

# Analytical Study of Modern Arabic Linguistic Terms

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## Introduction :

The main purpose of this paper is to survey major efforts to confront the use of modern Arabic linguistic terminology.

The major factors which affect modern linguistic studies in the Arab world are mentioned. Basically, they fall into two major categories : indirect and direct factors. The first type stems from the fact that modern linguistics is mainly a western product. During the colonial era, western political powers tried to replace classical Arabic with several dialects. Therefore, they supported various local and foreign attempts to do that.

This environment did not help the western born science grow in normal soil.

An example of a direct factor is the languages of the sources of linguistics. English, French and German are the main languages in which Arabs study linguistics. The main scholars in the field of linguistics received their training in one of these languages. I. Anis, A. Ayyub, A. Wafi, A. Frayha, T. Hassan and K. Bishr studied in English or American schools. An-Najjar, Hijazi, A. ar-Rajih, R. Abd-at-Tawwab and others have German training. North African and many well-known Middle Eastern linguists received French training. These different backgrounds had both positive and negative effects on Arabic linguistic terminology.

Arabic writers used various means to deal with the problems presented by linguistic terms. In early works,

some writers tried to explain the new terms while others supplied their books with glossaries. In the last ten to fifteen years, a limited number of dictionaries have been published. In this paper, there are comparative samples from major works such as the dictionaries of al-Khouli, Bakalla, Al-Hamzawi and al-Musaddi.

## Arabic : Its Linguistic and Cultural Nature

It is natural and normal for any human community to name newly invented things and to use language to express understanding of life's experiences. As a language, Arabic reflects the experiences of its society. The matter of terminology is neither an old nor a new subject, but one of every age, with its own circumstances and conditions. The story of the major schools of Arabic traditional linguistics is well known. Al-Basia had different ways in forming rules, conducting research and in choosing terms from Kufa School. Translation is a well-known factor in the transfer of terminology throughout all periods of time, beginning with the famous Abbasid time and continuing until now. Most modern Arabic academies in the Arab world are strongly involved in the problem of terminology.

Interrelations among cultures make up one of the major features of human life. These relations happen in various ways and leave many marks on languages, societies, and cultures. For a while during the medieval period, Arabic represented a sound and amazing culture and influenced many other languages, while the Arab

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inadequacy of the theory in axiomatic functionalism is, it seems, well-nigh impossible. Showing that a given description on the basis of the theory can be bettered, in terms of adequacy, by another description, with the same scope, on the basis of the same theory, is, of course, possible, though this would be regarded as nothing more than a demonstration that the former description is inadequate. Showing that a good description on the basis of the theory can be bettered by another description, with a wider scope, provided on the basis of a different theory, which, in principle, would constitute a stronger test of adequacy than the afore-mentioned one, showing this, while sticking to the Mulderian interpretation of adequacy, is an almost impossible task because it requires the researcher to commit the methodologically unwarranted step of going outside the domain laid by axiomatic functionalism. No wonder that, as far as adequacy with respect to the theory is concerned, axiomatic functionalists are proudly impressed by the 'resilience' of their theory, by its almost 'impregnable' character. It is, however, an 'impregnability' that is closely akin, if not perhaps identical with, immunisation: both appear to be of the same 'pedigree'.

Before closing this discussion, a final word on conventionalism is in order here. It is generally accepted by philosophers of science, including Popper, that conventionalism may be 'safely' utilised as a methodological framework in, particularly, the mature and exact sciences when theories in such sciences are in a settled state and are widely accepted by workers in the field, and when there are no 'awkward' examples facing these theories. In times of upheaval, however, conventionalism must be abandoned because of its tendency to provide its adherents with the logical armoury by means of which they can ward-off any 'legitimate' challenge to

conventionalist-based theories, thus impeding the progress of science. Linguistics is neither a mature and exact science, nor are its theories in a settled state and, either implicitly or explicitly, sufficiently widely accepted by the community of scholars who call themselves linguists. If anything, upheaval characterises the recent history of linguistics and predominates its present state. Applying conventionalism in linguistics seems, therefore, to be both pre-mature and out of historical 'context'.

## 5. Conclusion :

The preceding discussion shows that none of the philosophies of science dealt with in this article is devoid of serious problems. This must reflect adversely on the claim that linguistics is a 'science', albeit an emerging one. Generally speaking, linguists have either largely ignored any serious consideration of the scientific foundations of their theoretical frameworks and the descriptive studies based on them, or they have allowed themselves to indulge in what seems to be over-hasty theorising concerning the scientific foundations of their linguistic theories. To the present writer, the latter tendency is more dangerous than the former one: it creates 'mythologies' which often prove difficult to dismantle, and, more often than not, it leads to dangerous dogmatism, a dogmatism couched in apparently 'sound' terminological currency. A careful and 'scrupulous' working out of the scientific principles of modern linguistics and its theories is long over due. The present article does not, and is not intended to, meet this challenge. Its aim is a modest one: it is to sign-post some of the debris lying in the path of any future attempt designed to meet this challenge. A similar attempt in the field of epistemology is sorely needed.

