

THE EVOLUTION OF SCIENCE:
REFORMATION AND
COUNTER-REFORMATION

The remarks which follow * deal with the ideas which I developed in more detail in my book: *Between Experience and Metaphysics*.¹ They are inspired principally by the vigorous polemic aroused by the publication several years ago of a work which caused a great uproar in epistemological circles; I am speaking of *The Structure of Scientific Revolution* by T.S. Kuhn.² One could thus consider this essay, as well as my book, as an element to be added to that polemic's dossier. It goes without saying that I am indebted to a great number of those who before me have given their points of view on the question, whether I agree with their opinions or not.

Two kinds of problems are involved in this discussion; those concerning the model of evolution of knowledge, and especially

* Text of a lecture given in Boston at the Colloquium for Philosophy in October 1973.

¹ Stefan Amsterdamski: *Miedzy doswiadczeniem a metafizyka*, Warszawa 1973.

² T. S. Kuhn, *The Structure of Scientific Revolutions*, Chicago, 1962, 2nd ed. 1970.

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of revolutionary changes in the content of scientific theories, and those concerning the methods and the subject matter of philosophy of science. This entanglement does not seem to be accidental. It is a result of aspirations of philosophy of science to give a satisfactory account of the process of growth of knowledge, and of difficulties that the philosophy of science has to deal with when performing this task. So, the first question to be asked concerns the sources of these difficulties and the conditions under which they may be surmounted.

II

If there exists a point of agreement between all the polemicists, it consists in the opinion that the model of evolution of science and of revolutionary changes in its content, as presented by T.S. Kuhn, is incompatible with some general assumptions widely accepted hitherto in the contemporary philosophy of science. On the grounds of this common opinion two contradictory points of view were expressed:

(a) that the "paradigm" of contemporary philosophy of science needs some general modifications, without which it cannot be compatible with the facts provided by history of science. This is the opinion explicitly stated by Kuhn;³

(b) that the model of evolution of knowledge proposed by Kuhn is essentially wrong, and that no general conjectures of the epistemological paradigm are needed for coping with anomalies which appear when we are confronting historical facts with philosophical opinions concerning the process of growth of knowledge. If I understand Lakatos correctly, it is just this opinion that he has expressed in several papers.⁴

So, I would say that if Kuhn, Feyerabend, Hanson and others call for a reformation of philosophy of science because of its incompatibility with the real process of evolution of knowledge,

³ Cf. *The Structure...*, p. 121.

⁴ Cf. I. Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in: *Criticism and Growth of Knowledge*, Cambridge, 1970, p. 91-197; "History and its Rational Reconstructions," in: *Boston Studies in the Philosophy of Science*, t. VIII 1972, p. 91-136.

Lakatos tries to solve the acknowledged difficulties by means of some counter-reformation measures. He defends the hitherto accepted general assumptions concerning the aims, the methods and the subject matter of philosophy of science and tries to prove that thanks to some modification he has introduced into the Popperian logic of scientific discoveries, this logic is able to provide a basis for a reconstruction of the process of growth of knowledge.

I believe, however, that none of these opinions are right, i.e. that though the accepted paradigm of philosophical reflection on science needs indeed some substantial modifications, the model of evolution of knowledge, and especially of scientific revolutions advocated by Kuhn is also unsatisfactory. This is the point of view I am going to substantiate in this paper.

III

When we ask what is the object whose evolution Kuhn describes, we are faced with a troubling problem. When he speaks about the need of a new philosophical approach to science, he means obviously science *in general*. Philosophy of science is not concerned with construction of theories of evolution of physics, chemistry or any other special discipline, but with the evolution of scientific knowledge *tout court*. However, when Kuhn investigates the scientific revolutions, he speaks as a rule of what happens in specialized fields of research. The revolutions he speaks about are not revolutions *in science*, but revolutions *in sciences*. In the same way the concepts of normal science and of paradigm do not concern the evolution of knowledge in general, but the evolution of special disciplines.

At the same time it is by no means clear what he has in mind when he is speaking about specialized fields of research. Sometimes the field is taken very widely, and he talks about revolutions in astronomy, chemistry or physics. In other cases, however, the discipline is taken very narrowly, and he states that even the discovery of X-rays may be treated as a replacement of the old by the new paradigm⁵ as an example of revolution.

⁵ *The Structure...*, p. 57-58.

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This is not without consequences concerning the notion of paradigm. Namely, it has to embrace such special factors determining the evolution of knowledge as the construction of a new experimental device, as well as such general factors as changes of philosophical conceptions concerning the ontological structure of the world. In consequence the paradigm is not a theoretically synthesized entity, but rather a bottomless sack; all the factors different by nature, which are supposed to determine the evolution of knowledge, can be easily put into it.

It may be argued, of course, that "science in general" does not exist at all. And in some sense this argument is right: nobody does scientific research in general. However, if we accept that the set of special disciplines, even if changing historically in its composition, can be described theoretically, and not only by the enumeration of its components, then, by the same token, we accept that there is something which connects all these disciplines into a whole. And if so, a revolution in science and a revolution in a special field of research *are not the same cultural phenomenon*.

