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Farewell to Reason

The German version of this essay was based on the third German edition of Against Method *(AM for short) which differs from the English, the French, the Japanese and the Portuguese editions and was published in 1986.* Erkenntnis Für freie Menschen *(EFM for short) is the largely (two-thirds) rewritten German edition of* Science in a Free Society *(SFS). It does not contain the chapters on Kuhn, Aristotle and Copernicus and the replies to critics which constituted more than half of the English text. Instead there is a more detailed explanation of the relation between reason and practice, an extended chapter on relativism and a sketch of the rise of rationalism in antiquity. The criticisms on which I comment were published in H. P. Duerr (ed.)* Versuchungen, *2 vols, Frankfurt 1980/81.*

1 Survey

This chapter deals with the following topics: the structure of scientific reasoning and the role of a philosophy of science; the authority of science compared with other forms of life; the importance of such other forms of life; the role of abstract thought (philosophy, religion, metaphysics) and abstract ideals (humanitarianism, for example). It also contains replies to critical essays that appeared in German in 1980 and clarifies points made in AM and EFM.

2 The Structure of Science

My main thesis on this point is: the events and results that constitute the sciences have no common structure; there are no elements that occur in every scientific investigation but are missing elsewhere (the objection that without such elements the word 'science' has no meaning assumes a theory of meaning that has been criticized, with excellent arguments, by Ockham, Berkeley and Wittgenstein).

Concrete developments (such as the overthrow of steady state cosmologies or the discovery of the structure of DNA) have of course quite 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, and the moves that advance it are not always known to the movers. A theory of science that devises standards and structural elements of *all* scientific activities and authorizes them by reference to some rationality-theory 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. The most we can do for them from afar is to enumerate rules of thumb, give historical examples, present case studies containing diverging procedures, demonstrate the inherent complexity of research and so prepare them for the morass they are about to enter. Listening to our tale, scientists will get a feeling for the richness of the historical process they want to transform, they will be encouraged to leave behind childish things such as logical rules and epistemological principles and to start thinking in more complex ways – and this is all we can do *because of the nature of the material*. A 'theory' of knowledge that intends to do more loses touch with reality. Not only *are* its rules not used by scientists, they *cannot* possibly *be* used in all circumstances — just as it is impossible to climb mount Everest using the steps of classical ballet.

The ideas just presented (and illustrated with historical examples in AM and in my *Philosophical Papers*, Cambridge 1981) are not new. As I wrote in section 4 of chapter 6, we find them in philosophers like Mill (his *On Liberty* – the outstanding presentation of a libertarian epistemology), in scientists such as Boltzmann, Mach, Duhem, Einstein and Bohr, and then, in a

philosophically already quite desiccated way, in Wittgenstein. They were fruitful ideas: the revolutions of modern physics, relativity and quantum mechanics and the later changes in psychology, biology, biochemistry and high energy physics would have been impossible without them. Yet they had only a slight impact on philosophy. Even the most inconoclastic philosophical movement of the time, neopositivism, still clung to the ancient idea that philosophy must provide general standards for knowledge and action and that science and politics can only profit from adopting such standards. Surrounded by revolutionary discoveries in the sciences, interesting points of view in the arts and unforeseen developments in politics, the stern fathers of the Vienna Circle withdrew to a narrow and badly constructed bastion. The connection with history was dissolved; the close collaboration between scientific thought and philosophical speculation came to an end; terminology alien to the sciences and problems without scientific relevance took over.

Fleck, Polanyi and then Kuhn were (after a long time) the first thinkers to compare the resulting school philosophy with its alleged object – science – and to show its illusionary character. This did not improve matters. Philosophers did not return to history. They did not abandon the logical charades that are their trademark. They enriched these charades by further empty gestures, most of them taken from Kuhn ('paradigm', 'crisis', 'revolution', and so on) without regard for context, and thus complicated their doctrine; but they did not bring it closer to reality. Pre-Kuhnian positivism was infantile, but relatively clear (this includes Popper who is just a tiny puff of hot air in the positivistic teacup). Post-Kuhnian positivism has remained infantile – but it is also very unclear.

Imre Lakatos was the only philosopher of science to take up Kuhn's challenge. He fought Kuhn on his own ground and with his own weapons. He admitted that positivism (verificationism, falsificationism) neither enlightens scientists nor aids them in their research. However, he denied that stepping closer to history forces us to relativize all standards. This may be the reaction of a confused rationalist who for the first time faces history in its full splendour but, so Lakatos said, a more thorough study of the same material shows that scientific processes share a structure and obey general rules. We can have a theory of science and, more generally, a theory of rationality because thought enters history in a lawful way.

In AM as well as in chapter 10 of vol. 2 of my *Philosophical Papers* I tried to refute that thesis. My procedure was partly abstract, consisting in a criticism of Lakatos' interpretation of history, and partly historical. Some critics deny that the historical examples support my case: their objections will be dealt with below. However, if I am correct – and I am pretty sure I am – then it is necessary to return to the position of Mach, Einstein and Bohr. A theory of science is then impossible. All we have is the process of research and, side by side with it, all sorts of rules of thumb which *may* aid us in our attempt to further the process but which may also lead us astray. (What are the criteria that inform us that we have been misguided? They are criteria which seem to fit the situation at hand. How do we determine fitness? We *constitute* it by the research we do: criteria do not merely *judge* events and processes, they are often constituted by them and they must be introduced in this manner or else research could never get started: AM, 26.)

This is my simple answer to various critics who either chastise me for opposing theories of science and yet developing a theory myself, or take me to task for failing to give a 'positive determination of what good science consists in': if a collection of rules of thumb is called a 'theory', well, then of course I have a 'theory' – but it differs considerably from the antiseptic dream castles of Kant and Hegel and from Carnap's and Popper's dog huts. Mach, Einstein and Wittgenstein, on the other hand, lack a more impressive edifice of thought not because they are lacking in speculative power, but because they have realized that freezing this power into a system would mean the end of the sciences (the arts, religion, and so on). And the natural sciences, especially physics and astronomy, enter the argument not because I am 'fascinated by them', as some confused champions of the humanities remarked, but because they are the issue: they were the weapons which the positivists and their anxious foes, the 'critical' rationalists, trained on unloved philosophies, and they are the weapons which now cause their own demise instead. Nor do I speak of progress because I believe in it or pretend to know what it means (using a *reductio ad absurdum* does not commit the arguer to accepting the premises: cf. AM, p. 27). As for the slogan 'anything goes', which certain critics have attributed to me and then attacked: the slogan is not mine and it was not meant to summarise the case studies of AM and SFS. I am not looking

for new theories of science, I am asking if the search for such theories is a reasonable undertaking and I conclude that it is not: the knowledge we need to understand and to advance the sciences does not come from theories, it comes from participation. The examples, accordingly, are not details that can and should be omitted once the 'real account' is given – they *are* the real account. The critics, holding a belief I explicitly reject (that there can be a theory of science and of knowledge), read only part of my story and they read it in a way that is contradicted by the rest. Small wonder they are baffled by the result.

Similar remarks apply to readers who accept the slogan and interpret it as making research easier and success more accessible. My objection to these lazy 'anarchists' is again that they misread my intentions: 'anything goes' is not a 'principle' I defend, it is a 'principle' forced upon a rationalist who loves principles but who also takes history seriously. Besides, and more importantly, an absence of 'objective' standards does not mean less work; it means that scientists have to check *all* ingredients of their trade and not only those which philosophers and establishment scientists regard as characteristically scientific. Scientists can thus no longer say: we already have the correct methods and standards of research – all we need to do is to apply them. For according to the view of science that was defended by Mach, Boltzmann, Einstein and Bohr, and which I restated in AM, scientists are not only responsible for the correct *application* of standards they have imported from elsewhere, they are responsible *for the standards themselves*. Not even the laws of logic are exempt from their scrutiny for circumstances may force them to change logic as well (some such circumstances have arisen in the quantum theory).

This situation must be kept in mind when considering the relation between 'great thinkers' on the one hand and editors, moneybags and scientific institutions on the other. According to the traditional account, scientists with uncommon ideas and the institutions from which they seek support have certain general ideas in common: they are both 'rational'. All a scientist in search of money has to do is to show that his research, apart from containing novel suggestions, conforms to these ideas. According to the account defended by me, the scientists and their judges must first establish some common ground – they can no longer rely on standard slogans (their exchange is 'free', not 'guided': cf. SFS, p. 29).

In such a situation the demand of 'anarchic' scientists for

greater freedom can be interpreted in two ways. It can be interpreted as a demand for an open exchange that seeks understanding without being tied to specific rules. But it can also be interpreted as a demand for acceptance without examination. In the terms of AM and SFS, the latter demand might even be supported by pointing out that ideas that were once regarded as absurd have subsequently led to progress. The argument overlooks that the judges, editors or moneybags can use the same reasons: the status quo, too, has led to progress and 'anything goes' includes the methods of its defenders. It is therefore necessary to offer a little more than arrogance and vague generalities.

The case studies show that scientific rebels took this extra step. Galileo, for example, did not just complain, he tried to convince his opponents with the best means at his disposal. These means frequently differed from standard professional procedures, they even conflicted with commonsense – here is the anarchic component of Galileo's research; but they had a reason of their own which could be expressed in commonsense terms and they were occasionally successful. And let us not forget that a full democratization of science will make life even more difficult for the self-proclaimed discoverers of Great Ideas, who will then have to address people who do not even share their interest in science or research. What will our freedom-loving 'anarchists' do under such circumstances? When their opponents are no longer hated big-shots but much beloved free citizens?

