Marek Sikora (Poland)

SCIENTIFIC CHANGE IN THE LIGHT OF MODERN PHILOSOPHY OF SCIENCE: TWO APPROACHES

I

The Question about tradition in science, about when it is continued and when interrupted, will be taken advantage of to define the two most typical approaches to science (the term science denotes empirical sciences here) in modern philosophy. Let us focus our attention, in particular, on the question of change in science, which is a central issue in both approaches. One approach was given its most complete expression by I. Lakatos in his methodology of research projects, the other one by T. S. Kuhn in his book called *The Structure of Scientific Revolutions*. Their many differences notwithstanding, both concepts are descended from the same source, for they emerged as results of the debate on K. R. Popper's falsificationism.

Popper was the first to point out – in the mid-1930s – that the question of the growth of scientific knowledge is the principal gnoseological problem.[1] In his *Logik der Forschung*, Popper challenged the long-established radical empiricist view that unlike other human activities (myth, religion, art) science grows in a cumulative fashion, that is, sciences develop by collecting more and more perceived experiences and adding new to previous theories. Criticising logical positivists for ruling out the question of change in science as a possible philosophical query, for restricting reflections on science to synchronous studies of logical syntax, semantics or pragmatic implications, Popper called for a new approach to science.

Scientific growth, in his view, is not cumulative but cyclical in nature, and rather than adding new to previous research findings science grows by continually modifying previous knowledge as new perspectives are being adopted to look at particular problems. Because of change new ordered theories arise. When a theory fails to stand the test of practice, researchers come forward with a new one. At any given time the theory reigns which, as Popper puts it, has "braved" checking tests that confirmed rather than disproved them. As in many previous cases, the current theory may be proved false in the future.

Poppers pays particular attention to constant principles ordering successive theories. He recognises those principles as decisive in the development of science, for they decide which of the rivalling theories are eventually chosen. They make the development of science independent of scientists' idiosyncrasies and impart it a rational character to it. The supreme principle tells the scientist to confidently formulate scientific conjectures and at the same time to disprove them mercilessly by a relentless quest, for whatever errors, to disclose and eliminate them. Rational behaviour is essentially critical behaviour, implying a critical approach towards science. A researcher will also be versed in logical principles, for competing scientific theories are always comparable with one another in logical terms. Logic is the tool that is used, experience is the umpire, when a theory is to be chosen from among many.

In Popper's method, science is viewed less as reliable knowledge (*episteme*) than as historically changing knowledge. What is not subject to vicissitudes of history are the principles that order a sequence of successive theories. They make up something like a "logic" which is independent from anything else. That "logic", says Popper, should be respected by all, as it determines the future course of science.

Popper, then, believed in the rational character of the development of science, or, more precisely, he accepted the idea that definite rational rules of choice were possible to identify. But that was questioned by Kuhn. In His Book on The Structure of Scientific Revolutions, he launches the following idea supporting it with ample historical evidence: the history of science cannot be studied meaningfully in logical terms alone, while ignoring possible interventions of psychological or sociological factors. No such thing exists as one mechanism of science that can be completely described using logical terms alone and that could serve the scientist as a touchstone to choose between better or worse theories. New theories are not necessarily logical extensions of previous theories. A new theory is not commensurate with the old one.

Kuhn's incommensurability argument derives from the view that all scientific terms are burdened by theory.[2] Thus theory rejects the logical positivist distinction between theoretical and observation-based scientific terms and questions the purely empirical meaning of scientific terms – al terms are theoretical in nature. As there is no way to distinguish meanings of terms functioning in different theories in such a fashion as to demonstrate a common neutral language in those theories, then it is fair to say that the same terms have different meanings in different theories. Although scientists use the same terms in their descriptions, their terms mean different things. The meaning of a term depends on its context. No term can be meaningfully considered in isolation, for it should always be assumptions, a paradigm. When different paradigms are involved, Kuhn says, the same terms mean different things in different paradigms.[3] Accordingly, it is not meaningful to say that Einsteinian concepts are either identical or contradictory with Newtonian ones, but only that they are incommensurate with each other.

