VALUE AND THE KNOWLEDGE OF SOCIETY

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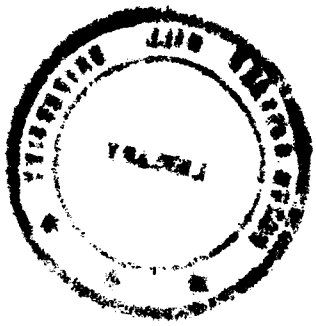

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Certified that the subject matter of this dissertation is the record of work done by Shri ZOLIANA DARKIM, that the contents of this dissertation did not form a basis of the award of any previous degree to him, or, to the best of my knowledge, to anybody else, and that the dissertation had not been submitted by him for any research degree in any other University.

In habit and character Shri ZOLIANA DARKIM is a and proper person for the degree of M.Phil.

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C O N T E N T S

CHAPTER - I

INTRODUCTION

This dissertation is primarily an attempt at showing how considerations of value might be inalienably involved in any effort at understanding human beings and human society. But it is also interesting to ask whether or not even investigation within the natural sciences might not be free from considerations of value. In the chapter immediately following the present one I attempt to show how in a minimal sense questions of value may be inescapable even for the natural scientists. The simple step on which my arrangement is based is rather the well accepted theories that no investigation in the natural phenomenon can be free from a commitment to a theoretical position. Frequently this commitment may be only later in the investigation and therefore may need to be, as if were, brought to the surface. Mere commitment to a theory, however, does not seem obviously to involve one in any evaluative stance as well. However, where value enters quite explicitly is in the matter of choice between different theoretical stances. This choice, whether wittingly or unwittingly, involves evaluative considerations of a diverse variety: moral, aesthetic, utilitarian, and so on.

In chapter III entitled "Sciences of Man and the Question of Value" I attempted to show how in the social sciences consideration of value are much more centrally involved than in the natural sciences. My argument is based on the articulation of what I consider to be the correct concept of man and human agency. I agree that it is only within an evaluative framework that it makes sense at all to talk about human action and the proper understanding of it.

In the final chapter I argue against the thesis that it is possible that the sciences of man are, in a strong sense, reducible to the sciences of nature, and I also argue against the thesis that there is a unity of method in the sciences. Although in a general way it may be quite acceptable to talk about the unity of all the sciences, there are fundamental differences between the sciences of man and the natural sciences arising primarily from the fact that the subject matter of the former is man (who is necessarily a self-conscious and self-explaining creature) while the subject matter of the latter is natural objects which are not self-conscious and self-explaining beings. This distinction between man and nature, as it were, is crucial and is logically impossible to ignore.

CHAPTER - II

THEORY AND INVESTIGATION IN THE NATURAL SCIENCES

I

Broadly speaking, philosophers of science have been chiefly occupied with the logic of science. They interpret science as a body of deductive, or quasi-deductive systems of assertions; these systems and their components are analysed and judged by using concepts and rules belonging to the study of formal inference and implication. This situation is typified by the fact that explanation is widely regarded as the primary explicandum for the philosophy of science, while the best known explications offered mould scientific explanations along deductive lines.

> ...the distinctive aim of the scientific enterprise is to provide systematic and responsibly supported explanations.[1]

The discussion of problems of explanations received its decisive impetus from a series of papers by Carl Hempel. In the first of these papers, Hempel maintains that the "scientific explanation of the occurance of an event of some specific kind E... consists of (1) a set of statements

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1. Ernest Nagel, The Structure of Science (New York,1961,p.15.

asserting the occurance of certain events C1...Cn at certain times and places, (2) a set of universal hypotheses, such that (a) the statements of both groups are reasonably well confirmed by empirical evidence, (b) from the two group of statements the sentence asserting the occurance of event E can be logically deduced;" group (1) describes the initial and boundary conditions for the occurance of E, while group (2) contains the general laws on which the explanation is based.[2]

Usually, contemporary philosophical treatments refrain from committing to an empirically testable description of the total, temporally successive stages of a scientific inquiry, concentrating instead on a conceptual analysis of the structure of scientific explanations and arguments, regardless of the detailed psychological and historical conditions and development of scientific thinking.

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2. Carl Gustay Hempel, "The Function of General Laws in History", The Journal of Philosophy, 39 (1942), p.35-48.

The neglect to offer what is meant to be a general chronicle of any scientific discovery as distinct from a logical reconstruction of scientific thought - the interest being in procedure rather than process - is a legitimate curtailment of the study of scientific inquiry. For example, it has been assumed that there are certain indispensable components of a unique scientific method: inter alia, devising hypotheses, deducing predictions from them, and empirically testing the hypotheses via the predictions, However, it is not at all clear whether there is or, can be such a scientific method; the very existence of scientific method is doubtful and is something not to be acknowledged as real datum. "The scientific method, as far as it is a method, is nothing more than doing one's damnedest with one's mind, no holds barred. What primarily distinguishes sciences from other intellectual enterprises in which the right answer has to be established is not method but the matter".[3]

Dealing logically with the nature of scientific laws again supposed that the notion of an 'observed fact' is uncontroversial and that all observed facts can be described independently of any scientific-theoretical

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3. P.W.Bridgeman, Reflections of a Physicist (New York, 1950) p.370.

interpretations; the relation of observed facts to scientific laws is treated simply in terms of such logical concepts as those of generalisation and deductive inference. Hence, the scepticism and criticism provoked.

Now, if we can show that the sciences are never essentially homogeneous at any period, that there is no unique, if there is any, scientific method, and that observed facts are dependent, in a strong logical sense, upon scientific-theoretical interpretation, the way is open for us to take the conclusion that scientific knowledge cannot be had in terms of a neutral language, conversely, that scientific knowledge would always be in terms of a scientific-theoretical interpretation such that is can never be value-neutral. And scientific-theoretical interpretation would render scientific knowledge non-value-neutral precisely because such interpretations would involve an evaluation exhibiting very fundamentally the scientists own conception about nature.

II

The reason why this static, unitary, and 'objective' (i.e. all scientific beliefs are determined solely by observed facts) has been attractive is the fact that philosophers were unduly disturbed by the threat of metaphysical incursions which would imperil the neutral-objective-language

in which scientific inquiry is supposed to be carried on.

The crucial problem in the philosophy of science, however, it may be asserted, is not the 'neutralisation' of scientific inquiry, rather it is to question the possibility or impossibility of going about neutrally or 'objectively' in scientific inquiry. According to P.K.Feyereband,

> What is not so well known is that ... some of the methods of modern empiricism which we introduced in the spirit of anti-dogmatism ...is bound to lead to the establishment of a dogmatic metaphysics... far from eliminating dogma and metaphysics..... modern empiricism had found a new way of making dogma and metaphysics respectable, viz., the way of calling them 'well-confirmed' theories, and of developing a method of confirmation in which experimental inquiry plays a large part though well controlled one.[4]

Thus, the attempts to go about in a neutral-language in scientific inquiry themselves would invariably be non-neutral; it would always lead to the adoption of what T.S.Kuhn called "ways of seeing the world"[5]. "Well-confirmed" theories, methods

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4. P.K.Feyereband, "How to be a Good Empiricist" reprinted in P.H. Nidditch ed. The Philosophy of Science,(Oxford University Press, 1968), p.13,italics added.

5. T.S.Kuhn, The Structure of Scientific Revolutions(University of Chicago Press, 1962).

of confirmation of scientific theories, what counts as "positive instances" of confirmation would all be then relative to ways of seeing the world.