3 Case Studies

In this section I shall deal mainly with objections to my treatment of Galileo. Let me repeat that I criticize not Galileo's procedures – which are excellent examples of the inventiveness of scientific practice mentioned in section 2 – but those philosophical theories which, if applied with a better knowledge of history, would have to reject them as 'irrational'. Galileo was irrational in terms of these theories – but he was also one of the greatest scientist-philosophers who ever lived.

According to Gunnar Andersson, the Galileo case may endanger an 'overly simple and naive version of falsificationism' – but it does not threaten a philosophy where both theories and observations are fallible. My interpretation of Galileo's assumptions

further reveals, according to Andersson, that I have not understood Popper's definition of *ad hoc* hypotheses. *Ad hoc* hypotheses, says Andersson, are not merely introduced to explain special effects; they also lower the degree of falsification of the system in which they occur.

Now that is precisely what Galileo's assumptions do. Galileo's account of motion turns the tower argument[1] from a refutation of Copernicus into a confirming instance and reduces the content of the Aristotelian dynamics that preceded it (AM, pp. 99f). This latter theory (explained in books i, ii, vi, and viii of the *Physics*) deals in a general way with a variety of changes including locomotion, generation, corruption, qualitative change (such as the transmission of knowledge from a well informed teacher to an ignorant pupil – an example often used by Aristotle), increase and decrease. It contains theorems such as: every motion is preceded by another motion; there exists a hierarchy of motions which starts from an unmoved cause of motion, is followed by a primary motion with constant (angular) velocity and branches out from there; the length of a moving object has no precise value – ascribing to an object a precise length means assuming that it is at rest; and so on. The first theorem was proved by assuming that the world is a lawful entity. (The proof can be used today against the Big Bang theory of the origin of the universe or against Wigner's idea that the reduction of the wave packet is due to an act of consciousness.) The last theorem which is based on Aristotle's account of continuity anticipates basic ideas of the quantum theory (cf. chapter 8 for details).

Aristotle's theory of motion is coherent and it was confirmed to a high degree. It stimulated research in physics (electricity – cf. J. L. Heilbron, *Electricity in the 17th and 18th Centuries*, University of California Press 1979), physiology, biology and epidemiology down to the late nineteenth century and it remains relevant today: the mechanical views of the seventeenth and eighteenth centuries and their modern sequels are incapable of dealing even with their own prize process, locomotion (cf. the work of Bohm and Prigogine as well as chapter 8 of the present

1. According to the Tower Argument (AM, chapter 7) a stone dropped from a tower on a moving earth will be left behind. It is not left behind, hence the earth does not move. The argument assumes (Aristotle's law of inertia) that an object outside the reach of forces remains at (returns to) rest. At the time of the debate this assumption was confirmed. It was used for a considerable time after the Copernican Revolution to establish the existence of flies' eggs, bacteria, viruses.

book). What did Galileo do? He replaced this complex and sophisticated theory, which already contained the distinction between laws of inertia (they describe what happens when no forces are acting) and laws of forces (they describe how forces influence motion), by his own law of inertia which lacked corroboration, applied to locomotion only and 'drastically reduced the degree of falsification of the entire system.'

Concerning the falsifiability of observational statements the situation is, however, as follows. Critical rationalism, the 'philosophy' defended by Andersson, is either a fruitful point of view that guides scientists, or it is empty talk that can be reconciled with any procedure. Popperians say it is the first (rejection of Neurath's assertion that any statement can be crossed out for any reason whatsoever). This is why they insist that basic statements intended to refute a theory must be highly corroborated. Galileo's telescopic observations did not satisfy this demand: they were self-contradictory, not everybody could repeat them, those who did repeat them (Kepler) got puzzling results and there existed no theory to separate 'phantoms' from veridical phenomena (physical optics, mentioned by Andersson, is irrelevant; the basic statements under discussion are not about rays of light but about the position, colour and structure of visual patches, and a popular hypothesis correlating the first with the second could be easily shown to be false: cf. AM, p. 137). Galileo's basic statements are therefore bold hypotheses without much corroboration. Andersson accepts this description – it needs time, he says, to obtain the corroborating evidence (and the related touchstone theories, to use an excellent expression of Lakatos's). The first interpretation of critical rationalism mentioned above asserts that during the search the statements have no refuting power. If one still says, as Andersson does, that Galileo refuted popular views by his observations, then one moves from the first interpretation to the second, where basic statements can be used in any way whatsoever. The verbiage remains critical – but its content has evaporated.

Next comes a criticism which T.A. Whitaker has published in two letters in the journal *Science* (May 2 and October 10, 1980). Whitaker points out that there exist two sets of pictures of the moon, the woodcuts (which I presented in AM) and the copperplates which are more accurate, from a modern point of view. The copperplates, says Whitaker, show Galileo to be a better observer of the moon than I make him out to be.

Now, first of all, I never doubted Galileo's ability as an

observer. Quoting R. Wolf, who writes that 'Galileo was not a great astronomical observer; or else the excitement of the many telescopic observations made by him at the time had temporarily blurred his skill or his critical sense', I reply (AM, p. 129) that

> this assertion may well be true (though I rather doubt it in view of the extraordinary observational skill which Galileo exhibits on other occasions). But it is poor in content and, I submit, not very interesting . . . There are however other hypotheses which lead to new suggestions and which show us how complex the situation was at the time of Galileo.

I then mention two such hypotheses, one dealing with general features of contemporary telescopic *vision*, the other considering the assumption that perceptions, i.e. the things seen with the naked eye, have a *history* (which may be discovered by combining the history of visual astronomy with the history of painting, poetry, etc.).

Secondly, reference to the copperplates does not remove all the troublesome aspects of Galileo's observations (of the moon). Galileo not only drew pictures, he also gave verbal descriptions. For example, he asked (cf. AM, p. 127): 'Why don't we see unevenness, roughness and waviness in the waxing moon's outmost periphery which faces West, in the waning moon's other circular edge which faces East and in the full moon's entire circumference? Why do they appear perfectly round and circular?' Kepler replied, on the basis of naked-eye observations (cf. AM, p. 127 fn. 24): 'If you look carefully at the moon when it is full, it seems perceptibly to be lacking in roundness', and he answered Galileo's question by saying: 'I do not know how carefully you have thought about this subject or whether your query, as is more likely, is based on popular impression. For . . . I stated that there was surely some imperfection in this outermost circle during full moon. Study the matter again and tell us how it looks to you.'

This little exchange shows, thirdly, that the problems of observation which existed at Galileo's time cannot be solved by showing that Galileo's observations agree with *our* view of the matter. To see how Galileo proceeded, if he was 'rational' or if he broke important rules of scientific method, we have to compare his achievements and his suggestions with *his* surroundings and *not* with the situation in an as yet unknown future. If it turns out that the phenomena reported by Galileo were not confirmed by anyone else, and that there were no reasons for

trusting the telescope as an instrument of research but many reasons, both theoretical and observational, that spoke against it, then it was as unscientific for Galileo to push the phenomena as it would be unscientific for us to push experimental results which lack independent corroboration and are obtained by doubtful methods – no matter how closely his observations approach our own. For to be scientific in the sense that is at issue here (and that *is criticized* in AM and SFS) means to act properly with respect to existing and not with respect to possible knowledge.

Now I used the woodcuts in order to gauge the reactions of Galileo's contemporaries. Note again that I did not try to argue that Galileo was a lousy scientist because the woodcuts differ from modern pictures of the moon – such an argument would have conflicted with the considerations just given. My assumption was, rather, that the moon as seen with the naked eye looks different from the woodcuts, that it might have looked different to Galileo's contemporaries and that some of them might have criticised the *Sidereus Nuncius* on the basis of their own naked-eye observations. This assumption is still useful, for the woodcuts accompanied most editions of the book. Does it apply to the engravings as well? It does, as is shown by Kepler's criticism.

There were, moreover, many reasons why the telescope was not uniformly regarded as a reliable producer of facts (some of these reasons, both empirical and theoretical, are assembled in AM). Whitaker's assertion, made in his second communication, that Galileo's drawings of the moon are of a high quality when compared with modern pictures has no bearing on this discussion.

John Worral ascribes to me the 'truism that "theoretical facts" are dependent on theory' as well as arguments that 'depend on taking "fact" at a very high theoretical level'. What I actually assert in the paper in which these matters are explained (now reprinted as chapter 2 of vol. 1 of my *Philosophical Papers*) is that *all* facts are *theoretical* (or, in the formal mode of speech, 'logically speaking all terms are "theoretical" ' – op. cit., p. 32, fn. 22) and not merely theory-*laden*. I also argue for this assertion and show that and why it is preferable to alternatives, including the alternative Worral seems to have in mind. Worral's complaints nowhere touch this position and these arguments.

John Worral's difficulties show how little Popperians have advanced beyond more naive forms of empiricism. Worral wants

to distinguish empirical facts and theoretical facts but he does not know how to proceed. Occasionally he proceeds psychologically, i.e. he distinguishes between facts that are accepted by all experts in a certain domain and more doubtful facts which give rise to debates. This Carnap (in *Testability and Meaning*) and I (in section 2 of the abovementioned paper) had done before him, and in a much clearer way. On other occasions he seems to assume that agreements reached go beyond psychology and are grounded in the facts themselves: empirical facts are less pervaded by theory than theoretical facts, they have an 'empirical core'. Neurath, Carnap and I would say that such facts *appear* to be less pervaded with theory: the old Greeks perceived their gods directly – the phenomena did not contain any theoretical element – but philologists eventually discovered the complex ideology at their basis and showed how even very simple divine 'facts' were constituted by a highly complex structure (AM, Ch. 17). Classical physicists described and we still describe our surroundings in a language which neglects the relation between observer and observed object (we assume stable and unchanging things, we base our experiments upon them) but the theory of relativity and the quantum theory showed that this language, this mode of perception and this manner of carrying out experiments rested on cosmological assumptions. The assumptions were not explicitly formulated – this is why we don't notice them and simply speak of empirical 'facts' – but they underlie all phenomena: the apparently empirical 'facts' are theoretical through and through. Yet they frequently function as judges between alternative views.

