IMRE LAKATOS

Edited by John Worrall and Gregory Currie

The methodology of scientific research programmes

Philosophical Papers Volume 1

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Contents

Editors' introduction

When Imre Lakatos died in 1974, many friends and colleagues expressed the hope that his unpublished papers would be made available. Some were also interested in seeing his contributions to journals and conference proceedings collected together in a book. At the request of the managing committee of the Imre Lakatos Appeal Fund we have prepared two volumes of selected papers which we hope will meet these demands.

None of the papers published here for the first time was regarded by Lakatos as entirely satisfactory. Some are early drafts, while others seem not to have been intended for publication. We have pursued a fairly liberal policy, including papers which, at least in their present form, Lakatos would not have allowed to go to print. As for previously published papers, we have included them all except for the two papers, 'The Role of Crucial Experiments in Science' and 'Criticism and the Methodology of Scientific Research Programmes', which would have introduced undue repetition, and except for *Proofs and Refutations*, which recently appeared in book form.

Volume 1 is a collection of Lakatos's best known articles developing the methodology of scientific research programmes, together with a hitherto unpublished essay on the effect of Newton's scientific achievement, and a new 'Postscript' to the already published paper on the Copernican Revolution.

Although Lakatos perhaps came to be better known for his work in the philosophy of the physical sciences, he regarded himself as primarily a philosopher of mathematics. Volume 2 contains papers on the philosophy of mathematics, as well as some critical essays on contemporary philosophers, and some short polemical pieces reflecting his concern with political and educational matters, which, among other things, give an impression of his forceful personality.

Information about the history of the material published here is included as introductory footnotes to each paper. These and other editorial footnotes are indicated by asterisks. (We have tried to minimise these editorial footnotes particularly in the case of previously published papers.)

Offprints of some of the published papers found in Lakatos's library contained handwritten corrections and we have incorporated these wherever possible. In preparing the previously unpublished papers for the press, we have taken the liberty of introducing some presentational alterations where the original text was incomplete, or seemed likely to be misleading, or where minor alterations seemed to produce major increases in readability. We felt justified in making these changes because Lakatos always took great care over the presentation of any of his material which was to be published and, prior to publication, he always had such material widely circulated among colleagues and friends for criticism and suggested improvements. These newly published papers would undoubtedly have undergone this treatment and the resulting changes have been much more far reaching than those we have dared to introduce. Wherever the device of enclosing our alterations within square brackets worked easily and smoothly we have adopted it. (However, square brackets within quotations from other authors enclose Lakatos's own insertions.)

Where Lakatos mentioned a paper reprinted in either of the present volumes, we have altered the style of reference. So, for example, 'Lakatos [1970*a*]' becomes 'this volume, chapter 1', and 'Lakatos [1968*b*]' becomes 'volume 2, chapter 8'.

Chapter 3 ('Popper on demarcation and induction') is reprinted by kind permission of Professor P. A. Schillp and the Open Court publishing company; chapter 4 ('Why did Copernicus's research programme supersede Ptolemy's?') is reprinted by kind permission of Professor Robert Westman and the Regents of California University Press.

A generous grant from the *Fritz Thyssen Stiftung* made possible the creation of an archive of Lakatos's papers – an essential preliminary to the publication of these volumes. We should like to thank Nicholas Krasso and Professors Kilmister and Yourgrau for helping us to supply some missing references, and Alex Bellamy and Allison Quick for compiling the indexes. We should also like to thank Sandra Mitchell for her help, especially for her research work in connection with chapter 5 of this volume. Several of our editorial problems were resolved during valuable discussions with John Watkins. We are especially grateful to Gillian Page for her kind cooperation in making Lakatos's papers available to us and for her consistently helpful advice.

The editing of these two volumes has been in many ways a sad and frustrating experience. 'If only we could talk this over with Imre', was a thought which often recurred. Nevertheless, as people whose own ideas were fundamentally affected by the force of his intellect and personality, we are very happy to have been involved in making Lakatos's work more widely available.

J.W.
G.C.

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:

* This paper was written in early 1973 and was originally delivered as a radio lecture. It was broadcast by the Open University on 30 June 1973. (*Eds.*)

If we take in our hand any volume; of divinity, or school metaphysics, for instance; let us ask, does it contain any abstract reasoning concerning quantity or number? No. Does it contain any experimental reasoning concerning matter of fact and existence? No. Commit it then to the flames. For it can contain nothing but sophistry and illusion.

But what is 'experimental' reasoning? If we look at the vast seventeenth-century literature on witchcraft, it is full of reports of careful observations and sworn evidence – even of experiments. Glanvill, the house philosopher of the early Royal Society, regarded witchcraft as the paradigm of experimental reasoning. We have to define experimental reasoning before we start Humean book burning.

In scientific reasoning, theories are confronted with facts; and one of the central conditions of scientific reasoning is that theories must be supported by facts. Now how exactly can facts support theory?

