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Preface

In 1970 Imre Lakatos, one of the best friends I ever had, cornered me at a party. 'Paul,' he said, 'you have such strange ideas. Why don't you write them down? I shall write a reply, we publish the whole thing and I promise you – we shall have lots of fun.' I liked the suggestion and started working. The manuscript of my part of the book was finished in 1972 and I sent it to London. There it disappeared under rather mysterious circumstances. Imre Lakatos, who loved dramatic gestures, notified Interpol and, indeed, Interpol found my manuscript and returned it to me. I reread it and made some final changes. In February 1974, only a few weeks after I had finished my revision, I was informed of Imre's death. I published my part of our common enterprise without his response. A year later I published a second volume, *Science in a Free Society*, containing additional material and replies to criticism.

This history explains the form of the book. It is not a systematic treatise; it is a letter to a friend and addresses his idiosyncrasies. For example, Imre Lakatos was a rationalist, hence rationalism plays a large role in the book. He also admired Popper and therefore Popper occurs much more frequently than his 'objective importance' would warrant. Imre Lakatos, somewhat jokingly, called me an anarchist and I had no objection to putting on the anarchist's mask. Finally, Imre Lakatos loved to embarrass serious opponents with jokes and irony and so I, too, occasionally wrote in a rather ironical vein. An example is the end of Chapter 1: 'anything goes' is not a 'principle' I hold – I do not think that 'principles' can be used and fruitfully discussed outside the concrete research situation they are supposed to affect – but the terrified exclamation of a rationalist who takes a closer look at history. Reading the many thorough, serious, longwinded and thoroughly misguided criticisms I received after publication of the first English edition I often recalled my exchanges with Imre; how we would both have laughed had we been able to read these effusions together.

The new edition merges parts of *Against Method* with excerpts from *Science in a Free Society*. I have omitted material no longer of interest,

added a chapter on the trial of Galileo and a chapter on the notion of reality that seems to be required by the fact that knowledge is part of a complex historical process, eliminated mistakes, shortened the argument wherever possible and freed it from some of its earlier idiosyncrasies. Again I want to make two points: first, that science can stand on its own feet and does not need any help from rationalists, secular humanists, Marxists and similar religious movements; and, secondly, that non-scientific cultures, procedures and assumptions can also stand on their own feet and should be allowed to do so, if this is the wish of their representatives. Science must be protected from ideologies; and societies, especially democratic societies, must be protected from science. This does not mean that scientists cannot profit from a philosophical education and that humanity has not and never will profit from the sciences. However, the profits should not be imposed; they should be examined and freely accepted by the parties of the exchange. In a democracy scientific institutions, research programmes, and suggestions must therefore be subjected to public control, there must be a separation of state and science just as there is a separation between state and religious institutions, and science should be taught as one view among many and not as the one and only road to truth and reality. There is nothing in the nature of science that excludes such institutional arrangements or shows that they are liable to lead to disaster.

None of the ideas that underlie my argument is new. My interpretation of scientific knowledge, for example, was a triviality for physicists like Mach, Boltzmann, Einstein and Bohr. But the ideas of these great thinkers were distorted beyond recognition by the rodents of neopositivism and the competing rodents of the church of 'critical' rationalism. Lakatos was, after Kuhn, one of the few thinkers who noticed the discrepancy and tried to eliminate it by means of a complex and very interesting theory of rationality. I don't think he has succeeded in this. But the attempt was worth the effort; it has led to interesting results in the history of science and to new insights into the limits of reason. I therefore dedicate also this second, already much more lonely version of our common work to his memory.

Earlier material relating to the problems in this book is now collected in my *Philosophical Papers*.[1] *Farewell to Reason*[2] contains historical material, especially from the early history of rationalism in the West and applications to the problems of today.

Berkeley, September 1987

1. 2 vols, Cambridge, 1981.
2. London, 1987.

Preface to the Third Edition

Many things have happened since I first published *Against Method* (AM for short). There have been dramatic political, social and ecological changes. Freedom has increased – but it has brought hunger, insecurity, nationalistic tensions, wars and straightforward murder. World leaders have met to deal with the deterioration of our resources; as is their habit, they have made speeches and signed agreements. The agreements are far from satisfactory; some of them are a sham. However, at least verbally, the environment has become a world-wide concern. Physicians, developmental agents, priests working with the poor and disadvantaged have realized that these people know more about their condition than a belief in the universal excellence of science or organized religion had assumed and they have changed their actions and their ideas accordingly (liberation theology; primary environmental care, etc.). Many intellectuals have adapted what they have learned at universities and special schools to make their knowledge more efficient and more humane.

On a more academic level historians (of science, of culture) have started approaching the past in its own terms. Already in 1933, in his inaugural lecture at the Collège de France, Lucien Febvre had ridiculed writers who, 'sitting at their desks, behind mountains of paper, having closed and covered their windows', made profound judgements about the life of landholders, peasants and farmhands. In a narrow field historians of science tried to reconstruct the distant and the more immediate past without distorting it by modern beliefs about truth (fact) and rationality. Philosophers then concluded that the various forms of rationalism that had offered their services had not only produced chimaeras but would have damaged the sciences had they been adopted as guides. Here Kuhn's masterpiece played a decisive role.[1] It led to new ideas. Unfortunately it also encouraged lots of trash. Kuhn's main terms ('paradigm', 'revolution', 'normal

1. *The Structure of Scientific Revolutions*, Chicago, 1962.

