THE HISTORICAL TURN IN THE PHILOSOPHY OF SCIENCE

1 Developments in the History of Science

The history of science has a long history. Aristotle's scientific works are prefaced by historical account of those sciences, and this model persisted through medieval times until and including the rise of modern science in the era of the scientific revolution. Joseph Priestley, for example, entitled two of his books of pioneering research *The History and Present State of Electricity* and *The History and Present State of Discoveries Relating to Vision, Light, and Colours*. For many such early modern authors the history of science serves as a propaedeutic. William Whewell's *A History of the Inductive Sciences* (1857) is regarded as the first genuinely modern work of the history of science. Even so, Whewell's scholarship has an extra-historical purpose, which was to furnish the materials against which a satisfactory philosophy of science could be constructed. While Whewell rejected a Leibnizian logic of discovery, he did nonetheless believe that general principles of scientific inference could be uncovered by careful consideration of the history of scientific research. Whewell's approach was followed by several early positivists, notably, Mach, Ostwald, and Duhem.

Nonetheless, as positivism developed philosophically it also became more ahistorical. Carnap's programme of *a priori* inductive logic was premised on a distinction between a context of discovery and a context of justification. The former concerned the process of coming up with an hypothesis, whereas the latter concerns its justification relative to the evidence. The former would be the province of psychology, although it may depend so much on details of individual biography that few general principles may be derived even *a posteriori*. Justification, however, is a matter of an *a priori* extension of the deductive logic of which Whewell had such a low opinion. Being *a priori* there was little room for the validation of inductive logic by historical examples. Rather, the conformity of historical episodes to a fully developed inductive logic would be a criterion of those episodes being genuine advances in knowledge rather than historical accidents—just as genuine mathematical knowledge must conform to the new logic of Frege, Peano, Russell, and Whitehead. Consequently, the middle period of the twentieth century (from the late 1920s to the early 1960s) was one in which the philosophy of science proceeded with little influence from or notice of the history of science.

Thus history and philosophy of science took separate paths during the years before and after the Second World War. History of science nonetheless continued to be influenced, initially at least, by broadly positivist inclinations. A crucial impetus was provided by the work of the Belgian George Sarton. In additional to his monumental *Introduction to the History of Science* and many other books and several hundred articles, Sarton was the founder, in 1912, of the first journal for the history of science, *Isis*, which has remained one of the leading journals in the field. Sarton, presaging C. P. Snow, aimed to bridge the chasm between the sciences and humanities by laying the foundations for a unified philosophy and history of science. He himself held that, "…the history of science is the only history which can illustrate the progress of mankind. In fact, 'progress' has no definite and unquestionable meaning in other fields than the field of science" (Sarton 1957: 5).

The view that the history of science is a history of progress nonetheless came under attack from several quarters. In 1931 Herbert Butterfield published his influential *The Whig Interpretation of History*. Whig history takes history to be a story of progress that justifies the present state of things. Whig historians, complained Butterfield, see the past from the perspective of the present. They regard history as a smooth transition from past to present and ignore contingency and discontinuity. History of science, from Whewell and before, up to and including Sarton (and even Butterfield's own history of science), displayed whiggish tendencies to a high degree, being often a chronology of discoveries leading to the present state of knowledge. It ignored the possibility that science might have developed in fundamentally different ways and downplayed the significance of the losers in scientific debates. While great scientists may revolutionize a field, that is typically more a matter of accelerating progress than a root and branch overturning of preceding science (exceptions being the founding fathers of modern science, such as Galileo and Copernicus, in whose cases the discontinuity is overemphasized, in order to show that what went before was hardly science at all).

In the year of the publication of Butterfield's book, Boris Hessen presented a paper at the Second International Congress of the History of Science in London, entitled "The Social and Economic Roots of Newton's *Principia*". Hessen argued that Newton's work was motivated by certain requirements of 17th century economy and society. What became known as the 'Hessen thesis', that scientific developments are best explained by their social, political, and economic context, became enormously influential—and controversial—in the history of science, even if Hessen's own explanation in the case of Newton is rejected as vulgar Marxism.[1] In particular the potential contingency of scientific thought—its dependence on accidental facts of history—stood in stark contrast to typical Whiggish view of the history of science and also to the idea of a logic of scientific discovery.

