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[43] This is done frequently, but see for eample, Suarez de Ribera, *Medicina Invencible Legal, O Theatro de Fiebres Intermitentes Complicadas* (Madrid: Francisco del Hierro, 1726), p. 280.

[44] Suarez de Ribera, *Canones Particulares de Cirugìa, con que se Libertan Muchos Desahuciados, si al Sagrado de sus fuentes se Refugian* (Madrid: Manuel de Moya, 1751), title page.

[45] Suarez de Ribera, *Escrutinio Medico ò Medicina Experimentada* (John Crerar Library copy missing title page, introductory letter dated 1723), pp. 323-324.

[46] Suarez de Ribera, *Anatomica Chymica, Inviolable, y Memorable* (Madrid: Manuel de Moya, 1743), pp. 21-22.

[47] Suarez de Ribera, *Clavicula Regulina* (Madrid: Diego Martinez Abad, 1718), p. 19.

[48] Suarez de Ribera, *Medicina Invencible*, pp. 262-263. On p. 298 he goes on to give a lenghty discussion of a Dr. Botoni on the circulation.

[49] Suarez de Ribera, *Canones Particulares de Cirugìa*, p. 73; *Cirugia Methodica Chymica Reformada* (Madrid: Francisco Laso, 1722), p. 8. Of the many promised Portuguese translations this is the only work by him that I have seen in that language: F. Soares da Ribeyra, *Cirurgia Methodica e Chymica Reformada*, traduzida de Castelhano em Portuguez ... Joseph Gomes Claro (Lisboa Occidental: Na Officina Ferreyrenciana, 1721).

[50] *Ibid.*, p. 136.

[51] *Ibid.*, p. 128.

[52] Suarez de Ribera, *Anatomica Chymica*, pp. 19-27.

[53] Suarez de Ribera, *Amenidades de la Magia Chyrurgica y Medica, Natural* (Madrid: Fernandez de Arrojo, 1736), p. 75.

[54] *Ibid.*, p. 38.

[55] See Suarez de Ribera, *Ilustracion, y Publicacion de los diez y Siete Secretos del Doctor Juan Curvo Semmedo, confirmadas sus virtudes con maravillosas observaciones* (Madrid: Domingo Fernandez de Arrojo, 1732).

[56] Suarez de Ribera, *Escrutinio Medico*, p. 271; *Amenidades de la Magia*, p. 73.

[57] Suarez de Ribera, *Canones Particulares de Cirugìa*, sig. ¶¶¶¶¶ 1v. See also *ibid.*, pp. 56-72.

[58] Suarez de Ribera, *Arcanismo Anti-Galico, o Margarita Mercurial* (Madrid: Juan de Ariztia, 1721), p. 200.

[59] Suarez de Ribera, *Medicina Invencible*, p. 280.

[60] Suarez de Ribera, *Ilustracion, y Publicacion*, sig. 4v.

[61] João Curvo Semmedo, *Secretos Medicos, y Chirurgicos ... Traducidos de Lengua Vulgar Portuguesa en Castellana* (Madrid: Bernardo Peralta [1731]).

[62] Dr. Don Joseph Diez de Medina, *Declaracion de los Verdaderos diez y siete secretos de Curvo de la incertidumbre de los publicados por el Doctor Rivera, y de algunos errores, que sobre otros Secretos de Curvo cometiò el Doctor Cortijo* (Madrid: Antonio Denferzan, 1735).

THE TREND IN HISTORY AND PHILOSOPHY OF SCIENCE

WILLIAM R. SHEA *

Let me begin by saying that half my half my task has already, and egregiously, been performed by my friend and colleague Professor Allen G. Debus. In his lecture on «The History of Science Today» to the Academia das Ciências de Lisboa he showed how recent history of science shifted from a narrow positivism to a much broader and more comprehensive study of what science meant at various periods in the history of mankind [1]. I agree with his analysis of the situation and I should like to extend to the philosophy of science the historiographic and conceptual tools that he used so deftly and accurately in his assessment of a new spirit of historical rigour and epistemological tolerance in the history of science.

Allow me to first recall that the mainstream of the philosophy of science in the second quarter of this century — the so-called «logical epiricist» or «logical positivist» movement — assumed that theoretical language in science is parasitic upon observation language and can be eliminated from scientific discourse by disinterpretation and formalization, or by explicit definition in or reduction to observational language.

In the present paper, I propose to show why logical positivism failed to do justice to the basic empirical and logical problems of philosophy of science. I also wish to consider why the drastic reaction, typified by Thomas Kuhn and Paul Feyerabend, fails to provide a suitable alternative, and to suggest some approaches that may hold out a genuine promise of dealing effectively with the central tasks that face the philosopher of science today.

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On its empirical side, logical positivism is best described as the heir of British empiricism. To the question, «What are theories about?», seventeenth-century Empiricists would have unhesitatingly answered: «They are about concealed entities whose existence and functioning become known to us by means of aids to the senses (e.g., telescopes, microscopes), and by inference to hidden causes from their observable effects». No one doubted that this was the aim of science and that ordinary language was adequate to describe small unobservable mechanisms. Although Locke queried the possibility of gaining actual knowledge about entities that were completely concealed beneath the appearances, questions about what it is to be such a hidden entity or why it is hidden simply did not arise. Assuming that hidden causes can be known, increasingly elaborate accounts were produced, purporting to describe entities of increasing hiddenness and more and more unfamiliar characteristics. Their outcome can be seen in the contradictions of aether theories in the nineteenth century.