The critics of the monoparadigmatic conception of evolution of science⁶ stressed that the monopolistic authority of a paradigm is rather an exception than a rule. Accepting this argument, I would like to add one more remark. The scientist dealing with a problem is determined in his activity not only by the assumptions concerning his narrow field, but also by the assumptions accepted by a wider scientific community, or even by all the scientists of his epoch. Let us assume that it would be possible to identify the assumptions constituting the paradigm of contemporary biology. It is obvious that examining not biology as a whole, but, for example, the cytology of plants, we would find that the specialists in this field accept some additional paradigmatic assumptions besides those accepted by all the biologists. The more narrow the field will be, the more specialized will be the paradigm. Hence, the discovery which would be treated as a revolution in respect to a specialized paradigm, would be considered as a successive step in the development of normal science in respect to a more general paradigm. If the paradigm is determined widely, it

⁶ Cf. the papers of J.W.N. Watkins, S. E. Toulmin, L. P. Williams, P. K. Feyerabend in *Criticism and Growth of Knowledge*, Cambridge, 1970.

does not contain all the assumptions accepted in a special field. If it is determined in a more detailed way, the revolution does not destroy all the paradigmatic assumptions accepted by a specialist. So, Kuhn's thesis concerning revolutions as points of discontinuity in the growth of knowledge seems to be questionable. (I will come back to this question below.) And if a revolution in a specialized field of research does not require refutation of all paradigmatic assumptions accepted by specialists, then the character and the scope of a revolution depends on the assumptions which are to be abandoned or modified. This is why the differentiation between the revolutions in special fields of research and in science in general seems to be unavoidable and to have a primordial importance for the understanding of the process of growth of knowledge. They are not the same cultural phenomena: the global revolutions happen in science very rarely, the local—almost every day: and, more often, the more the specialized field is taken into consideration.

IV

Why does not the author of *The Structure of Scientific Revolutions* perceive this ambiguity of the fundamental concepts of his work? What is a revolution in science: a modification of certain assumptions accepted, for example, in cytology of plants, a modification which does not even need to affect all the branches of biology, because it has a purely local character, or a modification of certain assumptions accepted in all the domains of scientific activity in the given period? It seems that something more is hidden behind this ambiguity than a lack of terminological precision. What is hidden behind this ambiguity, is a belief that the normal science, the tradition of the puzzle-solving process (to use Kuhn's term) is a characteristic feature of scientific activity in general. It is just this tradition, which according to Kuhn, may be treated as a criterion of demarcation of science.⁷ The characteristic of problems named by Kuhn *puzzles*,⁸ whose solution is to be the goal of normal science, does not consist in looking for new facts and theories. On the contrary, "even the project whose

⁷ Cf. T. S. Kuhn, "Logic of Discovery or Psychology of Research?" in *Criticism and Growth of Knowledge*, Cambridge, 1970, p. 7-10.

⁸ Cf. *The Structure...*, p. 35-40.

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goal is paradigm articulation does not aim at unexpected novelty.”⁹ It is hard to avoid the impression that Kuhn’s characteristic of normal science is very close to what is named *applied research*.¹⁰ In normal scientific activity as well as in applied research the success of the scientist consists in the demonstration that the accepted theory can be usefully applied to the solution of a puzzle. In both cases it is not a question of looking for a new way of ordering the world of human experience, but of strengthening the already existing order; not of looking for new truths (whatever this term may signify), but of utilizing the truths already achieved.

According to Popper the aim of science consists only in looking for truth, and the Popperian methodology has to serve this goal. Science has to be a “good philosophy”. It is turned skywards, but the skies are to be free from all clouds of metaphysics. On the other hand, the Kuhnian normal science, taken as a model of scientific activity, consists in solving problems whose solution is granted by the ruling paradigm. Science is turned towards the earth, but this earth is also free from all metaphysical problems: they were already solved, even if *ex proviso*, by the ruling paradigm, which tells the scientists “what problems are really scientific, and which are to be rejected as metaphysical, as a concern of another discipline, or sometimes too problematic to be worthy of time.”¹¹

If the Popperian criterion of demarcation is absolute, historically unchangeable, the Kuhnian is relative. But both of them separate the scientific investigation from all philosophical problems. From this point of view we may say that in spite of all the antipositivistic declarations formulated by Popper as well as by Kuhn, the controversy between them seems to be a polemic in an old family. Popper sees science as in a permanent state of revolution; Kuhn, introducing the concept of “normal science,” insists on its stability, but both views are absolutizations of one side of the function that science performs in human

⁹ *Ibid.*, p. 35.

¹⁰ Cf. Kuhn’s characteristic of normal science with the characteristic of the difference between basic and applied research given by R. Oppenheimer to the Senate Commission of Atomic Energy: H. Hall, “Scientists and Politicians,” in: Barber and Hirsch (eds.), *Sociology of Science*, Glencoe, 1962.

