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Finalization in science *

Three factors have given rise to the need for scientific reflection upon science: the exponential growth of science, the inadequate theoretical foundation of science policy and the problematic consequences of the applications of science.

The expansion of science — Derek de Solla Price has calculated that the number of scientists is growing three times as fast as the world population — has to an increasing degree become subject to limitations. The rise in the social investments necessary for continued scientific growth will increasingly provoke political resistance, especially as there is no dependable procedure at present for analysing the scientific results of increased outlays [1].

The attempts to rationalize science policy by a systems approach to the determination of priorities and by the use of analytic methods have so far been futile. It is not clear what elements and processes in science can be consciously regulated, and by what means this could be accomplished, nor are there any procedures at hand to control or to evaluate the effects of the venture.

Scientists themselves to an increasing degree tend to view the orientation of science towards economic, military and infrastructural goals as being problematic. On the one side this reflects their concern that this orientation may hinder or distort theoretical progress, on the other it reflects the insight that as long as the prevailing goal-orientations remain operative the demand for a socially relevant science must remain unfulfilled.

It is our assumption that the self-examination called forth within science by the impact of these problems both reveals and promotes a fundamental change in the relationship between science and its social environment, a kind of paradigm change of science itself. We intend to designate this paradigm change as "finalization" in science. "Finalization" is a process through which external goals for science become the guide-lines of the development of the scientific theory itself.

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The concept of finalization

1. *Finalization in science as an historical process*

What phenomena do we have in mind when we use the term "finalization" in science? Two examples may provide an initial point of reference. Aerodynamics, a sub-field of physics, may — *e.g.* under the pressure of a growing awareness of environmental problems — turn to the development of a theory of "noise". "Noise", however, is not a physical concept, in contrast to "sound" which is the subject matter of acoustics. "Noise" embodies elements of subjective perception and of social valuation. Disciplines as different as physics, physiology and labor medicine all share in its definition. Were aerodynamics, therefore, to endeavour to work out the physical representation of the noise quality of sound and to formulate a theory explaining the processes by which noise is generated and by which it can be reduced, then an external purpose would become the guide-line of its theory.

Chemical engineering *(Verfahrenstechnik)* offers another example. Research in chemical engineering (the handling and optimization of the technical apparatus and industrial processes of chemical manufacture) has up to the 1930's stayed on the level of the phenomenological conceptualization of fact gathering and the organization of empirical data. After that time chemical engineering became the subject of basic research. This was possible due to breakthroughs in some fundamental fields (theories of kinetics and absorption). On that basis the problem area of chemical engineering, which is mainly defined by industrial needs, could be integrated into fundamental theory. A special physics and physical chemistry emerged, the theoretical programme of which was the explanation and handling of complex technical systems on a molecular basis (*cf.* Buchholz, 1974, p. 23 ff.).

It is our assumption that the category of "finalization" will serve to reveal the structure of those scientific developments which are characterized by their linkage with social, military, and economic purposes, but which are not adequately described by the traditional category of "applied research". Attempts have been made, from different perspectives, to conceptualize this development of science as a "fusion of R and D", as "project-oriented science" (Hafele 1963), as "science as a productive force". In a more or less phenomenological description, more precision can be given to our conception of this mission-oriented science by stipulating four historical conditions governing finalization.

a) Objects which are of relevance to economic, military, medical or other purposes have themselves become increasingly laboratory phenomena. It was science which first produced or made accessible electricity, magnetism, nuclear man-made fibres, pharmaceutics. Fewer and fewer new procedures for handling and controlling such phenomena can be developed at the level

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of skills and inventions based on craftmanship and traditional engineering. They require scientific inputs, in fact the development of the theory of the respective subject matter in line with the respective mission-orientation.

b) In many cases, the theory development for particular object areas is the form in which science and external goals are linked together (*e.g.* noise research in aerodynamics, research on controlled nuclear fusion in plasma physics). This implies the scientification of the specific object area and not merely applying to it research results (laws, data) achieved in some other field[2].

c) The scientification of fields which are defined by external purposes is based on a "mature" theory. For the time being we shall designate as "mature" theories which within their explanatory programme have formulated laws from which sufficiently precise and reliable predictions can be derived for the subject-matter the theory addresses. Examples of this are classical mechanics, inorganic chemistry, quantum theory, molecular genetics, whereas the theory of cell organization is an example of a theory which has not matured.

d) Finally, a condition which is closely tied to that stipulated under *a*) : Modern science produces techniques. The power engines and machine-tools which induced the industrial revolution, many of the techniques of metallurgy, medicine and of scientific instrumentation were not, until late in the 19th century, the achievements of scientists, but of practitioners, of inventors and mechanics[3]. With the rise of the industries of chemistry and electricity during the 19th century this relationship was gradually reversed. Since then the technique-producing role has increasingly been assumed by science (by theory). This holds true in all fields — for the production of energy and of commodities, as well as for military, medical and scientific technology (the development of scientific instruments and apparatuses). Since this capacity of science has become evident the demands on it to provide problem-solutions and techniques for determinate purposes have steadily grown. In part, this is an effect of science itself. By virtue of the role it has come to play in augmenting the scientific-technical complexity of society it increases the dependence of society on further scientific progress.

2. *Demarcations*

The above set of historical conditions for finalization does not yet constitute a systematic derivation of the characteristics of finalized science. Nonetheless, it can serve to demarcate finalization in science from other, earlier forms of mission-oriented science, and to identify its precursors among them.

Regarding our thesis that finalization in science is a phenomenon of the present development of science it might be objected that sciences have always been brought forth and furthered by external purposes: medicine, statics,

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hydraulics, meteorology, organic chemistry — not to mention sciences oriented to the reinforcement of social control, such as economics, or of institutional legitimacy, such as historiography. Yet until the end of the 19th century, we will find in none of these sciences (with but a few exceptions) the close connection between the development of theory and the utilization of science which is the essential trait of finalization.

In some cases (*e.g.* mechanics, hydraulics) the theory was developed after the techniques and was more concerned with their systematization than with their refinement. In others (*e.g.* medicine, meteorology) no appreciable theoretical advance was made until the 19th century. These disciplines achieved stability in spite of the inadequate state of their theory because of their practical successes or, if nothing else, because of a strong interest in the practical solutions they seemed to offer. Finally, in chemistry, wherein major theoretical advances had been made even before the 19th century, they did not occur in connection with any possible applications, and sometimes they reduced chemistry's practical significance [4].