The collapse of metaphysical mechanism and the bizarre characteristics of modern theoretical physics marked the end of the headlong course of this uncritical realism. By the same token, it compelled philosophers of science to ponder how familiar language can be used to describe hidden entities, and what is really being referred to in this way. The logical positivists met this problem by retreating to the firm basis of the observable where there was thought to be comparatively no doubt about what people are talking about, nor about what our language means, nor how we know our assertations to be true [2]. In other words, the question of *meaning* (What do theories mean?) and the question of *acceptability* (How de we know what theories are true?) were believed to hinge on the question of *reference* (What are theories about?) which, in turn, was taken to raise no difficulty. This methodological assumption, which was embodied in the classifical distinction between self-evident «observation terms» and derivative «theoretical terms», constituded the *empirical* component of logical empiricism.

The *logical* component consisted in reliance on mathematical logic for formulating the problems of the philosophy of scence, often rechristened «the logic of the sciences» to emphasize its two caracteristic features. First, just as formal logic, since Aristotle, had been concerned with the form rather than the content of propositions and statements, so philosophy of science dealt with the logical form of scientific statements rather than their content. The job of the philosopher was conceived as the formal representation of scientific expressions in general, leaving to the practising scientist the task of confronting his conclusions with actual scientific procedure. Important advantages were said to accrue from the adoption of the analogy of formal logic. Philosophy of science, thus disengaged from the specific tenets of particular scientific theories, became immune from the vicissitudes of change and the overthrow of current beliefs. Since the philosopher of science could, in principle at least, outline the characteristics of al possible explanations, he could, by the same stroke, give the formal characteristics of all future explanations. This was one of the most attractive aspects of the logical empiricist programme: it appealed to the administrative machinery of our minds, accustomed to reduce understanding to linear arguments and dichotomous classifications capable of development by deductive logic.

The second characteristic of the «logic of the sciences» was its conception of scientific theories as formal systems, axiomatizable with the aid of the techniques of mathematical logic. The philosopher of science concentrated on perfected or idealized systems, and took for granted that science grew by the automatic incorporation of earlier hypotheses into later theories as special cases applicable in limited domains of experience. Thus, the empirical and the logical aspects of logical empiricism can be summarized in the following two tenets: theoretical terms are (a) grounded in self-evident observation terms, and (b) open to exhaustive manipulation through modern mathematical logic.

This unquestioning faith in the truth and ovious meaning of observational predicates was really a cover for a much more deep-rooted belief, stemming from the empiricist tradition, that there are entities and their properties which are given in perception, and that there are more or lesse well-defined areas of language which directly describe them. The positivist distinction between «observation» and «theory» is a direct lineal descendant of the distinction between «impressions» and «ideas» formulated during the development of British empiricism in the seventeenth and eighteenth centuries as a criterion of meaning and acceptability of concepts. The «observational-theoretical» dichotomy inherited the function of its predecessor but with three refinements. First, an attempt was made to avoid the misleading psychological overtones of the word «ideas» by speaking of «terms» and «statements». Secondly, Hume's problem of the origin of such terms was eschewed by justifying the acceptance or rejection of theories on the grounds of their «definability» and «reducibility». Finally, the new distinction was employed to tackle a problem to which the more general classification between ideas and impressions had not been applied. Dealing with theories, namely a network of interrelated concepts rather than with single ideas, the modern distinction had to consider and choose between rival sets of terms and statements. Hume and the early empiricists did not discuss competing sets of ideas, and it is here that the logical empiricists added a new dimension to the classical debate. Assuming that observational statements had direct empirical reference and were immediately decidable, they saw in the overlapping observational vocabularies of different theories a basis for instituting a comparison between them.

In view of the importance given to observational statements in assessing the relative merits of rival theories, it is surprisingly difficult to find in recent works in philosophy of science any detailed explanation of their nature. Accounts of the observational language are usually dependent on circular definitions of observability and its cognates, while the theoretical language is generally defined negatively as consisting of those scientific terms which are not observational. For instance, Braithwaite writes:

> ... experience, observation and cognate terms will be used in the widest sense to cover observed facts about material objects or events in them as well as directly known facts about the contents or objects of immediate experience [3].

Hempel states that:

> ... an *observation sentence* might be construed as a sentence ... which asserts or denies that a specified objects, or group of objects, of macroscopic size has a particular *observable characteristic,* i.e., a characteristic whose presence or absence can, under favorable circumstances, be ascertained by direct observation [4].

Even Nagel, who gives the most thorough account of the distinction, seems to presuppose that there is nothing promlematic about it:

> ... no precise criterion for distingushing between experimental laws and theories is available, and none will be proposed here. It nevertheless does not follow that the distinction is spurious because it is vague, any more than it follows that there is no difference between the front and the back of a man's head just because there is no exact line separating the two [5].

