

## Introduction: Science and Pseudoscience\*

Man's respect for knowledge is one of his most peculiar characteristics. Knowledge in Latin is *scientia*, and science came to be the name of the most respectable kind of knowledge. But what distinguishes knowledge from superstition, ideology or pseudoscience? The Catholic Church excommunicated Copernicans, the Communist Party persecuted Mendelians on the ground that their doctrines were pseudoscientific. The demarcation between science and pseudoscience is not merely a problem of armchair philosophy: it is of vital social and political relevance.

Many philosophers have tried to solve the problem of demarcation in the following terms: a statement constitutes knowledge if sufficiently many people believe it sufficiently strongly. But the history of thought shows us that many people were totally committed to absurd beliefs. If the strength of beliefs were a hallmark of knowledge, we should have to rank some tales about demons, angels, devils, and of heaven and hell as knowledge. Scientists, on the other hand, are very sceptical even of their best theories. Newton's is the most powerful theory science has yet produced, but Newton himself never believed that bodies attract each other at a distance. So no degree of commitment to beliefs makes them knowledge. Indeed, the hallmark of scientific behaviour is a certain scepticism even towards one's most cherished theories. Blind commitment to a theory is not an intellectual virtue: it is an intellectual crime.

Thus a statement may be pseudoscientific even if it is eminently 'plausible' and everybody believes in it, and it may be scientifically valuable even if it is unbelievable and nobody believes in it. A theory may even be of supreme scientific value even if no one understands it, let alone believes it.

The cognitive value of a theory has nothing to do with its psychological influence on people's minds. Belief, commitment, understanding are states of the human mind. But the objective, scientific value of a theory is independent of the human mind which creates it or understands it. Its scientific value depends only on what objective support these conjectures have in facts. As Hume said:

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ignores the remarkable tenacity of scientific theories. Scientists have thick skins. They do not abandon a theory merely because facts contradict it. They normally either invent some rescue hypothesis to explain what they then call a mere anomaly or, if they cannot explain the anomaly, they ignore it, and direct their attention to other problems. Note that scientists talk about anomalies, recalcitrant instances, not refutations. History of science, of course, is full of accounts of how crucial experiments allegedly killed theories. But such accounts are fabricated long after the theory had been abandoned. Had Popper ever asked a Newtonian scientist under what experimental conditions he would abandon Newtonian theory, some Newtonian scientists would have been exactly as nonplussed as are some Marxists.

What, then, is the hallmark of science? Do we have to capitulate and agree that a scientific revolution is just an irrational change in commitment, that it is a religious conversion? Tom Kuhn, a distinguished American philosopher of science, arrived at this conclusion after discovering the naïvety of Popper's falsificationism. But if Kuhn is right, then there is no explicit demarcation between science and pseudoscience, no distinction between scientific progress and intellectual decay, there is no objective standard of honesty. But what criteria can he then offer to demarcate scientific progress from intellectual degeneration?

In the last few years I have been advocating a methodology of scientific research programmes, which solves some of the problems which both Popper and Kuhn failed to solve.

First, I claim that the typical descriptive unit of great scientific achievements is not an isolated hypothesis but rather a research programme. Science is not simply trial and error, a series of conjectures and refutations. 'All swans are white' may be falsified by the discovery of one black swan. But such trivial trial and error does not rank as science. Newtonian science, for instance, is not simply a set of four conjectures – the three laws of mechanics and the law of gravitation. These four laws constitute only the 'hard core' of the Newtonian programme. But this hard core is tenaciously protected from refutation by a vast 'protective belt' of auxiliary hypotheses. And, even more importantly, the research programme also has a 'heuristic', that is, a powerful problem-solving machinery, which, with the help of sophisticated mathematical techniques, digests anomalies and even turns them into positive evidence. For instance, if a planet does not move exactly as it should, the Newtonian scientist checks his conjectures concerning atmospheric refraction, concerning propagation of light in magnetic storms, and hundreds of other conjectures which are all part of the programme. He may even invent a hitherto unknown planet and calculate its position, mass and velocity in order to explain the anomaly.

Now, Newton's theory of gravitation, Einstein's relativity theory.