A third development that requires mention is the publication in 1935 of Ludwik Fleck's *Genesis and Development of a Scientific Fact* which emphasized the psycho-sociological influences that determine how what becomes accepted a scientific fact does become so accepted. Two features are particularly important. The first is an individual's *thought-style*. The thought-style influences how a person sees the world, and in particular how a scientist interprets what she finds before her. What observations she makes are influenced by her thought-style, and that in turn is informed by the hypotheses she holds and is investigating. The second feature is the *thought-collective* which

[1] In fact it may be that Hessen was intentionally applying vulgar Marxism to Newton's unarguably successful science in order to deflect Marxist criticism of Einstein's 'bourgeois' theories of relativity. (Cf. Graham 1985.)

is a community of persons interacting intellectually, such as a group of scientists. To interact they must share the same thought-style. If this is correct, then further pressure is put on any notion of a timeless, universal scientific method and of a logic of justification. For both what a scientist takes to be the evidence and what she infers from it will depend on features of her psychology that in turn are sensitive to her peculiar historical situation. What enters the history books as accepted scientific discoveries depends on the sharing of such psychological features by the scientific community. Although Fleck's claims do not entail that these features are themselves sensitive to extra-scientific contingencies (e.g. socio-political facts), they nonetheless lend support to the Hessen thesis.

In addition to these direct challenges to the broadly positivist or logical empiricist picture[2] of scientific progress, the new discipline of history of science presented less direct problems for the orthodox picture. Thus the traditional story of the birth of scientific revolution held that Galileo helped found the modern scientific method by emphasizing the importance of reasoning only from observational evidence, rather than from scriptural sources (contra the Church) and against empty *a priori* reasoning (contra the orthodox scholastic philosophers). But Alexandre Koyré (1939) used detailed historical scholarship to argue precisely that Galileo typically did not carry out the experiments he is reputed to have engaged in, not even those he claimed to have performed. Instead his main tool was the *a priori* one of the thought experiment. Furthermore, he was happy, as had been Copernicus, to employ medieval problems and concepts. If so, the thesis that what marks Galileo and Copernicus out from their predecessors is that they had hit upon the correct scientific method (involving observation and logically justifiable inference) is mistaken, and so casts doubt on the existence of such a unique scientific method.

2 Thomas Kuhn

While developments in the history of science put pressure on positivist inspired views of the progress of science, in the postwar years logical positivism and logical empiricism more generally were facing mounting philosophical problems, such as Quine's (1951) attack on the analytic-synthetic distinction, Goodman's (1954) new riddle of induction.

Thus circumstances were ripe for a rapprochement between the history of science and the philosophy of science from a post-posivist perspective. The most significant figure in promoting such a synthesis was Thomas Kuhn. Kuhn had started his academic career as a physicist, gaining his PhD from Harvard in 1949. In the early 1950s Kuhn taught a class on science for humanities undergraduates at Harvard, based on historical case studies, which led him into a career in the history of science. In 1961 he took up a post in history of science at Berkeley, within the philosophy department, where his colleagues included Stanley Cavell and Paul Feyerabend. This followed a long standing

[2] I regard 'logical empiricism' as a larger class of thinkers that includes the positivists and also like minded philosophers of an empiricist, pro-science bent, who concurred on matters such as the needs for a formal account of justification (e.g. inductive confirmation, falsification), but who did not subscribe to key logical positivist theses, such as verificationism.

interest in philosophy, which culminated in his second book, the famous *The Structure of Scientific Revolutions* (1962). (Kuhn's first book *The Copernican Revolution* (1957) argued that Copernicus should not be seen as an entirely revolutionary figure, the first modern astronomer, but rather as an intermediate figure who retained many Ptolemaic and Aristotelian elements in his thinking.)

The Structure of Scientific Revolutions may be called 'theoretical history' by which I mean it does two things that have an analogue in natural science:

> (i) a *descriptive* element—it identifies a general pattern in the development of science: science is a puzzle-solving enterprise which shows a cyclical pattern of normal science, crisis, revolution, normal science;
> (ii) an *explanatory* element—it proposes an explanation of the pattern identified in (i): puzzle-solving is driven by adherence to a *paradigm* (an exemplary puzzle-solution).

Kuhn does not himself clearly distinguish these two elements and quite naturally describes the first in terms of the second. Nonetheless, it is useful to characterize the descriptive pattern independently of the explanatory mechanism.

2.1 Kuhn's description of the history of science

In Kuhn's description of scientific puzzle-solving, the history of a scientific field is dominated by periods of *normal science*. Normal science, superficially at least, resembles scientific progress as traditionally described and of the kind one might expect from a standard positivist viewpoint. Scientific success is cumulative; it is by and large steady; it does not encounter significant obstacles or anomalies; scientists of all levels of skill are able to make worthwhile contributions.

