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Chapter 9

MODELS FOR THE DEVELOPMENT OF SCIENCE

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INTRODUCTION

'Science policy' is generally understood to mean the strategic behavior of politicians in relation to science. But the term 'policy' need not be used so restrictively. Decisions regarding the formulation and execution of science policy are by no means the exclusive purview of the agencies of government. They are also made by scientific organizations, by research managements, and above all by scientists themselves, and not just by their choice of research themes. In point of fact, most of science policy making is performed by the scientists themselves via the activities of advisory boards, of funding organizations, and in the ongoing decision making processes of research centers.

The usual conception of science policy as dealing with the external direction of science leads to scholarly research on such topics as the relation of science to politics, and its finance and organization. Many science policy decisions, however, are in fact strategies concerning the content of science. Such issues as the 'maturity' of a problem, its scientific interest and its relevance condition decisions that are made on particular scientific research problems. All attempts at external control of science must reckon with several features of each particular area, such as its general state, the maturity of its theories or experimental techniques, its potential for development, applicability of basic knowledge, and so on. External policy makers normally seek this information from those with experience in the appropriate fields.

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But their knowledge is personal and informal. There is a need for a reflective knowledge of this aspect of science, of the forms of development of science.

The present chapter sets out to delineate a framework for such reflections upon science. I believe that knowledge of the general structures of the development of science is a prerequisite to rational science policy. Many of the contributions on this subject come from philosophers of science; and philosophy, therefore, will be the dominant theme of this chapter. Within the philosophy of science, a distinction can be drawn between work which is concerned with the problem of validation of scientific theories, and work which is chiefly concerned with the explanation of scientific change. This chapter will confine itself to the latter aspect; that is, it will be concerned with explanations of change, rather than with discussion of the nature of scientific knowledge and demarcation between knowledge and belief – questions which remain of central importance to the mainstream of traditional and critical work in the philosophy of science.

The thesis that a theory of the development of science is relevant to science policy is likely to be received with some skepticism. This skepticism, however, is diffuse. One argument holds that conceptualizations of the development of science have so far contributed little to science policy; another holds that a theory of scientific development is not possible at all. This chapter argues for an instrumentalist strategy: one must know the 'laws' of a field before one can reasonably attempt to plan its development. The trouble is that the theoretical approaches developed thus far have failed to satisfy these basic instrumental requirements. It has not proved possible, through knowledge of the structure of science, to apply to the development of science the strategies implicit among science policies, nor has theory produced the means and methods for evaluating policies subsequently adopted.

It should be pointed out, however, that hitherto the function of theories of scientific development has not generally been to assist policy making for science,[1] but to do something entirely different. Theories have provided a critique of ideology. In resisting planning, the scientific research community has rationalized its position by the thesis that scientific development is autonomous, determined by internal laws. The argument is that since the direction of the development of a science is determined by the inner logic of a set of scientific problems, and since the element of creativity makes the dynamics of this process unpredictable, what can be the sense of trying to influence it by planning? The fact that this thesis, while not refuted, has by now become a theme of scholarly research, may be seen as the result of a continuing debate about theories of scientific development. 'Internalism' and 'externalism' continue to constitute the main dividing line along which the main positions in this area can be ordered,[2] and the unification of these two terms remains a main problem confronting scholars in the field of science policy studies.

Pending such a unification of positions, science policy scholars can now formulate the questions which a theory of scientific development may be expected to answer: In what phase of its development is a science open to external influences? Under what conditions can social problems take the form of scientific problems? At what point in its internal development may a science be expected to resist external influences?[3] When is a discipline to be called 'mature', in the sense that it can act as a subsidiary to problem-solving strategies? When – again in the sense of at what stage in the development of its content – can problem solving in a particular field be organized by a division of labor and by a systematic breakdown into partial problems? What is a 'research front' and what are the mechanisms which generate problems within it? What are the determinants of diversification, and, on the other hand, what are the cognitive conditions under which interdisciplinary work will be successful?

More than anything else these questions point to the direction that research should take. No one will claim that on the basis of the concepts developed thus far, answers can be found. Nonetheless, it should become evident that a discussion initially determined by contradictory images of science (autonomous or controlled) has now reached a point where participants are compelled to seek answers to these pragmatic questions. But as the practical issue is not how science policy decisions are made, but rather how science policy can be devised rationally, no easy answers may be expected.

MODELS FOR THE DEVELOPMENT OF SCIENCE

The practical need of science policy to conceive of science as a well-defined domain of actions has coincided with a substantial interest among historians, philosophers and sociologists in treating science as an isolated object. This conception of science is not necessarily valid, as we know from the history of the rise of modern science as well as from the current tendency of science to diffuse itself into other spheres of human activity. Most of the theoretical

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[1] There have been, of course, a few exceptions. Thus the development of the Dutch universities in the Nineteenth Century bore the imprint of Comte's classification of the sciences; similarly, the policy of the BAAS followed Herschel's inductivism.

[2] See the most recent 'handbook' review of the discussion, Kuhn's contribution on 'The History of Science' in the *International Encyclopedia of the Social Sciences*, Volume XIV.

[3] A systematic approach to the latter two questions has been undertaken in van den Daele and Weingart, 1975.

approaches discussed in this section treat science as an isolated system whose states evolve internally, but which are also exposed to external influences from a changing environment. This mode of treatment seems justifiable at any rate for academic science in the Nineteenth Century, particularly in Europe. Moreover, the tendencies by which this isolation may be altered (through the fusion of science and technology, the dissolution of isolated scientific communities, and by linkages between scientific and social problems) can be described, provided one has given a definition of science as an isolated object. In what follows, all theories of development of science will assume an 'object/environment' schemata, except for those which from the outset approach science within a Marxist framework of theories related to the development of productive forces. In general, the distinction between external and internal factors of development is fundamental. Once this distinction has been established the question arises as to how such external factors influence the development of the content of science. If their significance is but a contingent one, it may be expected that it will prove possible to trace out a developmental logic within the development of science, that is, to give a rational reconstruction of this process. If we then ask what the rationale of the history of science is, the global properties of this history become relevant: Can continuity be demonstrated in the history of science, and if so, what assures it? Is the history of science genuinely cumulative or does it also exhibit losses? Has the history of science tended in a certain direction, toward a goal such as, for instance, 'the approximation of truth' or has it rather tended to follow a natural and unplanned process of development? And last of all, does not reflection upon science and the intention to develop it in a specific manner create conditions for converting the development of science from a 'natural' into a goal-directed process?

These questions, which assume the possibility of a developmental logic of science, lead towards more general questions about the transformation, through different epochs, of accepted models of science. From this vantage point we can consider the Marxist theories, which establish the 'external' links between the development of science and the forces of production. Early theories of the development of science can be categorized either within a theory of cultural evolution (as in Comte), or within the context of theories which establish a linkage between the development of science, the development of technology, and the processes of production.

The models discussed in the following section are important examples of current attempts to 'make sense' of scientific development from epistemological viewpoints. In most cases, their relevance, intended or otherwise, becomes clear in explication. All these models differ in various important respects, however, particularly in the weight given to external and internal factors and in their assumption of continuity or discontinuity as a governing principle of scientific development.