The most explicit statement is in Carnap's classic paper «Testability and Meaning» where he makes the following two points. First, he holds that «observable» must be a primitive of the metalogic of theory-structure, in terms of which other concepts such as «confirmation» and «testability» are defined. The reason he adduces for this is that what it is to be «observable» is a question for psychology and the behaviourist theory of language, not philosophy. Next, in approximate replacement of a definition, Carnap describes the concept of observability as follows: a predicate «P» is observable for a person N if for some object b, N can under suitable circumstances come to a decision to accept or reject «P(b)» with the help of a few observations. He continues:

> This explanation is necessarily vague. There is no sharp line between observable and non-observable predicates because a person will be more or less able to decide a certain sentence quickly, i.e. he will be inclined after a certain period of observation to accept the sentence. For the sake of simplicity we will here draw a sharp distinction between between observable and non-observable predicates ... Nevertheless the general philosophical, i.e. metholological question about the nature of meaning and testability will, as we shall see, not be distorted by our over-simplification. Even particular questions as to whether or not a given sentence is confirmable, and whether or not it is testable by a certain person, are affected, as we shall see, at most to a very small degree by the choice of the boundary line for observable predicates [6].

Carnap presupposed, therefore, that an initial distinction between observables and non-observables on pragmatic grounds leads to no distortion in the subsequent discussion of the relation of theory to observation. But there is not such guarantee. What is overlooked in his pragmatic account of observability is that the pragmatic conditions themselves are theory-laden: they are always formulated against a background of current scientific views and opinions. Furthermore, it is by no means obvious on Carnap's account that any predicate is, in principle, non-observable. Perception depends on training, past experience and contemporary hypotheses, and there are circumstances in which suitably educated persons can come to a quick decision about any predicate, however apparently theoretical, such as «meson» or «positron». The recourse to psychology or physiology to determine what an «observable» is offers no easy way out. Even if these sciences could provide us with a clear answer to the question, we would not escape the problem of a vicious regress. For what is it to be accepted as an «observable» for psycchology and physiology, and hence to be an empirical basis for their theories of «observability»?

Why was it for so long considered necessary to accept the comparatively unproblematic character of observation predicates? Could it be that a half-conscious fear of vicious circularity inhibited the desire to approach the problem from a different angle? If we have no firm observational basis on which to stand, how can we begin to examine the meaning of theories which are erected upon observables? It was good conservative policy, therefore, to assume that there could be explicit definition and hence complete equivalence between every theoretical predicate and a function of observation predicates. Attempts to implement the programme, however, proved that this was illusory. First it was shown that in many existing theories, including quantum theory and even Newtonian mechanics, such exhaustive translations cannot be carried out, and yet no theorist would wish to abandon otherwise satisfactory theories on this ground alone. Second, and more important, it was pointed out that explicit definitions of each theoretical predicate in terms of observation predicates would cripple the theory in its proper role of expanding to correlate other and different observation statements.

By this time the «firm basis» of observation language had become shaky, and philosophers had started looking elsewhere for a sure footing. But the breakdown of the theoretical-observational dichotomy was yet to come.

One of the reasons why the crisis was delayed was the possibility of shifting one's attention from the empirical to the logical aspect of the problem and to concentrate on the deductive character of theories. In the deductivist account, theoretical predicates need not be given explicit definitions nor even reduction definitions in terms of observables. On the analogy of mathematical logic, the postulates of the theory give the syntax and basic logic of the system, and the question of meaning becomes the problem of determining the extra-logical semantics or interpretation. Campbell and before him Maxwell held that this interpretation is given by a physical model which is intelligible, ultimately in observation terms, independently of the theory and its explanandum, and which therefore contributes to the theoretical predicate an interpretation not derived from any direct connection with its explanandum. More commonly, however, deductivists regarded such physical models as non-essential heuristic devices, and argued that the meaning of theoretical predicates was implicit or contextual [7]. Just as the terms «point» and «line» are not explicitly defined in a formalized geometry, but their meaning, or rules of use, is implicitly conveyed by the postulate set, so it is maintained that a predicate such as «positron» means just that entity which has the relation to other entities of atomic physics which are specified in the postulate system of physics. Physics differs from formalized geometry, however, in that some consequences of its formal postulates are translatable by means of correspondence rules (also called coordinating definitions, semantical rules, epistemic correlations) into observation statements, ensuring its empirical content, whereas fully formalized geometry does not presuppose any empirical content.

This interpretation is open to severe criticism. First, the parallel between physics and a physically interpreted geometry is imperfect and cannot adequately explain the notion of implicit definition of theoretical predicates. Secondly, the function of a logical model is to specify a set of entities with their predicates such that, when the postulate system is interpreted in the domain of these entities, the postulates are true in that domain. If no such model is specified for the postulates of a scientific theory, the best that can be said is that the postulate system «implicitly defines» a set of possible models, that is, all those that would realize the postulates, and that the deductive relation of the postulates to observable further limits these possible models to those that include the observable entities and their observable relations. Thus the «implicit definition» is not an interpretation of the theoretical predicates, but a definition of a set of possible models which partially intersect in the observable consequences of the theory. In any interesting case there are likely to be an indefinite number of such models. It is not at all clear that the notion of their set is well-defined, and in any non-trivial case some of them are likely to be inconsistent with others. It is an admitted consequence of deductivism that the notion of their set cannot add anything to the cognitive content of the theory over and above its already interpreted observable consequences, and hence that deductivist accounts, like positivist accounts, do not provide a solution to the problem of the *meaning* of theoretical predicates in its original form. A further question is whether they can provide a solution to the problem of *confirmation* or *acceptability* of theories.