Transitions from paradigm to paradigm cannot possibly be explained, Kuhn argues, with rules of logic alone. Change in science is not rational, or rationally reconstructible, as Popper claimed, and it does not end up in the realm of a "logic of scientific discovery." Kuhn was positive that to explain change, or more properly, revolutions, in science, non-logical factors have to be taken into account, that is, psychological as well as sociological.

Kuhn's arguments were debated heatedly. Some critics tried to identify decisive methodological criteria for the choice of theories. I. Lakatos, a disciple of Popper's, came forward with one such attempt. In a study called *Falsification and Methodology of Scientific Research Programmes*, Lakatos undertook to develop and reinforce rationality standards he borrowed from his master which guaranteed the continuity of scientific development.

Methodologies of scientific research programmes are based on the assumption that the real question in science is less to evaluate one theory than a series of theories. It is not of one isolated theory but of a series of theories that we can say they are scientific or not scientific, Lakatos says. A series of theories should be viewed as a continuous entity for two reasons. First, all theories presuppose definite sets of propositions, or a hard core of propositions, as Lakatos puts it. Secondly, common methodological rules hold within a series. The rules are "derived" from a scientific hypothesis which is a set of strong heuristic principles governing the development of the series of theories. In this sense, a series of theories can be said to be a research programme.

The core of a research programme can be a broad or narrow set of laws, which is its fixed unchanging part. Lakatos believes there are rules that ban the application of the *modus tollens* to the programme core in the event of prediction disagreeing with observation. Lakatos calls the methodological rules that protect the programme core the programme's negative heuristic.

The core is surrounded by a protective belt composed of supplementary hypotheses and initial conditions. Supplementary hypotheses indicate conditions for a phenomenon described in the *explanandum* to occur. Unlike the core, the protective belt changes and evolves continually. Anomalies that can harm theories are referred to the surrounding belt. As a theory is coming under growing pressure from anomalies, the theory's protective belt T_n (the *n*th theory of the programme) is being changed to become T_{n+1}.[4] The new theory which emerges from the addition of new initial conditions and supplementary hypotheses must meet the falsification principle. Theory T is viewed as falsified on the ground of the methodology of scientific research programmes when a new theory T_1 has been put forward that has the following qualities: 1) T_1 has a surplus of empirical evidence over T, or, it speaks of new facts which are impossible to explain or even ruled out by T; 2) T_1 accounts for the previous success of T, that is, all of the non-rejected part of T is contained (within the boundaries of observation error) in the substance of T_1; and 3) part of the empirical surplus of T_1 is confirmed.[5]

A research programme is progressive, in Lakatos's view, when its new theories meet the falsification principle. If new theories fail to meet conditions implicit in the falsification principle, the programme degenerates. It ceases to be fruitful, and the propositions making up its core lose their explicatory capacity. The programme is then discarded along with its core. A new hypothesis is substituted to provide a foundation for the new programme.

What undoubtedly distinguishes the methodology of scientific research programmes from Popper's falsificationism is the belief that no theory can really become falsified, however big the evidence against it may be, before a new better theory has appeared. Falsification for Lakatos is essentially a "historical" feature, a process unfolding in time. But even in such a modification, can falsification be defended against Kuhn's reservations? Lakatos transfers the problem of change in science from a level of theory to that of research programmes, and so the question arises about an objective (i.e., non-sociopsychological) choice between rivalling research programmes.

Let us try to answer this query by comparing Lakatos's methodology with Kuhn's approach. In particular, we shall look at the question of change in science from the angle of interpretative procedures followed by both philosophers. What are their attitudes towards theory and experience and are they linked to each other? How do these philosophers account for discoveries of new facts? I am sure these are all meaningful questions which, when answered, can cast a different light on the controversial issue of change in science.

II

First a word on interpretation. Philosophers of science generally agree (even though scientists may not) that there are two dichotomous interpretative procedures governing the relationship between theory and experience:

1) an empirical interpretation of theory in which observations (propositions formulated in virtue of experience) are linked up to theoretical propositions; and

2) a theoretical interpretation of experience in which theoretical propositions are linked up to observation-based propositions.[6]

Both procedures are directed towards the same goal, seeking as they do to establish the theory vs. fact relationship. The difference is that the former of the two implies the cognitive primacy of experience before theory, while the latter implies the opposite order.

