

CHAPTER 9

Theories as structures II: Research programs

Introducing Imre Lakatos

Imre Lakatos was a Hungarian who moved to England in the late 1950s and came under the influence of Karl Popper who, in Lakatos's own words "changed [his] life" (Worrall and Currie, 1978a, p. 139). Although an avid supporter of Popper's approach to science, Lakatos came to realise some of the difficulties that faced Popper's falsificationism, difficulties of the kind we have considered in chapter 7. By the mid-1960s Lakatos was aware of the alternative view of science contained in Kuhn's *The Structure of Scientific Revolutions*. Although Popper and Kuhn proposed rival accounts of science, their views do have much in common. In particular, they both take a stand against positivist, inductivist accounts of science. They both give priority to theory (or paradigm) over observation, and insist that the search for, interpretation and acceptance or rejection of the results of observation and experiment take place against a background of theory or paradigm. Lakatos carried on that tradition, and looked for a way of modifying Popper's falsificationism and ridding it of its difficulties, among other ways by drawing on some of the insights of Kuhn while totally rejecting the relativist aspects of the latter's position. Like Kuhn, Lakatos saw the merit in portraying scientific activity as taking place in a framework, and coined the phrase "research program" to name what were, in a sense, Lakatos's alternatives to Kuhn's paradigms. The primary source for an account of Lakatos's methodology is his 1970 text.

Lakatos's research programs

We saw in chapter 7 that one of the main difficulties with Popper's falsificationism was that there was no clear guidance concerning which part of a theoretical maze was to be blamed for an apparent falsification. If it is left to the whim of the individual scientist to place the blame wherever he or she might wish, then it is difficult to see how the mature sciences could progress in the coordinated and cohesive way that they seem to do. Lakatos's response was to suggest that not all parts of a science are on a par. Some laws or principles are more basic than others. Indeed, some are so fundamental as to come close to being the defining feature of a science. As such, they are not to be blamed for any apparent failure. Rather, the blame is to be placed on the less fundamental components. A science can then be seen as the programmatic development of the implications of the fundamental principles. Scientists can seek to solve problems by modifying the more peripheral assumptions as they see fit. Insofar as their efforts are successful they will be contributing to the development of the same *research program* however different their attempts to tinker with the peripheral assumptions might be.

Lakatos referred to the fundamental principles as the *hard core* of a research program. The hard core is, more than anything else, the defining characteristic of a program. It takes the form of some very general hypotheses that form the basis from which the program is to develop. Here are some examples. The hard core of the Copernican program in astronomy was the assumption that the earth and the planets orbit a stationary sun and that the earth spins on its axis once a day. The hard core of Newtonian physics is comprised of Newton's three laws of motion plus his law of gravitational attraction. The hard core of Marx's historical materialism would be something like the assumption that major social change is to be explained in terms of class struggle, the nature of the classes and the details of the struggle being determined, in the last instance, by the economic base.

The fundamentals of a program need to be augmented by

a range of supplementary assumptions in order to flesh it out to the point where definite predictions can be made. It will consist not only of explicit assumptions and laws supplementing the hard core, but also assumptions underlying the initial conditions used to specify particular situations and theories presupposed in the statement of observations and experimental results. For example, the hard core of the Copernican program needed to be supplemented by adding numerous epicycles to the initially circular orbits and it was also necessary to alter previous estimates of the distance of the stars from earth. Initially the program also involved the assumption that the naked eye serves to reveal accurate information concerning the position, size and brightness of stars and planets. Any inadequacy in the match between an articulated program and observation is to be attributed to the supplementary assumptions rather than the hard core. Lakatos referred to the sum of the additional hypotheses supplementing the hard core as the *protective belt*, to emphasise its role of protecting the hard core from falsification. According to Lakatos (1970, p. 133), the hard core is rendered unfalsifiable by "the methodological decisions of its protagonists". By contrast, assumptions in the protective belt are to be modified in an attempt to improve the match between the predictions of the program and the results of observation and experiment. For instance, the protective belt within the Copernican program was modified by substituting elliptical orbits for Copernicus's sets of epicycles and telescopic data for naked-eye data. The initial conditions also came to be modified eventually, with changes in the estimate of the distance of the stars from the earth and the addition of new planets. Lakatos made free use of the term "heuristic" in characterising research programs. A heuristic is a set of rules or hints to aid discovery or invention. For example, part of a heuristic for solving crossword puzzles might be "start with the clues requiring short-word answers and then proceed to those requiring long-word answers". Lakatos divided guidelines for work within research programs into a *negative heuristic* and a

positive heuristic. The negative heuristic specifies what the scientist is advised not to do. As we have already seen, scientists are advised not to tinker with the hard core of the program in which they work. If a scientist does modify the hard core then he or she has, in effect, opted out of the program. Tycho Brahe opted out of the Copernican program when he suggested that only the planets, but not the earth, orbit the sun and that the sun orbits the earth.

The *positive heuristic* of a program, that which specifies what scientists should do rather than what they should not do within a program, is more difficult to characterise specifically than the negative heuristic. The positive heuristic gives guidance on how the hard core is to be supplemented and how the resulting protective belt is to be modified in order for a program to yield explanations and predictions of observable phenomena. In Lakatos's own words (1970, p. 135), "the positive heuristic consists of a partially articulated set of suggestions or hints on how to change, develop, the 'refutable variants' of the research program, how to modify, sophisticate, the 'refutable' protective belt". The development of the program will involve not only the addition of suitable auxiliary hypotheses but also the development of adequate experimental and mathematical techniques. For instance, from the very inception of the Copernican program it was clear that mathematical techniques for combining and manipulating epicycles and improved techniques for observing planetary positions were necessary. Lakatos illustrated the notion of a positive heuristic with the story of Newton's early development of his gravitational theory. Here, the positive heuristic involved the idea that one should start with simple, idealised cases and then, having mastered them, one should proceed to more complicated, and more realistic, cases. Newton first arrived at the inverse square law of attraction by considering the elliptical motion of a point planet around a stationary point sun. It was clear that if the program was to be applied in practice to planetary motions then it would need to be developed from this idealised form to a more realistic one. But that

