Against Method

Fourth Edition

Paul Feyerabend

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Introduction to the Fourth Edition

by Ian Hacking

'*Against Method* is more than a book: it is an event.'[1] That was what it felt like, when the work came out in 1975. Feyerabend was notorious, adored by the young, loathed by the established. The turbulent sixties were winding down, and here was an intellectual testament to the ferment. This was the Woodstock of philosophy. The book should now be read in two ways, both as a part of that era, and also as a contribution to intellectual life in the long term.

There are many lovely things about the book. The first is the Analytical Index, 'Being a Sketch of the Main Argument'. This is not some machine-readable abstract of the type now required by scholarly journals. Paul Feyerabend is telling *you*, in his own plain (and thereby elegant) prose, what he thinks is interesting, chapter by chapter. Yes, it is OK if you skip a couple of chapters, or read the book from back to front. This is not to say you should not read the work sentence by sentence, but the great merit of a book is that you can take it hitch-hiking or to a sit-in, and read a bit while you are munching a few pilfered tomatoes or sheltering from a storm. You can pick up an idea, chase it, and relocate it in the Analytical Index, all the while being in a physical relation to the pages upon which you can scribble expostulations, if that is your wont.

I have been saying 'this book'. That is doubly wrong. First because, as Feyerabend truly said, '*AM* is not a book, it is a collage.'[2] Secondly, because there is more than one collage. There is the first edition of 1975, and the radically revised one of 1988, and then the third edition of 1993, reprinted here. The 1988 version is far more manifestly a collage than that of 1975, although the earlier printing was much more handsome. Feyerabend went on changing the text, but the biggest changes were for 1988. I shall mention some of them below. He is not quite right about the final changes he made, as described in the preface to the third edition below. Yes, the 1988 Chapter 20, not found in 1975, has been dropped. It is all about objectivity and

1. Jean Largeault, at the end of his review of the book in *Archives de Philosophie* 39 (1976), p. 389. This essay, by the most rigorous French philosopher of the sciences of his day, was far more perceptive than most of the English-language reviews at the time.

2. Paul Feyerabend, *Killing Time*, Chicago, 1995, p. 139.

the construction of objects, scientific and other. The 1988 Chapter 19 ('What's so great about science?') has been much extended. He says that he rewrote Chapter 16, which is essentially the 1975 Chapter 17, for the 1988 edition. That is misleading. He chiefly cut six pages from it: a discussion of incommensurability, a topic which, as he indicates in the Preface to the Third Edition, had been worked to death by 1993. And he added the epilogue on relativism, a bone on which he chewed over and over again.

The publishers of all three books are in effect the same, for New Left Books was the original trade name of Verso. Feyerabend had his little battles with New Left Books – see his amusing letters to Imre Lakatos, which include a frustrated cable from Feyerabend in New Zealand to Lakatos in London, dated 2 August 1972.[3] In the 1988 preface, Feyerabend indicated, in broad strokes, what he added, rearranged, or cut from 1975. I am by no means sure he was right to make the changes.

To Imre Lakatos

In 1975 there was a dedication, '*To* IMRE LAKATOS *Friend, and fellow-anarchist.*' It was removed in 1988. Nothing odd about that – Lakatos had died suddenly in 1974. In 1975 there was a single moving paragraph explaining that the book had been intended to be published in tandem with an equally vigorous response by Lakatos, one which was never written. In 1988 Feyerabend put this thought into a longer but not more effective preface. Here he repeated the dedication in clumsy embedded prose: 'I therefore dedicate also this second, already much more lonely version of our common work to his memory.' The 1975 front matter – a dedication on an empty page, followed by a brief paragraph on another empty page – is far more moving, and no less intellectually telling. And so, in my opinion, it goes. *Of course* the publishers could not reprint the first edition rather than the final one. Happily they have now put the 1975 original online.

One fundamental difference is that in 1975 the book had a long Chapter 16 on Lakatos, the ironically named 'fellow-anarchist' of the subtitle. It was deleted and in 1988 the analytical summary of the chapter was reduced to a mere footnote to the preceding chapter.[4] It refers us to

3. *For and Against Method: Imre Lakatos and Paul Feyerabend*, edited by Matteo Motterlini, Chicago, 1999, p. 290.

