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The Anthropology of Science

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Anthropology is loosely and popularly defined as the study of 'primitive' societies and cultures, where 'primitive' means 'pre-literate', 'pre-scientific', and 'technologically simple'. And sociology is usually distinguished from anthropology in that it is supposedly concerned with the study of scientifically and technologically advanced societies. This definition of anthropology, however, and the distinction between anthropology and sociology, are the result of purely accidental historical circumstances and there is no good reason why one should not do anthropology, so to speak, upon technologically advanced societies such as our own and upon the sub-societies and sub-cultures of such societies. One such sub-culture in our society is the sub-culture of science, the life-world of the scientific community, and I believe that an anthropological investigation of that cultural 'world' is not only possible and legitimate but could also be profoundly illuminating to anyone interested in the human significance of that extraordinary invention which we call 'science'.

Historically, it is true that anthropologists have for the most part concerned themselves with pre-scientific cultures. And that has meant of course that they have been occupied mainly with non-European or non-Western peoples. Anthropology, as a formal discipline, is a child of the nineteenth century and there is no doubt that it has been powerfully shaped by nineteenth century beliefs and attitudes. For the men of the nineteenth century science and technology were the great marks of civilization, so that any society which had not developed Western-style science and technology was by definition 'primitive' in the pejorative sense, that is to say, infantile or not fully developed. Some scholars in fact have linked the

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emergence of anthropology with the rise of European colonialism. Anthropology was, as it were, an expression of, and served as a reinforcement of, the sense of superiority that the European colonisers felt towards the Asians and the Africans and others whom they colonised. (The great French anthropologist, Claude Lévi-Strauss, who has spent most of his scholarly career demonstrating that the so-called 'primitives' are far from being primitive in the pejorative sense, remarks somewhere that the only definitive way of distinguishing Western culture from so called primitive cultures is that we have anthropologists and they don't. Levi-Strauss is far from confident that this is a mark of cultural superiority!) (1)

Whatever may be said about the historical and social circumstances of the birth of anthropology, what is important is that, in their investigations of pre-scientific and technologically simple cultures, the anthropologists were eventually forced to develop a distinctive method or approach. Thus, for example, in contrast to the 'objective' and 'impersonal' and 'neutral' attitude that is supposed to characterise the natural scientist, the twentieth century anthropologist found that he has to live with his tribe and participate as sympathetically as he can in the life-world of the tribe. An anthropologist attempting to understand one of the Australian aboriginal peoples, for example, would not get very far if he adopted an impersonal and 'objective' approach to his subjects - handing out questionnaires, conducting poll-type surveys and running the data through

1. The historical and critical self-awareness of anthropology has been a recent development. See, for example, the articles collected in Current Anthropology, 9, 5, 1968; J. Dwyer, 'The Dialogue of Ethnology', in Dialetical Anthropology 4, 3, 1979, 105-24; J. Copans, Critiques et politiques de l'anthropologie, Paris, 1974; G. Dening, Islands and Beaches: Discourse on a Silent Land: Marquesas 1774-1880, Melbourne, 1980: see especially pp.35-44 'On History at the Edges of Culture'.

a computer. At the same time, of course, if the anthropologist goes completely 'native', and becomes wholly immersed in the life of his tribe, he will never succeed in gaining a scientific understanding of that tribal society. While being a participant in the culture he is investigating, he must also stand outside that culture and observe it with a detached onlooker's eye, a 'stranger's' eye. Thus he must distinguish between, on the one hand, the accounts that the members of the tribe give about their culture and, on the other hand, the realities of life in that culture. Or, from another point of view, he must distinguish between the 'cultural discourse' of a people and the underlying 'grammar' of that discourse. As Lévi-Strauss says, the anthropologist seeks to understand the hidden structural 'rules' which govern everything that goes on in the culture - kinship relationships, sexual taboos, forms of art and architecture, modes of cooking, table manners, varieties of dress and ornamentation, attitudes to land cultivation etc. - rules of which the members of the tribe are usually 'unconscious'.

The happy and successful combination of those two attitudes - imaginative and sympathetic participation on the one hand, and dispassionate and critical observation on the other - is supremely difficult, but one can observe it, and admire it, in the work of a great anthropologist such as Evans Pritchard. (2)

2. On the difficulties inherent in 'participant observation' see J. Rabinow, Reflections on Fieldwork in Morocco, New York, 1978; J. O'Neill, Making Sense Together, New York, 1974; S. Reinharz, On Becoming a Social Scientist, San Francisco, 1979: see especially ch. 3, 'Dilemmas of Participant Observation'.

The anthropologist is then essentially a participant observer. He is also a relativiser. For the members of the tribe the fabric of their culture is typically seen as being god-given and necessary and unalterable. Thus in Thomas Berger's marvellous novel Little Big Man the insignificant American Indian tribe he describes calls itself 'The People' and sees itself as being at the centre of the universe. For the anthropologist however any culture is simply one possible way of coping with, or managing, or making sense out of the fundamental realities of human existence - birth and death, puberty and adulthood, sexuality and kinship, community and power etc. One can strike a useful analogy with language here. Languages, as the great early 20th century theorist of language, Ferdinand de Saussure, reminded us, are arbitrary, or conventional, or man-made systems of signs. Out of a vast range of possible speech sounds we have selected a sub-set of sounds and then devised rules that govern which speech sounds may be combined with which in order to make up what we call spoken words. We have also devised rules and conventions governing which words may be combined with which in order to make up meaningful sentences - the result being that linguistic system we call English. A particular language such as English is then completely contingent: it is but one possible man-made or conventional system of signs and there is nothing god-given or necessary or absolute about it. It can never claim to be the language instituted by God or dictated by nature. In the same way, for the anthropologist no one culture or cultural form can claim to be god-given or necessary or absolute: it is but one contingent or relative way of coming to terms with certain existential realities - one way out of a vast range of possible ways. In other words, for the anthropologist cultures are characterised by contingency and relativity.

