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I. INTRODUCTION

It is customary to say that the theoretical considerations on science, that is, on its history, its relations with other spheres of the human consciousness and activity, its developmental trends and prospects, its logical structure and methodological assumptions — are intended to get an *understanding* of it. But it is all too evident that actually this eventual target is differently conceived of within the disciplines studying science — history, sociology, psychology or methodology. To put it differently, each discipline isolates one specific aspect of the whole phenomenon and tends to get an understanding of precisely this aspect. Such a specialization is normal and inescapable in the process of developing any knowledge, not only of science, and it does in fact yield a more profound knowledge of the aspect concerned.

But the adverse effects of specialization are equally well known. As a rule, the danger involved in specialization grows in proportion to the degree of the actual interconnection between the particular aspects of the phenomenon under study, that is, when it becomes more and more indispensable to inquire into their reciprocal relationship, *also for an understanding of each of them separately.*

I think, and it is not only my opinion, that we face such a situation at present in the domain of the study of science. The present considerations deal therefore with the mutual relationship between different disciplines having science as their common object of inquiry, primarily between the methodology, the history and the sociology of science. Since it is in the problem of development of science that these disciplines get into their most direct contacts, this problem is primarily selected to exemplify the theoretical problems that are of interest to us.

The demarcation lines between the different disciplines studying the same object are as a rule conventional, arbitrary in character. They are being laid down in the course of the historical development of science and of research methods, and new achievements in knowledge may frequently impose the necessity to transgress them. But conventions may imperceptibly petrify into dogmas which hinder any further developments. This may be a consequence either of the researchers' habit of shunning the traditional boundaries of the aspect of reality "assigned" to them — a habit conveyed by the masters to their disciples in the course of teaching — or of the emergence of new theories sanctioning the conventional demarcation lines.

A specific trait of specialization is that it leads to formulating definite research programmes, which, as a rule, are getting increasingly narrow; these programmes, in turn, define the problems that must be solved and, moreover, imply the criteria of accepting the proposed solutions. In other words, the conventional boundaries between particular disciplines studying the same object provide the foundation for formulating research programmes and thus predetermine the questions that will be asked, those that will be skipped as "irrelevant", and those that will simply go unnoticed. Thus, to take up a study of these problems it may occasionally be necessary to reconsider the previous conventional distinctions between the "fields of competence" of different disciplines. Both the older and the more recent history of science furnish quite a number of such situations.

In the case of studying a social phenomenon — and science is unquestionably one — another factor comes into play: its own dynamics. Thus the manner of studying the phenomenon concerned must take into account ist place within the framework of social life accordingly and undergo changes together with the transformations of its object. Consequently, the conventional demarcation lines between disciplines studying the same phenomenon as well as the research programmes that are founded on them may require some modification also in result of circumstances that are, so to say, external with respect to the research process itself. At any rate, it cannot be apriorily precluded that the developmental dynamics of science in our epoch and the transformations in the sphere of its social functions will also require some modification of the traditional way of inquiry on science.

Therefore it seems worth considering how the demarcation lines between the particular disciplines studying different aspects of science have come to be laid down, how and in what sense they are theoretically sanctioned, and what are the consequences of this for the understanding of science in general and of its particular aspects.

To arrive conveniently at answers to these questions let us turn to the philosophy of science, as it is in this domain that the demarcation lines have been explicitly formulated. The impact of philosophy on the way of studying their research problems by other human disciplines needs no special comments.

II. THE CONTEXT OF DISCOVERY AND THE CONTEXT OF JUSTIFICATION

In his discussion of a fairly common view on the subject of philosophy of science Herbert Feigl wrote: "There is a fair measure of agreement today on how to conceive of *philosophy* of science as contrasted with the history, the psychology, or the sociology of science. All these disciplines are *about* science, but they are 'about' it in different ways... In the widely accepted terminology of Hans Reichenbach, studies of this sort pertain to the *context of discovery*, whereas the analyses pursued by philosophers of science pertain to the *context of justification.* It is one thing to ask how we arrive to our scientific knowledge-claims and what socio-cultural factors contribute to their acceptance or rejection; and it is another thing to ask what sort of evidence, and what general, objective rules and standards govern the testing, the confirmation or disconfirmation, and the acceptance or rejection of knowledge-claims in science." [1]

What we have here is a distinct demarcation line between the philosophy of science and the other disciplines studying science, a line laid down in virtue of distinguishing between two kinds of questions — about the origin and about the way of justification, *i.e.* historical *vs.* logical questions. H. Feigl is certainly right in saying that this is a widely accepted view on the contemporary philosophy of science. [2]

It is all too clear that the questions about how we arrive at definite knowledge-claims or about their socio-cultural determinants are not identical with the questions about their logical value or the way of their justification; moreover it is obvious that if we fail to realize this difference we get involved in misunderstandings. As a matter of fact, we have here to do with the old Kantian distinction between the *quid facti?* and the *quid juris?* questions.

But it is one thing to distinguish between the respective meanings of these questions, and it is a different thing to conceive this difference as a basis for a demarcation line delimiting the scope of interest of the philosophy of science, or as basis for a methodological directive how the study of philosophical problems ought to be pursued.

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[1] H. Feigl, *Philosophy of Science,* in: R. M. Chisholm, *et al.,* Prentice Hall, 1964, p. 472.

[2] Cf. K. R. Popper, *The Logic of Scientific Discovery,* London, 1959 (extended English translation of *Logik der Forschung,* Vienna, 1935), ch. I, § 2.

But, actually, why should we stick to that demarcation line? Because it is a useful convention or perhaps because some theoretical reasons suggest it? At any rate, from the fact that asking about the justification and asking about the origin of a knowledge-claim or proposition are two different things it does not follow that the philosophy of science ought in principle to study the former only. To justify such a view, it is indispensable to take recourse to some additional premisses. What these premisses should indicate is that there is no relationship between the way of justifying propositions and the way of arriving at them (*i.e.* the socio-historical conditions of practising scientific research work), or at least that no such relationship that may be *essential for the understanding of science* does exist. They should indicate that the understanding of science is equivalent to understanding its logical structure and the logic of its development, that is, the logic of passing from one theory to another, but that the history of science and its sociology contribute nothing essential to that understanding. It is known that such premisses are furnished by a definite philosophy. But the trouble is that they are furnished together with the construction of a definite conception of science, which may not be indisputable and is certainly one-sided only.

It is obviously impossible to refuse to anyone the right to restrict his interests to the study of the problems referring to the context of justification of scientific propositions or even to call them „philosophy of science". To put it formally, if the distinction between the context of discovery and the context of justification is conceived as a demarcation line delimiting the scope of problems falling within the realm of philosophy of science no consequences must result from this decision for the solution of specific problems: the problems that are located outside the area delimited would be simply excluded from the scope of the philosophy of science and turned over to some other discipline which would apply its own specific methods in dealing with them. It is of no importance to which discipline the particular problem will be assigned as no consequences concerning its solution follow from such assignements.