Worral assumes that judges must be neutral (hence the need for a solid empirical 'core') – i.e. he assumes that scientists who use facts when examining a variety of theories do not change them in the course of the examination. The assumption can easily be shown to be wrong. Relativists and aether theoreticians have different facts, even in the domain of observation. For the relativist, observed masses, lengths, or time intervals are projections of four-dimensional structures into certain reference systems (cf. Synge in de Witt and de Witt, *Relativity, Groups and Topology*, New York 1964), while the 'absolutists' regard them as inherent properties of physical objects. Relativists admit that classical *descriptions* (which were designed to express classical facts) may occasionally be used to convey information about relativistic facts and they employ them in the relevant circumstances. But this does not mean that they accept their classical *interpretation*.

On the contrary, their attitude is very close to that of a phychiatrist who talks to a patient claiming to be possessed in his, the patient's, language, without accepting an ontology of devils, angels, demons and so on: our ordinary ways of speaking, scientific arguments included, are much more elastic than is imagined by Worral.

The tower argument, according to Worral, was defused by Galileo in the following way: the moving earth taken in conjunction with the Aristotelian theory of motion (according to which an object not under the influence of forces comes to rest) increases the distance between the stone and the tower. The stone does not move away from the tower. Therefore, says Worral's Galileo, 'the experiment does not refute Copernicus, but a more complex theoretical system' and he replaces Aristotle's dynamics, which is part of this system, by his own law of inertia. Here he remains within the framework of Duhem's analysis of theory-change. More especially, he corrects a 'logical error' of the anti-Copernicans according to whom the false statement (the stone moves away from the tower) follows directly from the assumption that the earth rotates. So far John Worral.

Now first of all, the alleged 'logical error' was never committed by the anti-Copernicans. Being good Aristotelian logicians they knew very well that the derivation needed at least two premises. They even mentioned them explicitly, but they directed the arrow of falsification against only one premise – the motion of the earth – as the other was theoretically plausible and confirmed to a high degree and, besides, it was not the topic at issue (cf. Popper's comments on Duhem's argument against simple falsifiability). Secondly, the replacement of Aristotle's law of inertia was only part of the change carried out by Galileo. The Aristotelian law described absolute motions – and so did the tower argument (the predicted deviation of the stone from the tower is of course a relative change, but the problem under debate here is what Galileo changed and not what reasons he used when carrying out the change). If a new 'auxiliary hypothesis' is introduced then this hypothesis, too, must use absolute motions: it must be a form of the impetus theory. But Galileo gradually became a kinematic relativist (AM, p. 78 fn. 10; p. 96, fn. 15). *His* auxiliary hypothesis had to work *without* impetus. Thus he did not merely change one *hypothesis* of an otherwise unchanged conceptual system (absolute motion around the earth, or around the sun, but not straight towards the centre); he also replaced the *concepts* of the system – he introduced a new world view (which

had been prepared by others). The first process can be explained by Duhem's scheme, the second cannot.

Worral also criticizes the way in which I use Brownian motion to argue for a plurality of theories. This criticism is such a wonderful example of the shortcomings of a purely philosophical approach (as described by me in Vol. 2, chapter 5 of my *Philosophical Papers*) that it deserves our fullest attention.

In chapter 3 of AM, I showed that Brownian motion contradicts the second law of phenomenological thermodynamics only when analysed by the kinetic theory which also contradicts that law. Worral says he does not understand my argument. So far so good. There are many things which many people do not understand. In order to understand the argument, Worral translates it into a language he is familiar with, a kind of pidgin logic. This, too, is unobjectionable: if I don't understand an argument, then I shall try to reformulate it in my own way. Worral goes further. He complains that I did not formulate my argument in his language in the first place. But my argument was not part of a personal letter to him, it was addressed to physicists favouring a theoretical monism – and they seem to understand it perfectly. Besides, Worral does not just object to having been left out, he assumes that the language he understands is the only reasonable language there is. In this he is certainly mistaken, as is shown by the nonsense his translation produces (his notion of evidence, for example, makes it impossible to speak of unknown evidence or of events which, although well known, and although evidence, are not known to be evidence). Like a native speaker of a language too poor to express certain states of affairs, he projects the lacuna on to my argument and claims to have shown its incoherence. I, on the other hand, would conclude that there are better languages than pidgin logic. Using one such language my argument can be stated as follows.

Assume we have a theory T (and by this I mean the entire complex: theory plus initial conditions plus auxiliary hypotheses and so on). T says that C will occur. C does not occur, C' occurs instead. If this fact were known then one could say that T was refuted and C' would be the refuting evidence (note that I do not distinguish between facts and statements; no step in the argument depends on the distinction and no intelligent person will be confused by its absence). Assume further that there are laws of nature which prevent us from directly separating C and C': there exists no experiment that could tell us the difference. Finally, let us assume that it is possible to identify C' in a roundabout

manner, with the help of special effects which occur in the presence of C', but not in the presence of C, and which are postulated by an alternative theory T'. An example of such an effect would be that C' triggers a macroprocess M (Worral has difficulties with 'triggers': any dictionary will tell him what the word means). In this case T' gives us evidence against T that could not have been discovered by using T and the associated experiments only: for God, M or C' are evidence against T; we humans, however, need T' to ascertain that fact.

Brownian motion is a special case of the situation I have just described: C are the processes in an undisturbed medium in thermal equilibrium according to the phenomenological theory of thermodynamics; C' are the processes in such a medium according to the kinetic theory. C and C' cannot be experimentally distinguished because any instrument for the measurement of heat contains the very same fluctuations it is supposed to reveal in our special case. M is the motion of the Brownian particle, T' the kinetic theory. As in the Galilean case, we can press these elements into the Duhemian scheme by saying that one auxiliary hypothesis was replaced by another auxiliary hypothesis and that some difficulty was removed thereby. Note, however, that in our case it was not the difficulty that led to the replacement but the replacement that helped us find the difficulty – and *this* feature disappears in Worral's analysis.

Turning to more general objections, I wholeheartedly agree with Ian Hacking that the sciences are more complex and many-sided than I assumed in some of my earlier writings and also in parts of AM. I had simplistic ideas both about the elements of science and about their relations. Science does contain theories – but theories are neither its only ingredients nor can they be adequately analysed in terms of statements or other logical entities. We may admit that there exist axiomatic formulations and that some scientific ideas have been defined in a precise way; we may also admit that scientists when doing research occasionally rely on the results of these efforts. However, they use them in a rather loose way, combining axioms from different domains in a manner liable to give a heart attack to philosophers entranced by simple forms of logic. Logic itself has now entered a stage where formalizations are used in a freewheeling way and where 'anthropological' considerations (finitism) play an important role. Altogether the scientific enterprise seems to be much closer to the arts than older logicians and philosophers of science (myself

among them) once thought (for this side of the matter cf. my essay *Wissenschaft als Kunst*, Frankfurt 1984).

My first doubts about the identification of science with the explicit features of its theories and its observational reports arose in 1950 when I read a manuscript copy of Wittgenstein's *Philosophical Investigations*. I still expressed these doubts abstractly, in terms of conceptual problems (incommensurability; 'subjective' elements of the theory of explanation). Starting work on chapter 17 of AM, I was then led to question the adequacy of abstract procedures both in the sciences and in the philosophy of science. Here I learned from three books: Bruno Snell's magnificent *Discovery of the Mind*, recommended to me by Barbara Feyerabend; Heinrich Schaefer's *Principles of Egyptian Art*, a book of importance far beyond the subject matter treated; and Vasco Ronchi's *Optics, the Science of Vision*. Today I would add Panofsky's writings on the history of the arts (especially his pathbreaking essay *Die Perspektive als Symbolische Form*) and Alois Riegl's *Spätrömische Kunstindustrie* where the doctrine of artistic relativism is explained simply, and with powerful arguments. All I had to do to extend these arguments to the sciences was to realise that scientists, too, produce works of art — the difference being that their material is thought, not paint, nor marble, nor metal, nor melodious sound.

Regarding thought itself, I started my escape from positivism by distinguishing between two different kinds of traditions which I called abstract traditions and historical traditions respectively (details in ch. 1, Vol. 2 of my *Philosophical Papers*, and in *Wissenschaft als Kunst*, as well as in chapter 3 of the present book). There are many ways of characterising these traditions. One difference which I found a most helpful starting point is the way in which the two traditions deal with their objects (people, ideas, gods, matter, the universe, societies – and so on).

Abstract traditions formulate statements. The statements are subjected to certain rules (rules of logic; rules of testing; rules of argument – and so on) and events affect the statements only in accordance with the rules. This, it is said, guarantees the 'objectivity' of the information conveyed by the statements, or of the 'knowledge' they contain. It is possible to understand, criticize, or improve the statements without having met a single one of the objects described (examples: elementary particle physics; behavioural psychology; molecular biology which can be run by people who never in their life saw a dog, or a prostitute).