Nonetheless, it is also important to note that in Kuhn's description normal science is highly conservative. Karl Popper's philosophy of science (1959) turned positivism on it head, but nonetheless belongs to the broadly logical empiricist camp. Believing, on the ground of Hume's problem, that induction is not a rational inference procedure, Popper argued that the rationality of science lay not in the process of confirming theories but in the process of refuting them. A counterinstance falsifies a theory and by finding such instances and discarding refuted theories, we make progress. Thus Popper's philosophy of science generates an outline for the development of any rational science: it is the succession of the refutation of theories, not the accumulation of confirmed theories. Correspondingly, a rational scientist, desiring to make progress, should be attempting to refute extant theories. Scientists themselves found Popper's picture congenial, portraying them as disinterested seekers after truth, selflessly seeking weaknesses in their own favoured theories, while at the same time open-minded, radical thinkers, always ready to overthrow orthodoxy.

According to Kuhn, however, science is very rarely like that. During periods of normal science scientists share a great deal by way of accepted theory, methodology, experimental equipment and techniques, and values. These are not questioned during normal science; indeed an acceptance of these things is a prerequisite for entering the profession as a scientist in the relevant field. These provide the background that make normal science, the process of puzzle-solving, possible. Kuhn describes various kinds of puzzle-solving, including determining the value of constants in equations, perfecting experimental techniques, extending the application of an existing theory to new instances. In so doing the scientists are not challenging or attempting to refute basic theory, which, on the contrary, forms an essential assumption of their work.

In the course of basic science observations may be made that seem to conflict with the underlying accepted theory. These are *anomalies*. But even these do not count as Popperian refutations. Anomalies may themselves be regarded as just further fodder for puzzle-solving. The puzzle is to reconcile the observations and theory. A good example of this is the anomalous orbit of Uranus, which although in apparent conflict with Newton's law, was shown in fact to be in full conformity by the discovery of the existence of Neptune by Leverrier and Adams.

Other, unsolved, anomalies may be shelved for later consideration. They become troubling only when they arise in sufficient numbers, or more importantly they arise in an area that is particularly significant for the underlying theory or its applications (or which is central to the employment of some important technique or piece of apparatus) and continue to defy solution. Under such circumstances it is difficult for normal science to continue in its previously settled vein, and the field is on the verge of *crisis*. A crisis arises when the accumulation of significant solution-resistant anomalies is such that a sizable proportion of practitioners come to doubt the efficacy of the underlying theory (technique, equipment) to continue to support a puzzle-solving tradition. This in turn means that the field is ripe for *revolution*, which is the proposal of a new and rival theory to replace the old one.

Kuhn notes that revolutions are typically not smooth affairs. There may be considerable resistance to change. For reasons we will come to, Kuhn does not regard the decision to change as one that is rationally forced. However, an important factor may be noted immediately. This is the phenomenon known as 'Kuhn-loss'. According to Kuhn a new theory never solves all the puzzles that were regarded as solved by the old theory. It must solve a respectable proportion of the worrying anomalies, but this will be at the cost of leaving unsolved some of the puzzles that had previously be solved successfully. Thus there is a trade-off which may not have a rationally obvious balance of benefits over costs.

2.2 Kuhn's explanation for the history of science

Kuhn not only describes this cyclical pattern in the history of science, but also gives an explanation for it. Kuhn's key idea is that of a *paradigm*. Since that term has become something of a cliché, it is important to understand exactly what Kuhn meant by it. While its use in *The Structure of Scientific Revolutions* was somewhat varied, Kuhn later clarified that usage in to two related meanings. The broader meaning is that of a consensus around a variety of components of scientific activity: key theories and equations, a terminology, accepted mathematical techniques and experimental procedures. A constellation of such things around which there is a consensus in normal science Kuhn called a *disciplinary matrix*. For the narrower sense of 'paradigm' Kuhn used the term *exemplar*. Exemplars are one element of the disciplinary matrix. But they are the most important element, that which explains the remainder. An exemplar is a particularly significant scientific achievement—a puzzle-solution (or set of related puzzle-solutions) which is so effective that it can crystallize support around it and which serves as a model for future research.

The important feature of a paradigm-as-examplar's being a model for future research is that future proposed puzzle-solutions are evaluated according to their similarity to the exemplar. Making judgments of similarity is not a matter that can be settled by the application of rules. When students learn to become scientists they do not learn facts and methodological rules for making discoveries or for evaluating potential discoveries. Rather they are trained in the use of exemplary techniques. This training is a matter of familiarization through repeated exposure and practice. For example, undergraduate textbook exercises are intended to instill in the student a feel for how to solve the problems, so that the application of techniques and the apprehension of similarities between puzzles and between puzzle-solutions is immediate. The process is similar to the use of finger-exercises by musicians. Training with exemplars, and the sense of similarity and acquired techniques that it engenders, gives a scientist a particular thought-style of the kind that Fleck describes.