But actually the situation is different. The above distinction performs usually a different role than that of formally delimiting the scope of problems pertaining to the philosophy of science; what it actually does is to provide the foundation of the methodological directive indicating not which problems are to be assigned to the philosophy of science but rather how these problems, which — by some other principle or simply by tradition — are deemed to constitute its subject, ought to be studied. Now this must necessarily decide about the way in which they are to be solved and thus cannot be an irrelevant issue.

For instance, if one assumes that the philosophy of science is interested exclusively in the context of justification and simultaneously argues — as does Popper — that the problem of development of knowledge is not only a relevant but a central field of philosophical inquires,[3] then he necessarily reduces this problem to the logical questions of development of knowledge and believes that the logic of scientific discovery gives its *satisfactory* solution. (The reason for italicizing the word "satisfactory" will be clear in the next section.)

An excellent example of the function performed by the distinction discussed is the polemic on the book by T. S. Kuhn on *The Structure of Scientific Revolutions.*[4] The core of the discussion was Kuhn's thesis that in order to understand science we have to go beyond logic and methodology, to take recourse to categories of social psychology and of the sociology of scientific communities, that the methodological directives are by themselves insufficient "to dictate a unique substantive solution to many sorts of scientific questions".[5] Kuhn argues that to understand the development of science we have to study the values which scientists adopt as their guidelines in scientific research work and the institutions which manage or organize that work. He writes: "The explanation must, in the final analysis, be psychological or sociological. It must, that is, be a description of a value system, an ideology, together with an analysis of the institutions through which that system is transmitted and enforced."[6]

None of the critics of Kuhn's view argued that the problem of development of scientific knowledge, which in his opinion cannot be understood without going beyond the limits of the logic of scientific discovery, falls outside the domain of philosophy of science. But what they tried to show was that — in spite of Kuhn's arguments — the solution of this problem within the philosophy of science restricting itself to a study of the logic of development of konwledge is, or at least could be, *satisfactory.*[7]

Let us incidentally remark that whether or not a solution is recognized as *satisfactory* is largely dependent upon the programme of philosophical studies, that is, upon the opinion on what it is to and what it

[3] Cf. *ibid.*, "Preface" to the English edition.

[4] T. S. Kuhn, *The Structure of Scientific Revolutions*, Chicago, 1962. In 1965 an international symposium on the book by Kuhn was held in London. The proceedings of the symposium were published as *Criticism and Growth of Knowledge* (ed. by I. Lakatos and A. Musgrave, Cambridge University Press, 1970). Kuhn's reply to his critics was published as *Reflections on My Critics,* in: *Criticism...,* pp. 231–278 and in his "Postscript" to the second English edition of his book (Chicago, 1970). The Polish translation of Kuhn's *The Structure* appeared in 1968 with my "Postscript" (cf. *Struktura rewolucji naukowych,* PWN, Warszawa, 1968).

[5] T. S. Kuhn, *The Structure...*, p. 3.

[6] T. S. Kuhn, *Logic of Discovery or Psychology of Research?,* in: *Criticism...,* p. 21.

[7] I. Lakatos justifies this view in *Falsification and the Methodology of Scientific Research Programmes,* in: *Criticism...,* pp. 91–197. Impressed, no doubt, by Kuhn's criticism, he modifies in this study the methodological view of Popper. Cf. also S. Amsterdamski, "The Dispute over the Conception of Progress in the Development of Science" (in Polish), *Kwartalnik Historii Nauki i Techniki,* 1970, No. 3, pp. 489–506.

can provide. Thus, a solution which is *satisfactory* from the point of view of one programme may of course be *unsatisfactory* within another, if the latter has different tasks in view or pursues different objectives. Therefore if we say of a solution that it is *unsatisfactory* we must specify whether it fails to fulfill the requirements put up by the programme within which it has been obtained or whether we mean to say that we refuse to accept that programme of research, and that not necessarily because it fails to cope with the problems in accordance with its own assumptions but for other reasons; for example we may regard it as being too narrow or limited, in that it omits a number of questions that in our opinion are essential. This is the well-known difference between *immanent* criticism and *external* criticism, that is criticism from different standpoints.

Correspondingly, in our case we ought to distinguish between the following three questions: First, what tasks does a philosophy of science restricting its inquiries to the study of the context of justification, *i.e.* the logic of scientific discovery, put up for itself; second, whether or not the analysis of, *e.g.* the problem of development of scientific knowledge in terms of logic is satisfactory in view of these tasks imposed by the programme on itself; and, third, whether there are any arguments supporting the view that even if that programme makes possible a satisfactory solution of the problems it tackles it nevertheless requires some modification (extension?) for it fails to solve problems that must necessarily be solved if any understanding of science is to be achieved Anyone bringing up such arguments would obviously think that the understanding of the development of science provided by the criticized programme is unsatisfactory, too narrow, one-sided.

Finally, it must be observed that the criticized programme may be refuted for the two above reasons together: both because it provides unsatisfactory (with respect to its own requirements) solutions of the problems tackled and because it is narow, too limited; it can be thought that it is precisely the set of its programmatic assumptions that are responsible for its intrinsic difficulties and that they could be overcome only by modifying them.

This is our view, which we are going to justify below: the present text, though, will primarily treat about the limitations of the mentioned programme rather than about its intrinsic difficulties.

The programmatic restriction of philosophical analysis of science to the content of justification is linked with a specific articulation of the object of its study. Science is treated as a set of propositions recognized (whether synchronically or diachronically) as true, complying with definite rules of methodology, that is, as a ready product of human cognition objectivized in the form of intersubjectively communicable and intersubjectively controllable propositions. Thus, the understanding of science, which is to be furnished by the philosophy of science, is an understanding of only one aspect of it, even if we realize that it is not the only aspect which deserves analysis. Within the boundaries of such a programme of philosophy of science it is impossible to reach an understanding of science as a definite complex of cognitive activities in all their determinations (not only the logical), as a specific human activity being a product of a definite culture and constituting a definite "subculture" dynamically interrelated with the overall cultural system in which it is incorporated, as a social institution performing definite functions within the given civilization and simultaneously subjected to its reciprocal effects.

This programme deliberately abandons such an understanding of science as its task. It employs a supra-historical model of science as a product of rational cognitive activities the criteria of which remain always the same, a model of scientific activity whose only intent it is to multiply the data about the world and to check those obtained previously but not, *e.g.*, any utilization of those data.

Accordingly, the task of the philosophy of science would consist in formulating rules and criteria the application of which would secure the maximum effectiveness to the course of the process. *The philosophy of science is to be of a normative character;* it is to speak not about how science *is* being pursued but about how it *ought to* be pursued in order to be most rational, to secure the rapidest possible increment of information and the most efficient elimination of errors.

If, for instance, K.R. Ropper and I. Lakatos think the philosophy of science has as its task a "logical reconstruction" [8] of the development of scientific knowledge in the "third world", [9] and that this reconstruction is to be a model of a rational research procedure and as such is to serve as a frame of reference for the appraisal of the history of science, and as a normative model of research procedure in the future, then, of course, the argument that that reconstruction fails to represent the actual course of development of scientific knowledge is irrelevant from their point of view. The reconstruction was not aimed at obtaining the picture of that development, and — stil more — this is even impossible since

[8] K. R. Popper, *op. cit.*, pp. 31–32, wrote: "This reconstruction would not describe these processes as they actually happen: it can give only a logical skeleton of the procedure of testing. Still, this is perhaps all that is meant by those who speak of a 'rational reconstruction' of the ways in which we gain knowledge."