Members of *historical traditions* also use statements, but they

talk in a very different way. The assume, as it were, that the objects already have a language of their own and they try to learn this language. They try to learn it not on the basis of linguistic theories but by immersion, just as small children familiarise themselves with the world. And they try to learn the language of the objects as they are, and not as they appear after they have been subjected to standardizing procedures (experiments, mathematization). Categories of the abstract approach such as the concept of an objective truth cannot describe a process of this kind which depends on the idiosyncracies of both objects and observers (it makes no sense to speak of the 'objective existence' of a smile which, depending on context, can be seen as a kind smile, a cruel smile, or a bored smile).

Abstract and historical traditions have fought each other from the beginning of Western thought. Their contest started with the 'ancient battle between philosophy and poetry' (Plato, *Republic* 607b – see chapter 3 of the present book). It continued in medicine where the theoretical approach of Empedocles and the element-physicians was criticized by the author of *Ancient Medicine* (details in chapter 1, section 6 and in chapter 6, section 1). The antagonism characterized Thucydides's criticism of Herodotus and it has survived until today — in pyschology (behaviourism versus '*verstehende*' methods), biology (molecular biology versus qualitative types of biological research), medicine ('scientific' medicine versus healers of all sorts), ecology, and even mathematics (Cantorianism versus constructivism – to use terms first suggested by Poincaré). Abstract traditions change into historical traditions during times of crisis and revolution, which supports my thesis that *good sciences are arts, or humanities, not sciences in the textbook sense*. Ian Hacking's analysis of experimental procedures is an excellent illustration of the art-aspect of scientific research.

Alan Musgrave shows that the instrumentalistic tradition in ancient astronomy was much weaker than Duhem thought. He forgot to mention that modern scientific realism uses an instrumentalism of qualitites and qualitative laws: realists take it for granted that qualities which do not enter the body of science but which enable us to contribute to it will not lead us astray. Modern science, which created but never solved the mind-body problem, uses instrumentalism at its very basis – and it shows (for instance, in the quantum theory of measurement). In a short introduction, which has nothing to do with the bulk of his paper and which he seems to have added as an afterthought, Musgrave produces a

curious criticism of an earlier essay of mine (reprinted in *Philosophical Papers* Vol. 1, chapter 11). There I argued that most philosophical reasons for realism are too weak to overcome the physical reasons against it, and that they must be made stronger; and I then developed the needed stronger reasons. According to Musgrave I did the very opposite – I tried to find universal arguments for *instrumentalism*! I don't think that Alan misread what I had written, for he is a careful critic and the paper he criticizes one of the clearest papers I have written – but I am quite ready to accept a plea of temporary insanity. Let me add, incidentally, that I no longer believe in the relevance, for our understanding of the sciences, of general arguments such as those produced in my paper.

I agree with practically all the points and objections made in Grover Maxwell's beautiful essay on the mind-body problem. I admit that despite good intentions I 'too often relapsed into the empiricist . . . practice of treating . . . meaning in an apriori manner' (but I had my sane moments, too, and then I treated meanings as neurophysiological structures or 'programmes': see *Philosophical Papers* vol. 1, chapter 6; vol. 2, chapter 9). I also admit that I occasionally forgot the contingent nature of the pragmatic theory of observation (for my sane moments on this point cf. my little note 'Science without Experience', *Philosophical Papers*, vol. 1, chapter 7, which made Ayn Rand curse me in an open letter to all American philosophers). It is true that in criticizing acquaintance I 'set up a straw man'. Actually, the straw man (straw woman?) was set up not by me but by the sense-datists – but having removed her (it? him?) I thought I had removed all aspects of acquaintance – and in this I was certainly mistaken. I was not consistent in my mistake for I occasionally assumed, as Russell had done, that the brain could be directly perceived, but I did not draw the correct conclusion and declare some physical events to be mental. I am not too upset by the fact that some of my arguments may provide amunition for the eliminative mentalist – this, I think, applies to all arguments about contingent matters. Grover's own theory, on the other hand, seems to me to rely too heavily on scientific notions and procedures. Grover's assertion that 'science works' does not remove my uneasiness. Science works sometimes, it often fails and many success stories are rumours, not facts. Besides, the efficiency of science is determined by criteria that belong to the scientific tradition and thus cannot be regarded as objective judges. (For example, science does not save souls.) I conclude that Grover

has shown how our notions of mind and body can be developed *within the scientific framework* without thereby removing ideas that come from different traditions (the tradition of the Dogon, or the Azande, or of Ecuadorian peasants). And I am very glad that he has not succeeded in doing the latter; at least there is now a chance of meeting him again, on a different plane, in different circumstances but, hopefully, with his caustic humour unchanged.

4 Science – one Tradition among Many

The second topic of my writings is the authority of the sciences. I assert that there exist no 'objective' reasons for preferring science and Western rationalism to other traditions. Indeed, it is difficult to imagine what such reasons might be. Are they reasons that would convince a person, or the members of a culture, no matter what their customs, their beliefs or their social situation? Then what we know about cultures shows us that there are no 'objective' reasons in this sense. Are they reasons which convince a person who has been properly prepared? Then all cultures have 'objective' reasons in their favour. Are they reasons referring to results whose importance can be seen at a glance? Then again all cultures have at least some 'objective' reasons in their favour. Are they reasons which do not depend on 'subjective' elements such as commitment or personal preference? Then 'objective' reasons simply do not exist (the choice of objectivity as a measure is itself a personal and/or group choice – or else people simply accept it without much thought).

It is true that Western science has now infected the whole world like a contagious disease and that many people take its (intellectual and material) products for granted – but the question is: was this a result of argument (in the sense of the defenders of Western science), i.e. was every step of the advance covered by reasons that are in agreement with the principles of Western rationalism? Did the infection improve the lives of those who were touched by it? The answer is no to both questions. Western civilization was either imposed by force, not because of arguments showing its intrinsic truthfulness, or accepted because it produced better weapons (see chapter 1, section 9); and its advance, while doing some good, also caused enormous damage (for a survey consult J.H. Bodley, *Victims of Progress*, Menlo Park, California 1982). It not only destroyed spiritual values

which gave meaning to human lives, it also damaged a corresponding mastery of the material surroundings without replacing it by methods of comparable efficiency. 'Primitive' tribes knew how to deal with natural disasters such as plagues, floods, droughts – they had an 'immune system' that enabled them to overcome a great variety of threats to the social organism. In normal periods they exploited their environment without damaging it, using knowledge of the properties of plants, animals, climatic changes and ecological interactions which we are only slowly recovering (details and ample literature are given in Lévi-Strauss, *The Savage Mind*, and later more detailed studies of a similar kind). This knowledge was severely damaged and partly destroyed, first by the gangsters of colonialism and then by the humanitarians of developmental aid. The resulting helplessness of large parts of the so-called Third World is the result of, not a reason for, outside interference.

Majid Rahnema, an Iranian scholar, has compared the effects of developmental aid with the effects of the illness Aids which destroys the immune system of the human body (*From 'Aid' to 'Aids'*, unpublished manuscript, Stanford 1984). He has also commented on the way in which knowledge was turned from a common good to a rare and inaccessible commodity. 'Cultures and Civilizations,' he writes (*Education for Exclusion or Participation?*, manuscript, Stanford, 16 April 1985),

> were formed, enriched and transmitted by millions of people who were *learning by living and doing*, for whom living and learning was synonymous, as they had to learn for living and they learned whatever was meaningful to them and to the community they belonged to. Before the present school system came into being, for thousands of years, education was not a scarce commodity. It was not a product of some institutional factories, the possession of which could bestow upon a person the right to be called 'educated . . . The [new] school system . . . serve[d] as a rather efficient channel of sieving out, into the Power Establishment, the most ambitious – and sometimes the brightest – aiming at personal and professional fame. It also, paradoxically, did serve as a 'culture medium' to some outstanding individuals, among them radical thinkers and revolutionaries who used some of its unique learning resources for their own liberating purposes. Yet, on the whole, it soon became an 'infernal machine' which distinguished itself in the systematic organization of excluding processes against the poorest and the powerless . . . The old days . . . when 'every adult was a teacher' were over. Now, only those certified by the school system, according to its self devised criteria, could have the right to teach. *Education thus became a scarcity* [my emphasis].

It is interesting to see how little influence these discoveries have had on the sermons preached by professional rationalists. Karl Popper, for example, bemoans the 'general anti-rationalist atmosphere . . . of our time', praises Newton and Einstein as great benefactors of humanity but breathes not a single word about the crimes committed in the name of Reason and Civilization. On the contrary, he seems to think that the benefits of civilization may occasionally have to be imposed, on unwilling victims, by a 'form of imperialism' (see chapter 6, section 1).

There are various reasons why so many intellectuals still argue in this short-sighted way. One reasons is ignorance. Most intellectuals have not the foggiest idea about the positive achievements of life outside Western civilization. What we had (and, unfortunately, still have) in this area are rumours about the excellence of science and the dismal quality of everything else. Another reason lies in the immunizing moves rationalists have devised to overcome difficulties. For example, they distinguish between basic science and its applications: if any destroying was done, then this was the work of the appliers, not of the good and innocent theoreticians. But the theoreticians are not that innocent. *They* are recommending analysis over and above understanding, and this even in domains dealing with human beings; *they* extol the 'rationality' and 'objectivity' of science without realising that a procedure whose main aim it is to get rid of all human elements is bound to lead to inhuman actions. Or they distinguish between the good which science can do 'in principle' and the bad things it actually does. That can hardly give us comfort. All religions are good 'in principle' – but unfortunately this abstract Good has only rarely prevented their practitioners from behaving like bastards.