¹¹ *The Structure...*, p. 37.

culture. I mean the function of unifying two spheres of knowledge, knowledge of "why" and knowledge of "how?," the *episteme* and *techne*, the cosmological beliefs and the practical skills. At least since the Greek antiquity the history of science is a history of building bridges between these two spheres of knowledge, with which science, standing on the earth and gazing at the skies, was permanently connected. Standing on the earth, science was always empirical, but the world of human experience which it has to incorporate into the cosmological order, and the understanding of this experience, is not always the same. Gazing at the skies, science, however, is never purely empirical. Not only because it fills the gaps in our knowledge "by hypothesis," but also because it has to coordinate the knowledge about what is practically possible, and about what is theoretically impossible, about the world of human fortuitousness and about the cosmological necessity. It is not purely empirical because it has to coordinate the function of discovering facts and of formulating general principles which do not follow automatically from these facts. From this point of view the efforts of Aristotle, Copernicus, Descartes and Einstein aimed at the same goal, and fulfilled the same function in different stages of the development of human culture. If we see in science only an ordered system of rules of effective action, and if we project this vision into the past, this is a result of a certain real situation in which science becomes split into the producer of means of material production and consumption, and the provider of true knowledge.¹² It is also a result of this philosophy, which "regards technological efficiency as the highest value."¹³ This is when we do not perceive that the genetic continuity of science is rooted in its constant aspiration to introduce into the realm of our knowledge an order, which in the given conditions of human experience render the unity of practical human actions and of man's total image of the world and of himself possible. And every order which performs this basic function of science is to be called *rational*. In this sense we may say that history of science is a history of successive trials for a rational organization of the knowledge of "why?" and "how?," and that the methodological criteria on

¹² Cf. Krzysztof Pomian: "Działanie i sumienie," *Studia Filozoficzne* 1967, 3.

¹³ L. Kolakowski, *The Alienation of Reason*, New York, 1968, p. 202.

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which the science of each epoch is based are subordinated to the contemporary understanding of this rationality.

The Popperian as well as the Kuhnian conception splits science into two parts, and treats them respectively as models of the whole. The Popperian methodology does not perceive that scientific theories are formulated not only in order to obtain the true knowledge, but also for their utilization. So, it gives them no time to show their usefulness, because it requires their falsification as soon as possible.¹⁴ Kuhn's conception of normal science regards science as an instrumentation of the historically changeable practice. Therefore the modifications of this instrumentation are conceived as procedures undertaken only in exceptional circumstances. The normal science does not account for the fact that the puzzle-solving activity is not a goal for itself, that every single puzzle it solves is a fragment of a wider puzzle, whose solution is the goal of science since it exists, even if the shape of this puzzle changes.

V

I said before that Lakatos' conception was elaborated under the obvious impact of the critique of falsificationism provided by several authors, first of all by Kuhn, and that it was an attempt to neutralize this critique by means of a partial acceptance of its claims and their inclusion into the falsificationist stand-point. So, for example, the concept of research program corresponds obviously with the notion of paradigm. The concepts of degenerative and progressive series of theories in the frame of a program correspond respectively to the Kuhnian concepts of crisis and normal science. Lakatos' concepts of positive and negative heuristics correspond in turn with Kuhn's thesis concerning the "rules of the game" provided by paradigms. The concept of the protecting belt of hypotheses¹⁵ and the rule

¹⁴ According to Popper, a theory is subject to falsification if, and only if, it can be placed in conflict with experience; a theory which does not fulfill this condition is not scientific.

¹⁵ The protective belt of hypothesis: term used by Lakatos to name the body of hypotheses introduced by the scientist to eliminate the contradictions between the results of experiences and the "hard core"; these hypotheses change as one passes from one theory to another within the outline of the same program.

forbidding the application of *modus tollens*¹⁶ to the “hard core”¹⁷ of the program express in the language of methodological conventions Kuhn’s idea that scientists in their activity do not always try to falsify the ruling paradigm, but to solve the puzzles it has provided.

These and other similarities, however, should not delude us. They do not mean that Lakatos has accepted Kuhn’s model of evolution of knowledge and of revolutions. The neutralization of the critique does not consist in the acceptance of its theses, but in such an attempt of their assimilation as would allow us to preserve our previous point of view in the most principal questions, and would render it immune to the opponent’s argument. More precisely, if according to Kuhn the transition from the old to the new paradigm cannot be explained only in terms of methodology, according to Lakatos his methodology explains just these transitions. It is just what I mean when I say that the modifications proposed by Lakatos are counter-reformation measures. So, let us see if the methodology of scientific research programs is indeed free from these shortcomings which the critique advanced against Popperian methodology.

The methodological criteria advanced by Lakatos concern two problems: first, how does the transition from one theory to another occur in the frames of the accepted research program; second, how does the transition from the old to the new research program occur?

The first problem does not seem controversial. The conception of falsification of the old theory not before the new one is accepted abolishes the arguments advanced against the methodology of Popper.¹⁸ It is compatible with the fact that scientific

¹⁶ Modus tollens: a reasoning in the following manner: if a statement *p* implies a statement *q* and its contrary *non-q*, then *non-p*. In other words, any statement which implies two contradictory statements must be rejected.