## INTRODUCTION

quantum mechanics, Marxism, Freudianism, are all research programmes, each with a characteristic hard core stubbornly defended, each with its more flexible protective belt and each with its elaborate problem-solving machinery. Each of them, at any stage of its development, has unsolved problems and undigested anomalies. All theories, in this sense, are born refuted and die refuted. But are they equally good? Until now I have been describing what research programmes are like. But how can one distinguish a scientific or progressive programme from a pseudoscientific or degenerating one?

Contrary to Popper, the difference cannot be that some are still unrefuted, while others are already refuted. When Newton published his *Principia*, it was common knowledge that it could not properly explain even the motion of the moon; in fact, lunar motion refuted Newton. Kaufmann, a distinguished physicist, refuted Einstein's relativity theory in the very year it was published. But all the research programmes I admire have one characteristic in common. They all predict novel facts, facts which had been either undreamt of, or have indeed been contradicted by previous or rival programmes. In 1686, when Newton published his theory of gravitation, there were, for instance, two current theories concerning comets. The more popular one regarded comets as a signal from an angry God warning that He will strike and bring disaster. A little known theory of Kepler's held that comets were celestial bodies moving along straight lines. Now according to Newtonian theory, some of them moved in hyperbolas or parabolas never to return; others moved in ordinary ellipses. Halley, working in Newton's programme, calculated on the basis of observing a brief stretch of a comet's path that it would return in seventy-two years' time; he calculated to the minute when it would be seen again at a well-defined point of the sky. This was incredible. But seventy-two years later, when both Newton and Halley were long dead, Halley's comet returned exactly as Halley predicted. Similarly, Newtonian scientists predicted the existence and exact motion of small planets which had never been observed before. Or let us take Einstein's programme. This programme made the stunning prediction that if one measures the distance between two stars in the night and if one measures the distance between them during the day (when they are visible during an eclipse of the sun), the two measurements will be different. Nobody had thought to make such an observation before Einstein's programme. Thus, in a progressive research programme, theory leads to the discovery of hitherto unknown novel facts. In degenerating programmes, however, theories are fabricated only in order to accommodate known facts. Has, for instance, Marxism ever predicted a stunning novel fact successfully? Never! It has some famous unsuccessful predictions. It predicted the absolute impoverishment of the working class. It predicted that the first socialist revolution would take place in the industrially most developed society. It



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*empirically progressive* (or 'constitutes an *empirically progressive problemshift*') if some of this excess empirical content is also corroborated, that is, if each new theory leads us to the actual discovery of some *new fact*.<sup>1</sup> Finally, let us call a problemshift *progressive* if it is both theoretically and empirically progressive, and *degenerating* if it is not.<sup>2</sup> We 'accept' problemshifts as 'scientific' only if they are at least theoretically progressive; if they are not, we 'reject' them as 'pseudoscientific'. Progress is measured by the degree to which a problemshift is progressive, by the degree to which the series of theories leads us to the discovery of novel facts. We regard a theory in the series 'falsified' when it is superseded by a theory with higher corroborated content.<sup>3</sup>

This demarcation between progressive and degenerating problemshifts sheds new light on the appraisal of *scientific* – or, rather, *progressive* – explanations. If we put forward a theory to resolve a contradiction between a previous theory and a counterexample in such a way that the new theory, instead of offering a content-increasing (scientific) *explanation*, only offers a content-decreasing (linguistic) *reinterpretation*, the contradiction is resolved in a merely semantical, unscientific way. *A given fact is explained scientifically only if a new fact is also explained with it.*<sup>4</sup>

Sophisticated falsificationism thus shifts the problem of how to appraise *theories* to the problem of how to appraise *series of theories*. Not an isolated *theory*, but only a series of theories can be said to be scientific or unscientific: to apply the term 'scientific' to one single theory is a category mistake.<sup>5</sup>

<sup>1</sup> If I already know  $P_1$ : 'Swan A is white',  $P_\omega$ : 'All swans are white' represents no progress if I discover similar facts as

indeed, what he calls 'normal science' is nothing but a research programme that has achieved monopoly. But, as a matter of fact, research programmes have achieved complete monopoly only rarely and then only for relatively short periods, in spite of the efforts of some Cartesians, Newtonians and Bohrians. *The history of science has been and should be a history of competing research programmes (or, if you wish, 'paradigms'), but it has not been and must not become a succession of periods of normal science: the sooner competition starts, the better for progress.* 'Theoretical pluralism' is better than 'theoretical monism': on this point Popper and Feyerabend are right and Kuhn is wrong.<sup>1</sup>