Thoughtless people are in the habit of pointing out that every 'reasonable' person will be persuaded that science knows best. The comment admits a weakness of argumentation: arguments do not work on everyone, they work only on people who have been properly prepared. And this is a general feature of all idological debates: arguments in favour of a certain world view depend on assumptions which are accepted in some cultures, rejected in others, but which because of the ignorance of their defenders are thought to have universal validity. Kekes's attempt to overcome relativism is an excellent example of this situation.

He makes three assumptions: (1) it is important to solve problems; (2) there exist more or less unambiguous methods for

solving problems; (3) some problems are independent of all traditions – problems of this kind Kekes calls problems of life. Kekes also assumes that explicit conceptualisation plays an important part in recognizing, formulating, and solving problems. But for the Orphics, some Christians, and some Moslem fundamentalists, many of the things a Western intellectual might call problems were not undesirable situations waiting to be removed by human ingenuity but either tests of moral fibre (cf. the function of initiation rites), or preparations for a difficult task, or necessary ingredients of a life that would cease to be human without them. Some cultures treat problems as quirks which cause amusement, not consternation; one simply lets them pass instead of trying to 'solve' them.

White government officials in Central Africa were often upset by the fact that problems they noticed and conveyed to their black colleagues were not dealt with seriously, by an increased effort of thought, but were simply laughed out of court: the bigger the problem, the greater the hilarity. This, the white rationalists said, was a very irrational way to behave – and so it was, according to their standards. On the other hand – what a fine way of avoiding wars and the misery they create! 'Do something' certainly is not uniformly superior to 'Let it be'. Kekes articulates the procedures customary within certain traditions – he does not give us 'objective', i.e. transtraditional principles.

'Problems of life' in Kekes's sense are parts of special and relatively young traditions of a materialistic-humanistic bent. Their solutions cannot be impartial judges of the rest. Moreover, even secular solutions allow for many ways of living outside the sciences, as is shown by our artists and by the wide spectrum covered by apparently 'objective' concepts such as the concept of health (cf. Foucault). We have to admit that many values and many cultures have ceased to exist; they were killed and hardly anybody now remembers them. But this does not mean that we cannot learn from them and, besides, Kekes wants a *theoretical* solution to the problem of relativism – and such a solution is not forthcoming.

Similar remarks apply to Noretta Körtge's interesting and provocative essay. She must be praised for emphasizing that in dealing with citizens appearance is at least as important as 'reality' (which at any rate is nothing but the way in which things appear to fashionable experts): 'not only must justice be done but justice must *seem* to be done'. Well said! *What counts in a democracy is the experience of the citizens, i.e. their subjectivity*

and not what small gangs of autistic intellectuals declare to be real (if an expert does not like the ideas of the common folk then all he has to do is to talk to them and try to persuade them to think along different lines; in so doing, he must not forget that he is a beggar and not a 'teacher' trying to pound some truth into the heads of penitent pupils). But her attempt to separate this experience from some 'reality' cannot succeed. I agree that the sciences, and civilizations built around them, contain something called 'expert opinion' and that it differs from what experts call 'popular superstitions' – but I would add that this is true of other traditions as well (for example, it is true of the Dogon as Griaule has shown in his marvellous book). I also agree that expert opinion occasionally shows some uniformity – all churches have temporary uniformities – but the occasional convergences in some areas are more than compensated by disagreements in others. Nor does the convergence of expert opinion establish an objective authority and if it does, then we have many different authorities to choose from: the distinction between expert-reality and layman-appearance dissolves into what appears to every one of us, experts included.

That rationalists clamouring for objectivity and rationality are just trying to sell a tribal creed of their own becomes very clear from the reactions of some less gifted members of the tribe. Thus Tibor Macham, writing at the expense of an outfit ominously called the Reason Foundation (I am referring to a review of SFS which appeared in *Philosophy of the Social Sciences*, 1982), distinguishes between acceptable standards, ideas and traditions, and traditions that are 'mere caprice and destructive to human life'. What is the rationale for his distinction? A theory of man. What is the gist of his theory of man? That 'human beings are rational animals . . . biological beings with the distinctive need and capacity for principled (or conceptual) thought and action.' This, of course, is a perfect description of intellectuals (the only thing missing is the craving for large salaries) – but a person with a somewhat different perspective will have to point out, in all modesty, that Macham's 'theory of man' is but one view among many and that intellectuals, fortunately, are still only a small percentage of humanity. There is the view that humans are misfits in the material world, unable to understand their position and their purpose and 'with a distinctive need' for salvation; there is the view, closely related to the one just mentioned, that humans consist of a divine spark enclosed in an earthen vessel, a 'trace of gold embedded in dirt' as the Gnostics

were in the habit of saying, 'with the distinctive need' for liberation by faith. And these are not just abstract and 'capricious' views – they have been, and still are, part of the lives of millions of people. There is the view, found among Buddhists, that humans want to escape pain, that thought and purposeful action based on thought are the main causes of pain, and that pain will cease once customary distinctions are removed and customary purposes abolished. The Hopi Genesis represents humans as being originally in harmony with Nature. Thought and striving, or in other words the very same 'need for principled thought and action' which Macham makes the centre of humanity, destroy the original harmony, the animals withdraw from the humans, the human species is split into races, tribes and small groups with different ideas, and different languages arise until even individuals no longer understand each other. But humans, 'having the distinctive need and capacity for' harmony, can overcome the fragmentation by freeing themselves from the fetters of conceptual thought and the strife it creates and by basing their lives on love and intuitive understanding.

There are numerous views of this kind and they all differ from the theory mentioned *and taken for granted* by Macham. Now it is of course Macham's good right to favour one view and condemn another. But he does so posing as a rationalist and a humanitarian. He claims to have not only anathemas but also arguments and to be motivated by a love for humanity. A look at his criticism shows that both claims are suprious. His arguments are but curses pronounced in the stiff rhetoric of the self-conscious scholar and his love for humanity stops right at his office door (or at the cashier's desk of the Reason Foundation).

As is customary among intellectuals, Macham uses unanalysed cases such as the Jonestown killings to frighten his readers instead of trying to enlighten them (German 'rationalists' use Auschwitz and, more recently, terrorism *ad nauseam* for the same purpose). 'These are easy cases', says Macham. How naive can you get? In Jonestown, some people committed suicide freely, in the full knowledge of what they were doing (case 1). Others wavered, were undecided, would have liked to survive but submitted to the pressure of their peers and their leaders (case 2). Still others were simply murdered (case 3). For Macham the distinctions do not exist. But they are essential for an instructive analysis of the case. Case 3 may be 'easy' if one wants to talk in this superficial way, though there are sizeable problems even here (should one kill bodies to save souls? Rational Inquisitors

thought so and with excellent arguments: are these arguments to be disregarded? Are we to take materialism for granted? I have no objection to the latter step – but where does this leave a rationalist, i.e. a person who claims to have arguments for every move he makes?). Case 1 is again 'easy' though not in the way assumed by Macham. Of course, it is 'destructive to human life' – but is human life an overriding value? The Christian martyrs did not think so and neither Macham nor any other rationalist has succeeded in showing that they were mistaken. They had a different opinion – that is all. Socrates expressed a similar sentiment before dying; he was not alone, for the same sentiment can be found in Herodotus, in Sophocles and in other outstanding representatives of classical Greece. Not once does it occur to Macham that his view of a human being is one among many, that he is a party to the debate and not its supervisor.

There remains case 2; here I fully agree with those who demand that people be protected from peer group and leader pressures. But this caveat applies not only to religious leaders such as the Reverend Jones *but also* to secular leaders such as philosophers, Nobel Prize Winners, Marxists, Liberals, hitmen of foundations and their educational representatives: the young must be strengthened against being imposed upon by so-called teachers, and especially against ratiofascists like Macham and his peers. Unfortunately contemporary education is far from agreeing with this principle.

Finally there is the old argument that non-scientific traditions have already had their chance, that they did not survive the confrontation with science and rationalism and that attempts to revive them are therefore both irrational and unnecessary. Here the obvious question is: were they eliminated on rational grounds, by letting them compete with science in an impartial and controlled way, or was their disappearance the result of military (political, economic etc.) pressures? And the reply is almost always: the latter. The American Indians were not asked to present their views, they were first christianised, then sold out of their land and finally herded into reservations amidst a growing scientific-technological culture. Indian medicine (which was commonly used by medical practitioners of the nineteenth century) was not tested against the new pharmaceuticals that invaded the market, it was simply forbidden as belonging to an antediluvean age of healing. And so on.

Reference to past opportunities also overlooks the point that

even clear and unambiguous refutations do not seal the fate of an interesting point of view (for what follows, cf. SFS, pp. 100ff., and chapter 1, section 1 of the present book); the means of refutation (experimental equipment, the theories used for the interpretation of the results obtained) constantly change, and with them the nature of the argument. One should also note the striking similarity between the argument from success and comments such as those made by the Nazis after their triumph in 1933: liberalism had already had its chance, it was defeated by the national forces and it would be silly to try to reintroduce it.

Finally, it is up to the citizens to choose the traditions they prefer. Thus democracy, the fatal incompleteness of criticism, and the discovery that the prevalence of a view never is and never was the result of an exclusive application of rational principles, all suggest that attempts to revive old traditions and to introduce anti-scientific views are to be praised as the beginnings of a new age of enlightenment, where our actions are guided by insight and not merely by pious and often quite moronic slogans.

5 Reason and Practice

What I have said so far can be summarized in the following two statements:

(A) the way in which scientific problems are attacked and solved depends on the circumstances in which they arise, the (formal, experimental, ideological) means available at the time and the wishes of those dealing with them. There are no lasting boundary conditions of scientific research.

