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;

2. the possibility of the use of not purely quantitative, but also qualitative research methods (whatever these may be), assuming no strict segregation of "context of discovery" and "context of validation;"
3. a greater relationship as regards content between on the one hand the object of research and on the other this same object as it functions in everyday life (in other words, a clearer relationship between science and everyday human praxis).

It is, however, impossible to emphasize too strongly the disadvantages of both S_1 and S_2:

As regards S_1 it can be established that

1. empirical reality (whatever this may be) is distorted or artificially labelled, insofar as only those characteristics which can be quantitatively investigated are extracted, which means, to put it another way, that an object of research is adapted to the (often fortuitous) available research methods (operationalization);
2. this raises a clear tension between "objectivity" and "relevance" (cf. De Groot, 1969, ch. 6); [33]
3. S_1 can never claim to reproduce so-called "objective empirical reality" (whatever this might be).

As regards S_2 it must be emphasized that

1. there can be no question of an unambiguous definition of a concept of science, which makes it extremely difficult to say what should and what should not be called 'science';
2. the strict logical consistency of theories is, intentionally or otherwise, made impossible;
3. elements of subjectivity are openly or secretly permitted;
4. there is a wide scope for speculation and thus an absence of means of checking (i.e. amongst other things no possibility of falsification);
5. the possibilities of arriving at a strict explanation (and thereby at prediction and technology) and very limited (although on the other hand S_2 can arrive at a fuller interpretation [34] of the phenomena studied).

That various disadvantages would be imputed to S_2 was quite obvious,

considering the aforegoing. The relativization of S_1, on the other hand, requires some amplification:

i. S_1 refuses in principle to take up a standpoint in the classic issues of nominalism and realism. As far as a certain flirtation with these questions is risked, it certainly concerns "nominalism" rather than "realism." This means that -- in contrast to varying gradations within S_2 -- S_1 will never claim to express the "truth" (in both the philosophical sense as well as the meaning which is attached to this word in ordinary language) (cf. note 20).

ii. A necessary logical consequence of the requirement of exactitude is that everything which cannot (yet) be measured exactly, cannot (for the time being) be a subject of scientific research. This means a distortion (at least a passive one) of "empirical reality" (whatever this may be) -- a conclusion which, on the basis of both (*i*) and (*ii*), can be drawn *a priori* (i.e., independently of the question of precisely what "empirical reality" is).

iii. S_1 is also guilty of an active distortion of "empirical reality" (whatever this may be) by a consistent operationalization (completely understandable in view of *ii*) of (theoretical) "constructs" (cf. Kaplan, 1964, pp. 55 ff.). It is only in the light of this that the well-known distinction between "context of validation" and "context of discovery" becomes somewhat clear and also immune to the current criticism. Despite the fact that in actual scientific practice the two "contexts" can become interwoven (in S_2 with all too few inhibiting factors because the requirement for exactitude is in most cases rejected in advance, and in S_1 normally because of sloppy thinking), the logical distinction remains unassailable between statements in which all (theoretical) constructs are made measurable and statements in which (theoretical) constructs also retain qualitative aspects, between statements which are (artificially) stripped of value judgements and statements in which value judgements can freely occur, between statements which only seek to be measurable and statements which claim to reproduce the "truth."

iv. One of the most important presuppositions which S_1 assumes, usually tacitly, is what I shall call the "pragmatic criterion" (or the "criterion of pragmatism"). Formulated as a question, this is simply: "What can I do with *P*?" *P* can be a definition, an "operationalization," a theoretical construct, a hypothesis, a "law-like generalization" -- in short, a scientific statement -- but can also be an "empirical phenomenon" or a "theory." However, the question, "What can I do with *P*," requires a more careful interpretation. It certainly does not mean "What can I do with *P* practically (in the practice of every day life) or technically?" On the contrary, what is meant here is "What can I do with *P scientifically* (in the sense of S_1 and thus taking into account the five requirements we have formulated)?" This means "To what extent does *P* contribute to better explanation, prediction, and technology?" It is clear that this criterion cannot be logically justified in some kind of form, any more than the principle of induction [35] or even the principle of simplicity. [36]

It is, rather, dependent on essentially inductive arguments, so that in fact it is based on a sort of circular reasoning.[37] This explains the fact that in the social sciences in particular -- invoking this same pragmatic criterion -- use can be made of so-called quasi-laws, which can in no way give satisfactory explanations for certain empirical phenomena, but which are capable of producing neat predictions.[38] In the light of this, the term 'usefulness' from *Thesis*$_6$ must be understood to mean: A definition (in this case of 'science') is useful when it plays a functional role with respect to better explanations, predictions, and technology -- even though it appears less valid from the perspective of a "real definition" (cf. for example Opp, 1970, pp. 104 ff.)

All in all, we have thus arrived at a stricter relativism in regard to S_1 than is usually the case in discussions of the theory of science -- certainly in those discussions which touch on a possible "science of education." It is true that Hanson (1958), with his ideas on the "theory-ladenness" of scientific observations[39] and Habermas, with his emphasis on the "transcendental interests" which underlie every form of scientific practice, seem to hold similar views. But their criticism is primarily relevant only to the "context of discovery" and the "phase of evaluation" which is logically coupled with it (De Groot, 1969). In contrast, our analyses -- like those of Kuhn (1970), who otherwise approaches the problem from a totally different angle and comes to some conclusions which we find almost entirely unacceptable[40] -- are a fundamental relativization of the "context of validation" itself, without, on the contrary, a decisive argument for abolishing the distinction between the two contexts having emerged.

On the other hand, taking into account the "criterion of pragmatism" which we discussed earlier, it must be emphasized that S_1, in view of the technological possibilities and the power of prediction which it brings with it, can in no way be pushed aside as being irrelevant.

Our relativism is therefore exclusively *logical* in nature (in exactly the same way as the objections to the principle of induction can only be formulated in a logical context without detracting from the fact that this principle, viewed pragmatically, is of extreme importance). The pragmatic functionality of S_1 is in no sense affected by it.

4. THE FORMAL STRUCTURE OF THE SCIENCE OF EDUCATION

In Chapter 3, we have seen just how many different meanings the term 'educational theory' is actually interpreted as having. And Sections 1-3 of this chapter have indicated the difficulties which are attendant on a definition in general and the problems attached to a definition of the term 'science' in particular. On this basis, let us turn to the real problem in this chapter, namely determining a standpoint in respect of the function, the goals, and the formal structure of a science of education. It must be emphasized that while we have arrived at a definition of the term 'science' in general, which is in my opinion provisionally

satisfactory, we have so far not found any justifiable definition of the term 'education', an omission which naturally forms a serious obstacle to a meaningful amplification of the term 'science of education'

4.1. PRELIMINARIES

We have set out the five main requirements of science in general. We have seen how in this respect a logical relativism is inevitable. But on the other hand we also have seen that on the basis of our "criterion of pragmatism," in combination with one of the principles of "power of information," science interpreted as S_1 produces the best possibilities of explanation, prediction, and technology. Our "criterion of pragmatism" admittedly contains circular reasoning which has not yet been more fully elaborated.[41] And our view of science is in the narrowest sense of the term, viz., S_1.