Now, it is taken that any clarification of the term "science", and especially of the term 'empirical science', must say something about the relation between hypothesis and evidence; the hallmark of the scientific enterprise is the high degree of control which empirical evidence has upon hypothesis. Its theories, on this view, are confirmed by the facts; and hence their widespread, virtually compelled acceptance.

In charactering scientific laws as those generalisations that are confirmed by their instances we are however, not, interested in the analysis of such laws, rather we are interested in what constitutes a positive instance of generalisation and under what conditions the positive instances of a generalisation confirm a generalisation. We are then asking for an analysis of the relation that obtains between hypothesis and evidence.

However, in accepting scientific laws as a result of their having been confirmed by, say, laboratory obser-

vations, it is assumed implicitly or explicitly that it is a well-defined notion supported by such criteria as simplicity.[6] Let us examine this claim before we touch on what constitutes positive instances of generations.

Taken as a criterion of acceptability, simplicity is often justified in metaphysical grounds. Thus, some argue for, or simply assert, a "principle of the uniformity of nature" or a "principle of the simplicity of nature" typically on the ground that the scientific enterprise itself "presupposes" such principles. Simplicity as a criterion of acceptability is again justified on methodological grounds. Thus, scientific inquiry is taken to proceed as though induction worked, or nature were simple, and justify the procedure by the significance of the results gained.

The metaphysical efforts ask the question: the principles that are intended to justify and simplicity themselves require at least as much justification as the principle of simplicity. The methodological alternative

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6. Robert Ackermann, "Inductive Simplicity" reprinted in P.H.Nidditch ed. The Philosopny of Science(Oxford University Press, 1968).

missed the point of the question: the success of the scientific enterprise does not by itself show why certain preliminary assumptions are indispensible. Another argument on behalf of simplicity in scientific inquiry is that the scientific hypotheses are not more than summaries of recorded observations; their purpose is simply to abridge the catalogue of such observations. If scientific enterprise is such then the most economic hypothesis is to be sought. Hypotheses, however, are not merely summaries of recorded observations; they go beyond what has been observed to guide our expectations of future cases. In fact, hypotheses that describe and abridge the same set of facts can lead us to make very different predictions. In abridging a given set of facts, therefore, the question invariably arises: which one of the possible hypotheses most successfully abridge the data: which <u>is</u> the simplest hypothesis: what entitles us to say one hypothesis among the alternatives is "the simplest".

The point can be generalised in case of theories. It is sometimes suggested that the simpler of two theories is the one that has fewer basic assumptions, or fewer basic concepts, or has fewer primitive predicates. But

it is extremely difficult to count assumptions, concepts, objects, predicates - there is no simple correspondence between these and the symbols in terms of which the theory is formalised - and in any case, it is not at all clear whether the concepts, objects, and predicates in one are not necessarily those that are basic and primitive in another. To put it in a slightly different way, the claim that the fundamental laws of nature are simple first depends on the way a particular theory or constellation of theories structures nature. Nature, we might say, is not simple simpliciter.

At this point, let us see what can be said about confirmation itself. We consider a hypothesis to be the better confirmed the more our evidence, the greater the extent to which factors not previously known or explained can be brought under it. Nothing seems more obvious. However, profound difficulties lurk just under the surface of this deceptively simple account of confirmation. The problem is what constitutes the evidence, the facts that such a hypothesis explains.

This problem can be made clear in connection with two so-called "paradoxes" of confirmation. The first formulated by Carl Hempel,[7] is known as the "raven paradox". Suppose we want to confirm the following hypothesis H:

All revens are black.

In symbols this is (x) (Rx - Bx). On the present view, we look around for individual cases of black ravens; they constitute appropriate positive instances of the generalisation. But H is logically equivalent to H_1:

All nonblack things are nonravens.

In symbols this is (x) (-Bx - Rx), a particular thing that is not black and is not a raven is a positive instance of, and hence confirms, this generalisation. We are, then, left with a paradoxical result that anything that is not black and is not raven - e.g. "the Washington Monument" also confirms the hypothesis that all ravens are black.

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7. Carl Gustav Hempel, "Studies in the Logic of Confirmation", Mind LIV (1945), 8-p.18.

Matters can be made worse. H is logically equivalent not only to H_1 but also to H_2

> If anything is a reven or is not a raven, then if it is a raven it is black.

In symbols this is (x)((RxV-Rx) - (-Rx V Bx)). This hypothesis is confirmed, via its positive instances, by nonravens, and also by black objects. Since H_2 is logically equivalent to H_1 nonravens and black objects - as well as black ravens - confirms the hypothesis that all ravens are black.

According to Hempel, the trouble lies neither in our original notion of a positive instance nor in the equivalence condition. Rather, it lies in our thinking that the result to which they lead in implausible. In his words, "The impression of a paradoxical situation is not objectively founded; it is a psychological illusion".

In Hempel's view, this "illusion" has two sources. One is our mistaken tendency to think that generalisations of the form "All A's are B's" are <u>about</u> A's only. Such generalisations, however, are about all objects whatsoever in some specified domain of discourse. For example, "all ravens are black" asserts that if <u>anything</u> is a raven it is also black. We are misled in to thinking otherwise, and

hence into (mistakenly) rejecting non-black nonravens, black non-ravens, and so on, as perfectly good instances of this same generalisation.

The other source of the "illusion" is the frequent and illicit intrusion of additional information. What leads us to rule out, for example, the Washington Monument, as the subject of an acceptable positive instance of the raven generalisation is the fact that we already know of the Washington Monument that it is neither a raven nor black. Hence, it cannot provide us with any "new" evidence for the generalisation. If we assume nothing in advance about objects teste d in connection with some hypothesis, if we do not allow any additional confirmation we have about them to intrude, then the paradox "vanishes".

Two features of Hempel's account to emphasise are, firstly, Hempel retains the original notion of a positive instances and the equivalence condition, electing instead to explain away their apparent conflict with other intuitions we might have about confirmation and secondly, Hempel's is a "formal" account of the notion of an instances, and hence of confirmation. To put these points in a slightly different way, what determines the instances,

confirming and disconfirming, of a generalisation is the language in which the generalisation is formulated.

A response to the "raven paradox" which finds fault with both the features of Hempel's account may be considered. This response sides with, and so does not attempt to explain away, those of our intuitions about confirmation that conflict with the original notion of a positive instance and the equivalence condition, contending that neither can be characterised in a formal way. Thus, this approach would argue for a rejection of solely formal accounts of confirmation and, on the other, it would propose other criteria by which to characterize the notion of a confirming or disconfirming instance of a generalisation.

The argument for a rejection of solely formal account is simple: the implausible consequences to which they lead, for example, the "raven paradox", cannot be explained away, a la Hempel, as somehow "illusory". Rather, is is the formal account itself which must given way.

The main line of attack might be based on what is taken to be a more adequate description of scientific

inquiry. Certain crucial points emerge from such a description. In the first place, the scientist typically does not count just anything as a confirming or disconfirming instances of the hypothesis that the tests. The class of potential instances is vastly more delimited. Usually only a very few tests cases are considered. In the second place, information of the kind Hempel urges should be excluded constantly does intrudes, and it is not at all clear whether or not such information intrudes legitimately or not. In fact, it is often such information that allows us to rule out in advance whole classes of cases as potential instances. To suggest that there are descriptions under which C would count as evidence for the hypothesis 'All A's are B's' would seem at best counter-intuitive. In the third place, isolated hypotheses are rarely, if ever, put to direct experimental test in the way indicated. The testing of hypothesis, and the characterising of evidence for of against them is a great deal more complicated conceptually.