PHASES OF THE DEVELOPMENT OF SCIENCE: KUHN'S THEORY OF NORMAL AND REVOLUTIONARY SCIENCE

Kuhn's theory of the development of science, as elaborated mainly in *The Structure of Scientific Revolutions* (1962, 1970), is characterized by his conception of different phases of scientific development, which emerge not globally for science as a whole but in particular branches or disciplines. Two important elements are at the basis of Kuhn's theory – first, his belief in the existence of crisis-provoking anomalies and in the significance of discontinuities in the history of science, and second, his view of the impossibility of giving empirical and historical meaning to the logic of justification. Kuhn sought to account for the first belief by the use of 'scientific revolutions' and his concept of 'revolutionary science'; the second belief led him to introduce the concept of 'paradigm', a concept which has enabled him to reconstitute the meaning of such phenomena as 'authority', 'tradition' and 'dogma' in modern science (Kuhn, 1961).

In the development of particular sciences, Kuhn distinguishes three stages: 1) the 'pre-paradigm' stage, 2) the 'paradigm' stage of 'normal science', and 3) the stage of revolutionary science. In essence, the pre-paradigm stage is characterized by procedures of trial and error. In a particular research field delimited by its subject matter, facts and experience are collected, explanations and generalizations are sought, often in the absence of any assured results. Rather, the scientist is each time compelled to redefine the foundations of his field. This stage comes to an end with the emergence of a universally recognized scientific achievement that becomes exemplary for further practice. It must be an achievement in theory articulation, as is made evident by the function Kuhn assigns to it. For if scientific achievement is to assume the function of a paradigm, it must enable a scientist to take the foundations of his field for granted; it must also provide a criterion for choosing problems and, in part, provide tools for problem solving.

It is this function of the paradigm which allows Kuhn to designate scientific work in the paradigm phase as 'normal science'. In everyday practice the scientist stands on solid ground and is concerned with problems that can be assumed to have solutions; his activity is puzzle solving. In this 'normal science' there exist but three classes of problem: the determination of significant facts, the matching of facts with theory, and the articulation of theory (Kuhn, 1970, 96).

In the course of his education a paradigm is transmitted to the young scientist as a dogma. It might be argued that this socialization supplies the scientist with rules according to which he can plan his scientific work. Kuhn also refers to such rules (1970, 102), but adds that this is a considerably broadened sense of the term by which 'rule' can be equated with 'established viewpoint' or with 'preconception' (1970, 101). Kuhn's point in making this reservation about the traditional concept of rule is that the paradigm is grasped quasi-intuitively as an orientation, and that work in the style of the paradigm is learned by training and by imitation. Here Kuhn acquiesces to Polanyi's thesis regarding the effectiveness of 'tacit knowledge' in science (1969, 1962).

The vagueness of Kuhn's concept of 'paradigm', evident in this short summary, has been criticized by many, in particular by Masterman (1972). In his 'postscript' of 1969 (Kuhn, 1970), Kuhn reacted to this criticism by distinguishing two different senses in which the term paradigm can be used: in a sociological sense, and in the sense of an exemplary past achievement. In its sociological usage a paradigm stands for the constellation of shared group commitments, the so-called 'disciplinary matrix' of a field. The disciplinary matrix consists of four main components: 1) symbolic generalizations, 2) heuristics, 3) values, and 4) exemplars or paradigms in the narrower sense of the term. Symbolic generalizations denote both simple symbols as well as formulas. Heuristics are preferred permissible analogies and metaphors, which enable problem solving to occur routinely. Values pertain to the quality of predictions and theories. Predictions should have quantitative precision; theories should be simple, self-consistent and integrative.

With the fourth element of the disciplinary matrix, Kuhn reverts to the use of the term paradigm to mean 'exemplars', or particular achievements within a discipline which serve to orient scientists in their practice of problem solving. This fourth shared commitment is that which closely defines communities of specialists, or better, scientific specialties. 'More than other sorts of components of the disciplinary matrix, differences between sets of exemplars provide the community fine-structure of science' (Kuhn, 1970, 249).

For Kuhn, revolutionary phases in science occur when a paradigm can no longer fulfill its function of guiding research. There may be a variety of reasons for this, the most interesting being perhaps the failure of a paradigm in its problem-generating power, exhibiting what Lakatos (1972) describes as a degenerating problem shift. Kuhn himself gives more emphasis to the significance of anomalies (that is, of facts which stubbornly resist being subsumed under a dominant theory). In this phase, however, the revolutionary nature of scientific practice is characterized not only by a high degree of insecurity – there exist no reliable evaluative standards – but also by the impossibility of reaching a rational decision between alternative paradigms. The transition to a new paradigm itself constitutes a discontinuity in scientific development. The reason for this, Kuhn argues, resides in the fact that there exists no commensurability between theories which generate research in a specific field. In the first place, competing theories may entail different standards and definitions of science. Second, while competing theories may nominally incorporate similar or identical concepts, their meaning changes according to changed theoretical relationships (For a radicalization of this thesis, see Feyerabend, 1962). Third, the proponents of competing theories, employing different conceptual and manipulative apparatus, and having different approaches to the object and different theoretical expectations relative to empirical reality, 'practice in different worlds' (Kuhn, 1970, 212). It follows, according to Kuhn, that a decision between competing paradigm candidates is not possible. Conversions to a new paradigm are in part decided 'factually', when sooner or later a community forms as a single group around a theory and thereby gains dominance in the field, and in part by 'persuasive arguments', of which those of a strategic kind are the most important. In this case 'a decision between alternate ways of practicing science is called for, and in circumstance that decision must be based less on past achievement than on future promise' (Kuhn, 1970, 219). Let us briefly summarize the leading features of Kuhn's thesis:

A) First Kuhn's concept of the development of science has given the history and philosophy of science a sociological dimension. In Kuhn's perspective, the validity of a theory is a sociological phenomenon; decisive instances of scientific development, ~~namely~~ the transition from one paradigm to another, have as their corollary reorganizations within the scientific community or formulation of new communities (See Kuhn, 1970, 230). In this sense Kuhn's theses have had an extraordinarily stimulating effect on the sociology of science.

B) The second outcome of Kuhn's model is that scientific development can no longer be simply designated as 'progress'. That is, we may call it 'progress' in that it is a departure from a beginning, but it is not progress proceeding in the direction of any goal. Kuhn describes scientific development as a 'natural' process, and emphasizes the point by occasional comparisons with Darwinian evolutionary theory. According to Kuhn, 'progress' no longer appears in the form of an approximation to truth, but in an accumulation of technical experimental results and of solutions to problems within particular paradigms.

C) This view of progress underlines a third feature of Kuhn's model: the role played by external factors in scientific development. External influences may come into effect in trial and error procedures, which are closely related to craftsmanship and technological devices as well as to the problems posed by social and economic situations. Model and theory building are determined by scientists' experience and by metaphysical preconceptions. In the 'revolutionary stage', external influences affect scientific development in two ways. First, the crisis-inducing factors are themselves often external to science. Anomalies as such do not destabilize an existing paradigm; in fact, discrepancies may be set aside. But particular events may lend weight to particular anomalies. These may, as was the case with astronomy, include social pressures for a calendar reform, and thus be wholly external to science. Second, the factors which may prove decisive in a paradigm debate are in many cases extra-scientific and rooted in metaphysical principles.