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which may be based on the situation outlined above unimportant and negligible from the practical point of view. Such an argument, however, cannot be seriously entertained, since if a situation similar to the one described above were to hold for an inductively based or a modelling based theory, then this would be regarded by the proponents of these two approaches as a refutation of the theory itself - due to the fact that a theory of any of the above two types is said to have an empirical content - and also of its applicability as a descriptive tool. In contradistinction, axiomatic functionalism would not, strictly speaking, be seriously affected by such a situation. If such a situation were to obtain at any time in the future, axiomatic functionalists would, in principle, be able to ignore it and to continue in their espousal of and commitment to their theory as though nothing had happened. For those who either implicitly or explicitly adhere to it, the inner logic of conventionalism offers the possibility of such a stance. For non-conventionalists, such a stance has all the hallmarks of an immunisation strategy.

Let us tackle the same point from a slightly different angle by considering the criterion of adequacy with respect to the theory. In axiomatic functionalism, a theory is said to be adequate if it can be shown to be capable of yielding consistent, adequate and simple descriptions of the objects which lie within its own scope. As far as the empirical nature of the theory as a descriptive tool is concerned, the criterion of adequacy with respect to descriptions is the most important criterion of the set of criteria for assessing descriptions mentioned above. In what follows, I shall briefly concentrate on this criterion with a view to showing that, from the point of view of empirical testing, it leaves a great deal to be desired.

According to Mulder, adequacy with respect to descriptions involves the following three considerations: 'full-coverage' of data, 'detail of data' and 'material adequacy'. 'Full-coverage' of data pertains to matters of 'width' in descriptions, while 'detail of data' pertains to matters of depth of analysis in descriptions. 'Material adequacy' means the «consistency of

descriptive statements with the data as observed» (Mulder in Mulder and Hervey, 1980 : 12).

The application of the above adequacy considerations is very tightly controlled by the theory as a whole, including of course, its criterion of relevance, theorems of scope and its underlying conventionalist methodological foundations. The fact that descriptions based on the theory cannot deal with 'linguistic' data which do not fall within the scope of the theory and its criterion of relevance does not, due to the conventionalist nature of the theory, render the descriptions concerned inadequate, nor, by implication, does it render the theory, albeit indirectly, inadequate either. Likewise, the fact that 'deeper' analyses than those which can be secured on the basis of the theory may be shown to be attainable for linguistic data that lie within the scope of the theory, cannot, due to the conventionalist nature of the theory, be considered as a demonstration of the inadequacy of the description of such data on the basis of the theory, and / or, indirectly, of the inadequacy of the theory as a descriptive tool. A somewhat similar situation holds for 'material adequacy'. Since observation, both the activity and its results, are, under axiomatic functionalism, theory-laden, and since only data that lie within the scope of the theory constitute proper objects for description under that theory, and, finally, since descriptive statements must, among other things, satisfy the descriptive adequacy criteria of 'full-coverage' of data and 'detail of data', it automatically follows that nothing from outside the domain set by the above parameters can, under the conventionalist philosophy of axiomatic functionalism, be brought to bear on any attempt to assess the adequacy of descriptions and, indirectly, the theory they presuppose. In axiomatic functionalism, the theory, it seems, may be shown to be inadequate if, for at least one given set of linguistic data which fall within the scope of the theory, it can be conclusively demonstrated that no linguistic description for the set in question can be provided on the basis of the theory. Needless to say, this is a very weak test of adequacy, and considering the above limitations on the criterion of adequacy with respect to descriptions, the task of demonstrating the

recognising the important role played by «our actions and operations, planned in accordance with deductive reasoning, in conducting and interpreting scientific experiments» (Popper 1959 : 80). Secondly, conventionalism stresses the importance of consistency as a crucial requirement placed on all theoretical systems.

However, conventionalism suffers from one short-coming whose disadvantages far out-weigh the afore-mentioned advantages of this view : it comes dangerously close to rendering the theories based on it, in their capacity as descriptive instruments, susceptible to immunisation, thus effectively resulting in the obstruction of scientific growth. This feature of conventionalism and conventionalist theories is inextricably linked to the fact that, under this approach, «whatever was found not to conform [i.e. not to fall within the scope of the theory] to a [theory] would simply not come under the [theory] ;and whatever was not in accordance with [a] definition would not be what the definition defined» (Harris 1970 : 53). Workers applying conventionalist theories are, invariably, impressed by what appears to them as the resilience of their theories in the face of attempted refutation of their appropriateness as descriptive instruments. What, however, they fail to realise is that this resilience is more apparent than real, since it does not necessarily follow from any intrinsic strength of the theory as a descriptive tool, but, more often than not, from the fact that the theory they are applying is of a conventionalist nature, and, therefore, is susceptible to immunisation as a result of employing the strategy embodied in the above quotation from Harris. This is, of course, subject to the condition that the set of 'objects' that can, both in principle and, perhaps, in practice, be described by reference to the theory is not an empty set.