The point can be made clearer by way of examples. Confirming or disconfirming the raven generalisation, one might assert, is not a simple matter of discovering positive and negative instances. Suppose we were to find a white

raven, that is, a bird alike in every respect to ravens but white. This does not necessarily entitle us to say that the generalisation has been at least partially disconfirmed. If, for example, we believe that our white raven has been bleached we might be led to restrict our generalisation: all unbleached ravens are black. Again we might reject this one bird as a pathological case, or we might even be led to reject our criteria for ravenhood. The particular move we make is not arbitrary: it is dictated by a variety of different considerations and circumstances. The finding of a 'white raven' by itself does not dictate a response. By the same token, whether or not the case of white raven is to count as an instance, confirming or disconfirming, of the hypothesis depends on many factors other than the form of its description. There has, in fact, been a great deal of controversy in the history of science about the way in which particular cases should be treated.

The preceding remarks, of course, do not suggest that there is no such thing as an instance, or positive instance, of a hypothesis, or that generalisations are never confirmed or disconfirmed, or that the facts do not control the kind or speculation scientists engage in. What is suggested is that

what counts as an instance is not determined by form alone. A variety of different considerations are relevant, most of them tied to the theoretical framework in which the particular hypothesis at stake is embedded. Thus, P.K. Feyereband writes:

> ...scientific theories are way of looking at the world; and their adoption affects our general beliefs and expectations, and thereby also our experiences and our conception of reality. We may even say that what is regarded as 'nature' at a particular time is our own product in the sense that all the features ascribed to it have first been invented by us and then used for bringing order into our surroundings.[8]

> ... the meanings of observational terms depend on the theory on behalf of which the observations are made.[9]

Many of the points made also arise in the "grue paradox" formulated by Goodman.[10] This paradox may be explained as follows. We are asked to suppose that all emeralds examined before a certain time t are green. Given our original notion of confirmation via positive instances, at time

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8. P.K.Feyereband, 'Explanation, Reduction, and Empiricism' in Herbert Feige and Grover Maxwell, eds., Minnesota Studies in the Philosophy of Science (Minneapolis,1962). III, p.29.

9. Ibid, III, p.94.

10. Nelson Goodman, Fact,Fiction and Forecast. (Indianapolis, Indiana: The Bobbs - Merrill Company, Inc., 1965).

t our observations support or confirm the hypothesis that all emeralds are green. "Our evidence statements assert that emerald a is green, that emerald b is green, and so on: and each confirms that general hypothesis that all emeralds are green. So far, so good."

At this point, Goodman introduces a new predicate, "grue", which "applies to all things examined before t just in case they are green but to other things just in case they are blue". Now consider the two hypotheses:

H. 1 : All emeralds are green.

H. 2 : All emeralds are grue.

It should be clear from the way in which "grue" was introduced that at time t all the evidence for H.1 is also evidence for H.2, and vice versa. They are equally well confirmed; for at time t the two hypotheses have the same positive instances. However, this is paradoxical. In the first place, although we have been forced to say that they are equally well confirmed, they imply incompatible predictions about emeralds subsequently examined. The fact that all the emeralds examined so far have been green - hence also grue - seems not in the least to support the predictionthat the next emerald examined will be grue,

although it does seem to support the prediction that it will be green. In the second place, "grue" is a totally arbitrary predicate; we have no more reason for thinking that emeralds examined after time t will be blue than we have for thinking they will be red. So we have no more reason for asserting "All emeralds are grue" than we have for asserting "All emeralds are gred". We can cook up any number of "grue" type predicates. All will be true of emeralds to the same extent that "green" is; for the generalisations in which they figure are supported by precisely the same evidence. The evidence confirms just about any assertion we wish to make about emeralds; but as in the case of the "raven paraxox", this result is, in Goodman's word, "intolerable". In other words, Goodman's story about "grue" conflicts with a variety of entrenched intuitions about confirmations that we have.

Attempts to deal with the paradox, in one way or another, show how H.1 and H.2 are not equally well confirmed or, equivalently, that "grue" is not a projectable predicate in the sense that regularity in grueness does not confirm the prediction (or "projection") of future cases.

Goodman's own solution to the problem of distinguishing

between projectible and non-projectible predicates proceeds in terms of a notion of entrenchment. The characteristic distinction between H.1 and H.2, according to Goodman, is not formal or semantic, it has to do with the use we have in fact made of "green" and "grue" in formulating and testing hypothesis. In this sense "green" has a more impressive biography and hence is "much better entrenched than the predicate "grue".

Goodman's criterion, on the one hand, seems to be too weak. "Green" is more projectable than "grue" because it is better entrenched. No further reason is given why "green" should in fact have been projected earlier and more often. That "green" is projected rather than "grue" was a matter of chance. But this is counter intuitive. What must be done is to advance other sorts of reason why "green" and not "grue" is projectible per se, and not simply that it has in fact been more often projected. On the other hand, Goodman's criterion seems to be too strong. It rules out the possibility of certain kinds of scientific change. Consider, for example, the predicates 'has a mass' and 'has a weight' from a seventeenth century point of view. From that point of view, "has a weight" is much better entrenched than "has a mass" and hence is

more projectible. How, then, are we to explain that "mass" came to be projected instead of "weight", in fact in the course of time to replace it completely. Such a recurrent feature of the history of science cannot be explained by Goodman's criterion.

A suggested solution which relates to and enlarges upon the resolution of the "raven paradox" exploits both the difficulties of Goodman's proposal. It turns on the claim that the notion of an emerald is involved in H.1 and H.2. The projectibility of predicates is in large measure a function of the type of thing of which they are predicated. Typically, objects are characterised by the particular theories in which they figure, and hence the range of inductive predicates that are inductive with respect to them is determined. That emeralds are physical objects is enough to undermine the generalisation of "All emeralds are grue". However, we do know a great deal about them, even on very primitive theories, and this knowledge allows us to narrow the range much further still. To put the point in a very strong way, if you don't know what predicates are projectible with respect to emeralds, you don't know what emeralds are. We do know what emeralds are, relative to some theory; so we do know what predicates are projectible

with respect of them. Theory determines the range of projectible predicates; therefore, it determines the range of lawlike generalisations. We may even say that theory determines to a very great extent what the facts are. T.S. Kuhn writes that after about 1630

> ... ard particularly after the appearance of Descartes' immensely influential scientific writing, most physical scientists assumed that the universe was composed of microscopic corpuscles and that all natural phenomena could be explained in terms of corpuscular shape, size, motion and interaction. That nest of commitments proved to be both metaphysical and methodological. As metaphysical, it told scientists what sort of entities the universe did and did not contain: there are only shaped matter in motion. As methodological, it told them what ultimate laws and fundamental explanations must be like: laws must specify corpuscular motion and interaction, and explanation must reduce any given natural phenomena to corpuscular action under these laws. More important, the corpuscular conception of the universe told scientists what many of their research problems would be.[11]

Knowledge of nature is, therefore, determined by what Kuhn calls "ways of seeing the world", that is knowledge of nature would always involve an evaluation reflecting

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11. T. S. Kuhn, The Structure of Scientific Revolution, Chicago: Chicago University Press, 1962.

the scientist's own conception of nature and since there cannot be the way of seeing the world, or in other words, what can be claimed as objective criteria, for instance, simplicity, the scientist is faced with the problem of choice between diverse and even conflicting frameworks; and this choice is a highly evaluative one.