(B) the way in which problems of society and the interaction of cultures are attacked and solved also depends on the circumstances in which they arise, the means available at the time and the wishes of those dealing with them. There are no lasting boundary conditions of human action.

Thus I criticized the view which I shall call (C), that science and humanity must conform to conditions that can be determined independently of personal wishes and cultural circumstances. And I objected to the assumption, (D), that it is possible to solve problems from afar, without participating in the activities of the people concerned.

(C) and (D) are the core of what one might call *the intellectualistic approach to (science and) social problems*. They are a matter of course for academic Marxists, liberals, social scientists, businessmen, politicians eager to help 'underdeveloped nations'

and prophets of 'new ages'. Every writer who wants to improve knowledge and save humanity and who is dissatisfied with existing ideas (reductionism, for example) thinks that salvation can only come from a new *theory* and that all that is needed to develop such a theory are the right books and a few clever ideas.

(C) and (D) have also been used to discredit what I say about politics. According to my critics, I make a lot of noise but achieve little. My approach, they say, is entirely negative. I object to certain procedures – but I have nothing to offer in their stead. Marxists have been especially incensed at my mocking disregard for their two favourite playthings, Western science and humanitarianism.

These remarks are certainly correct. I have indeed no positive suggestions to make. But the reason is not that I have forgotten about the matter, or cannot compete with the speculative talents of my fellow academics – the reason is my respect for the traditions I am supposed to bless with my intellectual gifts. These traditions are historical traditions, not abstract traditions (see above, sections 2, 3 and 4, and chapter 3). Historical traditions cannot be understood from afar. Their assumptions, their possibilities, the (often unconscious) wishes of their bearers can be found only by immersion, i.e. *one must live the life one wants to change*. Neither (C) nor (D) apply to historical traditions. Boundary conditions and solutions invented by distant speculators can still be imposed *but only by disregarding the full humanity of the victims*. Intellectuals who support the imposition are not unaware of the 'human dimension'; they have 'theories of man' and they use them as guides to their actions. But these theories do not reflect their victims; they reflect the mentality of the place where they arose – university offices and seminar rooms, mainly (cf. my remarks on Tibor Macham in section 4 above): my main objection against intellectual solutions of social problems is that they start from a narrow cultural background, ascribe universal validity to it and use power to impose it on others. Is it surprising that I want to have nothing to do with such ratiofascistic dreams? Helping people does not mean kicking them around until they end up in somebody else's paradise, helping people means trying to introduce change *as a friend*, as a person, that is, who can identify with their wisdom *as well as* with their follies and who is sufficiently mature to let the latter prevail: *an abstract discussion of the lives of people I do not know and with whose situation I am not familiar is not only a waste of time, it is also inhumane and impertinent.*

It is a waste of time because the practical application of the

theories found will always have to be preceded by numerous changes which may wipe out the basic programme. It is impertinent: being unfamiliar with the conditions of the strangers, with the ways in which these conditions appear to them, having no direct experience of their dreams, fears, desires, I refuse to make my own standards, my so-called knowledge (whether puny or impressive – that does not matter), my own very limited humanity the basis of 'objective' diagnoses and suggestions (only very naive or intolerant people can believe that a study of the 'nature of man' is superior to personal contacts, in one's private lives as well as in politics). Jutta, who bears a woman's name but who seems bent on outdoing the chauvinism of the most pushy of her male academic colleagues, says that I am lacking in heart and imagination. On the contrary: *I* can imagine that there are situations of which I have never thought, which are not described in books, which scientists have never encountered and would not recognize when confronted with. I believe that such situations occur quite frequently; I can also imagine that such situations look different to different people, affect them in different ways, raise hopes, fears, emotions I have never felt, and I have the heart to subject my distant guesses to the impressions of those immediately concerned. Jutta says I should 'examine' with 'respect' what I do not know. Examine? If I love a woman and want to share her life, for my benefit and perhaps also for hers, then I shall not 'examine' that life, whether respectfully or with disdain, I shall try to *participate* in it (provided she lets me) so that I can understand it from within. Participating in her life, I change into a new person with new ideas, feelings, ways of seeing the world. Of course, I shall make lots of suggestions – I may even drive her nuts with all my talk *but only after the change has occurred* and on the basis of the new *and shared* sensibilities it has created. Now politics, as I understand it, is in many ways related to love. It respects people, considers their personal wishes, does not 'study' them whether by polls or by anthropological field work but again tries to understand them from within, and connects suggestions for change with the thoughts and the emotions that flow from such an understanding. In a word: *politics, rightly understood, is firmly 'subjective'*. It is impossible to develop 'objective' theoretical schemes for it.

6 Elements of a Free Society

How is this account related to my ideas about the police, the

equality of traditions, the separation of state and science? The answer has already been given in SFS and EFM (EFM, p. 77 and passim): ideas such as these must pass the filter of the traditions (of the citizens' initiatives) for which they were developed. A fundamental error of almost all the papers that deal with this part of my writings — and that includes the paper by Christiane van Briessen, who got my number in many other respects — is that they interpret my suggestions as if they should be read in the same way as politicians, philosophers, social critics, and 'great' men and women of all sorts want to be read: they interpret them as the outlines of a new social order which must now be imposed on people with the help of education, moral blackmail, a nice little revolution and treacly slogans (such as 'The Truth will Make you Free'), or by utilizing the pressures issuing from already existing institutions. But dreams of power such as these are not only very far from my mind – they positively make me sick. I have little love for the educator or moral reformer who treats his wretched effusions as if they were a new sun brightening the lives of those living in darkness; I despise so-called teachers who try to whet the appetite of their pupils until, losing all self-respect and self-control, they wallow in truth like pigs in the mud; I have only contempt for all the fine plans to enslave people in the name of 'god' or 'truth' or 'justice' or other abstractions, especially as their perpetrators are too cowardly to accept responsibility for these ideas but hide behind their alleged 'objectivity'. Many of my readers seem to regard such machinations as a very normal procedure – how else can I explain that they read my proposals in this manner? But the loose and sketchy remarks on the state, on ethics, on education, and on the business of science which I made in AM and SFS must be examined by the people for whom they are meant. They are subjective opinions, not objective guidelines; they are to be tested by other subjects, not by 'objective' criteria, and they receive political power only after everybody concerned has considered them: the consensus of those addressed, not my arguments, finally decides the matter.

The objection that people must first be taught to think only reflects the conceit and the ignorance of its authors, for the basic problem is: who can talk and who should remain silent? Who has knowledge and who is merely obstinate? Can we trust our experts, our physicists, our philosophers, our healers, our educators, do they know what they are talking about or do they merely want to duplicate their own miserable existence? Have our great minds, have Plato, Luther, Rousseau, Marx, anything

to offer, or is the reverence we feel for them merely a reflection of our own immaturity?

These questions concern all of us – and all of us must participate in their solution. The most stupid student and the most cunning peasant; the much honoured public servant and his long-suffering wife; academics and dog catchers, murderers and saints – they all have the right to say: look here, I, too, am human; I, too, have ideas, dreams, feelings, desires; I, too, have been created in god's image – but you never paid attention to my world in your pretty tales (it was different in the Middle Ages; cf. Friedrich Heer, *Die Dritte Kraft*, Frankfurt 1959). The relevance of abstract questions, the content of the answers given, the quality of life adumbrated in these answers – all these things can be decided only if everyone is permitted to participate in the debate and encouraged to give her or his views on the matter. The best and simplest outline of the ideas just explained is found in Protagoras's great speech (Plato, *Protagoras*, 320c-328d): the citizens of Athens do not need any instruction in their language, in the practice of justice, in the treatment of experts (warlords, architects, navigators); having grown up in an open society where learning is direct and not mediated and disturbed by educators, they learned all these things from scratch. As for the further objection that states and citizens' intitiatives do not arise out of the blue but must be set in motion by purposeful action — that is easy to answer: let the objector start a citizens' initiative, and he will soon find what he needs, what furthers his ambitions, what obstructs them, to what extent his ideas are a help to others, to what extent they hinder them, and so on.

This, then, is my answer to the various criticisms of 'my' 'political model'. The model is vague – very true – but the vagueness is necessary, for it is supposed to 'make room' (EFM, p. 160) for the concrete decisions of those using it. The model recommends an equality of traditions: any proposal must first be checked by the people for whom it is meant; nobody can foresee the result. (The pygmies, for example, or the Mindoro of the Philippines, do not want equal rights – they just want to be left alone.) Conflicts are not dealt with by 'education' but by a police force. Margherita von Brentano interprets the last suggestion as implying that citizens may only talk and perhaps write but that their actions are severely restricted. Other critics have thrown up their hands in despair: speak of the police – and liberals and Marxists alike are liable to wet their pants. This is precisely the mistake described above. For the police is not an external agent

that pushes the citizens around; it is introduced by citizens, consists of citizens and serves their needs (cf. my comments on the protective guards of the Black Muslims, EFM, p. 162, p. 297). Citizens do not just think, they decide about everything in their surroundings. I merely suggest that it is more humane to regulate behaviour by external restrictions – such restrictions can be easily removed when found impractical – than to improve souls. For assume we succeed in implanting The Good in everybody – how then shall we ever be able to return to Evil?