¹⁷ Hard core: name given by Lakatos to the body of hypotheses which form a program of research and which are maintained during the entire period of its realization.

¹⁸ Let us notice, however, that J. Agassi is right when he says that this conception is a deviation from the fundamental ideas of falsificationism. (Cf. J. Agassi, “Science in Flux,” in: *Boston Studies in the Philosophy of Science*, t. III 1967, p. 293-324. I believe that no matter how right is Lakatos’ conception, naming it *falsificationism* is nothing but *façon de parler*. If the term *falsificationism* has a definite meaning in the philosophy of science, it denotes a thesis that science evolves by means of successive falsifications of theories

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theories are not always refuted as soon as an empirical anomaly appears, and that sometimes they are refuted without any new empirical anomalies. What is questionable in this context, is Lakatos opinion that this conception can dispense with the methodological convention demanding the acceptance of background knowledge on which the scientific theories rest as unquestionable.¹⁹ This would be the case only if the background knowledge utilized in testing the two competitive theories were identical. And it is not at all obvious that the competitive theories, even belonging to the same program, do always rest on the same body of background knowledge. Anyway what Lakatos says about research programs does not make this condition granted. So, the thesis that the modifications introduced by Lakatos attenuate the conventionalist element of the Popperian methodology²⁰ is not evident.

Main doubts arise, however, in respect to the conception of transition from an old to a new research program, i.e., in respect to the methodological rules which are introduced in order to make possible the decision whether the old program is completely degenerating, and is to be replaced by a new one, or whether there exists a possibility of its future success.

Let us suppose that in trying to solve a problem posed by the accepted program we state an empirical anomaly, and according to the rule forbidding to use the *modus tolens*, we undertake some attempts to modify the protecting belt of hypotheses. In consequence we obtain a theory T', but it appears that we do not obtain a progressive series of theories (T' has consequences disconfirmed by experiments or provides no new consequences at all)²¹. Because the new theory cannot be accepted, the old is not falsified. However, the negative heuristic will not let us conclude that the general assumptions of the program belonging to its "hard core" are false. We are exactly in the same point where we were before advancing T'. What are the methodological rules indicating what is to be done in such a situation? Do we have

exposed to the most severe verdicts of experiments, as well as the methodological postulate of asking what are the facts which could contradict the accepted theory. Lakatos refutes this thesis as well as the methodological postulate.

¹⁹ Cf. I. Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in *Criticism and Growth of Knowledge*, p. 125.

²⁰ Ibid.

to repeat the trial, hoping that next time we will be able to formulate a theory which will satisfy the conditions of progressiveness, or do we have to refute the old program and look for a new? It is obviously the same difficulty with which the methodology of Popper had to deal on the level of theories. Introducing the concept of research program, Lakatos simply moved the difficulty from the level of theories to the level of programs. Solving the problem of choice between competitive theories, he created a new problem of choice between programs, and did not solve it satisfactorily.

The solution that we should not discard the old program before a new one appears, that is to propose the same solution as in the case of falsification of theories, is unacceptable for several reasons.

First, if all scientists accepted this rule, the new program would never appear. The rule proposed by Lakatos may be applied only if somebody has previously violated it, that is if somebody has advanced a new program before the old was falsified. Popper's ethically attractive prescription for undertaking the highest theoretical risk is here reduced to its contrary. It is so because in the case of an experiment disconfirming a theory the rules proposed by Lakatos say clearly: your theory is wrong, but do not reject it before being sure of obtaining a better one. But when we have to appraise the research program, the experiment says nothing about its value because the negative heuristics protects its "hard core" against falsification. We have the same reason to repeat the attempt aiming at the improvement of the old program, as to discard it, and look for a new. No methodological rules can solve the question of how long the attempts to save the old program are rational.

Second, let us suppose that we will remove the question of where the new competitive program comes from. We can assume that no research program has a monopoly, and that always different programs are in operation.²¹ But in order for the methodological rules proposed by Lakatos to be able to solve our problem, it is not enough that there exist several competitive programs. A more rigid condition must be satisfied, namely that one of these programs be able to give a solution of the empirical anomaly and to solve it without getting into troubles already

²¹ *Ibid.*, p. 116.

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solved by its competitor. It is obvious that this is not the usual situation. In the case of competition between programs it is not always so that one of them provides a progressive and another a degenerative series of theories. Before Louis De Broglie, neither the wave, nor the corpuscular theory of light was able to solve the empirical difficulties. The real crisis in science begins when none of the existing programs can provide a progressive series. So, even if we do not accept the conception of monoperadigmatic, or monoprogrammatic evolution of science, the problem of transition from the old to the new program remains unsolved.

Finally, we must ask what are the scientific research programs Lakatos speaks about? All the questions formulated above with respect to Kuhnian's paradigms seem to be actual here. Moreover a new one arises.