Let us illustrate this situation by considering Mulder's axiomatic functionalist theory. Mulder (Mulder and Hervey, 1980 : 41) defines language as a «semiotic system with double articulation». Like all other axiomatic functionalist definitions, this definition is of the stipulative type. By stipulative definition is meant the «explicit, self-conscious and arbitrary setting-up

of a meaning relation between a given definiendum and its definiens» (Suleiman 1983 : 445). The following quotation from Copi (1978 : 137 - 38) further explains the nature of stipulative definitions :

«A symbol defined by stipulative definition did not have that meaning prior to being given it by the definition. Hence, it cannot be regarded as a statement or report that the definiendum and the definiens have the same meaning. They actually have the same meaning for anyone who accepts the definition, but that is something which follows the definition rather than a fact asserted by it. A stipulative definition is neither empirically true nor false but should be regarded as a proposal or resolution to use the definiendum to mean what is meant by the definiens, or a request or a command. In this sense a stipulative definition is directive rather than informative. Proposals may be rejected, resolutions violated, requests refused, commands disobeyed, and stipulations ignored, but none of them on that account is true or false. So it is with stipulative definitions».

Given the stipulative and conventionalist nature of Mulder's definition of language, it automatically follows that if one were to come across a language, in the normal sense of the term, which did not exhibit the property of 'double articulation', then the language concerned would not be regarded as a language in the Mulderian sense, and, therefore, it would not fall within the descriptive scope of axiomatic functionalism. What is interesting about this would-be state of affairs is that it would neither imply that the language concerned was not a language in the normal sense of the term, nor would it be considered as a refutation of the appropriateness of the theory. Put differently, the status of the language concerned as a language in the normal sense of the term, and the appropriateness of the theory as a descriptive tool would remain completely unaffected by the situation we have sketched out above. It may, perhaps, be argued that the likelihood of such a situation occurring or being encountered in the linguistic world is infinitesimal if not, actually, quite improbable, thus rendering any conclusions

ciple can be carried out, and yet scientists continue to utilise this principle in the course of their relevant scientific investigations. As Losee (*ibid.*) points out, this led Poincaré to conclude that the generalised inertial principle «can be, and is, used as a convention which implicitly defines the phrase 'inertial motion' [as] motion of a body such that its acceleration depends only on its position and the positions and velocities of neighbouring bodies» (*ibid.*).

By the same token, we can say that Mulder's first axiom (1980 : 41) : «All features in semiotic sets are functional» is *a priori* accepted as a core principle by those linguists who adhere to axiomatic functionalism and apply it as a descriptive tool. This axiom can neither be refuted nor corroborated by confronting it, either directly or indirectly, with counter examples from the world of phenomena. In other words, this axiom is devoid of any factual content, and, therefore, cannot be empirically tested in the normal way in which empirically-based scientific laws can. This amounts to saying that Mulder's first axiom is a 'convention' which is adhered to by the 'community' of practising axiomatic functionalists. These linguists justify their acceptance of the afore-mentioned 'convention' by reference to its status as an integral part in an over-all theory which claims to be able to serve as an effective descriptive tool.

In their capacity as conventions, scientific laws, as well as the theoretical frameworks of which they are parts, are not open to empirical testing. Put differently, such laws and theories cannot be refuted by empirical evidence since they, in principle, cannot be shown to be empirically false. As conventions, scientific laws are *a priori* accepted as 'true' by definition or 'valid'. This property of scientific laws according to conventionalism may be appropriately labelled as their 'arbitrariness', in the Hjelmslevian, and not the lexical meaning, of this term.

To continue the present discussion in a Hjelmslevian vein, we may say that the arbitrariness of scientific laws under the conventionalism, is, implicitly, said to be tempered by their appropriateness. According to Hjelmslev (1969 : 14), the appropriateness of a given

theory means that the theory concerned «introduces certain premisses concerning which the theoretician knows from preceding experience that they fulfil the conditions for application to certain empirical data». This is more or less what Poincaré appears to have in mind in his more 'poetic' pronouncements concerning the nature of scientific laws : «our laws are...like those of an absolute monarch, who is wise and consults his council of state» (1952 : xiii) ; and : «The framework into which we wish to make everything fit is our own construction ; but we did not construct it at random, we constructed it by measurement so to speak ; and that is why we can fit the facts into it without altering their essential qualities» (*ibid.* : xxv).

Both glossematics and axiomatic functionalism adhere, albeit implicitly, to the chief tenets of conventionalism. This is clear from their espousal of the view, adopted in conventionalism, that science lacks a logic of scientific discovery, in the sense of a procedure for generating laws and theories. It is also clear from the fact that the above-mentioned linguistic theories advance the view, though only implicitly, that theoretical constructs are *a priori* accepted as 'valid'. More importantly, however, the conventionalist view that theories are both arbitrary and appropriate finds its fullest and neatest expression in axiomatic functionalism and, particularly, glossematics. The axioms of axiomatic functionalism, the premisses of glossematics and the definitions in each one of these theories are 'conventions' which, though they cannot be empirically tested, are hypothesised to fulfil the condition of being applicable to the data which fall within their scopes.

#### 4.2. Conventionalism : A Critique

Harris (1970 : 53) points out that Popper objects to conventionalism «because it is not compatible with the scientific ideal of devotion to truth» Nevertheless, Popper still regards conventionalism as a logically defensible philosophy of science that is worthy of credit on two grounds. Firstly, conventionalism presents a sound explanation of the nature of the relation between theory, on the one hand, and experiment and observation, on the other, by duly

matics in the area of philosophy of science is far too great to be regarded as accidental. In what follows, I shall deal mainly with Mulder's axiomatic functionalism, as this theory presents a more elaborate statement of its metatheoretical foundations than Hjelm'slev's glossematics does. However, prior to that it is important to clarify a certain point which strikes the present writer as essentially a problem of nomenclature.