Both Lakatos and Kuhn are doubtful about the distinction between purely empirical and purely theoretical propositions. As neither of them believe that propositions can ever be really free of theoretical components, they reject any interpretation implying a duality of theoretical and empirical propositions.

In his methodology, Lakatos follows Popper in taking for granted that all results of observation are trapped in some theoretical presumptions and so nothing like "pure fact" really exists. This is true of direct experience (i.e., observations), which is not free of certain preconceptions about the functioning of the subject's cognitive system, and it is true of indirect experience (experiments) where the establishment of fact may be affected by factors such as knowledge of the subject, efficiency of method and techniques used in experiments, or the appearance of a yet unknown phenomenon. All results of experience (direct or indirect) always call for interpretation, and that in turn calls for a theory.[7]

If each observation is encumbered with theoretical presumptions, what is the empirical foundation of science? To indicate it, Lakatos takes advantage – not uncritically though – of Popper's notion of base propositions, which of course are conventionally accepted individual existential propositions making it possible to disprove hypotheses purporting to usher in a scientific law. Such a hypothesis can be formulated as a negation of an existential proposition. Base propositions are accepted or rejected as the result of decisions or agreement and in this sense they are conventions.[8] Popper apparently draws quite an arbitrary line between theory and observation. Observations, and even more so observation-based propositions and propositions about results of experiments, are always interpretations of observed facts, interpretations in the light of a theory.[9]

As critics charged Popper with inconsistency, for not all scientific propositions were falsifiable in his approach, Lakatos modified his master's suggestions. The real problem encountered in the methodology of scientific research programmes, is not so much to evaluate a theory as to evaluate a series of theories. Falsification of a series does not lead up to its definitive "disproof" but merely to its "rejection," in the sense of making a decision to stop studying it. Lakatos narrows down Popper's conventionalism but does not eliminate it from science altogether. Like Popper, Lakatos thinks there is no way to escape decisions about pronouncing some propositions as observation-based and other ones as theoretical.[10] Whether it is fact or theory, what we have to do within a test situation will be decided by our own methodological choice, Lakatos argues.

He cautions, however, against proclaiming conventionalism in this sense as the opposite of objectivity. Objectivity, in his view, is preserved in science. Objectivity is warranted no longer by a confrontation of theory with bäse propositions but by a confrontation of two theories within a programme and the rivalry of two programmes on of which accounts for the success of its competitor (rival) and supersedes it by theoretically anticipating new facts (it's "heuristic power"). This brings us to the crucial problem of how can facts be established? Or, is the establishment of facts decided by scientific or non-scientific motives?

It is difficult to demonstrate, Lakatos concedes, that one of the rivalling research programmes accounts not only for facts that the competitor does, that is, for the "old facts," but for objectively " new facts" as well. Now and then it takes scientists a very long time to notice facts implicit in a programme. Scientists cannot reach for "cross-examination experiments," or experiments that are capable of disproving a research programme right away.[11] Scientists do not always take a correct view of heuristic situations. The kinetic theory is a case in point. It seemed to lag way behind the phenomenalist theory, until the Einstein–Smoluchowski interpretation of Brownian movement made scientists realise that what was long seen as a reinterpretation of old facts (about heat etc.) had turned out to be new facts (in nuclear physics). Those discoveries led to a change of problems. "In science, we do not learn about truth (or likelihood) or falseness (or improbability) of a «theory» but about a relative progress or degeneration of a research programme."[12] A reinterpreted known fact, connected with a progressive change of problems, is a new objective fact. Empirical progress, in Lakatos's view, makes it possible to choose between rivalling programmes: we always choose the programme that has a surplus of empirical substance.

