CHAPTER 7*

THE FORMAL STRUCTURE OF AN EMERGING SCIENCE OF EDUCATION

PART II: THE CONCEPT OF SCIENCE

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TRANSITION: Education is any and all kinds of knowledge about education. One of the kinds that is possible is scientific knowledge about education, or science of education. In Chapter 3, Monshouwer reviewed some of the rival conceptions of 'science of education' in contemporary Europe, Great Britain, and the United States. That review, in the context of Brezinka and Maccia's discussions of European contributions of educology and root suppositions of knowledge about education, established the setting for Steiner's development of the conception of educology and Christensen's explication of relationships between educology and other concepts common in discourse about education. Now, Monshouwer picks up the theme of science of education and investigates essential characteristics of an adequate conception of 'science of education'.

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1. PRELIMINARIES

It has slowly but surely become customary in our civilization for scientific knowledge to be regarded, if not as the highest form of knowledge, then at least as a paradigm for all other forms of knowledge. We are taught from childhood that scientific knowledge is better than non-scientific knowledge, as if this were really self-evident.

We will not consider what historical process led to this being considered as self-evident, nor will we consider the question of whether this process was linear or not. Similarly we cannot consider the question of whether this process always took place completely unconsciously, without due consideration. What we are concerned about is the statement (and this is not completely original) [1] that our current era is characterized to a substantial extent by a scientistic attitude.

This scientism [2] is no longer a particular view within the philoso-

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phy of science, based on a (justified or unjustified) optimism concerning the development of a certain science or sciences (such as for example during the second half of the 19th century and the first decades of the 20th century as a result of the enormous advances made by disciplines such as physics, biology, etc.). It is rather a general, basic attitude which is more implicit than explicit and which should maybe rather be explained by the unprecedented technical developments than by the advances made by sciences themselves.

Many practical effects can be attributed to this disguised or undisguised scientism: For example, the structure of our educational system which is, after all, biased towards cognitive intelligence; [3] or the irrational and exaggerated esteem which exists, particularly in Europe, for academic titles; or the fact that, as Habermas (1968a, pp. 48 ff.) asserts, to an increasing extent not only political decisions are taken by scientists and technologists (with their own way of thinking), but also technocratic thought can penetrate into the consciousness of the depoliticized mass of the population as background ideology, and can take on the function of a legitimation of practical decisions. [4] However important these and many other consequences of scientistic thought might be, we cannot go into them more deeply in this context. Only one of them is of direct importance to the issue at hand, namely the fact that the question as to the nature of science is seriously biased by scientism.

As a result of the scientistic attitude described above, every real or imagined discipline will strive to acquire the title of "science" and preferably even to be recognized as an academic discipline. Thus for years, strenuous efforts have been made, particularly in West Germany, but also in English-speaking countries, [5] to demonstrate the separate identity of a discipline of education. These attempts are without doubt based partly on the existing and continuing differences of opinion within the general philosophy of science and in particular on the distinction which we make between S_1 and S_2 (cf. below; unfortunately this distinction is not usually raised for discussion in the subject debates). But these efforts are at the same time a clear example of the phenomenon that in this matter fashionable opinions can clearly win the day over detached, meta-theoretical analyses. Thereby, a shift takes place from *prior* legitimation on logical grounds to legitimation *afterwards* [6] on the basis of accidential, contingent facts. In addition, all too often the naive principle is followed that "there is a one-to-one correlation between realms and disciplines" and that "every realm of things must be the object of some discipline," a principle which was correctly labelled by Scheffler (1966, pp. 66-68) as "clearly fallacious."

On the one hand, there is a struggle to achieve at all costs the official status of science, whether this be a so-called "science of the transcendental meditation" or the "science of education." [7] On the other hand, certain activities (such as family therapy, sensitivity training, etc.) are so intensively state-subsidized that in the long run people conclude that they are forms of science. [8]

In order to make possible a rational answer to the question, "What is a science," it is necessary to reject the scientism described above and all its practical consequences (in particular the status advantages of the title "scientific"). Only then is there a reasonable chance that the discussion will not develop into a power struggle. For this reason, I wish to use as my starting point the following epistemological thesis:

> *Thesis*$_1$ Scientific knowledge is not the only valid form of knowledge nor is it the highest form of knowledge. It is not impossible, indeed it is possible, that other forms of knowledge are more important for human existence than scientific knowledge.