Mulder characterises his theory, in terms of its underlying meta-theoretical principles, as a hypothetico-deductive theory. Hypothetico-deductivism in the Mulderian sense is fundamentally different from hypothetico-deductivism in its well-established Popperian sense. According to Popper (1959, 1969), the theory is both deductive and hypothetical. It is deductive in the sense that it can be used, ultimately, to deduce an unlimited number of singular statements concerning the universe of phenomena which lies within its own scope ; and, it is hypothetical in the sense that it is open to empirical testing and refutation by confronting it with states of affairs from that scope. In the Mulderian sense, the terms 'hypothetical' and 'deductive' are uniquely assigned to the descriptions and the theory upon which the descriptions are based, respectively. The descriptions are said to be hypothetical in the Popperian sense, while the theory is merely a deductive calculus or an 'inference ticket' containing no «existence postulate». This property of the theory, referred to as its arbitrariness, is neatly expressed by Hjelm'slev (1969 : 14) in the following fashion :

«A theory, in our sense, is in itself independent of any experience. In itself, it says nothing at all about the possibility of its application and relation to empirical data. It includes no existence postulate. It constitutes what has been called a purely deductive system, in the sense that it may be so used alone to compute the possibilities that follow from its premisses».

However, the arbitrariness of the theory is tempered by its appropriateness, i.e. by its ability to function as an instrument for yielding descriptions of empirical data of the type that lie within the domain of the theory, which descrip-

tions must satisfy certain evaluation criteria. Clearly, the theory achieves its empirical character indirectly via the descriptions based upon it, though this empiricism is of a different sort from that predicated of a proper Popperian-type theory. Saying that the theory is appropriate in the above sense does not, however, imply that the empirical data can strengthen or weaken the theory itself as a deductive calculus.

The discrepancy between Mulder's use of the term 'hypothetico-deductivism' and the well-established Popperian one cannot, strictly speaking, be construed as a criticism of Mulder's linguistic theory, for the very simple reason that Mulder's theory is not intended to be hypothetico-deductive in the Popperian sense. From a less formal point of view, however, Mulder may be criticised for upsetting well-established terminology, particularly as there is in the relevant literature a well-established label to designate the philosophy of science which underlies his theory, namely 'conventionalism'.

The originator and chief proponent of conventionalism in the field of philosophy of science is the French scientist and philosopher of science Henri Poincaré. Conventionalism shares with the modelling view its rejection of the inductivist premise that science has available to it a logic of scientific discovery. This rejection implies that conventionalists regard questions as to how theories or scientific laws are arrived at as pseudo-questions. As Poincaré states (1952 : xxiii), scientific laws are «the result of the unrestricted activity of the mind, which in this domain recognises no obstacle».

As its name indicates, the main thesis of conventionalism is that scientific laws essentially have the status of 'conventions' or stipulative definitions which specify the 'meanings' of scientific concepts ; they are, in Poincaré's (ibid : xxii) own words, «definitions or conventions in disguise». The generalised inertial principle which states that the «acceleration of a body depends only on its position, and on the positions and velocities of neighbouring bodies» (Losee, 1972 : 173) is just one such law. Poincaré observed that no decisive test of this prin-

relevant from the point of view of the philosophy of science, though it, undeniably, may be very interesting from the point of view of the psychology of science. Moreover, if the said expectation is of a psychological nature, then the above thesis of the modelling approach seems to rest on something approaching either 'faith' or 'animal habit'. Consequently, to linguists, linguist-philosophers and philosopher-linguists who adhere, whether implicitly or explicitly, to a rationalist view of the nature of science and scientific progress, the thesis of the modelling approach would appear to be seriously misguided.

If, however, the expectation Lockwood has in mind is of a logical nature, then this expectation must be a matter of probability. Now, since the model and the «unobservable system» are 'structurally finite', and since their 'behaviour' characterised in terms of the utterances or texts they can generate is infinite, it follows that the probability of the internal workings of the model corresponding to those of the internal structure of the «unobservable system», computed on the basis of any observed correspondence between the texts or sentences generated by these two objects, must, under any standard probability theory, be always zero. In addition to this, if we consider the internal workings of the model and the internal workings of the unobserved system as antecedents, and if we further consider the texts or sentences they generate as their respective consequences, then we may conclude that by trying to establish correspondence of an empirical nature between the model and the «unobservable system» on the basis of observed correspondence between their outputs, the above thesis advocated by the adherents of the modelling view in linguistics suffers from the logical fallacy of asserting the consequent.

The fallacy of asserting the consequent decrees that since it is possible, from the logical point of view, to derive factually true statements from demonstrably false premisses, it is, therefore, not valid to argue from the truth of the consequence to the truth of the antecedents from which the consequence is derived. In the following deductively valid argument, the factual

truth of the conclusion cannot be transmitted to the premisses, since the premisses concerned are demonstrably empirically false :

Premisses : All transformationalists are Americans

All Americans are Linguists

Conclusion : All transformationalists are linguists.