[9] "The first world is that of matter, the second the world of feelings, beliefs, consciousness, the third the world of objective knowledge articulated in propositions. This is an age-old and vitality important trichotomy; its leading contemporary proponent is Popper" (I. Lakatos, *History and Its Rational Reconstructions*, p. 25; this study will appear in the next issue of *Boston Studies in the Methodology of Science*, ed. by R. Cohen and R. Buck, in 1971. The author has kindly provided a mimeographed transcript). Cf. also K. R. Popper, *Epistemology without a Knowing Subject*, in: *Proceedings of the Third International Congress of Logic, Methodology and Philosophy of Science*, Amsterdam, 1968, pp. 333–373.

it is to be of normative nature. The only meaningful question could be whether the proposed model of research procedure is *realistic* (as opposed to utopian), whether it is practically feasible.

It may be observed, incidentally, that whereas Popper assigned to the philosophy of science the task of furnishing a model of rational research procedure to scientists and his requirement to submit scientific propositions to the possibly strongest tests of falsification is in its character a norm of scientific ethos, Lakatos holds a more reserved view. He seems to doubt whether there are any feasible possibilities to secure success in this task. He says that the fundamental task of methodologies is not to provide models of procedure but that "modern methodologies" or "logics of discovery" consist merely of a set of (possibly not even tightly knit, let alone mechanical) rules for the *appraisal* of ready, articulated theories. Often these rules, or systems of appraisal, also serve as "theories of scientific rationality", "demarcation criteria", or "definitions of science". [10]

What has been said so far seems to be sufficiently illustrative of the tasks assumed by a philosophy of science restricting itself to studying the context of justification and of the way it articulates the object of its inquiry. Since science is conceived of merely as a ready product of intellectual inquiry and studied as a set of statements isolated from the knowing subject (whether individual or collective), objectivized and transferred to the "third world", then it is obvious that the "understanding" of science can be achieved merely through a logical analysis. This is a consequence of the preliminary assumption adopted earlier. From this point of view, any requirement that philosophy of science go beyond the study of the context of justification is meaningless: for what may the psychology, history or sociology of science have to say in that "third world" of objective knowledge articulated in statements? The result is that we have to do not with a philosophy of science as a human activity but with a philosophy of science "without the knowing subject".

I have said at the outset (p. 3) that to restrict the philosophy of science to studying the context of justification requires moreover some other premiss in addition to distinguishing between the sense of questions about the origin and about the justification of knowledge-claims; such premiss should indicate that there is no relevant relationship between the way of justifying statements on the one hand and the way of arriving at them on the other (relevant for the understanding of science). Now, after what has been said above, it seems to be clear that that premiss is implicit in the assumption that the philosophy of science has its

[10] I. Lakatos, *History...*, p. 2.

object in the "third world" of ready knowledge articulated in propositions. In fact, no such relationship exists in that world. To deny this would mean refuting Husserl's critique of psychologism; notwithstanding the many differences, this critique performed an essential role in the formulation of a programme of philosophy of logical empiricism in the 1930's. Whereas Husserl argues that the laws of logic do not refer to *how we do think* but *how we ought to think* in order to think correctly, logical empiricism sees the philosophy of science reduced to logic saying not *how science is practised* bud *how it ought to be practised* in order to be practicized correctly (rationally). The fundamental difference between Husserl's understanding of the laws of logic as belonging to the world of ideal relations and the neopositivist conception of them as rules of language is here inessential.

But if by science we mean man's cognitive activities and not only its product, if the philosophy of science has as its object the actual world of cognitive activities, then *on this level of analysis* the distinction between the context of justification and the context of discovery is not a foundation for drawing a demarcation line delimiting the scope of the philosophy of science. Thus it can be said that the restriction of the tasks of philosophy of science to the analysis of the context of justification results from the manner in which the object of its inquiries has been articulated.

It seems safe to say that such a limited understanding of science, or — most strictly — the restricting of the philosophical study of science to the afore-mentioned single aspect only, is philosophically determined by a radical constrasting or separating the sphere of activity from the sphere of cognition. It is this limited understanding of science that makes the philosophy of science treat science merely as cognition and study it as a product of intellectual activity independent of the knowing subject. It leads to studying the cognitive activity as free from any involvements other than those in logic.

This is not to say that the above procedure fails to give any understanding of science or of its history; such a contention would be equivalent to denying any importance of rational thought in the creation of science, and thus a complete nonsense. But I am convinced that what we obtain is a very one-sided understanding, for it is an understanding of science as alienated from the context of its practising and functioning in the actual world of human culture and civilization. Moreover, it is also a suprahistorical understanding in the sense in which the logical rules and methodological criteria employed in the reconstruction are supra-historical.

Nor do I think that the philosophy of science can do without such an analysis. But I am convinced that the fact of confining oneself to it cannot be justified in any other way than by taking recourse to a definite (*i.e.* historically and sociologically determined, and thus not supra-historical) programme of philosophical inquiry and of understanding science.

III. TWO CONCEPTIONS OF SCIENCE

What has so far been said was intended to indicate at the limited character rather than at the immanent inefficiency of a programme of philosophy of science which, by principle, should be merely interested in the context of justification and which conceives of science as merely a world of objective knowledge articulated in propositions. Clearly, with such preassumptions the philosophy of science cannot extend beyond the study of the logical structure of knowledge and of the logic of its development. Let us now consider some consequences of that opinion.

The most direct contact between the methodology, the history, and the sociology of science is achieved in the problem of development of knowledge. The logical reconstruction of the development of science, which is the aim of methodology, cannot, by definition, be a description of actual history. As a rule, it has to turn out that neither in the past nor today scientists follow exactly the rational models of procedure elaborated by methodologists. This is caused by the fact that the model is normative and not descriptive in character.

But the question may arise as to what are the causes of that presupposed discrepancy between the rational model and the actual course of history. Does this discrepancy result from factors that are secondary in importance for the development of science or from such that belong to it "essence"? It depends upon the answer to this question whether the model, which is being constructed as normative in character, may practically serve as a model for research procedure. For, if the discrepancies result from, say, "subsidiary" causes, then there is at least the possibility that the model could serve in practice as a model of research procedure and scientists could make use of the suggestions contained in it. But if the discrepancies result from the "essence" of the process of development of knowledge, then its normative usefulness is utopian.

The answer to this question is also important in view of the fact that it implies a definite view on the "nature" of science and of cognitive activity.

The first answer to the above question is that the discrepancy between the model and the reality is caused by factors that are "subsidiary", to science, factors that have nothing to do with its "nature", specifically by metaphysics, myths, prejudices, errors or perhaps by peculiar historical circumstances in which science may be practised. In a word — by factors which are external to it, which distort or disturb its normal, *i.e.* rational course of development.