development involved the solution of theoretical problems and was not to be achieved without considerable theoretical labour. Newton himself, faced with a definite program, that is, guided by his positive heuristic, made considerable progress. He first took into account the fact that the sun as well as a planet moves under the influence of their mutual attraction. Then he took account of the finite size of the planets and treated them as spheres. After solving the mathematical problem posed by that move, Newton proceeded to allow for other complications such as those introduced by the possibility that a planet can spin, and the fact that there are gravitational forces between the individual planets as well as between each planet and the sun. Once Newton had progressed that far in the program, following a path that had presented itself as more or less necessary from the outset, he began to be concerned about the match between his theory and observation. When the match was found wanting he was able to proceed to non-spherical planets and so on. As well as the theoretical program, the positive heuristic contained an experimental one. That program included the development of more accurate telescopes, together with auxiliary theories necessary for their use in astronomy, such as those providing adequate means for allowing for refraction of light in the earth's atmosphere. The initial formulation of Newton's program already indicated the desirability of constructing apparatus sensitive enough to detect gravitational attraction on a laboratory scale (Cavendish's experiment).

The program that had Newton's laws of motion and his law of gravitation at its core gave strong heuristic guidance. That is, a fairly definite program was mapped out from the start. Lakatos (1970, pp. 140–55) gives an account of the development of Bohr's theory of the atom as another example of a positive heuristic in action. An important feature of these examples of developing research programs, stressed by Lakatos, is the comparatively late stage at which observational testing becomes relevant. This is in keeping with the comments about Galileo's construction of his mechanics in the

first section of chapter 8. Early work in a research program is portrayed as taking place without heed or in spite of apparent falsifications by observation. A research program must be given a chance to realise its full potential. A suitable sophisticated and adequate protective belt must be constructed. In our example of the Copernican program, this included the development of an adequate mechanics that could accommodate the earth's motion and an adequate optics to help interpret the telescopic data. When a program has been developed to the stage where it is appropriate to subject it to experimental tests, it is confirmations rather than falsifications that are of paramount significance, according to Lakatos. The worth of a research program is indicated by the extent to which it leads to novel predictions that are confirmed. The Newtonian program experienced dramatic confirmations of this kind when Galle first observed the planet Neptune and when Halley's comet returned as predicted. Failed predictions, such as Newton's early calculations of the moon's orbit, are simply indications that more work needs to be done on supplementing or modifying the protective belt.

The main indication of the merit of a research program is the extent to which it leads to novel predictions that are confirmed. A second indication, implicit in our discussion above, is that a research program should indeed offer a *program* of research. The positive heuristic should be sufficiently coherent to be able to guide future research by mapping out a program. Lakatos suggested Marxism and Freudian psychology as programs that lived up to the second indicator of merit but not to the first, and contemporary sociology as one that lives up to the first to some extent but not the second (although he did not back up these remarks with any detail). In any event, a *progressive* research program will be one that retains its coherence and at least intermittently leads to novel predictions that are confirmed, while a *degenerating* program will be one that loses its coherence and/or fails to lead to confirmed novel predictions. The

replacement of a degenerating program by a progressive one constitutes Lakatos's version of a scientific revolution.

Methodology within a program and the comparison of programs

We need to discuss Lakatos's methodology of scientific research programs in the context of work within a program and in the context of the clash between one research program and another. Work within a single research program involves the expansion and modification of its protective belt by the addition and articulation of various hypotheses. Any such move is permissible so long as it is not ad hoc in the sense discussed in chapter 6. Modifications or additions to the protective belt of a research program must be independently testable. Individual scientists or groups of scientists are open to modify or augment the protective belt in any way they choose, provided these moves open up the opportunity for new tests and hence the possibility of novel discoveries. By way of illustration, let us take an example from the development of the Newtonian program that we have employed several times before and consider the situation that confronted Leverrier and Adams when they addressed themselves to the troublesome orbit of the planet Uranus. Those scientists chose to modify the protective belt of the program by proposing that the initial conditions were inadequate and suggesting that there was an as yet unidentified planet close to Uranus and disturbing its orbit. Their move was in accordance with Lakatos's methodology because it was testable. The conjectured planet could be sought for by training telescopes on the appropriate region of the sky. But other possible responses would be legitimate according to Lakatos's position. For instance, the problematic orbit could be blamed on some new type of aberration of the telescope, provided the suggestion was made in a way that made it possible to test for the reality of such aberrations. In a sense, the more testable moves that are made to solve a problem such as this the better, because this increases the

chances of success, (where success means the confirmation of the novel predictions ensuing from a move). Moves that are ad hoc are ruled out by Lakatos's methodology. So, in our example, an attempt to accommodate Uranus's problematic orbit by simply labelling that complex orbit as the natural motion of Uranus would be ruled out. It opens up no new tests and hence no prospect of novel discoveries.

A second kind of move ruled out by Lakatos's methodology are ones that involve a departure from the hard core. Making such a move destroys the coherence of a program and amounts to opting out of that program. For instance, a scientist attempting to cope with Uranus's orbit by suggesting that the attraction between Uranus and the Sun was something other than the inverse square law would be opting out of the Newtonian research program.

The fact that any part of a complex theoretical maze might be responsible for an apparent falsification poses a serious problem for the falsificationist relying on an unqualified method of conjectures and refutations. For that person, the inability to locate the source of the trouble leads to unmethodical chaos. Lakatos's methodology is designed to avoid that consequence. Order is maintained by the inviolability of the hard core of the program and by the positive heuristic that accompanies it. The proliferation of ingenious conjectures within that framework will lead to progress provided some of the predictions resulting from those conjectures occasionally prove successful. Decisions to retain or reject an hypothesis are fairly straightforwardly determined by the results of experimental tests. The bearing of observation on an hypothesis under test is relatively unproblematic within a research program because the hard core and the positive heuristic serve to define a fairly stable observation language.