Several different answers have been proposed. Newton himself thought that he proved his laws from facts. He was proud of not uttering mere hypotheses: he only published theories proven from facts. In particular, he claimed that he deduced his laws from the 'phenomena' provided by Kepler. But his boast was nonsense, since according to Kepler, planets move in ellipses, but according to Newton's theory, planets would move in ellipses only if the planets did not disturb each other in their motion. But they do. This is why Newton had to devise a perturbation theory from which it follows that no planet moves in an ellipse.

One can today easily demonstrate that there can be no valid derivation of a law of nature from any finite number of facts; but we still keep reading about scientific theories being proved from facts. Why this stubborn resistance to elementary logic?

There is a very plausible explanation. Scientists want to make their theories respectable, deserving of the title 'science', that is, genuine knowledge. Now the most relevant knowledge in the seventeenth century, when science was born, concerned God, the Devil, Heaven and Hell. If one got one's conjectures about matters of divinity wrong, the consequence of one's mistake was eternal damnation. Theological knowledge cannot be fallible: it must be beyond doubt. Now the Enlightenment thought that we were fallible and ignorant about matters theological. There is no scientific theology and, therefore, no theological knowledge. Knowledge can only be about Nature, but this new type of knowledge had to be judged by the standards they took over straight from theology: it had to be proven beyond doubt. Science had to achieve the very certainty which had escaped theology. A scientist, worthy of the name, was not allowed to guess: he had to prove each sentence he uttered from facts. This was the criterion of scientific honesty. Theories unproven from facts were regarded as sinful pseudoscience, heresy in the scientific community.

It was only the downfall of Newtonian theory in this century which made scientists realize that their standards of honesty had been utopian. Before Einstein most scientists thought that Newton had deciphered God's ultimate laws by proving them from the facts. Ampère, in the early nineteenth century, felt he had to call his book on his speculations concerning electromagnetism: *Mathematical Theory of Electrodynamic Phenomena Unequivocally Deduced from Experiment.* But at the end of the volume he casually confesses that some of the experiments were never performed and even that the necessary instruments had not been constructed!

If all scientific theories are equally unprovable, what distinguishes scientific knowledge from ignorance, science from pseudoscience?

One answer to this question was provided in the twentieth century by 'inductive logicians'. Inductive logic set out to define the probabilities of different theories according to the available total evidence. If the mathematical probability of a theory is high, it qualifies as scientific; if it is low or even zero, it is not scientific. Thus the hallmark of scientific honesty would be never to say anything that is not at least highly probable. Probabilism has an attractive feature: instead of simply providing a black-and-white distinction between science and pseudoscience, it provides a continuous scale from poor theories with low probability to good theories with high probability. But, in 1934, Karl Popper, one of the most influential philosophers of our time, argued that the mathematical probability of all theories, scientific or pseudoscientific, given *any* amount of evidence is zero. If Popper is right, scientific theories are not only equally unprovable but also equally improbable. A new demarcation criterion was needed and Popper proposed a rather stunning one. A theory may be scientific even if there is not a shred of evidence in its favour, and it may be pseudoscientific even if all the available evidence is in its favour. That is, the scientific or non-scientific character of a theory can be determined independently of the facts. A theory is 'scientific' if one is prepared to specify in advance a crucial experiment (or observation) which can falsify it, and it is pseudoscientific if one refuses to specify such a 'potential falsifier'. But if so, we do not demarcate scientific theories from pseudoscientific ones, but rather scientific method from non-scientific method. Marxism, for a Popperian, is scientific if the Marxists are prepared to specify facts which, if observed, make them give up Marxism. If they refuse to do so, Marxism becomes a pseudoscience. It is always interesting to ask a Marxist, what conceivable event would make him abandon his Marxism. If he is committed to Marxism, he is bound to find it immoral to specify a state of affairs which can falsify it. Thus a proposition may petrify into pseudoscientific dogma or become genuine knowledge, depending on whether we are prepared to state observable conditions which would refute it.

Is, then, Popper's falsifiability criterion the solution to the problem of demarcating science from pseudoscience? No. For Popper's criterion 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, 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 predicted that socialist societies would be free of revolutions. It predicted that there will be no conflict of interests between socialist countries. Thus the early predictions of Marxism were bold and stunning but they failed. Marxists explained all their failures: they explained the rising living standards of the working class by devising a theory of imperialism; they even explained why the first socialist revolution occurred in industrially backward Russia. They 'explained' Berlin 1953, Budapest, 1956, Prague 1968. They 'explained' the Russian–Chinese conflict. But their auxiliary hypotheses were all cooked up after the event to protect Marxian theory from the facts. The Newtonian programme led to novel facts; the Marxian lagged behind the facts and has been running fast to catch up with them.

To sum up. The hallmark of empirical progress is not trivial verifications: Popper is right that there are millions of them. It is no success for Newtonian theory that stones, when dropped, fall towards the earth, no matter how often this is repeated. But so-called 'refutations' are not the hallmark of empirical failure, as Popper has preached, since all programmes grow in a permanent ocean of anomalies. What really count are dramatic, unexpected, stunning predictions: a few of them are enough to tilt the balance; where theory lags behind the facts, we are dealing with miserable degenerating research programmes.