The idea of competing scientific research programmes leads us to the problem: *how are research programmes eliminated?* It has transpired from our previous considerations that a degenerating problemshift is no more a sufficient reason to eliminate a research programme than some old-fashioned 'refutation' or a Kuhnian 'crisis'. *Can there be any objective (as opposed to socio-psychological) reason to reject a programme, that is, to eliminate its hard core and its programme for constructing protective belts?* Our answer, in outline, is that such an objective reason is provided by a rival research programme which explains the previous success of its rival and supersedes it by a further display of *heuristic power*.<sup>2</sup>

However, the criterion of 'heuristic power' strongly depends on how we construe '*factual novelty*'. Until now we have assumed that it is immediately ascertainable whether a new theory predicts a novel fact or not.<sup>3</sup> But *the novelty of a factual proposition can frequently be seen only after a long period has elapsed*. In order to show this, I shall start with an example.

Bohr's theory logically implied Balmer's formula for hydrogen lines as a consequence.<sup>4</sup> Was this a novel fact? One might have been tempted to deny this, since after all, Balmer's formula was well-known. But this is a half-truth. Balmer merely 'observed'  $B_1$ : that *hydrogen lines obey the Balmer formula*. Bohr predicted  $B_2$ : that *the differences in the energy levels in different orbits of the hydrogen electron obey the Balmer formula*. Now one may say that  $B_1$  already contains all the purely 'observational' content of  $B_2$ . But to say this presupposes that there

<sup>1</sup> Nevertheless there is something to be said for at least *some* people sticking to a research programme until it reaches its 'saturation point'; a new programme is then challenged to account for the full success of the old. It is no argument against this that the rival may, when it was first proposed, already have explained all the success of the first programme; the growth of a research programme cannot be predicted - it may stimulate important unforeseeable auxiliary theories of its own. Also, if a version  $A_n$  of a research programme  $P_1$  is mathematically equivalent to a version  $A_m$  of a rival  $P_2$ , one should develop both: their heuristic strength can still be very different.

<sup>2</sup> I use 'heuristic power' here as a technical term to characterize the power of a research programme to anticipate theoretically novel facts in its growth. I could of course use 'explanatory power': cf. above, p. 34, n. 4.

<sup>3</sup> Cf. above, p. 31, text to n. 4, and p. 49, text to n. 2.

<sup>4</sup> Cf. above, p. 61.



fact they are *different* discoveries, merged only later into a single one.<sup>1</sup>

A favourite hunting ground of externalists has been the related problem of why so much importance is attached to – and energy spent on – *priority disputes*. This can be explained only *externally* by the inductivist, the naive falsificationist, or the conventionalist; but in the light of the methodology of research programmes some priority disputes are vital *internal* problems, since in this methodology *it becomes all-important for rational appraisal which programme was first in anticipating a novel fact and which fitted in the by now old fact only later*. Some priority disputes can be explained by rational interest and not simply by vanity and greed for fame. It then becomes important that Tychonian theory, for instance, succeeded in explaining – only *post hoc* – the observed phases of, and the distance to, Venus which were originally precisely anticipated by Copernicans;<sup>2</sup> or that Cartesians managed to explain everything that the Newtonians *predicted* – but only *post hoc*. Newtonian optical theory explained *post hoc* many phenomena which were anticipated and first observed by Huyghensians.<sup>3</sup>

All these examples show how the methodology of scientific research programmes turns many problems which had been *external* problems for other historiographies into internal ones. But occasionally the borderline is moved in the opposite direction. For instance there may have been an experiment which was accepted *instantly* – in the absence of a better theory – as a negative crucial experiment. For the falsificationist such acceptance is part of internal history; for me it is not rational and has to be explained in terms of external history.

