A Science of Mars or of Venus?

MARY TILES

> Western science, from Heraclitus to Hiroshima, has only known martial Nature . . . The laws of Venus–Mother Nature cannot be deciphered by the children of Mars . . . [1]

For as long as there has been anything worthy of the name of science, there have been those who have criticized its claim to superior knowledge. With the birth and prodigious growth of modern science, the corresponding growth of critical opinion led, in the eighteenth century, to a divorce of the sciences from the humanities around which our educational institutions, and our universities in particular, have been built. It is this divorce which renders problematic the status of the social or human sciences. For the extent to which Man can be an object of scientific knowledge will be questioned by those insisting on an opposition between human knowledge and values as embodied in the humanities, and the dehumanized objective knowledge proclaimed within the natural sciences.

It is not my purpose here to become embroiled in any of these long-standing debates. They are mentioned merely to contrast criticisms of the natural sciences which are based on a presumption of this dichotomy with recent, internal critiques of natural science. These latter critiques come from a number of rather diverse sources (feminism, the movements for alternative medicine and appropriate technology, advocates of time irreversibility in physics, some African philosophers and others claiming to speak for the Third World, for example). There is, however, a common theme—the need to extend the horizons of science by breaking down the rigid division between the natural and the human sciences *not* by subsuming the human sciences under the natural sciences as presently constituted, but by reconsidering the project of the natural sciences themselves. In short the call is for development of science with a human face, science which aims more at co-operating with than at conquering Nature, which learns more by conversing or conducting a dialogue with Nature than by putting it on the rack, to force it to reveal its secrets, a science of Venus rather than a science of Mars.

[1] Michel Serres, *Hermes: Literature, Science, Philosophy*, J.V. Harari and David F. Bell (eds) (Baltimore: Johns Hopkins University Press, 1982), 99.

Such criticisms are potentially more radical in their effect than those of any anti-science movement, for the latter presume the *status quo*, accept the battle lines as currently drawn up and are prepared for combat which both sides know can never be mortal. The former, however, seek neither to reject nor to mutilate science but to enrich it by changing it. However, the two types of criticism have not always been distinguished, for they do have one element in common—they both rest on a rejection of values claimed to lie at the heart of modern, technological science. The customary defence against such criticism is to deny that values play any significant role in the natural sciences. If successful such a move can insulate science against criticism without there being any need for further discrimination amongst its critics. Thus we see increasing numbers of philosophers of science rallying under the banner of Realism to defend the view of science as aiming at objective Truth and as possessed of methods of theory choice which, even if they do not guarantee truth, do at least ensure objectivity by preventing the intrusion of non-scientific interests or values into theory choice.

I shall argue, however, that this move will not serve as a defence against the possibility of radical, internal critiques of modern science for two reasons. First, because there are crucial unclarities in its proposed 'ultimate criterion'[2] of theory choice, unclarities which serve to mask the intrusion of those very values to which critics most frequently take exception. Secondly because it is a defence mounted in the wrong place; it is a defence of science as constituted by the content of its theories, whereas the attack falls on science as delimited and determined by its methods and on the attitudes which lie behind their use in the normative definition of science and the scientific. In other words a defence of theory is mounted by appeal to method, but the attack falls on aspects of the implementation of that very method and on its formative role in science, rather than falling directly on theoretical content:

> This 'logic of domination' is not merely a problem of the abuse of science or its misapplication; it is rather embedded in the methodology itself. By this method, means and ends have become completely scrambled.[3]

Positions

Isaiah Berlin[4] describes the modern scientific tradition as the tradition of those who believe (i) that it is possible to make steady progress in the

[2] W. Newton-Smith, 'The Role of Interests in Science' in *Philosophy and Practice*, A. Phillips Griffiths (ed.) (Cambridge University Press, 1984), 70.

[3] M. Berman, *Social Change and Scientific Organizations: The Royal Institution 1799–1844* (London: Heinemann, 1978), xviii.