Now let us go back to Lakatos's view, which he took from Popper, that discoveries of new facts are determined by immanent rules of a third world that are independent of the acting subject. I do not share Lakatos's confidence that a well-defined unchallengeable set of necessary and certain methodological rules of a third world can be identified. Subjectivity, I am sure, cannot be avoided in our cognition. Take, for example, the practical implications of a research project. With those in mind, a scientist may pick – intentionally or not – one of several valid interpretations even though nothing shows that any of the other rivalling interpretation would be a less valid choice. The idea that initial assumptions with which we take to formulate a new problem (or a new research programme) are given in a Pickwickian sense and that "if we want we can disentangle from their net any time,"[13] is debatable indeed.

Let us briefly go back to the question of pronouncing new facts. What is Lakatos's definition of anomalies? What is their status in his view? Anomaly, he writes in *Falsification and Methodology of Scientific Research*

Programmes, is a problem appearing in a given research programme as a challenge to it. The problem of anomaly can be solved in one of three ways:

1) inside the initial research programme *P* (the anomaly then turns into a case confirming the programme *P*);

2) inside a programme independent from *P* (the anomaly is independent of *P*); or

3) on the ground of a programme rivalling programme P (the anomaly turns into a counter-example to *P*).

Anomalies, Lakatos says, are like the puzzles Kuhn wrote about in his *Structure of Scientific Revolution*. Anomalies (puzzles) are accounted for, within the methodology of research programmes, along one of the above three lines, yet they can always be referred to the initial programme *P*. Science's "internal history," which reflects a rational mechanism of scientific development, renders all research programmes meeting the requirement of correspondence comparable with one another.[14]

Removing an anomaly reshuffles the programme. Modification rules are contained in the programme's positive heuristics, but generally as a loose set of suggestions rather than a strictly-defined canon for scientists. It is never clear whether such a set of suggestions reflects a programme's "objective" heuristic or only one of several conceivable heuristics scientists can come forward with.[15] It is not clear, either, which of the modifications put forward to deal with the anomaly is right and which is wrong. That becomes clear only *ex post* in the light of further studies.

Rationality, Lakatos argues, works more slowly than most methodologists would think. Even so, "scientific change is rational or at least rationally reconstructible."[16] Rational rules of scientific procedure follow from the falsification principle. That, in turn, causes changes both inside a programme and in substituting one programme for another. The only thing to do is to reconstruct the process of scientific development in order to show that greater use in theoretical anticipation of facts (or, a greater heuristic capacity) is what ultimately decides the choice of both a better theory and a better programme. Lakatos's methodology, accordingly, does allow for objective reasons and rules which are conducive to change in science. Those reasons and rules apply to a mechanism operating in scientific development at large, irrespective of specific historical conditions.

III

Kuhn presented a very different approach to the question of scientific change. In his *Structure of Scientific Revolutions* and subsequent studies, Kuhn turned down the idea that scientific development was a continuous process. Changes, or, more properly, revolutions, in science do not come about as a result of constant well-defined methodological rules. Explanation of those revolutions "cannot be eventually anything but psychological or sociological in character. This means it has to be a description of a system of values, ideologies, as well as an analysis of institutions through which the system is transferred and imposed."[17] This is the main idea of Kuhn's. Rather than considering its many implications here, let us concentrate on those only which are connected with the question of change in science.

In Kuhn's view, we can speak of continuity in science only when science has reached a mature stage, when it has become "normal science." A "normal" science is one in which some of its results (theories, laws along with their applications, research methods) have been recognised as a pattern or paradigm of research work by "scientific communities." Adoption of one common paradigm by a majority of the scientific community restricts the studied problems. Research work is not designed to check a paradigm, for it boils down to bringing the problem down to a lower generality level. Scientists then focus their attention on those problems only which they recognise as particularly important in the light of the adopted paradigm. That is what makes their work efficient and wins the community's support. Problems addressed within normal science look like puzzles. Like puzzles, the problems have guaranteed solutions which are legitimate under the paradigm.

Normal science is definitely cumulative in character, "being extraordinarily efficient in its endeavour to expand the scope and increasing the precision of scientific knowledge."[18] It is not geared to the discovery of new things. No effort is made to find solutions to new phenomena inside such science, but only to bring them in agreement with the image determined by the paradigm. Yet new facts are being discovered, new theories are being put forward. What is the explanation behind this?