CHAPTER - III

SCIENCES OF MAN AND QUESTION OF VALUE

I

The problem of whether philosophical treatment of the sciences is merely that of reporting on the tactics of discovery and the canons of explanation which scientists do employ, or that of laying down norms of scientific propriety to which they ought to conform is frequently felt to be more glaring in the sciences of man, for, the normative or critical tendencies in the sciences of man are more apparent than in the sciences of nature.

Kai Nielsen in his article "The Myth of Neurtrality"[1] argues that there is something intrinsic to the very structure of the social sciences which makes it impossible for them to be normatively natural. To take Nielsen's own example, suppose one is to explain the differences in labour militancy of Quebec and Ontario. It is possible to aspire to a somewhat normative neutral stand as long as one limits oneself to descriptions of the order: so and so many workers are unionized in Quebec and so and so many in Ontorio, so and so many strikes occured in Quebec and so and so many strikes occured in Ontorio, the level of employment in Quebec and Ontorio is such and such, and so on.

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1. Indian Philosophical Quarterly, Vol.X,No.1,October,1983.

While such a descriptive lists may not even be normatively neutral-for instance, it may reflect a certain status quo-ist stand-point-according to Nielsen, explanation of social phenomena is not "such a laundry list". It also aspires to be explanatory, interpretative and to provide a conceptual framework. For instance, one may explain the greater militancy of the Quebec workers in terms of their class consciousness which in turn may be explained as a result of their longer and sharper history of exploitation as an American colony. Now, the very talk of "class consciousness", "exploitation", and being in effect "colonized" commits one to a Marxian or quasi-Marxian framework. This framework is highly interpretative and carries with it normative and ideological commitments.

Suppose we still insist that what social scientists should do is to simply tell it like it is without using such normatively freighted concepts. But what is it "to tell it like it is " here and still accurately explain the differing degrees of labour militancy ? Suppose we talk of the Quebec worker not in terms of class but of status, or even his anomie, his enhanced feelings of disenchantment with the system because of his different ethnic, religious and cultural affiliations. Similarly, we could

drop the word "exploitation" and talk of work under harsh conditions for minimal wages or of American economic penetration. The crucial thing to note here, however, is that there is no generally agreed on vocabulary which just tell it like it is without an interpretation which also involves an evaluation exhibiting very fundamentally the scientist's own conception about society. Our various and often conflicting images of society, together with our conceptions of values and beliefs go into the theoretical framework we will adopt to explain and interpret such phenomena.[2] And all these notions are highly evaluative.

The presence of diverse and even conflicting norm-laden conceptual frameworks would find us in what Charles Taylor has called the hermeneutical or interpretative circle. In the absence of objective criteria as a means to escape the circle, one has to choose between conflicting frameworks and this choice is a normative one.

The sciences of man cannot, therefore, be normatively neutral for they involve an evaluation exhibiting the scientists conception of man and society and his choice of and commitment to a conceptual framework.

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2. Charles Taylor, "Interpretation and the Science of Man" The Review of Metaphysics, Vol.XXV,No.1,(Sept.1971)

II

While the sciences of man cannot thus be normatively neutral involving, as it is, an evaluation exhibiting the scientist's conceptual framework, there is a more fundamental sense in which it may be said that the sciences of man cannot be value-neutral. This sense emerges from the fact that man the object of inquiry is also a self-inquiring, self-explaining creature who seeks to explain himself or of fellow human beings in terms of concepts such as thought, reason, intention, motive, wish, desire and variants of these concepts, e.g. hope, aspiration, ideal, and so on. For example, suppose a person is standing in a queue outside a polling booth on election day. The correct answer to the question "Why is the person standing in the queue ?" may be "The person is waiting for his turn to cast his vote". This will be an explanation in terms of the person's intention to vote. The latter may be further explained in terms, say, of the person's admiration for a particular candidate or for democracy as such, or his <u>hope</u> that his vote will make a difference, and so on. Now, it is always possible to ask of an intention or of a motive or of a hope or of a wish whether it is proper or improper, good or bad, right or wrong, and so on. The important point

that emerges is that a human action can always be made the subject of an evaluation judgement and that this is logically connected with the kind of explanation that is typically given of human action. In this sense, then, evaluative judgement enters into the sciences of man not because of the conception, choice, and commitment of the scientist but because human actions are necessarily couched in terms of concepts like reason, intention, and the like. And it may be pointed out here that intentions and the like are subject to evaluative judgement precisely because of the fact that they involve an evaluation on the part of the agent so that actions are in themselves never value-neutral. It may also be pointed out here that actions are in themselves never value-neutral because they take place within a certain conceptual framework which serves as a criterion for the agent in egaluating and conducting possible lines of action.

Now it may be said that the "good of man" is a moral concern, and although, accassionally, in choosing his framework or the subject-matter of study, the scientist may be motivated by moral considerations, ironically the same considerations may dictate that he abandons, even if for a

particular inquiry, the idea that man is a self-inquiring, self-explaining creature who seeks to explain himself and other fellow human beings in terms of concepts like reason, intention, motive etc. This will mean that moral considerations themselves might dictate that the scientist takes a "value-neutral" view of his subject-matter. However, we might advance a still further argument in support of the non-value-neutrality of the social scientist. The argument is that beyond the question of what is good three must arise the further problem of what is right, and it is to this question about the consideration we must give to the interests of other human beings as we pursue our own good that ethics addresses itself. Inquiry therefore must look into the conditions that must be satisfied if one person's action is to be justifiable to another individual who may be significantly affected by it. The central concept, then, is that of justice; and the central problem is whether the requirements of justice are compatible with the aspirations of each individual. The attempt, we may then say, is to lay the rational foundation of an order of compatibility and cooperation among human wills. The sciences of man, then, calls for a commitment to morality in so far as any claim to knowledge is seen as doing justice to the object of the

knowledge claim. It may even be argued that this commitment is perfectly consistent, even logically connected, with the kind of explanation that is given of human actions.

III

P.F.Strawson's essay "Freedom and Resentment"[3] can be usefully interpreted as an attempt to establish the view that human actions involve reason, intentions, motive, and the like, so that actions are connected with what the agent believes to be good and bad, proper and improper, right and wrong, and thus subjecting actions to evaluative judgement. Again the essay can be interpreted as a call to a moral commitment over and above one's commitment to a conceptual framework.

In the essay, Strawson makes a distinction between an "objective" attitude and a "reactive" or "participatory" attitude in one's relationship with other human beings. In Strawson's words:

> To adopt an objective attitude to another human being is to see him, perhaps as an object of social policy; as a subbject of what in a wide range of senses, might be called treatment; as something certainly to be taken account of, perhaps precautionary account, of; to be managed, or handled, or cured, or trained; perhaps simply to be avoided... The objective attitude may be emotionally toned in many ways,

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3. Freedom and Resentment and Other Essays. London, Methuen, 1974.