science', 'prescience', 'anomaly', 'puzzle-solving', etc.) turned up in various forms of pseudoscience while his general approach confused many writers: finding that science had been freed from the fetters of a dogmatic logic and epistemology they tried to tie it down again, this time with sociological ropes. That trend lasted well into the early seventies. By contrast there are now historians and sociologists who concentrate on particulars and allow generalities only to the extent that they are supported by sociohistorical connections. 'Nature', says Bruno Latour, referring to 'science in the making' is 'the consequence of [a] settlement' of 'controversies'.[2] Or, as I wrote in the first edition of AM: 'Creation of a *thing*, and creation plus full understanding of a *correct idea* of the thing, *are very often parts of one and the same indivisible process* and cannot be separated without bringing the process to a stop.'[3]

Examples of the new approach are Andrew Pickering, *Constructing Quarks*, Peter Galison, *How Experiments End*, Martin Rudwick, *The Great Devonian Controversy*, Arthur Fine, *The Shaky Game* and others.[4] There are studies of the various traditions (religious, stylistic, patronage, etc.) that influenced scientists and shaped their research;[5] they show the need for a far more complex account of scientific knowledge than that which had emerged from positivism and similar philosophies. On a more general level we have the older work of Michal Polanyi and then Putnam, van Fraassen, Cartwright, Marcello Pera[6] and, yes, Imre Lakatos, who was sufficiently optimistic to believe that history herself – a lady he took very seriously – offered simple rules of theory evaluation.

In sociology the attention to detail has led to a situation where the problem is no longer why and how 'science' changes but how it keeps together. Philosophers, philosophers of biology especially, suspected for some time that there is not one entity 'science' with clearly defined principles but that science contains a great variety of (high-level theoretical, phenomenological, experimental) approaches and that even a particular science such as physics is but a scattered collection of subjects (elasticity, hydrodynamics, rheology, thermodynamics, etc., etc.) each one containing contrary tendencies (example: Prandtl vs Helmholtz, Kelvin, Lamb, Rayleigh; Truesdell

2. *Science in Action*, Milton Keynes, 1987, pp. 4 and 98f.
3. London, 1975, p. 26, repeated on p. 17 of the present edition – original emphasis.
4. All Chicago University Press.
5. An example is Mario Biagioli, *Galileo Courtier*, forthcoming.
6. *Science and Rhetoric*, forthcoming.

vs Prandtl; Birkhoff vs 'physical commonsense'; Kinsman illustrating all trends – in hydrodynamics). For some authors this is not only a fact; it is also desirable.[7] Here again I contributed, in a small way, in Chapters 3, 4 and 11 of AM,[8] in section 6 of my contribution to Lakatos and Musgrave's *Criticism and the Growth of Knowledge* (criticism of the uniformity of paradigms in Kuhn)[9] and already in 1962, in my contribution to the *Delaware Studies for the Philosophy of Science.*[10]

Unity further disappears when we pay attention not only to breaks on the theoretical level, but to experiment and, especially, to modern laboratory science. As Ian Hacking has shown in his pathbreaking essay *Representing and Intervening*[11] and as emerges from Pickering's *Science as Practice and Culture*,[12] terms such as 'experiment' and 'observation' cover complex processes containing many strands. 'Facts' come from negotiations between different parties and the final product – the published report – is influenced by physical events, dataprocessors, compromises, exhaustion, lack of money, national pride and so on. Some microstudies of laboratory science resemble the 'New Journalism' of Jimmy Breslin, Guy Talese, Tom Wolfe and others; researchers no longer sit back and read the papers in a certain field; they are not content with silent visits to laboratories either – they walk right in, engage scientists in conversation and make things happen (Kuhn and his collaborators started the procedure in their interviews for the history of quantum mechanics). At any rate – we are a long way from the old (Platonic) idea of science as a system of statements growing with experiment and observation and kept in order by lasting rational standards.

AM is still partly proposition oriented; however, I also had my sane moments. My discussion of incommensurability, for example, does not 'reduce the difference to one of theory' as Pickering writes.[13] It includes art forms, perceptions (a large part of Chapter 16 is about the transition from Greek geometric art and poetry to the classical period), stages of child development and asserts 'that the views of scientists and especially their views on basic matters are often as different from each other as are the ideologies of different

7. J. Dupré, 'The Disunity of Science', *Mind*, 92, 1983.
8. Present edition. Taken over unamended from first edition.
9. I. Lakaros and A. Musgrave (eds), *Criticism and the Growth of Knowledge*, Cambridge, 1965.
10. 'How to be a Good Empiricist', *Delaware Studies*, Vol. 2, 1963.
11. Cambridge, 1983.
12. A. Pickering (ed.), *Science as Practice and Culture*, Chicago, 1992.
13. ibid., p. 10.

cultures'.[14] In this connection I examined the practical aspects of logic, the way, that is, in which ideas are related to each other in ongoing research rather than in the finished products (if there ever are such products). My discussion of the many events that constitute what is being observed[15] and especially my discussion of Galileo's telescopic discoveries[16] agree with the requirements of the new laboratory sociology except that Galileo's 'laboratory' was rather small by comparison. This case shows, incidentally, that like the older philosophies of science the new microsociology is not a universal account but a description of prominent aspects of a special period. It does not matter. A universal description of science at any rate can at most offer a list of events.[17] It was different in antiquity.

It is clear that the new situation requires a new philosophy and, above all, new terms. Yet some of the foremost researchers in the area are still asking themselves whether a particular piece of research produces a 'discovery', or an invention', or to what extent a (temporary) result is 'objective'. The problem arose in quantum mechanics; it is also a problem for classical science. Shall we continue using outmoded terms to describe novel insight or would it not be better to start using a new language? And wouldn't poets and journalists be of great help in finding such a language?