Hempel's «Studies in the Logic of Confirmation», which appeared in 1945, was the first major attempt to outline criteria of adequacy for the confirmation of statements related to evidence in deductive systems.

Hempel enquires what general requirements ought to be satisfied by a confirmation theory which does justice to accepted forms of theoretical inference. Two are particularly relevant to our discussion.

1) *Special Consequence Condition.* If an observational report E confirms a hypothesis H, then it also confirms every consequence of H [8]. This seems intuitively desirable in order to allow for the familiar type of inference where a theory is advanced on the basis of experimental laws which are entailed by it. For instance, Newton's theory is proposed on the evidence of Galileo's law of freely falling bodies and Kepler's laws of planetary motion which are entailed by it. Further as yet untested consequences are then drawn from the theory, for example the variation in the period of a pendulum as it approaches the equator and the influence of the moon on the tides. These are predicted with assurance on the basis of the confirmation of the theory by its observed consequences Indeed, it is sometimes the case that confidence in the consequence of the the theory is so great that it enables corrections to be made to laws that have already been accepted, as when Newton's theory amended Kepler'ss third law as received on the observed evidence of the periodic time of the planets.

2) *Converse Consequence Condition.* If an hypothesis H entails an observational report E, then the observational report E confirms any stronger hypothesis of which H is a consequence [9]. This condition seems at first sight to be required to explicate why evidence entailed by a theory confirms the theory. But if it i taken together with (1), we get the following counter-intuitive case: suppose $H \rightarrow E_1 . E_2$, but there is no reason to expect E_1 to confirm any arbitrary E_2 which may be conjoined with E_1 to form H, indeed in general it will not do so. This example shows that if (1) and (2) are both accepted, any evidence will confirm any statement whatever by the mere construction of a suitable *H* by conjunction.

For this reason, Hempel originally rejected (2) and retained (1) [10], thus demanding a stronger relation between hypothesis and evidence than mere entailment if evidence is to confirm the hypothesis. Carnap demonstrated however that this solution gave rise to serious difficulties, for if (1) is accepted and (2) rejected, then the confirmation function cannot be a probablity function, for in a probabilistic confirmation theory (2) is always satisfied for empirical E [11]. A further consequence

is that (1) is then not in general satisfied, for if E_1 is to confirm E_2 probabilistically there must be a stronger relation between E_1 and E_2 than their joint deducibility from some hypothesis H. For these reasons, Hempel has now accepted that condition (1) rather than (2) should be abandoned [12].

But wheter a probabilistic theory is aodpted or not, the upshot of the discussion is that the deductive criteria thus far suggested are unsatisfactory. It is hard to see how they could even begin to be relevant to the crucial problem of predictive inferences of the kind illustrated by Newton's theory and the laws it explains. For instance, let us suppose we are given an axiomatic system S from which some observation statements can be derived with an appropriate set of correspondence rules. Since S itself, on the deductive view, contains no observation predicates, no reason can be derived from the statements of S why one observation predicate should occur in a given correspondence rule rather than another. On the one hand, if the conjunction of S and the correspondence rules is merely to explain given observations, then the observation predicates in these rules are determined by the known relations of observation predicates in observation statements. On the other hand, if S is to be used predictively this involves deriving statements containing new observation predicates which do not appear in the correspondence rules. But S is powerless to determine what additional correspondence rules should be introduced, and consequently no predictions involving some particular new observation predicate rather than some other can be made with confidence from the theory. There are, however, countless examples of scientific inferences in which just such predictions have been made with confidence, for instance how the earth would appear when seen by astronauts landing on the moon.

In spite of its detailed analysis of the logical structures of postulate systems, the deductivist account of theories had shown itself grossly inadequate. A strong reaction was inevitable, and by the end of the fifties the complaint was increasingly voiced that in their concentration on technical problems of logic, the logical empiricist movement had lost contact with real science.

It was naive to read the past, as the positivists did, as the record of great men throwing off the shackles of a dark inheritance and heralding the dawn of scientific objectivity. Many older theories that were allegedly laden with superstition, for instance, medieval mechanics and the phogiston theory, were found to contain far more thant simpleminded error and prejudice. It was discovered that Newton ont only framed non-empirical hypotheses but that he was strongly influenced by the alchemical tradition, and that Galileo, the father of «empirical science» neither dropped balls from the Leaning Tower of Pisa nor cared for experiments as much as had hitherto been believed. As closer attention was paid to the framework of theories it became apparent that the theoretical context determined not only the questions that were raised but the terms in which the answers had to be expressed to be judged acceptable. This was a direct challenge to the logical empiricist position that there is an absolute, theory-independent observation language whose terms have the same core of common meaning for all competing theories. According to the new view, the meaning of all scientific terms, whether factual or theoretical is governed by the paradigm which underlies them and in which they are imbedded.