> but not in all ways; it may include repulsion or fear; it may include pity or even love, though not all kinds of love. But it cannot include the range of reactive feelings and attitudes which belong to involvement or participation with others in interpersonal relationship; it cannot include resentment, gratitude, forgiveness, anger or the sort of love which two adults can sometimes be said to feel reciprocally towards each other. If your attitude towards someone is wholly objective, then though you may fight him, you cannot quarrel with him, and though you may talk to him, even negotiate with him, you cannot reason with him. You can at most pretend to quarrel or reason with him.[4]

The objective attitude may be more or less partial as when one adopt this attitude towards a particular action of an agent while, at the same time, retaining the full range of reactive attitude towards him, and again, as when one adopt this attitude towards an agent (and not towards a particular action of his), but with regard to a more or less temporary phase of his life, as for instance, when he might be considered as "acting under abnormal circumstances". Then there are cases where there are compulsion on the objective attitude to extend beyond any particular phase of the agent's life to encompass as it were his "entire" life; and in such cases all our reactive attitudes tend, correspondingly, to be profoundly modified. Here, the agent, as it were, "is himself", but he is abnormal, "warped", or "deranged, neurotic or just a child".

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4. Op.cit.,p.9

> Seeing someone, then, as warped or deranged or compulsive in behaviour or particularly unfortunate in his formative circumstances - seeing someone so tends, at least to some extend, to set him apart from normal, participant reactive attitudes on the part of one who so sees him, and tends to promote, atleast in the civilised, objective attitudes.[5]

The important point that emerges is that the reactive or participatory attitudes are, as it were, constitutive of normal human life - "constitutive" not perhaps in the very strong Kantian sense of the term, but at least in the sense of being a very general, practically irrepudiable, fact of human life. "The human commitment to participation in ordinary interpersonal relationships is ... too thoroughgoing and deeply rooted for us to take seriously the thought that "our world might change in a way such that,

> in it, there were no longer any such things as interpersonal relationships as we normally understand them; and being involved in interpersonal relationships as we normally understand them precisely is being exposed to the range of reactive attitudes and feelings that is in question.[6]

Now, when one adopts an objective attitude towards a person and thus see him as "warped", "defanged" or "compulsive in behaviour", one necessarily excludes the range of reactive feelings and attitude like "resentment,

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5. Op.cit.,p.9.
6. Op.cit., p.11.

gratitude, forgiveness, anger or the sort of love which two adults can sometimes be said to feel reciprocally towards each other". To exclude these range of reactive feelings and attitudes in an interpersonal relationship is to see the person as somehow incapable of conducting his behaviour in ways other than the way he actually conducts his behaviour; it is to see him as somehow irresponsible, lacking, as he is, notions of what is good or bad, right or wrong. Thus, one cannot resent a person, or be grateful to him in an objective inter-relationship. On the other hand, one may resent a person or be grateful to him in a reactive inter-relationship and the pre-assumption behind these feelings and attitudes is that a person is seen as being able to reason and intend to act in one way rather than another; in a proper or improper way, a right or wrong way. Thus, we resent a person because we know he can behave in a much less compromising way. Seeing a person as thus, one can say that he <u>ought</u> or <u>ought not</u> to behave in such and such a way./Actions in a reactive or participatory interpersonal relationship - a relationship which is "constitutive" of normal human life-can always, therefore, be made the subject of an evaluative judgement.

Further, the reactive attitudes are also radically connected with the notion of morality. The ideas of resentment, forgiveness, gratitude and love are basic to morality atleast in so far as the idea of good-will is basic to it; because the availability of these ideas depends crucially on the possibility of genuine expressions of good-will and its opposite. Take forgiveness, for instance. To forgive another person is, on the one hand, to admit that resentment may be the proper attitude to take towards his behaviour to one, and on the other hand, at the same time, to repudiate this attitude. And just as behaviour towards which resentment is appropriate is expressive of ill-will, forgiveness is necessarily expressive of good-will.

IV

Now, if human actions are connected with the "freedom" to act in one way rather than another, that is, with reason, intention, motive and the like, and if this is what makes human actions a subject of evaluative judgement, it is pertinent to ask the question; what is it to say that human actions are free acts, that they are intentional or have reasons behind them, so that we are in a better position to claim that evaluative judgments

are logically connected with human actions and that this is logically connected with the kind of explanation that is typically given of human actions.

According to some, intentions have a psychological ring so that they are understood strictly in terms of the mental atmosphere, the thoughts and feelings and motivations of the object of inquiry. The method characteristic of such a view is a form of empathy. Now, whether such a recreation in the mind of the thought and feelings of a person can be done or not, - and there are not only empirical but logical difficulties involved - what is crucial is that a person's intention to act is not something which is simply there; it is a "configuration of circumstances which, to the agent of observer, appears as a meaningful "reason" of the behaviour in question".[7]

Thus, given that I am totally ignorant of the language of experimental physics it is impossible for me to intend to carry out an experiment in physics. And my tinkering with some physics equipment in the laboratory cannot be explained in terms of my intention to conduct an experiment in physics. For one to have an intention, is

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7. Max Weber, The Methodology of the Social Sciences. (New York, Macmillan, 1950), 33:Chapter 1.

also for one to be able to describe or envisage the situation which will count as a"meaningful reason" and hence the fulfillment of this intention. For the same reason, someone, who is completely unfamiliar with concepts such as people's representative, majority rule, polling etc., cannot possibly intend to vote. He may, indeed, go through the motions which are associated with voting; but he cannot really count as having voted. As far as he is concerned, he has merely put a piece of funny paper through a hole in a funny box. The point in having an intention is that it is at all possible only within the framework of a particular on-going institution - a way of life. Thus an experiment in physics is possible only against the background of what we might call the institution of scientific investigation or more appropriately, the way of life of scientific inquiry; or the conceptual framework, in terms of which the various activities we call scientific make sense. It is this latter which determines or contains the criteria for what is to count as an experiment at all, and for distinguishing between a proper or an improper one, a good or bad, a right or wrong one. Similarly, it is only against the background of the existence of certain kinds

of practices concerning decision-making in the social context that one may be said to participate in the act of voting and that the act of voting is good or bad, proper or improper, right or wrong.

Thus, if intentions are subject to evaluative judgements and if intentions are logically bound up with a particular way of life, it is this way of life which contains the criteria for distinguishing a proper form an improper intention, a good from a bad one, a right from a wrong one. In this way the claim that the sciences of man can never be value-neutral in that evaluation exhibiting the scientist's conceptual background always enters into knowledge seems to be logically unsound. If at all the non-value-neutrality of the sciences of man is claimed, it is to claim that human actions are connected with evaluative judgment and the criteria for this evaluative judgment lies not with the scientist's conceptual background but with the conceptual background of the object of inquiry.

V

Now, the claim that it is the conceptual framework - the way of life - of the object of inquiry which contain the criteria for evaluating knowledge, can be interpreted as a deterministic one. For example, the behaviourists interpretation of what Wittgenstien says of a rule, that

> When I follow a rule, I do not choose,
> I follow it blindly.[8]

In such a behaviourist interpretation it would be senseless to talk of human actions in terms of reasons, intentions, motives and such. Further, it would be senseless to talk of human actions as good or bad, proper or improper in more or less the same way as it would be senseless to talk of fire that it is good for it to burn.