Secondly, the new situation again raises the question of 'science' vs democracy. For me this was the most important question. 'My main reason for writing the book', I say in the Introduction to the Chinese Edition,[18] 'was humanitarian, not intellectual. I wanted to support people, not to "advance knowledge."' Now if science is no longer a unit, if different parts of it proceed in radically different ways and if connections between these ways are tied to particular research episodes, then scientific projects have to be taken individually. This is what government agencies started doing some time ago. In the late sixties 'the idea of a comprehensive science policy was gradually abandoned. It was realized that science was not one but many enterprises and that there could be no single policy for the support of all of them.[19] Government agencies no longer finance 'science', they finance particular projects. But then the word 'scientific' can no longer exclude 'unscientific' projects – we have to look at matters in

14. AM, first edition, p. 274.
15. ibid., pp. 149ff. Reprinted in the present edition.
16. Chapters 8 to 10 of the present edition.
17. Cf. my contribution to the 1992 Erasmus Symposium, 'Has the Scientific View of the World a Special Status Compared With Other Views?', forthcoming.
18. Contained in the present edition.
19. J. Ben-David, *Scientific Growth*, Berkeley, 1991, p. 525.

detail. Are the new philosophers and sociologists prepared to consider this consequence of their research?

There have been many other changes. Medical researchers and technologists have not only invented useful instruments (such as those employing the principles of fibre optics which in many contexts replace the more dangerous methods of X-ray diagnostic) but have become more open towards new (or older) ideas. Only twenty years ago the idea that the mind affects physical well-being, though supported by experience, was rather unpopular – today it is mainstream. Malpractice suits have made physicians more careful, sometimes too careful for the good of their patient, but they have also forced them to consult alternative opinions. (In Switzerland a belligerent plurality of views is almost part of culture – and I used it when arranging public confrontations between hardheaded scientists and 'alternative' thinkers.[20]) However, here as elsewhere, simple philosophies, whether of a dogmatic or a more liberal kind, have their limits. *There are no general solutions.* An increased liberalism in the definition of 'fact' can have grave repercussions,[21] while the idea that truth is concealed and even perverted by the processes that are meant to establish it makes excellent sense.[22] I therefore again warn the reader that I don't have the intention of replacing 'old and dogmatic' principles by 'new and more libertarian ones'. For example, I am neither a populist for whom an appeal to 'the people' is the basis of all knowledge, nor a relativist for whom there are no 'truths as such' but only truths for this or that group and/or individual. All I say is that non-experts often know more than experts *and should therefore be consulted* and that prophets of truth (including those who use arguments) more often than not are carried along by a vision that clashes with the very events the vision is supposed to be exploring. There exists ample evidence for both parts of this assertion.

A case I already mentioned is development: professionals dealing with the ecological, social and medical parts of developmental aid have by now realized that the imposition of 'rational' or 'scientific' procedures, though occasionally beneficial (removal of some parasites and infectious diseases), can lead to serious material and

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20. Cf. the series edited by Christian Thomas and myself and published by the Verlag der Fachvereine, Zurich, 1983–87.

21. Cf. Peter W. Huber, *Galileo's Revenge*, New York, 1991.

22. For a fictional account, cf. Tom Wolfe's *The Bonfire of the Vanities*, New York, 1987.

spiritual problems. They did not abandon what they had learned in their universities, however; they combined this knowledge with local beliefs and customs and thereby established a much needed link with the problems of life that surround us everywhere, in the First, Second, and Third Worlds.

The present edition contains major changes (Chapter 19 and part of Chapter 16 have been rewritten, the old Chapter 20 has been omitted), additions (a paragraph here, a paragraph there), stylistic changes (I hope they are improvements) and corrections as well as additions in the references. As far as I am concerned the main ideas of the essay (i.e. the ideas expressed in italics in the Introduction to the Chinese Edition) are rather trivial and appear trivial when expressed in suitable terms. I prefer more paradoxical formulations, however, for nothing dulls the mind as thoroughly as hearing familiar words and slogans. It is one of the merits of deconstruction to have undermined philosophical commonplaces and thus to have made some people think. Unfortunately it affected only a small circle of insiders and it affected them in ways that are not always clear, not even to them. That's why I prefer Nestroy, who was a great, popular and funny deconstructeur, while Derrida, for all his good intentions, can't even tell a story.

Rome, July 1992

Introduction to the Chinese Edition

This book proposes a thesis and draws consequences from it. The thesis is: *the events, procedures and results that constitute the sciences have no common structure*; there are no elements that occur in every scientific investigation but are missing elsewhere. Concrete developments (such as the overthrow of steady state cosmologies and the discovery of the structure of DNA) have distinct features and we can often explain why and how these features led to success. But not every discovery can be accounted for in the same manner, and procedures that paid off in the past may create havoc when imposed on the future. Successful research does not obey general standards; it relies now on one trick, now on another; the moves that advance it and the standards that define what counts as an advance are not always known to the movers. Far-reaching changes of outlook, such as the so-called 'Copernican Revolution' or the 'Darwinian Revolution', affect different areas of research in different ways and receive different impulses from them. A theory of science that devises standards and structural elements for *all* scientific activities and authorizes them by reference to 'Reason' or 'Rationality' may impress outsiders – but it is much too crude an instrument for the people on the spot, that is, for scientists facing some concrete research problem.

In this book I try to support the thesis by historical examples. Such support does not *establish* it; it makes it *plausible* and the way in which it is reached indicates how future statements about 'the nature of science' may be undermined: given any rule, or any general statement about the sciences, there always exist developments which are praised by those who support the rule but which show that the rule does more damage than good.

One consequence of the thesis is that *scientific successes cannot be explained in a simple way*. We cannot say: 'the structure of the atomic nucleus was found because people did A, B, C . . .' where A, B and C are procedures which can be understood independently of their use in nuclear physics. All we can do is to give a historical account of the

details, including social circumstances, accidents and personal idiosyncrasies.