As was mentioned above, Lakatos's version of a Kuhnian revolution involves the ousting of one research program by another. We have seen that Kuhn (1970, p. 94) was unable to give a clear answer to the question of the sense in which a paradigm can be said to be superior to the one it replaces, and

so left him with no option but to appeal to the authority of the scientific community. Later paradigms are superior to their predecessors because the scientific community judges them to be so, and ‘there is no standard higher than the assent of the relevant community’. Lakatos was dissatisfied with the relativist implications of Kuhn’s theory. He sought a standard that lay outside of particular paradigms or, in Lakatos’s case, research programs, which could be used to identify some non-relativist sense in which science progresses. To the extent that he had such a standard, it lay in his conception of progressing and degenerating research programs. Progress involves the replacement of a degenerating program with a progressive one, with the latter being an improvement on the former in the sense that it has been shown to be a more efficient predictor of novel phenomena.

Novel predictions

The non-relativist measure of progress that Lakatos proposed relied heavily on the notion of a novel prediction. One program is superior to another insofar as it is a more successful predictor of novel phenomena. As Lakatos came to realise, the notion of a novel prediction is not as straightforward as it might at first appear, and care is needed to mould that notion into a form that serves the purpose required of it within Lakatos’s methodology or, indeed, any methodology that seeks to make significant use of it.

We have already met novel predictions in the context of Popper’s methodology. In that context I suggested that the essence of Popper’s position is that a prediction is novel, at a particular time, to the extent that it does not figure in, or perhaps clashes with, the knowledge that is familiar and generally accepted at that time. For Popper, testing a theory by way of its novel predictions amounted to a severe test of that theory just because the prediction clashed with prevailing expectations. Lakatos’s use of novel predictions in something like the Popperian sense to help him characterise the

progressiveness of a research program will not do, as he himself came to realise, and this can be established by means of fairly straightforward counter examples, examples drawn from the very programs that Lakatos freely utilised to illustrate his position. The counter examples involve situations where the worth of a research program is demonstrated by its ability to explain phenomena that at the time were already well established and familiar, and so not novel in the Popperian sense.

There are features of planetary motion that have been well known since antiquity, but which were adequately explained only with the advent of the Copernican theory. They include the retrograde motion of the planets and the fact that the planets appear brightest when they are retrogressing, as well as the fact that Venus and Mercury never appear far from the sun. The qualitative features of these phenomena follow straightforwardly once it is assumed that the earth orbits the sun along with the planets and that the orbits of Mercury and Venus are inside that of the earth, whereas in the Ptolemaic theory they can only be explained by introducing epicycles designed specifically for the purpose. Lakatos joined Copernicus, and I imagine most of the rest of us, in recognising this as a major mark of the superiority of the Copernican over the Ptolemaic system. However, the Copernican prediction of the general features of planetary motion did not count as novel in the sense we have defined it for the straightforward reason that those phenomena had been well known since antiquity. The observation of parallax in the stars was probably the first confirmation of the Copernican theory by a prediction that counts as novel in the sense we are discussing, but that doesn’t suit Lakatos’s purpose at all, since it did not occur until well into the nineteenth century, well after the superiority of Copernicus over Ptolemy had been accepted within science.

Other examples are readily found. One of the few observations that could be invoked to support Einstein’s general theory of relativity was the precession of the perihelion of the

orbit of the planet Mercury, a phenomenon well known and accepted long before Einstein's theory explained it. One of the most impressive features of quantum mechanics was its ability to explain the spectra exhibited by the light emitted from gases, a phenomenon familiar to experimenters for over half a century before the quantum mechanical explanation was available. These successes can be described as involving the novel prediction of phenomena rather than the prediction of novel phenomena.

Lakatos came to realise, in the light of some considerations put forward by E. Zahar (1973), that the account of novel predictions in his original formulation of the methodology of scientific research programs needed to be modified. After all, when assessing the extent to which some observable phenomena supports a theory or program, surely it is a historically contingent fact of no philosophical relevance whether it is the theory or knowledge of the phenomena that comes first. Einstein's theory of relativity can explain the orbit of Mercury and also the bending of light rays in a gravitational field. These are both considerable achievements that support the theory. It so happens that the precession of the perihelion of Mercury was known prior to Einstein's formulation of the theory, whereas the bending of light rays was discovered subsequently. But would it make any difference to our assessment of Einstein's theory if it had been the other way around, or if both phenomena had been known before or both discovered after? The fine details of the appropriate response to these reflections are still being debated, for example by Alan Musgrave (1974b) and John Worrall (1985 and 1989a), but the intuition that needs to be grasped, and which is at work in the comparison of Copernicus and Ptolemy, seems straightforward enough. The Ptolemaic explanation of retrograde motion did not constitute significant support for that program because it was artificially fixed up to fit the observable data by adding epicycles especially designed for the purpose. By contrast, the observable phenomena followed in a natural way from the fundamentals of the Copernican theory without

any artificial adjustment. The predictions of a theory or program that count are those that are natural rather than contrived. Perhaps what lies behind the intuition here is the idea that evidence supports a theory if, without the theory, there are unexplained coincidences contained in the evidence. How could the Copernican theory successfully predict all the observable general features of planetary motion if it wasn't essentially correct? The same argument does not work in the case of the Ptolemaic explanation of the same phenomena. Even if the Ptolemaic theory is quite wrong, it is no coincidence that it can explain the phenomena because the epicycles have been added in such a way as to ensure that it does. This is the way in which Worrall (1985, 1989) treats the matter.

In the light of this, we should reformulate Lakatos's methodology so that a program is progressive to the extent that it makes natural, as opposed to novel, predictions that are confirmed, where "natural" stands opposed to "contrived" or "ad hoc". (We shall revisit this issue from a different and perhaps superior angle in chapter 13.)

Testing the methodology against history

Lakatos shared Kuhn's concern with the history of science. He believed it to be desirable that any theory of science be able to make sense of the history of science. That is, there is a sense in which a methodology or philosophy of science is to be tested against the history of science. However, the precise way in which this is so needs to be carefully spelt out, as Lakatos was well aware. If the need for a philosophy of science to match the history of science is interpreted undiscriminatingly, then a good philosophy of science will become nothing more than an accurate description of science. As such, it will be in no position to capture the essential characteristics of science or to discriminate between good science and bad science. Popper and Lakatos tended to regard Kuhn's account as "merely" descriptive, in this sense, and hence deficient. Popper was so wary of the problem that he, unlike Lakatos,

denied that comparison with the history of science was a legitimate way of arguing for a philosophy of science.