The relativity of human cultures, however, does not necessarily involve us in a position of relativism. In other words, just because a particular culture cannot claim to be the culture, as Berger's Indians claimed to be The People, it does not follow that it may not enshrine any permanent values. One can recognise the fact that a culture is but one possible and contingent way of coping with existence, and that certain moral and political and social and religious values are relative to that culture (in that they can only be understood in terms of that culture), without subscribing to relativism - the philosophical position that epistemological and moral and social and political and religious values do not have any permanence or absoluteness but are simply the reflections of a transient set of socio-cultural conditions. Thus the fact that the values of liberal democracy - individual liberty, equality etc. - have as a matter of history been developed within the context of bourgeois capitalist culture, does not mean that those values are limited to that particular and transient historical context and may be dismissed, as some Marxists claim, as merely 'bourgeois capitalist' values.

Once again, the analogy with language is useful. As we have seen, English is but one possible system of signs and there is nothing necessary or absolute about it: we cannot claim that English is the language. But it does not follow from this that we cannot express certain necessary and absolute(non-relative) truths in English. It would be ludicrous to say that the truth expressed by the English sentence 'Two plus two equals four' is simply an 'English truth'.

I labour this point here since it will become of prime importance when we address ourselves to the anthropology of science. For if it is true, as I wish to argue, that the scientific culture is

but one possible, conventional, 'arbitrary' and man-made way of coping with, or making sense out of, certain realities, and is not god-given or necessary or absolute in any way, it does not follow that science itself does not have any permanent values enshrined within it and that it is simply the transient epiphenomenon of a particular epoch in the history of Western culture.

The problem of relativism is of course a formidably difficult one and cannot be disposed of as quickly and neatly as this. All that has been said here is that, from the fact that scientific knowledge is socially 'constructed', and is in this sense relative to a certain cultural context (and cannot fully be understood save by reference to that context), it does not necessarily follow that what we call science is culturally relative in the sense that it is causally determined by, and explicable in terms of, a particular set of socio-cultural conditions. In other words, it is possible to hold both that scientific knowledge is a social phenomenon and that science is capable of expressing non-relative truths. How those two theses are to be reconciled is another question: we must nevertheless maintain both with equal force. Our slogan, so to speak, must be, 'relativity, yes! relativism, no!'

As I have said, the anthropologist is both a participant observer and a relativiser. These methodological attitudes have been forced upon him, so to speak, by the cultural phenomena with which he is concerned. In the same way the anthropologist has been forced, through his study of so-called primitive societies, to develop certain concepts and categories and foci. For example, the anthropologist

recognises the central importance of myths in human social life. A myth is a story that, so to speak, a society tells itself in order to justify or legitimate itself. It is not meant to be literally true; its function is rather to express the image that the society has of itself and to provide a framework of meaning. Again, the anthropologist is typically concerned with the use of symbols in a society, the ways in which the people of a society use objects or institutions or relationships in order to signify certain fundamental values and, as it were, to send messages. For the anthropologist nothing in a culture is ever to be taken at its face value: everything, including the most (apparently) trivial things, are charged with symbolic meaning, very much as for Freud the apparently trivial incidents of everyday life - forgetting names, word slips etc. - symbolise deep and important structures in the 'unconscious' mind.(3) The anthropologist is also vitally concerned with ritual behaviour, especially with what have been called the 'rites of passage', in other words, the rituals which surround the tribal member's passage into the world (birth), his passage into adulthood (initiation), his passage into marriage and the family and finally his passage into death. Every society has rituals to mark these crucial points in life.

Myths, symbols and rites are of course usually bound up with religious belief systems and institutions and the study of those

3. G.Dening, Islands and Beaches, p.44: ...'the essential quality of all cultural objects is that they are significant. They have meaning. All cultural things are signs and symbols of something else. Being cultured means being able to read the signs, not for the universal single meaning they have but for the meaning upon meaning that is piled up by context and condition'.

belief systems and institutions is a central part of anthropology. It is notoriously difficult to define what religion is and what function it has in a society or culture. Nevertheless we can say at least that religion adds a 'sacred' dimension to life; in other words it adds a special sanction or reinforcement to certain moral and social values, and it also discloses the possibility of another order or dimension of existence over and above the everyday world and ordinary life. As it has been put: 'there is a sense in which religion, with its institutions, lies at the centre of any culture. This is not to give it any primacy or to grade other realities in some causal relationship. It is only to say that religion in its rituals, its cosmologies, its rules of behaviour, its roles, reflects the ways men made their food, their wealth, their power over one another. Religion reflects those ways and legitimates them. It gives them permanence by providing a description of their origins: it gives them strength by investing them with nature out of the ordinary: it gives them meaning by explaining their purposes'.(4) The anthropologist must also take account of the religious professionals - priests, shamans, elders, gurus, charismatics - whose business it is to manage communication with the world of the sacred.

To sum up then: the anthropological approach is characterised by the method of participant observation, by the adoption of a relativising perspective, and by a preoccupation with certain concepts and categories such as myth, ritual and symbolic behaviour, and with religious belief-systems and institutions. Although, as I have remarked, this approach was devised in order to study pre-literate, pre-scientific,

4. G. Dening, loc. cit. p.169

technologically simple, non-European cultures, there is no reason why it should not be used to study other complex and technologically advanced cultures and sub-cultures. Indeed, as has been said, it can be very illuminating to look at certain of the sub-cultures in our society by using the method of the participant observer, by adopting a relativising perspective, by being attentive to the sustaining myths in those sub-cultures, and to the symbols and rituals (including the 'rites of passage') used in them, and the, as it were, religious belief-systems and institutions operative within them. We can, in fact, go further than this and say that an anthropology of science is necessary if science is to become fully aware of itself and fully understand its human meaning.