Kuhn's model of scientific development has been criticized from many viewpoints. It has been argued, for instance, that available sociological evidence does not support his claims of a connection between paradigms and scientific communities (Ben-David, 1975; for a summary of these claims, see Griffith and Mullins, 1972). Epistemological disagreements have been developed in Lakatos and Musgrave (1972), and the notion of a paradigm has itself been the subject of special criticism of different kinds (Masterman, 1972; Shapere, 1972; Shapere, 1964; Toulmin, 1972). And it has been argued that Kuhn fails to explicate the actual progress of scientific development (King, 1970). Finally, Kuhn's critics have challenged his distinction between 'normal' and 'revolutionary' science (Feyerabend, 1972; Popper, 1972; Toulmin, 1972b). However, these criticisms have all had in common one factor: they have taken weak points in Kuhn's theory as points of departure for several extremely interesting theories of scientific development.[4] This is true particularly in respect to Kuhn's critics who have aimed to establish the continuity of scientific development.

CONTINUITY IN THE DEVELOPMENT OF SCIENCE

Lakatos' methodology of scientific research programs (1972) emerged directly from his confrontation with Kuhn's theory. Departing from Popper, Lakatos initially elaborated a sophisticated falsificationism, within which scientists live with anomalies which in the strict sense falsify established theories. According to Lakatos, a theory is rejected only when there is a rival theory to replace it – a point already made by Popper (1971, 52-55, 83). Lakatos, however, has introduced a new criterion for judging the superiority of a theory: It must provide a 'progressive problem shift'; that is, it must not only resolve problems but extend the 'problem horizon'.

More important, Lakatos claims that a succession of theories can be logically reconstructed. According to Lakatos, a series of theories becomes connected in a research program which develops internally, or, as he has occasionally put it, dialectically. A research program involves a 'hard core' of fundamental ideas about the world, or more particularly about a sphere of reality, and a 'protective belt' of theories which provides a conceptual framework consistent with the hard core by which reality is grasped. In the logic of discovery, the hard core provides a negative heuristic – that is, a set of interdictions which indicate what conceptual tools are not admissible in explicating the subject matter. The function of the protective belt is to provide a positive heuristic, which sets out which theoretical tools are to be employed within a given sphere of reality. Lakatos claims, and in several instances he has persuasively demonstrated (e.g. the development of the Bohr model of the atom) that a series of theories may emerge totally within this protective belt, that is, in each case from a process of working out the difficulties and inconsistencies of a theory, something which does not even require an empirical challenge (1972, 149). These conclusions have led Lakatos to the thesis that the history of science can be rationally reconstructed, hence that external influences can at most generate contingent deviations from its internal history.

The major difficulty encountered in the rational reconstruction of the history of science is that of defining what standards of rationality can and should be applied. Lakatos and Krüger, the protagonists of the thesis of 'reconstructability', have taken the 'natural' standpoint, according to which the state of contemporary science provides an adequate standard of rationality. Leaving aside the obvious fact that earlier historical figures did not have this particular standard at their disposal, and recognizing that many of the problems raised by Kuhn cannot be settled in this manner, there is a danger that the history of science will be reconstructed from the vantage point of a history of 'victories'. True enough, Lakatos has recognized that such reconstruction can be effected only with hindsight. Nevertheless, the systematic problems entailed by a rational reconstruction of the history of science are far greater than the methodological ones already indicated. In fact, they entail a large-scale research program into inter-theory relations. A successful logical reconstruction above all requires a clear statement of the logical relations obtaining between succeeding theories.

In recent years, the philosophy of 'inter-theory relations' (Strauss, 1972; Bunge, 1970; Scheibe and Krüger, 1972) has come to have a direct bearing upon theories of scientific development. Progress within the frame of this program may be described as follows: the historical successions of two theories, T_1 and T_2, may under certain circumstances be justified by the statement that T_1 is the prerequisite condition for the articulation of T_2, in the sense that T_1 connects T_2 with empirical facts (the relation of classical physics and quantum mechanics), or in the sense that the conceptual articulation of T_2 is only possible if it is within the perspective of the already existing theory T_1 (the relation of statistical mechanics and thermodynamics: Krüger, 1974a). It might also be the case, by a rough analogy with the first possibility, that theory T_1 assumes for theory T_2 the role of a measurement theory for an object theory and that T_2 is thus not empirically testable without T_2. Another presumably historically frequent version could be that T_2 is an explanatory theory of T_1. In that event T_1 would obviously have to precede T_2 historically and there would also have to be some reason for wanting an explanation (e.g. the relation between Kepler's and Newton's theories of the planetary system; Scheibe, 1972). The last instance constitutes an example of T_1 being sublated into T_2.

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[4] For a general survey of the discussion, see also Kisiel and Johnson, 1974, and Weimer, 1974. Kisiel and Johnson contains a Kuhn bibliography.

To make this explanation comprehensible within a wider context, part of the literature of 'inter-theory relations' speaks of a 'developmental dialectic' (Strauss, 1972, 105-115). Perhaps the concept 'uplation' (Aufhebung) can be used to characterize the position.[5] This concept expresses the denial of discontinuity in the history of science, as well as a denial of the thesis that a theory can be 'displaced' and thus refuted by a succeeding theory. On the contrary, it argues that older theories are integrated with new theories into developing theory-constellations, so that

> it is still possible to determine within the respective state of a science specific fundamental features of its preceding development and this precisely with the help of the inter-theoretical relations it contains (Scheibe and Krüger, 1972, 5).

Accordingly, 'the continuing theoretical acknowledgement of an older theory simultaneously ensures the permanence of the data associated with it' (Krüger, 1974a). However, given that a later theory does not wholly replace an earlier one, this position implicitly rejects both Kuhn's claims as to discontinuity of theory development and discontinuity at the level of data.

Although the investigation of inter-theory relations may eventually lead to an understanding of logical interrelations within science, nothing so far indicates what contribution it can bring to an understanding of the 'dynamics' of this development. This holds also for the concepts created explicitly to grasp 'theory dynamics', concepts generated in the model theory of Sneed (1971) and Stegmüller (1973, 1974). These are attempts to elaborate the concept of physical theories in such a way that their identity becomes conceivable through a process of historical development and theory change.

What is at issue is the logical possibility of theory development, not its explication. The central point made by Sneed and Stegmüller is the so-called 'non-statement view' of theory. In this view, a theory in itself does not state anything but is merely the formulation of a complex structure, or in traditional terms, of a conceptual instrumentarium. Statements can be derived from theories only as statements of the existence of partial models of the theory – that is, of the existence of a sphere of reality which can be grasped with the conceptual tools of the theory in question. Departures in this direction are discernible in Lakatos' distinction between the 'hard core' and the 'protective belt'; they are also visible in the work of Strauss, who draws distinctions in physical theory between the mathematical substructure, the mathematical superstructure and physical interpretation. According to Sneed and Stegmüller, we must distinguish in a theory between the mathematical structures and the intended applications. The intended applications of a theory consist in the mapping of mathematical structures into partial models. These partial models may include functions which quite possibly belong to a different theory, and in any particular case ensure data generation independent of a given body of theory.