I suggest that the essentials of Lakatos's position, as described in his 1978 text, are these. There are episodes in the history of science that are unproblematically progressive and which can be recognised as such prior to any sophisticated philosophy of science. If someone wants to deny that Galileo's physics was an advance on Aristotle's or that Einstein's was an advance on Newton's then he or she is just not using the word science in the way that the rest of us are. To be concerned with the question of how best to categorise science we must have some pre-theoretical notion of what science is in order to formulate the question, and that pre-theoretical notion will include the ability to recognise classic examples of major scientific achievements such as those of Galileo and Einstein. With these presuppositions as a background, we can now demand that any philosophy or methodology of science be compatible with them. That is, any philosophy of science should be able to grasp the sense in which Galileo's achievements in astronomy and physics were in the main major advances. So if the history of science reveals that in his astronomy Galileo transformed what were considered to be the observable facts, and in his mechanics he relied mainly on thought experiments rather than real ones, then that poses a problem for those philosophies that portray scientific progress as cumulative, progressing by way of the accumulation of secure observational facts and cautious generalisations from them. Lakatos's own early version of his methodology of research programs can be criticised for utilising a notion of novel prediction in a way that makes it impossible to grasp the sense in which Copernicus's astronomy was progressive, as I did in the previous section.

With this mode of argument, Lakatos proceeds to criticise positivist and falsificationist methodologies on the grounds that they fail to make sense of classic episodes in the progress of science, and argues, by contrast, that his own account does not suffer from the same deficiency. Turning, then, to more

minor episodes in the history of science, Lakatos, or a supporter, can pick on episodes from the history of science that have puzzled historians and philosophers and show how they make complete sense from the point of view of the methodology of scientific research programs. Thus, for example, many have been puzzled by the fact that when Thomas Young proposed the wave theory of light in the early nineteenth century it won few supporters, whereas Fresnel's version, devised two decades later, won widespread acceptance. John Worrall (1976) gives historical support to Lakatos's position when he shows that, as a matter of historical fact, Young's theory was not strongly confirmed experimentally in a natural, as opposed to a contrived, way, as Fresnel's was, and that Fresnel's version of the wave theory had a vastly superior positive heuristic by virtue of the mathematical tools he was able to introduce. A number of Lakatos's students or former students carried out studies, appearing in Howson (1976), intended to support Lakatos's methodology in this kind of way.

Lakatos came to see the main virtue of his methodology to be the aid it gives to the writing of the history of science. The historian must attempt to identify research programs, characterise their hard cores and protective belts, and document the ways in which they progressed or degenerated. In this way, light can be shed on the way science progresses by way of the competition between programs. I think it must be conceded that Lakatos and his followers did succeed in casting useful light on some classic episodes in the history of the physical sciences by studies carried out in this way, as the essays in Howson (1976) reveal. Although Lakatos's methodology can offer advice to historians of science, it was not intended by Lakatos as a source of advice for scientists. This became an inevitable conclusion for Lakatos given the way he found it necessary to modify falsificationism to overcome the problems it faced. Theories should not be rejected in the face of apparent falsifications because the blame might in due course be directed at a source other than the theory, and single

successes certainly do not establish the merit of a theory for all time. That is why Lakatos introduced research programs, which are given time to develop and may come to progress after a degenerating period, or degenerate after early successes. (It is worth recalling in this connection that the Copernican theory degenerated for about a century after its early successes before the likes of Galileo and Kepler brought it to life again.) But once this move is taken, it is clear that there can be no on-the-spot advice forthcoming from Lakatos's methodology along the lines that scientists must give up a research program, or prefer a particular research program to its rival. It is not irrational or necessarily misguided for a scientist to remain working on a degenerating program if he or she thinks there are possible ways to bring it to life again. It is only in the long term (that is, from a historical perspective) that Lakatos's methodology can be used to meaningfully compare research programs. In this connection, Lakatos came to make a distinction between the *appraisal* of research programs, which can only be done with historical hindsight, and *advice* to scientists, which he denied it was the purpose of his methodology to offer. "There is no instant rationality in science" became one of Lakatos's slogans, capturing the sense in which he considered positivism and falsificationism, insofar as they can be interpreted as offering criteria that can be used for the acceptance and rejection of theories, as striving for too much.

Problems with Lakatos's methodology

As we have seen, Lakatos regarded it as appropriate to test methodologies against the history of science. It is therefore legitimate, even in his own terms, to raise the question of whether his methodology is descriptively adequate. There are grounds for doubting that it is. For instance, are there such things as "hard cores" serving to identify research programs to be found in the history of science? Counter evidence comes from the extent to which scientists do on occasions attempt

to solve problems by adjusting the fundamentals of the theories or programs in which they work. Copernicus himself, for example, moved the sun a little to the side of the centres of planetary orbits, had the moon orbit the earth rather than the sun, and came to use all sorts of devices to adjust the details of the epicyclical motions, to the extent that those motions ceased to be uniform. So what exactly was the hard core of the Copernican program? In the nineteenth century there were serious attempts to cope with problems such as the motion of the planet Mercury by modifying the inverse square law of attraction. There are violations of some of Lakatos's own prime examples of hard cores to be found in history, therefore.

A deeper problem concerns the reality or otherwise of the methodological decisions that play such an important role in Lakatos's account of science. For instance, as we have seen, according to Lakatos (1970, p. 133) the hard core of a program is rendered unfalsifiable by "the methodological decisions of its protagonists". Are these decisions a historical reality or a figment of Lakatos's imagination? Lakatos does not really give any evidence for the answer that he needs, and it is not totally clear what kind of study would provide that evidence. The issue is a vital one for Lakatos, for the methodological decisions are the locus of the distinction between his own position and that of Kuhn. Both Kuhn and Lakatos agree that scientists work in a coordinated way within a framework. For Kuhn, in one of his moods at least, the question of how and why they do so is to be revealed by sociological analysis. For Lakatos this leads to an unacceptable relativism. So for him, the cohesion is brought about by methodological decisions that are *rational*. Lakatos does not provide an answer to the charge that these decisions have no historical (or contemporary) reality, nor does he give a clear answer to the question of the sense in which they should be regarded as rational.