Now, how do scientific revolutions come about? If we have two rival research programmes, and one is progressing while the other is degenerating, scientists tend to join the progressive programme. This is the rationale of scientific revolutions. But while it is a matter of intellectual honesty to keep the record public, it is not dishonest to stick to a degenerating programme and try to turn it into a progressive one.

As opposed to Popper the methodology of scientific research programmes does not offer instant rationality. One must treat budding programmes leniently: programmes may take decades before they get off the ground and become empirically progressive. Criticism is not a Popperian quick kill, by refutation. Important criticism is always constructive: there is no refutation without a better theory. Kuhn is wrong in thinking that scientific revolutions are sudden, irrational changes in vision. The history of science refutes both Popper and Kuhn: on close inspection both Popperian crucial experiments and Kuhnian revolutions turn out to be myths: what normally happens is that progressive research programmes replace degenerating ones.

The problem of demarcation between science and pseudoscience has grave implications also for the institutionalization of criticism. Copernicus's theory was banned by the Catholic Church in 1616 because it was said to be pseudoscientific. It was taken off the index in 1820 because by that time the Church deemed that facts had proved it and therefore it became scientific. The Central Committee of the Soviet Communist Party in 1949 declared Mendelian genetics pseudoscientific and had its advocates, like Academician Vavilov, killed in concentration camps; after Vavilov's murder Mendelian genetics was rehabilitated; but the Party's right to decide what is science and publishable and what is pseudoscience and punishable was upheld. The new liberal Establishment of the West also exercises the right to deny freedom of speech to what it regards as pseudoscience, as we have seen in the case of the debate concerning race and intelligence. All these judgments were inevitably based on some sort of demarcation criterion. This is why the problem of demarcation between science and pseudoscience is not a pseudo-problem of armchair philosophers: it has grave ethical and political implications.

I

Falsification and the Methodology of Scientific Research Programmes*

1 SCIENCE: REASON OR RELIGION

For centuries knowledge meant proven knowledge – proven either by the power of the intellect or by the evidence of the senses. Wisdom and intellectual integrity demanded that one must desist from unproven utterances and minimize, even in thought, the gap between speculation and established knowledge. The proving power of the intellect or the senses was questioned by the sceptics more than two thousand years ago; but they were browbeaten into confusion by the glory of Newtonian physics. Einstein's results again turned the tables and now very few philosophers or scientists still think that scientific knowledge is, or can be, proven knowledge. But few realize that with this the whole classical structure of intellectual values falls in ruins and has to be replaced: one cannot simply water down the ideal of proven truth – as some logical empiricists do – to the ideal of 'probable truth'[1] or – as some sociologists of knowledge do – to 'truth by [changing] consensus'.[2]

Popper's distinction lies primarily in his having grasped the full implications of the collapse of the best-corroborated scientific theory of all times: Newtonian mechanics and the Newtonian theory of gravitation. In his view virtue lies not in caution in avoiding errors, but in ruthlessness in eliminating them. Boldness in conjectures on the one hand and austerity in refutations on the other: this is Popper's recipe. Intellectual honesty does not consist in trying to entrench

* This paper was written in 1968–9 and was first published as Lakatos [1970]. There Lakatos referred to the paper as an 'improved version' of his [1968*b*] and a 'crude version' of his 'forthcoming' *The Changing Logic of Scientific Discovery*, a projected book which he was never able to start. He makes the following acknowledgments: 'Some parts of [my [1968*b*]] are here reproduced without change with the permission of the Editor of the *Proceedings of the Aristotelian Society*. In the preparation of the new version I received much help from Tad Beckman, Colin Howson, Clive Kilmister, Larry Laudan, Eliot Leader, Alan Musgrave, Michael Sukale, John Watkins and John Worrall.' (*Eds.*)

1 The main contemporary proponent of the ideal of 'probable truth' is Rudolf Carnap. For the historical background and a criticism of this position, cf. volume 2, chapter 8.

2 The main contemporary proponents of the ideal of 'truth by consensus' are Polanyi and Kuhn. For the historical background and a criticism of this position, cf. Musgrave [1969*a*] and Musgrave [1969*b*].

or establish one's position by proving (or 'probabilifying') it – intellectual honesty consists rather in specifying precisely the conditions under which one is willing to give up one's position. Committed Marxists and Freudians refuse to specify such conditions: this is the hallmark of their intellectual dishonesty. *Belief* may be a regrettably unavoidable biological weakness to be kept under the control of criticism: but *commitment* is for Popper an outright crime.

Kuhn thinks otherwise. He too rejects the idea that science grows by accumulation of eternal truths.[1] He too takes his main inspiration from Einstein's overthrow of Newtonian physics. His main problem too is *scientific revolution.* But while according to Popper science is 'revolution in permanence', and criticism the heart of the scientific enterprise, according to Kuhn revolution is exceptional and, indeed, extra-scientific, and criticism is, in 'normal' times, anathema. Indeed for Kuhn the transition from criticism to commitment marks the point where progress – and 'normal' science – begins. For him the idea that on 'refutation' one can demand the rejection, the elimination of a theory, is 'naive' falsificationism. Criticism of the dominant theory and proposals of new theories are only allowed in the rare moments of 'crisis'. This last Kuhnian thesis has been widely criticized[2] and I shall not discuss it. My concern is rather that Kuhn, having recognized the failure both of justificationism and falsificationism in providing rational accounts of scientific growth, seems now to fall back on irrationalism.