If we accept this answer we also accept the view that scientific activity has always the same, supra-historical objectives to pursue, that it paves its way through diverse external vicissitudes, and that were it not for such external disturbancies it would follow precisely the path laid down by the logic of scientific discovery as constructed by methodology. In such a case, the history and the sociology of science are indeed irrelevant, or inessential, for the understanding of science; inessential at least to the extent in which they explain not the principal line of its development (which is explained by logic, whereas history can do no more than provide the necessary facts) but the adventitious deviations from it. Inessential to the extent in which they explain not what in its development is "natural", "rational", but what is "pathological"; not what conditions this development but what accounts for its disturbance, even if it is inescapable, for no one would say that science develops in a vacuum or in a "sterile" environment.

If such a conception of science is accepted it is indeed to be expected that a rational model of development of science may efficiently perform its normative functions: that is, to indicate the way to follow for scientists, to provide the "criteria of rationality", the "criteria of demarcation", the "definitions of science" that may prevent them from, or warn against, swerving from the main road of development of knowledge, against the deviations induced by the external, extra-rational (historical and sociological) conditions of practising science.

A philosophy of science which restricts itself to the study of the context of justification as sufficient for the understanding of science or of its development is in fact guided by that supra-historical concept of scientific activity which always and only pursues the one end of multiplying the valid (from the point of view of the rules formulated by it) information on the world. In accordance with its preliminary assumptions, such a philosophy of science assigns also the respective tasks for the other disciplines studying science. In terms of this conception, the historian of science must, on the one hand, "determine by what man and at what point in time each contemporary scientific fact, law, and theory was discovered or invented. On the other hand, he must describe and explain the congeries of error, myth, and superstition that have inhibited the more rapid accumulation of the constituents of the modern science text."[11] The materials provided by the history of science would in its part serve as an illustration of the logical scheme of development of knowledge and would in part account for

[11] T. S. Kuhn, *The Structure...*, p. 18.

the deviations from the methodological model. The sociology of science would fulfil a similar task: it would explain the social determinants of some or other deviations from the "ideal course" of the growth of knowledge, it would explain the possible inhibitions or accelerations in its rate, *etc.*

The view presented above has found both express and interesting formulation in the recent works of I. Lakatos. He defends against Kuhn Popper's idea that the logical reconstruction of the process of growth of science furnishes a satisfactory explanation of that growth. Lakatos refutes Kuhn's contention that the logic of discovery has to be complemented by social psychology and by sociology of scientific communities. Thus he writes that "the philosophy of science provides normative methodologies and in terms of these the historian reconstructs 'internal history' and thereby provides a rational explanation of the growth of objective knowledge." [12] But though he adds that a rational reconstruction never exhausts all history, that "history of science is always richer than its rational reconstruction", [13] and thus "any rational reconstruction needs to be supplemented by an empirical (socio-psychological) 'external' history", [14] his preliminary postulate makes him admit that the "external history is irrelevant for the understanding of science". [15] The "internal" history, which he himself describes as "a history of events which are selected and interpreted in a normative way", [16] explains what is rational in the development of science: whereas "external" history explains what cannot be included from the actual history in the rational reconstruction founded on a given logic of scientific discovery, that is, what in terms of that logic turns out to be "irrational" or "extra-rational". "When history differs from its rational reconstruction, 'external' history provides an empirical explanation of why it differs. But the rational aspect of scientific growth is fully accounted for by one's logic of scientific discovery." [17] This, incidentally, is obvious since this logic defines the criterion of rationality itself. Thus the circle is closed: first it is said that the logic of scientific discovery determines the criterion of rationality, next that it constitutes the foundation for the reconstruction of internal history, and, eventually, that what cannot be squeezed into this reconstruction is irrelevant for the understanding of science, for its rational aspect is wholly covered by the "internal history". Do the words "irrational" or "extra-rational" mean anything more in this context than "not falling with the model of development I have adopted"?

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[12] I. Lakatos, *History...*, p. 1.
[13] *Ibid.*, p. 23.
[14] *Ibid.*
[15] *Ibid.*
[16] *Ibid.*, p. 27.
[17] *Ibid.*, p. 23–24.

Lakatos is doubtlessly right in saying that every history of science is always written from the point of view of a definite philosophy of science. He is right, for it is philosophy that furnishes the ideas as to what is science, and what is its course of development. His own philosophy is no exception here. In this respect he gives a remarkably interesting analysis of how inductionism, conventionalism and falsificationism influenced the historiography of science.[18] I do not think, however, that his own conception defends itself against the arguments brought up against these theories.

Moreover, he makes an interesting point (and supports it with interesting evidence) that within the context of different logics of discovery the demarcation line between "external" and "internal" history of science will run different courses from case to case. Facts that may be covered by a rational reconstruction by one logic discover, may have to be transferred by another logic of discovery to the "external" history as they are "not rational and have to be explained in terms of 'external' history".[19]

Lakatos' own methodology of scientific research programmes,[20] which he worked out in reply to Kuhn's critique of falsificationism, makes it no doubt possible to include in the rational reconstruction more than that of Popper. But it still fails to solve the fundamental issue: to explain in methodological terms the transition from one research programme to another in the case of a scientific revolution.[21]

No doubt both Lakatos and Popper would like the logical reconstruction of the development of knowledge to comprise as many facts from actual history as possible, hence they take full account of the history of science and are outstanding experts on it. But at the same time neither of them accepts it as the yardstick of the proposed reconstruction. "In writting a historical case study [writes Lakatos] one should, I think, adopt the following procedure: (1) one gives a rational reconstruction; (2) one tries to compare this rational reconstruction with actual history, and to criticize both one's rational reconstruction for lack of rationality".[22] But this is inconsequential, since the admissible limit of criticism of a reconstruction for its ahistorical character is the adopted conception of a suprahistorical development of science.

It must be added here that both Popper and Lakatos differ essentially in their view from the philosophy of science of the logical empiricism of the 1930's, which was interested exclusively in the problem of the logical structure of ready knowledge and not in the logic of its

[18] Cf. *ibid.*, ch. I: "Rival Methodologies of Science". Cf. also J. Agassi, *Towards an Historiography of Science*, Mouton, 1963.

[19] I. Lakatos, *History...*, p. 21.

[20] I. Lakatos, *Falsification....*

[21] Cf. S. Amsterdamski, *op. cit.*, esp. p. 502.