Finally, it is highly unlikely that the modelling view can apply with the same effectiveness in the domain of the social sciences as it does in the domain of the natural sciences in which it has been most successfully applied. The main reason behind this is not so much the presumed complexity of the subject-matters of the social sciences, linguistics included - for one would not, due to conceptual and methodological limitations, be able to demonstrate easily and unequivocally, that the 'typical' subject-matter of a given social science is more complex than that of a given natural science - but the 'nature' of the 'material' of the social sciences *vis-à-vis* the requirement of exact testing, which is the hallmark of the natural sciences. Exact testing in the field of the social sciences is more often than not fraught with considerable problems pertaining to the identification of the precise range of the relevant and operative variables in any testing set-up, and, more importantly, to the task of devising satisfactory techniques for controlling all the variables in the testing set-up concerned, except for the single variable to be tested.

#### 4. Conventionalism :

##### 4.1. The Salient Features of Conventionalism

Of the three philosophies of science dealt with in this article the conventionalist approach is the least represented in modern linguistics. To the best of my knowledge, the only proponents of conventionalism in linguistics are Hjelmslev's glossematics (Hjelmslev, 1969) and Mulder's axiomatic functionalism (Mulder and Hervey, 1980). It is perhaps worth pointing out here, in the nature of an aside, that the similarity between the latter theory and Hjelmslev's glosse-

- of the observable behaviour, or manifestation, of the latent system in speech events ;
- (d) the descriptive model is of a hypothetical nature ;
  - (e) the closer the behaviour of the model - in predicting or generating sentences of the language it purports to describe - is to the linguistic system it pre-supposes «the closer the internal workings of the model can be expected to correspond to the internal structure of the actual system» (ibid.) ; the role of analogy is very clear here.
  - (f) of two «equally workable alternate models» of one and the same «unobservable system», constructed in accordance with the principles and procedures of the same theory, the simpler model is to be preferred. It is worth pointing out here that transformational generative grammar and stratificational grammar propose different interpretations of the simplicity criterion ; this, however, has no bearing on the discussion here.

### 3.2. The Modelling View : A Critique

The modelling view is superior to inductivism in one very important respect, namely in that it regards as misguided the inductivist tenet that there exists a logic scientific discovery which can be successfully and repeatedly utilised by the scientist in his conduct of his scientific investigations. In their publications, Chomsky and Lamb are constantly at pains to point out that in setting up a linguistic theory, or a model of a particular language, the linguist may, and, actually, does, utilise a host of disparate factors, such as his hunches, intuitions, experience with linguistic data, observations of such data and any methodological insights concerning techniques of theory construction and testing, etc. As such, a linguistic theory or / and model cannot be said to be **logically derived** from any source or origin. Consequently, any questions as to the logical source or origin of such constructs must, strictly speaking, be pronounced as 'scientifically meaningless'.

However, the modelling view is not without its serious short-comings. The basic assumption in this view according to which language, in the

pre-descriptive sense of the term, exists as a **system in the brain** is, from the viewpoint of testability, unwarranted. This springs from the fact that since language as a cognitive phenomenon is inaccessible to either direct or non-direct observation, it readily follows that the above modelling view can neither be asserted nor denied without recourse to metaphysicalism. Even if it was the case that harmless observational techniques were available to the modelling linguist, it would still be the case that such a linguist would not know what, exactly, to look for in the human brain ; nor would he be able to observe anything that he would, with the rest of the linguistic community, recognise as a language or 'language'. Moreover, statements to the effect that 'human language is a system' are, strictly speaking, 'scientifically meaningful' only in the context of language as a post-descriptive construct embodied in a model. By imputing to language in the pre-descriptive sense properties that are properly applicable only to language in the post-descriptive sense, and by failing to provide a reasonably reliable methodology for testing the empirical truth of the hypothesis that language in the post-descriptive sense, i.e. the model, is a 'true' description of language in the pre-descriptive sense, i.e. language as a cognitively real object whose seat is in the brain, the proponents of the modelling view betray their ideal of providing a **truly hypothetical** model of the object whose aim it is to describe and understand.

Equally indefensible is the very thesis of the analogical view in linguistics, stated by Lockwood (ibid.) in this manner : «the more closely the behaviour of the 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» [emphasis ours]. To begin with, it is not at all clear whether the expectation Lockwood has in mind is of a psychological or logical nature. Putting this problem of vagueness aside, we may proceed to say that if the expectation Lockwood has in mind is of a psychological nature, then we are faced with a situation which, following Feigl (1965), is not

method has two inter-connected aspects : (a) if two objects are alike, or, more precisely, can be shown to be alike, in some respects, then it is reasonable to expect that they will be alike in other respects ; and (b) the more resemblance there is, or can be established, between the two terms of an analogy, the stronger the expectation that there will be further resemblance between the objects involved in the analogy concerned, i.e. the stronger the argument from analogy. Trusted (1979 : 10) expresses the latter point as follows : «In general we may say that the closer the established analogy, the greater the confidence that there is yet further analogy, and the stronger the argument from analogy». Different types of analogy are employed in the modelling view, for example positive analogy, negative analogy and neutral analogy ; it is, however, not my intention to deal with these different types here.