Another consequence is that *the success of 'science' cannot be used as an argument for treating as yet unsolved problems in a standardized way*. That could be done only if there are procedures that can be detached from particular research situations and whose presence guarantees success. The thesis says that there are no such procedures. Referring to the success of 'science' in order to justify, say, quantifying human behaviour is therefore an argument without substance. Quantification works in some cases, fails in others; for example, it ran into difficulties in one of the apparently most quantitative of all sciences, celestial mechanics (special region: stability of the planetary system) and was replaced by qualitative (topological) considerations.

It also follows that *'non-scientific' procedures cannot be pushed aside by argument*. To say: 'the procedure you used is non-scientific, therefore we cannot trust your results and cannot give you money for research' assumes that 'science' is successful and that it is successful because it uses uniform procedures. The first part of the assertion ('science is always successful') is not true, if by 'science' we mean things done by scientists – there are lots of failures also. The second part – that successes are due to uniform procedures – is not true because there are no such procedures. Scientists are like architects who build buildings of different sizes and different shapes and who can be judged only *after* the event, i.e. only after they have finished their structure. It may stand up, it may fall down – nobody knows.

But if scientific achievements can be judged only after the event and if there is no abstract way of ensuring success beforehand, then there exists no special way of weighing scientific promises either – scientists are no better off than anybody else in these matters, they only know more details. This means that *the public can participate in the discussion without disturbing existing roads to success* (there are no such roads). In cases where the scientists' work affects the public it even *should* participate: first, because it is a concerned party (many scientific decisions affect public life); secondly, because such participation is the best scientific education the public can get – a full democratization of science (which includes the protection of minorities such as scientists) is not in conflict with science. It is in conflict with a philosophy, often called 'Rationalism', that uses a frozen image of science to terrorize people unfamiliar with its practice.

A consequence to which I allude in Chapter 19 and which is closely connected with its basic thesis is that *there can be many different kinds of science*. People starting from different social backgrounds will

approach the world in different ways and learn different things about it. People survived millennia before Western science arose; to do this they had to know their surroundings up to and including elements of astronomy. 'Several thousand Cuahuila Indians never exhausted the natural resources of a desert region in South California, in which today only a handful of white families manage to subsist. They lived in a land of plenty, for in this apparently completely barren territory, they were familiar with no less than sixty kinds of edible plants and twenty-eight others of narcotic, stimulant or medical properties'.[1] The knowledge that preserves the lifestyles of nomads was acquired and is preserved in a non-scientific way ('science' now being modern natural science). Chinese technology for a long time lacked any Western-scientific underpinning and yet it was far ahead of contemporary Western technology. It is true that Western science now reigns supreme all over the globe; however, the reason was not insight in its 'inherent rationality' but power play (the colonizing nations imposed their ways of living) and the need for weapons: Western science so far has created the most efficient instruments of death. The remark that without Western science many 'Third World nations' would be starving is correct but one should add that the troubles were created, not alleviated by earlier forms of 'development'. It is also true that Western medicine helped eradicate parasites and some infectious diseases but this does not show that Western science is the only tradition that has good things to offer and that other forms of inquiry are without any merit whatsoever. *First-world science is one science among many*; by claiming to be more it ceases to be an instrument of research and turns into a (political) pressure group. More on these matters can be found in my book *Farewell to Reason*.[2]

My main motive in writing the book was humanitarian, not intellectual. I wanted to support people, not to 'advance knowledge'. People all over the world have developed ways of surviving in partly dangerous, partly agreeable surroundings. The stories they told and the activities they engaged in enriched their lives, protected them and gave them meaning. The 'progress of knowledge and civilization' – as the process of pushing Western ways and values into all corners of the globe is being called – destroyed these wonderful products of human ingenuity and compassion without a single glance in their direction. 'Progress of knowledge' in many places meant killing of minds. Today old traditions are being revived and people try again to adapt

1. C. Lévi-Strauss, *The Savage Mind*, London, 1966, pp. 4f.
2. London, 1987.

their lives to the ideas of their ancestors. I have tried to show, by an analysis of the apparently hardest parts of science, the natural sciences, that science, properly understood, has no argument against such a procedure. There are many scientists who act accordingly. Physicians, anthropologists and environmentalists are starting to adapt their procedures to the values of the people they are supposed to advise. I am not against a science so understood. Such a science is one of the most wonderful inventions of the human mind. But I am against ideologies that use the name of science for cultural murder.

Analytical Index

Being a Sketch of the Main Argument

between facts and theories may be proof of progress. It is also a first step in our attempt to find the principles implicit in familiar observational notions.

6 *54*

As an example of such an attempt I examine the tower argument *which the Aristotelians used to refute the motion of the earth. The argument involves* natural interpretations *– ideas so closely connected with observations that it needs a special effort to realize their existence and to determine their content. Galileo identifies the natural interpretations which are inconsistent with Copernicus and replaces them by others.*

7 *65*

The new natural interpretations constitute a new and highly abstract observation language. They are introduced and concealed *so that one fails to notice the change that has taken place (method of anamnesis). They contain the idea of the* relativity of all motion *and the* law of circular inertia.