The five requirements which are the foundation of S_1 -- considering their positive results not only in the natural sciences but also in several of the social sciences (for example certain branches of psychology, economics, management science) -- can properly form the basis of what I intend to understand by 'science of education'. It must, however, immediately be noted that I am not claiming that these five requirements enable us to fathom the full extent[42] of the phenomenon of education (whatever this may be), nor even that these criteria form an adequate basis for taking concrete policy decisions. On the contrary, the science of education (conceived in the sense of S_1), because of its reductive character, is clearly deficient and will therefore have to be amplified -- specifically in order to satisfy certain practical needs.

The term 'science of education' has appeared in the heading of this chapter and more than once in the preceding pages of my text. We have thus gradually arrived at the point where we can no longer sidestep the age-old but nevertheless inevitable, indeed crucial, question of what precisely -- apart from the five requirements already mentioned -- such a science of education contains and, in particular, why we intuitively find the acceptance of the concept of a 'science of education' more reasonable than the recognition of, for example, a science of cookery.

4.2. 'EDUCATION' AND THE 'SCIENCE OF EDUCATION'

Let us start with a more detailed and precise analysis of the term 'education'. First it must be emphasized that an analysis of this sort -- like any definition which may result from it -- is much less important in the context of S_1 than is usually supposed by educational theorists. In the framework of S_1 scientific research, as far as structure is concerned, is after all not adapted to the subject of research; on the contrary, this subject is made to adapt to the five requirements on which S_1 is based.[43] In other words, the scientific approach to an empirical phenomenon is logically not dependent on the definition of this phenomenon. Such a definition of 'education' is however desirable, if we are no longer

discussing science in general, but expressly employ[44] the term 'science of education' and thereby suggest that this term has a certain distinctive character.

We have already observed that there exists a clear discrepancy regarding the concept of education between the English-speaking countries, where education is primarily if not entirely seen as school education,[45] and the countries of Continental Europe (including the Third World countries which they influence and without making any distinctions between the many contradictions in content), where the term 'education' is primarily conceived to be a parent-child relationship.[46] Where education is conceived as a parent-child relationship, not the school, but the family is considered as the most fundamental educational situation.

Although the discrepancy exists, it is not essential for our argument. As far as our scientific-theoretical problem is concerned, there is a more fundamental conflict. It cuts right across the conflict between the idea that education is mainly, if not wholly, related to schooling and the view that education is primarily family-oriented. This more fundamental conflict is the difference of opinion between those who immediately couple the term 'education' with *ethical aims*, whether or not concretely formulated, and those who base their definition on purely *formal* characteristics. The former link inextricably their definition of the concept of education with a moral aim advocated by them ("education towards a good life," "education towards democracy," "education towards the virtues made known by God," "education towards a socialist consciousness," and so on). They will not hesitate to assert that education which results in a scoundrel or a dictator or an atheist or an exploiting capitalist is not "education." The second group restrict themselves to definitions along the lines of:

> 'Education' should be understood to mean actions by means of which people endeavor lastingly to improve the structure of the psychic dispositions of other people in one respect or another or to maintain the components thereof which are considered valuable or to prevent the development of dispositions considered to be bad (wrong). (Brezinka, 1978, p. 45)

This controvery -- which is in no way a reflection of the conflict of views on the content of the term 'education' between English-speaking and non-English-speaking countries -- has far-reaching scientific-theoretical consequences with regard to a possible science of education. The "moral" viewpoint obviously cannot subscribe to requirement v of S_1, i.e. the principle of the absence of value judgements.[47] The "formal" concept accepts this requirement, or at least adopts a neutral attitude towards it.

A second source of controversy -- not divided along geographical lines -- is the question of whether the meaning of the term 'education' must be reduced to intentional influences or whether unintentional influences must

also be included. If one includes all unintentional influences (for example, the unintended influence exerted by a green-colored chalkboard as opposed to that exerted by a black one) in the subject of educational research, there is a real possibility that the science of education would have to develop into a sort of super-science. If, on the other hand, one restricts the subject of educational research to intentional influences only, one achieves, it is true, a relatively neatly defined area of research. However, at the same time, one is obliged to abandon any study of many factors which are important to "education," such as cultural influences, environmental influences, physical influences, etc.[48] This last controversy is also important from a scientific-theoretical viewpoint. The first point of view entails a science of education either based on the idea of a super-science (all empirical reality can thus be considered the subject of educational research), or inspired by the idea of interdisciplinary studies (as meant, for example, by Mialaret). The second point of view is inclined to put forward the claim of a strictly self-contained subject of research (with all its separistic implications: a science of education as opposed to sociology of education, psychology of education, physiology of education, etc. and their degradation to the status of foundational sciences or auxiliary sciences).

However, Brezinka's definition, which I have quoted literally, avoids both these viewpoints. It does, it is true, contain terms like "improve," "valuable," and "bad." But it offers no opinion as to which criteria should be applied to these terms. It does indeed mention "actions," a term which at first sight (in contrast to, for example, "behavior") does imply a certain degree of intentionality, but leaves the question of whether these actions are deliberate or unconscious. Moreover, it has the incidental advantage that it does not express the distinction between education as schooling and education as family education. Unfortunately, however, it has the great disadvantage that the terms "improve," "valuable," and "bad" are quite simply ambiguous in as far as it is not made clear if "improvement" is meant in the perspective of *ethical* ideals or if it is a *technical* "improvement."[49]

Thesis$_3$ stated that the power of a definition is increased by as strict a narrowness of the logical predicate as possible, and was based on principle (*ii*) of the two main principles of power of information which we have outlined. Principle (*i*) -- which says that the more general its logical subject is, the more a proposition gains power of information -- appeared to be trivial in regard to the definition of the concept of science we had in mind, particularly because the term 'science' from the outset referred to an artificially constructed *exploration of* empirical phenomena, so that the emphasis was primarily placed on a stipulative definition. The term 'education' however refers directly to an empirical phenomenon itself, which moreover has to function as the main logical subject of statements in the science of education. The term thus demands primarily an analytical definition. Moreover, education in this context is primarily seen as a possible subject of scientific research, specifically in the sense of S_1. This means that in a possible definition of the term 'education', emphasis will be placed on its formal structure. This emphasis

excludes differences in content, for example, those determined by ethical ideas. On the basis of these considerations, the term 'education' appears to embrace more than Brezinka's definition would suggest, although it must be emphasized that his definition is one of the most meaningful that one actually encounters in the educational literature. [50]

Education is without any doubt a form of exerting influence. From a formal point of view, there is not a single structural difference between influencing someone so that he ultimately becomes a criminal and influencing someone so that he eventually becomes a saint. From the same formal viewpoint, it is likewise irrelevant whether such an influence was deliberate or unintentional. However, the need for a certain degree of explicit or implicit intentionality must be included in a meaningful definition of the term 'education'. Otherwise, all of life, including for example all possible chemical processes, can be labelled 'education'. For the same reason, when formulating a definition of the concept of education, it is important to start from the standpoint that the influence in question stems from a human being. A situation in which a human undergoes a change in the structure of his personality because of a tortoise which happens to amble by, can hardly be described as educational. If so, then in that case, everything could be called 'education'. However, the situation in which this same human being is given a tortoise for his birthday because the giver himself loves animals and is trying to pass on his own love of animals to his friend can be called educational. Or the situation in which a psychotherapist purposely advises him to get a tortoise to help alleviate to some extent his problem of loneliness, can be called educational. Training of a human being or an animal by a human being can for the most part [51] be called education. Besides this, the principal logical conditions necessary to call something 'education' is that the change in question does not occur spontaneously, i.e., automatically, without (conscious or unconscious) intervention.

