

Explanation in the Social Sciences with particular
reference to Economics .

By

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ABSTRACT OF THESIS

The aim of this thesis is to discuss the nature of social phenomena, and to determine (with particular reference to economics) the appropriate way to explain them. Many of the contentions advanced rest largely upon the fact that social phenomena can be investigated only by methods which respect their distinctive character and status as social phenomena.

In chapter I it is argued that the most important difference between the social and the natural sciences is that the former have to employ intentional criteria to identify their explananda-phenomena. Because human and societal phenomena are intrinsically meaningful, the type of causation which prevails in the social realm is fundamentally different from that which prevails in the physical.

In chapter II the claim of Popper and Hayek that the task of the social sciences is to trace the unintended consequences of human actions is critically examined. Two examples of economic explanation are given in order to show the importance of unintended consequences, and to illustrate the general form explanations of social phenomena (apart from those of single actions) should adopt.

In chapter III the contention that the social sciences deal with inherently complex phenomena is examined and defended, and the main implications of this contention for social analysis are drawn. The extent to which social phenomena are in principle predictable is discussed.

In chapter IV the structural properties of formal scientific theories are briefly characterised, and then Friedman's

famous argument on the testability of economic theories is analysed and rejected. The role within a scientific theory of statements formulated with reference to idealizations of the phenomena being studied, and the role within a social explanation of the 'principle of rationality', are discussed.

In chapter V the central questions behind the methodological individualism/methodological holism controversy are brought to light. Provided that methodological individualism is not construed as a reductionist or mechanistic principle, it can successfully avoid the main objections of its detractors. It is argued that the method of functional analysis in sociology (in the form developed by Merton) is consistent with methodological individualism. Finally, it is claimed that (apart from a few minor exceptions) the principle of methodological individualism does indeed recommend the appropriate way to explain societal phenomena.

In chapter VI, the various strands of thought running through the five preceding chapters are drawn together in a brief summary of the most important points raised by this thesis.

PREFACE

This thesis assumes that the aim of science is to find true explanations of whatever phenomena require explanation, and seeks to show how the social sciences should proceed in order to realise this goal. Of the two lengthy examples of social explanation discussed in chapter II, I now consider the second to be somewhat out of place in the present time of national economic difficulty. In 1973 a more pertinent illustration of purposive actions leading to a societal repercussion not intended by the acting agents themselves, is given by the phenomenon of hyper-inflation; we all desire to maintain the purchasing power of our incomes, but in trying to achieve this aim (and in trying to gain an increment sufficiently large to offset expected price rises in the immediate future), our actions have the actual result of worsening the very situation which originally prompted us to act. However, while it would have been of greater contemporary interest to examine the example of hyper-inflation rather than an example concerning demand deficiency, I see no reason to detract what I assert near the end of the second chapter; namely, that the 'paradox of thrift' phenomenon is one of the finest examples, if not the finest example, known to the social sciences of actions directed towards a particular end bringing about a consequence utterly different from that intended by those whose actions cause the phenomenon in question.

I would like to record my gratitude to my supervisors whose help, advice and encouragement made it possible for me to write this thesis. First, I wish to thank Professor W.H. Walsh (my main supervisor) for his assistance throughout the period of my studies, and in particular for his kindness in reading and criticising so much of the material I produced during this time. Secondly, I wish to thank Professor A.J. Youngson for giving me his comments on my views concerning the basis and structure of economic theory.

I would like to thank also all those with whom I have had discussions over the past three years, and who stimulated me to think out a number of the points and arguments to be found in this thesis; I wish particularly to acknowledge the debt I owe in this

respect to my colleague Mr L.B. Briskman (but I do not of course claim that he agrees with everything in this work). I must also add that I have learnt much from my students, especially from those in the present and two previous Logic and Philosophy of Science classes upon whom I have tried out (in tutorials and lectures) many of my ideas.

Notwithstanding the aid and guidance I received from those I name above and from those I do not mention by name, I wish to state that I am responsible for this thesis and for its shortcomings.