Lakatos gave the example of Cartesian metaphysics as an element of the research program of Cartesian physics.²³ From the point of view defended by Lakatos this example seems to be amazing. Did Lakatos abandon all the falsificationist program of demarcation between science and metaphysics? If so, I would not argue. But does Lakatos accept this point of view consistently? I regret that what he says about research programs is not clear enough to answer this question. He does not say explicitly whether the hard core of the program contains some statements which do not satisfy the falsificationist criterion of demarcation. The norm forbidding to apply the *modus tolens* to the programmatic assumptions seems to show that they are empirically falsifiable: in any other case the prescription would be needless. If so, these assumptions are not metaphysical in the falsificationist sense. However, the example of Cartesian metaphysics seems to prove the contrary.

So, it seems to me that the modifications introduced by Lakatos to the logic of scientific discovery do not solve the problem they were intended to solve. They do not explain in methodological terms the transitions from the old to the new research programs. The question whether these transitions can be explained in methodological terms only remains unsolved.

²² I believe that the opinion according to which several research programs are *always* in operation is as wrong as the monoperadigmatic conception of evolution of knowledge.

²³ Cf. *Ibid.*, pp. 126-127.

VI

I think that Lakatos' failure to prove that the logic of scientific discovery can provide a satisfactory basis for the reconstruction of the process of evolution of knowledge, has its sources in the accepted starting assumptions concerning the aims, the methods and the subject matter of philosophy of science. I mean first of all these assumptions which concern the possibility of solving the so-called problem of demarcation of science, and, second, those which limit the interests of philosophy of science to the analysis of the context of justification. I would like to say very briefly why I think these assumptions inadmissible if philosophy of science is to explain the process of growth of knowledge.

(a) First of all, if we accept Popper's argument pointing out that every criterion of demarcation must be of normative character²⁴ (and I think these arguments are cogent), then the philosopher of science can construct only a normative concept of science. Such a concept can, of course, be confronted with reality, but the results of such a confrontation are irrelevant for the appraisal of the normative concept of science and of its method just as human moral behaviour is irrelevant for the appraisal of moral codes. Such a concept of science may serve as a basis for the evaluation of history of science, but it is itself immune against any appraisal on the grounds of historical evidence. If Lakatos says that such a reconstruction can be criticized for its ahistorism,²⁵ we must notice that the limits of this critique are a priori determined by the normative concept of rationality. Facts which are incompatible with the normative reconstruction are taken as non-rational,²⁶ and expelled to the scrap-heap of the external history, which is irrelevant for the understanding of science.²⁷ The proposed rational reconstruction is endowed by a self-defending mechanism: the facts incompatible with it may be treated as non-rational and irrelevant. But at the same time the terms *rational* and *irrational*

²⁴ K. R. Popper, *Logic of Scientific Discovery*, p. 52; cf. *ibid.* and 9, 10, 11.

²⁵ I. Lakatos, *Falsification...*, p. 138.

²⁶ I. Lakatos, "History and its Rational Reconstruction," *Boston Studies in the Philosophy of Science*, t. VIII, p. 105-109.

²⁷ *Ibid.*

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signify in this context nothing but compatibility (or incompatibility) with the normative criteria of rationality. I do not mean to say that a normative philosophy of science is of no value. What I do mean to say is that a purely normative philosophy of science either is immune against factual arguments and cannot be judged on their basis as compatible or incompatible with history, or explicitly does not care about such a confrontation.

(b) Secondly, the solution of the problem of demarcation by means of normative criteria, as well as the restriction of the interest of philosophy of science to the context of justification, is based on the assumption that the criteria of demarcation and, by the same token, the criteria of rationality in science are permanent, historically unchangeable. If this is not the case, and I think that the history of science disconfirms this assumption, then a concept of science reconstructed on this basis is not a concept of science *tout court* but at best a concept of science of a certain historical epoch. What is more, if the criteria of rationality are not permanent in science, then it is not true that the context of justification is completely independent from the context of discovery. The limitation of philosophy of science aiming at the explication of the mechanism of the evolution of knowledge to the context of justification would have been justified however only if the way of testing, of accepting and refuting the theories of science were independent of historical conditions. So, the indubitable difference between *quid juris* and *quid facti* questions is not a sufficient basis for limiting the philosophy of science to the analysis of the context of justification. It is not enough to put forward just any difference as a basis for a demarcation line.

(c) Thirdly, because of the normative character of the criteria of demarcation the concept of science cannot embrace several factors playing an important role in the evolution of knowledge. And because of the character of these factors, the differentiation between the internal (intellectual) and external history of science cannot be treated as a satisfactory solution, unless, of course, this differentiation is purely analytical. On the grounds of normative criteria of demarcation science is not only delimited on its borders from the ocean of unscientific (metaphysical) beliefs, but it is also delimited "from above," i.e. from its whole methodological

superstructure and from epistemological opinions without which it never could exist.