2. DEFINITIONAL PROBLEMS

Soltis (1968, pp. 18 ff.) has pointed out somewhat ironically and playfully, but nevertheless effectively, "that characterizing a discipline is a most difficult if not impossible undertaking" (p. 25). His arguments give rise to the following starting point:

> *Thesis*$_2$ In absolute terms the concept of science in the end can only be defined stipulatively.[9]

This is equivalent to asserting that, strictly speaking, every definition of the concept of science is arbitrary and can neither be true nor false since it is after all based purely on an implicit or explicit agreement. We should, it is true, recognize that the concept of science can also give rise to an analytical (cf. Opp, 1970, pp. 103 ff.) or lexical (cf. Robinson, 1954, pp. 35 ff.) or descriptive (cf. Scheffler, 1960, Chapter I) definition which can be true or false since it is based on an empirical reference, namely current or former use of language. But it is clear that this form of definition can offer no solution to the problem with which we are here concerned, namely bringing a certain amount of order to the patchwork of conceptions of science, since a definition of this kind can go no further than to establish the fact that this diversity exists.

Furthermore, *Thesis*$_2$ implies that the possibility of an essential or real definition[10] of the concept of science is rejected with the principal argument that one would have to view things from a supra-historical point outside the actual practice of science in order to make such a real definition. This means that only pragmatic arguments can be used in making a choice between what will be defined as scientific and non-scientific.

Thus, although every definition of the concept of science is, when looked at in absolute terms, completely arbitrary, the *function* of a definition in general nevertheless places on us certain obligations. The

principle function of a definition is without any doubt unambiguous transfer of information. In view of the principally stipulative character of our definition, the requirement of non-ambiguity can be satisfied by as great a degree as possible of "operationalization" (cf. below). The requirement of transfer of information can be translated into the requirement that a definition must possess *as great a power of information as possible*. Now the two most important principles of power of information are as follows:

i. the more general the logical subject of a statement, the more information power it possesses (cf. Opp, 1970, pp. 166 ff.);

ii. the more falsificators (in the logical predicate) a statement includes, the more power of information it possesses. [11]

The first principle is of minor importance for our problem since in our case we are evidently talking about definition along the lines of "it is true for all forms of science that . . . ." The second principle which in some philosophies of science is often overlooked, is however extrememly important and gives rise to:

Thesis$_3$ In view of the information function of a definition in general it is advisable also when defining the concept of science to give as narrow a definition as possible.

These starting points are however by no means sufficient. Strictly speaking, with only *Thesis*$_2$ and *Thesis*$_3$ as given information, one could easily decide to define the concept of science as "the ability to draw the letter *A*." Why does such a definition sound strange despite the fact that in absolute terms it is irreproachable? This is because it conflicts with common use of language. An important factor here is clearly that science already exists as an empirical phenomenon (even if one conceives science as "referring to linguistic entities only" {Rudner, 1966, p. 8}) and that furthermore much thought has already been devoted to the concept of science.

The above mentioned principles of power of information are clearly insufficient to guarantee the functionality of a definition with the result that a third principle appears to be indispensable, namely:

Thesis$_4$ An attempt to define concepts which refer to an empirical reality should take account of the substantial functionality of those concepts, particularly with a view to achieving greater power of information.

Under "functionality" I understand here for the time being (cf. below) as close a relationship to common use of language as possible (in order

to avoid as far as possible the occurrence of surplus meanings {cf. De Groot, 1969, pp. 66 ff.}), but at the same time without detracting from the other principles of definition. Although in absolute terms this criterion of functionality is completely relative (and thereby represents a confirmation of *Thesis*$_2$) we cannot just push aside for pragmatic reasons the communicative element contained therein.

Apparently an analysis of the various meanings which the concept of science contains and contained is more important than at first appeared from *Thesis*$_2$. This brings us to the following refinement of the previous thesis:

> *Thesis*$_5$ Although in absolute terms a definition of the concept of science (being a concept with empirical references) is of a stipulative nature, an analytical element in the definition of this concept is indispensable for the sake of greater power of information (cf. *Thesis*$_4$). This does not however impair substantially the fundamentally stipulative character of the definition.[12]

For the rest this thesis does not mean much more than that in the English language you should not call a particular seat a tiger, nor a particular form of wild animal a chair.