[22] I. Lakatos, *Falsification...*, p. 138.

development, in its actual dynamics. There, history of science could be merely a target of criticism because it did not fulfill the criteria of being scientific, meaningful *etc.* A subject of criticism — because it used to turn out that "for rather obscure reasons" scientists in the past proceeded differently than they should have.[23] And since the criteria of research procedures are formulated in virtue of the logical analysis of the currently reigning theories and treated as a supra-historical model of appraisal of all cognitive procedures, the history of "genuine" science commences actually with the last revolution in the development of knowledge. To render it in some caricature, that would mean that history of science does not exist at all: for what we regard as history of science is a history of errors, misinterpretations, a history of views that in the light of the methodological criteria and rules of that philosophy do not deserve the name of science. Lakatos is right in writing that philosophy "has never generated a programme of historical reconstruction. ...As an epistemological programme it has been degenerating for a long time; as a historiographical programme it never started."[24]

But disregarding the essential differences between the views of Popper and Lakatos on the one hand, and the programmatically ahistorical versions of the philosophy of science on the other, from our point of view another fact is of interest: namely, that their common restriction of the interest of philosophy of science to studying the context of justification and treating scientific activity as a process essentially independent of the historical conditions in which it is being practised (these, as it has been said, may only disturb, accelerate or inhibit it) must necessarily lead to a "liquidation" of the actual history of science or to splitting it into "internal" history, which fits the reconstructed logic of development of knowledge, and "external" history, which is irrelevant for the understanding of science as it does not fit into the rational reconstruction.

Moreover, it is evident that if we agree that the only task of philosophy of science is the logical reconstruction of the development of knowledge, there are no such discrepancies between this reconstruction and the actual history that would essentially disparage this reconstruction, for it is assumed in principle that such discrepancies are inescapable. Historians and sociologists may possibly tell us *ex post* why such discrepancies did occur, though in terms of a rational development of

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[23] H. Feigl, *op. cit.*, p. 505, wrote about the changes in the views of some representatives of that trend: "Some thinkers like Rudolf Carnap, W. S. Sellars, and K. P. Feyerabend have recently concluded that the whole story of the hierarchical level structure [of scientific theories] should be abandoned. Their new and different account deals with the historical succession of law and theories in terms of radical replacements rather than in terms of a standing hierarchy level." Incidentally, this is not the only change in the views of the "neopositivists" from that time.

[24] I. Lakatos, *History...*, p. 28.

science they should not have occured at all. Thus, even if a history of science is always written from the point of view of some philosophy, it is of no importance to the latter; more specifically, what cannot be explained as rational activity within the context of a definite logic of scientific discovery is relegated to the "laystall" of "external" history, which "is irrelevant for the understanding of science as a rational activity".

But also the reconstruction of the "internal", "rational" history of science generated by this philosophy cannot, by definition, be anything else than the history of ideas expounded *modo geometrico*. As a description of actual history it is false; as a normative proposal of a rational research procedure it is utopian: neither the creation of science nor its functioning occur in the "third world" of pure rationality. Accordingly, here we have to do not with the philosophy and the history of science as a human activity in the real world — an activity conditioned by a number of variable social and historical factors and intended to yield various values — but with a philosophy and a history of a supra-historical projection of science into the objective world of ideas and rational thinking.

The second answer to the afore-mentioned question about the cause of the discrepancy between the models of rational research procedure constructed by methodology and the actual history of science is essentially simple, which of course does not mean that it involves no intrinsic difficulties of its own. This answer consists in refuting the question as being incorrectly formulated, for it takes as its point of departure a false, one-sided conception of science and of cognitive activity. Provided this conception is refuted, the problem in question disappears together with it too. This is, I think, one of the fundamental tenets of Kuhn's book.

Let us take a closer look at this view. For, irrespective of the imperfections and intrinsic difficulties pointed out by its critics, [25] it opens a new way to an untraditional understanding of science and, accordingly, to a different direction of the theoretical (including philosophical) reflection on science.

Kuhn adheres to the (now increasingly frequent) approach of those historians who "rather than seeking the permanent contributions of an older science to our present vantage, attempt to display the historical integrity of that science in its own time", [26] that is, to understand it as a specific aspect of the intellectual culture of the respective epoch. That

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[25] Cf., *e.g.*, the studies by J. Watkins, K. R. Popper, I. Lakatos, M. Masterman, P. K. Feyerabend in *Criticism*... Cf. also my "Postscript" to the Polish edition of *The Structure*... (*Struktura rewolucji naukowych*, PWN, Warszawa 1968, *Posłowie*).

[26] T. S. Kuhn, *The Structure*..., p. 3.

this approach is fruitful is attested if only by the works of A. Koyré,[27] or by Kuhn's previous book on the Copernican Revolution,[28] or perhaps F. Jacob's book on the history of genetic theories.[29] These historians do not ask "about the relation of Galileo's view to those of modern science, but rather about the relationship between his views and those of his group, *i.e.* teachers, contemporaries, and immediate successors in the sciences. Furthermore, they insist upon studying the opinion of that group and other similar ones from the viewpoint — usually very different from that of modern science — that gives those opinions the maximum internal coherence and the closest possible fit to nature."[30]

To expose the justification of such an approach let us once more take recourse to a quotation: "The more carefully they study, say, Aristotelian dynamics, phlogistic chemistry, or caloric thermodynamics, the more certain they feel that those once current views of nature were, as a whole, neither less scientific nor more the product of human idiosyncrasy than those current today. If those out-of-date beliefs are to be called myths, then myths can be produced by the same sorts of methods and held for the same sorts of reasons that now lead to scientific knowledge. If, on the other hand, they are to be called science, then science has included bodies of belief quite incompatible with the ones we hold today. Given these alternatives, the historian must choose the latter. Out-of-date theories are not in principle unscientific because they have been discarded. That choice, however, makes it difficult to see scientific development as a process of accretion."[31]

The fundamental meaning of this view, in which a well-conceived historicism is well observable, is easy to grasp. If we approach the study of the history of science with an *a priori* supra-historical conception of what is scientific, a conception borrowed from some or other methodology, if we study history employing the "criteria of rationality", "the criteria of demarcation" or the "definitions of science" pertaining to that methodology, then we attempt to squeeze actual history into a rigid mould imposed on it from outside. Then, as Lakatos puts it, "history of science is a history of events that have been selected and interpreted in a normative way". But when we treat science as part of the intellectual culture of an epoch, as a specific "subculture" of people creating and utilizing it, then instead of asking whether or not its development

[27] A. Koyré, *Études Galliléennes*, Paris, 1939; *From Closed World to Infinite Universe*, Baltimore, 1957; *Études d'Histoire de la pensée philosophique*, Paris, 1961, esp. "De l'influence des conceptions philosophiques sur l'évolution des théories scientifiques" pp. 231–247.

[28] T. S. Kuhn, *The Copernican Revolution*, Harvard University Press, Cambridge, Mass., 1957.

[29] F. Jacob, *La logique du vivant*, Gallimard, Paris, 1970.

[30] T. S. Kuhn, *The Structure...*, p. 3.

[31] *Ibid.*

was in accordance with some rational model common to all epochs — a model which presupposes its permanent and unidirectional (from the viewpoint of the values it intends to achieve) development and instead of selecting from the history of science some facts, and interpreting them so as to adjust them to the rational model, while relegating to "external" history those which do not fit in as "irrelevant for the understanding of science", we attempt to find out what were the criteria of being scientific at the given time, and why these criteria themselves were evolving together with the development of knowledge.