With all the reservations in regard to possible borderline cases (cf. note 51), I understand by 'education':

> *every (implicitly conscious or explicitly conscious) intentional influencing of the socio-psychical dispositions of an individual that possesses such dispositions, with the assistance of other than physical means (although these last can have a useful function), such that some change in these dispositions in close relation to an arbitrary goal (again, naturally, implicitly conscious or explicitly conscious) takes place, without this change having occurred automatically -- that is to say, without the intentional influence in question.*

Obviously this definition includes not only the usual implications of the term 'education' but also training, brainwashing, and manipulations [52] in the widest sense of the word.

It is already clear at first glance that our definition, as far as

intention is concerned, is largely in agreement with Brezinka's definition. The major difference lies in the fact that we have carefully avoided such dubious terms as 'improve', 'valuable', 'bad', and 'actions'. Also, we have placed a greater emphasis on the term 'influence'.

The combination of the above-mentioned definition of the term 'education' and S_1 with the five requirements on which it is based produces in principle the description of what I understand by 'science of education': exact explanations, predictions, and technological recommendations with respect to "education" (in the meaning of the term described above). Although we must always continue to respect Scheffler's obviously correct observations, the phenomenon of education thus formulated from a pragmatic point of view [53] and in combination with the structure of S_1 is not only important. It is also extensive enough to justify a relatively separate science of education, with the proviso, of course, that in this connection the term 'separateness' is also only acceptable pragmatically (see note 53). Structurally, all sciences, in the sense of S_1, are one as far as their formal structure is concerned.

All this raises the question of whether the current distinction between sociology of education, psychology of education, science of education, and so on, has an adequate foundation. Some people maintain that a science of education is in fact nothing more than an addition sum -- sociology of education plus psychology of education. Persuasive arguments for this thesis are readily apparent. Not only is a formal-structural distinction between sociology and psychology of education on the one hand and a science of education on the other not plausible (the logical argument), but moreover most scientific (in the sense of S_1) knowledge of the phenomenon of education is primarily gathered through both of those branches of the social sciences (the empirical argument). If we try to reduce science of education to sociology and psychology of education -- taking into account that we must continually bear in mind our broad definition of the concept of science -- and also if we want to construct a science of education *alongside* the two aforementioned disciplines, we find ourselves in an extremely difficult position. In the first sense, the term 'science of education', which appears as the leading light in the title of this chapter, simply ceases to be legitimate. In the second case -- in the perspective of the current methods of approach to the problem which we are concerned with here -- it is absolutely impossible to state precisely what the formal-structural difference is between such a science of education and the other disciplines mentioned.

It must be postulated that on the level of their formal structure no distinction can be made either between natural sciences and social sciences or between the various social sciences themselves. [54] The only possibility of introducing any relative [55] distinctions within the formal structure of S_1 in question is the following. In the same way as in the natural sciences -- bearing in mind what is stated in note 55 -- meaningful distinctions can be made between theoretical research (for example theoretical physics), experimental research (for example experimental physics), and technology; so in the social sciences one can introduce the same (always

relative) distinctions. The first two should concern themselves primarily -- albeit from different angles of approach[56] -- with explanation and prediction, while the third should chiefly concentrate on a technological perspective (for a precise definition of the term 'technology', see below).

Because any science of education -- given the definition of the term 'education' -- is primarily concerned with possibilities of exerting influence and is therefore in the final analysis interested in technological problems, this term can acquire some meaning within the social sciences, on the condition that it could be demonstrated that technology can also be called 'scientific' on the level of the formal structure of S_1.

Elaboration of these last analyses undoubtedly leads to the conclusion that within the social sciences a distinction can be made between a technology which is directed towards the "*external*[57] conditions of human behavior" (e.g., management science and administration science in H. Simon's meaning) and a technology which is concerned with the "*internal* conditions of human behavior," i.e., the socio-psychic dispositions of human beings. Considering our definition of the term 'education', it is obvious that the second form of social technology is of far more direct importance than the first, as far as a possible science of education is concerned. It may even be identical to it. It must be emphasized that the two forms of social technology are alike on a formal-structural level, such that an important discipline like decision theory, for instance is of equal importance for both of them. The distinction between them is based solely on the difference between the influence-exerting techniques (based on their various subjects of research)[58] which they develop.

My thesis is that a science of education can only derive a relative distinction with regard to sociology of education and psychology of education form the fact that it can be conceived as a social technology in the second meaning of this term. If this thesis is correct, there immediately arises the urgent question: "What precisely does the formal structure of social technology consist of?" A second question which must inevitably be answered is, as we have already noted earlier: "From the standpoint of the general philosophy of science, can social technology indeed be called a science?"

4.3. THE FORMAL STRUCTURE OF TECHNOLOGY

In this section we will deal successively with: (i) the distinction between moral and technical norms; (ii) the distinction between technology and technics; (iii) the formal relation between explanation and prediction, on the one hand, and technology, on the other; (iv) the distinction between the terms 'applied science' and 'science of applying'.

i. Meaningful distinctions can be made between various sorts of norms (esthetic, logical, moral, technical, etc.). For an analysis of the term 'technology' (and indeed with regard to the problem of the

objectivity of science in general) the distinction between 'moral norms' and 'technical norms' appears to be the most important. I think that the most important points of difference between 'ethical norms' and 'technical norms' can be described as follows: [59]

With ethical norms the goal of the norms ultimately coincides with the norms themselves ("to do right for right's sake"). The norms themselves are seen as categorical, and there is no immanent sanction attached to these norms.

With technical norms, on the other hand, the goal of these norms is extrinsic with regard to these norms themselves. In other words the norm is only considered meaningful for achieving a definite goal: "If one wants to travel to New York one must buy an airplane ticket." The norms concerned are conditional, and there is a question of immanent sanction: "If you don't buy an airplane ticket, you can't fly to New York."

It is clear that the structure of a technology -- although ethical norms play an important part in every form of scientific research -- is primarily determined by what we have called technical norms. Technology passes judgements along the lines of: "If you want to teach a six-year-old child Greek in six months, then you must abide by this or that technical norm (in this case norms of educational technics); I cannot compel you to adopt this goal, but if you do you are automatically bound by these technical norms, otherwise you will not attain your goal (or at the most will attain it accidentally)."

ii. In the English-speaking countries and in Continental Europe the terms 'technology' and 'technics' are often muddled. Yet there is an evident functional distinction between the two terms. 'Technology' relates to pronouncements in the vein of: "If you wish to reach goal P -- but whether you want to adopt this goal is entirely your own affair (the principle of objectivity) -- then you must, assuming that you want to attain your goal as quickly and as completely as possible, apply methods and techniques $\{a_1, a_2, \ldots a_n\}$." The term 'technics', on the other hand, relates not only to an actual application of the methods and techniques in question, but also to a choice (in no way objective) of the goal (although this choice can also be left to others). An extremely simple example may serve to clarify this. Suppose that I (being neither technologist nor technician of education) want my ten-year-old son to master the principles of the theory of relativity in one year, then I will turn to an educational technician, or in other words an instructor, who -- if all is well -- will have received a sound technological grounding. He will tell me that it is entirely my own affair if I wish to strive for this rather absurd goal. He will then act as a technologist and -- possibly after consulting his lecture notes -- will come to the conclusion that in order to reach the desired goal certain methods and techniques of exerting influence will have to be applied. After that he will try to make it clear to me that in his view these methods and techniques are or are not ethically justifiable (depending on his ethical principles).