The decisive contribution of this concept to a theory of scientific development is, according to Stegmüller, that it explains the supposed fact that theories remain while scientists' convictions change. The reason for this is that the range of validity of a theory is not essentially a part of the theory itself. Rather, it is built up step by step in the course of the process of research by experimental applications, expansions and restrictions. What Stegmüller does, in order to come closer to Kuhn's concept, is to include the primary applications of a theory in a conceptual 'paradigm' (as a case in point, the application of classical Newtonian particle mechanics to the planetary system), so that each primary application becomes an essential component of the theory. 'Theory dynamics' also stresses the use of structural components which enable theories to maintain their structural identity throughout history.

The approach of Sneed and Stegmüller has a bearing on the research process of normal science, but this is within a perspective in which normal science (or more accurately, a Lakatosian research program) is conceived of as an interesting process in itself. While the conceptual instrumentalistic view of theories does not as yet provide a conceptual schema for the process of scientific revolutions, it at least enables us to understand why, under the particular circumstances, the transition from one theory to another occurs without logical justification. It in fact involves taking hold of a new instrument of knowledge when the old one has failed (Stegmüller, 1973, 246). For this there exist in principle only pragmatic justifications.

The main difficulty encountered by all theories which intend to present the development of science as a continuous process is indubitably that of bridging the hiatus caused by 'scientific revolutions'. It has been pointed out that in such revolutions, so-called superparadigmatic values and norms outlive the shock (Toulmin, 1972b; Radnitzky, 1971). One possibility, therefore, is to extend Lakatos' concept of the research program to embrace the development of modern natural science as a whole, in terms by which it would embody the emergence and realization of such characteristic values and norms as experimentalism, quantification, objectivization, and so on. Presumably this type of approach might make it possible to study certain global properties of the process.

Attempts to conceptualize at least one of these global aspects have been made, in relation to the progressive integration of scientific theories of various sciences. One of these concepts is von Weizsäcker's notion of a 'unified physics'.[6] Von Weizsäcker starts from the observation that physics

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[5] Translator's Note: The traditional German philosophical concept 'Aufhebung', usually translated as 'sublation', is given as 'uplation' in M. Strauss (1972). See Chapter 1 of this work, 'Concentration and Uplation in the Evolution of Physics'.

[6] This quest for a unity of science proceeded in an entirely different manner within the 'unity of science movement'. The expectations of this movement, finding expression

reveals 'an historical development . . . towards unity' (1971, 208). Here von Weizsäcker has in mind Newton's integration of terrestrial and celestial mechanics, the unification of mechanics and thermodynamics in statistical mechanics, and the integration of mechanics and electrodynamics within the special theory of relativity. According to von Weizsäcker, a principle of development underlies this integrative process. In this process, physics, and in a wider sense all of natural science, works out the conditions of its own possibility. The keystone of this development will be a theory which will be built upon concepts which are, in principle, necessary to grasp an object developing over time.

EVOLUTIONARY DEVELOPMENT MODELS

Philosophers who wish to see the continuity of scientific development vindicated are, as a rule, 'internalists'. That is, they are determined to account for the scientific process on those grounds that can be derived from the rationality of the process itself. Discontinuities in the development of science are held to be 'rationality gaps' by which extra scientific factors penetrate into the cognitive process and determine the further development of science. For the same reason scholars who construct the development of science according to a Darwinian model tend to emphasize the discontinuities, arguing that there are points where external factors come into play which make the development of science a natural process. Yet even among the protagonists of evolutionary development there are internalists. For them the process of selection of theories takes place in a domain screened off from others, that is within the scientific community, the community of those competent.

Among this range of models one may place Popper's (1972) theory of the progress of knowledge. According to Popper, theories are attempts to produce solutions to a problem. And it is the rational criticism of scientists which determines the outcome of the struggle for existence in which hypotheses must show their fitness for survival (Popper, 1972, 261). Certainly Popper's model is ambiguous. The question one must ask is whether the conditions of survival are set by the objective problem or by scientific criticism. The emphasis which Popper gives to an 'adequacy of truth' makes one assume the former. In terms of Popper's perspective, the fittest theory for survival is not a theory which answers some purpose, but a theory which is nearest to the 'truth'.[7]

> For I did not state that the fittest thesis is always the one which helps our own survival. I said rather, that the fittest hypothesis is the one which best solves the *problem* it was designed to solve, and which resists criticism better than competing hypotheses (Popper, 1972, 264).

In this juxtaposition of solution and criticism as the conditions for survival, Popper's ambiguity again comes to the fore. It becomes comprehensible, however, when one makes explicit the logic, or better, the strategy of theory construction. Thus, theories shall not only solve predefined problems. Rather, by having an excess of empirical content, they should lead to the discovery of new problems. They can, therefore, never be judged merely in terms of their truth content; rather, this requires the pragmatic evaluation of the scientific community competent in the particular field (Böhme, 1974b). For the process of criticism the truth is but a 'regulative idea' (Popper, 1972, 264).

A more elaborate model, although in principle not fundamentally different from Popper's, is the internalistic evolutionary model of Toulmin (1967, 1972a). Mutation and variations in the Darwinian evolutionary schema are in science matched by innovation and the production of ideas. This domain is open to all external influences; psychological and sociological factors can play a role here. Indeed, economic conditions are decisive for the quantity of mutants. From the population of ideas generated in this manner, those fittest for survival are selected. Survival conditions are set by disciplinary standards and explanatory ideals.

Obviously this process of selection loses any similarity to a process in nature in that it ultimately entails a reasoned selection. Toulmin encounters difficulties in those cases in which disciplinary standards and explanatory ideals, that is to say research strategies (1972a, 246), are up for reappraisal (1972a, 232). According to Toulmin, such cases cannot be dealt with by Darwinian concepts but rather according to the model of English common law, where a competent judge does not base his judgment on codified law but tries to dispense justice by taking into account the continuity of history: 'The choice between disciplinary goals or strategies is a matter for the judgment of authoritative and experienced individuals' (Toulmin, 1972a, 242). If Toulmin also is talking about 'populations' of strategies and explanatory ideals, the criterion he asserts is the increase in explanatory power and the deepening of explanations.

As far as the evolution of scientific ideas is concerned, Toulmin does, to be sure, assign a major role to the scientific community. Nonetheless, the

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in the project of an *Encyclopedia of Unified Science* (See Neurath, Carnap and Morris, 1971) were not directed as in the case of von Weizsäcker to a fundamental theory, but to the realization of a unifield scientific method. Neurath (1970) believed that from the elaboration of logical empiricism and empirical rationalism would emerge a consensus as to scientific method. Thus the form of science unified on the basis of its methods would be diversity in unity – the Encyclopedia.