For, it is erroneous to think — *and this if of paramount importance* — that the development of science means merely the changes in the content of the accepted theories. Were this the case, the search of a logical framework upon which all those changes can be strung like beads would be justified. The development of science means in an equal degree the changes in the methods, aims and methodological criteria that are employed by scientists. With respect to science, these are not anything external, something given once and for all by a supra-historical logic of scientific discovery. I should rather say they are part of science itself and, together with it, they undergo evolution. To use a somewhat risky comparison but adequate for our purposes, with a cybernetic system. I should say that science is a system which in the course of its evolution not only changes and reorganizes its memory, but also, at least partly, its programme. The changes in this programme are conditioned, among others, by the circumstances occurring at the "inputs" and "outputs" of the system, that is, by the social and historical conditions in which scientific knowledge is pursued and utilized. The study of these conditions contributes essential information to the understanding of science and its development. The changes are moreover conditioned by the transformations occurring in the "memory" of the system, which will be touched upon latter.

If this idea is right, then the methodological criteria that we want to employ in the reconstruction of the logic of development of science must be taken out from its history itself rather than from any supra-historical normative models of rational research procedure. This also means that the context of discovery and the context of justification — at least on this level of analysis — cannot be absolutely separated from one another. This is what I meant by saying at the outset that if we restrict ourselves to studying one aspect of science only, it may encumber the understanding not only of science in general but even of the selected aspect itself.

If we treat science as a specific part of the intelectual culture of an epoch, a division of its history into „internal", determined by a supra-historical logic of discovery, and „external" history determined by adventitious, subsidary historical and sociological factors becames immediately meaningless. The "essence" of the development of science in an epoch consists not only in the then reigning logic of development but also in the involvement in those factors and in situations that, from the viewpoint of logic, may be subsidiary. What from that point of view appears to be "pathological", „unnatural", "irrational", is, if seen from this angle, conceived of as being "natural". In a sense, metaphysics, myths, errors, prejudices are equally immanent parts of science as the facts that we endeavour to integrate into a rational reconstruction. To use somewhat provocative terms, the history of science comprises also the natural history of nonsense.

From this point of view, a logical model of development of knowledge not only cannot be a description of actual history, after which, as has been said, it does not aspire at all, but it is not even a legitimate system of reference for a critical, normative appraisal of all research procedures. It does not, for it is but one of the factors of development of scientific thought whereas the remaining ones are omitted by it as being external and irrelevant for the understanding of science. The view that any logic of development of scientific thought is capable of furnishing a picture of its actual history is false. But the idea that it may constitute a normative model of research procedure it *utopian*. I say utopian, for this procedure is determined and must necessarily be determined by extra-logical factors, the influence of the latter cannot be escaped even by the best, the most liberal criteria of rationality, of demarcation, of meaningfulness, nor the definitions furnished by the methodology. Therefore it is utopian to think that the methodological directives provided by a philosophy of science thus conceived may lay down the solutions of any research problems encountered by scientists. One is tempted to say that with criteria of rationality thus constructed the detection of an "irrational" element in the history of science is inescapable.

Hence derives Kuhn's thesis that the understanding of science requires that we go beyond the logic of development and beyond the methodology (these as parts of science are in need of explanation themselves) and take recourse to the categories of the psychology and sociology of scientific communities.

To paraphrase Quine's [32] comparison, science as a whole resembles a field of forces the boundary conditions of which are not only experience but also the conditions of its pursuance. Changes at the boundaries of the field would cause corresponding adjustments in its inside. A change in the appraisal of some propositions results in a change in the appraisal

[32] Cf. W. V. Q., *Quine,* in: *From the Logical Point of View. Two Dogmas of Empiricism.*

of others; the methodological criteria of this appraisal being simply further propositions of the system, certain other elements of the same field.

IV. AHISTORISM AND RELATIVISM IN THE STUDY OF SCIENCE

However, the conception of science and of its development presented in outline so far is not free from its own intrinsic difficulties. Ones of the most important difficulties seems to be the problem of its relative character. For, if the conception of a continuous, merely accumulative development of knowledge is refuted and if scentific revolutions are treated as radical turning-points not only in the growth of content of knowledge but also in the conceptual framework of science, *i. e.* of the research programmes laying down the questions and the criteria of accepting their solutions, then the problem arises as to what relationship does exist between the pre-revolutionary and post-revolutionary framework. Are they in any respect comparable, commensurable? Is a reconciliation between the representatives of the old and the new "point of view" (paradigms — to use Kuhn's term) possible or is the victory of a new one simply a result of biological ageing of the unrelenting adherents of the old theory and the "conversion" of the young ones to the new theory?

In his criticism of Kuhn's view Lakatos writes: "My concern is rather that Kuhn, having recognized the failure both of justificationism and falsificationism in providing rational accounts of scientific growth, seems now to fall on irrationalism.

For Popper scientific change is rational or at least rationally reconstructible and falls in the realm of the *logic of discovery*. For Kuhn scientific change — from one 'paradigm' to another — is a mystical conversion which falls totally within the realm of the (social) psychology of discovery. Scientific change is a kind of religious change.

The clash between Popper and Kuhn is not about a mere technical point in epistemology. It concerns our central intellectual values, and has implications not only for theoretical physics but also for the underdeveloped social sciences and even for moral and political philosophy. It even in science there is no other way of judging a theory but by assessing the number, faith and vocal energy of its supporters, then this must be even more so in the social sciences: truth lies in power. Thus Kuhn's position would vindicate, no doubt, unintentionally, the basic political *credo* of contemporary religious maniacs (student revolutionaries)." [33]

I myself am less eager to draw that type of "political conclusions"

[33] I. Lakatos, *Falsification...*, p. 93.

from anyone's views on the development of science, but it is not what I am now concerned with. Lakatos "sharpens" the view of his adversary, which considerably facilitates the polemic against it. Kuhn speaks explicitly about the partial incommensurability of the "viewpoints" and emphasizes that the periods of crisis are in science periods of heated discussions, both theoretical and philosophical; thus it is unfair to impute him the thesis about cutting down all rational communication between scientists in time of crisis. His thesis on the partial incommensurability of paradigms refers, if I understand well, not to the situation during the crisis when no new paradigm exists yet, but after the crisis. But the mere fact of "sharpening" the view of Kuhn enables us to get a better view at the problem in question.

First, two questions have to be distinguished. Suppose for a while that Lakatos and Popper are right, that is, that revolutions, passages from one paradigm to another, can be fully explained in terms of methodology (*e. g.*, that scientists choose the theory that is of higher explicative power) and therefore any recourse to categories of psychology or sociology is dispensable. Then, Kuhn would be wrong, but why should his view be labelled as "irrational"? Why should the idea that the choice between two concurrent competitive paradigms is codetermined by psychological and sociological factors be equivalent to professing irrationalism? Can the role of these factors not be explained in rational terms? At any rate Kuhn does not say anything like that, and Lakatos does not attempt to prove it; incidentally, it cannot be proved at all for that would mean no less than the disqualification of psychology and sociology as sciences. Kuhn suggests to take up rational scientific studies of that problem and to take into account the obtained results in considerations on the development of science. There is no irrationalism in that; Lakatos' charge, at any rate in such a formulation, does not contain anything besides the scheme of reasoning presented in section 10 before. Irrationalism is what does not agree with his, Lakatos', conception of rational development of science. [34]

But there is another face to that coin. I pointed it out in the Postscript to the Polish edition of *The Structure of Scientific Revolutions,* and Popper gave it an excellent wording: "I do amit that at any moment we are prisoners caught in the framework of our theories; our expectations; our past experiencies; our language. But we are prisoners in a Pickwickian sense: If we try, we can break out our framework at any time. Admittedly, we shall find ourselves again in a framework, but it will be a better and roomier one; and we can at any moment break out of it again.