It seems that the delimitation of purely cognitive endeavours (or of their results) from convictions and beliefs concerning their character, from methods according to which they should be performed, and from the goals at which they aim, is as impossible as the delimitation of consciousness from self-knowledge. And this is an essential problem not only in the domain of philosophy of science, but in the field of philosophy in general. This circumstance hardly facilitates the philosophical reflection on science, but if we want to avoid oversimplifications, we must take it into account. No matter how deep and unquestionable is the logical difference between the status of empirical and methodological or epistemological claims, it is impossible to study the growth of knowledge without taking into account their mutual links and impacts. So, I believe, that the principal question which must be solved in order to achieve an understanding of the development of science consists in the problem of mutual relationships between different levels of theoretical thought, rather than in drawing a demarcation line between science and non-science. And, as I said, it is not the problem of differentiating between the internal and the external history of science, but the question of delimiting the relevant factors of its internal (intellectual) development. The problem of growth of knowledge seems to be insoluble if we investigate the internal evolution of the field which is delimited in such a manner. The opinion which serves as a basis for all the attempts at solving the problem of demarcation, namely the opinion that all the claims of science are of the same logical status (are similar in respect to the distinction between empirical, analytical, normative and metaphysical statements) seems to be a wrong one, especially when we aspire by this means to reconstruct the evolution of knowledge.

So, in spite of the fact that on the grounds of all the proposed criteria of demarcation the analytical as well as the normative (methodological) statements should be excluded from the system called *science*, I think that they belong to this field, and perform a crucial role in its internal development. And what is more important, both of them change as the total field of science changes. Quine's remark that in the total field of science "there is much latitude of choice as to what statement to reevaluate in

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light of any simple contrary experience”,²⁸ concerns, in my opinion, both kinds of claims—the analytical as well as the methodological, or metascientific ones. I would only add, that the laws of logic which establish the connections between statements in the field, and the methodological rules for their testing, accepting or refuting, are elements of the field which are reevaluated only *in extremis*, i.e. when we cannot find any other means to reestablish the equilibrium of the field disturbed by the last conflict with experience. Therefore they change very seldom, are the most stable, the most permanent elements of the field. This gives rise to the delusion that the system changes and develops permanently on the grounds of the same rules, and that it is possible to formulate a definition of science *tout court*, or to solve the problem of demarcation pointing to the methodological rules whose requirements must always be satisfied by all the claims of science.

So, trying to answer the question “what is science and how does it develop?” we are faced by the alternative: either we determine science by the means of normative rules and criteria of demarcation, rationality and so on, treated as historically unchangeable and not belonging to the field which they delimit, or we treat the methodological rules and criteria as elements of this changing field. In this latter case they cannot serve as a principle of demarcation because of their own changeability. In other words: either we treat the methodological rules as permanent and thus they can serve as a means of demarcation, or we treat them as an element of the changing field, and then the changes they undergo require an explanation as well as the other modifications in the field. This explanation has to point out the relationships between them and the “boundary conditions”²⁹ of the field.

VII

Let us turn back to the problem of revolutions in science. When we are concerned with the relationship between the state of knowledge before and after a change named *revolution*, we have to deal with two different questions. First, does the new theory explain all the phenomena explained by its predecessor, i.e. does

²⁸ W. V. Quine, “Two Dogmas of Empiricism,” in: “*From Logical Point of View*”, 1961, p. 42-43.

²⁹ *Ibidem*.

the accumulation of knowledge really take place? And second, are the old and the new theories connected by the relation of correspondence?

Let us notice that the thesis on the correspondence between successive theories can be understood in two ways: (a) when the correspondence means that the old theory is formally (independently of its empirical meaning) a specific or a boundary case of the new; (b) when the correspondence means that statements of the old theory are after their respective relativization³⁰ not only true in the new theory, but moreover preserve their empirical meaning. In the first case we may speak of a formal, in the second, of a semantic correspondence between theories. And it is easy to show by means of historical examples that the formal correspondence between theories was satisfied in changes of the content of our knowledge which are usually treated as evolutionary, as well as in changes named *revolutions*. So, the polemic whether the relation of correspondence is, or is not preserved in scientific revolutions concerns the semantic version of the thesis.

The difference between the sense of the two questions (about accumulation and correspondence) results from the opinion that scientific facts are not bare empirical data, but interpretations of natural phenomena in terms of the knowledge and beliefs accepted previously. The same natural phenomenon can (but does not always have to) be another scientific fact in a different conceptual frame. Only if we, as the radical empiricists, do not perceive what part of scientific fact comes from the conceptual apparatus we use in order to give account of them, the problems of accumulation of knowledge and of correspondence between theories fuse together.

It seems obvious that the negative answer to the question concerning accumulation implies a negative answer to the question concerning semantic correspondence, but the opposite is not true. The new theory may explain all the natural phenomena explained by its predecessor without being tied to it by a relationship of semantic correspondence. If in one theory the free movement of the bodies is treated as their aiming at natural places in finite, physical space, in the next, as a motion along a cyclic orbit in the infinite, isotropic geometrical space, and in the third as a

³⁰ Cf. R. Suszko, "Formal Logic and the Development of Knowledge," in: *Problems of the Philosophy of Science*, t. III, Amsterdam, 1968, p. 210-222.

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motion along the geodesic in the finite but unlimited Riemann's space whose curve is determined by the distribution of masses, then it seems impossible to talk about a semantic correspondence between these theories.