This new interpretation has been urged with considerable vigour by Thomas Kuhn [13] and Paul Feyerabend. «The meaning of every term we use», Feyerabend writes, «depends upon the theoretical context in which it occurs. Words do not 'mean' something in isolation; they obtain their meanings by being part of a theoretical system» [14]. Whereas the logical empiricists considered theoretical terms as wholly dependent on observation ones, from the new viewpoint the exact reverse is true.

> The philosophies we have been discussing so far assumed that observation sentences are meaningful *per se*, that theories which have been separated from observations are not meaningful, and that such theories obtain their interpretation by being connected with some observation language that possesses a stable interpretation. According to the point of view I am advocating, the meaning of observation sentences is determined by the theories with which they are connected. Theories are meaningful independent of observations; observational statements are not meaningful unless they have been connected with theories... It is therefore the *observation sentence* that is in need of interpretation and *not* the theory [15].

It follows that a basic shift in the theoretical viewpoint entails a change in what counts as a real problem, a correct method, an acceptable explanation, and even a fact, since the meaning of observational terms is determined by the theory in which they occur.

Kuhn and Feyerabend have mercilessly exposed the inadequacies of the usual formulation of the distinction between theory and observation, and they have shown that the concept of «explanation» in science cannot be divorced completely from a reaction against logical positivism. However, they seem to have embraced an equally extreme view. For all its value and its suggestiveness, the position of these writers has not been formulated in a way that resolves the major problems of philosophy of science. It has only succeeded in making them more glaring. If the meaning of every term depends on its theoretical framework, a change of theory must produce a change of meaning of every term in the theory. But this sweeping solution brings a new series of problems in its wake. For instance, does the mere extension or application of a theory make a difference to the theoretical content and hence to the meaning of the terms involved? Does an alternative axiomatization alter the theoretical content so that the meanings of the expressions axiomatized change with reaxiomatization?

Apart from these technical difficulties, more fundamental objections can be raised. For instance, what is the point of making experiments if they can be interpreted to support any theory? If the meaning of observational terms depends completely on the theoretical framework, how can we ascribe any continuity to the different usages of the same terms in successive theories? Rival hypotheses can no longer be said to contradict one another, for in order for two sentences to be contradictory, what is denied by one must be affirmed by the other, and this is meaningless unless they have something in common. If Kuhn and Feyerabend have established the bankruptcy of a philosophy of science based on the allegedly firm foundation of «observations», they have produced an alternative account that severs it from empirical evidence altogether. Their position eventuates in a complete relativism in which it becomes impossible to compare any two scientific theories and to choose between them on any but the most subjective grounds. The positivists may well retort that if observation itself is said to share the uncertainties of theories in any important sense, we cannot avoid being cast adrift in a sea of hypotheses. Without a solid observational basis, we would seem to have no moorings, no plan, and no science.

Any viable account of the observation language must be able to show not only that we can keep afloat in such a sea but that we can make progress through it. Several attempts have been made recently to plot such a course. One of the most promising is Mary Hesse's outline of a self-correcting confirmation theory [16].

For the sake of brevity and ease of comprehension, she introduces a model of a world consisting of a finite number X of individuals described by means of a fixed set of «K» primitive monadic predicates each of which may or may not apply to each individual.

An essential feature of the model is that a predicate may be wrongly ascribed to an individual and that a definite small prior probability can be assigned to the occurrence of this error. Since this value is small, the ascription of predicates will for the most part be correct and certain regularities in the relation between predicates will begin to emerge. For instance, it may be the case when a predicate P has been applied to some individual, Q has usually been found to belong to it as well. Each predicate can be visualized as being a knot in a network of relationships with other predicates, where the strenght of the strings between the knots stands for some increasing function of the proportion among individuals in which the predicates have been reported as co-occurrent or co-absent. Assuming that there exists a probabilistic confirmation theory which explicates the more elementary kinds of inductive inference, and that prior probabilities can be assigned with the aid of some postulate such as Keynes' Principle of Limited Independent Variety, Bayes' Theorem can be used to calculate the probabilistic confirmation value of any as yet unobserved application of a predicate to a given individual on the basis of existing evidence.