*Note.* The methodology of research programmes was criticized both by Feyerabend and by Kuhn. According to Kuhn: '[Lakatos] must specify criteria which can be used *at the time* to distinguish a degenerative from a progressive research programme; and so on. Otherwise, *he has told us nothing at all*'. Actually, I *do* specify such criteria. But Kuhn probably meant that '[my

<sup>1</sup> This was illustrated convincingly, by Elkana, for the case of the so-called simultaneous discovery of the conservation of energy; cf. his [1971].

<sup>2</sup> Also cf. p. 115, n. 1.

<sup>3</sup> For the Mertonian brand of functionalism – as Alan Musgrave pointed out to me – priority disputes constitute a *prima facie* disfunction and therefore an anomaly for which Merton has been labouring to give a general socio-psychological explanation. (Cf. e.g. Merton [1957], [1963] and [1969].) According to Merton 'scientific knowledge is not the richer or the poorer for having credit given where credit is due: it is the social institution of science and individual men of science that would suffer from repeated failures to allocate credit justly' (Merton [1957], p. 648). But Merton overdoes his point: in important cases (like in some of Galileo's priority fights) there was more at stake than institutional interests: the problem was whether the Copernican research programme was progressive or not. (Of course, not all priority disputes have scientific relevance. For instance, the priority dispute between Adams and Leverrier about who was first to discover Neptune had no such relevance: whoever discovered it, the discovery strengthened the same (Newtonian) programme. In such cases Merton's external explanation may well be true.)

<sup>4</sup> Kuhn [1970b], p. 239, my italics.



But the question can be answered. I give my answer in two stages: I propose first a naive and then a more sophisticated answer. I start by recalling how Popper, according to his own account,<sup>1</sup> arrived at his criterion. He thought, like the best scientists of his time, that Newton's theory, although refuted, was a wonderful scientific achievement; that Einstein's theory was still better; and that astrology, Freudianism and twentieth century Marxism were pseudoscientific. His problem was to find a definition of science which yielded these 'basic judgments' concerning particular theories; and he offered a novel solution. Now let us consider the proposal that a *rationality theory* – or *demarcation criterion* – is to be rejected if it is inconsistent with an accepted 'basic value judgment' of the scientific elite. Indeed, this meta-methodological rule (meta-falsificationism) would seem to correspond to Popper's methodological rule (falsificationism) that a scientific theory is to be rejected if it is inconsistent with an ('empirical') basic statement unanimously accepted by the scientific community. Popper's whole methodology rests on the contention that there exist (relatively) singular statements on whose truth-value scientists can reach unanimous agreement; without such agreement there would be a new Babel and 'the soaring edifice of science would soon lie in ruins'.<sup>2</sup> But even if there were an agreement about 'basic' statements, if there were no agreement about how to appraise scientific achievement relative to this 'empirical basis', would not the soaring edifice of science equally soon lie in ruins? No doubt it would. While there has been little agreement concerning a *universal* criterion of the scientific character of theories, there has been considerable agreement over the last two centuries concerning single achievements. While there has been no *general* agreement concerning a theory of scientific rationality, there has been considerable agreement concerning whether a particular single step in the game was scientific or crankish, or whether a particular gambit was played correctly or not. A general definition of science thus must reconstruct the acknowledgedly best gambits as 'scientific': if it fails to do so, it has to be rejected.<sup>3</sup>

anomalies as a kind of 'background noise', as something that is 'to some extent necessary' (p. 49). But on the next page he identifies this 'dogmatism' with 'pseudoscience'. Is then pseudoscience 'to some extent necessary'? Also, cf. chapter 1, p. 89, n. 5.

<sup>1</sup> Cf. Popper [1963a], pp. 33–7.

<sup>2</sup> Popper [1934], section 29.

<sup>3</sup> This approach, of course, does not imply that we *believe* that the scientists' 'basic judgments' are unfailingly rational; it only means that we *accept* them in order to criticize universal definitions of science. (If we were to add that no such universal definition has been found and no such universal definition will ever be found, the stage would be set for Polanyi's conception of the lawless closed autocracy of science.)

My meta-criterion may be seen as a 'quasi-empirical' self-application of Popperian falsificationism. I introduced this 'quasi-empiricalness' earlier in the context of mathematical philosophy. We may abstract from *what* flows in the logical channels of a deductive system, whether it is something certain or something fallible, whether it is truth and falsehood or probability and improbability, or even moral or scientific desirability and undesirability: it is the *how* of the flow which decides whether the



Then let us propose tentatively that if a demarcation criterion is inconsistent with the 'basic' appraisals of the scientific élite, it should be rejected.