This explains the conservatism of normal science. During such a phase the shared thought-style of the thought-collective constrains and directs the thinking of its members. In enables them to see certain new puzzle-solutions and to come to a shared judgment concerning proposed puzzle-solutions. So long as the exemplar is fruitful, this process is efficient and effective. Kuhn also describes what he sometimes call pre-paradigm science. This is the initial phase of a science when there is no agreement on basics. Practitioners will be divided into different schools which differ according to their views on fundamental issues of theory, metaphysics, method, etc.. But rather than make progress with solving scientific problems, much energy is put into disputes between the schools. This pre-paradigm period can be brought to a close when one school makes a discovery—solves a shared problem in a spectacular fashion—which allows it to draw adherents away from its competitor schools. That discovery forms the paradigm, the model, for the first phase of normal science in this field. Kuhn's view is essentially conservative in the sense of Karl Mannheim (1953). He emphasizes the importance of tradition in shaping what people think and do. Indeed there is a normative element, since Kuhn thinks that science cannot function without some degree of respect for the tradition, without which we would be permanently in a state of pre-paradigm foundational dispute, failing to add to our knowledge. At the same time, scientists must be able to innovate and to discard paradigms that have outlived their usefulness. This conflict between tradition and innovation Kuhn describes in his essay "The Essential Tension" (1959).

The functioning of paradigms-as-exemplars also explains the nature of crisis and revolution. A single anomaly does not refute a theory in the simple logical fashion that Popper claimed. Equally there is no logically clear and decisive refutation of a theory by an accumulation of significant anomalies. Hence there is room for rational disagreement about whether and to what degree a paradigm is in trouble when anomalies arise. Similarly there is room for rational disagreement over whether a new paradigm should supersede an older one. For this reason a science in crisis is more open to influences external to science—Kuhn mentions the nationality of scientists—or influence that are internal to science but which do not typically play a role in normal science—such as the reputation of a scientist.

2.3 Philosophical significance

The Structure of Scientific Revolutions had an enormous impact on the philosophy of science, even though it was not philosophy in any recognizable way. Its significance lay in that fact that its portrayal of science and its history, and more importantly, the explanation in terms of paradigms-as-exemplars, was in deep tension with the conceptions of scientific reasoning provided by the logical empiricists. Philosophers as divergent as Carnap and Popper agreed that the inferential relationship (inductive confirmation or falsification) between evidence and theory should be a formal, logical matter. The proposed confirmation or falsification of an hypothesis is rule-governed, where the notion of a rule is of something that can be explicitly written down and followed algorithmically. That inference should be so understood was held to be a criterion of its rationality.

Consequently, that Kuhn should be suggesting that acceptance of an hypothesis is governed not by explicit, formal rules, but instead by a non-formal, imprecise condition of similarity to an exemplar was taken by his critics to be suggesting that science is irrational. To many Kuhn's proposal seemed to be a version of relativism, on the grounds that scientific acceptability is defined relative to a paradigm, rather than by reference to some fixed standard (such as a sempiternal logic). Indeed, many followers of Kuhn also took him to be saying this. They also took his brief reference to possible influences external to science (during revolutions) to be an endorsement of the externalist perspective on science that goes back to Hessen. In fact Kuhn's intentions were different. He did not regard his account of science as showing science to be irrational. Rather he was arguing, in effect, that scientific rationality is not as the logical empiricists took it to be. Learning from exemplars is an ubiquitous feature of human learning, especially, but not only, in language learning; it is not irrational elsewhere, nor is it in science.

Kuhn's work showed how history of science could be highly influential in philosophy of science. The philosophers of science held two theses: (i) if science is to be rational, scientific inference must take form X (viz. the following of logical rules); (ii) science is in fact rational. Since (ii) is a factual claim, the combination of these two had empirically testable results. Much of science, and the best science in particular, should show that it takes form X. The empirical tests here are a matter of looking at episodes from the history of science. Kuhn's work shows that science did not have form X at all. As we have seen, that could be taken as having only an empirical conclusion concerning science, that it is irrational. But if we agree that science is the best example of rationality we have (or at least an example), then we are forced instead to draw the philosophically more significant conclusion that the logical positivists and other logical empiricists were wrong about what constitutes rationality in science. Thus even if one thinks of history of science as descriptive and philosophy of science as normative, the former can be relevant to the latter in that, given certain assumptions about science in fact satisfying the norms (e.g. science is largely rational), it had better be that the historian's description meshes with the philosopher's prescription.

The consequential interrelationship between history of science and philosophy of science that became so prominent in the 1960s and 1970s lead to the development of programmes and department of history and philosophy of science. Kuhn (1977) himself denied that the two disciplines could merge. In his view quite different mindsets were required to practice each and there could not be a common objective to be achieved in carrying out both simultaneously—although one might be do both, but separately. Nonetheless, history of science could be a useful source of data in the manner described above. Very frequently, Kuhn complained, the picture of science, even as an idealized picture, provided by philosophers was unrecognizable to the historians of science and indeed to scientists themselves. (Kuhn felt that this relationship is asymmetrical. Historians would often need to know about the philosophical schools of thought prevalent in the periods they were studying. But they did not need to know any contemporary philosophy of science.)