We have hereby maneuvered ourselves into an extremely difficult position: if *Thesis*$_5$ is correct it is impossible to maintain *Thesis*$_3$ (in view of the varying opinions on what science and science of education are and considering the scientism described earlier by us) so that we would do better to end our account here. On the other hand, if we hold fast to *Thesis*$_3$ we must drop *Thesis*$_5$ unless we succeed in turning up other criteria or formulating our earlier statements in another way. In other words, if we accept anarchism in the style of Feyerband (1970) with respect to the philosophy of science, we irrevocably contradict *Thesis*$_3$; however, if we accept *Thesis*$_3$ without correction or amplification we will have to restrict ourselves to a sort of programmatic definition of the concept of science, which makes any chance of association with the current meaning of this term improbable from the start.

And yet this apparent antithesis can be resolved: In the first place it is nowhere stated that definition of the concept of science is *complete*, only that in *absolute terms* it is completely stipulative. In the second place there is no *complete* contradiction between, on the one hand, the requirement that a definition should be as narrow as possible and, on the other hand, the requirement that a definition of a concept which refers to an existing empirical entity must be a relationship to common use of language.[13]

I believe that I can resolve this paradox by specifying more closely the principle of functionality referred to earlier:

Thesis$_6$ A definition is functional if it combines as great a degree of narrowness as possible (as regards the logical predicate) with an improved usefulness.

In making this assertion I am, however, invoking a new term, namely, 'usefulness'.[14] Precisely what this term implies will only become clear in Section 3 where the "pragmatic criterion" will be discussed. At this state I wish to restrict myself to a simple comparison: we consider a traffic regulation useful so long as it is capable of optimal control of the traffic whilst resulting in a minimum of traffic accidents: similarly in science, a particular law-like generalization is accepted so long as it possesses an optimal power of explanation and is not falsified. It is clear that this criterion of usefulness demands other criteria to determine what is and what is not useful, and these latter criteria must themselves in turn be subjected to the criterion of usefulness. In other words, neither the criterion *of* usefulness nor the criteria *for* usefulness provide us with definitive information. When we narrow this down to our problem, namely the definition of the term 'science', we have to recognize plainly that the exciting element in this problem is constituted by the very fact that in the end it is indeed sciences themselves which (in their interaction with empirical reality, whatever this may be) can give concrete substance to this term 'usefulness'.

Yet this last relativization need not discourage us since, at the formal level with which it is concerned, definition has a further function which has not yet been explicitly mentioned but which is enclosed implicitly in principle (*ii*) of power of information (cf. above, p. 162). This is a function which it has in common with what is usually known as analysis and which provides a definition with a certain amount of objectivity:

Thesis$_7$ One of the most important objectives of definitions is the making of distinctions.

This thesis bears an excellent relationship to *Thesis*$_1$ inasmuch as it formulates the view that it can never be the task of a pure definition to make value judgements along the lines of "this knowledge is better than that knowledge," but that a definition must restrict itself to making formal distinctions.

3. 'SCIENCE'

In both the general philosophy of science and in discussions of the scientific status of the discipline of education we often hear (at least in Continental Europe) the expression, "alternative concept of science." This expression suggests that (and the number of adherents to this view in Continental Europe is substantial, even in academic circles) depending on one's conception of man (let us give this the dignified title of "philosophical anthropology"), socio-philosophical opinions and, in general, conception of the world, a varying concept of science can arise, not only

with regard to the social sciences, but also to a certain extent with regard to the natural sciences. This view both contradicts and agrees with *Thesis*$_2$. On the one hand, it seems to underline the (in absolute terms) arbitrary character of a definition of the concept of science; on the other hand, however, it implies that such a definition logically results from a specific philosophical anthropology or social philosophy. It quite clearly conflicts with both *Thesis*$_3$ and *Thesis*$_4$. The essence of such a "conflict of methods" [15] or "scientific pluralism" [16] within the social sciences is indeed not so much a divergence of opinion on the scientific methods to be recognized, as a differing conception of what is to be understood by the term 'science'. In such controversies, *Thesis*$_7$ is also usually disregarded, and there is an implicit rejection of both *Thesis*$_1$ and the real meaning of *Thesis*$_2$. Moreover, arguments of this sort -- relating here to social sciences and natural sciences -- often stem from a neglect of three important distinctions, namely that between "context of discovery" and "context of validation (or justification)" (cf. below), that between "science as process" and "science as product" (cf. Rudner, 1966, pp. 7 ff.) and that between pure scientific research on the one hand and, on the other, the concrete objectives of scientific research and the practical interpretation and application of the results of investigation, which are by no means completely free of value judgements.