For Popper scientific change is rational or at least rationally reconstructible and falls in the realm of the *logic of discovery*. For Kuhn scientific change – from one 'paradigm' to another – is a mystical conversion which is not and cannot be governed by rules of reason and which falls totally within the realm of the *(social) psychology of discovery*. Scientific change is a kind of religious change.

The clash between Popper and Kuhn is not about a mere technical point in epistemology. It concerns our central intellectual values, and has implications not only for theoretical physics but also for the underdeveloped social sciences and even for moral and political philosophy. If even in science there is no other way of judging a theory but by assessing the number, faith and vocal energy of its supporters,

[1] Indeed he introduces his [1962] by arguing against the 'development-by-accumulation' idea of scientific growth. But his intellectual debt is to Koyré rather than to Popper. Koyré showed that positivism gives bad guidance to the historian of science, for the history of physics can only be understood in the context of a succession of 'metaphysical' research programmes. Thus scientific changes are connected with vast cataclysmic metaphysical revolutions. Kuhn develops this message of Burtt and Koyré and the vast success of his book was partly due to his hard-hitting, direct criticism of justificationist historiography – which created a sensation among ordinary scientists and historians of science whom Burtt's, Koyré's (or Popper's) message had not yet reached. But, unfortunately, his message had some authoritarian and irrationalist overtones.

[2] Cf. e.g. Watkins [1970] and Feyerabend [1970*a*].

then this must be even more so in the social sciences: truth lies in power. Thus Kuhn's position vindicates, no doubt, unintentionally, the basic political *credo* of contemporary religious maniacs ('student revolutionaries').

In this paper I shall first show that in Popper's logic of scientific discovery two different positions are conflated. Kuhn understands only one of these, 'naive falsificationism' (I prefer the term 'naive methodological falsificationism'); I think that his criticism of it is correct, and I shall even strengthen it. But Kuhn does not understand a more sophisticated position the rationality of which is not based on 'naive' falsificationism. I shall try to explain – and further strengthen – this stronger Popperian position which, I think, may escape Kuhn's strictures and present scientific revolutions not as constituting religious conversions but rather as rational progress.

2 FALLIBILISM VERSUS FALSIFICATIONISM

To see the conflicting theses more clearly, we have to reconstruct the situation as it was in philosophy of science after the breakdown of 'justificationism'.

According to the 'justificationists' scientific knowledge consisted of proven propositions. Having recognized that strictly logical deductions enable us only to infer (transmit truth) but not to prove (establish truth), they disagreed about the nature of those propositions (axioms) whose truth can be proved by extralogical means. *Classical intellectualists* (or 'rationalists' in the narrow sense of the term) admitted very varied – and powerful – sorts of extralogical 'proofs' by revelation, intellectual intuition, experience. These, with the help of logic, enabled them to prove every sort of scientific proposition. *Classical empiricists* accepted as axioms only a relatively small set of 'factual propositions' which expressed the 'hard facts'. Their truth-value was established by experience and they constituted the *empirical basis* of science. In order to prove scientific *theories* from nothing else but the narrow empirical basis, they needed a logic much more powerful than the deductive logic of the classical intellectualists: '*inductive logic*'. All justificationists, whether intellectualists or empiricists, agreed that a singular statement expressing a 'hard fact' may *disprove* a universal theory;[1] but few of them thought that a finite conjunction of factual

[1] Justificationists repeatedly stressed this asymmetry between singular factual statements and universal theories. Cf. e.g. Popkin's discussion of Pascal in Popkin [1968], p. 14 and Kant's statement to the same effect as quoted in the new *motto* of the third 1969 German edition of Popper's *Logik der Forschung*. (Popper's choice of this time-honoured cornerstone of elementary logic as a *motto* of the new edition of his classic shows his main concern: to fight *probabilism*, in which this asymmetry becomes irrelevant; for probabilists theories may become almost as well established as factual propositions.)

propositions might be sufficient to *prove* 'inductively' a universal theory.[1]

Justificationism, that is, the identification of knowledge with proven knowledge, was the dominant tradition in rational thought throughout the ages. Scepticism did not deny justificatonism: it only claimed that there was (and could be) no proven knowledge and *therefore* no knowledge whatsoever. For the sceptics 'knowledge' was nothing but animal belief. Thus justificationist scepticism ridiculed objective thought and opened the door to irrationalism, mysticism, superstition.