4. Note 11, p. 161 below.

an adequate but passionless paper printed elsewhere.[5] So the 1988 book is no longer, as Feyerabend put it in 1975, 'a long and rather personal *letter* to Imre' – one such that 'every wicked phrase it contains was written in anticipation of an even more wicked reply from the recipient'.[6]

Lakatos and Popper

Feyerabend speaks for himself. An introduction by someone else is wanted only to suggest the historical setting in which the first book was published. The following notes are for people born after 1975, for whom the event called *Against Method* is somewhere back in prehistory, like the fall of the Berlin Wall. Since the book is dedicated to Imre Lakatos (1922–1974), we can start with their relationship, although that means I shall subsequently have to move backwards in time. For a deeper grasp of the friendship between Lakatos and Feyerabend, dip into *For and Against Method*.[7] It includes 250 pages of rambunctious, irreverent, but astute letters between the two men, written between 1967 and 1974, exactly the period when *Against Method* came into being.

Lakatos, educated in Budapest, had a turbulent youth. Towards the end of the Second World War he led a cell in the Communist resistance against Hungarian fascism. He became an influential figure in the party, was disgraced, jailed, released, and left Hungary after the failed rebellion of 1956. He arrived at the University of Cambridge. His PhD thesis was published as *Proofs and Refutations*. It is one of the most original twentieth-century contributions to the philosophy of mathematics, although it is usually regarded more as pedagogy than as philosophy. He turned to the philosophy of the sciences and gravitated, in 1960, to the London School of Economics, where Karl Popper ruled.

No philosopher of the sciences was more admired by working scientists than Popper. His watchword, or phrase, was 'Conjectures and Refutations'. Science is hypothetico-deductive. Scientists frame conjectures and test their logical consequences. A proposition is scientific if and only if it is falsifiable. Otherwise it is 'metaphysical' – not meaningless or useless, as logical

5. Paul Feyerabend, 'The Methodology of Scientific Research Programmes', in *Problems of Empiricism: Philosophical Papers II*, Cambridge, 1981, pp. 202–31.

6. Paul Feyerabend, *Against Method*, New Left Books, 1975, p. 7 (not numbered).

7. See note 3.

positivists tended to say, but in need of clarification, dialectical analysis, and deep thought, in order to be reworked into something testable. On numerous occasions Feyerabend was to recall that Popper began his class by saying that there is no scientific method. And then (said Feyerabend) he began to go wrong, enunciating, in effect, the method of conjectures and refutations.

In 1969, Lakatos inherited Popper's chair. He devised a 'Methodology of Scientific Research Programmes' which was, in Lakatos's own terminology, a progressive problem shift from Popper's inquiry into the nature of rationality and science. Lakatos continued the tradition of the 'Popper Seminar', a weekly happening during term. Under both Popper and Lakatos it was famous for confrontation. An invited guest seldom got through more than ten minutes of exposition before being subject to violent criticism. A great many people hated the experience and feared what they thought was hostility. Some loved the ambience. One was Feyerabend. I do not know when the two men, Lakatos and Feyerabend, first met; in the early 1960s, perhaps. By 1967, when the published correspondence between them begins, they were soulmates.

In 1965, Lakatos organized an important meeting in London, the first major collective response to Thomas Kuhn's *Structure of Scientific Revolutions* (1962). There were already plenty of conservative criticisms of Kuhn published or in the works. But the core of the meeting was radical criticism from a Popperian and 'post-Popperian' point of view. Lakatos thought that conference proceedings ought to be written after the conference, so that the authors learned from what had gone on. The boring papers of his own conference were published in standard channels, three forgotten volumes, but the main upshot was the memorable *Criticism and the Growth of Knowledge*.[8]

It includes pieces by Feyerabend, Kuhn, Popper, Toulmin – and Lakatos's first sustained statement of his new methodology, which constitutes almost half of the book. Feyerabend says in a note that his own essay for the volume, 'Consolations for the Specialist', was presented at the Popper seminar in 1967, but to judge by later letters to Lakatos, it was still being reworked in 1968. In fact it is not clear which work in progress becomes which final work. He speaks of 'my Kuhn paper'[9] and, six weeks later, 'my latest anti-Parmenidian

8. *Criticism and the Growth of Knowledge*, edited by Imre Lakatos and Alan Musgrave, Cambridge, 1970.