Peter Winch[9] emphasising that all "meaningful behaviour" are cases of what it is to follow a rule and that rule presupposes social interactivity - a way of life - himself rejects the behaviourist interpretation. The interpretation according to Winch, assumes

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8. Philosophical Investigations, 1.219.
9. The Idea of a Social Science, (London, Routledge, 1958)

> that most human behaviour can be adequately described in terms of the notion of habit or custom and that neither the notion of rule nor that of reflectiveness is essential to it.[10]

Winch admits that meaningful behaviour or "knowing how to do something" is possible without reflecting upon the rules of such activity, and that, in fact, all behaviour following traditions is, in this sense, without reflection. He, however, emphasised that any behaviour which follows a rule can be justified if reflected upon, for example a Freudian case where the behaviour of a person is explained by an observer to the person concerned. The essential question is not whether a person can formulate the rule which he follows, but rather whether it is meaningful

> to distinguish a right and a wrong way of doing things in connection with what he does[11]

"Learning how to do something" is, then, not just copying what someone else does, one has to acquire the ability to apply a criterion; one has to learn not merely to do things in the same way but also what counts as the same way.

From this analysis Winch concludes that human behaviour which follows a rule - in contrast to the behaviour of the dog who merely responds to appropriate

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10. Ibid, p.57.
11. Ibid, p.58.

stimulus without any conception of what it is doing and what would be to respond differently - cannot be called "blind" because it implies understanding. And understanding which involves not only what is simply there but the contradictory too:

> I understand what it is to act honestly just so far as and no further than I understand what it is not to act honestly.[12]

Meaningful behaviour is, then, characterised as presupposing a principle (maxim); and on the other hand the very idea of a maxim of behaviour presupposed actual behaviour, that is, a form of life. Therefore, to say that form of life determines what is and what is not to count as meaningful behaviour is not to invoke a deterministic thesis in interpreting human actions. Rather it is consistent with the claim that human actions are free and that it is precisely this freedom, the existence of alternative actions, which logically bound up human actions with evaluation.

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12. Ibid., p.65.

CHAPTER - IV

IS THERE A UNITY OF THE SCIENCES ?

I

There seems to be a sense in which one may claim that there is a unity of the sciences. This sense emerge from the fact that the 'objects' of inquiry in the sciences are ultimately to be understood in terms of cultural framework to which the 'objects' belong.

In our effort at knowing man, we have claimed, value is logically bound up with facts. We have come to this conclusion because of the fact that man as object of inquiry is a self-understanding and self-explicating creature so that understanding his actions involve concepts like intention, reason, motives, and so on.[1] Intentions and motives are neither a species of causal explanations nor are they replaceable by such explanations. Social concepts, likevoting or money, cannot be adequately described in 'neutral' terms; in observing manifest behaviour we do not observe the internal meaning of an action; this meaning lies in the logical connections the action has with the complex of intentions, choices, desires, conventions, and moral rules that are all inextricably involved together in social life. Finally,

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1. See Chapter-II, pp. 28

we have claimed that 'social life' is not, indeed cannot be, independent of the way in which man's culture formulates his vision of what he is seeing; there are only cultural constructions of reality and these cultural constructions of reality are decisive in what is perceived, what is experienced, what is understood; the 'facts of life' are always a special case of the cultural definition of things, they have no independent existence apart from how they are defined by culture.

Our inquiry about man is therefore, ultimately about his culture.

In more or less the same way, the facts of the natural sciences are what they are in relation to the cultural scientific framework to which they belong; the facts of science are necessarily theory-laden, and theory cannot be taken apart from the conceptual cultural framework. Science, therefore, can be said to be a cultural system of a very special sort, but it is no less a part of culture. Just because science determines that a particular construction of reality is also real makes it no less a cultural construction of reality. The scientific reality of today is not the scientific reality of yesterday nor will it be the scientific reality of tomorrow. Reality

in the sciences is itself constructed by the beliefs, understanding and comprehensions entailed in cultural meanings.

Understanding the facts of science, therefore, must ultimately be in terms of culture. In this sense, we may, then, claim that there is a sense of unity of all the sciences.

II

We have, however, seen that while both the sciences of man and nature may be united culturally, there is a fundamental distinction between the explanations involved in them. Whereas in the sciences of man, explanation is in terms of concepts like reasons, intention, motives and so on, explanation in the natural sciences is in terms of causal concepts. This difference in the explanations involved in the two sciences suggests that there must be fundamental distinction between the objects of inquiry in the two sciences: whereas it is a necessary fact that man is a self- understanding, self-inquiring creature capable of using language, it is not at all plausible to take of natural objects in the same way without serious conceptual distortions. While man is an active participant in the creation, existence, continuance, or modification of culture,

natural objects are passive in the sense that they are structured to fit into a cultural framework; they cannot have meaning unless they are 'worked upon'.

We may then examine the attempts which have been put forward by proponents of the unit of all sciences and see whether or not their attempts are in concordance with the view we have put forward.

The defence of what is sometimes called 'unified science' is often ambiguous as between the claim that there is a unity of scientific method and the claim that ultimately all the disparate sciences ought to be 'reduced' to some single science. There is a common-sense assumption running through these claims that the sciences of man must be, in principle, part of the complete science of nature. This feeling amounts to the claim that since men and their social dealings are part of the natural order, they must be in principle amenable to explanation in terms of the same sort of naturalistic principles that every aspect of nature conforms to. Historically, this has been the keynote of empiricism, and from Kant onwards it has been met by a variety of Idealist arguments intended to show that we are at any rate not merely part of the

natural order. These historical origins of the argument go some way towards explaining how the denial of the 'naturalness' of human behaviour is confronted as if the denial was either wilfully obscure, or the first step in invoking the supernatural.

III

First, on the claim of reduction. The success of Newtonian mechanics and its accompanying if not wholly congruous, atomism has long been the symbol of the ambition to reduce all the sciences, including that of man to the science of matter in motion. In this analogy, it has been a frequent ambition to reduce sociology to psychology and psychology to physiology, thus paving the way for a complete reduction of the science of man to the science of nature. What has emerged from this ambition is the demonstration that since the terms belonging to the science which is to be 'reduced' are of a different logical category from those of the science to which it is proposed to 'reduce' it, the process of reduction cannot be straightforwardly deductive.[2] Rather, it requires what have been called ' bridge-statements', theoretical and therefore empirical linking propositions to the

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2. Ernest Nagel, The Structure of Science, (London, 1961), p. 358-66.

effect that one kind of phenomena - say changes in the colour of a light source - are also, or can be properly be understood as, phenomena of a different kind - say changes in the rate of photon emission. To reduce the science of man to the science of nature would therefore be the result of empirical discoveries which expanded the scope of the natural science, not of philosophical legislation.

There is much controversy as to the nature of the 'bridging-statements' used in effecting reduction and which serves to link the entities and attributes of the reducing and the reduced branch of science. Broadly these bridging statements can be understood as empressing 'nomological coextensionalities' of thing predicates and attribute - predicates or as an identity statement (analytic and synthetic). In other words, to reduce certain phenomena to other, on the one hand, is to show that the laws that describe the former follow logically from the laws that describe the latter. The other way is to reduce in the sense of 'decomposing' the former in terms of the latter.

A common example of the above two senses of reduction may be cited from within the natural science. Thermal

phenomena may be studied both by referring to the laws of thermodynamics say those dealing with the impossibility of building perpetual motion machines of assorted types, and the Boyle - Charles law which asserts the functional relationships between pressure, volume, and the temperature of gas. Now, gas is composed of molecules having only mechanical properties which mean that the phenomenon of heat can be 'decomposed' to mechanical phenomenon. If such an 'identical' reduction can be had, it is possible derive the laws of thermodynamics from the statistical mechanical laws that describe mechanical phenomena.