8 *77*

In addition to natural interpretations, Galileo also changes sensations *that seem to endanger Copernicus. He admits that there are such sensations, he praises Copernicus for having disregarded them, he claims to have removed them with the help of the* telescope. *However, he offers no* theoretical *reasons why the telescope should be expected to give a true picture of the sky.*

9 *86*

Nor does the initial experience *with the telescope provide such reasons. The first telescopic observations of the sky are indistinct, indeterminate, contradictory and in conflict with what everyone can see with his unaided eyes. And, the only theory that could have helped to separate telescopic illusions from veridical phenomena was refuted by simple tests.*

10 *103*

On the other hand, there are some telescopic phenomena which are plainly Copernican. Galileo introduces these phenomena as independent evidence for Copernicus while the situation is rather that one refuted view – Copernicanism – has a certain similarity with phenomena emerging from another refuted view – the idea that telescopic phenomena are faithful images of the sky.

11 *106*

Such 'irrational' methods of support are needed because of the 'uneven development' (Marx, Lenin) of different parts of science. Copernicanism and other essential ingredients of modern science survived only because reason was frequently overruled in their past.

19 *238*

Science is neither a single tradition, nor the best tradition there is, except for people who have become accustomed to its presence, its benefits and its disadvantages. In a democracy it should be separated from the state just as churches are now separated from the state.

20 *252*

The point of view underlying this book is not the result of a well-planned train of thought but of arguments prompted by accidental encounters. Anger at the wanton destruction of cultural achievements from which we all could have learned, at the conceited assurance with which some intellectuals interfere with the lives of people, and contempt for the treacly phrases they use to embellish their misdeeds was and still is the motive force behind my work.

Introduction

Science is an essentially anarchic enterprise: theoretical anarchism is more humanitarian and more likely to encourage progress than its law-and-order alternatives.

Ordnung ist heutzutage meistens dort,
wo nichts ist.
Es ist eine Mangelerscheinung.

BRECHT

The following essay is written in the conviction that *anarchism*, while perhaps not the most attractive *political* philosophy, is certainly excellent medicine for *epistemology*, and for the *philosophy of science.*

The reason is not difficult to find.

'History generally, and the history of revolution in particular, is always richer in content, more varied, more many-sided, more lively and subtle than even' the best historian and the best methodologist can imagine.[1] History is full of 'accidents and conjunctures and curious juxtapositions of events'[2] and it demonstrates to us the 'complexity of human change and the unpredictable character of the ultimate consequences of any given act or decision of men'.[3] Are we really to believe that the naive and simple-minded rules which methodologists take as their guide are capable of accounting for such a 'maze of interactions'?[4] And is it not clear that successful

1. 'History as a whole, and the history of revolutions in particular, is always richer in content, more varied, more multiform, more lively and ingenious than is imagined by even the best parties, the most conscious vanguards of the most advanced classes' (V.I. Lenin, 'Left-Wing Communism – An Infantile Disorder', *Selected Works*, Vol. 3, London, 1967, p. 401). Lenin is addressing parties and revolutionary vanguards rather than scientists and methodologists; the lesson, however, is the same. Cf. footnote 5.

2. Herbert Butterfield, *The Whig Interpretation of History*, New York, 1965, p. 66.

3. ibid., p. 21.

4. ibid., p. 25, cf. Hegel, *Philosophie der Geschichte*, *Werke*, Vol. 9, ed. Edward Gans, Berlin, 1837, p. 9: 'But what experience and history teach us is this, that nations and

participation in a process of this kind is possible only for a ruthless opportunist who is not tied to any particular philosophy and who adopts whatever procedure seems to fit the occasion?

This is indeed the conclusion that has been drawn by intelligent and thoughtful observers. 'Two very important practical conclusions follow from this [character of the historical process],' writes Lenin,[5] continuing the passage from which I have just quoted. 'First, that in order to fulfil its task, the revolutionary class [i.e. the class of those who want to change either a part of society such as science, or society as a whole] must be able to master *all* forms or aspects of social activity without exception [it must be able to understand, and to apply, not only one particular methodology, but any methodology, and any variation thereof it can imagine]...; second [it] must be ready to pass from one to another in the quickest and most unexpected manner.' 'The external conditions', writes Einstein,[6] 'which are set for [the scientist] by the facts of experience do not permit him to let himself be too much restricted, in the construction of his conceptual world, by the adherence to an epistemological system. He, therefore, must appear to the systematic epistemologist as a type of unscrupulous opportunist....' A complex medium containing surprising and unforeseen developments demands complex procedures and defies analysis on the basis of rules which

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governments have never learned anything from history, or acted according to rules that might have derived from it. Every period has such peculiar circumstances, is in such an individual state, that decisions will have to be made, and decisions *can* only be made, in it and out of it.' – 'Very clever'; 'shrewd and very clever'; 'NB' writes Lenin in his marginal notes to this passage. (*Collected Works*, Vol. 38, London, 1961, p. 307.)

5. ibid. We see here very clearly how a few substitutions can turn a political lesson into a lesson for *methodology*. This is not at all surprising. Methodology and politics are both means for moving from one historical stage to another. We also see how an individual, such as Lenin, who is not intimidated by traditional boundaries and whose thought is not tied to the ideology of a particular profession, can give useful advice to everyone, philosophers of science included. In the 19th century the idea of an elastic and historically informed methodology was a matter of course. Thus Ernst Mach wrote in his book *Erkenntnis und Irrtum*, Neudruck, Wissenschaftliche Buchgesellschaft, Darmstadt, 1980, p. 200: 'It is often said that research cannot be taught. That is quite correct, in a certain sense. The schemata of *formal* logic and of *inductive* logic are of little use for the intellectual situations are never exactly the same. But the examples of great scientists are very suggestive.' They are not suggestive because we can abstract rules from them and subject future research to their jurisdiction; they are suggestive because they make the mind nimble and capable of inventing entirely new research traditions. For a more detailed account of Mach's philosophy see my essay *Farewell to Reason*, London, 1987, Chapter 7, as well as Vol. 2, Chapters 5 and 6 of my *Philosophical Papers*, Cambridge, 1981.