Finally we will arrive at the following situation: if our technician can reconcile both the methods and techniques in question and the goal (which I have set) with his own ethical principles, he will offer -- for a hefty fee naturally -- to apply these methods and techniques to my son (≡ technics) whereby -- if his *technological* knowledge and *technical* readiness are of a sufficiently high standard -- my son will be guaranteed (or, what is more usual in this sort of case, guaranteed with a probability of *r*) to gain the insight (which I wish him to have) within the set time limit. In the second case, there will arise a discussion of ethical principles, which is, logically viewed, insoluble but which can be solved on an emotional level, and which can be carried on without the technical norms in question themselves being affected in any way as far as their technical value is concerned. The same thing can be expressed more simply: An architect (in this case taken to mean a building expert and not an esthete) is, in the framework of our distinction between technology and technics, a technologist, while a bricklayer must be called a technician.

iii. The formal structure of technology has to date largely escaped the notice of philosophers of science. Bunge (1967, II, p. 133) is quite right when he speaks of an "unexplored territory."[60] One occasionally finds in the literature a few useful analyses (the most important work in this field to date is undoubtedly Rapp, 1974), but usually, in the philosophy of science, observations on the function of technology are expressed in terms of the problem as regards content of whether technology should be viewed as a blessing or an evil for the development of a human society. This problem is essentially an ethical one. Technology's formal structure is rarely, if ever, considered.

In the aforegoing, we have seen something of what this formal structure comprises. As we have already stated, the most important question as far as we are concerned is whether technology, on the basis of its formal structure, can be called a science. I have already raised this question in a previous publication (Monshouwer, 1975). The results of my analyses in this publication can be summarized as follows:

If

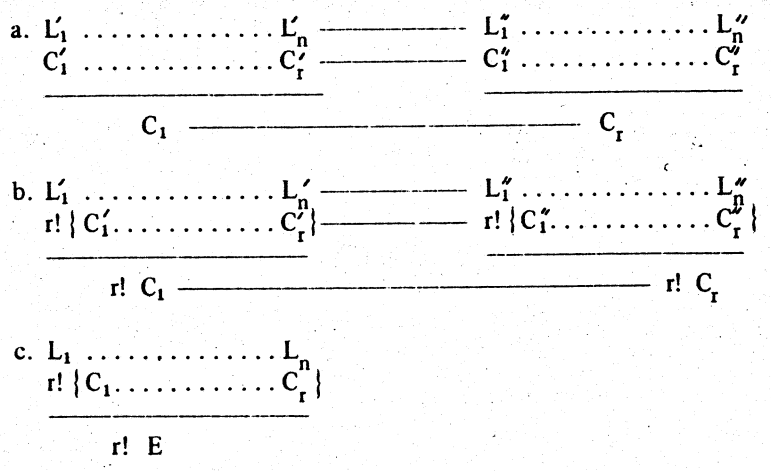
$L_1 \ldots\ldots L_n$

$C_1 \ldots\ldots C_r$

E

reproduces the formal structure of a deterministic explanation and prediction[61] in accordance with Hempel's *D-N* Model (1965, pp. 335 ff.),[62] in which *L* stands for 'law', *C* for 'empirical condition' and *E* for 'event', the specific task of technology can consist of nothing other than the *forming of theories concerning possible, purposeful realization* (N.B. not the actual realization, because we have just called this 'technics') of

the finite set $\{C_1 \ldots\ldots C_r\}$, which leaves open whether or not E is seen, for whatever reason, as desirable. Greatly simplified, the formal structure of a deterministic technology (e.g., electronics) can thus be expressed in the following formula:



in which *r!* is the symbol for 'possible purposeful realization'.

Three important points must be borne in mind:

a. One must take into account the fact that, for example, *r! C*3 can contain an impediment to the realization of an arbitrary event E_S and that on the other hand it is possible, if not essential, that for example *C*9 already exists independently of any intervention.

b. If one is to be able to speak of technology and technics, then there must be at least one *C* which can only be realized with the assistance of human intervention ('intervention' here taken in both the positive and the negative sense of the word).

c. The set of all *C*'s which can only be realized through human intervention (possibly within a specified, desired period of time), must necessarily be *finite*. If there was any question of an infinite set of *C*'s, the principle of controllability, which in different ways forms the basis of both technology and technics, would be violated.

In the social sciences, however, it is almost impossible to work with deterministic explanations and predictions. In the modern natural sciences, too, particularly micro-physics and macro-physics, probabilistic explanations and predictions are playing an increasingly important part. It is thus impossible to avoid subjecting the formal relation between probabil-

istic technology and these explanations and predictions, which are based on probabilistic laws, to a closer analysis:

If

$$p\ (G,F) = \underline{r}$$

$$F_i$$

$$\text{---------- } p = r$$

$$G_i$$

is a reproduction of the formal structure of what Hempel (1965, pp. 381 ff.) calls an "inductive-statistical explanation and prediction," then the formal structure of an inductive-statistical technology, again greatly simplified, can be formulated as follows:

$$p\ (G,F) = r$$

$$p\ (r!\ F_i) = st$$

$$\text{---------- } p = \underline{rst}$$

$$r!\ G_i$$

because

$$p\ (F,K) = \underline{t}$$

$$p\ (K,H) = \underline{s}$$

$$r!\ H_i$$

$$\text{---------- } p = \underline{st}$$

$$r!\ F_i$$

in which *p* of course stands for 'probability'.