3 Imre Lakatos

3.1 Kuhn versus Popper; Lakatos's methodology of scientific research programmes

As described above, Kuhn's conception of science contrasts not only with that of the positivists but also with Sir Karl Popper's critical rationalism. Methodological falsificationism, as explicated in Popper's (1959) *Logic of Scientific Discovery*, is the specific application of critical rationalism to the process of scientific change. According to Popper a scientist's attitude towards favoured hypotheses should not be one of seeking confirming instances but is rather one of testing them rigorously in order to discover potential refuting instances. On uncovering such an instance, the scientist ought to reject the hypothesis and seek an alternative. Scientific progress is a matter of repeated conjecture and refutation, new conjecture and so forth.

When set against Kuhn's emphasis on the significance of normal science, Popper's falsificationism offers a stark contrast. In Popper's view scientific progress occurs only as a result of the rejection of a hypothesis, whereas in Kuhn's account the latter occurs only during *extraordinary* science, which is to say as the result of a scientific revolution. Thus Popper ignores normal science and regards all progressive science as revolutionary. Furthermore Popper regards refutation as a logical matter whereas Kuhn holds that the rejection of an old paradigm is not logically compelling and may be a matter over which rational disagreement is possible, as a consequence of which a scientific revolution may be a drawn out affair. Normal science violates the requirements of critical rationalism. Rather than criticize accepted theories, Kuhnian normal scientists unquestioningly take them as given and seek to fill in any remaining gaps in those theories or toapply them to new phenomena. During normal science any anomalies are typically shelved rather than taken as grounds for rejecting the theory. Only the accumulation of particularly problematic anomalies—those that present difficulties for the very practice of normal science—leads to doubt concerning the paradigm theories. According to Popper Kuhnian normal science shows pernicious conservatism. According to Kuhn Popperian methodological falsificationism fails to match the facts of the history of science.

Furthermore Kuhn's critics regarded Kuhn as declaring that science is irrational. In addition to the failure to criticize during normal science, Kuhn tells us that revolutionary disputes cannot be decided by rational, logical argument. Kuhn does say this,

> Individual scientists embrace a new paradigm for all sorts of reasons and usually for several at once. Some of these reasons—for example, the sun worship that helped make Kepler a Copernican—lie outside the apparent sphere of science altogether. Others may depend upon idiosyncrasies of autobiography and personality. Even the nationality or the prior reputation of the innovator and his teachers can sometimes play a significant role. (Kuhn 1962: 153)

which indicates that external factors may play a part in the outcomes of scientific disputes. The significance of such factors for Kuhn has been overstated; this short passage is followed by a much longer and more detailed discussion of how the most effective way of advancing a new paradigm is to show that it solves the problems that led the old one into crisis, the role that crucial experiments may play, and the significance of quantitative precision—all of which are highly internal factors. Nonetheless, in the 1960s this passage and, more generally, Kuhn's rejection of the idea that rules of rationality pay a significant role in science, led Imre Lakatos to regard Kuhn as taking scientific change to be a matter of 'mob psychology' (Lakatos 1970: 178). Nonetheless, Lakatos did recognize the force of Kuhn's historical criticism of Popper—all important theories have been surrounded by an 'ocean of anomalies', which on a falsificationist view would require the rejection of the theory outright. In his "Falsification and the Methodology of Scientific Research Programmes" (1970) Lakatos sought to reconcile the rationalism of Popperian falsificationism with what seemed to be its own refutation by history.

Popper's conception of a theory and its relationship to the evidence is essentially a static one, driven by the logical relation between a general statement and the singular statements that may contradict it. Lakatos instead took the object of research not to be a theory understood as a static set of propositions, but instead took it to be a dynamic entity which may change over time, the *research programme*. At its heart is the *hard core*, the leading theoretical idea. Lakatos noted, following Pierre Duhem (1914), that theoretical claims do not get tested against observation directly, but only via intermediary or *auxiliary* propositions. In Lakatos's central example, the Newtonian research programme, the hard core consists of the laws of motion and gravitation. But these have no lesson for what we should observe when looking at the Moon, Sun, and the planets, unless we add various claims about their masses, positions at particular times, even their shapes and orientations. To progress the research programme, Newtonians sought to add to the body of auxiliary propositions in such a way that the application of the combined theory and auxiliary belt grows in scope and accuracy. Anomalies are to be expected in a young research programme whose auxiliary hypotheses may be oversimplified, inaccurate, or incomplete. In the Newtonian case, the application of the hard core to the Sun and each of the planets individually will produce anomalous results, since such applications ignore the gravitational force of the other planets. In such a case the programme itself shows clearly how one is to develop the auxiliary belt in order to eliminate those anomalies, for it tells us that the other planets will have a gravitational attraction which though small will need to be considered for the programme to grown in accuracy. The steer that the programme gives to its own development Lakatos called the *positive heuristic*. This complements the *negative heuristic*, which is the injunction not to change the hard core in the face of an anomaly. For, following Quine's development of Duhem, Lakatos noted that any proposition may be saved from falsification if one is willing to make sufficient changes to other propositions with which it is connected. The negative heuristic directs change away from the hard core to the auxiliary belt, while the positive heuristic tells us which changes to make.