To analyse analogical relationships closely and coherently, the concept of a model is normally employed. In science, interest centres on so-called 'real' or 'iconic' models. An iconic model is «some real or imagined thing or process which [is] similar to [another] thing or process in various ways, and whose function is to further our understanding» (Harre 1978 : 174). With respect to their function, iconic models serve the epistemological goal of extending our knowledge or understanding of the universe of phenomena. In other words, iconic models, primarily, have an explanatory, rather than just a heuristic function in science. In the following few paragraphs, I shall outline the manner in which the modelling view is interpreted or applied in the two linguistic approaches mentioned above. It is, however, important to point out here that a certain degree of generality is inevitable in the following discussion, due to the vast nature of the relevant literature.

In justifying their adoption of the modelling view, both transformational generative grammar and stratificational grammar start from the assumption that human language is a cognitive phenomenon, albeit that the former views language as a psycholinguistic phenomenon while the latter regards it as a neurolinguis-

tic one. As such, human language can neither directly nor non-directly be usefully observed with the aid of the observational techniques available to us at the present time. Neither direct observation (opening people's heads) nor non-direct observation (employment of powerful but harmful and destructive X-ray techniques) can be resorted to because they damage or seriously impair the human brain in which the object of investigation, i.e. human language, is said to reside. Now, due to the cognitive nature of human language, and, also, due to the aforementioned limitations on the observational techniques of linguistic inquiry, it is argued that an 'indirect' method of investigating human language via its manifestations in human speech is imposed on the linguist, namely the modelling or analogical view. To the best of my knowledge, the clearest statement outlining the mode of applicability of this view in linguistics is given by Lockwood (1972 : 4) :

«This [view] is implemented by constructing hypothetical systems known as models. In constructing these models, we try to make their behaviour parallel the behaviour of the unobservable system as closely as possible. Underlying this method of investigation is the assumption that the more closely the behaviour of a model approximates that of the unobservable system of investigation, the closer the internal workings of the model can be expected to correspond to the internal structure of the internal system».

The possibility of two or more «equally workable alternate» models for one the same «unobservable system» is not ruled out by the proponents of the modelling view. In such cases, the choice between the competing models is decided on the basis of a criterion of simplicity. To sum up, the main features of the modelling view in linguistic inquiry may be stated in the following, albeit very general, manner :

- (a) 'human language' exists as a system in the brain ;
- (b) this system can neither directly nor non-directly be observed ;
- (c) in describing this system the linguist sets up a model of this system mainly on the basis

answer to this question can be provided. Furthermore, one may ask if it is really possible, both in theory and in practice, to give a proper delimitation of the width of the data-base antecedating a given generalisation in inductivism, considering the fact that the extension-of the observational field to which the data-base belongs is, barring enumerative induction, infinite, without recourse to something in the nature of a theory. Taking note of the non-controversial view that there is more to seeing and observation than meets the eye-ball (Hanson : 1972, 1975), it would be safe to assume that the inductivist must invoke something in the nature of a theory in delimiting the extension of the observational fields ante-dating his inductive generalisations. But such a recourse to 'theory' would, from the inductivist point of view, be self-defeating : it rests on the very basis which inductivism, explicitly, denies.

The inductivist view that scientific knowledge is proven knowledge, and that scientific growth consists in the never-ending process of piling-up proven facts in an additive manner are both historically and 'materially' unsound. Studies in the history of science amply demonstrate that seemingly proven knowledge may turn out to be empirically false. Furthermore, progress in science cannot be fully accounted for without due consideration to the significant role played by advances in such fields as methods of theory construction and testing, and, also, frameworks for the resolution of thorny conceptual problems. By emphasising empirical growth in scientific progress, inductivism comes dangerously close to promulgating a distorted picture of this progress.

Finally, due to the inductivists' intense concentration on the inductive, theory generating stage in their exposition of their view of the nature of scientific inquiry, they detract from the importance of the deductive and, particularly, testing stages in their approach. It would, of course, be unfair, if not absurd, to criticise, inductivism for what it advocates and adherents do in its name, but what this view of science can be legitimately criticised for is the fact that it wrongly suggests that there is a precise method

for generating scientific theories and laws. This suggestion is historically unsound because it fails to explain why scientists who made breathtaking scientific discoveries often failed to repeat their spectacular past successes, despite the fact that they unremittingly devoted the rest of their lives to their chosen fields in science. The problem of how scientific discoveries, including theories, are arrived at is, undeniably, an interesting one ; however, it is a problem of interest to the psychologist of science and not, strictly speaking, to the philosopher of science.

My aim in the preceding discussion has been to show the untenability of the inductivist view of the nature of science and the method of scientific inquiry. The attempt to demonstrate this point has been carried out by reference to logical, historical and descriptive considerations, the intention behind this being to suggest that those linguistic approaches which apply, or base themselves on, inductivism are subject to the criticisms of this philosophy of science mentioned above. Inductivism presents a deceptively simple and straight-forward view of science, the activity and its results, but, as has been shown above, this view is fraught with intractable problems of various kinds. These problems are, willy-nilly, transmitted to those theories or schools in linguistics which presuppose inductivism.