To be sure, from the 19th century onwards it was to an increasing degree the scientists who supplied the technical knowledge needed for solving miscellaneous problems. Even so, they did not, as a rule, do this by establishing a new theory especially for a relevant field. In part this was done by inventive steps, perhaps planned but non-theoretical, proceeding by trial and error [5]. In part the scientists also merely transferred the findings of theoretical study to practical problems [6]. In none of these cases did goals set from the outside become the guide-line of theory.

First examples of finalization are then found around 1850. Agricultural chemistry is one example. Liebig and others, from 1840 on, establish and institutionalize it as a special development of chemistry in which on the basis of an external goal — namely: increasing agricultural yields without exhausting the soil — the interaction between soil, atmosphere, plants and animals as well as the agricultural variation of natural cycles became scientific problems. Without the external goals of increasing yields, chemistry would not have had to deal with these subjects (*cf.* Krohn and Schäfer 1976).

Towards the end of the 19th century it becomes virtually impossible to draw clear distinctions between the application of scientific results and theory development since by then the application of science itself had, in many areas, become theory-intensive. Applications thus were turned into "applied research" requiring theory-inputs on an ever increasing scale. The production of techniques by science then assumed the form of special theoretical developments for specific fields. At that time theoretical engineering sciences came to be developed (*e.g.* for machine engineering, low temperature technology, chemical engineering).

What we have expounded as the structure of finalization in science, namely the openness of science to external purposes becoming the guide-lines of theory, is visible in the developmental process of natural science as a whole, in the

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course of which — starting with physics and going on to chemistry — the disciplines of biology, physiology, and possibly even of psychology and sociology, will become "hard" sciences achieving objectifying theories for their subject matter [7]. Thus, for instance, in the programme of molecular biology, the cell is incorporated into the explanatory propositions of chemistry. Yet the "cell" is not a chemical concept and cannot be completely translated into the categories of conventional chemistry. From the vantage point of normal chemistry the explanation of the cell is an "external" goal, which can only be assimilated if chemistry goes on to become molecular biology with its own methods, theories and models. Then the external purpose, "the cell", will be internalized by chemistry as it becomes the guide-line to its theoretical development [8].

However, the form of goal-setting exemplified above is not identical with what we have in mind when we speak of finalization in science. The goals that guide theoretical development in this case are apparently neither arbitrary nor can they be intentionally disposed of. The tendency that seems to operate in the above process of the extension of the explanatory statements of chemistry to the biological cell is one of successive scientification of all experience according to the paradigm of experimental and nomological science. In the process of theory formation conceived on the model of physics, which appears to have gained ground successively in biology, physiology and psychology a theory program immanent to natural science is realized.

The above tendency of the development of natural science runs counter to the original "anthropofugal" tendency (Krüger, 1970) of modern physics. The concept of science postulated by experimental physics at the start of the modern era has introduced the eccentricity of man into the scientific *Weltbild.* The diffusion of this notion to all areas of experience puts man and the human world again into the center of natural science. This is a process whereby external goals are internalized by theoretically advanced disciplines and come to act as the guide-line to their progress. However, the content and the consequences of such goals are determined by the inherent structure of theoretical evolution which is realized, so to speak, behind the backs of the individual scientists, as an unconscious and natural process.

Admittedly, such a sharp division between the anthropocentric orientation of the theory programme of natural science and finalization in science is problematic. First, there is some evidence suggesting that the anthropocentric orientation of theory development is no longer merely a resultant (*i.e.* the outcome of efforts to fill in voids in the theory of underdeveloped disciplines by transferring the theories of related fields), but that it has become a strategy consciously adopted by scientists (often with a reductionist colouring) to extend well-established theories to further subject areas [9]. Second, finalization in science can only be explained against the background of the underlying anthropocentric orientation of theory. Which external purposes can

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be internalized to act as guide-lines to theory, and which conditions render this possible does depend on the extent to which the theory programme of natural science has evolved in terms of anthropocentric orientation.

3. *Replacement of Darwinism in the development of science*

If external purposes become the guide-lines not only for the "off-springs" of scientific progress, *i.e.* the applications of research, but also for the underlying theory, then it will become possible to replace Darwinism in the history of science by a political-strategic control of science. The history of science may be characterized as Darwinistic insofar as decisions on alternatives in science have occurred which can neither be accounted for by the deliberate strategies of scientists nor perhaps by cogent criteria of scientific rationality (truth), but have to be explained as resulting from influences in the environment of science. (*Cf.* Kuhn 1963, Böhme, van den Daele, Krohn 1973.) The Darwinism of science is but "factual"; for it seems possible to rationalize scientific progress by conscious planning and in our time this has become a necessary task. This is a presupposition underlying the practice of all science policy. However, the theoretical weakness of science policy so far is that the possibility of political control is seldom based on insights into the conditions for scientific growth and on an evaluation of the historical status quo of science. Consequently, it is open to the objection, on the one hand, that it proceeds from a decisionistic philosophy of science and, on the other hand, that it blocks scientific creativity by policy planning. The concept of finalization in science is an attempt to describe which properties of the existing structure of science can produce conditions necessary for a research planning based as much on the interests of science as on the needs of society.

This is not to imply that according to criteria of political regulation a mission-orientation will be achieved which is structurally different from that caused by external parameters of selection in past epochs. The influence, however, operates in a different manner: as soon as the internal mechanisms of scientific progress (or, to put it in a more neutral way, of successful scientific work) become transparent they can be strategically employed. Theories which so far, under suitable conditions of the socio-cultural environment have, so to speak, grown "naturally", can now be designed as strategies. "Normal science" then will no longer be confined to exhausting the respective field of knowledge in accordance with scientific viewpoints, but, with the help of a kind of cartography of the philosophy of science, will be able to traverse the terrain in the direction of the goals set by policy.