When one reflects upon the matter, it is very strange that the sub-culture of science, the life-world of the scientist, has not been studied in this way. Science is pre-eminently a social process in that it is an activity of human beings interacting with one another in a community and using their own language and symbols. In fact science is possible only within a community or group with its own culture. And that culture has its own distinctive social structure, its own special rules for membership, and procedures for excluding people from membership, its own symbols, its own rituals marking the birth, initiation and death of the members vis-à-vis the scientific community, its own ways of ensuring the 'purity' of the community from 'pollution', its own 'religious' structures for adding a 'sacred' dimension to the life of the community, and its own religious professionals - priests, shamans, elders, gurus and charismatics. One would think then that the need for a social understanding of science would be obvious. And yet, of course, very little has been done in this area. As a recent

scholar has put it: 'Since the turn of the century, scores of men and women have penetrated deep forests, lived in hostile climates, and weathered hostility, boredom and disease in order to gather the remnants of so-called primitive societies. By contrast to the frequency of these anthropological excursions, relatively few attempts have been made to penetrate the intimacy of life among tribes which are much nearer at hand. This is perhaps surprising in view of the reception and importance attached to their product in modern civilised societies: we refer, of course, to tribes of scientists and to their production of science. Whereas we now have fairly detailed knowledge of the myths and circumcision rituals of exotic tribes, we remain relatively ignorant of the details of equivalent activity among tribes of scientists whose work is commonly heralded as having startling or, at least, extremely significant effects on our civilisation.'(4)

There has, of course, been a great deal done on the social history of science and, latterly, on the sociology of knowledge and the sociology of science - the social determinants of scientific knowledge at particular historical epochs. One thinks here of the great work of Robert Merton, Social Theory and Social Structure (1968), and of people like Karl Mannheim (Ideology and Utopia, 1936) and, more tendentiously, of Marxist theorists such as J. D. Bernal. But there has been a curious reluctance on the part of the sociologists of science to give science, so to speak, the full sociological treatment. (5)

4. Bruno Latour and Steve Woolgar, Laboratory Life: The Social Construction of Scientific Facts, Beverley Hills, 1979, p.17

5. The work of B.Barnes, Scientific Knowledge and Sociological Theories, London 1974, was one of the first systematic attempts to provide a sociology of science. Barnes says of his study: 'As a sociological study it is unusual in that the form and content of scientific knowledge is the main concern and not its organisation or distribution'. For a more radical, and controversial, position see D. Bloor, Knowledge and Social Imagery, London, 1976. See also M. Mulkay, Science and the Sociology of Knowledge, London, 1979, for a general survey of the question.

One may as a sociologist relativise religion and adopt a reductionist attitude to it, showing that religious beliefs and attitudes can be reduced to social or socio-economic terms, but there is a resistance to adopting the same relativising and reductionist attitude to science. This is particularly striking in the case of Marxist theorists such as Bernal. One would think that for the Marxists the development of science could be explained in socio-economic terms. If science is part of what Marx calls the 'superstructure' - along with philosophy, law, art, political forms, religion etc. - then its development could presumably be seen, like that of liberal democracy, as an expression of a bourgeois capitalist socio-economic order, and the members of the scientific community could be seen as a sub-group of those 'bourgeois ideologists' whom Marx refers to in the Manifesto. However, most Marxist theorists do not treat science as they treat the other elements of the superstructure. Philosophy and religion and art are seen as 'ideological' expressions of the bourgeois capitalist order serving to support and reinforce the interests of the dominant bourgeois capitalist class. However, science apparently escapes this kind of socio-economic determination. It may be used for bourgeois capitalist purposes, but it seems to be assumed that in itself it enshrines eternal or trans-historical values and so escapes being categorised as 'ideology'. (6)

6. Marx himself did not develop a coherent account of natural science. On the one hand, he saw the natural sciences as the creation of and the ally of capitalism. Thus, he says in the Grundrisse (ed.D. McLellan, Penguin, 1973,p40) that capitalism promotes 'the development of the natural sciences to their highest point'. Capitalism is unthinkable without the technology that derives from science and science is unthinkable outside capitalist socio-economic structures. On the other hand, Marx stops short of saying that the content and method of natural science is socio-economically determined and he seems to hold that while science may be used for ideological ends it is in itself non-ideological. For differing interpretations of Marx's view of natural science see M. Mulkay, Science and the Sociology of Knowledge, London 1979, pp.5-10: J. Habermas, Knowledge and Human Interests, London, 1972; M. Markovic, 'Science and Ideology' in The Contemporary Marx, Nottingham, 1974, pp.42-78; A. Sohn-Rethel, Intellectual and Manual Labour. A Critique of Epistemology, London, 1978. Sohn-Rethel argues that modern science is an 'abstract' form of knowledge (like ideological thinking) brought about by the division of mental from manual labour.