New discoveries, says the author of the *Structure of Scientific Revolution*, are not isolated events but prolonged episodes with repeated structures.[19] They have their point of departure in anomalies, which appear when forecasts based on the binding paradigm fail. Emerging anomalies increasingly attract attention. Scientists make repeated efforts to overcome them. In that manner, the paradigm is modified, and many versions of it appear. A strong and lasting awareness of anomalies is a sign of a science being in crisis. The crisis begins to undercut scientists' confidence in the paradigm, while its rules for solving problems are being eased. As a consequence, scientists begin to take notice of rivalling theories which let the anomalies appear in a different light.

Although confidence in the paradigm is waning, tradition is a powerful obstacle to the spread of rivalling theories. In science, Kuhn says, canons are very difficult to break up. Every new thing hits a barrier of entrenched beliefs. At first, anomalies are not treated seriously. They are often ignored, and only things anticipated in keeping with the paradigm are acknowledged.[20] Only when anomalies are around for a long time do scientists acknowledge them and a crisis is provoked in science. The crisis is an initial condition to identify and put forward new theories that are to supersede the current paradigm.

The anomalies alone are not enough for the paradigm to be rejected: some facts are always around that are at odds with the paradigm, yet they are not viewed as counter-evidence but only as normal problems which can be solved within the paradigm. What is indispensable to the rejection of a paradigm is a rival, or a new more satisfactory paradigm capable of accounting for the value not merely on the grounds of a confrontation of theory with experience. Which paradigms are to be accepted and which rejected, results therefore not only from comparisons with nature, but also with one another.

Choosing from among rivalling theories breeds problems that cannot be solved inside the scope of logical rules. The new paradigm supplies new notional categories, which for their part turn, what under the previous paradigm used to be an anomaly, into a predictable thing – a scientific fact. Kuhn uses the term revolution to describe the passage from the old to a new paradigm. After such a revolution, although the world has not changed, scientists live in a different world.[21] This way, continuity in science is broken up.

Comparisons of theories from before and after the revolution do not boil down to reinterpretations of individual established facts. Nothing like a neutral fact and its interpretation exist. Kuhn makes a strong point of the impossibility of such an interpretation, as no unchanging foundations seem to exist for that. Each interpretation, he says, is inextricably bound up with a paradigm. "Interpretation (...) can only cause a more particular reformulation of the paradigm, but not its correction."[22] It follows that scientists subscribing to two different paradigms, will attribute different meanings to theoretical terms occurring in laws even though their wording or the terms used in either case are the same.

So, Lakatos and Kuhn agree at least that the meaning of a scientific term depends on its theoretical context. Yet Lakatos questions Kuhn's view that the meaning of any scientific term found in a theory changes radically as soon as the theory has changed.

Their respective ways of presenting the notion of anomaly demonstrate the main difference between Lakatos's and Kuhn's positions. To Lakatos, an anomaly is something like a puzzle. Its explanation, no matter whether provided within the research programme *P* or in its competitor, or perhaps in some other research programme which is neutral towards P, does not break up the continuity of science. There is still that "internal history" which accounts for the rational (or logical) aspect of growth of scientific knowledge and so furnishes a foundation for comparing all scientific terms with one another.

Kuhn refutes that comparison of anomalies to puzzles. Puzzles are problems that normal science deals with. They have guaranteed solutions which are legitimate under a given paradigm. Anomalies appear when the paradigm's anticipated governing rules fail. Anomalies disturb tradition-bound scientific practices, prompting scientists to look for and adopt new unconventional studies. Anomalies spawn discoveries of new facts and formulations of new theories which are incommensurate with previous ones.