4. *The decline of self-regulatives*

Finalization in science presupposes a specific latitude of science towards alternative developments; it implies openness to the social or political deter-

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mination of the course it is to follow. The existence of such latitude is the basic premise for all attempts to control the consequences and the utility of science. We do not intend to investigate the alternatives which actually exist for finalization in present-day science, nor do we investigate the political and institutional conditions under which any specific alternative may be realized. We intend only to conceptualize the development of cognitive structures in science which permit alternative external orientation. The question we therefore are asking is, what makes such alternatives possible or what has made them possible in the past.

Alternatives in science can be defined as possibilities of scientific development that are not excluded by the logic of inquiry (the complex of all the self-regulatives to which scientific growth is subject). Yet the self-regulatives themselves are subject to historical change: they may be modified by the scientific progress which they regulate or by changes in the norms upheld by the scientific community. Any hypotheses regarding alternatives in science have therefore to formulate presuppositions as to the structure and range of the self-regulatives operative in science. Only these offer the prospect that existing alternatives will not only be discovered, but at least in part be accounted for.

There are, to our mind, three possible approaches in analyzing the decline of self-regulatives in science:

a) With the increasing accumulation of knowledge of law-like relationships and by certain disciplines having reached a stage in which their theory has been completed to a certain extent ("theoretical maturity" of physics and chemistry, for instance) the power of the self-regulatives of the logic of inquiry to determine the direction of theory development diminishes. Decisions as to the theoretical relevance of further research have to resort to additional criteria relating to the purposes of research.

b) In different areas of research we can observe a disqualification of the claims of self-regulatives which so far have enforced a certain autonomy of theory construction, *e.g.* a partial abandonment of the goal of causal explanation. The underlying reason may, possibly, be that a highly developed causal theory offers more than is in fact required for the application of science. Causal explanations then are pursued only to a point which is necessary for controlling functional interrelationships. This can be exemplified by research in pharmacology, psychology, sociology, economics and systems research. The result is a specific type of external orientation of scientific development which we have called "functionalization".

c) Finally, in some disciplines, the results of which are extensively applied technically, the claim to the universal validity of scientific knowledge is being questioned by the practical effects of science. The claim to the general vali-

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dity of scientific knowledge is predicated on the reproducibility of the scientific experiment as the warrant of its truth. The technical application of science, which is the actual repetition of the experiment, reveals the limitations of this claim. The changes that are effected by the actual *making* of experience in the realm of experience itself (see *e.g.* the effects of DDT) are systematically excluded from the realm of scientific knowledge and truth. This weakness is corrected when natural sciences begin to differentiate into ecological disciplines. The ecological disciplines demonstrate a specific faculty for allowing social norms to make themselves felt within the structure of science.

Only the first of these approaches is expounded in the present paper to a greater extent. It is considered to provide the essential elements of the structure of finalization.

Finalization as a consequence of the theoretical completion of a discipline

The potential of a scientific field for further development and the effectiveness of the internal norms which regulate its growth depend on the stage of development it has achieved. We assume "theoretical maturity" to be the property by which the integration of external goals into the theoretical research programme of a field is made possible and is required.

We shall outline this in a three-phase model: a science passes during its growth through various phases which in a different manner are "open" for its development to be oriented towards external goals.

Before any general theory of its subject is in sight, a science more or less resembles the experimenting of the amateur inventors at the beginning of the modern era: the research follows no definite theoretical or methodological programme; scientific strategies are widely determined by techniques of inquiry, the main goal being discovery, not explanation (pre-paradigmatic science in the sense of Kuhn, 1962). In this pre-theoretical phase, a discipline has to some extent the possibility of selecting its research problems in line with given external goals.

As a science gains maturity, more and more the theoretical interest itself will define the orientation of research strategies and of scientific development. Kuhn has described this process as the emergence of a paradigm. We would prefer, with Lakatos, to call it the progressing of a research programme. A paradigm does not emerge all of a piece; rather, one or several fruitful approaches induce a phase (in our model phase 2) in which through a succession of theories the development of the field reaches some kind of completion, that is a fundamental theory by which all the problems in the respective area of research are resolved "in principle". In this phase the scientists' attention is focussed on the difficulties presented by the theories so far incomplete and in part inconsistent. To be sure, in this phase, too, there is the type of science

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Kuhn calls "normal science". Even provisional theories, if they are of any value for the dynamics of inquiry, must suggest special experimental and theoretical questions. The development of the discipline, however, is not determined by these specific questions but rather by the necessity to work out the solution of problems of principle in order to arrive at a fundamental theory[10]. Besides, it seems that this pattern is repeated at a higher stage, where a series of "provisional" paradigms advances the development of a whole science toward a sort of super-paradigm, which then becomes fundamental to the entire science. This is the interpretation given by C.F. von Weizsäcker to the history of physics and to the particular role assumed by quantum theory[11].

As long as a science has not developed a fundamental theory and is characterized by frequent crises with brief intermissions of normal science, attempts to link the advance of science to external mission-orientations offers little promise. Of course, scientists can be forced to work on applied problems on a phenomenological or trial-and-error level. However, for theoretical mission-oriented science the required basis in fundamental research is lacking. There is not only a lack of the specific fundamentals of the problem under inquiry but a lack of the very general theoretical foundations of the total field.

The theoretical problems demarcate a clear-cut frontier of science to which scientists are committed. At the level of the universal theory which is to be developed it is impossible to differentiate knowledge in terms of the purpose it is intended to serve. To put it sharply: at the level of the law of the conservation of energy or of relativity theory no theoretical criterion exists for distinguishing between military and civilian research. In all theoretical crises of a science, therefore, possible mission-orientations become inconsequential. The theory itself is the only effective guide-line of scientific advance and the only one meaningful for the progress of knowledge.

This changes, however, as soon as a universal theory has essentially been completed and a stable and universal paradigm set up for a particular research area. Then, for a relatively long phase (phase 3 in our model), the discipline in question is basically characterized by normal science: the fundamental theory is specialized, differentiated, and modified in order to extend its range of applications[12]. The further differentiation has advanced the less its direction will be prescribed by theory. Expressed schematically: the fundamental theory is a focal point at which all the knowledge accumulated throughout the previous developmental phases is bundled into general laws and from which theoretical differentiations can evolve in any direction whatever. If we were to take physics as an example then the differentiation process would cover *e.g.* solid state physics, plasmaphysics, low temperature physics, all of which developed within the paradigm of quantum theory[13].