[4] Isaiah Berlin, 'The Divorce between the Sciences and the Humanities' in his *Against the Current* (Oxford University Press, 1981).

entire sphere of human knowledge, (ii) that methods and goals are, or should be, ultimately identical throughout this sphere, and (iii) that we have reached a stage where the achievements of the natural sciences are such that it is possible to derive their structure from a single set of clear principles which, if correctly applied, make possible indefinite further progress in unravelling the mysteries of Nature. He goes on to link this with a tradition in Western thought which he traces back to Plato and characterizes as resting on the following assumptions:

(a) Every genuine question has exactly one true answer; all others being false.
(b) The method which leads to correct solutions to all genuine problems is rational in character and is, in essence, if not in detailed application, identical in all fields.
(c) The solutions, whether or not they are discovered, are true universally, eternally and immutably: true for all times, places and men.

Berlin goes on to say:

> The implication of this position is that the world is a single system which can be described and explained by the use of rational methods; with the practical corollary that if man's life is to be organized at all, and not left to chaos and the play of uncontrolled chance, then it can be organized only in the light of such principles and laws (op. cit. note 4, p. 81).

The voices urging departure from this tradition reject one or more of assumptions (a)–(c) and thus also the practical implication drawn from them. For example:

Those development professionals who claim to speak for the Third World stress the way in which the priorities of the affluent societies within which scientific experts are educated (sophisticated armaments, diseases of the overfed and ageing, multiplicities of costly drugs, high input mechanized agriculture, and so on) distract scientific attention away from the problems facing poorer countries to the extent that these problems are not visible to the expert:

> The values and preferences of first professionals are typically polar opposites of last realities . . . Most professionals see first values as sophisticated and scientific, and last realities as primitive and based on ignorance.[5]

Marxist historians of science have linked the rise to dominance of scientific/technological modes of thought with the rise of capitalism and hence with class struggle:

[5] Robert Chambers, 'Professional Thinking and Rural Poverty: Putting the Last First', paper delivered to the Other Economic Summit 1985, p. 3.

> The perception of reality in scientific terms is not the result of the successful, inevitable progress of the history of ideas. It is, rather, rooted in class society, and in this sense it was the Industrial Revolution that put the Scientific Revolution on the map. Defined as a commodity or as the crux of professional expertise, science was, in the nineteenth century, recreated in capitalism's image. The very success of the latter obscured the ideological roots of science, as might be expected; and the rather obvious decay of industrial society (whether socialist or capitalist) has led us, just as inevitably, to seek to uncover them (Berman, op. cit. note 3, p. 189).

Feminists, on the other hand, have seen scientific culture as the product of a distinctively male consciousness, embodying forms of rationality which are alien to women. They have found in this an explanation for the continued under-representation of women in science. But as Evelyn Fox Keller argues,[6] the rejection of science as 'male' by women is a temptation which should be resisted. To follow this path is to leave rationality and objectivity in the male domain, securely dominated by men, thus forcing women to retreat to a purely 'female' subjectivity:

> By rejecting objectivity as a masculine ideal, it simultaneously lends its voice to an enemy chorus and dooms women to residing outside the realpolitik of modern culture; it exacerbates the very problem it wishes to solve. It also nullifies the radical potential of feminist criticism for our understanding of science. As I see it, the task of a feminist in science is twofold: to distinguish that which is parochial from that which is universal in the scientific impulse, reclaiming for women what has historically been denied to them; and to legitimate those elements of scientific culture that have been denied precisely because they are female (op. cit. note 6, p. 593).

She claims that there are, and always have been, elements in scientific culture which embody values traditionally classified as 'female'. The opposition between conceptions of science as 'dominating' and as 'conversing with' nature is seen as representing

> . . . a dual theme played out in the work of all scientists, in all ages. But the two poles of this dialectic do not appear with equal weight in the history of science. What we therefore need to attend to is the evolutionary process that selects one theme as dominant . . . (op. cit. note 6, p. 601).

Her suggestion is that it is in the process of selection that ideological forces come into play and that it is therefore at this level that masculine

[6] Evelyn Fox Keller, 'Feminism and Science', *Signs: Journal of Women in Culture and Society* **7**, No. 3 (1982).

ideology will be found to have influenced perceptions of the history of science and hence also the conception of science itself.

