

HARD AND SOFT SCIENCE

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It is part and parcel of what the 'myth of science' that the so-called natural or physical sciences are viewed as being in some way central or paradigmatic. They are taken to be the models or 'beau ideals' of what science is, and it is by reference to them that we judge whether or not a given body of knowledge is 'science'.

The natural sciences, according to the myth, deal with 'hard' data or facts - 'hard' meaning testable in a straightforward way and quantifiable and expressible in precise mathematical form. Again, in the physical sciences we are able to formulate general laws and theories of a strict and necessary kind - these laws and theories expressing invariant causal relationships between different parts of nature. A consequence of this is that they allow us to make predictions with a high degree of certainty. A further consequence is that they are 'value free'; they describe how nature is and they are not concerned about whether what they describe is good or bad, desirable or undesirable. (A lady once remarked to Thomas Carlyle that she 'accepted' the universe. 'By God, Ma'am', said Carlyle, 'you'd better'! And that's very much the attitude of the natural sciences: for him there is just no question about whether the universe is acceptable or unacceptable - it just is as it is.) Finally, these sciences claim to be purely 'objective' in that the natural scientist

deliberately adopts the role of the dispassionate, impersonal observer. He rigorously excludes his own subjective feelings and emotions and desires and expectations, and tries to make himself as much like a neutral scientific instrument as possible. The 'facts' the natural scientist deals with are *brute* 'brute facts' i.e., facts without any admixture of 'interpretation'. It is part of this whole idea of science that we can make a sharp and rigorous distinction between the 'facts' and our 'interpretations' (which introduce a 'subjective' element) of the facts.

These then are the 'hard' sciences - 'hard' in that they deal with 'brute' and precisely quantifiable facts, formulate strict laws and theories, make precise predictions, are 'value-free', and are rigorously objective in their methods - and the relative 'hardness' or 'softness' of other bodies of knowledge is gauged by reference to them.

The idea that the methods of the natural sciences can be applied to human behaviour, and especially to man's social behaviour, is of course an old and venerable one. Thomas Hobbes in the seventeenth century, for example, was one of the first to dream of the possibility of a science of society where man's social relationships would be seen to be based upon the laws of mechanics. And this science of society would be just as 'hard' and rigorous as the natural sciences are.

The dream of a natural science of human behaviour has had a long and chequered history, through Condorcet in the

eighteenth century, for example, and Auguste Comte in the nineteenth century. There is, again, a strong dose of it in Marx's theory of society, and Marx at times certainly thought of himself as a kind of social physicist. Freud also saw himself as a natural scientist of the human psyche and he continually spoke as though the functioning of the psyche could be explained in quasi-mechanical terms - that is to say, in terms of pushes and pulls, pressures and so on.

Certain wishes and desires are excluded from our conscious mind into what Freud calls the 'unconscious'; but they emerge in other forms - dreams, aberrant mental behaviour, neurotic symptoms - very much in the same way as when we put certain liquids under pressure they emerge in other forms, as gases etc.

Basically, Freud believed, everything about man could be explained in terms of his primitive biological urges and drives, and they in turn could ultimately be explained in terms of physics and chemistry.

At first sight, however, human behaviour does not seem to lend itself easily to investigation by the natural sciences. The facts or data of man's psychological and social behaviour seem, for example, to be very 'soft'. For one thing they are extremely difficult to quantify and measure. Again, any generalisations we may make in the field of human behaviour are very loose and open-ended and admit of all kinds of exceptions, so that our predictions about human behaviour are likewise loose and fallible. Further, a good deal of human behaviour is purposive or teleological or goal-directed, and, prima facie at least, we

speak of motives and reasons for acting rather than of causes of action. We seem not to be talking about invariant causal connexions between sets of natural phenomena, but about a quite different kind of connexion between ends and means. And finally, in this realm of human behaviour where we are talking about motives and intentions and purposes, it seems wholly artificial to attempt to exclude value-judgements/and the 'subjective'. (For example, to describe a certain kind of human behaviour as 'neurotic' is also to evaluate it. Neurotic behaviour is obviously bad or undesirable behaviour.) For the same reason also, the dispassionate, impersonal stance of the natural scientist seems out of place here. What is needed, rather, is a kind of empathetic understanding.