Conspicuous in this formula is the use of a broken double line between explanans and explanandum to make it clear that with probablistic explanation, prediction, and technology -- in contrast to their deterministic counterparts -- there can be no question of a logical deduction of the explained, predicted, or desired event from the premisses which are usually called explanans for the sake of convenience, but that this logical conclusion too is subject to a degree of probability. It is therefore no longer a logical conclusion in the strict sense of the word. The probability of the correctness of this conclusion, which is not a logical conclusion, is also expressly mentioned. Very simply expressed, this means

that on the basis of the probabilistic law, "All heavy smokers get lung cancer," but with the -- not unimportant -- note that this law only has a probability of 0.9 (fortunately this high probability is of course fictitious), combined with the condition, "John is a heavy smoker," it may not be concluded that John will get 90% lung cancer, nor strictly speaking can the conclusion be drawn that John has a 90% chance of getting lung cancer. The only logically justifiable reasoning is: There exists a law which states, "All smokers get lung cancer," but this law[63] only has a probability of 0.9. Considering the fact that John is a heavy smoker, I can predict that John will get lung cancer. Unfortunately (but luckily for him) the *power of prediction* of this prediction (i.e., the transition from the explanans to the predicted event) only has a probability of 0.9.

iv. Our analyses of the formal structure of a deterministic or probabilistic technology have clearly demonstrated that there exists an unmistakable logical relation between such a technology on the one hand and explanation and prediction on the other, which plays a dominant part in S_1 (requirement *iii*). Beside this, arguments in favor of the thesis that technology does not have to satisfy requirements *i*, *ii*, and *iv* (logical consistency, empirical reference, and exactitude respectively) are not obvious. To my knowledge not one philosopher of science has ventured upon arguments of this sort. Requirement *v*, the requirement for the absence of value judgements, does present a problem -- but one which is relatively simple to solve -- because in this connection, without any doubt, the extremely important distinction between technology and technics rather abruptly comes up for discussion again. While technology by definition (it concerns, after all, pronouncements along the lines of: "If you want to realize such and such a thing -- but the ethical or otherwise responsibility for this goal is entirely yours -- then you must apply these particular methods and techniques") respects this requirement completely, it will be impossible for a technician (for example, a psychiatrist or a teacher) to avoid certain ethical decisions.

With these observations we have achieved the main aim of this subsection; namely we have demonstrated that technology (at least on the level of its formal structure) can be called science in the meaning of S_1. This means that the view that any science of education must primarily be seen as a sort of social technology has become clearly more acceptable.

The term 'technology', however, raises other, if you will, more psychological problems when there is a question of its being linked to a possible science of education.

In the first place, one must take into account the apparently ineradicable tendency to consider technology as applied science. Even Bunge (1974), one of the few well-known philosophers of science who has shown any signs of interest in the formal structure of technology, accepts it as self-evident that technology is a form of applied science. As far as

the science of education is concerned, similar opinions can be heard from, for example, O'Connor and Scheffler. It is clear that applied science -- and the very name is wrong for a start: it should be referred to as the 'applying of scientific knowledge' -- can never be called science in the sense of S_1. In fact, with this dubious term, we again come up against the problem which we have already encountered several times: of the distinction between technology and technics. Technics is indeed, in the full meaning of the word, an application of scientific knowledge with all its attendant consequences, for example, the impossibility of avoiding making ethical value judgements. It cannot therefore possibly be considered for the qualification 'science' in the sense of S_1, however logically and systematically it sets about things. Technology, on the other hand, can in no way be characterized by the term 'application', but rather deserves a name which we would like to describe with the neologism *science of applying*. Technology, which in my opinion includes, for example, electronics, science of education, and -- in a more formal area of research -- decisions theory) tries to develop scientific theories in the sense of S_1 about the way in which explanatory knowledge can *potentially* be applied technically, without actually functioning technically itself.

In the second place, in using the term 'technology', one must abstract it from all sorts of irrational secondary meanings which this term has wrongly acquired in the various cultures. With regard to Continental Europe, where the term 'technology' -- certainly as far as social technology is concerned -- often produces an immediate emotional resistance, because it is identified with ethically unjustifiable manipulation techniques and brainwashing,[64] it must be stated that social technology (although, as we have seen, the field comprises much, much more) is, it is true, also concerned with the theoretical devising of manipulation techniques, but that it only offers these entirely non-commitally and that, as a technology, it will never be allowed to venture upon value judgements like, "All sexual offenders must be castrated." As far as the English-speaking countries are concerned, where the distinction we have made between a technologist and a technician is, even linguistically, almost non-existent and where the term 'technology' in most cases is immediately linked with hardware (cf. Steiner [Maccia], 1977b, pp. 9-10), it must be emphasized that so-called hardware is only one of the many aspects of social technology. The actual construction of hardware in the strictest sense is not technology but technics. On the basis of our above-mentioned analyses, a sharp distinction between technology and technics is indispensable, precisely with regard to the formal structure of the practice of science.

5. CONCLUDING REMARKS

Starting from the view that scientific knowledge does not *per se* have to be better knowledge than other forms of knowledge, and on the basis of some definitional principles, we have arrived in the preceding pages at a very narrow definition of the concept of science, which, for the sake of convenience, we have called S_1. Our analyses have shown that

there is no meaningful argument against regarding a science of education as S_1, too, without declaring an educational theory in the sense of S_2 as impossible or undesirable. The problem arose that a possible science of education could neither logically nor pragmatically be distinguished from sociology of education and psychology of education. Our thesis was that there was only one possibility of giving a science of education, in the authentic meaning of S_1, an identity of its own in respect of these disciplines, namely by taking the approach that such a science of education must be seen as a social technology. Analyses of the formal structure of technology demonstrated that -- in contrast to technics -- it can properly be recognized as science, in the sense of S_1. This opened up the possibility of viewing a science of education in fact primarily as a form of social technology. We should here like to translate this logical possibility on the basis of arguments we put forward earlier into the suggestion that this actually be done -- although it must be emphasized that this is indeed only a non-committal suggestion, considering that it is not the philosophy of science, but the actual practice of science which has the deciding vote -- naturally taking into account the pragmatic criterion -- in this sort of question.

If we now confront this suggestion and the analyses which precede it with the ideas described in Chapter 3 about the scientific status of an emerging science of education, we can be relatively brief.

Leaving aside the fact that some people in any case dispute the meaningful use of the term 'science of education', the majority of those participants in the discussion whom we have mentioned are concerned with the field that we have designated S_2 and are therefore not of direct importance to our problem -- although we have not said and cannot say anything derogatory about an educational theory in the sense of S_2. Views of this kind in a wider framework must without doubt be considered interesting, indeed in many cases essential.

Those who do work on the basis of S_1, however, are definitely not unanimous in their point of view.

The proponents of educational research without doubt implicitly start from the principles of S_1, but more often than not seem to consider a closer analysis of the term 'science of education' as an unnecessary luxury, with the exception of, for example, Suppes and Kerlinger, although these latter also fail to get around to a total analysis.

O'Connor and Nagel who likewise apparently choose S_1 as the starting-point for their philosophy of science, in fact arrive at the -- perhaps not unjustified -- conclusion that a science of education does not yet exist, nor can one be expected in the short term. They omit, however, to make a clear distinction between technology and technics, so that the credibility of their assertions is impaired. O'Connor, in particular, expressly draws a parallel between education as science on the one hand, and engineering (here clearly regarded as technics and not technology) and medicine on the other. Both are victims of the fact that they cannot

or will not see this important distinction. It is true that, for example, medicine can to a large extent be considered as technics (a surgeon is indeed, just like a plumber, primarily a technician and not a technologist) but this fails to take into account that at the basis of every technical (in this case surgical) intervention there is a certain technology (along the lines of -- and I will repeat *ad nauseam* -- "if you want to remove this tumor -- but whether you want to do this is entirely your own responsibility -- you must apply these particular methods and techniques").

In the East Bloc countries, there will certainly not be complete agreement with our conclusions, because the distinction we have employed between formal structure and matters of content is not acceptable in these countries on ideological grounds. Actually, i.e., in the practice of science, however, this distinction is in fact adhered to.