To conclude, let us go back to the query asked at the outset about tradition in science, specifically about when it is continued and when broken up. Realising the broad theoretical implications of this question, let us only look at the problem of scientific change in Lakatos's and Kuhn's approaches. I think that two lines of tradition can be identified inside Lakatos's methodology of research programmes: an internal and a supra-systemic. The internal tradition is confined within a single research programme. It is that tradition that causes the programme to grow, or to produce its successive versions. The other line of tradition, which I call the supra-systemic tradition, goes beyond the confines of a programme to encompass the whole area of scientific research. It marks a quantum leap in the development of science, indicating a substitution of one system (or programme) for another, richer one. Lakatos bases both research traditions on "objective standards," on what are common to all science sets of methodological standards, regardless of their respective historical references.

Kuhn takes a slightly different view of tradition. To be accurate, he does envisage an internal tradition much like Lakatos's which operates inside a given paradigm. But as Kuhn recognises no criteria above individual paradigms, there is no question about any supra-systemic tradition in his approach. Transition from one paradigm to another is a "conversion" rather than an objective choice. Individual scientists, Kuhn believes, are unable to comprehend at the same time, theories divided by a scientific revolution or to confront them with the real work or with one another.

[1] K. R. Popper, *Logic of Scientific Discovery*, New York, 1961, p. 15.

[2] The same idea was put forward independently and at the same time by P. K. Feyerabend.

[3] T. S. Kuhn, *Struktura rewolucji naukowych* [Polish translation], Warsaw, 1968, p. 118. T. S. Kuhn, *The Structure of Scientific Revolutions*,2nd edn. enlarged, The University of Chicago Press, Chicago, 1970, p. 101f, "Apparently Newtonian dynamics has been derived from Einsteinian, subject to a few limiting conditions. Yet the derivation is spurious. (...) But the physical referents of these Einsteinian concepts are by no means identical with those of Newtonian concepts that bear the same name."

[4] Lakatos unfortunately does not explain exactly how the programme's protective belt changes. He only talks of the programme's positive heuristics ("research policy") which reportedly governs the course of the programme.

[5] I. Lakatos, *Falsification and Methodology of Scientific Research Programmes*, in: I. Lakatos, A. Musgrave (eds.), *Criticism and the Growth of Knowledge*, Cambridge, 1970, p. 117.

[6] Cf. J. Such, Different interpretative procedures in science [in Polish] *Studia Methodologiczne* No. 6, 1969.

[7] M. Przełęcki, *A Model-Theoretical Approach to the Problem of Interpretation of Empirical Languages*, un: Przełęcki M., Wójcicki R. (eds.), *Twenty-Five Years of Logic Methodology in Poland* Warsaw, 1977.

[8] Cf. K. R. Popper, *Logic...*, op.cit., p. 106.

[9] Cf. ibid., p. 107 n. 2.

[10] I. Lakatos, op.cit., p. 127.

[11] Ibid., p. 173.

[12] I. Lakatos, *Mathematics, Science and Epistemology*, Philosophical Papers, Cambridge 1978, p. 213.

[13] K. R. Popper, *Normal Science and Its Dangers*, in: *Criticism and the Growth...*, op.cit., p. 56.

[14] Lakatos distinguished between an "internal" and an "external" history of science. Internal history presents the rational aspect of scientific development (the one which is in keeping with rules of logic). The other history, as a product of non-logical deviations, is of little avail for the understanding of science.

[15] Scientists alone, and not any guaranteed objectivity, decides the future of a research programme. Historical relativism, which Lakatos staunchly opposed, does seem to have a foundation.

[16] I. Lakatos, *Falsification and Methodology...*, op.cit., p. 93.

[17] T. S. Kuhn, *Dwa bieguny* [Two Poles]. Warsaw, 1985, p. 402. T. S. Kuhn, *Logic of Discovery or Psychology of Research*, in: *Criticism and the Growth...*, op.cit., p. 20.

[18] T. S. Kuhn, *Struktura...*, op.cit., p. 68. T. S. Kuhn, *Logic of Discovery or Psychology of Research*, op.cit., p. 20.

[19] Ibid., p. 69. Ibid., p. 52.

[20] Cf. ibid., p. 79. Cf. ibid., p. 62.

[21] Cf. ibid., p. 137. Cf. ibid., p. 122.

[22] Ibid., p. 122.