If so, the question arises: if we accept, as we do, that some changes in the content of our knowledge are revolutions (in the sense that there is no semantic correspondence between successive theories), are we obliged to accept too, that the transition from the old to the new point of view occurs irrationally and cannot be rationally explained? Is it true that if two successive theories are not linked by a relation of semantical correspondence, then there does not exist any bridge which connects them, there exists no common conceptual frames for the two incommensurable theories? I believe, that the fact that we can never reach a suprahistorical point of view which could be a point of reference for the rational appraisal of two incommensurable theories, does not signify that the transition from the old to the new theory occurs irrationally. Lakatos as well as Kuhn are wrongly identifying the two theses. Accepting Kuhn's opinion that the pre- and post-revolutionary theories do not correspond (and I think that this thesis is a legitimate consequence of the Popperian critique of the conception of a purely empirical basis of science) we are, however, not compelled to accept that there are no ways for a rational transition to the new theory. On the other hand, accepting Popper's and Lakatos' opinion that such transitions occur in science rationally we are not forced to believe that it is due to the historically unchangeable criteria of rationality determined by the logic of scientific discovery. The fact that the criteria of rationality are changeable does not mean that they do not exist at all. So the problem arises: what are the conceptual frames in which scientific revolutions occur, and how do they determine the rational transition from the old to the new point of view?

VIII

I have pointed above to the ambiguity of the Kuhnian concept of scientific revolutions. Now, according to what has been said, we can differentiate, as I believe, three different kinds of changes in the content of knowledge: (a) such cases where the refutation

of a scientific statement and its replacement by a new one does not need a semantic reinterpretation of concepts by means of which these statements are explained. This is the case of evolutionary changes. (b) Such cases where the elimination of an empirical anomaly requires a semantic reinterpretation of concepts by means of which we have to explain the discovered fact, but only of those concepts whose scope of application is restricted to a specialized field of investigations. In this case I would speak of local revolutions. (c) Finally the cases where the elimination of empirical anomaly requires a change of meaning of those general concepts whose scope of application is not restricted to any particular field of research, and whose reinterpretation modifies the global world perspective. This would be the case of global revolutions.

If we differentiate the revolutions in science in respect to their scope, it is not enough to say that they consist in changes of paradigms or research programs and that the pre- and post-revolutionary points of view are incommensurable. We have to discover which paradigmatic assumptions were refuted, and which were preserved. And just those assumptions which were not destroyed by the crisis may constitute the frame in which the crisis was rationally surmounted and mutual communication between scientists was possible.

There are no such beliefs (including the methodological rules, the criteria of rationality, the concepts of experience and of truth) which in some point of evolution of knowledge could not be affected by the crisis. Hence, when we look from a long historical perspective backwards, the transitions which really occurred seem to be incomprehensible and rationally inexplicable, especially if the frames of common beliefs which made the transition possible were destroyed in the subsequent crisis. In order to reach an understanding of how the transition occurred, we have to construct not the supra-historical logic of scientific discovery, but the (scientific as well as unscientific) beliefs commonly accepted in the epoch. We have to look for rational modes of transition which were then available, and investigate how the scientists moved forward, surmounting the crisis and becoming "slaves" of the new point of view.

In every case of a crisis there exists a revocatory instance. If we do not perceive it, it is because the accepted criterion of

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demarcation excludes it from the set of factors determining the evolution of knowledge. But this "revocatory instance" is itself an element of the total field, and hence it is revocable temporarily, it can itself be menaced by a crisis. It is a judge who judges until he will be judged himself. If we differentiate the revolutions in science in respect to their scope and in respect to the beliefs whose refutation they demand, if we acknowledge not only the inhibitory role of "unscientific" beliefs, but also their regulatory role as frames in which the crises in science are surmounted, if we do not separate science from the human culture of which it is a part, then we can avoid choosing between considering the growth of knowledge either as a process obeying some permanent criteria of rationality, or as a series of irrational jumps from one point of view to another.

IX

What I said above about the difference between local and global revolutions needs, of course, further specification. Namely, it is indispensable to point out what are the concepts whose scope of application is not restricted exclusively to any special field of investigation, and whose reinterpretation modifies the global world perspective. Without pretending to exhaust the problem I will point out two kinds of such concepts.

(a) For each period of development of science there exists a *basic discipline*. Mechanics, for example, played this role in the 18th and 19th centuries, while physics seems to play it now. The thesis that physics, for example, is a basic discipline, signifies that it is believed (1) that each really existing object has some properties whose investigation is the goal of physics, and (2) that there exist such objects which do not have any properties beside those which are investigated by physics. If so, each really existing object, no matter what are its specific properties, due to which it may be investigated by other disciplines, may be characterized physically. This does not mean, of course, that the physical characteristics of the object provides an exhausting knowledge of the object. But it means that no scientific discipline can neglect the physical characteristics of the object it studies, and that while studying even the most specific processes, it still has to look for their physical background. Just in this sense the

global revolutions consist in modifications of those concepts of the basic discipline which are indispensable for a characteristic of each object of investigation, and which co-determine the ontology serving as basis for all scientific enterprise. It is obvious that not only the content but also the set of those concepts change historically. So, I would say that if a local revolution reorganizes a restricted field of research, and does not modify the ontological vision of the world, the global revolution modifies just this vision.