9. *For and Against Method*, p. 120.

paper',[10] both of which the editor of the correspondence identifies as 'Consolations'. A postscript to the first letter says he is thinking of calling his anti-Kuhn paper 'Against Method', adding parenthetically 'this in analogy to Susan Sontag's *Against Interpretation*'.[11] In the autobiography he refers to his 'pro/anti Kuhn paper'.[12] Certainly in 'Consolations' we get Feyerabend's most famous utterance, 'anything goes'.[13] But he published a paper with the title 'Against Method: Outline of an Anarchistic Theory of Knowledge', also in 1970.[14] This really is a preliminary version of parts of the 1975 book.

Now flash backwards some twenty years, to 1952. Feyerabend, in Vienna, arranged to go to Cambridge to study under Wittgenstein. Wittgenstein died in that year. So Feyerabend went to work with Popper at the LSE. His autobiography, *Killing Time*, tells how he quickly established a reputation in the English-speaking world and was offered a job at Berkeley, where he took up a position in the autumn of 1958. He tells how America opened up his life in a way that England never could.

Kuhn and Feyerabend

In his autobiography Feyerabend never even mentions that he soon began having intense conversations with Thomas Kuhn, in which, together, they hatched the idea of incommensurability that was soon to take the world by storm.[15] He rightly connects his thoughts on incommensurability with N. R. Hanson's wonderful but now largely forgotten book, *Patterns of Discovery*, a book that insisted that observational statements are theory-loaded, so that a change in theory implies a change in the meaning even of reports of observations.[16] But he did read a draft of Kuhn's *Structure*

10. Ibid. p. 129.

11. Ibid. p. 125.

12. *Killing Time*, p. 128.

13. 'Consolations for the Specialist', in *Criticism and the Growth of Knowledge*, pp. 197–230, on p. 229.

14. Paul Feyerabend, 'Against Method: Outline of an Anarchistic Theory of Knowledge', *Minnesota Studies in the Philosophy of Science* 4 (1970): pp. 17–130.

15. However the pro/anti Kuhn paper, 'Consolations' (see note 13), begins on p. 197 with the statement: 'In the years 1960 and 1961, when Kuhn was a member of the philosophy department at the University of California in Berkeley, I had the good fortune of being able to discuss with him various aspects of science. I have profited enormously from these discussions and I have looked at science in a new way ever since.'

16. N. R. Hanson, *Patterns of Discovery*, Cambridge, 1958.

'around 1960'.[17] Kuhn as a person is not mentioned until, at the end of the autobiography, he becomes 'my old friend'.[18] In the autobiography there is a photo of the two men sitting side by side, in a café near Zurich.

In other work, of course, he grants that Kuhn had the most important ideas about science outside the LSE circle. (Hanson, who did stunt aerobatics among many other things, died when he crashed his plane in 1967 at the age of forty-two.) By the preface to this third edition of *AM*, you will finally find him speaking of 'Kuhn's masterpiece'. In the autobiography, conversely, Feyerabend has many kind things to say about men such as Rudolf Carnap and Herbert Feigl, one-time logical positivists from Vienna, who together with other German-speaking immigrants and refugees changed the face of American philosophy forever. Elsewhere, he was really quite rude about the Vienna Circle. How about 'rodents of neopositivism' in the 1988 Preface to the Chinese edition?[19]

The differences between the personalities of Kuhn and Feyerabend were profound. One of them is best indicated by two adjectives which out of context would be condescending. Kuhn was dogged, and Feyerabend was flighty. Kuhn gnawed at incommensurability for the rest of his life, and left as yet unpublished material which, in my personal judgement, goes wrong in an attempt to produce a theory of incommensurability that would suit linguists and cognitive scientists. Feyerabend revelled in his off-the-wall illustration of the incommensurability of archaic and ancient Greek systems of thought.

Anything goes

Feyerabend will be forever cursed by a statement of his own making, and for which he is fully responsible, the notorious aphorism 'anything goes'. In the Chinese preface, he says it is 'the terrified exclamation of a rationalist who takes a closer look at history'. Yet he would sometimes argue, not in terror but with delight, that even Lakatos's methodology shared with Feyerabend's anti-methodology 'a position of "anything goes" '.[20]

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17. *Killing Time*, p. 141.

18. Ibid. p.162.

19. In case you think he could not possibly have meant the Vienna Circle by this term of abuse, compare the passage here with the diagram in a letter to Lakatos, *For and Against Method*, p. 245.