In any case, despite the relatively recent interest in the sociology of science it is true to say that very little attention has been given to the internal structure of the scientific community and the scientific culture as such. It is one thing to show, as Merton and others have done, how scientific ideas are connected with social and economic factors operative in the wider society of which the scientist is a part. But it is another thing to look at the scientific community as a society of its own, a 'tribe' with its own distinctive culture. As it has been put: 'Although our knowledge of the external effects and reception of science has increased, our understanding of the complex activities which constitute the internal workings of scientific activity remains undeveloped'(7)

Of course, mythical or idealised accounts of the scientific community abound. Thus, for example, the celebrated philosopher of science Karl Popper presents us with a completely utopian picture of the scientific community based upon his theory of science. For Popper the crucial and definitive mark of science is that it proceeds by the method of 'conjecture and refutation'. The scientist formulates hypotheses and then attempts to refute or falsify them. A scientific proposition is by definition a proposition that is open to falsification. A consequence of this is that there are no final truths in science. The best that the scientist can ever say is that a given scientific conjecture has so far resisted falsification, and must remain open to the possibility of refutation or falsification in the future. The scientific community is then, for Popper, characterised by an anti-dogmatic attitude and a critical, open and tolerant spirit which refuses final truths and absolutes and keeps itself open to correction when new data come in. Popper sees the scientific community in fact as a model for

7. Latour and Woolgar, op.cit. p.17

what he calls the 'open society' i.e. a democratic, anti-totalitarian, form of society. A rational and truly human political order will have the same anti-dogmatism and openess, the same critical spirit and tolerance as the scientific community. (8)

Popper's account of the scientific community is completely a priori and is not in any sense based upon an examination of the scientific culture in an historical and anthropological perspective. In a sense it might be said to be a 'mythical' view of the scientific community in that it represents an ideal image which legitimates scientific activity in the face of the messy reality of science as it is actually practised. It provides a story, so to speak, for scientists to tell themselves to keep their courage up in moments of doubt. Thus it purports to provide a clear-cut distinction between authentic science and the various forms of non-science (religion, metaphysics) and pseudo-sciences (Marxism, Freudianism etc.). And, further, it associates scientific activity with acceptable ethical values - tolerance, anti-dogmatism, openess.

Though it is allegedly based upon the history of scientific practice, the picture of the scientific community in the work of Thomas Kuhn, the noted American philosopher of science, is similarly ideal and a priori In his celebrated study The Structure of Scientific Revolution (9), Kuhn argues that the development of science has not taken place in a linear and evolutionary way, as though what Newton meant by 'science' is continuous with what twentieth century quantum physicists mean by 'science'. In fact, Kuhn

8. K. R. Popper, Conjectures and Refutations, London, 1963; Objective Knowledge, Oxford, 1972; The Open Society and its Enemies, vol. 2., London, 1966.

9. T. Kuhn, The Structure of Scientific Revolution, Cambridge, Mass. 1962

says, the model or paradigm of science which Newtown took for granted is a very different one from that of Planck. The development of science is fact has occurred by a discontinuous series of 'revolutions' involving 'perspectival shifts'. But, for our purposes, what is important is Kuhn's account is that the 'paradigms' of science at any one time are created or set up by the scientific community. It is this community which in any given scientific epoch says what is 'normal science' and what is 'fringe science'. As it has been put: 'What is striking in Kuhn's account is that the view of the world which the paradigm embodies is enforced with a positively totalitarian severity. A man who does not practise science in the approved manner will simply not count as a scientist at all. In a graphic phrase, he will be "read out" of the profession. And in the twentieth century this is a heavily sanctioned matter for it means that he will get no grants, have no research workers to help him and find it impossible to get his ideas published. As in Orwell's 1984 he will become for scientific purposes an "unperson". (10)

Kuhn's account of scientific revolutions can be criticised on a number of grounds; for example, as an historical account of what has actually happened in the history of science, and as a philosophical account of the nature of the scientific process. However, despite this, Kuhn's account represents a considerable progress on previous accounts in that it shows how much science depends upon the scientific community and to what an extent it is socially constructed. Again, it introduces a relativising note in that it emphasises that the way in which 'normal science' is defined at any one time is the result of a 'cultural choice' similar to the 'choice' which generates a particular language such as English. It remains true nevertheless that Kuhn is not really interested in the internal structure of the scientific community, nor in the way in which science is shaped by social forces

10. A. Ryan, '"Normal" Science and Political Ideology', in A. Ryan (ed) The Philosophy of the Social Sciences, Oxford, p.90

and processes in the wider society of which it is a part. He is not concerned,in other words, with the understanding of science as a social phenomenon.

It was suggested before that the principal reason why science has not been subjected to anthropological investigation is that it has been thought to have a special and privileged status. In other words, science is seen not as just one way out of a large range of possible ways of relating to the world but as the way. As we saw, even the Marxists exempt science from the 'historical materialist' explanations they give of other elements of the 'superstructure'. We may have 'bourgeois morality', and 'bourgeois philosophy' and bourgeois religion', but not 'bourgeois science'. Again, part of the myth of science is that it is characterised by objectivity and impersonality, a rigorous exclusion of the subjective and the personal, so that, like mathematics and logic, science is thought to have no real history. It may be of some interest to know how scientists such as the discoverers of the DNA structure, Crick and Watson, for example, felt about their work and what their personal intentions and motives might have been. Similarly, it may be of some curious interest to study how groups of scientists actually behave in the flesh. But all of this, so it is argued, is strictly irrelevant to a study of science since the impersonal and objective and universal findings of science are there to be assessed as true or false regardless of the personal and subjective circumstances of scientists. In science it is the actual objective results that count, so that we can discard the whole personal and social side of science as irrelevant. Thus, regardless of the rather dubious personal motives of Crick and Watson - so unlike the ideal motives of a Popperian scientist - their discovery of the structure of DNA is an objective result and it is that which counts. All the rest is so much gossip.