A recent voice speaking for one of Fox Keller's less dominant themes has been that of Ilya Prigogine.[7] He sees the basic conflicting attitudes of the 'two cultures' as grounded in an opposition between the atemporal view of classical science and the historical time-orientated view of the social sciences and humanities. His claim (based on his work in thermodynamics) is that modern science is rediscovering time (op. cit. note 7, p. xxviii) and that to the extent that time-irreversible processes become fundamental in physics, science itself will have to abandon its atemporal vantage point. One source of conflict between scientific and non-scientific world views would then have been removed:

> Must we choose between a science that leads to alienation and an antiscientific metaphysical view of nature? We think such a choice is no longer necessary, since the changes that science is undergoing today lead to a radically new situation (op. cit. note 7, p. 7).

> We can no longer accept the old *a priori* distinction between scientific and ethical values. This was possible at a time when the external world and our internal world appeared to conflict, to be nearly orthogonal. Today we know that time is a construction and therefore carries an ethical responsibility (op. cit. note 7, p. 312).

The Defence of Scientific Neutrality

The first move made in defence of the view that science is value free, using objective standards to select theories which approximate ever more closely to the truth about the physcial world, is concessionary. It has to be admitted that political and economic concerns frequently influence the direction of science. Similarly it has to be admitted that the predominance of white middle-class males from developed countries in the scientific professions may affect the selection of research topics. In other words it is allowed that non-scientific interests and values have a role in determining the *direction* of scientific research. What is rejected is the claim that values have a role in determining the *content* of science. Scientific results, if acceptable at all, should be acceptable to anyone, regardless of his concerns, because the ultimate standard of acceptability is empirical adequacy. It is ultimately Nature, not the scientist, who determines which theory will survive.

It will then be suggested that all potentially radical critiques of science in fact fall on those factors which determine the direction of

[7] Ilya Prigogine and Isabelle Stengers, *Order Out of Chaos: Man's New Dialogue with Nature* (London: Fontana, 1985).

scientific research, and not on factors determining the results of that research once its direction is determined. It is admitted that these factors may currently be such as to encourage research in those areas which hold promise of military application, to discourage research on matters (such as development of a male contraceptive pill) which might be expected to be of more concern to women than men, to discourage medical research on, for example, cheap preventive measures or the effectiveness of homoeopathic remedies, which might not be in the interests of the medical establishment, to discourage research on low input agriculture, which is not in the interests of the chemical companies funding agricultural research, and so on. But the fault here lies with political and social formations, with the professional organization and funding of science, not with science or its methods as such. Military, male-dominated, first world science is, and can be, no different *qua science* from pacific, female-dominated or Third World science.

In order to complete this argument it is necessary to insist on the distinction between the practice of science and its cognitive content. For science is being defended only as a putative body of objective knowledge; the totality of current scientific practice is not being defended. However, this distinction cannot be made as sharply as seems to be supposed. For the defence of science as a body of knowledge is made by appeal to method. It is argued that in the natural sciences empirical adequacy provides an ultimate objective criterion (op. cit. note 2). Given a choice between two competing theories, that which is empirically more successful is that which should be chosen. Where relative merits on this score are unclear, experimental testing of both should be pursued. There may be situations in which theory choice is not *determined* by relative empirical adequacy, but there can be no going against this standard. It cannot be overridden by other concerns, resulting in choice of the less empirically adequate theory. Globally it is argued that the manifest success of science at the level of conferring increased ability to predict and control suggests that non-scientific interests have not in fact played any significant role in theory choice (empirical adequacy has not been overridden). Hence such interests have not played any significant role in determining the cognitive content of science.

There are two claims here. The first is a *normative* claim about how theory choice *should* go. The second is an *evaluative* claim that science has been successful. This success is itself taken as evidence that past choices have in fact (by and large) been made as good scientific choices should have been made (in such a way as to exclude non-scientific interests and values).

It has been argued[8] that this prescription and this evaluation are grounded in an interest which is universal. Every human being in his right mind (every rational person) has an interest in increasing his technological control over his environment, for only in this way can his living conditions be improved and his life be made more secure. If this is the case, then increased ability to predict and control should command universal respect. But from the fact that in some sense of the words everyone values the ability to predict and control, it does not follow that everyone must or should respect and exclusively value the particular forms of technological control achieved by modern science. This is a point to which we shall return after completing the strategy of the Realist defence.

