

T H I R D E D I T I O N

A g a i n s t
M e t h o d



P A U L F E Y E R A B E N D

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 Nestor, 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 Cuahuitla 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 *Farwell 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

Introduction

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

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.

2 For example, we may use hypotheses that contradict well-confirmed theories and/or well-established experimental results. We may advance science by proceeding counterinductively.

3 The consistency condition which demands that new hypotheses agree with accepted theories is unreasonable because it preserves the older theory, and not the better theory. Hypotheses contradicting well-confirmed theories give us evidence that cannot be obtained in any other way. Proliferation of theories is beneficial for science, while uniformity impairs its critical power. Uniformity also endangers the free development of the individual.

4 There is no idea, however ancient and absurd, that is not capable of improving our knowledge. The whole history of thought is absorbed into science and is used for improving every single theory. Nor is political interference rejected. It may be needed to overcome the chaosmism of science that resists alternatives to the status quo.

5 No theory ever agrees with all the facts in its domain, yet it is not always the theory that is to blame. Facts are constituted by older ideologies, and a clash

6 AGAINST METHOD

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 As an example of such an attempt I examine the tower argument which the
54 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 The new natural interpretations constitute a new and highly abstract
65 observational language. They are introduced and concealed so that one fails to notice the change that has taken place (method of amnesia). They contain the idea of the relativity of all motion and the law of circular inertia.

8 In addition to natural interpretations, Galileo also changes sensations that
77 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 Nor does the initial experience with the telescope provide such reasons. The
86 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 On the other hand, there are some telescopic phenomena which are plainly
103 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 Such 'irrational' methods of support are needed because of the 'unseen
106 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.

ANALYTICAL INDEX

12 Galileo's method works in other fields as well. For example, it can be used to
123 eliminate the existing arguments against materialism, and to put an end to the philosophical mind/body problem (the corresponding scientific problems remain untouched, however). It does not follow that it should be universally applied.

13 The Church at the time of Galileo not only kept closer to reason as defined
125 then and, in part, even now: it also considered the ethical and social consequences of Galileo's views. Its indictment of Galileo was rational and only opportunism and a lack of perspective can demand a revision.

14 Galileo's inquiries formed only a small part of the so-called Copernican
135 Revolution. Adding the remaining elements makes it still more difficult to reconcile the development with familiar principles of theory evaluation.

15 The results obtained so far suggest abolishing the distinction between a
147 context of discovery and a context of justification, norms and facts, observational terms and theoretical terms. None of these distinctions plays a role in scientific practice. Attempts to enforce them would have disastrous consequences. Popper's critical rationalism fails for the same reasons.

Appendix 1

16 Finally, the kind of comparison that underlies most methodologies is possible
164 only in some rather simple cases. It breaks down when we try to compare non-scientific views with science and when we consider the most advanced, most general and therefore most mythological parts of science itself.

Appendix 2

17 Neither science nor rationality are universal measures of excellence. They are
214 particular traditions, unaware of their historical grounding.

18 Yet it is possible to evaluate standards of rationality and to improve them.
230 The principles of improvement are neither above tradition nor beyond change and it is impossible to nail them down.

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.

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 concerted assurance with which some intellectuals interfere with the lives of people, and contempt for the treaty 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 alternative.

Ordnung ist heutzutage meistens dort,

wo nichts ist.

Es ist eine Mangelercheinung.

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

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 *Ekenntnis 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: Philosophical Scientific*, 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 ideological. 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 outline⁸ 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 nullifying 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, unhindered 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 Alexievich 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. xcvi. Commenting on these and similar phenomena Stirnberg writes (*Frühwerke*): '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

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 Muzaffer 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 meanness, 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.