It is doubtdul whether the sciences of man can be 'reduced' to the natural sciences for one thing it is not at all clear whether we can have 'laws of human nature' as envisaged by Mill and others, the study of man being non-empirical but conceptual.[3] If we cannot, then, have law of human nature it would be absurd to talk of deriving the laws of human nature from the laws of nature or vice versa. To talk of man in terms of cells which may be further understood in terms of atoms or such would do injustice to our concept of man.

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3. See Chapter II above, especially see.4.

Moreover, the concept of reduction itself contains a lot of difficulties and inconsistency; its opacity is evident even within the 'established' sciences.

The above two senses of reduction not only seems to be connected with one another, the same sort of picture is also associated them. The picture is that of a hierarchy of levels, with each of which there is associated a unique range of objects and properties. Much of the controversy surrounding the issue of reduction in fact, results from this hierarchical picture in the background. It was Descartes who, in fact sketched this picture. Descartes claimed that science has a certain unity about it, in that all of its seperate branches may be arranged in a hierarchical order in terms of the complexity of their objects. At the bottom of the hierarchy stands physics, the branch of science which studies the objects of which the objects studied by the other branches are ultimately composed of cells. Since biology studies cell behaviour it is a more basic science than psychology, in the sense that the explanations it provides are more all encompassing and profound. But cells are composed of submicroscopic particles. Hence physics, which studies the behaviour and simple properties of these particles, is ultimately more explanatory

than biology. Thus, the question is typically not whether, for example, biology can be reduced to some other subject, but whether it can be reduced to physics. Conversely, it is maintained that unless biology can be reduced to physics, then biological phenomena have not been scientifically explained. Not only is physics the touchstone, but a particular mechanistic - deterministic physics. We are asked whether animate nature, specifically man, is a machine, whether a 'mechanistic' position can account for 'vitalistic' phenomena, and so on. But these are embellishments. The central issue is not 'mechanism versus vitalism' but whether certain phenomena can be reduced, in the intended sense, to others.

Reduction can then be crudely characterised as a relation between 'levels'. Understood in one way, it is a logical relation. Understood in another way, it is somewhat of the model of part/whole relation.

Connected with this is the so-called doctrine of 'emergence' which claims that certain phenomena cannot be reduced to others, hence are emergent with respect to them, hence cannot be scientifically explained. It has two variations. The first claims merely that in the theories

that presently characterise the different levels', reduction, in either sense, is not possible. For example, given the present state of physics, a reduction of biology is out of the question. What is 'emergent' today might be reducible tomorrow. Opponents of the doctrine of 'emergence' like to say that it turns on what state science is in, a contingent matter.[5] In fact, its proponents like C.D.Broad claim that the reducibility of certain types of phenomenon is impossible in principle.

For instance, C.D.Broad, argues in the following ways: Microscopic qualities of objects cannot be reduced to and explained in terms of other properties of their parts, because this would entail that, given the parts and their properties, other microscopic qualities could be predicted in advance of their having been observed and that is impossible. For example he asserts that we could not possibly have formed the concept of such a colour as blue unless we had perceived positive instances of it. As a result, even a superman could not deduce from his knowledge of the microscopic structures of a given object that the object would appear blue as it does to human beings.

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5. For example, N.Nagel dismisses the pretended implications of the emergentist position with the remark that it it has failed to observe that 'novelty is a relational characteristic of properties with respect to a given theory'. In A.Danto and S.Morgenbesser, eds.Phil of Sc. (Meridian Books, 1960) p.311.

> If the existence of the so-called 'secondary' qualities, or the fact of their appearance depends on the microscopic movements and arrangements of material particles which do not have these qualities themselves then the laws of this dependend are of the emergent type.6

Broad's argument seems to rest on what many have contended is a simple point of logic: No term that does not accur in the premisses of a valid argument can occur in the conclussion; from premisses that describe the movements and arrangements of material particles, we cannot derive conclusions about their atoms.

Assume that the logical point must be granted, then, if phenomena on one 'level' are to be reduced to (derived from) phenomena on another 'level' there must be a way of connecting the concepts that occur on one level to concepts that occur on the other. There can be two suggestions. One is that the connection is 'empirical'. By this is meant that in fact the concepts on different levels that we wish to connect refer to the same state of affairs. Reduction, so those who make this move claim, requires the introduction of factual identities.

The difficulty with this view is that in the course of reduction the meanings of the terms only contingently

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6. C.D.Board, The Mind and its place in Nature (London: Routledge and Kegan Paul, Ltd., 1925) p.71-72.

identified seems to change as well. Once 'temperature' is identified with 'mean kinetic energy', for example in the reduction of thermodynamics to mechanics, it is applicable to a variety of circumstances, and with a meaning that it did not have previously. This fact gives rise to a second suggestion, that in the course of reduction, concepts on one 'level' are redefined in terms of those of another 'level'. The connection between them is thus quasi-conceptual. The difficulty is that if, for example, the concepts of thermodynamics or biology are redefined, then in what is it that thermodynamics or biology as we originally understood them have been reduced.

The second 'emergent' argument which is more specifically directed against reduction is in the 'decomposition' sense. Suppose we assert that water is composed by hydrogen and exygen, hence in the intended sense, reducible to them; its properties can be explained in terms of the properties of the chemical elements. Or, to take a more problematic case, suppose we assert that sensations are made up of events in the central nervous system, hence reducible to them; the phenomena of sensations are explained by neurophysiology. The argument is

that water or sansation, or more accurately their properties (for example, 'is a thirst-quencher', 'is blue') are 'emergent' with respect to oxygen and hydrogen, on the one hand, and events in the central nervous system, on the other. For to say that some phenomena can be reduced to other phenomena in the sense of being decomposed in terms of them is to say that the former can be identified with the latter. But this is impossible. For if, for example, a pail of water could be identified with a collection of chemical elements, then it should be possible to attribute the same properties to each. But this cannot be done. Being a thirst-quencher is a property of water, having a numerically determinate atomic weight is not. Conversely, being a thirst-quencher is not a property of chemical elements. Hence, water and combinations of chemical elements cannot be identified, and hence, the former cannot be reduced to the latter.

To this it might be replied that it is a mistake to think that reduction implies identification. What happens, rather is that we replace our old concept of water and talk about water with the concept of chemical elements. Reduction is replacement, more strongly, elimination, not identification. But this reply in turn gives rise to a new argument that since it ignores certain properties or phenomena at the expense of others; something is 'left-out'.

The claim of reduction is, therefore, replete with difficulties, difficulties created, it may be said, by the opacity of the concept of reduction itself. And more importantly the claim of reduction would result in serious conceptual distortions, at least of the conception of man which has been put forward.

IV

If the claims of reduction are not clear, the claims of the unity of method are a good deal clearer. They are the claim that the logical properties of an adequate explanation are the same throughout the sciences, and the attendent assumption that any methodological requirements valid in the natural sciences will be equally valid in the sciences of man.