The Realist tends to focus attention on scientific laws and theories (the content of science), insisting that both the truth-or-falsity and scientific acceptance or rejection of any proposed law is independent of whatever factors may have conditioned the route to its proposal. The context of discovery should not be confused with the context of justification. The strength of the claim that values play no role in determining the content of science derives largely from the plausibility of the claim that laws such as the second law of thermodynamics, or of gravitational attraction, could not, and would not, be rejected by any of those mounting critiques of modern science. They do not propose, even as possible, societies equipped with perpetual motion machines, air transport provided by courtesy of lumps of matter not affected by gravitational forces, and in which kettles are boiled by standing them on icebergs. It is because critics of modern science do not reject the laws of its core theories (interpreted as acceptance of their truth), that it is argued that science (its cognitive content) is independent of interests and values.

Breaching the Defences: The Possibility of Radical Critique

But this is to miss the point of the more sophisticated critiques of science, for what is under attack is precisely exclusive attention on this conception of cognitive content. What is attacked is the idea that any law or theory which withstands tests devised in a certain way could ever represent the whole truth on any matter. The claim that science, including its content, is conditioned by interests and values, is not the claim that content is *determined* by these conditions, but that it is *limited* by them. This does not require rejection of scientific results.

[8] Charles Taylor, 'Rationality', in *Rationality and Relativism*, M. Hollis and S. Lukes (eds) (Oxford: Basil Blackwell, 1982), 101.

The mistake made in failing to recognize the conditioned nature of the cognitive claims of science is that of proclaiming as the whole truth (truth *simpliciter*) that which is, and can only ever be, a partial truth, something which is not to be rejected, but which is not to be over-extended either. Newtonian mechanics, still highly useful, must be distinguished from Newtonian mechanism as a world view, which no longer plays the role of guiding scientific research. Similarly scientific laws in general must be distinguished from the world view determined by projecting them along with the methods of modern science as the whole truth and as sole arbiters of truth. A conditioned view is not false, but partial. What is to be rejected is the claim that it is unconditioned. The two-valued rhetoric of truth and falsity must be avoided if there is to be room for non-destructive criticism.

Now it is not at all clear that one can rest with the idea that the cognitive content of science is simply a sum of claims made in individual statements in its theories. An integral component of the conception of *scientific*, as opposed to general, knowledge is that it is theoretical in the sense of being systematically organized. Laws do not merely function as true or false statements but serve in the provision of explanations and in the organization and classification of phenomena. Theories impose a structure, and this structure is arguably part of the cognitive content of the theory. To claim that a theory is correct is not just to claim the truth of its laws taken individually but also to make claims about the interrelations between laws and hence also between the phenomena to which they apply. Some laws, such as the conservation of energy, are treated as being more fundamental than others, such as Hooke's law, and fundamental laws of a given theory (such as the laws of motion in Newton's mechanics) are unlikely to be independent parts of that theory. Within theories laws are given an epistemological value based on their theoretical, explanatory role rather than on their mere empirical adequacy. The far-reaching consequences of reversals of values of this kind can be seen in the current debate over the status of thermodynamics. Is it a non-fundamental, derivative science, dealing only in phenomenal laws with quantum mechanics and particle physics as the underlying fundamental theories, or is it to be treated as fundamental in the way that Prigogine argues? Whatever the merits of the arguments, it cannot, I think, be denied that a decision one way or the other here has a profound effect not only on the cognitive content of the theories concerned, but also on that of future science.

If, then, the content of science is extended to cover theories and not merely laws, it would appear that place must be accorded at least to the notion of *epistemological value*. The epistemological value accorded to a law or group of laws may change even though they continue to be widely used and accepted. But in this case their significance, their scientific, theoretical content will have changed. Criticisms which bear on epistemological values can thus have a bearing on the content of science, and epistemological values are themselves a function not only of empirical adequacy but also of explanatory ideals and conceptions of the goal of scientific endeavour.