Thus far Lakatos' account seems to be a little more than a redescription of Kuhn's, which the hard core replacing the paradigm theory, the development of the auxiliary belt being the practice of normal science, the positive heuristic being the model provided by the exemplary applications of the paradigm theory and the negative heuristic being the fact that paradigms are unquestioned during normal science. There are nonetheless important differences. One is thes fact that for Kuhn much of the operation of a paradigm is tacit, which Lakatos regarded as an anti-rationalistic elitism. More significant still was the difference in view as concerns revolutions or the refutation of a research programme. Lakatos did accept against Popper that scientists do not reject a hitherto successful theory in the face of even serious anomalies, unless some alternative is available. Thus the question is 'when is a theory refuted?' but 'when is one theory shown to be superior to another?' But unlike Kuhn, Lakatos thought that this question may be given a definite rational answer. Just as a research programme is progressing when it increases in content and has independent corroboration for its growing content, a research programme is *degenerating* when, in order to obey the negative heuristic ('protect the hard core'), the programme reduces its scope (e.g. by building in exceptions) or adds uncorroborated *ad hoc* hypotheses. A research programme will be degenerating during the period that Kuhn would identify as a crisis. A revolution occurs when a rival, progressive research programme supersedes the degenerating one, as occurred, argues Lakatos's student Elie (Zahar 1973a,b), when Einstein's programme superseded Lorentz's in the early years of the twentieth century. According to Lakatos it is acceptable for a scientist to continue working on a degenerating research programme. Nonetheless, such a scientist should keep a score of the relative merits of that programme and is rivals. Rational scientists, whichever programme they happen to be working on, should be able to agree on the score at any given time.

3.2 Lakatos's Hegelianism and the function of the history of science

Kuhn accused Lakatos of rewriting history when it came to showing how history vindicated his position. The relationship between history of science and philosophy of science is a difficult one. One could take the view that philosophy of science is normative, articulating what inferences scientists *should* make, while history is descriptive, telling us what scientists in fact did. These could be independent—we do not think that normative ethics is answerable to history, since we all know that people do not do what they (morally) ought to do. However science is different, since most philosophers of science think that scientists are by and large rational or at least that 'science' is rational. In which case a good philosophical theory should not diverge as regards what it says ought to happen in science too far from what history tells us actually does happen. In this way a philosophical methodology turns into an historical research programme. For example, Popperian methodology becomes the historical claim that revolutions are frequent and are accompanied by decisive crucial experiments. Consequently history can help in arbitrating methodological disputes between inductivism, falsificationism, conventionalism, and, of course, Lakatos's methodology of scientific research programmes.

In the light of this, Kuhn's accusation that Lakatos in effect falsifies history ought to be a serious charge—perhaps history does not vindicate Lakatos as strongly as he thought it did? However, Lakatos did not think that the *historical* research programme of scientific methodology is just a matter of writing history with as much detailed accuracy as possible, independently of any conception of rationality, to give a result that may be compared with the various philosophical methodologies. Instead Lakatos conceived of the appropriate kind of history as a rational *reconstruction* of history.