The crucial problem is to detect and correct the erroneous ascription of a predicate. Since all evidence is necessarily in terms of reports that *P* applies to *b*, and it is assumed that there is finite probability that such a report may be wrong, a distinction must be drawn between a report and «what really is the case». What must be done in this situation is to try to identify errors on the basis of other reports, some of which may make the correctness of a given application extremely improbable. It is at this point that a charge of circularity could be brought against this model. It can be avoided, however, for while it is true that all predicates could be erroneously applied, it is not the case that all could be known to be wrong at the same time. For instance, alchemical terms used in modern chemistry are no longer predicated of individuals in the same sense as they were in the middle ages, but the transformation of their meaning did not happen for all of them at the same time. What is contrasted with the report that something or other is the case is not what *really is* the case, to which we have no direct access, but rather what in a given theory *should be* the case, which may in a small proportion of cases contradict what has been reported. The fact that this is not as arbitrary as may seem can be illustrated by the following case-

history in geology. When Louis Agassiz, fresh from studying glacial action in Switzerland, visited Scotland in 1840, he immediately recognized moraine, roches moutonnées, and glacial striae as distinctive features of the Scottish scenery. In this case, his extensive knowledge of the effects of glaciers and glaciation may be said to have enhanced his perception. But this way of looking at objects became so ingrained that later he began to *see* evidence of glacial action where certainly there had been none, as in the Tijuca hills behind Rio de Janeiro. His reports, however, were only revised when it was shown that they were *inconsistent* with later and more detailed accounts.

It does not follow from this that the theory which contradicts the fewest reports is the theory with the highest confirmatiion value, for the amount of «correct evidence» according to this theory may be counteracted by the higher prior distribution of some other theories. For instance, if Keynes' Principle is built into the prior distribution, preference will be given to theories which cluster individuals, that is to say, which allow individuals to be classified in more or less well-defined classes with high similarity between members of a class and a non-member of that class. Without going into further refinements which could be introduced, we can see how this model, however oversimplified, offers an alternative to the positivist observational language.

On the new interpretation, the meaning of descriptive predicates is no longer determined exclusively by direct empirical reference in a way that is radically different from theoretical predicates. The meaning of both observatiional and theoretical terms has elements of stimulus-response in situations of easy empirical reference, and also of contextual relation with other predicates which co-occur or are co-absent. It follows that whereas error on the positivist view is taken to be relative to «what is really the case», on this interpretation it is considered relative to a theory. This has the interesting consequence of allowing the possibility that the source of error may not merely be lack of attention to what is observed, but can also occur in a sincere and careful report, when ascription of a predicate does not fit in well with the general outline of the theory. This is to say that the correct ascription of a predicate depends both on a careful response to a stimulus situation, and also on the relations of this predicate to other predicates in this and other individuals. A theory is constituted by a set of such relations. Hence confirmation and falsification will depend not on one allegedly crucial experiment, but on all the reported evidence in other situations, together with the verdicts of different theories on the correctness of these reports. Thus the directeness of empirical reference and the immediate decidability which the positivists considered as the characteristics of observation predicates marking them off from theoretical predicates, appear illusory.

Whether this approach can be shown to be adequate to all types of theoretical inference is too extensive a question to prejudge here, for it stands in need of a confirmation theory based in a language containing the real number continuum, for which many as yet unsolved mathematical problems arise. But perhaps enough has been said to conclude that we are not compelled to adopt a kind of historical relativism and to regard theoretical inference as fundamentally irrational. The problems and the techniques of the logical empiricists can be pushed further than they themselves thought possible. Both the question of meaning and the question of confirmation are more complex and interrelated than had hitherto been imagined, but there is, as yet, no reason to despair of a logic of scientific discoveries.

While Mary Hesse tackles the problem of scientific discourse from the logical angle, Dudley Shapere is mainly concerned with the lessons to be learned from the history of science. He grants that Kuhn and Feyerabend are right in stressing that the framework of contemporary hypotheses, the paradigm or «disciplinary matrix» determines to a large extent what questions can be raised, and what views can be suggested about a given scientific problem. He objects, however, that the «incommensurability» of theories, which is a consequence of the extreme form of their position, distorts the very nature of the scientific enterprise. Shapere argues, for instance, that it makes sense to compare the medieval theory of impetus with Aristotelian mechanics on the one hand, and with the principles of inertia on the other.

> The impetus theory stands in a transitional phase between these two traditions — still in the tradition of Aristotelian physics in its view that impetus is a cause of motion, but heading toward the modern view in making impetus an internal and incorporeal force rather than an external, corporeal one [17].

The impetus theory encouraged a fresh approach to traditional problems by removing long-standing conceptual barriers. For instance, the Aristotelians rejected outright any suggestion that the earth rotated because the physical assumptions of their system entailed that a strong wind would be set up in the direction opposite to the earth's motion.

By affirming that the air would receive an impetus and would be carried around as though nothing has happened, the impetus theory made it possible to entertain the idea that the earth might move. Likewise by internalizing the cause of motion it shifted attention to a new of possibilities. Since air could no longer be the force producing motion but merely an impediment, motion in a vacuum ceased to be implausible, and thus the impetus theory cleared the ground for thinking about the idealized case of a body moving in the absence of impeding forces. Furthermore, by treating all cases of motion, whether terrestrial or celestial, natural or constrained, in terms of one kind of cause, namely impetus, it paved the way for a unified account of all motion and provided an alternative approach to the traditional Aristotelian division.