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TABLE OF CONTENTS

ABSTRACT OF THESIS	i
PREFACE	iii
CHAPTER I. EXPLANATION AND THE SCIENCES OF SOCIETY	
(i) The Concept of Explanation	1
(ii) Teleological Explanation and Social Causation	12
(iii) Scientism and Social Science	31
CHAPTER II. PURPOSIVE ACTIONS AND RESULTANT SOCIETAL PHENOMENA	
(i) Intended and Unintended Consequences	56
(ii) Example (a) - The Phenomenon of Price Oscillation	78
(iii) Example (b) - The 'Paradox of Thrift' Phenomenon	92
CHAPTER III. THE EXPLANATION AND PREDICTION OF COMPLEX PHENOMENA	
(i) Science and Complexity	104
(ii) The Analysis of Social Complexity	116
(iii) Social Prediction and its Limits	135
CHAPTER IV. PROBLEMS OF THEORETICAL AND IDIOGRAPHIC EXPLANATION	
(i) The Structure of Formal Theories	157
(ii) Testability and Economic Assumptions	167
(iii) The Status and Function of Ideal Types	184
(iv) Degrees of Rationality	196
CHAPTER V. INDIVIDUALISM, HOLISM AND FUNCTIONALISM	
(i) What is Methodological Individualism?	213
(ii) Institutionalistic and Psychologistic Individualism	218
(iii) Methodological Individualism versus Methodological Holism	233
(iv) Functionalism - 'Strong' and 'Weak'	247
CHAPTER VI. CONCLUSION	264
BIBLIOGRAPHY OF WORKS CITED	268

EXPLANATION IN THE SOCIAL SCIENCES WITH PARTICULAR
REFERENCE TO ECONOMICS .

I EXPLANATION AND THE SCIENCES OF SOCIETY

(i) The Concept of Explanation

What constitutes an explanation? The answer that has dominated the philosophy of scientific method (or 'methodology', as this is usually called) in modern times can be found advocated in the nineteenth century by John Stuart Mill (1806-1873). In A System of Logic he writes:

"An individual fact is said to be explained by pointing out its cause, that is, by stating the law or laws of causation of which its production is an instance. and in a similar manner, a law of uniformity is said to be explained when another law or laws are pointed out, of which that law itself is but a case, and from which it could be deduced." ¹

This account of the nature of explanation was re-developed by K.R. Popper (now Sir Karl Popper) in his Logik der Forschung of 1934.² However,

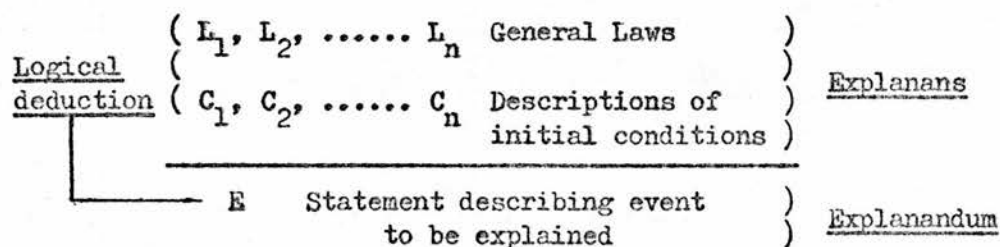
1. J.S. Mill, A System of Logic, 1843, (Longmans, London, edit., 1965), p. 305 (i.e. Bk. III, Ch. XII, sec. 1)
2. K.R. Popper, The Logic of Scientific Discovery, (Hutchinson, London, 1959, edit., 1968), pp. 59-62 Also:
 K.R. Popper, The Open Society and its Enemies, Vol. II, (Routledge and Kegan Paul, London, 1945, fifth edit., 1966), pp. 362-364
 K.R. Popper, The Poverty of Historicism, (Routledge and Kegan Paul, London, 1957, second edit., 1966), pp. 122-124

although in recent times the discussion undoubtedly originated with Popper, the best known expositions and elaborations of this view of explanation are those presented by Carl G. Hempel in 1942, and by Hempel jointly with Paul Oppenheim in 1948.¹ Mention should also be made of the lucid analyses of Ernest Nagel² and R.B. Braithwaite³, which too have become acknowledged classics within this area of thought.