To understand Lakatos's rational reconstructions of history it is important to recognize the Hegelian element in Lakatos's thought. According to Hegel history has an underlying 'logic' (to which we will come shortly). While that logic is inevitable, particular events may be mere chance occurrences of no lasting significance, that nonetheless obscure the underlying logic. While a perfect chronology might record such facts, such a chronology would fail to reveal the deeper structure of history (rather as a mere record of experimental outcomes would fail to show the underlying laws of nature). A philosophical history should demonstrate the working of that structure and may thus ignore the distracting details that may on occasion deviate from it. The 'logic' referred to is well-known. An idea or *thesis*, which may govern some historical epoch, has within itself certain 'contradictions' (internal tensions) which in due course give rise to an opposing idea, the *antithesis*. The creative friction between thesis and antithesis brings about a third idea, the *synthesis* which is a resolution of that struggle. In Lakatos's work (1976), the Hegelian triad first appears in the description of how 'countrexamples' to a mathematical proof lead to conceptual improvement and a more generalized proof. In the methodology of scientific research programmes, a similar idea is at work. The thesis is the current state of the research programme, and an anomaly plays the role of the antithesis, so that the synthesis is the later stage of the research programme, with the auxiliary belt amended and improved so as to expand its scope and accuracy. In both the mathematical and the scientific cases, the Hegelian element comes not simply in the application of the triad but also in the fact that its application matches a progressive and rational underlying logic. The history of mathematics and science ought to lay bare the operation of that logic and thus should display and clarify the *rationality* of the process; but as mentioned, that logic may be obscured, especially by individual chance occurrences. Consequently what is required is not a description of the events but a reconstruction of them so that they display the rational and progressive nature of the unfolding of history. In this respect, rationally reconstructed history is rather like the report of an experiment in a scientific paper. On the one hand the paper is a report of an historical event and the reasoning engaged in by the scientists. On the other hand, it is not a warts-and-all, chronological and causal history. Thus details will be left out (e.g. bad data caused by the failure to recalibrate the instruments) or rearranged (the order of the ideas in the scientists' reasoning) in order to display clearly the relevant facts and their logical significance.

Kuhn and Lakatos thus had widely differing conceptions of the relationship between philosophy of science and history of science. Kuhn held that the relationship was asymmetrical. Philosophy of science needed history of science to ensure that its implicit descriptions of science indeed do apply to some actual practice. On the other hand, history of science might get along fine without any philosophy of science. Lakatos, on the other hand, saw a rather more subtle relationship between the two. Appropriating Kant, Lakatos (1971: 91) remarked that "Philosophy of science without history of science is empty; history of science without philosophy of science is blind." He thus agrees with Kuhn that philosophy of science needs history of science in order to have a subject matter—indeed he goes further since the very point of philosophy of science is to reveal the Hegelian logic underlying the surface history of events. At the same time, the aim of the history of science should be to demonstrate the working out of the logic in particular cases, which it can hardly do in ignorance of philosophy, without which history will be the blind collection of miscellaneous facts. This difference in approach is also reflected in their attitudes towards the concepts of truth and rationality. Kuhn regards the truth of a scientific hypothesis as explanatorily irrelevant to its fate and has little to say on the question of rationality. Such an approach would later be magnified into the *symmetry principle* of the Sociology of Scientific Knowledge, according to which true beliesf and false beliefs must be treated in an explanatorily equivalent manner, likewise rational beliefs and irrational ones. That principle was aimed in large part explicitly at Lakatos, since on Lakatos's view a rational shift in theory is to be explained by the methodology of scientific research programmes while an irrational episode (such as the survival of a degenerating research programme in the face of a progressive rival) requires a quite different kind of explanation—such pathological episodes are those explained by social and political factors. Thus for Lakatos the internal-external distinction also marks a rational-irrational divide; for Kuhn there is an internal-external distinction but this is not a matter of the boundaries of rationality but concerns rather what counts as engaging in (modern) science; for later sociologists of science there is no meaningful internal-external distinction at all.

4 Paul Feyerabend

Paul Feyerabend started his philosophical life with an interest in the Vienna Circle and Wittgenstein's philosophy, but became a student of Popper's in 1952, adopting a liberal falsificationism. Feyerabend was later a colleague of Kuhn's at Berkeley. He initially stressed the normative character of the philosophy of science, following Popper. But as the 1960s progressed the emphasis shifted towards a more descriptive, historical approach to understanding science. (The shift is one of emphasis, since Feyerabend urged a closer attention to historical details even before he met Kuhn and employed historical arguments against normative proposals in the early 1960s, while even in the 1970s he continued to make normative claims. Oberheim (2007) argues that Feyerabend was opportunistic with regard to using whatever arguments were available in advancing understanding through criticism.)

In 1970 Feyerabend published a long article which later he turned into the book *Against Method* (1975)—having previously intended to publish it alongside a rationalist argument *for* method by Lakatos, a project prevented by Lakatos's death in 1974. Feyerabend sought to show that no methodological rule would promote scientific progress under all circumstances—any proposed rule would inhibit progress under some circumstance or other. Feyerabend's approach was to consider historical episodes that we pre-theoretically regard as progressive and then to show that those episodes violate the methodological prescriptions that one might expect to apply.

