point of view itself depends on the *possibility* of satisfying, under such a point of view, all the other requirements. As I also, arbitrarily to be sure, stipulate that linguistic descriptions should not be speculative, I have to reject, *a priori*, the Chomskyan *point of view* as embodied in the relation between «competence» and «performance». One sees, furthermore, that, ultimately, the appropriateness of the theory fully depends on the possibility of an adequate coverage of the data, so this approach is no less *empirical* than any inductivist approach might be. In fact, in my opinion, the empirical principle is only fully realisable under an approach of this kind.

I should, perhaps, explain what I mean by «material adequacy». As the term «material» already suggest, it means: the consistency of descriptive statements with the data as observed. Inconsistency ment in question. That is, every descriptive statement ultimately stands or falls with its being materially adequate. Material adequacy has the last and decisive word in this approach, which emphasises again the fully empirical and realistic nature of the suggested methodology. However, as to the nature of the activity (and its result) called «observation», we have to realise two things: a) it is by very nature subjective and impressionistic; in linguistics, unlike in some natural sciences, we have no recourse even to observe the totality of an event rather than an aspect af this totality. That is to say, we simultaneously observe the wholelof a semtence utterance, not, say, just its syntactic or phonological aspect. methods of reducing this to a certain extent, and b) we actually observe the totality of an event rather than an aspec of this totality. That is to say, we simultaneously observe the whole of a sentence utterance, not, say, just its syntactic op phonological aspect. In fact, some aspects, including those just mentioned, cannot be isolated by purely observational means. The only aspects that can to an important extent be isolated within the event as such are its phonetic shape, and its communicative value, i.e. its «message». We may, therefore, say that the phonetic and semantic aspects have a geater protocolising potential than other aspects relevant to the functionalist have. Consequently, the material adequacy of descriptive statements related to other aspects can sometimes only be indirectly examined by simultaneous reference to the protocolising aspects. In practice this means that phonological statements have to be rejected if they conflict with phonetic observations and phonetic descriptive statements, or with consequences of these, whereas grammatical statements, e.g. about syntactic structure, have to be consistent with what we know to be, and have, perhaps, described as being, the meaning of the construction in question. If there is inconsistency, and one's protocolising observation and the ensuing statements (hypotheses) are sufficiently corroborated,

the statement in question, about not directly observable phenomena, has to be considered as being refuted by indirect reference to the data. It is, therefore, truly a matter of corroborating the material adequacy of one's statement, however indirect this may be. In this way, also one of the aspects of the requirement of internal consistency of the description as a whole is satisfied — which, is of course, a deductive, not a hypothetical, consideration —, but one should not be misled by this fact of «operational simultaneity», into thinking that we are testing here the meta-hypothesis of consistency, rather than that of the material adequacy of a descriptive statement (hypothesis). We are, in fact, doing both at the same time. I should like to stress that this is a very frequently applied consideration, especially in syntactic description, i.e. the above considerations are not purely academic. One should also not be misled into thinking that, in this way, semantic or phonetic arguments are let in the back door. They are not used as *arguments in the deduction*, only in the «adequation», i.e. the *testing* of hypotheses.