Hempel and Oppenheim declare that a particular occurrence (of an event of a certain sort) is explained if, and only if, a statement describing it can be validly deduced from a set of independently testable true statements composed of (a) statements of

1. C.G. Hempel, 'The Function of General Laws in History' (1942), in P. Gardiner (ed), Theories of History, (The Free Press, Glencoe, Illinois, 1959), pp. 344-356
- C.G. Hempel and P. Oppenheim, 'The Logic of Explanation' (1948), in H. Feigl and M. Brodbeck (eds), Readings in the Philosophy of Science, (Appleton-Century-Crofts, New York, 1953), pp. 319-352
- Also:
- C.G. Hempel, 'Deductive-Nomological vs. Statistical Explanation', in H. Feigl and G. Maxwell (eds), Minnesota Studies in the Philosophy of Science, Vol. III, (University of Minnesota Press, Minneapolis, 1962), pp. 98-169
- C.G. Hempel, Aspects of Scientific Explanation, and other Essays in the Philosophy of Science, (The Free Press, New York, 1965, edit., 1970), pp. 331-496
- C.G. Hempel, Philosophy of Natural Science, (Prentice-Hall, Englewood Cliffs, N.J., 1966), pp. 47-69
2. E. Nagel, The Structure of Science, (Routledge and Kegan Paul, London, 1961, edit., 1968), pp. 15-46
3. R.B. Braithwaite, Scientific Explanation, (Cambridge University Press, Cambridge, 1953, edit., 1955), pp. 319-341

general laws, and (b) singular existential statements describing a number of antecedent (or 'initial') spatio-temporal conditions. The statement describing the event to be explained is called the 'explanandum', the event itself is called the 'explanandum-event', and the set of premises is called the 'explanans'. Schematically, the form of an explanation as envisaged by this theory can be laid out thus:¹



According to this account, the two most important characteristics of an explanation are that (i) "the explanandum must be logically deducible from the information contained in the explanans"², and that (ii) "the explanans must contain general laws, and these must actually be required for the derivation of the explanandum"³. Because of the claim that "the decisive requirement for every sound explanation (is) that it subsume the explanandum under general laws"⁴, this analysis of what constitutes an explanation is known as the 'deductive-nomological theory of explanation'.

The 1948 article advances a severely formalistic approach to explanation. It lays down that unless one is able to deduce the explanandum from explicitly exhibited statements of nomological laws and

1. C.G. Hempel and P. Oppenheim, op. cit., p. 322

2. Ibid., p. 321

3. Ibid., p. 321

4. Ibid., p. 331 (*italics added*)

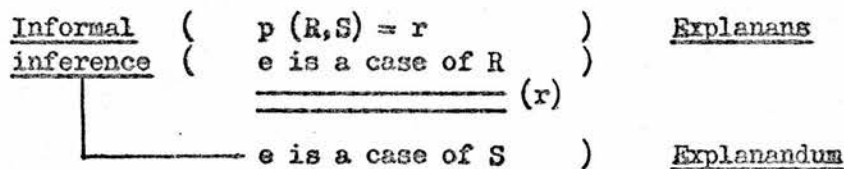
initial conditions, one has not explained the explanandum-event. (The general laws with reference to which the event in question is explained, are themselves to be explained by deduction from a formal theory. This formal theory is in turn to be explained by deduction from a wider theory and so on, if not ad infinitum then at least ad indefinitum.) For only if these rigorous criteria of adequacy are fulfilled can it be shown conclusively why a particular event occurred rather than did not occur. An explanation that conforms to the deductive-nomological pattern can do this because it rules out the possibility (since the explanandum is a logical consequence of the explanans) that a statement describing the non-occurrence of the explanandum-event could be consistent with the truth of the explanans. Thus, on this account, an event is either explained or it is not. The former is the case only if the formal criteria of adequacy are actually fulfilled.¹ Therefore, if one advances a