This can be further illustrated by returning to the argument which goes from the manifest success of science to the absence of influence by non-scientific interests. When we ask for evidence of the success of science, we find ourselves directed to the marvels of modern technology. This suggests, following the pattern of inference used, that past theory choices have been made with an eye to technological applications. But we might well ask whether an interest in technology is an intrinsically scientific interest. Was this what was meant by the criterion of empirical adequacy? What interests are internal to science? This depends on how the goal of science is characterized.

The new science proclaimed by Bacon and prefigured in the new astrology and magic of the Renaissance magi seeks to shift the *primary* focus of scientific attention away from contemplatively perceived truth to the goal of mastery over nature. The pursuit of truth is no longer disinterested; the interest is in increasing man's ability to dominate and control. Knowledge is sought and valued to the extent that it confers this ability. Thus Bacon says:

> It is by witness of works, rather than by logic or even observation, that truth is revealed and established. Whence it follows that the improvement of man's mind and the improvement of his lot are one and the same thing.[9]

Genuine knowledge is seen as the only route to superhuman (god-like) power and therefore also to being or becoming more god-like. It is thus quite correct to say, with Newton-Smith (op. cit. note 2, p. 59), that without an interest in prediction and control modern science would not exist. But it should at the same time be noted (a) that this interest is an expression of a particular conception of the nature of Man and of his relation to Nature, (b) how this interest determines the conception of what constitutes scientific knowledge—the knowledge sought, and (c) how it also determines the empirical, evidential base of science.

(a) The scientific, intellectual interest in prediction and control presupposes a view of Man on which such an interest is worthy of his dignity, an expression and fulfilment of a distinctively human potential, which raises man above the animals and the rest of nature.

(b) The knowledge which confers power is primarily knowledge of laws of action and of the contexts in which they are applicable. It

[9] Francis Bacon, 'Thoughts and Conclusions' in B. Farrington, *The Philosophy of Francis Bacon* (University of Chicago Press, 1966), 93.

dictates a shift away from the Aristotelian goal of knowledge of natures qualitatively expressed in terms of natural dispositions and tendencies. This is a classic illustration of a shift of epistemological values, for the knowledge most valued by Aristotelians is not rejected by Baconians, it is indeed still necessary, but it is no longer the goal. Similarly, knowledge of regularities was recognized by Aristotelians, but did not form the goal of Aristotelian science; those disciplines dealing in regularities (astronomy, optics) were not granted the status of true sciences.

(c) Knowledge of how to divert the course of nature cannot be obtained merely by observing the natural course of events, by watching nature take her course. For the aim is not merely to imitate nature but to dominate it, to achieve things never naturally achieved (how else are supernatural powers to be revealed?). Thus the primary learning will be from attempts to manipulate, from artefacts and from artificially contrived situations. Here too there is a shift of epistemological values. The observation of artefacts available to Aristotelians was discounted by them as not productive of the sort of knowledge required by natural philosophers. Bacon turns the tables, placing much less value on naturally occurring phenomena and much more on experimental situations. Internal critiques of science seek to redress the balance, rescuing knowledge of the natural from its seventeenth-century devaluation.

Here there is a very strong sense in which interest in mastery over nature shapes the content of science; it determines what form interesting knowledge shall take. It involves redrawing the conception of the natural thought of as that which does not require explanation. It can no longer be that which occurs naturally, in the course of nature, or that which is explicable by reference to the nature of a thing. It is now that which occurs according to a basic law of action, when this is not subject to interference. The whole conception of what needs and what does not need explanation, of what is fundamental and what derivative, is changed.

But if the aim of science is to obtain knowledge which confers power, there may be several distinct clarifications of this aim, depending on the further specification of the power sought. Evidence of the success of science, evidence that the knowledge presented does indeed confer power requires exercise of that power. If the power sought is that of technological innovation, then production of new technology is evidence of success. If the power sought is merely the ability to dominate and conquer Nature, then obliteration of pests, development of powerful defoliants and other weapons of destruction is evidence of success. If the power sought is power to improve the lot of Mankind in general, and of its poorer groups in particular, then ability to provide on a large scale food, water, housing in stable communities viable not just for a limited period, but in the long term, and without sacrificing the opportunity for others to have the same benefits, would count as success.