This situation explains the enormous effort invested by classical rationalists in trying to save the synthetic *a priori* principles of intellectualism and by classical empiricists in trying to save the certainty of an empirical basis and the validity of inductive inference. For all of them *scientific honesty demanded that one assert nothing that is unproven.* However, both were defeated: Kantians by non-Euclidean geometry and by non-Newtonian physics, and empiricists by the logical impossibility of establishing an empirical basis (as Kantians pointed out, facts cannot prove propositions) and of establishing an inductive logic (no logic can infallibly increase content). It turned out that *all theories are equally unprovable.*

Philosophers were slow to recognize this, for obvious reasons: classical justificationists feared that once they conceded that theoretical science is unprovable, they would have also to conclude that it is sophistry and illusion, a dishonest fraud. The philosophical importance of *probabilism* (or '*neojustificationism*') lies in the denial that such a conclusion is necessary.

Probabilism was elaborated by a group of Cambridge philosophers who thought that although scientific theories are equally unprovable, they have different degrees of probability (in the sense of the calculus of probability) relative to the available empirical evidence.[2] *Scientific honesty then requires less than had been thought: it consists in uttering only highly probable theories; or even in merely specifying, for each scientific theory, the evidence, and the probability of the theory in the light of this evidence.*

Of course, replacing proof by probability was a major retreat for justificationist thought. But even this retreat turned out to be insufficient. It was soon shown, mainly by Popper's persistent efforts, that under very general conditions all theories have zero probability, whatever the evidence; *all theories are not only equally unprovable but also equally improbable.*[3]

[1] Indeed, even some of these few shifted, following Mill, the rather obviously insoluble problem of inductive proof (of universal from particular propositions) to the slightly less obviously insoluble problem of proving *particular* factual propositions from other *particular* factual propositions.

[2] The founding fathers of probabilism were intellectualists; Carnap's later efforts to build up an empiricist brand of probabilism failed. Cf. volume 2, chapter 8, p. 164 and also p. 160, n. 2.

[3] For a detailed discussion, cf. volume 2, chapter 8, especially pp. 154 ff.

Many philosophers still argue that the failure to obtain at least a probabilistic solution of the problem of induction means that we 'throw over almost everything that is regarded as knowledge by science and common sense.'[1] It is against this background that one must appreciate the dramatic change brought about by falsificationism in evaluating theories, and in general, in the standards of intellectual honesty. Falsificationism was, in a sense, a new and considerable retreat for rational thought. But since it was a retreat from utopian standards, it cleared away much hypocrisy and muddled thought, and thus, in fact, it represented an advance.

(a) *Dogmatic (or naturalistic) falsificationism. The empirical basis*

First I shall discuss a most important brand of falsificationism: dogmatic (or 'naturalistic')[2] falsificationism. Dogmatic falsificationism admits the fallibility of *all* scientific theories without qualification, but it retains a sort of infallible empirical basis. It is strictly empiricist without being inductivist: it denies that the certainty of the empirical basis can be transmitted to theories. *Thus dogmatic falsificationism is the weakest brand of justificationism.*

It is extremely important to stress that admitting (fortified) empirical counterevidence as a final arbiter against a theory does not make one a dogmatic falsificationist. Any Kantian or inductivist will agree to such arbitration. But both the Kantian and the inductivist, while bowing to a negative crucial experiment, will also specify conditions of how to establish, entrench one unrefuted theory more than another. Kantians held that Euclidean geometry and Newtonian mechanics were established with certainty; inductivists held they had probability 1. For the dogmatic falsificationist, however, empirical *counter*evidence is the *one and only* arbiter which may judge a theory.

The hallmark of dogmatic falsificationism is then the recognition that all theories are equally conjectural. Science cannot *prove* any theory. But although science cannot *prove*, it can *disprove*: it 'can perform with complete logical certainty [the act of] repudiation of what is false',[3] that is, there is an absolutely firm empirical basis of facts which can be used to disprove theories. Falsificationists provide new – very modest – standards of scientific honesty: they are willing to regard a proposition as 'scientific' not only if it is a proven factual proposition, but even if it is nothing more than a falsifiable one, that is, if there are experimental and mathematical techniques avail-

[1] Russell [1943], p. 683. For a discussion of Russell's justificationism, cf. volume 2, chapter 1, especially pp. 11 ff.

[2] For the explanation of this term, cf. *below*, p. 14, n. 2.

[3] Medawar [1967], p. 144. Also cf. *below*, p. 93, n. 2.

able at the time which designate certain statements as potential falsifiers.[1]

Scientific honesty then consists of specifying, in advance, an experiment such that if the result contradicts the theory, the theory has to be given up.[2] The falsificationist demands that once a proposition is disproved, there must be no prevarication: the proposition must be unconditionally rejected. To (non-tautologous) unfalsifiable propositions the dogmatic falsificationist gives short shrift; he brands them 'metaphysical' and denies them scientific standing.