It has already been remarked that every primitive tribe imagines that it has a god-given and necessary place in the scheme of things. In the same way the scientific community imagines that it is in some way privileged and, like Berger's Indians, at the centre of the universe. Other communities and cultures may be relativised and shown to be contingent and transient creations, but not the scientific community and the scientific culture; it has been founded by the gods![11] If, however, we take up an anthropological stance towards science we cannot accept this point of view; rather in de Saussure's terms, science must be seen as simply one 'arbitrary' language or way of speaking the world.[12]

In fact, when one looks at its historical beginnings in the Renaissance, science was then viewed as simply one way of getting to grips with the world. What we now know as 'normal science' was not sharply distinguished from alchemy and magic and other fringe forms of scientific knowledge. Copernicus was,for example, deeply influenced by the mystical ideas of the ancient Pythagorean Philolaus, and Newton

11. Cf. Latour and Woolgar, Op. cit p.20: 'Whereas other tribes believe in gods and complicated mythologies, the members of this tribe insist that their activity is in no way to be associated with beliefs, a culture or a mythology. Instead, they claim to be concerned only with 'hard facts'. The observer is puzzled precisely because his informants insist that everything is straightforward. Moreover, they argue that if he were a scientist himself, he would understand this.'

12. Or, to use yet another analogy, science provides us with a 'conceptual map See on this Malcolm Crick, Explorations in Language and Meaning: Towards a Semantic Anthropology, London 1976. See especially pp. 137-143.

himself indiscriminately mixed scientific and quasi-theological speculation without any embarrassment. By the end of the nineteenth century magic was seen as the ally of religion by scholars such as Tylor and Frazer, but at the beginnings of science in the sixteenth century magic was far more the ally of science. In fact the sharp distinction that we make between 'normal science' and 'fringe science' or 'pseudo science' was a relatively late development which one might see as due to the operation of certain ritual mechanisms within the scientific culture. Thus the English anthropologist Mary Douglas has shown in her book Purity and Danger [13] that every society has ritual mechanisms to ensure its purity, and it would be very interesting to study the development of the idea of 'pure science' from this point of view, as well as the emergence of the claim that science occupies a special and privileged position in the scheme of human knowledge.

We are all so much in the grip of the myth of science that we find it very difficult to take seriously the idea that science is simply one contingent way of coming to grips with the world and that it cannot claim to be the one and only real way, any more than we can say that one particular language, say English, is the privileged language. Or, put in another way, we are such committed participants in the culture of science that we find it very difficult to be detached observers. In order to loosen the grip of that myth let me cite part of a statement (admittedly extreme) by a contemporary philosopher of science, Paul Feyerabend.

13. M. Douglas, Purity and Danger: An Analysis of Concepts of Pollution and Taboo, London, 1966

Referring to the argument that science deserves a special position because it has produced results, Feyerabend has this to say: 'This is an argument only if it can be taken for granted that nothing else has ever produced results. Now it may be admitted that almost everyone who discussed the matter makes such an assumption. It may also be admitted that it is not easy to show that the assumption is false. Forms of life different from science have either disappeared or have degenerated to an extent that makes a fair comparison impossible. Still, the situation is not as hopeless as it was only a decade ago. We have become acquainted with methods of medical diagnosis and therapy which are effective (and perhaps even more effective than the corresponding parts of Western medicine) and which are yet based on an ideology that is radically different from the ideology of Western science. We have learned that there are phenomena such as telepathy and telekinesis which are obliterated by a scientific approach and which could be used to do research in an entirely novel way (earlier thinkers such as Agrippa of Nettesheim, John Dee, and even Bacon were aware of these phenomena)... The fact that science has results counts in its favour only if these results were achieved by science alone, and without any outside help. A look at history shows that science hardly ever gets its results in this way. When Copernicus introduced a new view of the universe, he did not consult scientific predecessors, he consulted a crazy Pythogorean such as Philolaos. He adopted his ideas and he maintained them in the face of all sound rules of scientific method. Mechanics and optics owe a lot to artisans, medicine to midwives and witches..... Wherever we look we see that great scientific advances are due to outside interference which is made to prevail in the face of the most basic and most "rational" methodological rules. The lesson is plain: there does not exist a

single argument that could be used to support the exceptional role which science today plays in society. Science has done many things, but so have other ideologies. Science often proceeds systematically, but so do other ideologies (just consult the records of the many doctrinal debates that took place in the Church) and, besides, there are no overriding rules which are adhered to under any circumstances; there is no "scientific methodology" that can be used to separate science from the rest. Science is just one of the many ideologies that propel society and it should be treated as such.' (14)

Feyerabend at times appears to confuse what I would call the cultural relativity of science - the fact that the scientific culture is simply one possible and contingent cultural form of life - with a position of radical relativism, the position that science cannot claim any lasting value but is simply the ideological expression of a limited and transient set of historical circumstances. In my view, as I have been insisting, one can admit the relativity of science without falling into this kind of relativism. However, in so far as he emphasises the cultural relativity or contingency of science, Feyerabend's statement, despite its occasional extravagances, is completely in line with the anthropological approach to science that has been advocated here.

Once we have become aware of, first, the social character of science - that it takes place within, indeed depends upon, a specific community and is in fact a social product or construction - and second, the cultural contingency or relativity of science, then we can begin to look at the scientific community and culture in a properly anthropological way, examining the characteristic myths of the scientific culture and the way they function within that culture as well as their structural connexions with

14. Paul Feyerabend, in Radical Philosophy, no. 3, pp.

other myths in the wider culture of which the scientific culture is a part; analysing the central symbols of science and the rites of passage within the scientific community, as well as the mechanisms for ensuring the purity of the community against the deviance and pollution represented by fringe and pseudo science.