Now if we apply this quasi-empirical meta-criterion (which I am going to reject later), Popper's demarcation criterion – that is, Popper's rules of the game of science – has to be rejected.<sup>1</sup>

Popper's basic rule is that the scientist must specify in advance under what experimental conditions he will give up even his most basic assumptions. For instance, he writes, when criticizing psychoanalysis: 'Criteria of refutation have to be laid down beforehand: it must be agreed which observable situations, if actually observed, mean that the theory is refuted. But what kind of clinical responses would refute to the satisfaction of the analyst *not merely a particular analytic diagnosis but psychoanalysis itself*? And have such criteria ever been discussed or agreed upon by analysts?'<sup>2</sup> In the case of psychoanalysis Popper was right: no answer has been forthcoming. Freudians have been nonplussed by Popper's basic challenge concerning scientific honesty. Indeed, they have refused to specify experimental conditions under which they would give up their basic assumptions. For Popper this was the hallmark of their intellectual dishonesty. But what if we put Popper's question to the Newtonian scientist: 'What kind of observation would refute to the satisfaction of the Newtonian not merely a particular Newtonian explanation but Newtonian dynamics and gravitational theory itself? And have such criteria ever been discussed or agreed upon by Newtonians?' The Newtonian will, alas, scarcely be able to give a positive answer.<sup>3</sup> But then if analysts are to be condemned as dishonest by Popper's standards, Newtonians must also be condemned. Newtonian science, however, in spite of this sort of 'dogmatism', is highly regarded by the greatest scientists, and, indeed, by Popper himself. Newtonian 'dogmatism' then is a 'falsification' of Popper's definition: it defies Popper's rational reconstruction.

Popper may certainly withdraw his celebrated challenge and demand falsifiability – and rejection on falsification – only for systems of theories, including initial conditions and all sorts of auxiliary and observational theories.<sup>4</sup> This is a considerable withdrawal, for it allows

system is negativist, 'quasi-empirical', dominated by *modus tollens* or whether it is justificationist, 'quasi-Euclidean', dominated by *modus ponens*. (Cf. volume 2, chapter 2.) This 'quasi-empirical' approach may be applied to any kind of normative knowledge: Watkins has already applied it to ethics in his [1963] and [1967]. But now I prefer another approach: cf. p. 133, n. 4.

It may be noted that this meta-criterion does not have to be construed as psychological, or 'naturalistic' in Popper's sense. (Cf. his [1934], section 10.) The definition of the 'scientific élite' is not simply an empirical matter.

Popper [1963a], p. 38, n. 3, my italics. This, of course, is equivalent to his celebrated 'demarcation criterion' between (internal, rationally reconstructed) science and non-science (or 'metaphysics'). The latter may be (externally) 'influential' and has to be branded as pseudoscience only if it declares itself to be science.

Cf. chapter 1, pp. 16–17.

<sup>4</sup> Cf. e.g. his [1934], section 18.



forward. For it seems to offer a coherent account of *more* old, isolated basic value judgments; moreover, it has led to new and, at least for the justificationist or naive falsificationist, surprising basic value judgments. For instance, according to Popper's theory, it was irrational to retain and further elaborate Newton's gravitational theory after the discovery of Mercury's anomalous perihelion; or again, it was irrational to develop Bohr's old quantum theory based on inconsistent foundations. From my point of view these were perfectly rational developments: some rearguard actions in the defence of defeated programmes – even after the so-called 'crucial experiments' – are perfectly rational. Thus my methodology leads to the reversal of those historiographical judgments which deleted these rearguard actions both from inductivist and from falsificationist party histories.<sup>1</sup>