On the Kuhn-Feyerabend view one is practically driven to describe scientific change in revolutionary terms, to speak, for instance, of the overthrow of Aristotelian mechanics by the impetus theory and of the latter by Newtonian science. From the viewpoint Shapere adopts a more balanced description seems possible. The Aristotelian-Scholastic tradition applied, as a template, an intricately connected web of concepts and propositions to the data of perception and everyday experience. In laying down this system of interpretation, it simultaneously set up obstacles or limitations, both by theoretical precept and by suggestion, to thinking in certain other ways. The vacuum and the actual infinite, for example, appeared self-contradictory, while the motion of the earth seemed physically impossible. Scientific advance is related to a reassembling of the pattern of our experiences, and there is immense resistance to this. «In this sense», writes Shapere,

> ... we can speak of the Aristotelian view as having involved certain 'presuppositions' specifying (for example) what could and what could not count as an explanation. To this extent Kuhn and Feyerabend have made an important point. But these 'presuppositions' were not mysterious, invisible, behind-the-scene 'paradigms' (Kuhn) or 'high-level background theories' (Feyerabend), but were involved in the straigtforward scientific statements themselves, even though there were disagreements about details (and even about fundamentals), and even though the way in which they restricted thought, or the importance of these restrictions, vould not be seen so easily [18].

After all, they were seen by Philoponos, Oresme, Buridan and others who reassembled the facts in a different pattern!

The relativism of the Kuhn-Feyerabend position is not the result of an investigation of actual science and its history but the purely logical consequence of a narrow presupposition about what «meaning» is. These writers hold that if scientific terms do not retain precisely the same meaning over the history of their incorporation into more general theories, then these theories cannot be compared, and the similarities they exhibit must be considered at the best as superfical and at the worst as desceptive and misleading. This claim rests on the assumption that two expressions or sets of expression must either have exactly the same meaning or else must be completely different. The only possibility left open by this rigid dichotomy of «meanings» is that history of science, since it is not a process of development by accumulation, must be a completely noncumulative processe of replacement.

The inherent weakness of this position turns out to be its retention of a positivistic concept of «meaning». If anything the revolution isn't radical enough. Kuhn and Feyerabend, in spite of their spirited attack on the positivist view that «theories» are parasitic on «observations», nevertheless approach their problems with that distinction in mind. They have applied the old classification to a new purpose rather than invented new conceptual tools for dealing with old problems. They have merely inverted the respective role of the two members of the classical distinction: it is now the «theory» that determines the meaning and acceptability of the «observation», rather than the other way round.

It is much more radical to call the distinction itself in question and to escape from the horns of the dilemma by breaking them. It may well be that several problems that bedevil contemporary philosophy of science are heightened (if not created) by the deficiencies of the distinction between a theoretical and an observational language. If this is the case, it is no longer the solution that is seen to be problematic but the very way in which the question is framed. For instance, the notorious problem of the ontological status of theoretical entities or the question whether a realistic interpretation of scientific theories can be upheld may be partly generated by an inadequate concept of the working of theories. It is all too easy to view the distinction between observational and theoretical as paralleling a distinction between existent and non-existent. If observation terms are said to have a clear and direct reference to entitites that exist while theoretical terms do not, it becomes difficult to know what theories are about. This is not due to any intrinsic opaqueness of the concept of existence, but to the sharpness of the distinction between theory and observation. As long as it was believed that theoretical terms could be exhaustively described via observational ones, theories could be handled as a convenient shorthand. When it became apparent that such a reduction was only possible in part, the extra meaning of theoretical terms was sought in the position they occupied in the context of the system they belonged to .Thus theoretical entitites could not exist in the same sense as table and chairs, and it became a first-class puzzle to know in what sense they could not be said to exist, short of being merely useful fictions.

The reaction of writers such as Kuhn and Feyerabend was to claim that this indicates that observations are governed by theories, which are, in last resort, irrational guesses at what the universe really looks like. But this did not solve the problem of meaning; it merely replaced the positivist thesis of meaning invariance with the doctrine of incommensurable meanings. A lesse rigid interpretation is possible: meanings can be considered similar or analogous, namely comparable in some respects while differing in others. By taking this path, we can hope to preserve the fact that theories, for instance, Newtonian and relativistic dynamics, are not incommensurable, although they are more fundamentally different than the most usual logical empiricist views make them [19].

The difficulty in this interpretation lies in the concept of similarity or degrees of likeness of meanings. It is only too tempting to distinguish, against the background of a particular theory, between what is and what is not an essencial parte of the meaning of a term. For instance, it seems obvious (in the light of subsequent developments) that Newton's absolute space and absolute time are «irrelevant features» of his mechanical theory. Yet those very features, for some purposes, may prove to be the very ones that are of central importance in comparing two uses. An absolute distinction between essential and accessory features could only rest on an *a priori* conception of scientific understanding and would reintroduce the fallacy of the theoretical-observational dichotomy. It would thus seem wiser to allow all features of the use of a term to be equally potentially relevant in comparing the usage of the terms in different contexts. Kuhn himself has suggested such a flexibility [20], and Ian Hacking and Nancy Cartwright have tried to stear a course between the Charybdis of asserting too much and the Scylla of asserting nothing in their complementary books *Representing and Intervening* and *How the Laws of Physics Lie* [21].