The Davy lamp was heralded as a success for science. Davy had solved a problem, that of producing a lamp which would burn in a methane-rich atmosphere without causing an explosion. But in what sense was this a success for science? Davy had analysed the problem, come up with a theoretical solution and tested it in his laboratory. But as a miner's safety lamp it was hardly a success. In the conditions in which it was used down the mines, it did cause explosions. Davy lacked the knowledge of mining necessary to perceive the difference between his problem, which he had solved, and the problem of producing a lamp which had the required property when used down a mine, which he had not solved. The problem put before Davy was the mine-owner's problem of how to get a light that would function in the methane-rich atmosphere of 'crept' workings. The miners' concern, on the other hand, was the reduction of accident rates. The Davy lamp benefited the mine owners, but at the expense of the miners. (The lamp was introduced in 1816. In Durham and Northumberland in 1798–1816 there were twenty-seven explosions and 447 deaths, in 1817–35 there were forty-two explosions and 538 deaths.) Was this then a success to be notched up for science?[10]

What has to be recognized is that the knowledge which is sufficient for success in the laboratory, or for observational success, does not on its own confer the other powers, nor can the methods for achieving this success automatically be assumed to be those which will yield success in wider spheres. The question of the extent to which this knowledge is necessary to and has been productive of success in wider fields is the central issue in debates about the relation between science and technology and about the role of technology in the solution of human problems. The theoretical and explanatory endeavour of the natural sciences has been analytic and foundational—the quest for fundamental laws, for basic constituents of matter, the basic building blocks by reference to which the characteristics of all more complex objects and phenomena are to be explained. The empirical investigative methods which form part of this endeavour have been correspondingly analytic, concentrating on the isolation of causal factors, on the creation of controlled experimental conditions in which the effects of variations in single variables can be studied. But the application of knowledge gained in this way depends on skills and knowledge of a different kind. Analysis has to be accompanied by synthesis, work in controlled

[10] For further discussion of Davy's work and its social and political context see op. cit. note 3, and David Albury and Joseph Schwartz, *Partial Progress* (London: Pluto Press, 1982), Ch. 1.

environments has to be extended to work in much more variable conditions. Knowledge of the variable conditions of application is as important as knowledge of fundamental theory, practical skill is as important as theoretical understanding.

It is easy to see how emphasis on the analytic approach can lead away from any immediate practical concerns to total immersion in fundamental problems, whose answers raise further problems of their own. Puzzle-solving is fun in itself and does not have to be related to any further pay-off. Disinterested science as a puzzle-solving activity with a momentum of its own is quite understandable. What then becomes problematic is the relation of this autonomous activity and its results to wider concerns.

The fate of Scholastic philosophy should serve here both as an illustration and as a warning. The fact that modern science is experimental does not insure it against scholasticism (immersion in a puzzle-solving tradition divorced from its roots and hence ultimately directionless and of little relevance). Laboratory experiments involve manufactured conditions and problems raised by these conditions can spawn one another just as easily as pure theoretical problems. When empirical success means success purely relative to laboratory experiments the standard becomes wholly internal to the research tradition and prediction and control is narrowed to ability to predict what will happen in carefully controlled conditions together with the ability to produce those conditions. The valuation of the theoretical over the practical tends to restrict the content of science to the point where its practical value may be called into question.

These debates cannot be pursued further here. The present point is simply that once the notion of scientific interest is extended beyond truth to some form of power or control, the question of what constitutes a scientific and what a non-scientific interest is no longer clear cut. This is not to reject the extension, for it is only this move which makes empirically grounded, theoretical knowledge of the world possible. The defenders of scientific neutrality are right to stress the importance of empirical success as a criterion for the assessment of claims to scientific knowledge, if wrong to think that it follows that science is value-free, that there is no room for debate about what constitutes success, or that the nature of the scientific project is beyond dispute. Absence of values is not a precondition of objectivity.