If one were to sketch out a program for such an anthropology of science, primary attention would have to be paid to the myths prevalent in the scientific community. Myths, as said before, are stories that societies tell themselves in order to justify and legitimate themselves: they are stories used to express the image that the society has of itself and to provide frameworks or structures of meaning within which the beliefs and activities of that something can be situated. We have already mentioned some of the myths that are operative within the scientific community. The myth of the 'purity' of science, for example, is a very central one. The idea of 'pure science' comforts the scientist in his belief that there must be a clear-cut distinction between authentic science and pseudo-science. Despite the methodological difficulties of providing a satisfactory justification for that belief the scientist nevertheless makes an act of faith that there is a sharp and definite distinction.(15) But the myth of 'pure science' also helps the scientist to escape having to attend to, and bear responsibility for,

15. Bloor, Knowledge and Social Imagery, pp. 82-3, has pointed out the part that the concept of 'purity' plays in Frege's philosophy of mathematics: 'Frege is particularly concerned to maintain a boundary between mathematics on the one hand, and on the other the psychological and even the natural sciences. He speaks of psychological methods of argument as having "penetrated even into the field of logic". The consequence of this penetration, the reader is told, is that all becomes foggy and indefinite when really order and regularity should reign. The concepts of mathematics, he avers, have a fineness of structure and a greater purity than any other science...The Foundations of Arithmetic is seen today as a classic in logic. This it is: but it is also an intensely polemical work and this aspect of it tends to be imbibed and transmitted with hardly a comment. It is steeped in the rhetoric of purit and danger, and full of the images of invasion, penetration, disparagement, contempt and the threat of ruin'.

any moral and political implications of his scientific activity. According to the myth there is a sharp distinction between the domain of science, which is morally and politically neutral or 'value free', and the domain of values. The scientific investigation of nuclear fission is one thing; what other people do with the results of that investigation is not the interest or the responsibility of the scientist as such. No matter how the results of pure science may be applied by wicked politicians and others, the scientists' hands are always clean.

A central assumption of the myth of pure science is that there is a kind of pre-established harmony between scientific progress and human happiness, so that any conflict between the two is a priori impossible. It may, perhaps, appear from time to time that there are conflicts between the advance of science and human welfare; but all such cases, so the myth reassures the scientist, will turn out to be merely apparent and not real. It is unthinkable that a scientist might be faced with a situation where a given piece of scientific research must be stopped on the grounds that it is likely to have anti-human or anti-social effects.

This assumption, that there is some kind of pre-established harmony between science and human happiness, has been expressed in a number of different ways. Thus, for example, J. Bronowski defended the scientific endeavour which resulted in the first atom bomb by arguing that it was extra-scientific values which dictated that the findings of the physicists of the Manhattan Project should be used by President Truman to kill the innocent people of Nagasaki and Hiroshima. 'Science', Bronowski says, 'has nothing to be ashamed of even in the ruins of Nagasaki. The shame is theirs who appeal to other values than the human imaginative values which science has evolved'. (16)

16. J. Bronowski, Science and Human Values.

The case of Galileo (or, rather, the standard scientists' account of the case) is, of course, the great exemplification of this view, for Galileo pursued his scientific discoveries without concerning himself about the possible theological repercussions of his scientific work. Galileo saw that scientific truth must be declared regardless of the ulterior effects it might have; and, so the received interpretation goes, it is only reactionaries like the Church inquisitors who refuse to see and accept this. Within the context of the myth of pure science the symbolic meaning of Galileo's case goes far beyond its immediate historical circumstances. Thus, it is interpreted generally to mean, first, that the scientist's search for the truth about the workings of nature is an absolute and intrinsic good which cannot be outweighed, so to speak, by other goods; second, that the scientist may, as it has been put, 'disclaim all responsibility for the application of this knowledge'; and third, that any attempt to call these two principles into question must spring from a spirit of reactionary obscurantism.

The myth of 'pure science' is linked with another myth about the 'impersonality' of science. Science, natural science, is characterised by the fact that the scientist adopts a completely impersonal attitude. His own personal subjective feelings, motives, intentions, wishes, have nothing to do with his scientific activity. In this view the ideal scientist approximates as closely as possible to the status of a pure instrument: the scientist is an instrument whose task it is to read other instruments; he is a pure observer and the whole personal and interpersonal side of his activity is left out of account as irrelevant. It may be that in the social sciences - the 'soft' sciences - personal and subjective factors play a part in the scientist's mode of theorising, in the selection of his area of research interest and in dictating the style

of his research; but in the 'hard' sciences, so it is claimed, these factors have no place. According to the myth, the natural sciences have no real history and the scientist has no biography. This impersonality is also reflected in the peculiar style and rhetoric of scientific monographs. As it has been pointed out, the actual practice of science is completely misrepresented by the mode of presentation used in the reporting of scientific activities.(17)

This attitude is emphasised very clearly in the work of the great nineteenth century physicist, Ernst Mach. For Mach every scientific statement can be translated into statements about the scientist's sensations, but these sensations are seen by Mach as being physical reactions of the same kind as those that occur in scientific instruments. They are not attached to a self or ego; as Mach put it, 'The ego must be given up'. (18) In reality, of course, as Michael Polanyi has often pointed out, personal and subjective factors play a very large part in scientific activity just because it is a human activity. But the myth of impersonality demands the suppression of the ego and prevents us from adverting to these factors.

It is no doubt because of the myth of scientific impersonality that there have been relatively few serious psychological studies of scientists. Although this is incidental to our anthropological

17. See P. Medawar, 'Is the scientific paper fraudulent? Yes, it misrepresents scientific thought', Saturday Review, Aug. 1, 1964, pp. 42-3; J. R. Ravetz, Scientific Knowledge and its Social Problems, Oxford, ; J. Gusfield, 'The Literary Rhetoric of Science', American Sociological Review, 41,(1), pp. 16-34