The ideas which show the closest affinity to our views on the status of a science of education are undoubtedly those of W. Brezinka (1971 and 1978) and E. Steiner [Maccia].

Steiner's idea of an educology deserves attention, not only because in this framework she introduces the term 'praxiology of education' or because she mentions the necessity of a science of education, but also because by the term 'educology', she wants to suggest that such an important phenomenon as education cannot only be meaningfully studied by means of strict scientific methods. She calls upon other forms of knowledge, specifically, on what we have called S_2. In fact, she even demands these ("educational research is broader than educational scientific research," 1964, p. 9).

In the light of the ideas put forward in this chapter, however, we cannot avoid at least two [65] points of criticism.

1. To my knowledge, Steiner nowhere goes into what precisely should be understood by a science of education. We are told, it is true, that it is characterized by "explanation, prediction, or development" (1978, p. 4) and that in "quantitative research -- but this also includes 'philosophy of education' and 'praxiology of education' -- three kinds of reasoning are employed: retroduction, deduction and induction" (1978, p. 7). Moreover, the author makes it clear that such a science of education is "non-axiological" (1978, p. 4) and thus "value-free" (1977b, p. 9). However, in no respect are the questions answered: Why does the phenomenon of education (and not, for instance, the phenomenon of cookery) justify a separate science; what the difference is between a science of education, psychology of education, and sociology of education; what the formal relationship between the quantitative, qualitative, and performative approach in educology consists of; and on what formal grounds within the quantitative approach a distinction must be made between science of education, philosophy of education, and, above all, praxiology of education.

2. More problematic still is what Steiner calls "praxiology" -- a term which more or less agrees with T. Kotarbiński (1965) and which

-- albeit in a somewhat different meaning -- is currently being used by Benner (1973) in West Germany. Leaving aside the difficulty, already mentioned above, that Steiner fails to analyze the formal relationship between the terms 'praxiology of education' and 'science of education', she states categorically that this sort of 'praxiology of education' can under no circumstances whatsoever be considered as technology (and here she is again -- we have already encountered a similar tendency more than once -- apparently identifying technology with technics):

> Praxiology of education is not merely science of education applied to bring about educational states of affairs which are taken as valuable in the sense of having positive effect. This has been the usual conception of technology, i.e. as applied science. I use 'praxiology' instead of 'technology' to avoid the unwanted notions of hardware (in discourse about education 'technology' relates to the hardware aspect of educational practices) and of technique with its connotation of specificity. Rather praxiology is a quantitative knowledge of actions not solely derivable from science. (Steiner, 1977b, p. 9-10)

Praxiology is primarily concerned with "axiological instrumental relations" (1978, p. 4), with "valuable means" (1977b, p. 29), with "means-ends relations" (1978, p. 4), with "effective education" (1977b, p. 10). As regards this last, a clear distinction must be made between "what is *taken to be effective* in education" (one of the tasks of the science of education is "to describe quantitatively what is taken as effective," *Ibid.*) and "what *is effective* in education." It will therefore not cause much surprise when we describe these definitions -- which are the most pregnant -- as extremely vague.

Brezinka, too, emphasizes the fact that the phenomenon of education merits a broader method of approach than solely the strictly scientific approach. This is in contrast to Von Cube (1977), who in particular explicitly rejects the significance of a philosophy of education. According to him, philosophy of education can in essence be reduced to politics (p. 78). At the same time, this undermines the significance of a "practical theory of education" ("*praktisch Pädagogik*").[66] In contrast, Brezinka makes a plea -- exactly as we're aiming at, amongst other things, in our above-mentioned analyses -- for a clear distinction between the various systems of statements which are constantly invoked in the study of the phenomenon of education. Broadly speaking, we are in agreement with most of Brezinka's views, not only on the basis of our definition of the concepts of science and education, but also from the perspective of *Thesis* 1. Nevertheless we feel we must make some critical notes in the margin.

1. Despite the fact that Brezinka has formulated many things more trenchantly in his 1978 book than his book published in 1971[67]--thus he now speaks of an "application of theories to resolve technological

problems in education" (1978, p. 162) instead of "technological application of scientific theories of education" (1971, p. 85) -- it remains uncertain whether a science of education must be regarded exclusively as technology or whether it has a broader task. From our point of view the mere use of the term 'application' in connection with 'technology' arouses a certain mistrust. Moreover his latest formulation also leaves it unclear whether or not such an application is identical to what he calls the "methodical aspect" (1970, pp. 257 ff.) of his "practical theory of education," and this lack of clarity is all the more irritating because -- both in Brezinka's and in our conception -- this "practical theory of education" cannot be called science in the sense of S_1.

2. Like E. Steiner [Maccia], Brezinka reduces the function of the philosophy of education to the framing of normative-ethical and meta-ethical statements. But while Steiner does not adopt a clear meta-ethical standpoint and states only that such a philosophy of education is "the pursuit of truth about intrinsic goodness" (1972, p. 7), Brezinka makes a plea -- in our view, not wrongly -- for a fairly strict emotivism. In contrast to Steiner's position on this (cf. note 65) Brezinka's ideas in this respect have substantial consequences on the level of the philosophy of science: specifically that which he calls the "teleological aspect" (1978, pp. 254 ff.) of the "practical theory of education" is left rather up in the air.

3. The formal relationship between on the one hand "practical theory of education" and in particular the "science of education" on the other is left unclear by Brezinka. It is clear that such a "practical theory of education" can certainly not be accommodated in what we have called S_1. It can, however, perhaps be characterized as a sort of technology in the sense of S_2.

Nevertheless, despite these unclear areas -- and the same is true of Steiner's "praxiology of education" -- a "practical theory of education" of this kind is to be taken seriously, even if only to exclude in advance every form of scientism. Let us therefore try to interpret in a positive way the ideas which lie at the root of these designations.

It must first be emphasized that Brezinka's "practical theory of education" seems to include much more than Elizabeth Steiner's "praxiology" (cf. Monshouwer, 1979b, pp. 70-71). It is true that Brezinka states (1978, p. 38) that there is here a question of isomorphism, but the validity of this assertion is immediately refuted by the fact that Steiner conceives a praxiology exclusively as "instrumental-axiological." One of the four elements of Brezinka's practical education theory is "teleological," which is to say "non-instrumental-axiological" (Steiner [Maccia], 1978, p. 4). On the other hand, it is of course possible that the two do mean roughly the same thing -- not in words, but in intention. If I should have to point to an English-language equivalent of Brezinka's "practical theory of education" -- going solely on the basis of the wording used to formulate the various theses -- I should not choose Steiner's "praxiology," but would opt for Hirst's "educational theory" (cf. Monshouwer, 1978),

albeit with one -- certainly not unimportant -- comment: Hirst does not recognize a science of education alongside his educational theory, while both Elizabeth Steiner and Wolfgang Brezinka do.