Indeed, this methodology confidently predicts that where the falsificationist sees the instant defeat of a theory through a simple battle with some fact, the historian will detect a complicated war of attrition, starting long before, and ending after, the alleged 'crucial experiment'; and where the falsificationist sees consistent and unrefuted theories, it predicts the existence of hordes of known anomalies in research programmes progressing on possibly inconsistent foundations.<sup>2</sup> Where the conventionalist sees the clue to the victory of a theory over its predecessor in the former's intuitive simplicity, this methodology predicts that it will be found that victory was due to empirical degeneration in the old and empirical progress in the new programme.<sup>3</sup> Where Kuhn and Feyerabend see irrational change, I predict that the historian will be able to show that there has been rational change. The methodology of research programmes thus predicts (or, if you wish, 'postdicts') novel historical facts, unexpected in the light of extant (internal and external) historiographies and these predictions will, I hope, be corroborated by historical research. If they are, then the methodology of scientific research programmes will itself constitute a progressive problemshift.

Thus progress in the theory of scientific rationality is marked by discoveries of novel historical facts, by the reconstruction of a growing bulk of value-impregnated history as rational.<sup>4</sup> In other words, the theory of scientific

<sup>1</sup> Cf. chapter 1, section 3(c).

<sup>2</sup> Cf. chapter 1, pp. 52–86.

<sup>3</sup> Duhem himself gives only one explicit example: the victory of wave optics over Newtonian optics [1906], chapter VI, §10 (also see chapter IV, §4). But where Duhem relies on intuitive 'common sense', I rely on an analysis of rival problemshifts.

<sup>4</sup> One may introduce the notion of 'degree of correctness' into the meta-theory of methodologies, which would be analogous to Popper's empirical 'normative basic statements' (like the statement that 'Planck's radiation formula is arbitrary').

Let me point out here that the methodology of research programmes may be applied not only to norm-impregnated historical knowledge but to any normative knowledge, including even ethics and aesthetics. This would then supersede the naive falsificationist 'quasi-empirical' approach as outlined in n. 3, p. 124.



'laws' proposed by the apriorist philosophers of science have turned out to be wrong in the light of the verdicts of the best scientists. Up to the present day it has been the scientific standards, as applied 'instinctively' by the scientific *élite* in particular cases, which have constituted the main – although not the exclusive – yardstick of the philosopher's *universal* laws. But if so, methodological progress, at least as far as the most advanced sciences are concerned, still lags behind common scientific wisdom. Is it not then *hubris* to try to impose some *a priori* philosophy of science on the most advanced sciences? Is it not *hubris* to demand that if, say, Newtonian or Einsteinian science turns out to have violated Bacon's, Carnap's or Popper's *a priori* rules of the game, the business of science should be started anew?

I think it is. And, indeed, the methodology of historiographical research programmes implies a pluralistic system of authority, partly because the wisdom of the scientific jury and its case law has not been, and cannot be, fully articulated by the philosopher's statute law, and partly because the philosopher's statute law may occasionally be right when the scientists' judgment fails. I disagree, therefore, both with those philosophers of science who have taken it for granted that general scientific standards are immutable and reason can recognize them *a priori*,<sup>1</sup> and with those who have thought that the light of reason illuminates only particular cases. The methodology of historiographical research programmes specifies ways both for the philosopher of science to learn from the historian of science and *vice versa*.

But this two-way traffic need not always be balanced. The statute law approach should become much more important when a tradition degenerates<sup>2</sup> or a new bad tradition is founded.<sup>3</sup> In such cases statute law may thwart the authority of the corrupted case law, and slow down or even reverse the process of degeneration.<sup>4</sup> When a scientific school degenerates into pseudoscience, it may be worthwhile to force a methodological debate in the hope that working scientists will learn more from it than philosophers (just as when ordinary language degenerates into, say, journalese, it may be worthwhile to invoke the rules of grammar).<sup>5</sup>

<sup>1</sup> Some might claim that Popper does *not* fall into this category. After all, Popper defined 'science' in such a way that it should include the refuted Newtonian theory and exclude unrefuted astrology, Marxism and Freudianism.

<sup>2</sup> This seems to be the case in modern particle physics; or according to some philosophers and physicists even in the Copenhagen school of quantum physics.

<sup>3</sup> This is the case with some of the main schools of modern sociology, psychology and social psychology.

<sup>4</sup> This, of course, explains why a good methodology – 'distilled' from the mature sciences – may play an important role for immature and, indeed, dubious disciplines. While Polanyiite academic autonomy should be defended for departments of theoretical physics, it must not be tolerated, say, in institutes for computerized social astrology, science planning or social imagistics. (For an authoritative study of the latter, cf. Priestley [1968].)