6. Albert Einstein, *Albert Einstein: Philosopher Scientist*, ed. P.A. Schilpp, New York, 1951, pp. 683f.

have been set up in advance and without regard to the ever-changing conditions of history.

Now it is, of course, possible to simplify the medium in which a scientist works by simplifying its main actors. The history of science, after all, does not just consist of facts and conclusions drawn from facts. It also contains ideas, interpretations of facts, problems created by conflicting interpretations, mistakes, and so on. On closer analysis we even find that science knows no 'bare facts' at all but that the 'facts' that enter our knowledge are already viewed in a certain way and are, therefore, essentially ideational. This being the case, the history of science will be as complex, chaotic, full of mistakes, and entertaining as the ideas it contains, and these ideas in turn will be as complex, chaotic, full of mistakes, and entertaining as are the minds of those who invented them. Conversely, a little brainwashing will go a long way in making the history of science duller, simpler, more uniform, more 'objective' and more easily accessible to treatment by strict and unchangeable rules.

Scientific education as we know it today has precisely this aim. It simplifies 'science' by simplifying its participants: first, a domain of research is defined. The domain is separated from the rest of history (physics, for example, is separated from metaphysics and from theology) and given a 'logic' of its own. A thorough training in such a 'logic' then conditions those working in the domain; it makes *their actions* more uniform and it freezes large parts of the *historical process* as well. Stable 'facts' arise and persevere despite the vicissitudes of history. An essential part of the training that makes such facts appear consists in the attempt to inhibit intuitions that might lead to a blurring of boundaries. A person's religion, for example, or his metaphysics, or his sense of humour (his *natural* sense of humour and not the inbred and always rather nasty kind of jocularity one finds in specialized professions) must not have the slightest connection with his scientific activity. His imagination is restrained, and even his language ceases to be his own. This is again reflected in the nature of scientific 'facts' which are experienced as being independent of opinion, belief, and cultural background.

It is thus *possible* to create a tradition that is held together by strict rules, and that is also successful to some extent. But is it *desirable* to support such a tradition to the exclusion of everything else? Should we transfer to it the sole rights for dealing in knowledge, so that any result that has been obtained by other methods is at once ruled out of court? And did scientists ever remain within the boundaries of the traditions they defined in this narrow way? These are the questions I intend to ask in the present essay. And to these questions my answer will be a firm and resounding NO.

There are two reasons why such an answer seems to be appropriate. The first reason is that the world which we want to explore is a largely unknown entity. We must, therefore, keep our options open and we must not restrict ourselves in advance. Epistemological prescriptions may look splendid when compared with other epistemological prescriptions, or with general principles – but who can guarantee that they are the best way to discover, not just a few isolated 'facts', but also some deep-lying secrets of nature? The second reason is that a scientific education as described above (and as practised in our schools) cannot be reconciled with a humanitarian attitude. It is in conflict 'with the cultivation of individuality which alone produces, or can produce, well-developed human beings';[7] it 'maims by compression, like a Chinese lady's foot, every part of human nature which stands out prominently, and tends to make a person markedly different in outline'[8] from the ideals of rationality that happen to be fashionable in science, or in the philosophy of science. The attempt to increase liberty, to lead a full and rewarding life, and the corresponding attempt to discover the secrets of nature and of man, entails, therefore, the rejection of all universal standards and of all rigid traditions. (Naturally, it also entails the rejection of a large part of contemporary science.)

It is surprising to see how rarely the stultifying effect of 'the Laws of Reason' or of scientific practice is examined by professional anarchists. Professional anarchists oppose any kind of restriction and they demand that the individual be permitted to develop freely, unhampered by laws, duties or obligations. And yet they swallow without protest all the severe standards which scientists and logicians impose upon research and upon any kind of knowledge-creating and knowledge-changing activity. Occasionally, the laws of scientific method, or what are thought to be the laws of scientific method by a particular writer, are even integrated into anarchism itself. 'Anarchism is a world concept based upon a mechanical explanation of all phenomena,' writes Kropotkin.[9] 'Its method of investigation is

7. John Stuart Mill, 'On Liberty', in *The Philosophy of John Stuart Mill*, ed. Marshall Cohen, New York, 1961, p. 258.

8. ibid., p. 265.

9. Peter Alexeivich Kropotkin, 'Modern Science and Anarchism', *Kropotkin's Revolutionary Pamphlets*, ed. R.W. Baldwin, New York, 1970, pp. 150–2. 'It is one of Ibsen's great distinctions that nothing was valid for him but science.' B. Shaw, *Back to Methuselah*, New York, 1921, p. xcvii. Commenting on these and similar phenomena Strindberg writes (*Antibarbarus*): 'A generation that had the courage to get rid of God, to crush the state and church, and to overthrow society and morality, still bowed before Science. And in Science, where freedom ought to reign, the order of the day was "believe in the authorities or off with your head".'

that of the exact natural sciences ... the method of induction and deduction.' 'It is not so clear,' writes a modern 'radical' professor at Columbia,[10] 'that scientific research demands an absolute freedom of speech and debate. Rather the evidence suggests that certain kinds of unfreedom place no obstacle in the way of science....'

There are certainly some people to whom this is 'not so clear'. Let us, therefore, start with our outline of an anarchistic methodology and a corresponding anarchistic science. There is no need to fear that the diminished concern for law and order in science and society that characterizes an anarchism of this kind will lead to chaos. The human nervous system is too well organized for that.[11] There may, of course, come a time when it will be necessary to give reason a temporary advantage and when it will be wise to defend its rules to the exclusion of everything else. I do not think that we are living in such a time today.[12]

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10. R.P. Wolff, *The Poverty of Liberalism*, Boston, 1968, p. 15. For a criticism of Wolff see footnote 52 of my essay 'Against Method', in *Minnesota Studies in the Philosophy of Science*, Vol. 4, Minneapolis, 1970.