In the light of the above, I believe that the function of a "practical theory of education" or "praxiology of education" can only be justified by pointing out the strongly reductive character of what we have called science of education. Through its unconditional subjection to the requirement of exactitude, a science of education speaks an entirely different language from the educational practitioner (the teacher, the social worker, the adult education specialist, the worker in special education, etc.). In the practice of education, problems will arise which will have to be translated into terms such that a scientist can do something with them. On the other hand, the results of scientific educational research, and also the philosophy of education, will be formulated in words that a practitioner in the field of education will hardly be able to understand and which, in any case, he will be unable to link to any direct practical consequences. It seems to me that the principal task of a "practical theory of education" or "praxiology of education" in this respect -- and, what is more, unhampered by the requirements of S_1 -- is to function as a sort of bridge between science and philosophy on the one hand, and practice on the other. This relationship must be regarded as reciprocal.

Finally, the question still remains as to why we speak of a science of education, but not of a science of cookery. From a logical point of view, the answer to this question can only be unsatisfactory. Let us postulate that an S_1 of cookery is possible. For the time being, I see no reason to doubt this possibility, particularly when we consider such a science of cookery -- just like the science of education -- as technology in the meaning we have described earlier.[68] In principle, every empirical phenomenon can be studied on the basis of the requirements of S_1, both with a view to explanation and prediction and in the perspective of technology.[69] As far as formal structure is concerned, there exists only one science, at least in the sense of S_1. Speaking of a science of *education* can therefore ultimately be justified only on pragmatic, and not on logical, grounds (cr. also Brezinka, 1971, p. 38). In our society, education is regarded as so important and is moreover so complex in its nature that multi-facetted research is desirable. The term 'science of education' is now nothing more than the collective term for all these researches insofar as they satisfy the requirements of S_1. It is not so much concerned with the essence of education but rather -- like the designations 'psychology', 'sociology', etc. -- with a perhaps not often realized but certainly desirable *co-ordination of research*.

FOOTNOTES

1. Cf. for example Marcuse (1964) and Habermas (1968a).

2. Cf. Strasser (1963, pp. 193 ff.) and Lalands (1960, p. 960).

3. Cf. the discussions about the Coleman Report and the words of Jensen and Jencks.

4. Habermas, 1968a, p. 81. Elsewhere he says that "the ideological heart of this [technocratic] conscience" consists of "the elimination of the distinction between praxis and technology." (p. 91)

5. Cf. the discussion above in Chapter 3.

6. In order to avoid unnecessary misunderstandings it is of the utmost importance to emphasize that by this assertion I do *not* mean to suggest that a philosophy of science should determine *a priori* the foundations and logic of science. On the contrary, as philosophical "meta-theory," philosophy of science also has a "second-order character" (cf. Peters, 1966, p. 60).

7. On the one hand, the "Frankfurter Schule" has exposed this often ideological character of scientific-theoretic discussions. On the other, the representatives of this school have failed to avoid the same pitfall by presenting their views (which are in essence philosophical and therefore non-scientific) as "science" with a view to giving the concept of science an extremely broad meaning.

8. Von Cube (1977, p. 8) comes to similar conclusions. His idea that we are here concerned with a sort of political infilitration, however, seems to me doubtful.

9. Stipulative definitions are definitions by agreement and can therefore be neither true nor false. They are not in logical contradiction to analytical definitions, but they are in logical contradiction to "essential" or real definitions.

10. "Essential" or real definitions are not recognized by science in the sense of S_1, but are reduced to disguised analytical definitions, stipulative definitions, explications of concepts, unfounded empirical generalizations, and so (cf. Opp, 1970, pp. 104 ff.).

11. "Falsificators" means here "exclusions of uncertainties." Thus a logical conjunction will be more information than a logical disjunction, as can be clearly seen from their respective truth tables (cf. Opp, 1970, p. 167).

12. It is obvious that all analytical definitions can ultimately be reduced (both logically and historically) to an arbitrary agreement.

13. In fact these same problems also occur in "operationalization" or "operational definition." De Groot (1969, p. 169) mentions in this connection a possible "tension, or rather *mutual contrariness of, objectivity and relevance*."

14. I am not of course using this term in the trite, everyday meaning of the word, but more or less in the meaning which it has in Pragmatism (cf. Peirce, Dewey).

15. Cf. Adorno *et al.* (1972) and Büttemeyer/Möller (1979)

16. Cf. Von Cube (1977, pp. 45 ff.).

17. As far as Continental Europe is concerned, we can also refer to the Marxist concept of science and the discussions in West Germany (cf. Chapter 3).

18. The heuristic function of a calculus consists of the following. A calculus is first framed, and only then looked at to see whether such a calculus can be interpreted empirically. In fact, in most cases, this amounts to the use of mathematical models.

19. For example, reaching the exit of a building as quickly as possible.

20. The term 'empirical data' is an extremely relative one. Hanson (1958) rightly pointed out the "theory-ladenness" of scientific observations and, with it, testing. In science in the sense of S_1, "operationalization" (not to be confused with the school of thought in the philosophy of science which is called 'operationalism') plays such an important part that it may properly be stated that what is called "empirical data" in science is always based on a reduction of empirical reality (whatever that may be). In general, it can be stated that science by definition refrains from taking a position in the controversy between nominalism and realism or -- perhaps more accurately -- feels itself more akin to nominalism than to realism. Science in the sense of S_1 will never be able to claim to reproduce "reality." That such reproductions are not ruled out, indeed even occur frequently, can be seen from the fact that on the basis of strict scientific theories rockets can be sent to the moon, certain diseases can be cured and people can be led to prefer canned soup to homemade soup.

21. Our *Thesis*$_1$ prevents us from attaching any pejorative connotation to this observation. Scientific practice after all shows that science in the sense of S_1 is impossible without mathematics.

22. Cf. Rudner (1966). This term is sometimes used to indicate that laws -- considering their logically unjustifiable inductive basis and their claim of universality -- can never be verified, but at most can have a strong confirmation value (cf. De Groot, 1969, Ch. IV).

23. Explorative and descriptive research (for example, an inquiry) have no scientific value in themselves, but have such value only in the perspective of possible hypothesis-forming.

24. Cf. Hempel (1965, pp. 331 ff.). The formal structure of a scientific explanation as well as the formal relation between explanation, prediction, and technology will be of the utmost importance for our further argument. (Cf. below.)

25. These analyses also fail to qualify, since they belong in the philosophy of science and are therefore meta-theoretical in nature.

26. For the term 'falsification' or 'refutation', cf. Popper (1968) and amongst others, De Groot (1969) and Brezinka (1978).

27. The key concept here is again "operationalization" (cf. note 20), logically based on the requirement of exactitude.

28. The well-known discussions amongst Kuhn, Popper, and Lakatos (Lakatos / Musgrave, 1970) and Stegmüller's commentary (1973) must be left out of consideration here, because they have little or no bearing on the main points of our argument. Their great importance for the general philosophy of science must, however, be acknowledged without reservation. Feyerabend's views (1970) have no significance at all for the thread of our narrative. It is true that they contain criticism of the claim to be able to give a "real" definition of the concept of science (in which we -- considering the undermentioned and what follows -- are in agreement with Feyerabend), but on the other hand Feyerabend repudiates without any reason the possibility of applying specific distinctions (completely non-committal or, if you will, free of value judgements) with regard to a possible definition of the term 'science'.

29. "Absence of value judgements" in the double meaning of (i) logically justifiable, and (ii) independent of the distinction between "better" and "worse" knowledge.