Once this point is reached an external goal of research can become the regulative of where and with what intensity theory will further develop. This

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gives rise to alternative theories, not only to different lines of research, all of which can be viewed as vehicles of one and the same theoretical progress. Thus, for instance, from the general Newtonian mechanics tidal theory on the one hand and a theory of heat, such as statistical mechanics, on the other, were derived two totally different theories which arose from the interest in particular types of phenomena. In this case, however, the orientations did not follow from social interests. The latter become discernible, for example, in the case of hydro- and aerodynamics. These fields, being sub-fields of applied mechanics, can certainly be understood to have themselves gained theoretical maturity some time ago: they have reached a stage where by means of a couple of equations (Stoke's law, Bernoulli's equation, etc.) all problems are resolved "in principle"[14]. The social interest in specific themes has induced the development of the general theory into various directions. Thus, technical and military interest have promoted the aerodynamics of aircraft construction, the environmental problem of noise pollution fostered aeroacoustics, and the needs of cardiac research will eventually lead to the development of some physiological theory of fluids.

For a science which has passed over the threshold of theoretical differentiation into specialization the methodological neutrality of theory regarding the purposes of research becomes looser "...the scientific abstraction from final causes becomes obsolete in science's own terms. Science itself has rendered it possible to make final causes the proper domain of science" (Marcuse, 1964, p. 232). Marcuse's formula can serve to explain the fact that while our three-phase model is formulated on the basis of the traditional concept of theory, it is precisely this foundation which is affected and modified by the realization of phase three.

If it is an original element of the "neutrality" of theory that its explanations of the properties of physical objects are of so general a nature that they are in principle not correlated with any social group or political interest per se (but can only be adopted by means of social power, something for which science bears no responsibility) then this neutrality is changed because the subject matter of the phase 3 disciplines becomes more concrete, or rather more phenomenal. With growing specificity of the research field, of the explanations and technologies based thereon, the correlation of science and social interests becomes more specific, too. The more specific the goals and the more closely they tie in with specific social interest, the more "neutrality" will disappear from theory development. Theory becomes subordinated to political strategy[15].

What is now required is a step by step theoretical specification of this model to explicate the implied hypotheses.

a) *What characterizes a theory as being the fundamental theory of a discipline?* Contrary to the assumptions of fallibilism (Popper, Feyerabend), the history of science exhibits the surprising phenomenon that theoretical developments

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arrive at a completion. Scientific theories may gain not only provisional acceptance but may also acquire classical status. Heisenberg has apostrophied such theories as "closed theories" (1971, p. 87 ff.). Roughly speaking their characteristic feature is that they no longer can be improved by minor modifications. Substantial changes, however, give rise to an entirely new theory. (The classic example of this is the relationship of classical mechanics and relativity theory.) Closer to the essence of the problem, however, is Heisenberg's statement that in the transition to such a new theory the "closed theory" is not invalidated. The closed theory is valid for all times; as long as experience can be described in the terms of the theory, be it even in the remote future, the laws of the theory will always be correct (Heisenberg 1971, p. 93). Such theories define the constituent structures which characterize a particular object area as such. By implication, therefore, once such theories have been built, it should be possible to establish them as being transcendental[16].

When we speak of fundamental theories we may refer to the phenomenon of closed theories as a major example. Fundamental theories already contain the basic structure of their subject matter. The further development of the discipline which deals with this research field will, therefore, be more phenomenon-oriented, more strongly determined by a practical interest in the subject matter. In this way Newtonian mechanics were developed into a theory of the planetary system, into aero- and hydrodynamics, and into statistical mechanics.

The next question then is:

b) *How should we conceive the relationship of specialized theories to the fundamental theory? And: what is the status of such partial theories in relation to the empirical data and to the theory of other fields?*

Phase three in our model has not been conceived as an application of the findings of a fundamental theory, but as a way of concretely embodying it in a special direction of development. The theories of the sub-fields arising from differentiation are not in general logically derivable from the "mother discipline". As a rule they are not even reducible to the mother discipline once they have — in whatever manner — been developed.

The relationship with the fundamental theory may for instance be constituted by some method of approximation, the meaning and validity of which can be justified only on the grounds of particular problems or even of practical tasks. (Methods of approximation used to solve general equations in aero- and hydrodynamics may serve as an example.) Or, to give another instance, the transition to the "daughter discipline" may take the form of a transition to statistical entities. It may be found that the latter are governed by laws which are not confirmed by the fundamental theory conceptualized for the elements (*e.g.* the irreversibility of thermodynamic processes as against the reversibility of the mechanical processes in which the former are

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"grounded"). A general review of these "intertheory relations" is beyond the scope of the present essay[17].

For our present purpose it will suffice to establish that although the transition of research to special theories is based upon fundamental theory, nonetheless, it cannot dispense with empirical strategies, experimentalism, inductive model constructions; on the other hand, it has, within the limits set by the general theory, to go through the whole process of theory development. Accordingly, for the special disciplines new scientific self-regulatives that limit the possibility of their external control arise also: namely systematically open problems (*e.g.* in solid state physics, which is a speciality of quantum physics, the problems of crystallization and catalysis, in hydrodynamics, which is a speciality of mechanics, the friction of soft media).

The establishment of such special disciplines leads, however, to an increasing restriction of the field and hence is incomparable with the theoretical development of the fundamental discipline. The theoretical relevance of the questions asked diminishes according to the degree of specialization. The special disciplines often constitute the scientification (theorization) of special technologies. At their open frontiers they attain empirical knowledge (*e.g.* tables of measurement in engineering), which, if it were reconstructed theoretically, would lead to perhaps highly complex but at the same time superfluous and trivial research. Consequently, the theoretical developments of the special discipline exhaust themselves scientifically and sooner or later shift into techniques based on empirical knowledge which is not grounded in theory. (*Cf.* the transition from solid state physics to science of materials.) The development of this type of science primarily finds its legitimation in the external goals for which its results are to be used. To be sure, special theoretical developments may yield a substantial spill-over for a general theoretical advance but, accepting the assumption that any scientific development will produce some fall-out, this in itself will not suffice to substantiate the relevance of any particular special development.

c) *In what way does the structure of the open problems in any discipline depend on the state of development of its theory?*

According to our three-phase model two things are possible: first, that a discipline has not yet gained maturity. In that case it is primarily concerned with solving its theoretical problems, and with developing a fundamental theory by resolving these problems. Its development will be determined by this frontier of research, the open problems function as a binding programme. Out of the broad spectrum of what we do not know, special open problems are selected as being relevant to the discipline. The perspective of scientific work is to eliminate theory deficits and deficiencies. A deficit means that a theory does not adequately cover its subject matter. (This is, for instance, the case in today's physical chemistry, which has so far not supplied a sufficient quantum physical and appropriate mathematical description

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of chemical phenomena.) A deficiency means that a theory contains conceptual inconsistencies or produces anomalies. (A deficient theory was, for instance, Bohr's earlier atomic theory, the heuristic value of which was generally accepted, although it was evident that the conceptual difficulties it raised would make theoretical modifications necessary.)