<sup>5</sup> Of course, a critical discussion of scientific standards, possibly leading even to their improvement, is impossible without articulating them in general terms: just as if one



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rival rational reconstructions for any historical change and one reconstruction is better than another if it explains more of the actual history of science; that is, rational reconstructions of history are research programmes, with a normative appraisal as hard core and psychological hypotheses (and initial conditions) in the protective belt. These historiographical research programmes are to be appraised as any other research programme for progress and degeneration. Which historiographical research programme is superior may be tested by seeing how successfully they explain scientific progress. In the case of the Copernican Revolution this talk was only programmatic: the real test will come only when the appraisal is supplemented by a full explanation.

Finally, I wish to clarify a few points arising from earlier discussions of my theory.

First, it is not the case that I *propose* a rational reconstruction of history of science *as opposed to* describing and explaining it. Rather I maintain that all historians of science *who hold that the progress of science is progress in objective knowledge*, use, willy-nilly, some rational reconstruction.

Secondly, in my particular programme of rational reconstruction (for which I now accept Zahar's important amendment), there is no 'attempt to protect [myself] from real history'.<sup>1</sup> This Kuhnian charge stems probably from a rather unsuccessful joke of mine. Some years ago I wrote that 'One way to indicate discrepancies between history and its rational reconstruction is to relate the internal history *in the text*, and indicate *in the footnotes* how actual history "misbehaved" in the light of its rational reconstruction'.<sup>2</sup> Of course, such parodies may be written, and may even be instructive; but I never said that this is the way in which history actually ought to be written and, indeed, I never wrote history in this way except for one occasion.<sup>3</sup>

Kuhn's charge that my conception of history 'is not history at all but philosophy fabricating examples', is misconceived. I hold that all histories of science are *always* philosophies fabricating examples. Philosophy of science determines to a large extent historical explanation; and Kuhn provided us with probably the most colourful one. But, equally, all physics or any kind of empirical assertion (i.e. theory) is 'philosophy fabricating examples'. Surely since Kant and Bergson this is a commonplace. But, of course, some fabrications in physics are better than others and some fabrications in history are better than others. And I offer sharp criteria using which one can compare rival fabrications both in physics *and* in its history – and I claim that my fabrications contain more truth than Kuhn's.

<sup>1</sup> Kuhn [1971], p. 143.

<sup>2</sup> Chapter 2 above, p. 120; quoted and criticized in Kuhn [1971], p. 142.

<sup>3</sup> I used this style extensively in my *Proofs and Refutations*, but there my purpose was to distill a methodological message from the history, rather than to write history itself.



# NEWTON'S EFFECT ON SCIENTIFIC STANDARDS

Royal, was a real, unschizophrenic inductivist; he slowed down Newton's and his associates' work more than anybody else had done, by refusing to let them have the results of observations he made of the Moon. At first, Newton and Flamsteed corresponded frequently, but Flamsteed soon became annoyed by Newton's use of Flamsteed's data as touchstones of his, Newton's, lunar theories, the first dozen of which ended up in Newton's wastepaper-basket.<sup>1</sup> He complained to his friend Lowthorp in 1700:

[Newton] had made lunar tables once to answer his conceived laws, but when he came to compare them with the heavens, (that is, the moon's observed places) he found he had mistook, and was forced to throw them all aside: that I had imparted above 200 of her observed places to him, which one would think would be sufficient to limit any theory by; and since he has altered and suited his theory till it fitted these observations, 'tis no wonder that it represents them: but still he is more beholden to them for it than he is to his speculations about gravity, which had misled him.<sup>2</sup>

But he does not mention to Lowthorp that some of *his observations* too ended up in the wastepaper-basket. For instance, Newton visited him on 1 September 1694 when working full time on his lunar theory and told him to reinterpret some of his data since they contradicted his theory and explained to him exactly how to do it. Flamsteed obeyed Newton and wrote to him on 7 October 1694: 'Since you went home, I examined the observations I employed for determining the greatest equations of the earth's orbit, and considering the moon's places at the times... I find that (*if, as you intimate, the earth inclines on that side the moon then is*) you may abate about 20" from it.'<sup>3</sup> Thus Newton constantly criticized and corrected Flamsteed's observational, touchstone theories. Newton taught Flamsteed for instance a better theory of the refractive power of the atmosphere which Flamsteed accepted and which corrected his original 'data'. One can understand the constant humiliation and slowly increasing fury of this great observer, having his data criticized and improved by a man, who, on his own confession, made no observations himself.<sup>4</sup>