Another fundamental criticism of Lakatos is directly connected with the central theme of this book, the question of what, if anything, is characteristic of scientific knowledge.

Lakatos's rhetoric, at least, suggests that his methodology was intended to give a definitive answer to that question. He claimed that the "central problem in the philosophy of science is — the problem of stating *universal* conditions under which a theory is scientific", a problem that is "closely linked with the problem of the rationality of science" and whose solution "ought to give us guidance as to when the acceptance of a scientific theory is rational or not" (Worrall and Currie, 1978a, pp. 168–9, italics in original). Lakatos (1970, p. 176) portrayed his methodology as a solution to these problems that would "help us in devising laws for stemming — intellectual pollution". "I [Lakatos] give criteria for progression and stagnation within a program and also rules for the 'elimination' of whole research programs" (Worrall and Currie, 1978a, p. 112). It is clear from the details of Lakatos's position, and his own comments on those details, that Lakatos's methodology was not capable of living up to these expectations. He did not give rules for the elimination of whole research programs because it is rational to stick to a degenerating program in the hope that it will make a comeback. And if it was scientific to stick to the Copernican theory for the century that it took for that theory to bear significant fruit, why aren't contemporary Marxists (one of Lakatos's prime targets) scientific in attempting to develop historical materialism to a point where it will bear significant fruit. Lakatos in effect conceded that his methodology was in no position to diagnose any contemporary theory as non-scientific "intellectual pollution" once he recognised and acknowledged, in the context of physical science, that his methodology could only make judgments in retrospect, with the benefit of historical hindsight. If there is no "instant rationality" then there can be no on-the-spot rejection of Marxism, sociology or any other of Lakatos's *bêtes noir*.

Another basic problem with Lakatos's methodology stems from the way in which he deemed it necessary to support it by studies from the history of science. Lakatos and his followers made the necessary case by means of case studies of

physical sciences over the last three hundred years. But if the methodology supported in this way is then used to judge other areas, such as Marxism or astrology, what is in effect being assumed without argument is that all areas of study, if they are to be regarded as "scientific", must share the basic characteristics of physics. Paul Feyerabend (1976) has criticised Lakatos in this way. Lakatos's procedure certainly begs an important fundamental question and has only to be explicitly stated to reveal a problem. There are a number of *prima facie* reasons at least why one might expect that a methodology and set of standards for judging physics might not be appropriate in other areas. Physics can, and often does, proceed by isolating individual mechanisms — gravity, electromagnetic forces, the mechanisms at work when fundamental particles collide and so on — in the artificial circumstances of a controlled experiment. People and societies cannot in general be treated in this way without destroying what it is that is being investigated. A great deal of complexity is necessary for living systems to function as such, so even biology can be expected to exhibit some important differences from physics. In social sciences the knowledge that is produced itself forms an important component of the systems being studied. So, for example, economic theories can effect the way in which individuals operate in the market place, so that a change in theory can bring about a change in the economic system being studied. This is a complication that does not apply in the physical sciences. The planets do not change their motions in the light of our theories about those motions. Whatever the force of the arguments that can be developed from reflections such as these, it remains the case that Lakatos presupposes, without argument, that all scientific knowledge should in some fundamental sense be like the physics of the last three hundred years.

Another fundamental issue is brought to light when we consider the implications of a study by Lakatos (1976a), published posthumously, on "Newton's effect on scientific standards". In that study, Lakatos makes the case that New-

ton, in practice, brought about a change in scientific standards, a change that Lakatos clearly regards as progressive. But the fact that Lakatos can make such a case does not rest easily with the assumption he makes repeatedly elsewhere, that an appraisal of science must be made with respect to some "universal" criterion. If Newton changed scientific standards for the better, then one can ask, "with respect to what standard was the change progressive"? We have a problem of a similar kind to the one that confronted Kuhn. It is a problem we will need to confront, or perhaps dispel, later in this book.

Further reading

The central text for Lakatos's methodology is his 1970 text, "Falsification and the Methodology of Scientific Research Programmes". Most of the other key papers have been collected in Worrall and Currie (1978a and 1978b). Also important is Lakatos (1968), *The Problem of Inductive Logic*, and (1971), "Replies to Critics". A fascinating account of Lakatos's application of his ideas to mathematics is his *Proofs and Refutations* (1976b). Howson (1976) contains historical case studies designed to support Lakatos's position. Another such study is Lakatos and Zahar (1975). Cohen, Feyerabend and Wartofsky (1976) is a collection of essays in memory of Lakatos. Feyerabend (1976) is an important critique of Lakatos's methodology. The notion of a novel prediction is discussed by Musgrave (1974b), Worrall (1985), Worrall (1989a) and Mayo (1996). A useful overview of Lakatos's work is B. Larvor (1998), *Lakatos: An Introduction*.

CHAPTER 10

Feyerabend's anarchistic theory of science

The story so far

We seem to be having trouble with our search for the characterisation of science that will serve to pick out what distinguishes it from other kinds of knowledge. We started with the idea, adopted by the positivists who were so influential earlier in the century, that science is special because it is derived from the facts, but this attempt floundered because facts are not sufficiently straightforward for this view to be sustained, since they are "theory-dependent" and fallible, and because no clear account of how theories can be "derived" from the facts could be found. Falsificationism did not fare much better, mainly because in any realistic situation in science it is not possible to locate the cause of a faulty prediction, so a clear sense of how theories can be falsified becomes almost as elusive as a clear sense of how they can be confirmed. Both Kuhn and Lakatos tried to solve the problem by focusing attention on the theoretical framework in which scientists work. However, Kuhn, for his part, stressed the extent to which workers in rival paradigms "live in different worlds" to such a degree that he left himself with inadequate resources for elucidating a sense in which a change from one paradigm to another in the course of a scientific revolution is a step forward. Lakatos tried to avoid that trap, but, apart from problems concerning the reality of the methodological decisions he freely invoked in his answer, he ended up with a criterion for characterising science that was so lax that few intellectual pursuits could be ruled out. One philosopher of science who was not surprised by, and who attempted to draw out what he saw to be the full implications of, these failures was Paul Feyerabend, whose controversial but nevertheless

influential “anarchistic” account of science is described and assessed in this chapter.