The most famous defence of this claim is, perhaps, Mill's System of Logic, though it has been equally vigorously defended by Popper, Hempel and others.[7]

Briefly, the claim is that the essence of explanatory virtue lies in deductively relating the explanandum -

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7. J.S. Mill, A System of Logic (London, 1961), VII, 2; K.Popper, The Poverty of Historicism (London, 1957) Sect.29; L.C.Hempel, 'The Function of General Laws in History, Journal of Philosophy, XXXIX (1942), 35, 48.

what is to be explained - to its explanans - the laws to conditions explaining it. Hence the proper method of science is the constant testing of explanatory principles by matching derived predictions against observed results, whether these results are obtained experimentally or thrown up by nature. Of course, there will be many differences in the characteristic interests of particulars sciences, both natural and social; but these neither fall into categories which coincide with the division between the social and the natural sciences nor do they require any substantial modification of this deductive account. The claim that the social sciences are 'non-experimental' would mark at most a practical distinction concerning the case with which we can obtain evidence, it obviously being simpler to manufacture most chemical reactions than to rerun a 'meaningful action' for the sake of testing our understanding of it. Practical difficulties aside, then, what made any discipline scientific is the seriousness with which it tests its principles against its observations.

There is no claim that the explanations of social scientists look like deductive argument, but it is claimed that they can in principle be turned into deductive arguments, a move which, both elucidates their explanatory force and renders their covering laws amenable to test.

The extreme diffidence with which social scientists usually proceed when asked to formulate their explanation in this form stems, on this account, from three possible sources. The first is that the generalizations on which social science relies are usually imprecise, common-sensical and rule of thumb ones which no one has thought out in the sort of detail which would render them amenable to vigorous testing. So, although a political scientist might agree that it is unwise for a government to raise taxes just before an election, he would not want to commit himself to the generalization that, say, 'all voters vote against government which raise taxes just before an election'. It takes no great wit to think of dozens of possible exceptions. It is not obvious that such an open-ended imprecision is only to be found in the social science, but it is surely characteristic of explanation in at least many of them. The second reason why the deductiveness of explanations in social science is not apparent is that the backing laws are not drawn from the same subject matter as the phenomena they explain. Thus, the generalizations which explain how men behave in political situations are either common place or recondite psychological generalizations about the effects of, say, pride, anger, public spiritedness, self-interest, and so on. The political scientist would be

interested in using these generalizations to make sense of political events, but he would not, qua-political scientist, be interested in spelling them out explicitly or trying to test them. And this brings up the third possibility, which is that social scientists are commonly much more concerned about the situations within which they are applying their generalizations than with the generalizations themselves. That is, many of the underlying generalizations are so taken for granted that there is no real question of trying to justify them. Rather, what we need to know is whether we have read the situation of their application correctly. Thus when a starving populace fails to revolt against its government, we do not at once wonder whether people enjoy starving and are grateful to ~~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. In short, if the social scientist is producing deductively valid explanations, with some of the covering laws left implicit, he still seems to be much more critically concerned than are most natural scientists with the establishment of the 'initial conditions' element in his explanations.

This third suggestion about the way in which one might defend the claims of the unity of science at onces raises a point which goes against the very claim it starts for. In the above example, the 'initial conditions' were described in terms of the beliefs and wishes of the people whose actions we are explaining. This means that the important definition of the situation so far as the validity of the explanation is concerned turns out to be the definition accepted by the people concerned. It is not a matter of our believing that governments ought to secure the food supply of their subjects; it is a matter of what those subjects believe. The question, then is not whether the social scientist, conceived as a quasi-experimental scientist, has correctly described the causal antecedents of a phenomenon, but whether he has understood how the persons involved would have described their situation. There seems to be no parallel to this in the natural sciences though it may be argued that this does not straight-forwardly rule out a belief in the unity of scientific method. Popper, for instance, insists on the applicability of the hypothetic - deductive model to historical explanation, while involving it by way of what he terms 'the logic of the situation'[8]

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8. Karl Popper, The Poverty of Historicism, (London,1957).

but it does mean 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.

So far as our contemporaries go, one can ask them the reasons for their actions; for our predecessors we can rely on written documents, or make plausible inferences from these. All this seems to be possible only with human beings, and where it is usable it seems to be the best possible way of inquiry - allowance being made for deception, the difficulties of translation and so on. Where we cannot readily get such information, we are forced to rely on whatever contextual evidence we can find, or to put ourselves in the shoes of those whose behaviour we wish to understand. That such empathic methods seems to be both possible and so generally necessary has frequently been taken as the great dividing line between the natural and the social sciences. Since the point of the empathic recapturing of past situations is to supply us with an account of what those situations meant to those involved in them, Verstehen has universally

become the jargon term for this kind of understanding which tries to recapture the meaning of actions and situations. Collingwood at one stage had written as though the task of the historian or social scientist were to re-experience the situation to be explained, to re-enact it in thought.[9]

It is easy enough to picture Verstehen as simply a heuristic device, whereby we put ourselves in another man's shoes in order to inspire hypotheses about his behaviour, the scientific side of our activities then being confined to testing those hypotheses. Even if the method of Verstehen is no more than a heuristic device, it is perfectly consistent with vigorous empirical checking at the level of 'causal adequacy'. We may recreate as best as we can in our minds the belief and values of people in a certain society. These beliefs must have certain behavioural consequences. Unless we can find men behaving in this way, we must have failed to understand either their beliefs or their situation or some related phenomena. Just as in any other area of inquiry, it is impossible to be dogmatic about which of the indefinite number of conditions we have got wrong; the

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9. R.G.Collingwood, The Idea of History,(Oxford,1943)pp.282.

crucial point is that it is not always the case that the social scientist is in principle exactly like any other natural scientist in his position. However, it is not always the case that Verstehen can most plausibly be understood as a heuristic device. At some levels of sociological or anthropological analysis it seems to be an essential element in knowing what is going on. Thus S.F. Nadel, describing the anthropologists' efforts to uncover the social structue of an alien society, argues that he regards behavioural regularities as evidence for underlying norms and believes which together govern the behaviour of men in that society.[10] Here it seems that it is not a matter of Verstehen suggesting hypotheses about empirical regularities, but rather that the regularities are made intelligible by being related to the subjective meanings which agents attach to their behaviour. In other words, behavioral regularities may be better understood by showing how they spring from the way the agents conform their behaviour to rules which gives them reasons for behaving in one way rather than another. Here the 'internal' aspect of the rules appears to be on essential element in their nature and one which gives Verstehen a role strikingly greater than that of

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10. S.F.Nadel, The Theory of Social Structure (London, 1954), p. 25.

the heuristic. On such an account, the sciences of man would certainly show some characteristic differences from the natural sciences both in terms of the content of their explanations and in the way in which they make their subject matter intelligible to us.

Finally, none of the above discussion means either that we cannot put explanations of human actions into the hypotheticodeductive pattern (though it strongly suggests that the effort is not worthwhile), or that there is no hope of explaining the findings of the social science in terms of those of the natural science. In fact, one may assert tha t it ought, presumably, to be one of the ambitions of an adequate human biology to explain such typical capacities as those for speech and rationality, and these are the basis of those subjective and intentional aspect of social life to which the concept of Verstehen draws our attention.[11] This however, would not bridge the gap between the explanations in the social and the natural sciences without committing serious conceptual distortions. Again, whatever, knowledge we possess, we can always step back from that knowledge and ask ourselves: 'How in

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11. N.Chomsky, Language and Mind (New York, 1968), Ch.I

the light of that knowledge, shall I decide to act ?' There seems to be a irreplacable distinction between 'the observed natural course of events' and 'a man's decisions about the natural course of events'. Knowing why an agent want X is no substitute for the agent's deciding that he want X. Man's action cannot be explained by a neutral vocabulary of natural laws.

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