By 1700 Newton and Flamsteed were not on corresponding terms any more, but earlier, when Newton still went to great lengths to get Flamsteed's data, he tried to explain to him patiently, that his (Newton's) 'theory will be a demonstration of their exactness... Without such a theory to recommend them, they will only be thrown into the heap of the observations of former astronomers, till somebody shall arise that, by perfecting the theory of the moon, shall discover

<sup>1</sup> 'Wastepaper-baskets' were containers used in the seventeenth century for the disposal of some first versions of manuscripts which self-criticism – or private criticism of learned friends – ruled out on the first reading. In our age of publication explosion most people have no time to read their manuscripts, and the function of wastepaper-baskets has now been taken over by scientific journals.

<sup>2</sup> Baily [1835], p. 176.

<sup>3</sup> Cf. Brewster [1855], volume 2, p. 168, my italics.

<sup>4</sup> Cf. Newton [1694].



inductive logic.<sup>1</sup> In this sense one may say that, while Newton's method created modern science, Newton's theory of method created modern philosophy of science.

Moreover, the worst part of Newton's theory of method was set up as a rulebook for the underdeveloped disciplines and especially for the social sciences. Newtonianism, preached by semiliterates, like John Stuart Mill, who never read Newton, exerted a powerful influence in keeping underdeveloped disciplines underdeveloped.<sup>2</sup>

The influence of Newtonian success reached even political thought. It created a veritable euphoria among the dogmatists: before Newton the problem was whether it is possible at all to arrive at *episteme*; after Newton the problem became *how* it was possible to arrive at *episteme*, and how one can extend it to other spheres of knowledge. Without appreciating this problemshift one cannot understand eighteenth century thought. The struggle over the recognition of Newton's celestial mechanics as *episteme* took some time; but once it was recognized, the whole intellectual climate underwent a tremendous change. Much of eighteenth century thinking was determined by two major seventeenth century events conflicting in their effect. One was the tremendous suffering and chaos created by catholic-protestant warfare. The other was Newton's discoveries. The reaction to the first was *tolerant sceptical enlightenment*: there was no way to obtain proven truth about the most essential matters, therefore everyone should have the right to his beliefs. The best known exponent of this position was Bayle. The reaction to the second was *intolerant dogmatist enlightenment*: the light of science – to be extended to all domains of human knowledge – was to dispel pre-Newtonian darkness and also the darkness of the Church.<sup>3</sup> The leader of this movement was the Newtonian Voltaire.<sup>4</sup> The influence of intolerant dogmatist enlightenment soon superseded that of its tolerant sceptical counterpart and bred the ideas of totalitarian democracy. Scientific scepticism, defeated by Newton, degenerated into Humean psychologism and joined forces with dogmatism: human reason may not give assent to Newton, but human nature must. But then the study of (unchanging, external, universal) human nature will lead us to a theory of (monolithic) 'healthy' belief.

The influence of Newtonian success was then possibly the most powerful influence on modern thought. But it is not the aim of this essay to map out the whole story; our attention is focussed on, if not

dogmatist, started from the position of 'methodological solipsism' and wanted to establish basic propositions at second-world level, in the form of Neurathian 'protocol statements': 'At 9 o'clock I saw...' It was Popper who, in 1932, persuaded him to replace second-world 'protocol-statements' by third-world 'basic statements'.

<sup>1</sup> For the degenerating problemshifts in inductive logic cf. volume 2, chapter 8.

<sup>2</sup> One is tempted to say that Newton created *two cultures*; one which developed his method, another which 'developed' his methodology.

<sup>3</sup> The discovery of distant lands – a third important factor – worked both ways.

<sup>4</sup> This analysis, if correct, makes nonsense of the Marxist approach to the history of the eighteenth century.