The French philosopher Descartes long ago advocated not to do what transformationalists do (i.e. give one's approach a monolithic structure), but advised one to divide every problem into as many subordinate ones as possible. If one has a multi-level approach, as in Axiomatic Functionalism, great simplicity ensues from the fact that on each of the levels one can take for granted, and one need not burden oneself with, what has already been solved or is easier soluble at another level. For instance the output of phonetics is the input for phonotactics, and on the latter level one has no longer to concern oneself with an analysis into distinctive features. And on having noted, under the heading of «allophony», the variant phonetic forms of phonological entities, one can leave those out of further consideration. A similar operational economy ensues by dividing *grammar* up into different areas. But also within the theory as a whole one can make a division as to the *type* of problem we are dealing with, and so come to a number of sub-divisions. As I have indicated already, part of the structuralist linguist's activity leads to the establishment of theoretical or descriptive notions which are called «models». For the linguist these «models» are *objects,* onto which, in the case of *descriptive* models, but also indirectly so in the case of *theoretical* models (meta-models), speech-phenomena can be mapped. These models stand, as it were, for speech-phenomena, and can, unlike the speech-phenomena themselves, be manipulated by the linguist, who acts as if actual or potential speech-phenomena were instantiations or realisations of those models. So we may say that the linguist is constantly dealing with «linguistic objects», especially phonological ones and grammatical ones. Due to historical development, the structuralist linguist is usually in the first place interested in the *internal* deployment of those objects, i.e. in their analyticity and combinability, and connected issues. This has led to what in some schools has constitued the whole of linguistics, but which I regard as one of the three major sub-disciplines or sub-theories. It is in Axiomatic Functionalism itself sub-divided again into phonology and grammar, and the former is further divided into phonematics, phonotactics, and para-phonotactics, and grammar into morphology, syntax, and para-syntax. Strongly connected with this are the areas of phonetics, allophony, and allomorphy, (Mulder, Postulates; Mulder 1975). I have called this subdiscipline: systemology (Mulder 1975). But apart from this, linguists are, or they should be, also interested in the *external* deployment, i.e. the actual meaningful use in communication, of linguistic objects whose significance goes beyond their mere form. It is the sub-discipline of *Semantics* that deals with this area. But without a precise and explicit awareness of the relative ontological status of the linguistic objects we are dealing with, and without a clear insight into their nature, we would, in effect, still be groping in the dark, no matter in what sophisticated way our systemology and semantics had been set up. In Axiomatic Functionalism it is the Sign-theory, or rather *Signum*-theory, that has been established in order to deal with the ontological aspect of our quest for knowledge concerning communication by means of language. Of course there are wider, and, perhaps, very interesting, horizons beyond Sign-theory, Systemology, and Semantics, as such, but those do not fall within what functionalists would considerer *core*-linguistics.

Apart from enhancing descriptive simplicity, the step by step, level by level, and type of problem by type of problem strategy also leads to a greater precision in one's statements, and, consequently, in the whole of the calculus. Having a sound philosophical foundation for one's approach — though of overriding importance — is not sufficient in practice. It will amount to not paying more than lip-service to ontological and other categorial considerations, if the theory does not cater for the finest distinctions possible in that respect. To give only a handful of exemples: not consistently distinguishing between *the* expression of a sign and its one or more variants (allomorphs), between the latter and their phonological forms, between phonological forms, allophones, and the phonetic forms of the latter, between items qua sign and items (perhaps the same) qua grammatical entity, between functionally different, though perhaps physically simultaneous, prosodic features, (Martinet 1954; Mulder, Postulates), between sign (type) and utterance (token), between the denotation of a sign (which is a «correspondence» relation) and the denoting by an utteranace (which is an «act»), between denotation and other aspects of meaning (Hervey, Postulates, and Mulder and Hervey 1972), and so on, unavoidably will lead to imprecision.

Functionalism, in my opinion, has all the ingredients for scientific solidity, but it is up to the functionalist to use those ingredients to best advantage. Functionalists have not been unduly perturbed by the worldwide «cultural revolution» of the nineteen-sixties (wich spilled over into the early seventies), which was by no means confined to linguistics. By remaining unimpressed by empty slogans they have been able to preserve a great deal of the Saussurian heritage. Now the tremors of the transformationalist eruption are gradually dying away, it is opportune not only to contemplate the intrinsic value of our approach, but also to build up our strategy in the most sophisticated ways possible. This involves, as I have argued, basing oneself on sound philosophical and methodological principles, and developing our theories in such a way that no one can dispute their appropriateness. The task ahead is now to re-assert ourselves, and return linguistic debate to the real issues that have been left in abeyance for too long.

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