Wheter these post-positivistic strategies can be shown to be more faithful to the history of science and more adequate to the logic of theoretical inference remains to be seen. The discussion, which is only in its early stages, will involve a critical examination of the assumption that most scientific inferences rest on analogy, and, if so, in what sense. Whatever the final outcome of the debate, the benefits already accrued from this radical reappraisal of the observational-theoretical distinction make this venture one of the most promising in contemporary philosophy of science.

FOOTNOTES

1 Allen G. Debus, «The History of Science Today», *Memórias da Academia das Ciências de Lisboa. Classe de Ciências*, t. XXV (1984), pp. 89-111.

2 «We assumed that there was a certain rock bottom of knowledge, the knowledge of the immediately given, which was indubitable. Every other kind of knowledge was supposed to be firmly supported by this basis and therefore likewise decidable with certainty» (Rudolf Carnap, «Intellectual Autobiography» in P. A. Schilpp (ed.)), *The Philosophy of Rudolf Carnap*, Lasalle, Ill.: Open Court, 1963, p. 57. The best account of logical positivism is Franco Barone, *Il Neopositivismo logico*, 3rd edition, Bari: Laterza, 1986.

3 Richard C. Braithwaite, *Scientific Explanation*, Cambridge: Cambridge University Press, 1959, p. 8.

4 Carl G. Hempel, *Aspects of Scientific Explanation*, New York: The Free Presse, 1965, pp. 102-103.

5 Ernest Nagel, *The Structure of Science*, London: Routledge & Kegan Paul, 1961, p. 83.

6 Rudolf Carnap, «Testability and Meaning», in Herbert Feigl and May Brodbeck (eds.), *Readings in the Philosophy of Science*, New York: Appleton-Century-Crofts, 1953, pp. 63-64.

7 For a critical examination of these two interpretations, see Mary B. Hesse, *Models and Analogies*, London: Sheed and Ward, 1963.

8 Carl G. Hempel, *op. cit.*, p. 31.

9 *Ibid.*, p. 32.

10 *Ibid.*

11 Rudolf Carnap, *Logical Foundations Probability*, Chicago: Chicago University Press, 1962, pp. 478-479.

12 Carl G. Hempel, *op. cit.*, pp. 49-50. See the collection of essays on this topic in Peter Achinstein (ed.), *The Concebt of Evidence*. Oxford: Oxford University Press, 1983.

13 Thomas S. Kuhn, *The Structure of Scientific Revolutions*, Chicago: University Press, 1962.

14 Paul Feyerabend, «Problems in Empiricism (Part I)», in Robert G. Colodny (ed.), *Beyond the Edge of Certainty*, Englewood Cliffs, N. J.: Prentice-Hall, 1965, p. 180. See also Paul Feyerabend, *Against Method*, London: Verso Edition, 1978.

15 *Ibid.*, p. 213.

16 Mary B. Hesse, *The Structure of Scientific Inference*. London: Macmillan, 1974 and *Revolutions and Reconstruction in the Philosophy of Science*. Brighton: Harvester Press, 1980.

17 Dudley Shapere, «Meaning and Scientific Change», in Robert G. Colodny (ed.), *Mind and Cosmos*. Pittsburgh: Pittsburgh University Press, 1966, p. 75.

[18] *Ibid.*, pp. 78-79.

[19] Ses W.H. Newton-Smith, *The Rationality of Science.* London: Routledge and Kegan Paul, 1981, especially pp. 266-273.

[20] Thomas S. Kuhn, *The Essential Tension*, Chicago: The University of Chicago Press, 1977.

[21] Ian Hacking, *Representing and Intervening.* Cambridge: Cambridge University Press, 1983; Nancy Cartwright, *How the Laws of Physics Lie.* Oxford, Clarendon Press, 1983. For an empiricist alternative to both scientific realism and logical positivism, see Bas C. van Fraassen, *The Scientific Image.* Oxford: Clarendon Press, 1980.

A FUNDAÇÃO DA ACADEMIA DAS CIÊNCIAS DE LISBOA

ALBERTO IRIA

SUMMARY

The author comments on the foundation of the Academy of Sciences of Lisbon and on the important role that this institution is having in the development of Science in Portugal.

He considers that three main conclusions can be taken from the history of the Academy:

1 — the services rendered to the city where it was founded and grown as weel as to all the country. He quotes, as an example, the public free vaccination campaigns;

2 — the zeal and love of all the academicians for their country;

3 — the union and solidarity among the academicians, that has exceeded national and international crises along history.

Next, the author indicates the most important men that, with the help of the reignant Queen, organized and consolidated the Royal Academy of Sciences of Lisbon: the 2nd Duke of Lafões, the 6th Viscount of Barbacena and the Abbot Correia da Serra.

According to the Statutes of 1780 the primary duty of the Academy would be the defense of home language, as stated Teodoro de Almeida in his polemic inauguration speech on 4 July 1780.

The contribution of the Academy to the development of Science in Portugal is related to the institution of several academic prizes, as a means to support and develop creativeness of our scientists and also as a solution to practical problems, especially in the agriculture field by the end of XVIII century.

The author finishes by appealing to the preservation and divulgation of its extraordinary library, that should be brought to day light, so that the world could learn the importance of its cultural action, since the foundation until nowadays.