[7] Such an incremental form becomes intelligible in terms of Popper's (1972) concept of verisimilitude.

scientific community is in these terms always characterized by specific competences; and ultimately selection proves to be an objective process:

> The ultimate verdict . . . remains an objective, and even a factual matter. For the ways in which Nature will actually respond to our attempts at understanding her is something that goes beyond all human tasks and all human power to alter (Toulmin, 1972a, 245).

In this Toulmin shares Popper's realism.

In a certain sense Toulmin's and Popper's conception of evolutionary scientific development is paradoxical. Or to put it more precisely, it presents a paradox insofar as the Darwinian conception of evolution sees the process of evolution as one which cannot be comprehended by reason, but which can only be explained by causes. For Popper and Toulmin the process of the selection of scientific theories is a rational choice, not a 'natural' process. In this respect the evolutionary model described by Böhme, van den Daele and Krohn (1972) comes closer to Darwinian theory. In this model, external factors do not, as with Popper, have significance only for the rate of scientific development, for problem selection and for the number of innovations; they have relevance also for the selection of theories themselves. Hence this model describes not only the 'intellectual' survival capacity of theories but also their social survival capacity. Since the 'life' of a theory is determined by the existence of a scientific community working on it, its survival will also depend on whether the corresponding scientific community has any real opportunity to establish itself. This possibility in turn is very strongly affected by external conditions, e.g. by whether society shows any need for cultivating such problem-solving capacities. According to Böhme, van den Daele and Krohn, Darwinism acts on the history of science not solely by means of the social selection of alternatives generated within the science. Rather, there is active adaptation to the social and economic survival conditions of science.[8] This adaptation takes place through the operation of 'regulatives' which ensure that results satisfy determinate norms. The authors distinguish between 'internal' and 'external' regulatives. Among the former they class logical-transcendental, logical-strategic and methodological factors; among the latter, social, socio-economic, cultural and religious ones.

According to Böhme and his colleagues, factors external to the development of science have a bearing upon not only the contingent traits but also upon the content of this development itself. If the question is how far external factors act directly upon the determination of the content, then the answer would be that a distinction must be made between social externalism and cognitive externalism; social externalism relates to all situations in which power structures or social and economic needs act as the selective mechanism for internally generated alternatives in science. Cognitive externalism refers to the external determination of what is accepted internally in science, in part through the process of justification (explanatory ideals, legitimacy and relevance criteria: see Böhme, 1974b), and in part through the technical-experimental constitution of the objects of a study (Böhme, 1974a).

Historical change of developmental models All the models discussed so far have one characteristic in common: their validity is not limited to particular historical epochs. Yet it is implausible that science has always developed in the same way. The inference may well be that the developmental model *per se* is subject to historical change and that in each epoch the whole of science exhibits a different form. In this section, therefore, we will discuss concepts which are premised on such radical transformations in the progress of science. We shall deal first with the concept of 'finalization'.

Böhme, van den Daele and Krohn, in characterizing the history of science, use the Darwinian evolutionary concept to emphasize the natural character of this process. But this concept becomes meaningful only when it is viewed against the background of the possible purposiveness of scientific development. Their model, therefore, has qualified Darwinism in the history of science as merely contingent, and does not claim that scientific development can be explained by this concept on a global scale and in all epochs. The model assumes, rather, that changes in the developmental pattern occur in the development of science as a whole, as well as in the development of particular fields. The decisive transformation of the natural process of scientific development into an intended, that is conscious, mission-oriented and planned development is designated as 'finalization'. The notion of 'finalization' in no way signifies any termination of the scientific process but, appealing to the Aristotelian *causa finalis*, denotes a purposive development. Hence 'finalization' as a characteristic of the global process of science marks out the transition between two developmental phases in which scientific development must be understood on the basis of diverse theoretical concepts.

Finalization in any particular discipline is described by a 'phase model' which expands the one formulated by Kuhn. Böhme and his colleagues (Böhme *et al.*, 1976) do not believe that once it has reached the paradigm stage, scientific progress proceeds through the steady succession of paradigms. Rather, with Heisenberg (1971), they state that for determined object areas, there exists something in the order of 'closed theories'. Used strictly in the sense of Heisenberg's terminology this term designates theories which can no longer be improved by minor modifications. Using the term in a somewhat weaker sense, one can speak also of 'completed theories' as referring to theories by which an object of study is comprehended. When a science has achieved a 'complete' theory for any particular field, it is labelled a 'mature discipline'. Once it has reached 'maturity', a discipline can be guided by external purposes in its theoretical development. In a way it is even compelled toward 'maturity' because the internally generated criteria of relevance

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[8] A Lamarckian variant of Darwinism which was contained in Darwin's theory.

are no longer adequate to select possible scientific tasks. Research fronts are no longer defined by the problems internal to theory development.

A closer definition of the process of finalization can be seen in four historical dimensions. First, the objects of science itself (e.g. laboratory phenomena) become scientific products. Second, goal orientation implicates not only application of theory but also the development of theory. Third, fundamental theory must have reached a certain degree of maturity. Fourth, science itself starts producing techniques (Böhme *et al.*, 1976, 308f). However, conversion of the pattern of development to a pattern of intentional development cannot be demarcated by these criteria alone. This is the reason why the authors speak not only of finalization but also of its variant 'functionalization'. Functionalization refers to the situation in which, without interposition of the phase of 'complete theory', a short circuit occurs between a science and external goal orientations by input-output models. On the other hand, the authors consider an even more intense version of finalization in which scientific development may not only be guided by external goals, but that the forming of concepts in science will acquire normative dimensions (which will at first seem wholly objectionable to natural scientists) relating it to the social sphere. This prognosis becomes more plausible if we look to human efforts to transform nature, which become ever more salient within science and also ever more politically contentious. Thus the first case study completed on the concept of finalization is concerned with the development of agricultural chemistry (Krohn, Schäfer, 1976).

The finalization concept conjoins three factors which since the latter half of the Nineteenth Century have been decisive for the development of science. First, science is incorporated into the production sphere. This moment is treated by Marxist theoreticians under the rubric 'transformation of science into a direct productive force'. Second, the internal development of science in various disciplines and in various areas has resulted in a certain completion. Third, science is involved in a process which in the transition from traditional to modern society results in the differentiation, planning and administration of particular sections of life. This process has been labelled also as 'the scientification' of the world, and particularly as 'rationalization' (Weber). In recent years science has become reflective during this process and is now increasingly subjected to goal-directed planning.

To date, the concept of finalization has not been tested against a large body of empirical evidence. Moreover, it entails systematic problems, including the definition of 'theoretical maturity', demarcations between 'finalized' and applied science, and the description of the relationship between mature 'fundamental theory' and theoretical special developments. Finally it is, of course, questionable whether a theoretical model can in principle yield the central variable which explains the striking phenomena of scientific development in the Twentieth Century, or whether the most useful frame of reference is not more likely to be found in, for instance, models of

the socialization of science (Prüss, 1974).