Confronted with this prima facie difference between the 'hard' facts of natural science and the 'soft' facts of human behaviour, three distinct attitudes have been taken up. First, it has been held that the difference is merely an apparent one and that in reality it is possible to explain human behaviour in exactly the same way as we explain natural phenomena. Thus, for example, the psychologist D. O. Hebb in his classical work, The Organisation of Behaviour, defines psychology as 'the task of understanding behaviour and reducing the ~~bagaries~~ bagaries of human thought to a mechanical process of cause and effect', and he says that the ideal of the psychologist is to understand 'the principles of behaviour to the degree that we understand the principles of a chemical reaction'. If it is not possible to reduce the so-called social sciences to the natural sciences, the 'soft' sciences to the 'hard', immediately (as some of the earlier social scientists had optimistically hoped), the reduction

of the one to the other is at least possible in principle. It represents a kind of ideal limit to which psychology, and sociology, and history and the other 'sciences of man' should tend.

As one writer in psychology has put it: 'The prevailing uncertainty of psychological explanation need imply no fault more severe than scientific immaturity. Indeed, the contemporary situation in psychology is strikingly parallel to that of physiology in the sixteenth century. The notable development of modern experimental physiology leads one confidently to expect that a coherent science of mind will slowly take shape in the general framework of the sciences of life'. (O. L. Zangwill) The general idea here is that psychology, and all the social sciences, are young and immature disciplines and that, as they gradually adopt the methods of the natural sciences, they will become progressively more respectable.

The second position on the relationship between the 'hard' and the 'soft' sciences is a sceptical one. Thus, it is pointed out that, despite the desperate attempts of one hundred years of psychology and sociology to pretend otherwise, human behaviour is irredeemably 'soft' and will never be able to be made 'hard', never be amenable to the methods of the natural sciences. With human behaviour we are dealing with the unquantifiable and unmeasurable, with the unpredictable and the 'subjective', so that the basic conditions for science are missing. Again, as one writer has put it: 'In view of the complexity of its data and the difficulties confronting crucial experiments in the psychological sphere, the explanations offered by psychology

remain at the descriptive level. Hypotheses intending to co-ordinate large bodies of fact, such as the psycho-analytic or the Gestalt theory, fall short of the necessary requirements for truly scientific precision.'

In this view the natural sciences are still taken as the paradigm or ideal of science but it is regretfully conceded that the so-called social sciences have not measured, and will never be able to measure, up to that ideal. They are, as I have said, irredeemably 'soft' - or as one male chauvinist philosopher of science has put it: if physics and chemistry are 'boy' sciences, then psychology and sociology and history are essentially 'girl' sciences!

The third attitude a propos the relationship between the natural sciences and the social sciences challenges the 'myth of science' which gives a central status to the natural sciences, and questions the whole assumption that psychology and sociology can only be made respectable if they become something like physics or chemistry. Why, it is asked, should we suppose that the social sciences must be reducible in some way to the ¹physic sciences; and why shouldn't we admit the possibility that the social sciences constitute a distinct and autonomous realm of their own?

The great German sociologist, Max Weber, was one of the first to suggest (though very ambiguously) that the methods and aims of the social sciences were intrinsically different from those of the natural sciences, and further, that this did not represent any kind of disgrace. As Weber pointed out,

what we typically do in psychology and sociology and history is not to explain the causal antecedents of some phenomenon; rather, we try to understand how the people involved in a given situation see it. Thus, as it has been put: 'When a starving populace fails to revolt against its government, we do not at once wonder whether people enjoy starving or are grateful to those who give them the chance to starve. What we wonder is whether they believe their hunger is the government's fault, whether they think there is any alternative to the present regime, and so on That is, the question is not whether the social scientist, conceived of as a quasi-experimental scientist, has correctly described the causal antecedents of a phenomenon, but whether he has understood how the person involved would have described their situation We have to know what the agent thinks the situation is before it can begin to possess a logic for him, and therefore for us. And discovering this seems both to permit and to demand methods for which there is no place in the natural sciences.'

(A. Ryan, introduction to The Philosophy of Social Explanation)

According to this view, what is essential in the social sciences is a kind of empathetic understanding - Verstehen, as Weber called it - which enables us to put ourselves in the boots of people in other situations, other cultures, other periods of history, and which enables us to understand or decode or interpret their behaviour.

Weber's concept of Verstehen and his view of social science was, unfortunately, not fully worked out and there remains a good deal of ambiguity about it. However, much the same idea has been developed from another angle by certain neo-Freudian

thinkers. I remarked before that Freud viewed himself as a scientist of the human psyche and that his theory of the psyche was a quasi-mechanical one. However, a number of contemporary commentators on Freud have argued that if we look at Freud's practice, and leave aside his own misleading account of what he was doing, he was in fact not going on like a natural scientist at all. What Freud did in practice was to understand and interpret the behaviour of his patients very much as we understand and interpret a language. (It is no accident, according to these commentators, that Freud's most famous, and fundamental, book was called The Interpretation of Dreams.)