11. Even in undetermined and ambiguous situations, uniformity of action is soon achieved and adhered to tenaciously. See Muzafer Sherif, *The Psychology of Social Norms*, New York, 1964.

12. This was my opinion in 1970 when I wrote the first version of this essay. Times have changed. Considering some tendencies in US education ('politically correct', academic menus, etc.), in philosophy (postmodernism) and in the world at large I think that reason should now be given greater weight not because it is and always was fundamental but because it seems to be needed, in circumstances that occur rather frequently today (but may disappear tomorrow), to create a more humane approach.

1

This is shown both by an examination of historical episodes and by an abstract analysis of the relation between idea and action. The only principle that does not inhibit progress is: anything goes.

The idea of a method that contains firm, unchanging, and absolutely binding principles for conducting the business of science meets considerable difficulty when confronted with the results of historical research. We find, then, that there is not a single rule, however plausible, and however firmly grounded in epistemology, that is not violated at some time or other. It becomes evident that such violations are not accidental events, they are not results of insufficient knowledge or of inattention which might have been avoided. On the contrary, we see that they are necessary for progress. Indeed, one of the most striking features of recent discussions in the history and philosophy of science is the realization that events and developments, such as the invention of atomism in antiquity, the Copernican Revolution, the rise of modern atomism (kinetic theory; dispersion theory; stereochemistry; quantum theory), the gradual emergence of the wave theory of light, occurred only because some thinkers either *decided* not to be bound by certain 'obvious' methodological rules, or because they *unwittingly broke* them.

This liberal practice, I repeat, is not just a *fact* of the history of science. It is both reasonable and *absolutely necessary* for the growth of knowledge. More specifically, one can show the following: given any rule, however 'fundamental' or 'rational', there are always circumstances when it is advisable not only to ignore the rule, but to adopt its opposite. For example, there are circumstances when it is advisable to introduce, elaborate, and defend *ad hoc* hypotheses, or hypotheses which contradict well-established and generally accepted experimental results, or hypotheses whose content is smaller than the

content of the existing and empirically adequate alternative, or self-inconsistent hypotheses, and so on.[1]

There are even circumstances – and they occur rather frequently – when *argument* loses its forward-looking aspect and becomes a hindrance to progress. Nobody would claim that the teaching of *small children* is exclusively a matter of argument (though argument may enter into it, and should enter into it to a larger extent than is customary), and almost everyone now agrees that what looks like a result of reason – the mastery of a language, the existence of a richly articulated perceptual world, logical ability – is due partly to indoctrination and partly to a process of *growth* that proceeds with the force of natural law. And where arguments do seem to have an effect, this is more often due to their *physical repetition* than to their *semantic content*.

Having admitted this much, we must also concede the possibility of non-argumentative growth in the *adult* as well as in (the theoretical parts of) *institutions* such as science, religion, prostitution, and so on.

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1. One of the few thinkers to understand this feature of the development of knowledge was Niels Bohr: '... he would never try to outline any finished picture, but would patiently go through all the phases of the development of a problem, starting from some apparent paradox, and gradually leading to its elucidation. In fact, he never regarded achieved results in any other light than as starting points for further exploration. In speculating about the prospects of some line of investigation, he would dismiss the usual consideration of simplicity, elegance or even consistency with the remark that such qualities can only be properly judged *after* [my italics] the event....' L. Rosenfeld in *Niels Bohr. His Life and Work as seen by his Friends and Colleagues*, S. Rosental (ed.), New York, 1967, p. 117. Now science is never a completed process, therefore it is always 'before' the event. Hence simplicity, elegance or consistency are *never* necessary conditions of (scientific) practice.

Considerations such as these are usually criticized by the childish remark that a contradiction 'entails' everything. But contradictions do not 'entail' anything unless people use them in certain ways. And people will use them as entailing everything only if they accept some rather simple-minded rules of derivation. Scientists proposing theories with logical faults and obtaining interesting results with their help (for example: the results of early forms of the calculus; of a geometry where lines consist of points, planes of lines and volumes of planes; the predictions of the older quantum theory and of early forms of the quantum theory of radiation – and so on) evidently proceed according to different rules. The criticism therefore falls back on its authors unless it can be shown that a logically decontaminated science has better results. Such a demonstration is impossible. Logically perfect versions (if such versions exist) usually arrive only long after the imperfect versions have enriched science by their contributions. For example, wave mechanics was not a 'logical reconstruction' of preceding theories; it was an attempt to preserve their achievements and to solve the physical problems that had arisen from their use. Both the achievements and the problems were produced in a way very different from the ways of those who want to subject everything to the tyranny of 'logic'.

We certainly cannot take it for granted that what is possible for a small child – to acquire new modes of behaviour on the slightest provocation, to slide into them without any noticeable effort – is beyond the reach of his elders. One should rather expect that catastrophic changes in the physical environment, wars, the breakdown of encompassing systems of morality, political revolutions, will transform adult reaction patterns as well, including important patterns of argumentation. Such a transformation may again be an entirely natural process and the only function of a rational argument may lie in the fact that it increases the mental tension that preceded *and caused* the behavioural outburst.