20. Ibid., p. 229.

Since the aphorism is often taken to be anti-science, a sort of New Age waffle, we must emphasize that Feyerabend never meant for one minute that anything *except* the scientific method (whatever that is) 'goes'. He meant that lots of ways of getting on, *including* the innumerable methods of the diverse sciences, 'go'. He also meant that an anti-rationalist, like himself, was perfectly entitled to use rationalist arguments to discomfit the rationalists whom he opposed. What he did dislike was any kind of intellectual or ideological hegemony. His favoured text was Mill's *On Liberty*, even if his preferred style was Dada. Single-mindedness in pursuit of any goal, including truth and understanding, yields great rewards. But single vision is folly if it makes you think you see (or even glimpse) *the* truth, the one and only truth. Hence the need for the counter-irritant maxim 'anything goes'.

Anarchism and Dada

For some time Feyerabend cheerfully accepted Lakatos's label 'anarchist'. On 10 October 1970 he wrote to Lakatos that he considered saying in the Preface to *Against Method*: 'I am for anarchism in *thinking*, in one's *private life*, BUT NOT in *public life*.'[21] He did not insert that thought, but he went one better: in 1975, *Against Method* had a subtitle, *Outlines of an anarchistic* theory of knowledge*. Yes, with a footnote to the subtitle, directing the reader to explanations in the text of the term 'anarchism', including the passage that I shall quote in a moment.

The subtitle was abandoned in 1988, although the initial discussion of anarchism in the Introduction is much the same. Moreover, that footnote to the subtitle referred to a chapter that was deleted, though to some extent pasted back here and there in 1988. An important part of that chapter is a discussion of the relation between Dada and intellectual anarchism. The footnote to the subtitle also referred to a very long footnote 12 in the Introduction to the 1975 book. Feyerabend wrote it out in a letter to Lakatos, 7 August 1972.[22] Since it was deleted in 1988, I shall quote it here.

> When choosing the term 'anarchism' for my enterprise I simply followed general usage. However anarchism, as it has been practised in the past and as it is being practised today by an ever increasing number of people has

21. Ibid., p. 219.
22. Ibid., pp. 294–5.

features I am not prepared to support. It cares little for human lives and human happiness (except for the lives and the happiness of those who belong to some special group); and it contains precisely the kind of Puritanical dedication and seriousness which I detest. (There are some exquisite exceptions such as Cohn-Bendit,[23] but they are in the minority.) It is for these reasons that I now prefer to use the term *Dadaism*. A Dadaist would not hurt a fly – let alone a human being. A Dadaist is utterly unimpressed by any serious enterprise and he smells a rat whenever people stop smiling and assume that attitude and those facial expressions which indicate that something important is about to be said. A Dadaist is convinced that a worthwhile life will arise only when we start taking things *lightly* and when we remove from our speech the profound but already putrid meanings it has accumulated over the centuries ('search for truth'; 'defence of justice'; 'passionate concern'; etc., etc.). A Dadaist is prepared to initiate joyful experiments even in those domains where change and experimentation seem to be out of the question (example: the basic functions of language). I hope that having read the pamphlet[24] the reader will remember me as a flippant Dadaist and *not* as a serious anarchist.

Let us remember him, then, as a Dadaist. Lakatos objected to the claim that a Dadaist would never hurt a fly: sometimes a Dadaist has to do harm when it is the lesser of two evils. Lakatos undoubtedly had in mind a controversial incident in his own past as a resistance fighter, when he compelled his cell to order a young woman to take poison because if caught she would compromise the group. Feyerabend said he accepted Lakatos's criticism. But the argument seems to me not to harm Feyerabend's footnote but, rather, to show that there are times when Dada is not enough. Feyerabend had the moral luck to have been physically injured in war but

23. Daniel Cohn-Bendit (born 1945) was a central personality in the Paris student uprisings, May 1968, denounced alike by the Gaullist right and the communist left. In the 1970s he edited a German magazine of anarchist orientation, which increasingly moved toward environmental politics. In 1994 he was elected to the European Parliament as a green, and in 1999 he became leader of the French Green Party. In the June 2009 elections to the European Parliament he was extraordinarily popular with French voters. The right despises him as favouring immigrants, lessening penalties on drugs, and general welfarism, while the left hates his policies of armed intervention in the former Yugoslavia and his fierce support for the European Union.

24. The 'pamphlet' is of course *Against Method*: this is the word Feyerabend used in writing to Lakatos, and he left the paragraph intact when he put it in to the book.