30. One may here apply all possible criteria to the terms 'important' and 'more important' (except of course the five requirements we have listed, because this would lead to triviality).

31. In a future publication, I hope to analyze more closely these various degrees on the basis of the operative scientific concepts.

32. I do not assert that the undermentioned philosophers of science have precisely the same views. As far as secondary questions are concerned, this is certainly not the case. But even with regard to primary matters, some of them also have the occasional difference of opinion.

33. Cf. note 13. By this I mean the following: Starting from the re-

quirement of exactitude, in an investigation into, for example, "power structure," all non-measurable variables which relate to the concept of power will have to be left on one side. Suppose that only one single variable (e.g., income) is measurable. Then an investigation into the distribution of income, be it ever so objective, will have only very limited relevance to the actual question, viz., power relationships.

34. For the distinction between interpretation and explanation, cf, De Groot (1969), pp. 42 ff.

35. Cf. Popper (1968), Ch. I, 1.

36. Cf. Popper (1969), Ch. VII.

37. Along the lines of: *P* has proved (=induction) to contribute to better explanation, prediction, and technology, therefore *P* will, for the time being, continue to occupy an important place in my theory. The reason for this is that experience (=inductive argument) has taught me that explanation, prediction, and technology produce valuable knowledge (at least in my opinion). My conviction that this is so is in particular due to *P* (=induction).

38. Cf. Albert (1970), pp. 131 ff.

39. Cf. also Suppe (1974), pp. 151 ff.

40. My greatest objection, however, does not concern so much the analyses of Kuhn himself, which after all seem plausible in many respects, but rather the conclusions which many interpreters have drawn from them. In particular, it utterly escapes me how anyone can draw *direct* conclusions in the area of the *logic* of science from *historical* analyses, correct or otherwise, of the development of science (and then, what is more, primarily concerning physics).

41. On the one hand, explanation, prediction, and technology are justified as a goal of scientific knowledge by the pragmatic criterion. On the other hand, explanation, prediction, and technology are the logical bases on which this criterion is founded. (Cf. also note 37.)

42. This is, of course, also true of the natural sciences, which for example, reduce a piece of music to vibrations and thirst-quenching water to H_2O.

43. This assertion is yet another formulation of the reductive transforming character of S_1. This observation does not, however, hold good for the so-called "context of discovery." Thus, observation techniques will try to adapt themselves thoroughly to the subject of research, and therefore a distinction between psychiatric diagnos-

tics and astronomical observations is inevitable, indeed, meaningful (and in which, of course, Hanson's "theory ladenness" continues to play an undiminished part).

44. For the rest, such a definition will usually be pre-scientific (cf. Strasser, 1963, pp. 70 ff.) in nature, because an operational definition has little meaning in this connection.

45. A good example of this is the description given by E. Steiner [Maccia] (1978, p. 1), who understands by 'education' the 'teaching-studenting process'.

46. Cf. for example Lalande (1960), p. 265.

47. And with it the other four requirements, which must at least acquire a wider meaning than the one we have suggested.

48. It must be borne in mind here that someone can act intentionally without being fully conscious the whole time of the original goal; in other words, when the action has become a habit and the goal has become implicit (or integrated in the habit). The accidental influence exerted by the color of a door is a phenomenon which can be clearly distinguished from this.

49. For the distinction between moral norms and technical norms, see below.

50. Most educational theorists in the English-speaking countries -- leaving aside exceptions like E. Steiner [Maccia] (see note 45) -- refrain for the sake of convenience from venturing upon a clear definition. In the course of this year I hope to publish an article on this subject. As far as the definitions of the term 'education' which are customary in Continental Europe are concerned, cf. Brezinka (1974a, pp. 34-99).

51. This is not the place to work out in detail where precisely the borderline lies.

52. It is not out of the question that by the application of certain manipulation techniques someone can develop greater creativity, and indeed even learn to get on more independently with acquired knowledge.

53. See below.

54. For all those who have understood what we mean by S_1, this observation is completely superfluous.

55. The relativity of these distinctions on the level of the formal structure is equally valid on the practical level. Such sub-disciplines maintain a complete cohesion with one another and in fact

are only a concession to the necessity for a certain division of tasks.

56. The one will place more emphasis on, for example, mathematical models, the other more on experimental research.

57. I am aware that the terms 'internal' and 'external' are not models of clarity. However, I believe I can assume that what is meant by them is sufficiently understandable in the context. In any case, this is not the right place to go into them in more depth.

58. Cf. note 43.

59. I have used this distinction in my lectures since 1964. Habermas, too, (1968a, pp. 62 ff.) works out this distinction, albeit from an entirely different perspective and in somewhat different terminology.

60. We cannot here go more fully into Bunge's analysis (1967, II, pp. 132-147).

61. The formal structure of explanation and that of prediction are not completely identical. The latter must properly be reproduced as follows:

$$L_1 \ldots\ldots L_n$$

$$C_1 \ldots (C_t) \ldots C_r$$

$$E$$

Prediction always includes a time factor: in order to speak meaningfully of prediction, at least one C must be not yet realized and at the same time it must be possible to indicate with greater or lesser certainty when this realization of (C_t) will take place. When I know that the last as yet unrealized condition (C_t) will be realized in precisely one week, I can also predict E with the same margin of time.

62. Criticism has been levelled at Hempel's *D-N* Model from various quarters (cf. for example R. Harré, *The Principles of Scientific Thinking* (London: Macmillan, 1970) and A.A. Derksen, "*Poespas rond verklaren in de sociale wetenschappen*" ("The fuss about explaining in the social sciences"), Internal paper of the Institute of Philosophy and History of Education, University of Nijmegen, The Netherlands, 1974). Because we only need Hempel's model to demonstrate a close relationship between explanation, prediction, and technology, we see no need to give more detailed analyses of these sorts of criticisms, particularly since to my knowledge none of the critics in question denies this relationship.

63. It concerns here a lawlike generalization which claims to a reason-

able extent to be universal and not a purely descriptive pronouncement along the lines of "90% of all Dutch smokers get lung cancer."

64. One thinks, for example, of the horror with which Skinner's approach was received in Continental Europe.

65. Her reduction of the task of a philosophy of education to purely ethical-normative problems (cf. Monshouwer, 1979b, p. 65 and p. 69) -- like similar views held by Brezinka, see below -- also gives us cause for criticism, but this question does not come within the scope of this chapter.

66. Von Cube (1977, pp. 61 ff.) makes a plea for a "*konstruktive Erziehungswissenschaft*," which fits very well in the formal structure of what we have called social technology. This "constructive science of education" must be concerned with "the development and optimalization of strategies with regard to education, or teaching and learning." He takes as his starting-point for this a narrower definition of the term 'education' than we have used.

67. Cf. Von Cube's criticism (1977), pp. 77 ff.

68. All the same, there are clear differences. A possible science of cookery cannot possibly be called a technology of exerting influence.

69. Scheffler's argument (cf. above in Chapter 3) is less self-evident than it appears at first sight. What is more, he does not give a single criterion on the basis of which it can be determined which empirical phenomena do and do not make possible a "separate" science of . . . .

REFERENCES

The references for Chapter 7 are listed with those for Chapter 3 on pp. 78-85.