The American therapist Thomas Szasz was one of the first to argue that Freud's theory ought not to be seen as a scientific one analogous to the theories of physics and chemistry. So in his book The Myth of Mental Illness, Szasz says that 'So-called mental illnesses are like languages'. And the problem of hysteria, for example, 'resembles the problem of a person speaking a foreign language rather than that of a person having a bodily disease'. 'It follows then', Szasz concludes, 'that if hysteria is regarded as a special form of communicative behaviour it is meaningless to enquire into its causes. As with languages, we shall only be able to ask how hysteria was learned and what it means. This is exactly what Freud did with dreams. He regarded the dream as a language and proceeded to elucidate its structure and meanings'.

In very much the same way, the English analyst Charles Rycroft claims that Freud's technique was not to elucidate the causes of neurotic behaviour but rather to make sense of it.

'It can indeed be argued that much of Freud's work was really semantic, and that he made a revolutionary discovery in semantics, viz. that neurotic symptoms are meaningful disguised communications'. (Psychoanalysis Observed) The same approach to Freud has been taken by the French philosopher Paul Ricoeur in his vast work On Interpreting Freud. Freud's interpretations, Ricoeur says, are akin to the interpretation of a text, and Freud's theory is to be seen a kind of 'hermeneutics' - hermeneutics being the study of the rules which govern the interpretation of texts. According to Ricoeur, we decide whether a psychoanalytic interpretation is a good or bad one in much the same way as we decide whether an interpretation of a text is a good or bad one. Those critics of psychoanalysis, like Professor Eysenck, who have objected that Freudian explanations are neither scientifically verifiable nor falsifiable, are missing Freud's whole point. One might just as well complain that, say, Dr. Leavis's interpretations of the novels of Dickens are not scientifically verifiable.

From a rather different angle, the French analyst Jacques Lacan also claims that we must interpret Freud's psychoanalytic theory in much the same way that Freud interpreted the behaviour of his own patients. We must learn to re-read Freud, taking note of what he does not say as well as of what he does say. When we do this we discover that Freud is really a 'structuralist': for him neurotic behaviour is a language and we must interpret it as we interpret a language by exposing its basic 'grammar' or 'structure'. For Lacan, the 'unconscious' works according to laws analogous to those which, according to Chomsky and others, govern language.

This same idea, that human behaviour in all its dimensions - psychological, sociological, historical - is analogous to a language which is not to be explained in causal terms but rather de-coded and interpreted, is also central to Claude Levi-Strauss's 'structuralism'. The gist of structuralism is that understanding a society or a culture is exactly like understanding a language, and that the methods we use in the one case are strictly analogous to the methods we use in the other. To try to understand, say, Chinese by the methods of the natural sciences - observing regularities in the appearance of certain ideographs and sounds etc., and attempting to establish general laws on the basis of those regularities - would be futile. We would never succeed in understanding Chinese this way. What we have to do is to grasp the grammatical and syntactical rules which determine whether a given collection of symbols or sounds is meaningful or not. And this is something quite different from explaining natural phenomena by subsuming them under some general law.

To admit that the social sciences have different methods and aims from the physical sciences seems to offend, however, against the ideal of the 'unity of science'. In its most general form, the 'unity of science' means that 'since men and their social dealings are part of the natural order, they must in principle be amenable to explanation in terms of the same sort of naturalistic principles that every aspect of nature conforms to'. (Ryan, op.cit.) In more specific terms, the 'unity of science' is sometimes taken to mean that all the sciences are 'one' in that they are ultimately reducible to the physical sciences; or sometimes to mean that they are all 'one' in that they use the same method of explanation. But why should we make these assumptions?

Why should science be 'one' any more than, say, art is 'one'. It would be fatuous to pretend to 'reduce' all the arts to some one basic form, or to claim that the procedures of painting, music, poetry, the drama, dancing, were identical.

There is, no doubt, a powerful psychological factor operating behind the 'unity of science' programme in that the human mind seems to like the simplicity and economy of unitary hypotheses. It seems neater and more aesthetic to have one single form of science, than to have to admit that there are two irreducibly distinct forms of scientific method and scientific explanation. But once again, why should we suppose that reality ought nicely to oblige our preconceptions about it?

It might, after all, be just the case that there are more things in heaven and on earth than the natural scientist dreams of. And it might be just the case that rivalry is much richer and more various and 'softer' (more girlish!) than we think.

Reality