(so he made out) never morally touched. He had the privilege of being able to practise a kind of Dada throughout his life. But it is important to insist that Dada implies passion, not indifference. It may help to understand this by quoting 'a letter to the reader' which was intended to precede his last book, *The Conquest of Abundance*. Although written for another purpose, it can usefully be read by those embarking on *Against Method*.[25]

FEYERABEND'S LAST LETTER

Dear reader,

In a few pages you will find a story written in a style you may be familiar with. There are facts and generalizations therefrom, there are arguments and there are lots of footnotes. In other words, you will find a (perhaps not very outstanding) example of a scholarly essay. Let me therefore warn you that it is not my intention to inform, or to establish some truth. What I want to do is to change your attitude. I want you to sense chaos where at first you noticed an orderly arrangement of well behaved things and processes. It is clear that only a trick can get me from my starting point – the footnote-heavy essay I just mentioned – to where I would like you, the reader, to arrive.

My trick is to present events which dissolve the circumstances that made them happen. Given the circumstances the events are absurd, unheard of, frightening, evil – they simply do not make sense. I take a closer look at the circumstances and find features that may be regarded as anticipations of the event. The features are not unknown; they are not hidden either; however, they can be read in a variety of ways and only some readings create trouble.

The absurdity is therefore not laid out in advance; it is created by living in a certain way – and so is the sense perceived by those who produce the disruptive event. What is interesting is that both parties use the same material; they start from the same life, but they continue it in different directions. (The same applies to the scholars who years and even centuries later try to figure out 'what really happened'.)

I conclude that the life we lead is ambiguous. It contains not only one future, but many, and it contains them neither ready-made nor as possibilities that can be turned into any direction. It is not at all different

25. The letter was found by Grazia Borromini-Feyerabend on a disc on 11 October 1999, and was first printed in the London Review of Books, 22 June 2000, p. 28.

from a movie, or a specially constructed play. Imagine such a play. It has gone on for about forty minutes. You know the characters, you have become accustomed to their idiosyncrasies, and you are already tired of their peculiar habits. Now they stand before you with their familiar gestures and it seems that nothing interesting is ever going to happen – when suddenly, because of a trick used by the writer, the 'reality' you perceived turns out to be a chimaera. (Alfred Hitchcock, Anthony Shaffer and Ira Levin are masters of this kind of switch.) Looking back you can now say that things were not what they seemed to be, and looking forward with the experience in mind you will regard any clear and definite arrangement with suspicion, on the stage, and elsewhere. Also, your suspicion will be the greater the more solid the initial story seemed to be. This is why I have chosen a scholarly essay as my starting point.

It is very important not to let this suspicion deteriorate into a truth, or a theory, for example into a theory with the principle: things are never what they seem to be. Reality, or Being, or God, or whatever it is that sustains us cannot be captured that easily. The problem is not why we are so often confused; the problem is why we seem to possess useful and enlightening knowledge.

You must also resist the temptation to classify what I say by giving it a well-established name, for example the name of relativism. Relativism as defined by philosophers and sociologists is much too definite a view to fit the situation – unless it is regarded as a passing chimaera, or as a rule of thumb. You cannot even deny the existence of eternal truths unless the denial is again meant as a cautionary hint given to those visiting the theatre of life. Is argument without a purpose? No, it is not; it accompanies us on our journey without tying it to a fixed road. Is there a way of identifying what is going on? There are many ways and we are using them all the time though often believing that they are part of a stable framework which encompasses everything. Is there a name for an attitude or a view like this? Yes, if names are that important I can easily provide one – mysticism – though it is a mysticism that uses examples, arguments, tightly reasoned passages of text, scientific theories and experiments to raise itself into consciousness.

This, my dear reader, is the warning I want you to remember from time to time, and especially when the story seems to become so definite that it almost turns into a clearly thought out and precisely structured point of view.

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.

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 that 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

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

people try again to adapt 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.

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 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

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

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 Michael 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 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

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.
7. J. Dupré, 'The Disunity of Science', *Mind* 92, 1983.
8. Present edition. Taken over unamended from first edition.

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), and 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 cultures'.[14] In this connection I examined the practical aspects of logic, the way, that is, in which ideas are related to

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.

14. AM, first edition, p. 274.

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'

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.

can no longer exclude 'unscientific' projects – we have to look at matters in 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 diagnostics) 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 patients, 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

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.

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 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

Analytical Index

Being a Sketch of the Main Argument

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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 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.

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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.*

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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.

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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.*

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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.*

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

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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 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.)

clear that successful *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

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 Buchgesell-schaft, 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

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.

explanation of all phenomena,' writes Kropotkin.[9] 'Its method of investigation is 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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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".'

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.