Feyerabend's much-discussed case study concerns Galileo's arguments for Copernicanism. According to Feyerabend, had Galileo been either a naive empiricist or a Popperian falsificationist, then Galileo would have had to abandon his endorsement of Copernicanism. For example, Galileo defended Copernicanism against the Tower Argument. Were the Earth moving, the agument proceeds, we would expect a rock dropped from a tower not to fall at its base but rather at some distance, the distance that part of the Earth has moved during the fall of the rock. Galileo counters by describing the case of throwing a ball within the cabin of a moving ship. The force with which the ball should be thrown and its direction are independent of the ship's (uniform) motion, which is shared by the throwers. Galileo's argument shows that the rock falling at the tower's base is predicted by his theory also. In which case his moving-Earth theory and the Aristotelian static-Earth theory are observationally equivalent. If that is the case the naive empiricist should refuse to prefer one theory to the other. Thus Galileo's endorsement of one over the other is inconsistent with naive empiricism. Assuming that Galileo did assist science in progressing, then naive empiricism is a methodological prescription that would have been anti-progressive in that context.

Galileo's behaviour does not respect the requirements of naive falsificationism, since Copernicanism is refuted by the observed sizes of Mars and Venus. There ought to be much greater perceived variation in size of the planets, depending on whether Venus is at superior or inferior conjunction and Mars is at conjunction or opposition, than we in fact observe. Feyerabend also considered sophisticated falsificationism, according to which we should prefer theories with greater empirical content, including additional falsifiable predictions. Feyerabend claims that the Copernican system had no additional empirical content. It is true that Galileo asserted that the telescopic observations of the phases of Venus are direct confirmation of a novel prediction of the theory (while the observations of the moons of Jupiter are indirect supporting evidence). Feyerbend argued that Galileo was not entitled to rely upon such observations because the telescope could not be held to be reliable for celestial observations, and indeed the competing Aristotelian theory justified not inferring from the telescope's terrestrial reliability to celestial reliability, because it held the laws to differ in the two regions. Furthermore, Galileo's new physics represented a reduction in content in that it concerned only locomotion as compared with the wider range of phenomena of change encompassed by Aristotelian physics—which in addition to locomotion included qualitative change, and generation and corruption.

When it comes to Lakatos's Methodology of Scientific Research Programmes, matters are a different. For Lakatos accepts that early in the development of a new theory it will encounter apparently falsifying instances and may need to reduce its scope and hence empirical content relative to a well-established rival. Hence the evidence Feyerabend presents does no damage to that view. Instead Feyerabend claims that Lakatos's account fails to provide any methodological prescriptions worth the name. Indeed

Feyerband regards Lakatos's view as being closet anarchism disguised as methodological rationalism. It should be noted that Feyerabend's claim was not that standard methodological rules should never be obeyed, but rather that sometimes progress is made by abandoning them. In the absence of a generally accepted rule, there is a need for alternative methods of persuasion. According to Feyerabend, Galileo employed stylistic and rhetorical techniques to convince his reader, while he also wrote in Italian rather than Latin and directed his arguments to those already temperamentally inclined to accept them.

5 Recent developments

Feyerabend's work caused considerable debate, principally over the historical accuracy of his interpretation of Galileo (e.g. Machamer 1973). Moreover, the focus on conceptions of rationality and method then prominent leaves room for other conceptions that may be consistent with Galileo's behaviour.

Nonetheless, Feyerabend's work, along with Kuhn's, did have the effect of persuading philosophers of science and others that their accounts of science, even if intended normatively, should be tested against the history of science. The legacy of this historical philosophy of science may be regarded as having bifurcated, with radical historians and sociologists of science on the one side and the majority of philosophers of science on the other. On the former side the tacit assumption that scientific rationality, were there such a thing, would be a matter of following rules of method, is accepted. This, along with Kuhn's and Feyerabend's demonstration that scientists do not follow such rules, leads to the conclusion that science is not the rational enterprise it is often held to be. Feyerabend's emphasis on rhetoric and other non-rational forms of persuasion meshes with versions of the Hessen thesis, that scientific change is explained by social and political forces rather than new evidence. Consequently much effort has been put into historico-sociological work, much of it under the heading 'Sociology of Scientific Knowledge', intended to show such forces at work in key episodes in the history of science.

Among philosophers of science a typical response has been to dissociate rationality from the idea of a scientific method. Thus science might be rational even if there are no fixed rules of method. For example, it might be rational for a scientist to infer the likely truth of the hypothesis that is the best explanation of the evidence; but there may be no methodical rule for determining which hypothesis is the best explanation. Furthermore, many philosophers of science have taken on board the lessons of naturalized epistemology. According to one version of that view, the methods of inquiry that lead to progress or truth cannot be uncovered *a priori*, as the logical empiricists including Popper thought, but need themselves to be discovered *a posteriori* by scientific and other means. Consequently, prescriptive philosophy of science has largely been abandoned. Descriptive philosophy of science remains, in that one may wish to describe the general features of rational science, and in such cases philosophers recognize the importance of showing that concrete historical episodes do exemplify these generalized descriptions.

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