Feyerabend’s case against method

Paul Feyerabend, an Austrian who was based in Berkeley, California, for most of his academic career, but who also spent time interacting with (and antagonising) Popper and Lakatos in London, published a book in 1975 with the title *Against Method: Outline of an Anarchistic Theory of Knowledge*. In it he challenged all of the attempts to give an account of scientific method that would serve to capture its special status by arguing that there is no such method and, indeed, that science does not possess features that render it necessarily superior to other forms of knowledge. If there is a single, unchanging principle of scientific method, Feyerabend came to profess, it is the principle “anything goes”. There are passages in Feyerabend’s writings, both early and late, that can be drawn on to severely qualify the extreme anarchistic account of science that is contained in the bulk of *Against Method*. However, it will be most instructive for our purpose to stick to the unqualified, anarchistic theory of science to see what we can learn from it. In any case, it is the extreme form of Feyerabend’s position that has made its mark in the literature and which philosophers of science have, not without difficulty, attempted to counter.

Feyerabend’s main line of argument attempts to undermine characterisations of method and progress in science offered by philosophers by challenging them on their own ground in the following way. He takes examples of scientific change which his opponents (including the vast majority of philosophers) consider to be classic instances of scientific progress and shows that, as a matter of historical fact, those changes did not conform to the theories of science proposed by those philosophers. (Feyerabend does not have to himself agree that the episodes in question were progressive for his argument to go through.) The main example appealed to by

Feyerabend involves the advances in physics and astronomy made by Galileo. Feyerabend’s point is that if an account of method and progress in science cannot even make sense of Galileo’s innovations, then it is not much of an account of science. In this outline of Feyerabend’s position I will stick largely to the Galileo example, mainly because it is sufficient to illustrate Feyerabend’s position, but also because the example is readily understood without requiring resort to recdondite technicalities.

A number of Feyerabend’s points will be familiar because I have already drawn on them for various purposes earlier in this book.

Quotations invoked in chapter 1 of this book illustrate the positivist or inductivist view that Galileo’s innovations can be explained in terms of the extent to which he took the observable facts seriously and built his theories to fit them. The following passage from Galileo’s *Dialogue Concerning the Two Chief World Systems* (1967), cited by Feyerabend (1975, pp. 100–101), indicates that Galileo thought otherwise.

You wonder that there are so few followers of the Pythagorean opinion [that the earth moves] while I am astonished that there have been any up to this day who have embraced and followed it. Nor can I ever sufficiently admire the outstanding acumen of those who have taken hold of this opinion and accepted it as true: they have, through sheer force of intellect done such violence to their own senses as to prefer what reason told them over that which sensible experience plainly showed them to the contrary. For the arguments against the whirling of the earth we have already examined are very plausible, as we have seen: and the fact that the Ptolemaics and the Aristotelians and all their disciples took them to be conclusive is indeed a strong argument of their effectiveness. But the experiences which overtly contradict the annual movement are indeed so much greater in their apparent force that, I repeat, there is no limit to my astonishment when I reflect that Aristarchus and Copernicus were able to make reason so conquer sense that, in defiance of the latter, the former became mistress of their belief.

Far from accepting the facts considered to be borne out by

the senses by his contemporaries, it was necessary for Galileo (1967, p. 328) to conquer sense by reason and even to replace the senses by "a superior and better sense", namely the telescope. Let us consider two instances where Galileo needed to "conquer" the evidence of the senses — his rejection of the claim that the earth is stationary and his rejection of the claim that the apparent sizes of Venus and Mars do not change appreciably during the course of the year.

If a stone is dropped from the top of a tower it falls to the base of the tower. This, and other experiences like it, can be taken as evidence that the earth is stationary. For if the earth moves, spinning on its axis, say, (the whirling of the earth referred to by Galileo in the passage cited) then should it not move from beneath the stone during its fall, with the result that the stone should fall some distance from the base of the tower? Did Galileo reject this argument by appealing to the facts? That is certainly not how Galileo did it in the *Dialogue*, as Feyerabend pointed out. Galileo (1967, p. 125 ff) achieved the desired result by "picking the brains" of the reader. He argued as follows. The speed of a ball set rolling down a frictionless slope will increase, because it is "falling" towards the centre of the earth to some degree. Conversely, the speed of a ball rolled up a frictionless slope will decrease because it is rising away from the centre of the earth. Having persuaded the reader to accept this as obvious, he or she is now asked what will happen to the speed of the ball if the slope is perfectly horizontal. It would seem that the answer is that the speed will neither increase nor decrease since the ball will be neither rising nor falling. The horizontal motion of the ball persists and remains constant. Although this falls short of Newton's law of inertia, it is an example of a uniform motion that persists without a cause, and it is sufficient for Galileo to counter a range of arguments against the spinning earth. Galileo draws the implication that the horizontal motion of the stone falling from the tower, which it shares with the tower as the earth spins, remains unchanged. That is why it stays with the tower, striking the ground at its foot. So the

tower argument does not establish that the earth is stationary in the way many had supposed. To the extent that Galileo's case was successful it did not involve appealing to the results of observation and experiment, at his own admittance. (I point out here that frictionless slopes were even harder to obtain in Galileo's time than they are now, and that measuring the speed of a ball at various locations on the slope lay beyond what was feasible at the time.)