18. E. Mach, The Analysis of Sensations, p.34

interest here, it is worthwhile saying something in parenthesis about the psychology of the scientific character. Wilhelm Ostwald in the nineteenth century claimed that most of the great scientists could be characterised as either romantics or classicists. The scientific romantics are, according to Ostwald, the revolutionaries who bring about radical changes in science and become the founders of schools. The classicists, on the other hand, regard their work as private, personal vocation and they work slowly and painstakingly on a narrow front.(19) Ostwald's classification of scientists is obviously not based upon any real psychological research and does not tell us a great deal. Disappointingly, the same must be said of a more recent study by the American psychologist, Abraham Maslow.(20) Maslow is on the side of the angels in that he insists very strongly upon the personal and subjective aspect of science, but at the same time he implicitly accepts the idea that science has a special and priviliged place and his characterisations of the psychology of the scientist are largely idealised and a priori. Here is part of Maslow's summing up: 'About fifteen years ago I began an investigation into the motivations of characterologically different types of scientists. I asked them simply to ramble on at length in answer to my two questions "Why did you pick your line of work, your field, your problem?" and "What are the main rewards (the gratifications, the pleasure, the kicks, the peak moments of highest happiness) that you get out of your work? What keeps you at it? Why do love your work?" These two questions parallel the difference between "Why did you fall in love?" and "Why do you stay married?"

19. On Ostwald see L. Feuer, Einstein and the Generations of Science, New York, 1974, pp. 346-47.

20. A. Maslow, The Psychology of Science, New York, 1966

For various reasons, I had to give up this research after interviewing perhaps a dozen scientists in various fields. But even with these few I became impressed with the variety of covert motives that impelled scientists to their work and kept them at it. As with other human beings, their world view, their pleasures and satisfactions, their likes and dislikes, their vocational choices, and their styles of work were in part an expression of their "characters".

I was confronted again, as so many other investigators have been, with the temptation to differentiate the contrasting types that have been called by so many names, tough-minded and tender-minded, Apollonian and Dionysian, anal and oral, obsessional and hysterical, masculine and feminine, controlled and impulsive, dominating and receptive, suspicious and trusting, etc. For a time I used the designations x character and y character, defining these as the common elements in all these pairs of antonyms. At other times I used the words "cool" and "warm" because neither of these is invidious or insulting, and I thought also that the "physiognomic quality" of these words was better than more explicitly defined words in the present state of knowledge. For the same reasons I have also tried the "blue-green" (end of the spectrum)and contrasted it with "red-orange-yellow" people. Finally I put the problem aside, even though the feeling of being on the edge of some vast illumination even yet lingers. The trouble is that it has remained in this same teasing position for fifteen years, without my getting any closer to illumination.

One impression, tentative at the time, has become more convincing over the years, and I offer it here for more careful testing. Those individuals that I thought of as "cool" or "blue-green" or tough-minded" in character and outlook tended, it seemed to me, to have as the goals of their scientific work the establishment of law, of regularities,

of certainty, of exactness. They spoke of 'explanation', and by this they clearly implied the tendency toward parsimony, and economy, the simple, the monistic. The moment of reductiveness, i.e. of a reduction in the number of variables, was a moment of triumph and of high achievement. By contrast I felt that the "warm" people, the red-orange-yellow, the intuitive ones (who came closer to the poet-artist-musician than to the engineer-technologist), the "tender-minded" and "soft-nosed" scientists tended to speak glowingly of the moment of "understanding" as the high spot and the reward of investigation!' [21]

Clearly, Maslow's "impressions" are little more than intuitive guesses and once again have little to tell us. The wonder is that, as a psychologist, he does not feel the need to examine the character of this scientific mentality (or mentalities) in the same close and detailed way as, say, the religious mentality has been examined.

A more ambitious attempt to provide some kind of psychoanalytical insight into the nature of the scientific psyche is that of Lewis S. Feuer in his curious book Einstein and the Generations of Science. It is, of course, notoriously difficult to control these quasi-Freudian analyses and notoriously easy to fall into nonsense and Feuer does not always avoid these pitfalls. This is, for example, what he has to say about a dream reported by Ernest Mach. 'While dreaming', Mach relates, I saw in my laboratory a beaker filled with water, in which a candle was

21. Ibid. p.
On the motivations behind scientists' choices of research fields see Latour and Woolgar, op.cit. Ch. 5., "Cycles of Credit"; W.O. Hagstrom, The Scientific Community, New York 1965; P. Bouvier, "The specificity of the scientific field and the social conditions of the progress of reason", Social Science Information, 14, (6) pp. 19-47.

serenely burning. "Where does it get its oxygen from?" I thought. "It is absorbed in the water", was the answer, "Where do the gases produced in the combustion go to'. The bubbles from the flame mounted upwards in the water, and I was satisfied'. 'This dream', Feuer comments, 'plays boldly with the theme of a reversal of the laws of physics, and in a curious way is egalitarian in its motivation; it aims to eliminate privileged male status. We may venture to explicate its familiar symbols. In the beaker filled with water (the female container), a candle (the erect male organ), is "serenely burning". Whence, however, asks Mach, does it get the oxygen it needs for burning? At this point, Mach's egalitarian feelong asserts itself; the oxygen is the woman's fluids themselves. But then what happens with the products of the combustion, the intercourse? The "bubbles" (the semen), mount upward in the woman. Then, writes Mach, in the language of a lover, "I was satisfied". Thus the import of the dream is is making the woman man's equal, his equivalent, and a valid source of oxygen. The dream seems an eloquent testimony that in sexuality Ernst Mach achieved a liberation from fears and excessive discipline; the woman, enflaming him, giving him the oxygen to keep him burning, disenthralls him from the father-fear that menaced his potency'.[22]

As we saw before, a central preoccupation of the anthropologist is with the various forms of ritual behaviour including the 'rites of passage'. So called primitive societies are heavily ritualised, but our own society is also full of rituals if only we look for them.