The aforegoing leads us to the following position:

> *Thesis*$_8$ There is little to be gained from speaking of "alternative conceptions of science." When this terminology is employed, there is always a conflict between those who want to give a narrow definition of the concept of science and those who want to employ a broader definition.

An excellent example of such a conflict with respect to the scientific status of the discipline of education is provided, in the English-speaking countries, [17] by the argument between O'Connor and Hirst (cf. Chapter 3 and Monshouwer, 1978, pp. 110-116). This entire controversy can properly be reduced to a misunderstanding (mainly) on Hirst's part with respect to our *Theses 1, 2, 3, 7,* and *8*. It would therefore seem useful to analyze this controversy in a broader scientific-theoretical framework.

I believe that the major stumbling-blocks in this and similar differences of opinion are as follows:

i. the requirement of *logical consistency*
ii. the requirement of *empirical reference or correspondence*
iii. the requirement of *explanatory knowledge*
iv. the requirement of *exactitude or measurability*

and finally (logically included in *iv*, but to avoid misunderstandings nevertheless worth listing separately):

v. the requirement of *the absence of value judgements*, in other words, *objectivity*.

At first glance, demands *i*, *ii*, and *iii* would not appear to give rise to differences of opinion, because practically all disciplines calling themselves 'science' claim to adhere to them. The reason for this claim is that these requirements are capable of more than one interpretation. It is therefore necessary to define their meanings more precisely (as is obviously the case for demands *iv* and *v*, which are less self-evident at first sight).

Logical consistency (*i*) implies that a system of statements (in our case a scientific theory) must be capable of fundamental reduction to a logical calculus or axiomatic system (cf. Rudner, 1966, pp. 10 ff.; Braithwaite, 1968; Opp, 1970, pp., 214 ff., and so on). Here we will merely note in passing the fact that such a calculus does not only function as the only acceptable check with respect to the logical consistency of a scientific theory, but at the same time -- to an increasing extent within certain branches of the social sciences -- has a heuristic function. [18] If this criterion is applied, not only the majority of philosophical and theological theories, but also history and many forms of systems of statements in social sciences, are immediately eliminated as possible candidates for the title 'scientific'. It can be seen, however, that this requirement is not set too high from, amongst other things, the fact that every game with fixed rules (chess, for example), and indeed every logic-systematic or efficient action, [19] contains a calculus.

Empirical reference or correspondence (*ii*) in no way implies that scientific theories have to be a reproduction of the "empirical reality" (whatever that might be). What this requirement does imply is that all statements in the system of statements concerned must be capable of being tested, directly or indirectly, with positive results, against empirical data. [20] A centuries-old discipline like mathematics fails by definition to satisfy this requirement and cannot therefore be considered a science in the strict meaning of the word. [21] On the other hand, all theories which are based wholly or partly on mere speculation -- which includes many current educational theories -- cannot be considered for the title 'scientific', because no predictions capable of being tested can be derived from them.

The requirement of explanatory knowledge (*iii*) implies that descriptive knowledge and exploratory investigation cannot yet be considered as scientific knowledge in themselves, but at the most as a (in many cases necessary) preparatory stage of scientific knowledge. In other words, if we accept the validity of this requirement, descriptive knowledge and exploratory research only have scientific value when they aim explicitly at the formulation of justifiable hypotheses from which potential law-like generalizations [22] can be derived; [23] it is only on this basis that explanation, prediction, and technology become possible. [24] On the basis of this requirement -- at least if one accepts my strict definition of 'explanation' -- there are few disciplines within the social sciences which qualify

for the description of 'scientific'.