We saw in chapter 1 that the apparent sizes of Venus and Mars were important insofar as the Copernican theory predicted that they should change appreciably, a prediction not borne out by naked-eye observations. The problem is resolved once the telescopic rather than the naked-eye data is accepted. But how was the preference for the telescopic data to be defended? Feyerabend's rendering of the situation and Galileo's response to it run as follows. Accepting what the telescope revealed in the astronomical context was by no means straightforward. Galileo did not have an adequate or detailed theory of the telescope, so he could not defend the telescopic data by appeal to one. It is true that in a terrestrial context there were trial and error methods of vindicating telescopic sightings. For instance, the reading of an inscription on a distant building, indiscernible to the naked eye, could be checked by going close to the building, and the identification of the cargo of a distant ship could be vindicated once the ship arrived in port. But the vindication of terrestrial use could not be straightforwardly employed to justify astronomical use of the telescope. Terrestrial use of the telescope is aided by a range of visual cues absent in the astronomical case. Genuine images can be distinguished from many artifacts of the telescope because we are familiar with the kinds of things being inspected. So, for instance, if the telescope reveals the mast of a distant ship to be wavy, red on one side and blue on the other and accompanied by black specks hovering above it, the distortions, colours and specks can be dismissed as artifacts. However, when looking into the heavens, we are in unfamiliar territory and lack clear guidance as

to what is really there as opposed to an artifact. What is more, comparison with familiar objects to help judge size, and the use of parallax and overlap to help judge what is far and what is near, is a luxury not in general available in astronomy and it is certainly not the case that Galileo could check telescopic sightings of planets by moving closer to them to check with the naked eye. There was even direct evidence that the telescopic data was erratic insofar as it magnified the moon to a different degree than it magnified the planets and stars.

According to Feyerabend (1975, p. 141), these difficulties were such that recourse to argument would have been inadequate for the task of convincing those opponents who wished to deny both the Copernican theory and the telescopic data relating to the heavens. Consequently, Galileo needed to, and did, resort to propaganda and trickery.

On the other hand, there are some telescopic phenomena which are plainly Copernican. Galileo introduces these phenomena as independent evidence for Copernicus while the situation is rather that one refuted view — Copernicanism — has a certain similarity to phenomena emerging from another refuted view — the idea that telescopic phenomena are faithful images of the sky. Galileo prevails because of his style and his clever techniques of persuasion, because he writes in Italian rather than in Latin, and because he appeals to people who are temperamentally opposed to the old ideas and the standards of learning connected with them.

It should be clear that if Feyerabend's construal of Galileo's methodology is correct and typical of science, then standard positivist, inductivist and falsificationist accounts of science have serious problems accommodating it. It can be accommodated into Lakatos's methodology, according to Feyerabend, but only because that methodology is so lax that it can accommodate almost anything. Feyerabend teased Lakatos by welcoming him as a "fellow anarchist", albeit one "in disguise", playfully dedicating *Against Method* to Lakatos "friend, and fellow anarchist". The way in which Feyerabend construes the two frameworks, the Aristotelian stationary

earth framework backed up by naked-eye data and the Copernican, moving earth theory supported by telescopic data, as mutually exclusive circles of thought, as it were, is reminiscent of Kuhn's portrayal of paradigms as mutually exclusive ways of seeing the world. Indeed, the two philosophers both independently coined the word "incommensurable" to describe the relationship between two theories or paradigms that cannot be logically compared for lack of theory-neutral facts to exploit in the comparison. Kuhn avoided Feyerabend's anarchistic conclusions essentially by appealing to social consensus to restore law and order. Feyerabend (1970) rejected Kuhn's appeal to the social consensus of the scientific community, partly because he did not think Kuhn distinguished between legitimate and illegitimate ways (for example by killing all opponents) of achieving consensus, and also because he did not think the appeal to consensus was capable of distinguishing between science and other activities such as theology and organised crime.

Given the failure of attempts to capture the special features of scientific knowledge that render it superior to other forms, which failure Feyerabend considered himself to have established, he drew the conclusion that the high status attributed to science in our society, and the superiority it is presumed to have not only over Marxism, say, but over such things as black magic and voodoo, are not justified. According to Feyerabend, the high regard for science is a dangerous dogma, playing a repressive role similar to that which he portrays Christianity as having played in the seventeenth century, having in mind such things as Galileo's struggles with the Church.

Feyerabend's advocacy of freedom

Feyerabend's theory of science is situated in an ethical framework which places a high value on individual freedom, involving an attitude that Feyerabend described as the "humanitarian attitude". According to that attitude, individual humans should

be free and possess liberty in something like the sense the nineteenth-century philosopher John Stuart Mill (1975) defended in his essay "On Liberty". Feyerabend (1975, p. 20) declared himself in favour of "the attempt to increase liberty, to lead a full and rewarding life" and supports Mill in advocating "the cultivation of individuality which alone produces, or can produce, well-developed human beings". From this humanitarian point of view, Feyerabend supports his anarchistic account of science on the grounds that it increases the freedom of scientists by removing them from methodological constraints and, more generally, leaves individuals the freedom to choose between science and other forms of knowledge.

From Feyerabend's point of view, the institutionalisation of science in our society is inconsistent with the humanitarian attitude. In schools, for example, science is taught as a matter of course. "Thus, while an American can now choose the religion he likes, he is still not permitted to demand that his children learn magic rather than science at school. There is a separation between state and Church, there is no separation between state and science" (1975, p. 299). What we need to do in the light of this, wrote Feyerabend (1975, p. 307), is to "free society from the strangling hold of an ideologically petrified science just as our ancestors freed us from the strangling hold of the One True Religion!". In Feyerabend's image of a free society, science will not be given preference over other forms of knowledge or over other traditions. A mature citizen in a free society is "a person who has learned to make up his mind and who has then *decided* in favour of what he thinks suits him best". Science will be studied as a historical phenomenon "together with other fairy tales such as the myths of 'primitive' societies" so that each individual "has the information needed for arriving at a free decision" (1975, p. 308, *italics in original*). In Feyerabend's ideal society the state is ideologically neutral between ideologies to ensure that individuals maintain freedom of choice and do not have an ideology imposed on them against their will.

The culmination of Feyerabend's case against method, together with his advocacy of a particular brand of freedom for the individual, is his anarchistic theory of knowledge (1975, pp. 284–5, *italics in original*).