The central point is that a critical discussion and a comparison of the

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[34] *Ibid.*, p. 189–191.

various frameworks is always possible. It is just a dogma — a dangerous dogma — that the different frameworks are like mutually untranslatable languages [...] The Myth of Framework is, in our time, the central bulwark of irrationalism." [35]

It is easy to guess that such a "supra-paradigmatic" frame of reference, such a supreme umpire to decide in disputes between "prisoners" of different points of view is to be — at least in science — the category of objective truth as formulated by Tarski. [36] It is symptomatic that this notion does not appear in Kuhn's book at all, which he duly emphasizes in the concluding chapter. [37]

Is it then indeed so that in studying science we face the alternative: either ahistoricism or extreme relativism? Either to use the category of truth and progress in the development of science and to create the continuous, merely accumulative model of its development and to recognize as "external" or "irrelevant" from the point of view of its development what does not fit this model, or else to study science "historically" but to pay for it the hard price of extreme relativism, of abandoning the category of truth and progress.

The answer to this crucial question, which has been at the core of many ages of philosophical inquiry, is not easy, and it would be naive to expect the present writer to produce it now.

While restricting myself to a narrow fragment of this problem, *i. e.* to the questions of development of science considered here, and being unable to venture upon a discussion of the concept of truth, I wish however put down same sketchy remarks.

To my eye, the principal simplification of Kuhn's book — the main ideas of which are, as the Reader may have observed, close to mine — is the conception that the development of knowledge in a given discipline in a definite epoch can be described as a result of the reign of a *single* paradigmatic point of view; then the revolution must be conceived of as a radical disruption of the continuity of the growth of knowledge. On the other hand, his adversaries oppose him putting forward a conception of development of science as a process that is also determined by a *single* factor — by the disinterested striving for truth conceived of always identically. This striving delimits the diverse concurrent research programmes and is the supreme umpire in their competition. It accounts for the maintenance of the supra-historical continuity in the development of science, and due to this striving we can speak of progress consisting in a continuous accretion of knowledge, in the attainment of ever closer approximations to truth.

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[35] K. R. Popper, *Normal Science and Its Dangers,* in: *Criticism ...,* p. 56.
[36] Popper says that explicitly, cf. *ibid.* .
[37] *Cf.* T. S. Kuhn, *The Structure...,* ch. 13: "Progress through Revolutions".

Now I think that none of these conceptions can be kept in such a form.

The paradigms that affect the scientist's work and determine his research endavours are many and diverse.[38] They may be some general convictions referring to the way of practising science as such or to practising a given discipline or specialization in a given period; they may derive from the previous achievements of science, or from philosophical views, or from practical needs imposing a definite direction of research. Briefly, the scientist in his work is not guided by the unique "point of view" of his narrow specialization but by a number of paradigms; some of them are more specialized, others more general. And it is by no means certain that they are always consistent with one another, if only such a consistence ever occurs in a science as a whole. Thus, if paradigms determine the tradition of normal scientific research in a given discipline, then the tradition is never so homogeneous as it is presented in Kuhn's book.

Suppose that some theses pertaining to the paradigm of modern physics could be articulated; they should then be accepted by all physicists. But it is unquestionable that if we should analyse not physics as a whole but a branch of it, say quantum mechanics, we might notice that people working in this special field cherish moreover some other paradigmatic assumptions extending beyond those accepted by all physicists. The narrower the respective specialization, the less general the paradigm. Thus it must turn out that a discovery which, from one (narrower) point of view is already a disruption of the tradition, a divergence of a paradigm a revolution, from another (broader) perspective will fit in the tradition, will be its continuation (one of those possible), and will be treated as another step toward the development of normal science. For instance, from one point of view the theory of relativity is a disruption of the tradition of Newton's classical mechanics, but from another it may be treated as, in a sense, a continuation of its deterministic programme. It is well known that the adherents of the Copenhagen interpretation of quantum mechanics thought Einstein to be a dogmatist in that he refused to abandon some of the paradigmatic assumptions of classical physics (the requirement of determinism and of objective description of the physical world). To put it briefly, "if the concept of paradigm is but loosely defined, then it appears that is does not include

[38] M. Masterman in his study *The Nature of Paradigm,* in: *Criticism...,* pp. 59–91 has shown that Kuhn uses the term "paradigm" in many meanings — she found as many as 21 of them in his book. A number of differences in meaning indicated by her are undoubtedly verbal in character, but nevertheless she is right in distinguishing between metaphysical paradigms, sociological paradigms and paradigms functioning as artefacts of concrete scientific achievements (cf. p. 65). T. S. Kuhn has agreed with these critical remarks to a wide extent (cf. the "Postscript" to the second English edition of *The Structure...,* pp. 181–191, and *Reflections on My Critics,* pp. 231–279).

all the convictions that determine the work of the overhelming majority of scientists in a given period; if it is rendered more specific, then it appears not to delimit the tradition of normal science, for this is composed of a whole family of paradigms." [39]

What must be stressed is that the problem consists not only in what Lakatos rightly emphasizes but what fails to solve our problem, namely that within one specialization *not all scientists* are guided by the same research paradigm; what is primarily essential is that *one scientist* is in his research work a slave of several paradigms of different levels of generality, which may superimpose upon one another, my conflict *etc.*

Now, if the foregoing critical remarks on Kuhn's conception are correct then important consequences result which may defend it against at least some of the accusations of relativism or irrationalism (as Popper worded them) and moreover permit to retain what is most valuable in it.

Since the development of science or of a discipline and the scientist's cognitive activity are not determined by one single paradigm, which in time of revolution falls into crisis, then a revolution is not an abrupt discontinuation of the development. To use once more Quine's illustrative comparison, a disturbance in one area of the field does indeed spread, or radiate to the continuous areas, but this is not equivalent to disturbing the whole field. If the field is viewed as a whole, there is always some revolution going on somewhere in it, and in this sense science is in a permanent crisis. But these are always more or less local crises which do not turn everything upside down.

The possibility of rational agreement — at least in science-between the adherents to the old and the new "points of view" in a discipline that suffers from a crisis is not completely lost. Some paradigms that are common to them survive, and there is still a more comprehensive "consensus" left than what can be threatened by the revolution. Even if it is true (and I think it is) that the old "point of view" and the new one established after the crisis are partially incommensurable, it does not follow that the transition from one to the other could have been effected in an irrational way, by "conversion".