The second possibility is that a discipline is in principle completed; in that event further theoretical problems, and thereby, finalizations, will depend on the emergence of practical problems. This is exemplified by classical mechanics (as already noted) and by quantum physics. This is not intended to imply that in disciplines in which the theoretical development is completed (in Heisenberg's sense) there is no perspective of further theoretical advance[18]. But the open problems of the fundamental theory themselves become a specialty of the discipline. They no longer have any structuring function; rather, the development of the discipline requires selecting from new problems those which can be theoretically dealt with on the basis of the fundamental theory. Such a selection is indispensable, in order to avoid, for instance, triviality in the development of the discipline; it cannot, on the other hand, be itself effected according to the theoretical standards of the discipline[19].

On closer examination, the above classification into purely theory-guided and purely goal-oriented instances, requires further differentiation: it should be borne in mind that, frequently, theoretical problems can only be raised for analysis once a scientific development has been started for external purposes.

In space research, for instance, we find a constellation of external causes opening a new field, of internal problems the solution of which advances fundamental theories, and of external purposes which set off special developments within the field. The factors that gave rise to empirical outer space research were external ones, with economic, technical and military interests predominating. However, once space research was established, the scientists raised problems that bore largely on the development of an empirical theory of the planetary system, the solution of which is seen as a pre-condition to the scientific progress of empirical cosmology. At the same time, however, diverse external mission-orientations entered into the program of space research and its theoretical problems which in turn gave rise to special developments in the field of space research (*e.g.* metallurgy, solid state physics)[20].

The functionalization of science as a consequence of the interest in the control of complex systems

We can observe a particular tendency in the science system to establish the knowledge of functional relationships as a legitimate end of scientific inquiry. The recognition of functional relations, for instance in an input-output model, has at all times constituted an important step in scientific advance. Yet

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it was always looked upon as being merely the "pre-form" of the scientific explanation itself, which was expected to establish the necessity of the relationship asserted by disclosing the mechanisms underlying it. Pharmacology offers an illustration: pharmacology largely proceeds functionally by finding "causes" for specific effects (*e.g.* identifying drugs which eliminate headache), without uncovering the mechanisms of these effects and without theoretically explaining them. Admittedly, this is considered to be a theoretical deficiency and it is hoped that eventually biochemistry, neurology, etc., may fill in these explanatory gaps (considered important in particular with a view to the early detection of side effects).

It appears, however, that functional thinking can better satisfy the scientific regulatives of any discipline the more complex its subject matter; *viz.*, the learning theories in psychology, systems theory in sociology, as well as the theoretical approach of the type of Forrester's *World dynamics* (1971). This reduction in theoretical claims corresponds strikingly to the technical interests operative in the management of complex subject matters. This interest is no longer directed towards reconstruction — the reproduction of automats — but is directed towards control and regulation — the modification of behavior, crisis management, etc. Functional theories characterize the type of science which is appropriate for a *strategic* age. A complex system cannot be reconstructed unless its mechanisms are understood but it can be handled strategically, provided, whatever mechanisms may be operative, its most important functional structures are known. Of special relevance to the problem area under discussion is that the emergence of sciences with reduced explanatory claims renders possible a fusion of science and technology and an orientation of scientific development towards external goals without the interposition of a phase of theory construction in the traditional sense. This is possible because the lack of explanatory power of the science does not preclude the utilization of its findings for specific purposes (of control). As a type of theory it can only gain acceptance in the scientific community if the classical scientific standards of causal explanation (and of quantification as well) become less rigorous.

In order to explain this transformation of theory into a systematic strategy, a more thorough analysis of the relationship between functional and causal nexus is required. Presumably until now, no real process has been causally explained. Only relations idealized by isolation have been explained[21]. The discovery of any particular causality follows from a particular question and provides precisely the answer to this question. Moreover, causality has an intensive dimension: the degrees of specificity and the range of the explanation are not determined "objectively", but by the range of the question that can be articulated theoretically[22]. Consequently, the characteristic of the functional as against the causal approach is not a disregard of the complexity of reality but the lack of a regulative which continuously advances causal explanation. The cognitive interest addresses itself from the very beginning to

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the utility of the apparatus, to the functional system. Causal explanation becomes auxiliary to this interest. This seems to hold true for the investigation of the biological cell, as well as for noise research, or for global systems research.

Functionalism, therefore, is a tendency enhanced by the complexity of the object. Without recourse to functionalism complex objects cannot be structured scientifically nor are they always sufficiently relevant to causal theory formation. Increasing complexity is connected with increasing individuality. The more complex an object the less its infrastructure will be identical with that of other objects — the less the general scientific relevance of its explanation will be. The political-strategic interest in functional knowledge seems to be paralleled by a corresponding scientific interest. Causal analysis is only extended to elements of the object that can be reconstructed whereas the remaining elements are taken into account solely as functional variables. This tendency is also apparent in the traditionally "hard" natural sciences. It remains to be seen whether such a functional approach will ultimately prevail if causal explanation is adhered to as the principle goal of scientific investigation. The greater the complexity of the areas to which natural science research is extended, the more science will be compelled to work with macro-variables in order to organize the multiple and complex interrelationships [23].

There can be no question that functionalistic macro analysis as a short-cut to the utilization of research is important for science policy. To obtain effective control over complex objects without being bound to the advance of scientific theory of the object ought to be a worthwhile pay-off for any science policy.