7 Good and Evil

With this remark I come to a point which has enraged many readers and disappointed many friends – my refusal to condemn even an extreme fascism and my suggestion that it should be allowed to survive. Now one thing should have been clear: fascism is not my cup of tea (cf. EFM, 156: 'despite my own very widely developed sentimentality and my almost instinctive tendency to "act in a humanitarian manner" '). *That* is not the problem. The problem is the *relevance* of my attitude: is it an inclination which I follow and welcome in others; or has it an 'objective core' that would enable me to combat fascism not just because *it does not please me*, but because *it is inherently evil*? And my answer is: we have an inclination – nothing more. The inclination, like every other inclination, is surrounded by lots of hot air and entire philosophical systems have been built on it. Some of these systems speak of objective qualities and of objective duties to maintain them. But my question is not how we speak but what content can be given to our verbiage. And all I can find when trying to identify some content are different systems asserting different sets of values with nothing but our inclination to decide between them (SFS, part 1). Now if inclination opposes inclination then in the end the stronger inclination wins, which means, today, and in the West: the bigger banks, the fatter books, the more determined educators, the bigger guns. Right now, and again in the West, bigness seems to favour a scientifically distorted and belligerent (nuclear weapons!) humanitarianism – and so the matter has come to a temporary rest at this point.

This, incidentally, was one of the lessons I learned from the life of Remigius, the inquisitor. Margherita von Brentano, who mentions my reference to him, was kind enough not to assume

that I am pleading for a revival of witchcraft and witchcraft persecutions. Of course, this is not my intention. Nor do I think I would remain a silent witness of such persecutions. But my explanation would be that the matter does not please me and not that it is inherently evil or based on a backward view of the universe. Such expressions far exceed what can be supported by the best intentions and the most clever arguments. They give the user an authority he simply does not possess. They put him on the side of the angels when all he does is to express his personal opinions. Truth herself seems to be his companion when again we are dealing only with an opinion and a very badly argued one at that. There existed lots of arguments against atoms, the motion of the earth, the aether – and yet all these things returned to the scene. The existence of God, the Devil, heaven, hell was never attacked with even half-way decent reasons. Thus if I want to remove Remigius and the spirit of his times then I can of course proceed to do so, but I must admit that the only instruments available to me are the powers of rhetoric and self-righteousness. If, on the other hand, I accept only 'objective' reasons, then the situation forces me to be tolerant, for there are no such reasons, in this case any more than in others (SFS, parts 1 and 2; EFM, chapter 3).

Remigius believed in God, he believed in an afterlife, in hell and its tortures and he also believed that the children of witches who were not burned would end up in hell. He did not just believe those things, he could have provided arguments. He would not have argued in our manner and his evidence (the Bible, the Church Fathers, the decisions of Church councils etc.) would not have been what we call evidence. But this does not mean that his ideas were without substance. For what do *we* have to oppose him? The belief that there is a scientific method and that science is successful? The first part of the belief is false (cf. section 2 above); the second part is correct but must be supplemented by saying that there were and still are many failures as well and that the successes occur in a narrow domain that hardly touches what is at issue here (the soul, for example, never enters the scene). What falls outside the domain, such as the idea of hell, was never *examined*, it was *lost*, just as the scientific achievements of antiquity were lost by the early Christians.

Within the framework of his thought Remigius acted as a responsible and rational human being and he should be praised, at least by rationalists. If we are repelled by his views and unable to give him his due then we must realise that there are absolutely

no 'objective' arguments to support our repulsion. We can of course sing moral arias, we may even write an entire opera where these arias hang together beautifully – but we cannot build a bridge from all that noise to Remigius and, appealing to *his* reason, bring him over to our side. For he does use his reason, but with a different purpose, according to different rules and on the basis of different evidence. There is no way out: *we bear full responsibility for not proceeding as Remigius does and no objective values will plead our case should we discover that our actions have led to disaster.*

On the other hand, let us not forget our own inquisitors, our scientists, physicians, educators, sociologists, politicians, 'developers'. Just look at those physicians who until quite recently cut, poisoned and irradiated without having examined alternative methods of treatment which were well known, had no dangerous consequences and could claim to be successful. Was it not worth trying such methods (was it not worth trying to keep the children of witches alive)? It was worth trying. But all we heard in reply was: *anathema sit*! Or let us examine the efforts of our educators who year in year out are let loose on the younger generation to fill it with 'knowledge' without regard for the background of the pupils. Entire cultures have been killed, their immune systems destroyed (cf. section 4), their knowledge turned into a scarcity – and all that in the name of progress (and money, of course): the spirit of Remigius, my dear Margherita von Brentano, is still with us, in economics, in energy production and (mis)use, in foreign aid, in education, the important difference being that Remigius acted for humanitarian reasons (he wanted to save little children from eternal damnation) while his modern successors only care for their professional integrity: they not only lack perspective, they also lack humanity. I don't like them, either — but here my motives are again not objective standards, but dreams of a better life.

Now if one combines such dreams (which I have) with an idea of objective values (which I reject) and calls the result a moral conscience then *I have no moral conscience* and fortunately so, I would say, for most of the misery in our world, wars, destruction of minds and bodies, endless butcheries are caused not by evil individuals but by people who have objectivised their personal wishes and inclinations and thus have made them inhuman.

This, incidentally, is the only thing Agassi seems to have noticed in his strange outburst. Agassi says he will speak the truth. That

is nice of him but does not give us much comfort. For as critics of his scientific work have pointed out, long ago, he only rarely knows what he is talking about even when he is trying to tell the truth (for example: item 882 in Rosen's Copernicus bibliography, *Three Copernican Treatises*, New York 1971). His paper confirms the impression. He says I volunteered for the German army – I was drafted. He says I tried to forget the political and moral aspects of the Second World War – I did not notice them; at eighteen I was a book-worm not a *mensch*. He says I idolised Popper. Now it is quite true that I like to idolize people, I like to be able to look up to somebody, to admire her, to take her as an example – but Popper is not the stuff idols are made of. Agassi calls me a disciple of Popper. This is true in one sense, quite untrue in another. It is true that I listened to Popper's lectures, sat in his seminar, occasionally visited him and talked to his cat. This I did not of my own free will but because Popper was my supervisor: working with him was a condition of my being paid by the British Council. I had not chosen Popper for this job, I had chosen Wittgenstein and Wittgenstein had accepted. But Wittgenstein died and Popper was the next candidate on my list. Also, doesn't Agassi remember how often he begged me, on his knees, to give up my *reservatio mentalis*, fully to commit myself to Popper's 'philosophy' and, especially, to spread lots of Popper-footnotes all over my essays? I did the latter – well, I am a nice guy and quite willing to help those who seem to live only when they see their name in print – but not the first: at the end of the year Agassi is speaking of (1953), Popper asked me to become his assistant; I said no despite the fact that I had no money and had to be fed now by the one, now by the other of my more pecunious friends.

Agassi also produces some of the rumours which are apparently needed to make life in the Popperian Church bearable: he quotes Popper as saying that I once tearfully regretted having participated in the Second World War. That is quite possible, I am an emotional person and have done many stupid things in my life – but it is unlikely: I never discuss personal matters with strangers and, besides, there was nothing to be sorry about except perhaps insufficient intelligence in the attempt to escape the draft. The tears, most likely, were tears of boredom which flowed rather freely during my visits to the Master. It is a sad sign of the decay of standards of scholarship in Germany that a piece of lachrymose trash like Agassi's essay could be written with the

aid of a stipend that bears the old and honourable name of Alexander von Humboldt.

There is only one point where Agassi shows some grasp of reality and this concerns our discussion of moral issues. I remember the discussion well. Agassi urged me to take a stand, i.e. to sing moral arias. I felt very uncomfortable. On the one hand the matter seemed quite idiotic – I sing my aria, the Nazi sings his – now what? On the other hand I felt the irrational pressure of Auschwitz which Agassi and many ideological street singers before and after him have used shamelessly to urge people into empty gestures (or to brainwash them so that the gestures receive 'meaning'). What do I say today?

I say that Auschwitz is an extreme manifestation of an attitude that still thrives in our midst. It shows itself in the treatment of minorities in industrial democracies; in education, education to a humanitarian point of view included, which most of the time consists in turning wonderful young people into colourless and self-righteous copies of their teachers; it becomes manifest in the nuclear threat, the constant increase in the number and power of deadly weapons and the readiness of some so-called patriots to start a war compared with which the holocaust will shrink into insignificance. It shows itself in the killing of nature and of 'primitive' cultures with never a thought spent on those thus deprived of meaning for their lives; in the colossal conceit of our intellectuals, their belief that they know precisely what humanity needs and their relentless efforts to recreate people in their own, sorry image; in the infantile megalomania of some of our physicians who blackmail their patients with fear, mutilate them and then persecute them with large bills; in the lack of feeling of many so-called searchers for truth who systematically torture animals, study their discomfort and receive prizes for their cruelty.

As far as I am concerned there exists no difference whatsoever between the henchmen of Auschwitz and these 'benefactors of mankind' – life is misused for special purposes in both cases. The problem is the growing disregard for spiritual values and their replacement by a crude but 'scientific' materialism, occasionally even called humanism: man (i.e. humans as trained by their experts) can solve all problems – they do not need any trust in and any assistance from other agencies. How can I take a person seriously who bemoans distant crimes but praises the criminals in his own neighbourhood? And how can I decide a case from afar

seeing that reality is richer than even the most wonderful imagination.

It is one thing to be in the forefront of the fight against cruelty and oppression, for then you can see and smell your enemy; and your whole being, not only your ability to rhapsodize, will be engaged in the attempt to defeat him. It is quite a different thing to shake one's head and to decide about Good and Evil while sitting in a comfortable office. I know – many of my friends can make such a decision with both hands tied behind their back – they obviously have a well developed moral conscience. I, on the other hand, taking the distance seriously, would like to consider a different view where Evil is part of Life just as it was part of Creation. One does not welcome it – but one is not content with infantile reactions either. One delimits it – but one lets it persist in its domain. For nobody can say how much good it still contains and to what extent the existence of even the most insignificant good thing is tied up with the most atrocious crimes.