22. L. S. Feuer, Einstein and the Generations of Science, New York, 1974. p.

It would not, I think, be too difficult to find analogues to the rites of passage within the scientific community - initiation rituals such as the Ph.D. governing entry into the community, the passage into adulthood within the community, the ritual exclusion and outlawing of members who offend against the taboos (as the French philosopher, Michel Foucault, puts it, a society or culture defines itself in terms of those whom it excludes). There are again, as we have noted, rituals connected with maintaining the purity of the scientific community against contamination from pseudo-science and also from moral and political involvement. Finally, there are rituals governing the relations bewteen the generations within the scientific community. Feuer's book Einstein and the Generations of Science, referred to before, attempts to give an account of nineteenth and twentieth century physics in terms of generational conflict, that is in terms of conflict between the young and their fathers and elders, conflict between innovators and the tradition-bound establishment. Feuer also claims that the scientific community has devised means for containing and neutralising this conflict: 'Only inthe sciences has human society devised a means for resolving the conflict of generations. The scientific community has undergone basic reconstructions of ideas without suffering the equivalent of social revolutions. Fundamental theoretical changes have been made in a rational, constitutional spirit; a common loyalty to scientific truth had overridden divisive generational, national, political and religious forces.'[23] Instead of this rather idealised account, an anthropologist would say that the scientific community has very powerful ritual mechanisms for managing generational conflict. Some Christian churches (the Roman Catholic Church in particular) for whom continuity with their

23. op cit. p. v

origins and sources are of prime importance, have similar mechanisms. Regardless of the profound changes that take place at the doctrinal and institutional level, they claim that these changes are 'developments' (reconstructions) and not true revolutions.

Apart from the central myths and rituals in the scientific community, there are obvious analogies with the religious institutions of primitive societies. It is not difficult to discern the priests and shamans, the elders and the gurus within the scientific tribe. At another level the anthropologist of science will be concerned to study the power structure within the scientific community. Here quite concrete issues arise: who controls the research funds (the means of grace, spiritual power) and how does one get access to them? How does one get a paper published in a scientific journal and so display one's manhood? How does one get a reputation within the scientific community? How does one become a culture hero and win a Nobel prize? These are all questions an anthropologist would be interested in if he were investigating a primitive society. From another point of view, one could also look at the political aspects of science, the ways in which certain styles of scientific research are dictated by surreptitious political considerations. Here, of course, there has been a great deal of work done ranging from the neo-Marxist studies of Stephen and Hilary Rose to the very interesting analyses by a group of German science policy experts at the Max Planck Institute.[24]

24. See, for example, W. Van Den Daele, W. Krohn, P. Weingart, 'The Political Direction of Scientific Development' in The Social Production of Scientific Knowledge, Dordrecht/Boston, 1977. See also K. D. Knorr, H. Strasser, H. G. Zilian (eds). Determinants and Controls of Scientific Development, Boston, 1975.

What is striking about this latter group, and their American and English colleagues who are associated with the new Sociology of the Sciences Yearbook, is that they are all aware of the relevance of anthropology to the study of the scientific community.

So far we have been speaking about the anthropology of science in general, but in a sense there no such thing as science in general; rather, there is a loosely connected cluster of disciplines which have merely a family resemblance to each other. The large and deep differences between physics and chemistry on the one hand and the biological sciences, on the other hand are well known. But even within the biological sciences, for example, there are very radical differences between the different disciplines. Again, new disciplines can emerge and come to have their own distinctive methodologies and styles and 'mythologies'. Thus the relatively new discipline of neuroendocrinology - the result of a hybridisation in the 1940s between neurology (the science of the nervous system) and endocrinology (the science of the hormonal system) - has now its own 'culture'.[25]

25. See Latour and Woolgar, op.cit. p.54:

> 'Neuroendocrinology seemed to have all the attributes of a mythology: it had had its precursors, its mythical founders, and its revolutions. In its simplest version, the mythology goes as follows:
>
> After World War II it was realised that nerve cells could also secrete hormones and that there is no nerve connection between brain and pituitary to bridge the gap between the central nervous system and the hormonal system. A competing perspective, designated the'hormonal feedback model' was soundly defeated after a long struggle by participants who are now regarded as veterans. As in many mythological versions of the scientific past the struggle is now formulated in terms of a fight between abstract entities such as models and ideas.'
>
> On the emergence of other new disciplines - agricultural chemistry, tropical medicine, X-ray protein crystallography etc., see G. Lemaine, R. Macleod, M. Mulkay, P. Weingart, Perspectives on the Emergence of Scientific Disciplines, The Hague/Paris, 1976 .

A systematic anthropology of science would therefore have to go beyond macro-analyses of the general scientific community and investigate quite specific scientific sub-groups. A French scholar, Bruno Latour, has in fact recently conducted such a micro-investigation into the life-world of a group of scientists at the Salk Laboratory in the U.S. - a laboratory concerned with fundamental research into neuroendocrinology. After two years anthropological fieldwork as a participant observer in the Salk Laboratory, Latour was able to cast a good deal of light on what he calls 'the social construction of scientific facts'.[26]

What is needed then are a number of authentically anthropological studies of this kind both in the physical sciences and the life sciences, and then, within those broad categories, in established 'mainstream' disciplines, new emergent disciplines, and disciplines in a state of crisis and breakdown. We would then be able to build up gradually a corpus of anthropological knowledge about the scientific tribes comparable to that which we already possess about the African tribal peoples, for example, or the Australian Aboriginal peoples.

This approach to the scientific life-world, or life-worlds, is not inspired by any kind of debunking, anti-science, intention. Rather,it is concerned to remind us of the truism, which we constantly forget, that science is a human and social phenomenon and that it is only by becoming aware of this that scientists themselves will become fully self-aware.

26. B. Latour and S. Woolgar, Laboratory Life; see especially ch. 2 'An Anthropologist visits the Laboratory'.