This reinforces a tendency to utilize prematurely scientific findings; side effects or long-term consequences of having applied the knowledge may at the most be empirically registered but can not be anticipated theoretically. The difficulties arising from this kind of external orientation — which is feasible without investing in theory — may correspondingly become apparent in the capacity it is producing: the increase of planning capacity combined with an accumulation of relative ignorance will augment the system's susceptibility to crisis. It is not clear, moreover, whether functionalization does not excessively exploit the scientific resources, *i.e.* that use is made of the available knowledge without it being possible to provide for coming needs in science by timely investments [24].

The limitation of the claims to the general validity of scientific laws and the development of natural science into a normative science

In the discussion of functionalization we mentioned the limitations of scientific generalization that follow from the concern with complex and, in a sense,

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unique objects of inquiry. In what follows, some consequences of this will be developed further.

It has until now been a tenet of the logic of inquiry in the natural sciences that scientific knowledge can be generalized. Once this becomes questionable, the form of the classical law must be modified. A tendency towards degeneralization is most conspicuous in the ecological sciences but shows itself also in those disciplines, which are concerned in some manner with the conceptualization of evolution. If reference is made to the contradistinction Rickert has made between the ideographically proceeding historical sciences and the nomothetically proceeding natural sciences, then the process of de-generalization can be designated as a process of the "historization" of natural science. It is suggested that this term is applicable in a sense going beyond the problem of method.

The most elementary cases are the fields explicitly concerned with "history of nature": cosmology and Darwinism. Here the singularity of the object is obvious: the world and any biological species occur only once. This has methodological consequences, going beyond the de-generalization of the scientific propositions: causal statements cannot assume the form of experimentally verifiable hypotheses; no arrangement of controlled experimental conditions is possible.

There can, however, be a more complex case: solid state physics manifests a tendency to fuse with mineralogy. We have suggested above that the specialization of disciplines on the basis of a fundamental theory implies a concretization of the subject matter towards a more phenomenal approach. If that is correct, the scientification of mineralogy should at the same time be a concretization of solid state physics in the direction of objects, the arrangement and individual environment of which approximates them to historically singular cases. For it is precisely the phenomenal reality which no longer should be cancelled out by scientific idealizations, but be maintained as much as possible. It is an open question whether the concern for concrete objects may eventually lead to the introduction of evolutionary categories into solid state physics; the more scientific analysis will hold strictly to the concreteness of its object, the more historical its conceptualizations.

Of particular interest to us is the case of what might be described as degeneralization of scientific knowledge by the historical impact of this knowledge. The universal validity of scientific knowledge is constituted by the fact that the experiments establishing it can be reproduced and repeated. The following case will make clear the ambivalence of this condition: reproducible experiments establish the validity of the claim that the chemical substance DDT acts as an insecticide. In actual fact these experiments were repeated, perhaps millions of times, not in the laboratory but through their technical application. Yet, it is precisely the repetition of the experiment that has invalidated the proposition that DDT is an insecticide since its being repeated has led to the selection of resistant insect strains. How then can

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the "reproducibility" of the scientific experiment establish the truth of propositions if the actual repetition of the experiment invalidates the proposition? The theoretical underdevelopment of natural science is reflected in this dialectic: its incapacity to integrate the effects of scientific knowledge into the concept of this knowledge and thus, for instance, to conceive temporal limitations to its validity or to quantify the degree of its reproducibility. The premise of unlimited reproducibility implies a presupposition which is explicit in the above mentioned theoretical incapacity of natural science: nature is conceived as an infinite reservoir for which the possible effects of any number of testing experiments will always be a negligible quantity[25].

Some disciplines, it is true, may have reacted to the limitations to an effective, *i.e.* a technical generalization of scientific knowledge. They planned to conceive the subject as a system into the structure of which the effects of scientific knowledge may somehow be integrated. This is, however, only the beginning of the recognition that the scientific goal to know what is (or occurs) must be theoretically supplemented by the goal to know what the knowledge will cause to be (or to occur). Only when the operations of knowing can thus be taken into account in the concept of knowledge, will science in theory reach the level of its praxis[26].

De-generalization of scientific knowledge is something that seems to be connected with the increase in complexity and concreteness of the object area. The greater the differentiation of objects and the more limited, consequently, the scope of the natural "reservoir", the more likely it is that the technical operations of the production or reproduction of experience are of significance for the existence and the constitution of the object of inquiry. A concept of nature which may be consistent with a little developed technology is invalidated by the effects of natural science itself: by the technology in which the instrumentalism of science is fully realized. Nature, not as a whole, but in its partial processes is something which cannot only be controlled and dominated, but something which also can be changed and destroyed. Until now the adequate concept of nature expressing these limits has been formulated outside the sciences as political or as moral restrictions to their applications. The perspective of nature as an infinite reservoir for intervention cannot be abandoned as long as the theories of natural science do not make it possible to integrate the historical impact of such intervention into their concepts. Therefore, scientific knowledge will continue to refer to reproducible possibilities, even if the presuppositions of these possibilities are undermined precisely by the process of their reproduction.

The modern interest in ecology as a science is a reaction to the results of the "construction of nature" by the scientific-technical interventions of man. Ecological analysis has to define its subject matter as a reproduction process (system). This implies on one hand conceding that nature has norms (goals) of its own — the distinction between "is" and "ought" is appropriate even within the frame of natural science itself. On the other hand, however,

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since man himself and nature constitute interdependent systems for which stability and reproducibility are required, it seems to be impossible to determine even those natural norms without a recourse to some human norms for the intervention within nature. The reproduction of nature is no longer determinable solely in terms of natural variables. The definition of the nominal values of natural systems will increasingly be mixed together with "human" determinants of nature, *i.e.* with forms and purposes of man's development of nature. Ecology, thereby, develops from an objective natural science in the traditional sense to a science with normative, strategic elements, supplying evolutionary concepts of nature. It thus becomes open to the analysis of processes by which nature reproduces itself in terms of alternative goals.

In ecology, and even more so in systems analysis in the social sciences, the concept of "goal" has entered into the conceptual field of norms[27]. However, the recognition that purposes or goals exist in nature cannot as such be equated with a normative approach. After all, the teleology of natural systems hinges only on the interest of understanding the stable self-regulation and the evolutionary determinants of natural complexes, on the methodological problem of organizing the data correspondingly. Norms, on the contrary, contain not only the element of functional regulation but also attributes of value. The latter, however, can only become relevant within the development of the natural sciences once the "purposes of nature" are, at least in part, at the disposal of the social system. And this itself depends on the extent to which science provides a potential with which nature can be technically manipulated. The capacity to command nature is evident especially in the ecological sciences: they are not only concerned with understanding the natural processes, or eliminating disturbances, but also with designing new cycles of reproduction[28].