The concept of finalization is ostensibly externalist. Nonetheless, it has some internalist facets. The emergence of a new developmental type, in the final analysis, is assigned to internal factors ('theoretical maturity'). The reverse holds true in the French epistemological tradition. With the emphasis on internalism (Bachelard in particular), it views fundamental revolutions in science as a part of the general process of historical and cultural developments. The intellectual father of this tradition is no doubt Auguste Comte (1830-1842). In Comte's view, the progress of science must be seen within a sequence of cultural epochs, from 'theological' to 'metaphysical' to 'positive'. Only with the advent of the 'positive' epoch in the Nineteenth Century does science become adequate to its essence. Science only becomes effective when the vain search for 'first causes' and 'forces' is abandoned and the object of man's research becomes purely phenomenological and nomological. Each field of science also goes through three phases which correspond to the major epochs of cultural development, but which exhibit phase lags in relation to each other. The advance of each science depends on the previous advances of other sciences. An inquiry into more concrete, or more complex, subjects can only enter the phase of 'effective' science after the more abstract and elementary sciences have reached it. Thus a succession occurs from arithmetic through geometry, mechanics, chemistry and biology, up to sociology.

A similar concept of major epochs of scientific development was developed by Gaston Bachelard (1972). For Bachelard as for Comte, the prescientific epoch reaches into the Eighteenth Century. It is the Nineteenth Century that marks the beginning of the 'scientific age'. In it the systematization of knowledge in geometrical form penetrates all fields of science. But it is not until Einstein's work (1905) that Bachelard describes the rise of a new epoch in which the spirit of science has definitively divorced itself from all intuitive notions and images.

According to Bachelard, one can distinguish particular epochs by levels of abstraction, and particular developments within each of these epochs by epistemological obstacles which the scientific spirit must surmount. Bachelard's concept is informed by the thesis that there exists no continuity between prescientific knowledge and science. Rather, the transition from the former to the latter is made difficult by the obstacles which must be overcome each time. Hence Bachelard views all prescientific conceptions not as a foundation of science but more as something contrary to science – as an error.

The scientific optimism underlying this standpoint is hardly likely to be shared by many today. Thus Bachelard's disciple, Michel Foucault, propounds an epochal theory from which any idea of progress has been carefully eliminated. To be sure, Foucault's inquiry – dealing with the emergence of clinical medicine (1973), of political economy, of philology and biology (1971) – is mainly centered around the 'epochal threshold' (1775-1825).

Foucault has sought to show that a science concerned with a particular object area has no continuity extending beyond culturally and politically determined thresholds. Rather, within an epoch there exists a close affinity, in terms of their subject matter, between entirely disparate sciences; this affinity is rooted in their epistemic structure. The epistemological 'coupure' which occurs in each of them has greater significance for the development of any particular field than does its intrinsic continuity. Thus, Foucault argues, in the classical age in France, universal grammar, natural history and the analysis of wealth, together conceived their subject matter in terms of the schema of representation and the represented. Science thereby became the 'representation of representation'. After the 'epochal threshold', these respective fields reconceptualized in the light of principles which organized them internally. Thus economics was understood and developed from the perspective of labor, linguistics from that of inflection, the realm of living from that of life. The object areas in question thereby assumed a life of their own; the fields of study assumed a dynamic, and they became 'historicized'.[9]

The studies referred to in this section are rich in historical data and materials. But what is more important is that they may act as a necessary corrective of assumptions made only too rashly in the generation of philosophical models. Another historical dimension of the development of science, in particular its relation to technology, has been investigated on so many levels and in such detail that it is necessary to discuss it separately. To be sure, this is not as yet a field in which scholars have generally accepted explanatory models. There exist only global models describing the connection of science with technology, and on the other hand there are special hypotheses regarding their interaction in particular periods of time.

THE INTERACTION BETWEEN SCIENTIFIC DEVELOPMENT AND TECHNICAL DEVELOPMENT

Bernal (1957), who pursued the relationship of science and technology for the development of science since antiquity, advanced the thesis that science has shown substantial progress only when there had been a relation to practice: in Ionia of ancient times; in the Renaissance with contact between craftspersons and scholars; in the Seventeenth Century within the Royal Society; in the Eighteenth Century in the Lunar society; in the Nineteenth in the Royal Institution; and later, as repeatedly in history scientists contributed to war efforts, an effort which in the Twentieth Century contributed to organized and planned science.

Of particular concern is the relationship of science and technology at the time of the rise of modern natural science. Zilsel developed the thesis, derived from Olschki (1919), that modern science is in essence rooted in the artisanal crafts of the early Renaissance. Zilsel (1942a) traced the emergence of a modern natural science back to the social conjunction of two previously noncommunicating strata: the artisans and the scholars. He believed that through this combination the crafts gave rise to some major characteristics of modern natural science. Thus the concept of scientific progress and of the 'contribution' of technology to science goes back to the tradition of technical improvement (Zilsel, 1945); and by the same token, important elements of the concept of law of natural science are, according to Zilsel, traceable to the craft rules (1942b). This thesis has been criticized by Hall (1959). According to Hall, technology has been significant for science in offering impulses and problems. This explanation has been shared by Hessen (1931), Merton (1970) and also by Bernal (1970). However, Koyré (1948) and later also Hall (1961) tried to show that precisely in its decisive achievement, modern science does not depend on technology but rather has enabled technology to reach a new level of development: to grasp the sensible world with exact concepts. In contrast to this, technology moved in a world of the *à peu près* far into the Eighteenth Century. Calculations were neither customary nor in many cases possible; often no instruments were available for exact measurements. Instruments, in turn, originated from the requirements of science, and they imported the idea of exactness into the domain of technology. With the scientific instrument came the idea of exactness, the 'réalisation consciente de la théorie' (Koyré, 1948, 819).[10]

Depending on the perspective adopted, the direction of this influence will differ, and for each epoch different theses may be advanced. On the whole the following approaches to the relationship between science and technology can be distinguished:

A) One view holds that science and technology develop autonomously and independently of one another. Some support for this position comes from attempts to distinguish science and technology on the basis of their different intentions and of the behavior of those engaged in them (Derek Price, 1965). The question of how these two developments relate to one another is then solved: either in the form of a 'dialectical arrangement'

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[9] Arguably, these models have little relevance to science policy. Their significance lies primarily in the field of education, which was always at the center of Bachelard's concern. In general, these studies do not question the causal factors in scientific development. However, to this general rule Foucault's *The Birth of the Clinic*, which assesses the impact of the institutional changes brought about by the French Revolution on the development of medicine, is an important exception.

[10] This idea has recently been made the primary interpretative principle of Galilean physics; see Mittelstrass, 1970. Mittelstrass (1974) has also given some outlines for a 'constructive theory of science history'.

(Kranzberg, 1967, 1968) in the sense that at certain stages of its development science uses technology instrumentally for its own ends (or vice versa); or in the form of an evolutionary model (Toulmin, 1969) in which technology sets the conditions for the selection of scientific variants (or vice versa).

B) A second view argues that science has developed by orienting itself to technical apparatus and instruments. In these terms, science consists largely of theoretical attempts to grasp and systematize the manner in which instruments function. Cases in point are the emergence of Gilbert's theory of magnetism (which was based on the existing use of the compass) and the emergence of thermodynamics on the basis of the technical development of the steam engine.

C) A third view, attributable to Koyré, militates against this thesis and claims that the scientific instrument is the decisive connection which links science with technology, and that the technology of science (measurement and experiment) at all times outruns the technology of everyday life.