8 Farewell to Reason

What was the origin of the criticisms on which I have commented in this chapter? And why did I write a reply?

It is easy to answer the first question.

About eight years ago (1979), Hans Peter Duerr was invited to become an author of the famous Suhrkamp publishing house in Germany. He refused, for he had other obligations. But he also had a bad conscience – it is not easy for Hans Peter to reject friendly invitations. Dr. Unseld, the guiding spirit of the Suhrkamp publishing house whose ability to sniff out the bad conscience of people is only exceeded by his expertise in manipulating it, discovered Hans Peter's predicament and treated it with words, food, and drink. Result: Hans Peter conceived the idea of a PKF festival and started sending letters in all directions. Some of the letters were returned unopened, others with reflections on his sanity, still others with the customary excuse of lack of time – but quite a few people decided to praise me or to curse me, or to exorcise me by surrounding me with rhetorical circles. Thus it was not the merit of my 'work' that led to this collection, but the power of alcohol.

It is much more difficult to answer the second question. Many people — scientists, artists, lawyers, politicians, priests — draw

no distinction between their profession and their lives. If they are successful, then they take this as an affirmation of their very existence. If they fail in their profession, then they think they have failed as human beings as well, no matter how much joy they may have given to their friends, children, wives, lovers, dogs. If they write books, be they novels, collections of poems, or philosophical treatises, then these books become part of an edifice built from their very substance. 'Who am I?' Schopenhauer asked himself – and he replied, 'I am the person who wrote *The World as Will and Idea* and solved the great problem of being.' Parents, brothers, sisters, husbands, mistresses, parakeets (budgerigars for my British readers), even the most personal feelings of the author, his dreams, fears, expectations, have meaning only with respect to that edifice and they are described accordingly: the wife, well, she knew how to cook, to clean, to wash and to create the right atmosphere; the friends, well, they understood the poor chap during trying times and gave him support, they lent him money, they eagerly helped with the birth of the monsters he brought forth – and so on and so forth. This attitude is widespread. It is the basis of almost all biographies and autobiographies. It is found in really great thinkers (Socrates, a few hours before his death, gets rid of his wife and children so that he can chat about profound things with his adoring students: *Phaedo* 60a7. The artistic parallel is told with gusto and much hatred by Claire Goll in her autobiography, *Ich verzeihe keinem*, Munich 1980), but it is also quite common among the academic rodents of today.

To me this attitude is alien, incomprehensible and slightly sinister. True, I, too, once admired the phenomenon from afar; I hoped to enter the castles from whence it spread and to participate in the wars of enlightenment the learned knights had started all over the world. Eventually I noticed the more pedestrian aspects of the matter: the fact, that is, that the knights – the professors – serve masters who pay them and tell tham what to do: they are not free minds in search of harmony and happiness for all, they are civil servants (*Denkbeamte*, to use a marvellous German word) and their mania for order is not the result of a balanced inquiry, or of a closeness to humanity, it is a professional disease. So while I made full use of the sizeable salaries I got for doing very little, I was careful to protect the poor humans (and, in Berkeley, dogs, cats, racoons, even a monkey now and then) who came to my lectures from the disease. After all, I said to myself, I have some kind of responsibility for these people and

I must not misuse their trust. I told them stories and I tried to strengthen their natural contrariness, for this, I thought, would be the best defence against the ideological street singers they were about to meet: *the best education consists in immunizing people against systematic attempts at education*. But even these friendly considerations never established a closer bond between me and my job. Frequently, when driving by the university, be it now in Berkeley, or in London, or in Berlin, or here in Zürich where I am paid in solid Swiss Francs, I was startled at the thought that I was 'one of them'. 'I am a professor,' I said to myself – 'impossible – how did it happen?'

Concerning my so-called 'ideas' my attitude was exactly the same. I always liked to debate with friends, about religion, the arts, politics, sex, murder, the theatre, the quantum theory of measurement and many other topics. In such discussions I took now one, now another position: I changed positions — and even the shape of my life — partly to escape boredom, partly because I am counter suggestive (as Karl Popper once sadly remarked) and partly because of my growing conviction that even the most stupid and inhumane point of view has merit and deserves a good defence. Almost all my written . . . well, let us call it 'work', starting with my thesis, arose from such live discussions and shows the impact of the participants. Occasionally I believed I had thoughts of my own – who does not now and then become a victim of such delusions? – but I would have never dreamt of regarding these thoughts as an essential part of myself. I, so I said when considering this matter, am very different indeed from the most sublime invention I have produced and the most deeply felt conviction that pervades me, and I must never permit these inventions and convictions to get the upper hand and to turn me into their obedient servant. I might even 'take a stand' (though the practice and even the phrase with its Puritanical connotations put me off), but when I did so, then the reason was a passing whim, not a 'moral conscience' or any other nonsense of that kind.

There was another element hidden behind my unwillingness to 'take a stand', and I have discovered it only recently. I wrote AM partly to tease Lakatos (who was supposed to write a reply but died before he could do so) and partly to defend scientific practice from the rule of philosophical law. Having absorbed Ernst Mach when about fifteen and having been a student of Hans Thirring and Felix Ehrenhaft in physics, I took it for granted that the work of scientists was self-supporting and did not need any

outside legitimation. I got impatient with people who though lacking any experience of the complexity of scientific research still claimed to know what it was all about and how it could be improved. I guess I was a kind of a scientific libertarian and my battle cry could have been 'leave science to the scientists!' Of course, I had once been a rationalist myself – but it needed only a simple practical example, it needed only Prof. von Weizsäcker's concrete arguments, in Hamburg, back in 1965 (I believe), to reveal the shallowness of rationalistic orations and to make me return to Mach.

There was a second experience that had a tremendous influence upon me. I repeat it in the words in which I first described it (SFS, 118f):

> In the years 1964ff Mexicans, Blacks, Indians entered the university as a result of new educational policies. There they sat, partly curious, partly disdainful, partly simply confused, hoping to get an 'education'. What an opportunity for a prophet in search of a following! What an opportunity, my rationalist friends told me, to contribute to the spreading of reason and the improvement of mankind! What a marvellous opportunity for a new wave of enlightenment! I felt very differently. For it dawned on me that the intricate arguments and the wonderful stories I had so far told to my more or less sophisticated audience might be just dreams, reflections of the conceit of a small group who had succeeded in enslaving everyone else with their ideas. Who was I to tell these people what and how to think? I did not know their problems though I knew they had many. I was not familiar with their interests, their feelings, their fears though I knew they were eager to learn. Were the arid sophistications which philosophers had managed to accumulate over the ages and which liberals had surrounded with schmaltzy phrases to make them palatable the right thing to offer to people who had been robbed of their land, their culture, their dignity and who were now supposed to absorb patiently and then to repeat the anaemic ideas of the mouthpieces of the oh so human captors? They wanted to know, they wanted to learn, they wanted to understand the strange world around them – did they not deserve better nourishment? Their ancestors had developed cultures of their own, colourful languages, harmonious views of the relation between man and man and man and nature whose remnants are a living criticism of the tendencies of separation, analysis, self-centredness inherent in Western thought . . . These were the ideas that went through my head as I looked at my audience and they made me recoil in revulsion and terror from the task I was supposed to perform. For the task – this now became clear to me – was that of a very refined, very sophisticated slavedriver. And a slavedriver I did not want to be.

This experience was similar in nature to my experience vis-a-vis physics. There, too, I had strongly felt the superficiality and presumptions of a philosophy that wanted to interfere with a well formed practice. However while science is only part of culture and needs other ingredients to arrive at a full life, the traditions of my audience had been complete from the very beginning. Thus the interference was much more serious and much stronger resistance was needed. Trying to build up such resistance I considered intellectual solutions, that is, I still took it for granted that it was up to me and the likes of me to devise policies for other people. Of course, I intended to devise much better policies than those imposed by President Johnson and his aides but in doing so I, like he, took responsibility away from those I wanted to help, I dealt with them as if they were not capable of taking care of themselves. It seems that I was aware of this contradiction and it was this unconscious awareness that made me act in a distant and unconcerned way and made me refuse 'to take a stand'.

Now comes the third experience on my path – my acquaintance with Grazia Borrini, a gentle but determined fighter for peace and self reliance. Grazia had studied physics, as I had. Like me she had found this study too confining. But while I was still using abstractions (such as the idea of a 'free society') to arrive at a wider and more humane point of view, her ideas were part of 'historical traditions' (to relapse into my own constipated manner of speaking). I *did* know about these traditions and I had written about them even before I met Grazia, but again it needed a concrete encounter to make me realise what that implied. Grazia also gave me books and papers written by outstanding scholars dealing with the problems of economic and cultural (ex)change. This was a real find. First, I now had much better examples of the limits of a scientific approach than those I had been in the habit of using (astrology, voodoo, a little bit of medicine). Secondly I realised that my efforts had not been in vain and that it needed only a slight change in attitude to make them effective, both in my own eyes, and in the eyes of others. You *can* help people by writing books. I was very surprised and deeply moved when I noticed that people from different cultures whose actions I respected had read some of the things I had written and had welcomed them. So, I finally gave up my self-cynicism and decided to write one last, but good book, for Grazia, because I know her and because I write best when I have a smiling face before me (remember, I wrote AM with Imre Lakatos in mind), and, through her, for all the people who

despite hunger, oppression, wars try to survive and to achieve a little bit of dignity and happiness. Of course, to write such a book I shall have to cut the remaining strings that still tie me to the abstract approach or, to revert to my usual irresponsible way of talking, I shall have to say

FAREWELL TO REASON.