Ecological thinking is, so far, a minor strand of scientific development and it remains to be seen whether it will have any effect on the concepts and strategies of the traditional natural science disciplines and its related technologies. It seems, however, that in such areas as climatology, geophysics, oceanography a description of the natural status quo and more so an analysis of possible developments has to a certain degree to consider factors that are derived from the technical strategies man is implementing in nature (*e.g.* changes of climatic circles due to thermal pollution, of earth tectonics due to programmes of water regulation, etc.). A conceptual analysis of such cases is still to be desired. It will, however, get little help from the traditional philosophy of science, since the fact that social norms may be correlated with natural purposes and incorporated into the concepts of natural sciences is not accessible to the analytical philosophy.

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Perspective

We have attempted to devise some strategies for investigating the emerging new relationship of scientific structures and social interest. We should not conclude, without trying to figure out what the self-understanding of the scientific community of this new constellation might be — even though it might only be possible to speculate. Two alternative developments seem possible:

Scientists may extend the communalistic and universalistic ethos of scientific research to the social relations of their research. This would imply more than simply a claim for collective and discursive (democratic) procedures. This appreciation was established at the origins of modern science: there was a pretended conjunction of the universal claims of the bourgeoisie (to promote general welfare) and of science, rooted in the universality of reason which was invoked both for its theoretical aspect (the equal ability of all citizens to reason) and its technical one (the general practicability and utilization of experiments). Furthermore, it is precisely the restrictiveness of bourgeois society which also limits the universality of science: private control withdraws productive as well as innovative social functions from decision making by democratic processes. It is therefore doubtful whether the goals that become effective for scientific activity are also rational goals of society. The claim for universality inherent in scientific thinking is opposed to such forms of systematic privacy and in favor of a social structure in which the path of science can be traced on the basis of a rationally generated social consensus.

The possibility of such a move is especially visible in the structure of normatively oriented sciences. Their external goals are neither objects nor methods but the resultants of social valuations: if the external goals determine the theoretical structure of the disciplines the latter can establish their generality only if the social valuations of such norms are grounded in *general* claims. Where natural science is normative, the point of reference of scientific generality should be universality in society, not in nature. Thus generality in the form of unlimited reproducibility would yield to the generality of social consensus. This, however, is possible only if the theoretical concepts and perspectives of a discipline make it possible to represent the social consensus in its historical contingency within science. The de-generalization of the traditional scientific claims as indicated above is one presupposition for this.

A quite different (even opposite) development of the science system is possible on the ground of the same initial conditions. Finalization in science not only makes the generality of social consensus a possible point of reference for scientific generality, but at the same time removes the traditional resistance of theory towards external control. It thus renders the hope illusionary that theoretical development and conformity of science to power might in principle be incompatible since political adaption of science of necessity would

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lead to theoretical sterility. "Finalization" means precisely receptiveness to theoretical internalization of external goals. Within this structure science can in principle maximize both its conformity to power and its services for particularistic social interests without degenerating itself as a useful science. The possible achievements of such a science go even further: by internalizing the stabilization of power as a goal into its theory it renders domination itself scientific. Its conditions and structures are developed according to immanent scientific standards — independent of the changing definitions social groups might give to them. (*Cf.* on this, the role of industrial psychology, sociology of organizations, pedagogics — which are to provide motivations, consensus, apathy, "tolerance" for inconsistencies, etc.) The adapted science will understand the conditions of power better than those in power themselves. Should this kind of scientificness come to dominate the science system then the emancipatory phase of modern science has been but an episode.

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Notes

* The three papers published together in this issue were presented at the Project Parex meeting at Starnberg (Germany) in December 1974. Project Parex is a European programme of pluridisciplinary cooperation in the social studies of science. The Project is coordinated by Clemens Heller (Maison des Sciences de l'Homme, Paris), Gérard Lemaine (École des Hautes Études en Sciences Sociales, Paris), and Roy MacLeod (University of Sussex). The secretariat is located at the Maison des Sciences de l'Homme.

The main focus of the Starnberg session was on the "finalization" in science thesis. The papers should be considered as contributions to an on-going discussion.

This paper is a corrected version of an article which was first published as "Finalisierung der Wissenschaft" in *Zeitschrift für Soziologie* 2, 1973, pp. 128-144. The term *Finalisierung* especially when translated into the English *finalization* is sometimes misunderstood. It is derived from the traditional category of *causa finalis*, its connotations then are the goals or purposes of science not the end of science.

1. The respective estimations by (de Solla) Price (1963, p. 92) have not escaped criticism; *cf.* Toulmin, 1966. Regarding the problems of applying cost-benefit analysis see Klump, 1972.

2. The history of the production of artificial dyes provides a good example of both ways of relating science to purposes. The English chemist Perkin discovered the aniline dye mauveine in 1856 when he tried to synthetize chinine. One year later, he started its industrial production. Later Hofmann and Kekulé provided the theoretical foundations of a chemistry of dyes and thus made the systematic discovery of dyes possible (*cf.* Haber, 1958, p. 81).

3. "Neither the textile and coal industries, nor the railways and shipping as such, depended on science or contributed much directly to its advance." "The engineering industry, though more linked with science than the making of metal, still remained largely outside the main scientific movement of the nineteenth century" (Bernal, 1953, pp. 141, 145).

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4. The ambitious theoretical efforts of R. Boyle to build a concept of the chemical element using the rationality of mechanics failed to produce any technical results in his period (Partington, 1961, p. 496 ff.). The chemistry of colours which had been a very early link between scientists, artists and technicians (with traditions reaching back to the Renaissance) became disconnected from theory by the success of the quantitative chemistry of Priestley and Lavoisier. Chemistry entered on a programme of isolating and transforming substances and the chemical investigation of colours was postponed till the end of the 19th century.