### 3. The Modelling View :

#### 3.1. The Salient Features of the Modelling View

The chief adherents of the modelling view in linguistics are Chomsky's transformational generative grammar (Chomsky 1965, 1972 ; Katz, 1964) and Lamb's stratificational grammar (Lamb 1966). Reasoning from analogy is the principal thesis of this view. By its very nature, analogy always involves a relationship between two objects in the widest sense of the term. As such, it «allows inferences to be made about one of the things, usually that about which we know least, on the basis of what we know least, on the basis of what we know about the other» (Harré 1978 : 172). The assumption underlying this chief thesis of the analogical

been observed to rise in the past, this, in no way, constitutes a logical guarantee that the sun will continue to rise in the future, or, even, that it will rise tomorrow.

Equally untenable is the 'empirical' justification of naive induction by recourse to experience. The empirical justification of induction may take the following form : induction is justified because it has been observed to apply successfully in a large number of cases in the past. This justification is historically suspect because it fails to take account of the role of cases in which induction had failed to apply successfully in the history of science as crucial factors in assessing the empirical justifiability of induction. This short-coming, it is worth pointing out, is a 'natural' consequence of the verificationist - as opposed to the Popperian falsificationist - stance generally adopted by the inductivists. Moreover, the empirical justification is logically indefensible because it, clearly, offers an inductivist justification of induction, thus resulting in infinite regress. The demonstration of this point is very simple : the statement that induction is justified because it has been observed to work in the past is, itself, an inductive generalisation. As such, it requires further justification which, in turn, requires further justification, *ad infinitum*.

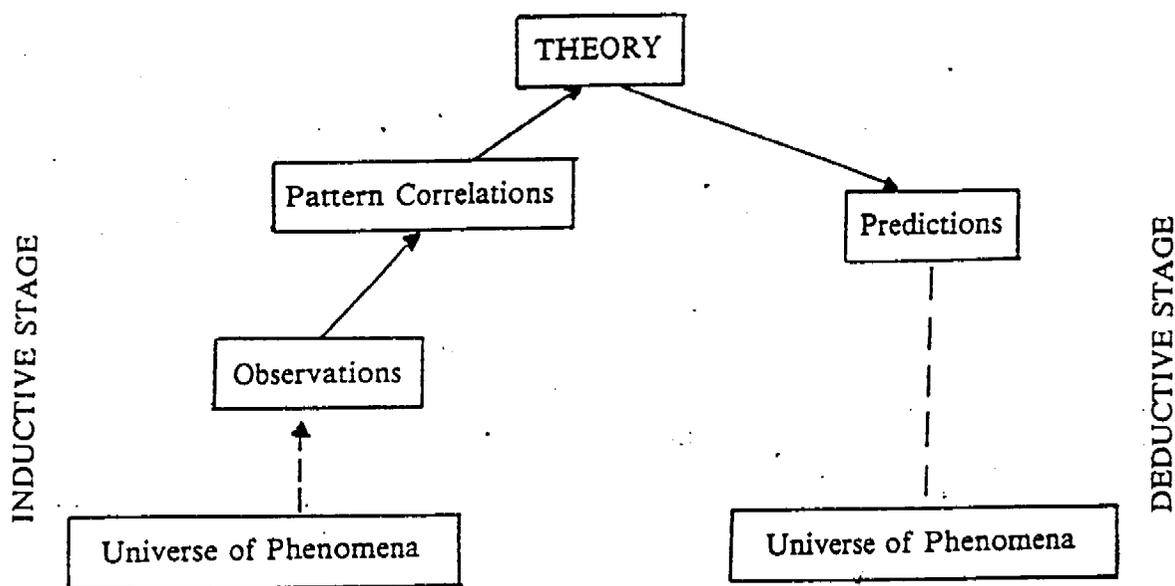
Hume's realisation that induction cannot be either logically or empirically justified, coupled with his 'belief' in the inescapable necessity of inductive procedures in the conduct of scientific inquiry, led him to propose a psychological justification of induction. Roughly expressed, the essence of this justification is that human beings are so constituted that they cannot help but think in terms of induction and its procedures (Popper, 1969). However, this justification cannot be seriously entertained as a legitimate justification due, first, to its metaphysical nature, and, secondly, to the fact that it reduces scientific knowledge, and the methodology for securing it, to something in the nature of animal 'habit' and 'instinct'.

Also untenable is the inductivist view that science starts with pure observation, and that observation constitutes the secure basis of

science (Popper, 1959). On the one hand, observation in science is not a haphazard and blind activity, but is invariably motivated by a problem and is willy-nilly guided and interpreted by reference to a point of view, no matter how covert this point of view may be. The significance of this property of science in linguistics has long been observed by Saussure who states that «far from it being the object that antedates the viewpoint, it would seem that it is the view point that creates the object» (1974 : 8). On the other hand, observation cannot be legitimately claimed to constitute a secure basis for science, since observation may seriously err, and since it is as fallible and empirically vulnerable as the theoretical framework it, by logical necessity, presupposes.

No less problematical as a feature of inductivism is the question of how many observations are needed before an inductive generalisation or theory may be allowed to be induced, and, similarly, how wide, both temporally and spatially, the data-base if a given generalisation or theory should be before the generalisation or theory concerned is allowed to emerge. Would one instance of a swan being black be sufficient to allow the inductivist to derive the generalisation «All swans are black» ? If the answer is in the negative, as it most probably would be, then how many observations reporting the 'existence' of black swans would be sufficient before the inductivist can allow the above generalisation to be derived ? If the answer to the above question is in the affirmative, though it is perhaps unlikely that it would be, then would not that be a foolhardy position to take, since the next swan one may come across may turn out to be white and not black ?