None of the methods which Carnap, Hempel, Nagel [three prominent positivists], Popper or even Lakatos want to use for rationalising scientific changes can be applied, and the one that can be applied, refutation, is greatly reduced in strength. What remains are aesthetic judgments, judgments of taste, metaphysical prejudices, religious desires, in short, *what remains are our subjective wishes*: science at its most advanced and general returns to the individual a freedom he seems to lose in its more pedestrian parts.

There is no scientific method, then. Scientists should follow their subjective wishes. Anything goes.

Critique of Feyerabend's individualism

A critique of Feyerabend's understanding of human freedom will act as a useful preliminary to an appraisal of his critique of method. A central problem with Feyerabend's notion of freedom stems from the degree to which it is entirely negative, in the sense that freedom is understood as freedom from constraints. Individuals should be free of constraints to the extent that they can follow their subjective wishes and do what they like. This overlooks the positive side of the issue, the extent to which individuals have access to the means to fulfil their wishes. For example, freedom of speech can be, and often is, discussed in terms of freedom from constraints, in the form of state suppression, libel laws and the like. So, for example, if students disrupt a lecture on campus by an academic expressing views sympathetic to Fascism they might well be accused of denying the speaker freedom of speech. They are accused of putting an obstacle in the way of the speaker's natural right. However, freedom of speech can be considered, from the positive point of view, in terms of the resources available to individuals to have their views heard

by others. What access does a particular individual have to the media, for example? This point of view puts our example in a different light. The disruption of the lecture could perhaps be justified on the grounds that the speaker was given access to a university lecture hall, microphone, media advertising and so on in a way that those advocating other views were not. The eighteenth-century philosopher David Hume nicely illustrated the point I am getting at when he criticised John Locke's idea of the Social Contract. Locke had construed the social contract as being freely adopted by members of a democratic society and argued that anyone not wishing to subscribe to the contract was free to emigrate. Hume responded as follows:

Can we seriously say, that a poor peasant or artisan has a free choice to leave his country, when he knows no foreign language or manners, and lives from day to day, by the small wages which he acquires? We may as well assert that a man, by remaining in a vessel, freely consents to the domination of the master; though he was carried on board while asleep, and must leap into the ocean and perish, the moment he leaves her.¹

Individuals are born into a society that pre-exists them and which, in that sense, possesses characteristics they do not choose and cannot be in a position to choose. The courses of action open to them, and, consequently, the precise senses in which they are free, will be determined by the access that they have in practice to the resources necessary for various courses of action. In science too an individual who wishes to make a contribution to a science will be confronted by the situation as it stands: various theories, mathematical techniques, instruments and experimental techniques. The paths of action open to scientists in general will be delimited by that objectively existing situation, while the paths open to a particular scientist will be determined by the subset of the existing resources to which that individual scientist has access. Scientists will be free to follow their "subjective wishes" only insofar as they are free to chose among the restricted range of options open to them. What is more, a prerequisite for an

understanding of that situation will be a characterisation of the situation that individuals face, like it or not. Whether it be changes in science or in society generally, the main theoretical work involves understanding the situations confronted by individuals rather than involving some generalised appeal to unconstrained freedom.

It is ironic that Feyerabend, who in his study of science goes to great lengths to deny the existence of theory-neutral facts, in his social theory appeals to the far more ambitious notion of an ideology-neutral State. How on earth would such a State come into existence, how would it function and what would sustain it? In the light of work that has been done in making serious attempts to get to grips with questions about the origin and nature of "the State", Feyerabend's fanciful speculations about a utopia in which all individuals are free to follow their inclinations in an unrestricted way appear childish.

Criticising Feyerabend for setting his views on science in an individualist framework involving a naive notion of freedom is one thing. Getting to grips with the details of the case he makes "against method" in science is another. In the next chapter we will see what can be constructively salvaged from Feyerabend's attack on method.

Further reading

Feyerabend develops some of the ideas of his *Against Method: Outline of an Anarchistic Theory of Knowledge* (1975) in *Science in a Free Society* (1978). *Realism, Rationalism and Scientific Method* (Feyerabend, 1981a) and *Problems of Empiricism* (Feyerabend, 1981b) are collections of his articles, a number of which predate his "anarchistic" phase. "Consolations for the Specialist" (1970) and "On the Critique of Scientific Reason" (1976) are his critiques of Kuhn and Lakatos respectively. I have taken issue with Feyerabend's portrayal of Galileo's science in "Galileo's Telescopic Observations of

Venus and Mars" (Chalmers, 1985) and "The Galileo that Feyerabend Missed" (Chalmers, 1986).

CHAPTER 11

Methodical changes in method

Against universal method

We saw in the previous chapter that Feyerabend made a case against the various accounts of scientific method that have been put forward by philosophers as attempts to capture the distinctive feature of scientific knowledge. A key strategy that he employed was to argue for the incompatibility of those accounts and Galileo's advances in physics and astronomy. Elsewhere (in Chalmers, 1985 and 1986) I have taken issue with Feyerabend's historical account of the Galileo episode and some of the details of my disagreement will be introduced and exploited in the next section. Once that history is corrected I believe it to remain the case that the corrected history poses problems for standard accounts of science and the scientific method. That is, I suggest there is a sense in which Feyerabend's case against method can be sustained, *provided we are clear about the notion of method that has been refuted*. Feyerabend's case tells against the claim that there is a universal, ahistorical method of science that contains standards that all sciences should live up to if they are to be worthy of the title "science". Here the term "universal" is used to indicate that the proposed method is to apply to all sciences or putative sciences — physics, psychology, creation science or whatever — while the term "ahistorical" signals the timeless character of the method. It is to be used to appraise Aristotle's physics as much as Einstein's and Democritus's atomism as much as modern atomic physics. I am happy to join Feyerabend in regarding the idea of a universal and ahistoric method as highly implausible and even absurd. As Feyerabend (1975, p. 295) says, "The idea that science can, and should, be run according to fixed and universal rules is both unrealistic and pernicious", is "detrimental to science,

A.F Chalmers

**What is this thing
called Science?**

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