I should say that the transition could be effected along the "byways", that is accordingly with other paradigms that are commonly shared by all those working in the discipline concerned and that are unaffected by the crisis at the moment; they are shared as paradigms of a more comprehensive discipline within which the agreement is possible as they do not include the more specific, then disputable "points of view" or else as common philosophical convictions concerning the structure of the world and the "nature" of knowledge, its functions, tasks *etc.* In result of this the field goes through a reorganization.

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[39] S. Amsterdamski, "Postscript" to the Polish edition of Kuhn's *The Structure...*, p. 201.

If after some time we look back, especially into a remote past, the concurrent paradigms seem to us incommensurable. It is beyond our comprehension how, without counterfeiting the actual history, to describe the transition that did indeed occur, especially if in the meantime those "byways" were destroyed in result of later crises. Therefore, to understand what change did occur, and how it could happen that people renowned as the leading minds of their epochs could first cherish views that to us appear to be "myths", and later abandoned them for other, no less "mythical" convictions, we have to reconstruct not any supra-historical logic of development but rather the actual state of the field at the moment of crisis. We have to inquire into what "rational" ways to agreement — legitimate within that field — were open to them, and what use they made of the ways to overcome the crisis and becoming in turn "prisoners" of a new paradigm.

In my opinion, the problem consists essentially in that the field includes no such area that could never be affected by a crisis, and that *the rules of practising science, methodological criteria and the principles of rational research procedure,* which are also elements of this field, may be together with it subject of change.

This means that, like Kuhn, I refute the absolutist view that there exist some permanent principle, supra-paradigmatic and external to changing field, which determines the logic of development and which may function as the umpire in deciding about the rightness of one or another solution of a research problem. The theory of truth is, as evidenced by the history of the sciences and philosophy, also an element of the field, and it is by no means immune against crises which are, among others, evoked by changes in the substance of knowledge. (Recall the discussions about the necessity to change the logic under the impact of the quantum theory, or those about multi-valued logics and probabilistic inferences, recall the disputes over operationism, instrumentalism and realism and over the principles of accepting or refuting scientific statements put forward by these theories; examples could be multiplied.) Incidentally, it may be interesting to consider what place is in the field occupied by the principles of methodology, that is, to consider their immediate and indirect connections with the "boundary conditions of the field".

Furthermore, this means that I cherish a relativist attitude. But it is relativistic only in the sense that the criteria of evaluation are variable and not that no such criteria exist in any moment in sience. Thus I think that it is not true that in the case of crisis the transition from one "point of view" to another is effected "irrationally", by "conversion". It is effected "rationally", that is in accordance with the criteria of rationality reigning at the moment. In each particular crisis, in any competition between paradigms or research programmes there is a supreme court of appeal; some umpire that decides between them. In the course of the crisis of their discipline or specialization scientists do not break up their discussions; on the contrary, usually they intensify it; nor is there anything to suggest that they are unable to communicate among themselves. The incommensurability of the old and the new points of view is an effect of the overcoming of the crisis rather than its characteristic symptom. But the supreme court of appeal is itself an element of the field and not something external to it; consequently it is itself revocable, temporary, and may also be inflicted with a crisis of distrust. It is a court which judges until it is sentenced itself.

Accordingly, I think that it is not true that revolution means a complete break in continuity. But that continuity is not determined by a single pattern external to the actual history, by a single logical framework onto which we should expect to be able "to string on" all the facts. This continuity is determined by a number of concurrent factors which successively, but obviously not all at once, supersede one another. After some time, the state of the field may prove to be very different from the former one, it may be very dissimilar, "incommensurable". As Kuhn says, we have come to a new world in which we put different questions and apply different criteria of evaluation and acceptance of propositions. Any direct derivation of the present state from the preceding one in virtue of the current criteria of rationality in the field cannot but counterfeit the actual history, not only as regards the facts but also the pattern of the effected development. I should say that the continuity is analogous to that of Theseus' vessel which had its particular decayed parts successively replaced be new ones. It maintained that kind of continuity which Reichenbach pertinently — though with reference to other problems — called *genidentity.*

Thus, it follows that the alternative: either to appraise the development of knowledge from the point of view of some absolute knowledge and thus to run into a self-evident antinomy (for then we asses the progress made in virtue of approaching a point that has not yet been attained and thus is by then unknown to us), or to abandon any intention of appraising the development of human knowledge in terms of progress — is inapt. Such an appraisal is possible by comparing two diachronic states of the field to one another regarding a selected feature; there is no certainty, however, that if the same criterion of comparison is applied many times to compare a number of successive situations in pairs (first *B* to *A,* next *C* to *B, D* to *C, etc.*) a picture of unidirectional development will be obtained. The concept of truth is certainly the main, but not the only criterion of progress thus appraised, but we ought to remember that this concept itself is not immune to changes. Neither the knowledge of laws governing one period of history is a sufficient foundation for their extrapolation into the past (or the future) unless there is evidence at hand to suggest that the laws are subject to no changes, nor does the knowledge of two state of a system divided by a definite span of time authorize us to hold that the earlier state was developing so as to pass into the later state in the simplest possible way. Historism is not equivalent to teleologism, and human anatomy cannot always be taken as a key to the anatomy of the monkey. Unless we keep in mind this circumstance, the study of the past may easily slip into creating genealogies for our needs.

To conclude this part of our considerations I would say that after some modifications as specified above, the charge that the relativist approach to the study of the history of science as represented by Kuhn must lead to irrationalism in the sense in which Popper puts forward his arguments, can be refuted.

As the Reader may have gathered from this next, the point of departure of a human scientist's reflection on science is in the analysis of science as a human activity oriented, like any other human activity, to definite values. It is the values that *codetermine* the "boundary conditions of the field" or the state of the "inputs" and "outputs" of the system known as science. Viewed from this angle, logic and methodology are means of optimalizing the attainment of the values. In other words, methodology, which is normative in character, must be backed by some well-defined values, for only values can constitute a justified rationalization of the norms of procedure. The study and the identification of the value are indispensable conditions for the understanding of science. These problems, which I have outlined elsewhere, [40] demand particular attention from sociologists; it is among others at this point that the sociology of science becomes a necessary element of understanding science and its methodology as well as their development. "Scientific knowledge, like language, is intrinsically the common property of a group or else nothing at all. To understand it we shall need to know the special characteristics of the groups that create and use it" [41]

It is obviously unimportant how we should call the discipline that could take as its task a study of science thus conceived — be it the theory of science, the philosophy of science or still otherwise. What, however, seemed to me important to show was that the traditional boundaries between different dsciplines that today deal with a theoretical study of science handicap the understanding of it, for they put up artificial barriers between its mutually interrelated constituents. The value of the results obtained by specialized studies on science notwithstanding, they seem insufficient both in view of the problems of modern science and of needs of a human scientist's reflection on science.

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[40] S. Amsterdamski, "Modern Science and Values. An Outline of Problems" (in Polish), *Zagadnienia Naukoznawstwa*, 1971, No. 1.

[41] T. S. Kuhn, "Postscript", in his work *The Structure...*, 2nd ed., Chicago, 1970, p. 210.