Similar problems arise with respect to the delimitation of the width of the observational-field required in the process of deriving an inductive generalisation. One may ask how wide, both temporally and spatially, should the conditions under which a given phenomenon is observed be, for example, the expanding of metals when heated, before the inductivist can allow the generalisation concerning this phenomenon to be derived ? It is highly unlikely that a satisfactory



No account of inductivism would be complete without giving due emphasis to the following features of this view. **First**, the empiricism of science is said to be equally strongly based on both the fact that theories are derived from observations concerning the empirical world, and on the fact that such theories are tested, with respect to empirical 'truth' and 'falsity', via the empirical 'truth' or 'falsity' of their deductive consequences, which 'truth' or 'falsity' are established by confronting what is asserted by these 'consequences, with the empirical world. **Secondly**, science is said to start with and from pure observation which, i.e. observation, constitutes the secure basis of science as a body of proven facts. **Thirdly**, inductive generalisations, and, also, theories as coherent sets of such generalisations, must be based on a 'large number' of observations carried out under a 'wide variety' of conditions. **Fourthly**, progress in science is additive or cumulative ; it consists, in principle if not always in practice, in the constant accretion of proven or 'well-attested' facts. My aim in the rest of this section is to give a brief critique of the inductivist view of science, thus criticising, albeit indirectly, the application of this view in linguistics.

## 2.2. Inductivism : A Critique

A close examination of inductivism shows that this view of science suffers from certain very serious defects. Considered from the standpoint of what scientists do in the normal course of their investigations, inductivism is a completely 'mythical' view of science. It, simply, is not 'true' that scientists, even those who claim to be inductivists, characteristically follow, whether consciously or sub-consciously, the methodological prescriptions of inductivism (Popper, 1959, 1969). Practicing scientists, reflecting on the 'steps' carried out by them in their endeavours to realise their scientific goals, would confirm the position expressed in the preceding sentence. Moreover, as was shown by Hume a long time ago, induction, in its 'naive' form (Chalmers : 1978) cannot be either logically or empirically justified. From the logical standpoint, induction is not logically justifiable because there is no logical guarantee that if the singular observational statements presupposed or implied by the theory are true, then the theory itself will, via its logical consequences, be necessarily true. To borrow one of the classic examples in the literature, no matter how many times the sun has

be firmly rooted in objective observation and in highly controlled experimentation. Moreover, science is said to stick to the facts and to shun mere opinion, speculation, pre-conceptions and personal preferences and prejudices (Harris, 1970). As a result of this, scientific knowledge is said to have the status of proven knowledge, i.e. 'true' knowledge with respect to what the universe of phenomena is like. Scientific progress is seen as consisting in the never-ending process of piling-up proven knowledge, through the constant addition of fresh proven knowledge to the body of previously proven knowledge.

In linguistics, the inductivist view of science is adhered to, in some form or another, by Bloomfield (1939, 1976), Hockett (1958), Pike's tagmemics (Cook, 1971, and Pike, 1976), Robins's word-and-paradigm (1959) and systemic grammar, at least in the early stages of the latter theory, and at least as far as the underlying methodological assumptions of this approach are presented by Dixon in this *Linguistics Sciences and Logic* (1963). According to Dixon (ibid : 11 - 12) :

«We say that the 'raw material' of science consists of observations. The scientist will recognise a certain pattern which is common to a number of observations ; in other words, he will notice that, in some particular way, the observations are similar to each other. Having recognised certain patterns he will then compare and correlate them... A theory is obtained by generalisation upon pattern correlations. Thus a theory can be looked upon as an abstraction from a number of observations. What is abstracted from each is that pattern or patterns which is seen to recur in similar form in other observations... a theory is... an abbreviatory form of statement».

Having arrived at his theory in, more or less, the manner outlined by Dixon in the above

quotation, the inductivist scientist proceeds to the second stage in his approach, namely that of deductively deriving from his theory, in its capacity as a set of universal statements, singular statements which have the status of predictions concerning the states of affair that constitute the universe of phenomena lying within scope of the theory. Needless to say, such predictions transcend, both temporally and spatially, the data-base presupposed by the theory, i.e. the set of observations upon which the theory is based. Testing constitutes the third stage of the inductivist view of science. It is carried out by confronting the predictions derived from the theory with the states of affairs embodied in further observations concerning the relevant universe of phenomena.

Induction, then, consists of three stages : (1) the inductive or theory-generating stage, whose aim is to arrive at a theory, in its capacity as a coherent set of related statements, by generalising from a finite set of observations and pattern correlations, the latter being no more than second-order observations, viz. observations twice-removed from the world of facts or experience ; (2) the deductive stage, whose aim is to deduce by logical means a set of singular statements, having the status of predictions concerning states of affairs in the relevant universe of phenomena. For the predictions to be logically valid they must be capable of being shown to follow from the theory in an unequivocal manner ; however, their fate vis-à-vis the empirical world falls within the domain of the third stage ; and (3) the testing stage, whose aim is to verify the truth of the theory by confronting what is asserted by the individual predictions derived from it with the empirical world via further observation and experimentation. This view of the methodological nature of science may be schematised as follows :