D) A fourth view holds that not until the late Nineteenth Century was there a possibility of converting scientific knowledge into technology (Hall, 1961). It is further assumed that during the Nineteenth Century the relation of science to technology was reversed partly in conjunction with the 'scientification' of technology. This transition to a scientific technology or, as Koyré put it, to a 'néotechnique', is determined by forms of energy and materials which are man-made, and not supplied by nature. However, opposing this assumption of a unidirectional transformation of technology by science, Moscovici (1968) speaks of reciprocal modification: within the 'division naturelle' both partners assume new roles. From being a mechanistic philosophy where metaphysics conditions experiments, the sciences are transformed into positive sciences; and craft technique is superseded by applied science. This development makes technology receptive to offers from science; natural science, having lost its function of creating world views, itself tends to become merely technical science (Technikwissenschaft).[11]

MARXIST CONCEPTS OF THE DEVELOPMENT OF SCIENCE

It was said earlier in this chapter that most of the theories to be discussed conceive of scientific development in terms of 'internal' and 'external' factors. This does not hold for Marxist theories, which do not conceive of science as an autonomous complex but rather as one aspect of the process of social development. Within this frame of reference two main concerns may be distinguished. One point of interest for Marxist scholars is the social constitution of scientific concepts, the other the functioning of science as a productive force.

This difference in the focus of interest may be given in regional terms; for example, it seems that for Eastern European Marxists, consideration of constitution theory is barred by 'copy theory'. The various Marxist approaches to constitution theory can be classified in terms of the basic concepts of Marxist theory. For example, the constitution of the object of science is grounded in labor (Moscovici, 1968), in the relations of production (Borkenau, 1971), in productive forces (Grossman, 1935), in forms of social intercourse and association (Sohn-Rethel, 1972) and in the value form (Bahr, 1973). According to Moscovici, the dominant form of labor at each stage constitutes a different concept of nature: thus artisanal craft connects with organic nature, the engineer's activity with mechanic nature, and the regulative and inventive labor characterizing our time with cybernetic and synthetic nature. In postulating an epochal change of the forms taken by labor and nature, Moscovici's theory is also a theory of the development of science. Borkenau in 1934 (1971) attempted to derive the rise of modern natural science from the relations of production from the Seventeenth Century onwards. He believed that the compulsion to rationalize had given rise to the downgrading and dehumanizing of human labor, an attitude which was duplicated by the mechanistic view of nature. Grossman (1935) opposed this theory on the basis of economic and historical arguments, and himself formulated the theory that 'mechanics itself had actually acquired its basic concepts from observation of the mechanisms of the machine' (Grossman, 1935, 166).

Sohn-Rethel's 'constitution theory' (1972) also centers on the 'dequalification' or downgrading of nature. Sohn-Rethel holds that in a society in which commodity exchange is the dominant form of social intercourse, the 'real abstraction' from the qualities that exchange entails engenders a conceptual competence which is then sedimented into the modern concepts of nature with their dequalifying impact. With this method, Sohn-Rethel derives such concepts as substance, causality and interaction. Bahr, proceeding along similar lines (1973) has grounded his argument on the form of value which products must assume to act as values in market exchanges. In this way Bahr derives such concepts as weight, number and length.

Notwithstanding their diversity of approach, these attempts to construct a Marxist constitution theory fall short of expectations and intentions. Very rarely does any one of them produce concepts actually employed by natural science; and those concepts which do results are not, as a rule, concepts of the scientific object but meta-concepts, that is to say concepts of the theory of knowledge and philosophy of science, as for instance in the case of Sohn-Rethel, those of Kantian philosophy. And even in those instances, as

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[11] See Janich, 1973. Regarding the tendency of science and technology to fuse in the Twentieth Century, see also the finalization thesis and the section on 'Marxist Concepts of the Development of Science' which follows directly.

with Bahr, where the concepts used are those of natural science, no effort is made to show in what way they contribute to the development of natural science. Another criticism is that scarcely any connection is made between these considerations and the actual developmental process of science. Social constitution theories must be derived empirically, with reference to particular historical cases or particular periods and contexts. Only in this way can they become an essential component of a general theory of scientific development.

Given that from the Marxist point of view science is primarily an aspect of social development, profound changes in the developmental pattern of science are by the same token revealed at times of social upheaval, and certain scientific developments can be related to specific changing social functions. Wolkow has distinguished three major epochs of scientific development: 1) orientation to man, in which the main social function of science is the generation of world views; 2) orientation to technology, in which the main social function of science is the development of material wealth; and 3) orientation to man, in which the social function of science is to perfect man's biological and social environment (Wolkow, 1969, 720).[12] From this viewpoint it is a revolution in tools which provides the determining impulse to change and which ushers in the transition from one epoch to the next. Thus the Industrial Revolution and the scientific-technical revolution can be said to represent the two epochal thresholds of scientific development in modern Europe. In the first revolution, the function of science resides primarily in generating world views and thus its main function is explanation and enlightenment. Theoretically it is therefore held to be a form of social consciousness and subsumed in the superstructure. In the second revolution, science is determined by the industrial revolution which in the essence consists of the mechanization of labor. From then onwards science can no longer be assigned to the superstructure but itself becomes a productive force (Kosing, 1964).

Evidently science in the industrial phase is at first a productive force only in the objectified form of machinery. Science still depends upon living labor and merely raises its productivity. By functioning indirectly as a productive force, science at the same time can become independent of the sphere of production. Since it enters production only in the objectified form of the work tool, it can progress outside the sphere of production. The separation of applied from pure science and the autonomy gained by science in the Nineteenth Century both appear as particular expressions of the split between intellectual and physical work (Wolkow, 1969).

The industrial revolution also marks the beginning of the socialization of science: that is, science becomes social labor. It is not as yet productive

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[12] It is a particular weakness of Wolkow's concept that he lets the second period start with the rise of natural science (712). To locate the actual turning point in the period of the 'young Marx', that is in the industrialization period (715), will more readily meet with a consensus of opinion.

independently, but constitutes general labor[13] whose purely ideal product, in order to be realized, must rely upon living labor.[14] The second epoch of scientific development is thus characterized by antagonistic tendencies. Science becomes a productive force, but as pure science it is ranged against the sphere of production; scientific activity has become social labor, but at the same time as professional intellectual labor it comes into antagonistic social conflict with the working class (Wolkow, 1969).

The third epoch of scientific development, the so-called scientific and technical revolution,[15] also begins with a revolution in the tools of labor. In its effects the scientific-technical revolution 'replaces the logical functions of the producer by machines'. It thereby reinforces the basic law of the development of productive forces, that is, 'the gradual transference of the work functions of the producer to technical instruments' (Autorenkollektiv, 1972, 133, 136). Unlike the industrial revolution, however, the scientific-technical revolution is not merely a technical revolution but is from the very start determined partly by science. This is due on the one side to sciences like cybernetics being a factor in automation and on the other to the dependency of many major industries on the state of science. The scientific-technical revolution entails a fundamental change in the social function of science and its transformation into a direct productive force (Autorenkollektiv, 1972, 193; Wolkow, 1969, 716; Richta and Kollektiv, 1971, 30; Lassow, 1963, 377; see also Moscovici, 1968).[16]

This line of analysis provides an explanation of the increasing economic importance of science and its convergence with other fields of human work. Even the social sciences must on the basis of growing 'socialization' be conceived of as productive forces (Lassow, 1971; Lades and Burrichter, 1970); leading examples of this are industrial psychology and organization theory. Such processes as are not directly productive become scientific as well, such as 'management' functions.