Dogmatic falsificationists draw a sharp demarcation between the theoretician and the experimenter: the theoretician proposes, the experimenter – in the name of Nature – disposes. As Weyl put it: 'I wish to record my unbounded admiration for the work of the experimenter in his struggle to wrest interpretable facts from an unyielding Nature who knows so well how to meet our theories with a decisive *No* – or with an inaudible *Yes*.'[3] Braithwaite gives a particularly lucid exposition of dogmatic falsificationism. He raises the problem of the objectivity of science: 'To what extent, then, should an established scientific deductive system be regarded as a free creation of the human mind, and to what extent should it be regarded as giving an objective account of the facts of nature?' His answer is:

> The form of a statement of a scientific hypothesis and its use to express a general proposition, is a human device; what is due to Nature are the observable facts which refute or fail to refute the scientific hypothesis...[In science] we hand over to Nature the task of deciding whether any of the contingent lowest-level conclusions are false. This objective test of falsity it is which makes the deductive system, in whose construction we have very great freedom, a deductive system of scientific hypotheses. Man proposes a system of hypotheses: Nature disposes of its truth or falsity. Man invents a scientific system, and then discovers whether or not it accords with observed fact.[4]

According to the logic of dogmatic falsificationism, science grows by repeated overthrow of theories with the help of hard facts. For instance, according to this view, Descartes's vortex theory of gravity was refuted – and eliminated – by the *fact* that planets moved in ellipses rather than in

[1] This discussion already indicates the vital importance of a demarcation between provable factual and unprovable theoretical propositions for the dogmatic falsificationist.

[2] '*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' (Popper [1963*a*], p. 38, n. 3).

[3] Quoted in Popper [1934], section 85, with Popper's comment: 'I fully agree.'

[4] Braithwaite [1953], pp. 367–8. For the 'incorrigibility' of Braithwaite's observed facts, cf. his [1938]. While in the quoted passage Braithwaite gives a forceful answer to the problem of scientific objectivity, in another passage he points out that 'except for the straightforward generalizations of observable facts...complete refutation is no more possible than is complete proof' ([1953], p. 19). Also cf. *below*, p. 29, n. 3.

Cartesian circles; Newton's theory, however, explained successfully the then available facts, both those which had been explained by Descartes's theory and those which refuted it. Therefore Newton's theory replaced Descartes's theory. Analogously, as seen by falsificationists, Newton's theory was, in turn, refuted – proved false – by the anomalous perihelion of Mercury, while Einstein's explained that too. Thus science proceeds by bold speculations, which are never proved or even made probable, but some of which are later eliminated by hard, conclusive refutations and then replaced by still bolder, new and, at least at the start, unrefuted speculations.

Dogmatic falsificationism, however, is untenable. It rests on two false assumptions and on a too narrow criterion of demarcation between scientific and non-scientific.

The *first assumption* is that there is a natural, *psychological* borderline between theoretical or speculative propositions on the one hand and factual or observational (or basic) propositions on the other. (This, of course, is part of the '*naturalistic approach*' to scientific method.[1])

The *second assumption* is that if a proposition satisfies the psychological criterion of being factual or observational (or basic) then it is true; one may say that it was *proved* from facts. (I shall call this the *doctrine of observational (or experimental) proof.*[2])

These two assumptions secure for the dogmatic falsificationist's deadly disproofs an empirical basis from which proven falsehood can be carried by deductive logic to the theory under test.

These assumptions are complemented by a *demarcation criterion*: only those theories are 'scientific' which forbid certain observable states of affairs and therefore are factually disprovable. *Or, a theory is 'scientific' if it has an empirical basis.*[3]

But both assumptions are false. Psychology testifies against the first, logic against the second, and, finally, methodological judgment testifies against the demarcation criterion. I shall discuss them in turn.

(1) A first glance at a few characteristic examples already undermines the *first assumption*. Galileo claimed that he could 'observe' mountains on the moon and spots on the sun and that these 'observations' refuted the time-honoured theory that celestial bodies are faultless crystal balls. But his 'observations' were not 'observational'

[1] Cf. Popper [1934], section 10.

[2] For these assumptions and their criticism, cf. Popper [1934], sections 4 and 10. It is because of this assumption that – following Popper – I call this brand of falsificationism 'naturalistic'. Popper's 'basic propositions' should not be confused with the basic propositions discussed in this section; cf. *below*, p. 22, n. 6.

It is important to point out that these two assumptions are also shared by many justificationists who are not falsificationists: they may add to experimental proofs 'intuitive proofs' – as did Kant – or 'inductive proofs – as did Mill. Our falsificationist accepts experimental proofs *only*.

[3] The empirical basis of a theory is the set of its potential falsifiers: the set of those observational propositions which may disprove it.