Now, if there are events, not necessarily arguments, which *cause* us to adopt new standards, including new and more complex forms of argumentation, is it then not up to the defenders of the *status quo* to provide, not just counter-arguments, but also contrary *causes*? ('Virtue without terror is ineffective,' says Robespierre.) And if the old forms of argumentation turn out to be too weak a cause, must not these defenders either give up or resort to stronger and more 'irrational' means? (It is very difficult, and perhaps entirely impossible, to combat the effects of brainwashing by argument.) Even the most puritanical rationalist will then be forced to stop reasoning and to use *propaganda* and *coercion*, not because some of his *reasons* have ceased to be valid, but because the *psychological conditions* which make them effective, and capable of influencing others, have disappeared. And what is the use of an argument that leaves people unmoved?

Of course, the problem never arises quite in this form. The teaching of standards and their defence never consists merely in putting them before the mind of the student and making them as *clear* as possible. The standards are supposed to have maximal *causal efficacy* as well. This makes it very difficult indeed to distinguish between the *logical force* and the *material effect* of an argument. Just as a well-trained pet will obey his master no matter how great the confusion in which he finds himself, and no matter how urgent the need to adopt new patterns of behaviour, so in the very same way a well-trained rationalist will obey the mental image of *his* master, he will conform to the standards of argumentation he has learned, he will adhere to these standards no matter how great the confusion in which he finds himself, and he will be quite incapable of realizing that what he regards as the 'voice of reason' is but a *causal after-effect* of the training he had received. He will be quite unable to discover that the appeal to reason to which he succumbs so readily is nothing but a *political manoeuvre*.

That interests, forces, propaganda and brainwashing techniques play a much greater role than is commonly believed in the growth of our knowledge and in the growth of science, can also be seen from an analysis of the *relation between idea and action*. It is often taken for granted that a clear and distinct understanding of new ideas precedes, and should precede, their formulation and their institutional expression. *First*, we have an idea, or a problem, *then* we act, i.e. either speak, or build, or destroy. Yet this is certainly not the way in which small children develop. They use words, they combine them, they play with them, until they grasp a meaning that has so far been beyond their reach. And the initial playful activity is an essential prerequisite of the final act of understanding. There is no reason why this mechanism should cease to function in the adult. We must expect, for example, that the *idea* of liberty could be made clear only by means of the very same actions, which were supposed to *create* liberty. Creation of a *thing*, and creation plus full understanding of a *correct idea* of the thing, *are very often parts of one and the same indivisible process* and cannot be separated without bringing the process to a stop. The process itself is not guided by a well-defined programme, and cannot be guided by such a programme, for it contains the conditions for the realization of all possible programmes. It is guided rather by a vague urge, by a 'passion' (Kierkegaard). The passion gives rise to specific behaviour which in turn creates the circumstances and the ideas necessary for analysing and explaining the process, for making it 'rational'.

The development of the Copernican point of view from Galileo to the 20th century is a perfect example of the situation I want to describe. We start with a strong belief that runs counter to contemporary reason and contemporary experience. The belief spreads and finds support in other beliefs which are equally unreasonable, if not more so (law of inertia; the telescope). Research now gets deflected in new directions, new kinds of instruments are built, 'evidence' is related to theories in new ways until there arises an ideology that is rich enough to provide independent arguments for any particular part of it and mobile enough to find such arguments whenever they seem to be required. We can say today that Galileo was on the right track, for his persistent pursuit of what once seemed to be a silly cosmology has by now created the material needed to defend it against all those who will accept a view only if it is told in a certain way and who will trust it only if it contains certain magical phrases, called 'observational reports'. And this is not an exception – it is the normal case: theories become clear and 'reasonable' only *after* incoherent parts of them have been used for a long time. Such

unreasonable, nonsensical, unmethodical foreplay thus turns out to be an unavoidable precondition of clarity and of empirical success.

Now, when we attempt to describe and to understand developments of this kind in a general way, we are, of course, obliged to appeal to the existing forms of speech which do not take them into account and which must be distorted, misused, beaten into new patterns in order to fit unforeseen situations (without a constant misuse of language there cannot be any discovery, any progress). 'Moreover, since the traditional categories are the gospel of everyday thinking (including ordinary scientific thinking) and of everyday practice, [such an attempt at understanding] in effect presents rules and forms of false thinking and action – false, that is, from the standpoint of (scientific) common sense.'[2] This is how *dialectical thinking* arises as a form of thought that 'dissolves into nothing the detailed determinations of the understanding',[3] formal logic included.

(Incidentally, it should be pointed out that my frequent use of such words as 'progress', 'advance', 'improvement', etc., does not mean that I claim to possess special knowledge about what is good and what is bad in the sciences and that I want to impose this knowledge upon my readers. *Everyone can read the terms in his own way* and in accordance with the tradition to which he belongs. Thus for an empiricist, 'progress' will mean transition to a theory that provides direct empirical tests for most of its basic assumptions. Some people believe the quantum theory to be a theory of this kind. For others, 'progress' may mean unification and harmony, perhaps even at the expense of empirical adequacy. This is how Einstein viewed the general theory of relativity. *And my thesis is that anarchism helps to achieve progress in any one of the senses one cares to choose.* Even a law-and-order science will succeed only if anarchistic moves are occasionally allowed to take place.)

It is clear, then, that the idea of a fixed method, or of a fixed theory of rationality, rests on too naive a view of man and his social surroundings. To those who look at the rich material provided by history, and who are not intent on impoverishing it in order to please their lower instincts, their craving for intellectual security in the form of clarity, precision, 'objectivity', 'truth', it will become clear that there is only one principle that can be defended under *all*

2. Herbert Marcuse, *Reason and Revolution*, London, 1941, p. 130.
3. Hegel, *Wissenschaft der Logik*, Vol. 1, Hamburg, 1965, p. 6.

circumstances and in all stages of human development. It is the principle: *anything goes*.

This abstract principle must now be examined and explained in concrete detail.