It must be said that within the frame of the development of productive forces, these Marxist models of the development of science as yet lack a compelling empirical basis, by which it can be shown that the connections stated are in fact causal connections. Moreover, the advantage of these models – i.e. that they do not have to introduce the significance of the social and

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[13] Regarding the Marxist concept of 'general labor' see Kröber and Laitko, 1972, 52.

[14] As the cooperative character of the labour-process becomes more and more marked, so, as a necessary consequence, does our notion of productive labour, and of its agent the productive labourer, become extended. In order to labour productively, it is no longer necessary for you to do manual work yourself; enough, if you are an organ of the collective labourer, and perform one of its subordinate functions (Marx, 508-509).

[15] See Richta (1971 and 1972) and Autorenkollektiv (1974).

[16] For a discussion of this concept see Autorenkollektiv, 1972; also Kedrov, 1966; Klotz and Rüm, 1963; and the volume referred to under Kösing, 1964.

economic development via 'external' factors – is largely counterbalanced by the fact that they almost completely disregard the development of the content of science. Their concern is almost exclusively with such questions as organization, the planning of science and its transfer to production. They pay attention only to the progressive integration of science, apparently because it accommodates the idea of a dialectic synthesis on the basis of materialistic philosophy (Rochhausen, 1970; Malecki and Olszweski, 1965; Autorenkollektiv, 1968, 114, 117; Kedrov, 1973). Nonetheless it seems apparent that precisely this perspective offers some promise of grasping the causal dimension of scientific development.

THE STATE OF THE ART: FUTURE PROSPECTS

Indeed, reviewing the different Marxist and non-Marxist models of scientific development, one has the sense that this is a research field rich in exciting ideas as well as in unsolved problems. This may motivate one eagerly to enter the field; or, conversely, it may cause one to keep away from it. Obviously bright ideas alone will accomplish very little, and at any rate progress will be slow. What is needed is conceptual clarification and integration and collaboration among the philosophers and historians and sociologists of science. Many current conceptual inadequacies result largely from the lack of cooperation among these domains. This itself gives rise to specious controversies in which mutual recriminations and accusations – for example, of 'relativism', 'irrationalism', 'sociologism', 'historicism' – are but symptoms of a deeper malaise and mistrust among what are, in many cases, competing intellectual traditions.

Few philosophical models of scientific development have been developed to the level of historically testable hypotheses, and to be sure, hypothesis building and verification have very little tradition in history. Nonetheless, the use of case studies of scientific development, in the context of scientific disciplines and specialties, may prove to be an excellent point of departure (Mullins, 1972, 1973; Lakatos, 1972).

In looking to the future and in examining the outstanding theoretical difficulties with an open mind, one must ask whether the analyses developed so far have managed to integrate the relevant phenomena; and whether the state of theory development itself can be assessed, and if so, in what way.

First, in devising developmental models, a line must be drawn between recurrent part processes and global trends. Processes of the first kind include, for instance, the formation of disciplines and specialties, the rise of interdisciplinarity and the transformation of disciplines into a propadeutic. It is here more than anywhere else that it should prove possible to construct

empirically verifiable hypotheses. Second, it is evident that model building must be evaluated in the light of the phenomena which characterize science itself. In this sense the most striking phenomena to be considered are: quantitative growth (Derek Price, 1963), cumulativeness,[17] the tendency toward unity (von Weizsäcker, 1971; Neurath, 1971), the survival capacity of outlived theories, the usefulness of a given type of knowledge, and the self-thematization of science for the past century.

An actual evaluation of the various approaches in the light of such phenomena is outside the scope of the present essay, except perhaps for a few general remarks about the difficulty of theory construction in taking account of these phenomena. The quantitative growth of science is regarded by many researchers as a stumbling block. Hitherto no one has managed to link this development with other relevant characteristics of the development of science. If anything, social psychology has made a beginning by studying the motivations of scientists since on this basis, for example, it would prove possible to construct the curve of growth of new specialties (Holton, 1953). In addition, Derek Price's findings, viewed by some as ominous, have given rise to reflection about 'qualitative growth' and about the possibility of new forms of scientific integration.

As in the case of the quantitative phenomena, the recognition of the 'unitary tendency', or reductionism and the symptoms of 'the end of science' (von Weizsäcker, 1971; Stent, 1969) seems to be hampered by emotional resistance. Since the notion of the express rights of the different sciences and the familiar view of science as an unbounded field of the unexplored cannot be reconciled with these phenomena, the latter remain external to most models of scientific development.

The question of how 'external factors' affect scientific development raises very complex conceptual problems which fall within the compass of such concepts as truth and objectivity. Yet what is even more important here is the absence of relevant empirical work within the history of science. Presumably this work will occur when historians become more interested in problems arising from the philosophy and sociology of science. A particular deficiency in this respect is the relative neglect of the history of technology, or better, an account of the history of technology in terms of the history of science.

Similarly, the effects of self-thematization or, more generally, the 'reflexive' quality of science have been grasped only inadequately, if at all. It is possible to observe specific shifts in the reflexive self-consciousness of science, shifts which were provoked by difficulties arising within a given discipline: the historical difficulties of empirical psychology gave rise to reflection on the problems of quantification; and those of sociology have today led to reflection on the process of theory construction.

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[17] For the classification and critique of the 'cumulation theories of science', see Lejkin, 1972.

More fundamentally, the way in which knowledge of 'how science happens' has affected and continues to affect the development of science is not generally agreed upon. The same question applies, with special force, to the influence of the sociology of science on scientific development. It is widely believed that the conceptual difficulties in which the different lines of inquiry seem to be caught now require: 1) a new cognitive sociology of science (Whitley, 1974; Weingart, 1974; Böhme, 1975); 2) a philosophy of science which takes into account the real situation of research (*Pragmatic Philosophy of Science,* Stegmüller, 1973, Vol. 4, Introduction; Böhme, 1974b); and 3) a theory-oriented history of science (*Theoretical History of Science,* Diederich, 1974). This endeavor may help to give a new and more compelling insight into why science is a social process; why outgrown theories are still being used; how social purposes can be integrated into theory development; what has been the significance of conceptual arguments; and what significance may be ascribed to the role of instruments in the development of science.

It seems that so far particular models designed to encompass global trends are justifiable only partially and in terms of distinct epochs. Hence to arrive at a theory of global scientific development one will probably have to start with a theory relating to specific epochs.

Further, if the result is not to be a new type of 'internal history of science' but an avenue of approach capable of overcoming the 'internal' and 'external' dichotomy, the logic of cognitive development must be seen as parallel to the development of the forces of production. Perhaps this will provide models which in their reciprocal interaction will start off new lines of research and explanation in the social and cognitive organization of science.

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