in the sense of being observed by the – unaided – senses: their reliability depended on the reliability of his telescope – and of the optical theory of the telescope – which was violently questioned by his contemporaries. It was not Galileo's – pure, untheoretical – *observations* that confronted Aristotelian *theory* but rather Galileo's 'observations' in the light of his optical theory that confronted the Aristotelians' 'observations' in the light of their theory of the heavens.[1] This leaves us with two inconsistent theories, *prima facie* on a par. Some empiricists may concede this point and agree that Galileo's 'observations' were not genuine observations; but they still hold that there is a 'natural demarcation' between statements impressed on an empty and passive mind directly by the senses – only these constitute genuine 'immediate knowledge' – and between statements which are suggested by impure, theory-impregnated sensations. Indeed, *all* brands of justificationist theories of knowledge which acknowledge the senses as a source (whether as *one* source or as *the* source) of knowledge are bound to contain a *psychology of observation*. Such psychologies specify the 'right', 'normal', 'healthy', 'unbiased', 'careful' or 'scientific' state of the senses – or rather the state of mind as a whole – in which they observe truth as it is. For instance, Aristotle – and the Stoics – thought that the right mind was the medically healthy mind. Modern thinkers recognized that there is more to the right mind than simple 'health'. Descartes's right mind is one steeled in the fire of sceptical doubt which leaves nothing but the final loneliness of the *cogito* in which the *ego* can then be re-established and God's guiding hand found to recognize truth. All schools of modern justificationism can be characterized by the particular *psychotherapy* by which they propose to prepare the mind to receive the grace of proven truth in the course of a mystical communion. In particular, for classical empiricists the right mind is a *tabula rasa*, emptied of all original content, freed from all prejudice of theory. But it transpires from the work of Kant and Popper – and from the work of psychologists influenced by them – that such empiricist psychotherapy can never succeed. For there are and can be no sensations unimpregnated by expectation and therefore *there is no natural (i.e. psychological) demarcation between observational and theoretical propositions.*[2]

(2) But even if there was such a natural demarcation, logic would still destroy the *second assumption* of dogmatic falsificationism. For the

[1] Incidentally, Galileo also showed – with the help of his optics – that if the moon was a faultless crystal ball, it would be invisible (Galileo [1632]).

[2] True, most psychologists who turned against the idea of justificationist sensationalism did so under the influence of pragmatist philosophers like William James who denied the possibility of any sort of objective knowledge. But, even so, Kant's influence through Oswald Külpe, Franz Brentano and Popper's influence through Egon Brunswick and Donald Campbell played a role in the shaping of modern psychology; and if psychology ever vanquishes psychologism, it will be due to an increased understanding of the Kant–Popper mainline of objectivist philosophy.

truth-value of the 'observational' propositions cannot be indubitably decided: *no factual proposition can ever be proved from an experiment.* Propositions can only be derived from other propositions, they cannot be derived from facts: one cannot prove statements from experiences – 'no more than by thumping the table'.[1] This is one of the basic points of elementary logic, but one which is understood by relatively few people even today.[2]

If factual propositions are unprovable then they are fallible. If they are fallible then clashes between theories and factual propositions are not 'falsifications' but merely inconsistencies. Our imagination may play a greater role in the formulation of 'theories' than in the formulation of 'factual propositions',[3] but they are both fallible. Thus *we cannot prove theories and we cannot disprove them either.*[4] The demarcation between the soft, unproven 'theories' and the hard, proven 'empirical basis' is non-existent: *all* propositions of science are theoretical and, incurably, fallible.[5]

(3) Finally, even if there were a natural demarcation between observation statements and theories, and even if the truth-value of observation statements could be indubitably established, dogmatic falsificationism would still be useless for eliminating the most important class of what are commonly regarded as scientific theories. For even if experiments *could* prove experimental reports, their disproving power would still be miserably restricted: *exactly the most admired scientific theories simply fail to forbid any observable state of affairs.*

To support this last contention, I shall first tell a characteristic story and then propose a general argument.

The story is about an imaginary case of planetary misbehaviour. A physicist of the pre-Einsteinian era takes Newton's mechanics and his law of gravitation, (N), the accepted initial conditions, I, and calculates, with their help, the path of a newly discovered small planet, p. But the planet deviates from the calculated path. Does our Newtonian

[1] Cf. Popper [1934], section 29.

[2] It seems that the first philosopher to emphasize this was Fries in 1837 (cf. Popper [1934], section 29, n. 3). This is of course a special case of the general thesis that logical relations, like logical probability or consistency, refer to *propositions.* Thus, for instance, the proposition 'nature is consistent' is false (or, if you wish, meaningless), for nature is not a proposition (or a conjunction of propositions).

[3] Incidentally, even this is questionable. Cf. *below*, p. 42 ff.

[4] As Popper put it: 'No conclusive disproof of a theory can ever be produced'; those who wait for an infallible disproof before eliminating a theory will have to wait for ever and 'will never benefit from experience' ([1934], section 9).

[5] Both Kant and his English follower, Whewell, realized that all scientific propositions, whether *a priori* or *a posteriori*, are equally theoretical; but both held that they are equally provable. Kantians saw clearly that the propositions of science are theoretical in the sense that they are not written by sensations on the *tabula rasa* of an empty mind, nor deduced or induced from such propositions. A factual proposition is only a special kind of theoretical proposition. In this Popper sided with Kant against the empiricist version of dogmatism. But Popper went a step further: in his view the propositions of science are not only theoretical but they are all also *fallible*, conjectural for ever.