(b) A similar role in science is accomplished by the conceptions which concern man as the knowing subject. They co-determine the common frame of all cognitive efforts and furnish rules of all scientific investigation. Together with ontological conceptions they constitute what may be called the style of scientific thinking in the given epoch, or the heuristics of science, its regulative principles.³¹ What is characteristic for global revolutions, is the fact that they imply not only changes in the way of ordering the sphere of human experience, but also changes in men's cognitive relation to the world and their conception of themselves. They provide a new vision of the world as well as a new conception of cognition. Hence, the global revolutions in science are by the same token revolutions in the philosophical conceptions of man.

If we wished to give a shortened characteristic of the 17th century's scientific revolution, we could say that the position of God, who had been considered as a measure of everything, was taken over by the man. But the man, as the subject of cognition was by the same token endowed by some of the God's attributes: he was to be able to be an ideal observer, standing outside the world he investigates, and to achieve an absolute truth about it. Since Bacon and Descartes to Kant and Hegel this conception of man as a being capable of cognition co-determined the style of scientific thinking. We find it in the empiricistic as well as in the rationalistic epistemology. It was this conception which determined the concept of experience, of truth and falsehood, and of possible limits of human cognition. The contemporary science, by putting man as the subject of cognition into the

³¹ Cf. Helena Eilstein: "Hipotezy ontologiczne i orientacje ontologiczne," in: *Teoria i doswiadczenie*, Warsaw, 1966, p. 223-242.

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world, and depriving him of his privileged position, undermined by the same token its own epistemological assumptions. Today it is neither God, nor man, as knowing subject, standing outside the world, who is the measure of everything. It is nature itself. The theory of relativity, paradoxically in view of its name, states the absolute significance of the laws of nature, which are to be true for any knowing subject—but this subject is not transcendental in respect to nature.³²

If scientific cognition consists in some relation between the object and the knowing man, science cannot be practiced without assumptions concerning the world, as well as the man. These assumptions may be neglected insofar as the cognitive effort neglects self-analysis. But this self-analysis becomes indispensable when it transpires that the accepted assumptions, the regulative principles of scientific enterprise do not assure success any more. If we do not believe, as Kant did, that these assumptions and regulative principles are self-evident, if we believe that neither experience nor mind provides an unquestionable basis of cognition, we have to treat the regulative principles of science as products of human scientific efforts and to accept this bewildering fact that on the grounds of achieved results the human mind is able to criticize and change the assumptions due to which it achieved just these results.

The concepts and beliefs determining the global world prospective are usually called metaphysics. They are indeed in some sense untestable; they can be neither confirmed nor disconfirmed on the grounds of an experiment. But this does not mean that they are completely immune against a rational critique. We know perfectly that the metaphysical assumptions accepted in science are not everlasting; they are dismissed when they are not able to accomplish their heuristic function any longer, that is when they do not allow us to unify our knowledge into a coherent system, to surmount the local crises in sciences, when they do not provide a basis for a critical analysis of our knowledge. I would say that while they are untestable on the grounds of any single experiment, the global process of evolution of knowledge, the human cognitive experience gives them,

³² Cf. A. Koyre "De l'influence des conceptions philosophiques sur l'évolution des théories scientifiques," in: *Etudes d'histoire de la pensée philosophique*, Paris, 1961, p. 246.

however, some confirmation, or disconfirms them, of course not definitely. The same mechanism which introduces them into the realm of science compels us to abandon them.

The program of elimination of metaphysics from the realm of scientific knowledge would have been justified, if the acceptance or refutation of metaphysical beliefs were purely arbitrary, that is if science had no means at all for their rational appraisal, choice and elimination. I believe, however, that it is not the case.

First, because without heuristic programs providing the historically changeable criteria of rationality, scientific activity would be impossible. Secondly, because metaphysics in the realm of science is not simply a set of untestable beliefs, but a set of regulative principles constituting the heuristic programs which—though on another level of experience than the empirical statements—can be confirmed or disconfirmed as fruitful or sterile. It is by no means true that the rational critique in science is limited to experiments and observations.

Our knowledge is never a closed and coherent system. It would be a closed system if all the assumptions we really accept were stated explicitly, and if the critique of these assumptions needed no other assumptions. It would have been a coherent system if all the explicitly and implicitly accepted assumptions were mutually compatible.

If on the basis of a set of assumptions we may obtain a statement which cannot be proved true on the grounds of these assumptions, then the fact that on the grounds of the results we obtained we must change the assumptions is not so amazing as it seemed. On the contrary, this is the necessary condition for solving the problems raised by the evolution of knowledge.

If it is impossible to prove the coherence of a system of sentences on the grounds of the assumptions which serve as its basis, then our aspiration to have a coherent system of knowledge compels us to accept some new assumptions. But by the same token we reproduce the problem we aspired to solve. The philosophical sense of Gödel's theorems seems to point to the conclusion that when the theoretical thinking aspires to find and justify the assumptions on which it is based, it must by the same token transgress these assumptions.