5. The work of J. Wedgewood may be regarded as a prime example of such procedures during the industrial revolution. Wedgewood was called the "scientific potter". He discovered the ceramic "Jasper" after countless experiments. "His trial pieces are said to have run well over ten thousand" (Schofield, 1963, p. 94.).

6. *Cf.* the above example of the discovery and industrial exploitation of the first artificial dyes. The development of the electromagnetic telegraph (indeed of modern communication devices, telephone, telegraph) was a direct result and a transfer of results from the scientific doctrine of electricity (Oerstad, Schweigger). *Cf.* on this Singer *et al.*, 1958, vol. 4, pp. 654-655.

7. This must not be taken to mean that these sciences will simply be reduced to physics. The meaning of "objectifying theory" will itself undergo changes in the course of this development.

8. It thus becomes an internal goal of the then established cell chemistry. This transformation of external to internal goals can also be found in the transition from aero- and hydrodynamics to noise research. Within the process of finalization the so far external goal becomes the research programme of a modified discipline and defines internally open problems and theoretical deficiencies.

9. *Cf.* for instance, the efforts to proceed from cybernetics to a theory of the brain and to a comprehensive general systems theory.

10. A good example of such a development is provided by the transition from Bohr's model of an atom to Heisenberg's quantum theory. *Cf.* the case study of Lakatos, 1970, p. 140 ff.

11. Von Weizsäcker, 1971, part II, chap. 1: "Die Einheit der bisherigen Physik", part II, chap. 3: "Die Einheit der Physik als konstruktive Aufgabe" and part II, chap. 4: "Ein Entwurf der Einheit der Physik".

12. The development of the fundamental theory of a field is characterized by periodic crises, whereas the specialization proceeds according to well established methodological, theoretical and professional standards.

13. Whereas field physics, elementary particle physics and cosmology are areas which still aim at the development of new fundamental theories.

14. This does not apply to problems of turbulent fluids. Even apart from this there seem to be some unresolved fundamental problems, such as the friction of soft media.

15. From the point of view of the postulate of value-free science (*cf.* Weber, 1968, p. 582 ff.) this development might be regarded as perverting science. The thesis of "finalization in science" is, however, to indicate that the orientation towards external goals is conditioned by the development of theory itself. To be sure, the emerging explicit relation of science to society demands that the processes of consensus formation and unrestricted information, which have been constitutive for the social process within the science system, be extended to its relations with the controlling social and political agencies. A closer analysis of the cases that are usually put forth to demonstrate the dangers of relating science to social goals (such as Lyssenkow or the "German physics") shows that they failed precisely in complying with this condition (Medwedjew, 1972; O. Scherzer, 1965).

16. This was first tried by Kant with respect to Newtonian mechanics. Today von Weizsäcker (1971) is attempting the same with respect to quantum theory.

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17. The philosophy of science has only just started to investigate them systematically (*cf.* Bunge, 1970). In Germany the group of E. Scheibe and L. Krüger (Göttingen) studies them within a special programme of the Deutsche Forschungsgemeinschaft (*cf.* Krüger, 1973).

18. The conclusive criteria of what constitutes a finished theory are difficult to set forth, *cf.* for this Weizsäcker's postulate of "semantic consistency" of a finished theory, 1971, p. 193 ff.

19. The term "finished theory" must not lead to the conclusion that with such a theory (*e.g.* quantum theory) physics itself is finished. The open problems of, for instance, integrating quantum and relativity theory do constitute a classical research frontier, the solution of which will produce new fundamental theories. This point, however, illustrates to what extent the dynamics of physics have already shifted to oriented special developments: the remaining fundamental problems are neither the main reason for the present support of this science, nor are they in the center of educational or occupational interests in physics.

20. We do not deal with the question whether the relatively low theoretical significance of space research can justify its excessive costs or whether the theoretical interests which scientists do articulate are biased by professional self-interest or industrial commitments.

21. The following example may emphasize the difference between functionalization and the classical procedure of idealization: Newton attempted to calculate the precise ballistic curves of cannon balls. He was able to take into account the resistance of air and the specific weight of the metal but had to neglect the irregularities of the cannon itself and of its elevation mechanisms (*cf.* Schneider, 1970). Due to these idealizations his calculations remained useless until at the end of nineteenth century when cannons could be built that met the ideal assumptions. Today an irregular equipment could be described with sufficient data. It thus could be disposed of in scientific calculations (without investigating the various causal relations) since such calculations depend on the amount of data (not causes) which can be handled.

22. The discovery of the germ cells, for instance, provided a causal explanation of the process of reproduction as long as processes on the level of chromosomes and later of molecules could not be analyzed.

23. This is today evident, for instance, in cell chemistry. Although most scientists assume that a causal analysis (in terms of chemical and physical laws) is "in principle" possible, they regard it as impossible to handle the amount of relations that would be implied in such a causal description.

24. A closer analysis of "functionalization" is provided in van den Daele and Krohn (1975); *cf.* also Schanz (1974).

25. This is perhaps not true to the same extent for the physics of quantum theory, which essentially takes into account the changes that are induced within the recognized nature by the very act of recognition. This would, however, mean that natural science in general has not yet reached the methodological level of the best of its theories.

26. The connection of recent ecological crises with the traditional operative thinking in the natural sciences and the techniques based thereon is emphasized by P. Shepard: "Sometimes naturalists propose projects too, but the project approach is itself apparently the fault, the need for projects a consequence of linear, compartmental thinking, of machine-like units to be controlled and manipulated. If the ecological crisis were merely a matter of alternative techniques, the issue would belong among the technicians and the developers" (Shepard, 1969, pp. 9, 10).

27. Norms in this context are not used in the sense of Parsons and others as internalized rules of social behaviour, but as conscious rules of technical action, that originate from scientific insight and from political interest.

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28. One of the earliest examples of the scientific construction of reproduction cycles in nature is probably agricultural chemistry since 1840. In the normative relation with nature it takes into consideration that its cultural norms have to be rules of nature as well. Prior to the scientific analysis of agricultural chemistry, higher agricultural yields were either obtained at the cost of a destruction of nature (exhaustion of soil) causing substantial secondary effects, or nature was left in a "natural" state, the price being a scarcity of food (*cf.* Krohn and Schäfer, 1976). Today's debate on ecological problems reveals the fact that agricultural chemistry acted prematurely in many respects in the cultural standardization of nature.

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