physicist consider that the deviation was forbidden by Newton's theory and therefore that, once established, it refutes the theory N? No. He suggests that there must be a hitherto unknown planet p' which perturbs the path of p. He calculates the mass orbit, etc., of this hypothetical planet and then asks an experimental astronomer to test his hypothesis. The planet p' is so small that even the biggest available telescopes cannot possibly observe it: the experimental astronomer applies for a research grant to build yet a bigger one.[1] In three years' time the new telescope is ready. Were the unknown planet p' to be discovered, it would be hailed as a new victory of Newtonian science. But it is not. Does our scientist abandon Newton's theory and his idea of the perturbing planet? No. He suggests that a cloud of cosmic dust hides the planet from us. He calculates the location and properties of this cloud and asks for a research grant to send up a satellite to test his calculations. Were the satellite's instruments (possibly new ones, based on a little-tested theory) to record the existence of the conjectural cloud, the result would be hailed as an outstanding victory for Newtonian science. But the cloud is not found. Does our scientist abandon Newton's theory, together with the idea of the perturbing planet and the idea of the cloud which hides it? No. He suggests that there is some magnetic field in that region of the universe which disturbed the instruments of the satellite. A new satellite is sent up. Were the magnetic field to be found, Newtonians would celebrate a sensational victory. But it is not. Is this regarded as a refutation of Newtonian science? No. Either yet another ingenious auxiliary hypothesis is proposed or...the whole story is buried in the dusty volumes of periodicals and the story never mentioned again.[2]

This story strongly suggests that even a most respected scientific theory, like Newton's dynamics and theory of gravitation, may fail to forbid any observable state of affairs.[3] Indeed, *some scientific theories forbid an event occurring in some specified finite spatio-temporal region* (*or briefly, a 'singular event'*) *only on the condition that no other factor* (possibly hidden in some distant and unspecified spatio-temporal corner of the universe) *has any influence on it.* But then *such theories never alone contradict a 'basic' statement:* they contradict at most a conjunction of

[1] If the tiny conjectural planet were out of the reach even of the biggest *possible* optical telescopes, he might try some quite novel instrument (like a radiotelescope) in order to enable him to 'observe it', that is, to ask Nature about it, even if only indirectly. (The new 'observational' theory may itself not be properly articulated, let alone severely tested, but he would care no more than Galileo did.)

[2] At least not until a new research programme supersedes Newton's programme which happens to explain this previously recalcitrant phenomenon. In this case, the phenomenon will be unearthed and enthroned as a 'crucial experiment'; cf. *below*, p. 68 ff.

[3] Popper asks: 'What kind of clinical responses would refute to the satisfaction of the analyst not merely a particular diagnosis but psychoanalysis itself?' ([1963], p. 38, n. 3.) But what kind of observation would refute to the satisfaction of the Newtonian not merely a particular version but Newtonian theory itself?

a basic statement describing a spatio-temporally singular event and of a universal non-existence statement saying that no other relevant cause is at work anywhere in the universe. And the dogmatic falsificationist cannot possibly claim that such universal non-existence statements belong to the empirical basis: that they can be observed and proved by experience.

Another way of putting this is to say that some scientific theories are normally interpreted as containing a *ceteris paribus* clause:[1] in such cases it is always a specific theory *together* with this clause which may be refuted. But such a refutation is inconsequential for the *specific* theory under test because by replacing the *ceteris paribus* clause by a different one the *specific* theory can always be retained whatever the tests say.

If so, the 'inexorable' disproof procedure of dogmatic falsificationism breaks down in these cases *even if* there were a firmly established empirical basis to serve as a launching pad for the arrow of the *modus tollens*: the prime target remains hopelessly elusive.[2] And as it happens, it is exactly the most important, 'mature' theories in the history of science which are *prima facie* undisprovable in this way.[3] Moreover, by the standards of dogmatic falsificationism all probabilistic theories also come under this head: for no finite sample can ever *disprove* a universal probabilistic theory;[4] probabilistic theories, like theories with a *ceteris paribus* clause, have no empirical basis. But then the dogmatic falsificationist relegates the most important scientific theories *on his own admission* to metaphysics where rational discussion – consisting, by his standards, of proofs and disproofs – has no place, since a metaphysical theory is neither provable nor disprovable. The demarcation criterion of dogmatic falsificationism is thus still strongly antitheoretical.

(Moreover, *one can easily argue that ceteris paribus clauses are not exceptions, but the rule in science.* Science, after all, must be demarcated from a curiosity shop where funny local – or cosmic – oddities are collected and displayed. The assertion that 'all Britons died from lung cancer between 1950 and 1960' is logically possible, and might even have been true. But if it has been only an occurrence of an event with minute probability, it would have only curiosity value for the crankish fact-collector, it would have a macabre entertainment value, but no scientific value. A proposition might be said to be scientific only if it

[1] This '*ceteris paribus*' clause need not normally be interpreted as a separate premise. For a discussion, cf. *below*, p. 98.

[2] Incidentally, we might persuade the dogmatic falsificationist that his demarcation criterion was a very naive mistake. If he gives it up but retains his two basic assumptions, he will have to ban theories from science and regard the growth of science as an accumulation of proven basic statements. This indeed is the final stage of classical empiricism after the evaporation of the hope that facts can prove or at least disprove theories.

[3] This is no coincidence; cf. *below*, p. 88 ff.

[4] Cf. Popper [1934], chapter VIII.