Even if it is maintained that science aims solely at knowledge, at the construction of true theories about the physical world so that its only internal interest is in truth and in empirical adequacy as a necessary condition of truth, empirical adequacy can still mean *either* predictive observational success, constituted by agreement with a predefined, fixed class of possible observations of natural occurrences (saving the phenomena), or experimental success, the prediction and successful production of novel, artificial, phenomena. The difference between these clearly constitutes a difference in the evidential base against which scientific theories are assessed. The significance of the difference can be brought out by considering the change which occurs with the shift to modern, experimental science, which is, as we have seen, dictated by a changed conception of the sort of knowledge sought. This change was in turn seen to be related to the value placed on that knowledge and to the wider interests that it was hoped would be served by obtaining it.

There are two components present at the dawn of modern science[11] and it is important to see that acceptance of one does not entail acceptance of the other. First there is a reorientation of values in which active involvement with the physical world and a concern for material well-being come to be seen as expressive of human dignity, rather than as something to be despised by the philosopher, the seeker after truth. The disinterested contemplation of truth was previously valued for its effective distancing of man from the material world. It was a disengagement from the technological and the practical and thus expressive of human dignity defined more strictly in terms of spiritual and intellectual, as opposed to material, life. But of course the reorientation urged by Bacon and Dee was not, when they wrote, a reality. Science conforming to that ideal did not really begin to emerge until the mid-nineteenth century. Even now science seems not to be fully prepared to endorse the unification of theory with practice that would be entailed. Pure research science is, on the one hand, characterized as the disinterested pursuit of truth (for its own sake) without regard to possible applications, and on the other urged to be necessary to ensure future technological progress. This tension in the image of science was present at the outset. And here we come to the second component.

Given that active involvement with the physical world is endorsed and incorporated into the goal of science, so that interest in successful engagement becomes an interest internal to science (a scientific interest), there is still a question of the basis from which this engagement is made and on which it is valued.

> What can be said about nature, is she an enemy or a slave, an adversary or a partner in a contract . . . ? (op. cit. note 1. p. 105).

For Dee and Bacon the basic relation is that between superior and inferior. Through his engagement with the world Man will demon-

[11] These are more fully discussed in Mary Tiles, 'Mathesis and the Masculine Birth of Time', in *International Studies in the Philosophy of Science*, Vol. 1 (London: Routledge & Kegan Paul, 1986).

strate his superiority, show that he is set apart from the rest of Nature, capable by virtue of his intellect of transcending it. The standpoint at which he aims is still a standpoint outside the world, a standpoint distanced from the object of his scientific study and of his manipulation. It is this which continues to feed the conception of the objectivity of scientific knowledge as consisting in disengagement, as being disinterested, not materially conditioned, and hence as value-free. The intellect places man beyond Nature and there is a presumption that only intellectual knowledge, knowledge from a disengaged, non-human perspective can confer real power over Nature. We are thus heirs to a paradoxical conceit of knowledge characterized as neutral, disinterested, value-free, not materially conditioned, but none the less sought and valued just in so far as it confers material power. The quest for such knowledge is far from being disinterested.

But the basic relation need not be that postulated by Dee and Bacon. The exploration of alternatives is what is implicit in the many and various internal critiques of science. These accept the fundamental reorientation towards the practical and towards active involvement with the world both as a means and as an end in science, but question the basis on which the engagement is made.

Any proposal which seeks to base science on a relation between Man and Nature in which the two are put on a more equal footing will have the effect of treating Man as more fully a part of Nature. This necessarily requires abandoning the presumed availability of a non-materially conditioned viewpoint. This does not however entail abandonment of standards of objectivity. The importance of successful completion of practical projects and of reliably repeatable methods of achieving results can still be recognized even when it is realized that projects, and hence success or failure in them, are set and judged against a background of conditions which determine and limit their significance:

> . . . there is a tendency to forget that all science is bound up with human culture in general, and that scientific findings, even those which at the moment appear the most advanced and esoteric and difficult to grasp, are meaningless outside their cultural context.[12]

Royal Institute of Philosophy

[12] E. Schrödinger, 'Are there Quantum Jumps?' *British Journal for the Philosophy of Science* **III** (1952), 109.