Exactitude (*iv*) supplements (*ii*) and implies that all statements of a scientific theory must not only be capable directly or indirectly of being tested, in the broad meaning of the word, but as far as their terms are concerned must also be *measurable* (in the quantitative sense). Because this requirement automatically contains the requirement of operationalization -- most phenomena and theoretical constructs are after all only artificially measureable -- it has far-reaching consequences for the epistemological value of scientific statements. This requirement in any case implies that the whole field of philosophy as well as still more "theories" in social sciences now fail to qualify for the title 'scientific'.[25]

The absence of value judgements (*v*) is by no means an unambiguous term. If one observes the distinction between 'moral norms' and 'technical norms' -- something done by only a few in the discussions about this issue -- it must immediately be recognized that *technical* value judgements are an essential component of science. Furthermore, *moral* value judgements play a greater part in science-as-process than some philosophers of science are willing to admit: implicit in the practice of science is the idea that science is a good thing; the choice of a subject for investigation and the financing of it presupposes that such a research project is important; the evaluation of the results of investigation involves practical decisions which are by no means always based on purely technical norms, and so on. The term 'objectivity' is also ambiguous: on the one hand reducing this concept to, for example, 'intersubjectivity' solves nothing, because the opinion of a million people is still not an objective opinion; on the other hand my previous assertions that science cannot claim to reproduce "empirical reality" (whatever this may be) and the fact that science in principle does not (cannot) provide an answer to the (metaphysical) question of whether ("objective") reality exists, form a serious obstacle to the coupling of the term 'objectivity' with the concept 'empirical reality'. What is therefore meant by the requirement of "absence of value judgements" and "objectivity" in this connection is that no subjective, personal biasing factors may play a part in the "context of validation" and that within this context there must be an abstraction from moral judgements. But this definition is still not clear enough: from a pragmatic point of view the requirement of "objectivity" and "absence of value judgements" cannot be much more than the requirement that every form of testing must in principle be capable of being checked against "empirical reality" (whatever this may be); in other words that every test must be repeatable, with the possible consequence of falsification.[26] In any case it must never be forgotten -- on pain of the scientism we have rejected -- that both "absence of value judgements" and "objectivity," in the sense that I mean, are only possible within an artificially constructed frame of reference.[27] Scientific knowledge is therefore characterized by an essential relativity.[28]

We will now stipulate that these five criteria are the minimum conditions which must be fulfilled in order to call a discipline 'science'.

But the term 'science' is, as appears from the aforegoing, so relative that we must be prepared -- bearing in mind that we have denounced every form of scientism -- to recognize as valuable other forms of knowledge which do not satisfy these criteria and if necessary to accord to them the title 'science' (albeit in a wider meaning), if people set great store by this nomenclature. We are after all concerned only with a logical distinction (cf. *Thesis*$_7$). Our analyses have thus led us to a distinction, completely free of value judgements,[29] between disciplines which satisfy these criteria (which we shall henceforth arbitrarily designate S_1) and disciplines which, wholly or partly, do not (will not or cannot) fulfill these requirements (let us call these S_2). In making this distinction we must emphasize once again that S_2 is, or can be, at least as important, if not more important,[30] than S_1 (cf. *Thesis*$_1$). We should, moreover, underline the fact that various degrees of fulfillment and non-fulfillment of the criteria in question can be distinguished within S_2.[31]

When one selects these five requirements as a starting-point and thereby arrives at a distinction between S_1 and S_2, it implies that generally speaking,[32] as far as S_1 is concerned, the more detailed approaches of Popper (1968), Nagel (1961), De Groot (1961), Hempel (1965), Rudner (1967), Opp (1970), Stëgumller (1969 ff.), Brezinka (1978), etc., must be accepted -- approaches which we will assume here are generally known and which in any case do not fall within the scope of this discussion. It is, however, essential for the purposes of our argument that the advantages and disadvantages of both S_1 and S_2 be clearly set out:

The advantages of S_1 are obvious: S_1 guarantees

1. an unambiguous definition of the concept of science;
2. a single concept of science (the ideal of a "unified science," so that the distinction between natural sciences and social sciences is dispensed with);
3. strict logical consistency of scientific theories;
4. absence of value judgements in the research phase (based on a strict segregation of "context of discovery" and "context of validation");
5. capacity for checking research and the results of investigation, and thereby a guarantee of possible falsification;
6. an unbreakable connection between explanation, prediction, and technology.

S_2 has the following advantages:

1. greater adaptation to the inherent nature of the object of research;