1. Once the two main criteria of adequacy have been fulfilled another feature of explanation (on this analysis) becomes discernible. This is the ability of the general form of the explanation to provide the structure for a prediction of the event in question - "an explanation is not fully adequate unless its explanans, if taken account of in time, could have served as a basis for predicting the phenomenon under consideration". (C.G. Hempel and P. Oppenheim, op. cit., p. 323) Thus, according to the deductive-nomological theory, the explanatory and predictive processes are formally symmetrical and hence identical in respect of logical status. The only difference between them pertains to the temporal position of the inquirer: after the occurrence, to deduce the explanandum is to provide an explanation; before the occurrence, a prediction.

At the beginning of chapter III section (iii) of this discussion the ambiguities of the term 'prediction' will be made plain. At the present moment it is sufficient to say that if by 'prediction' one means 'hypothetical prediction', then in most instances one's predictive ability will be as good as the degree to which one's explanations of the kind of events in question can conform to the formal structure advocated by the deductive-nomological theory.

candidate for an explanation and if, upon examination, this does not measure up to the required stringent standard, then what in fact one is offering is either, to use the terminology Hempel employs in his 1942 paper, a "pseudo explanation" or a mere "explanation sketch"¹.

However, in his later writings Hempel abandons the ultra-formalistic view that seemingly he held earlier, namely that 'to deduce an explanandum from an explanans containing at least one general law' is what is properly meant by the phrase 'to give an explanation of an occurrence'. He acknowledges, in clear contradistinction to the spirit pervading the 1942 and 1948 articles, that one can have genuine explanations in cases where the explanandum is not (and cannot be) formally deduced from the explanans, but receives only informal or practical support from it. In what he calls "probabilistic explanations"² of particular occurrences, statistical laws take the place of universal laws in the explanans. The general schema for an explanation of this kind is as follows:



Here 'R' stands for the reference class of events, while 'S' stands for the class of events whose members are those elements of the reference class which possess some specified property. The statistical law asserts

1. C.G. Hempel, in P. Gardiner (ed), op. cit., p. 351
2. C.G. Hempel, Philosophy of Natural Science, op. cit., p. 58ff
C.G. Hempel, Aspects, op. cit., p. 376ff

that the probability (or limit of the relative frequency) with which the members of the given reference class have the specific property in an infinite series of such events is of value r . The initial conditions include the statement that a particular event e is a member of the reference class. Given the truth of the explanans, it can be inferred informally that, with a likelihood of value r , e is a member of the specific class of events.

It is obvious that with probabilistic explanations the truth of the explanans is logically compatible with the falsity of the explanandum whenever the value of r is other than 1. Should it not therefore be argued that probabilistic explanations, since they cannot show why the occurrence of an event had to take place but only that it was likely to do so, are not proper explanations but only 'explanation sketches'? Hempel faces up to this criticism, but his reply involves

a sharp modification of his earlier explications of the concept of explanation. He writes:

"It is sometimes said that a probabilistic account does not explain the occurrence of an event, since the explanans does not logically preclude its nonoccurrence. But the important, steadily expanding role that probabilistic laws and theories play in science and its applications, makes it preferable to view accounts based on such principles as affording explanations as well, though of a less stringent kind than those of deductive-nomological form." 1

As an instance of an explanation that conforms to the general schema for a probabilistic explanation, Hempel gives a concrete example from the physical sciences. This concerns the radioactive decay of a sample of

1. C.G. Hempel, Philosophy of Natural Science, op. cit., p. 68

With reference to the same point, Hempel writes elsewhere:

- a) " this objection to the idea of probabilistic explanation rests on a too restrictive conception of scientific explanation: for there can be no question that statistical generalisations are widely invoked for explanatory and predictive purposes in such diverse fields as physics, genetics and sociology."
(Minnesota Studies in the Philosophy of Science, Vol. III, op. cit., pp. 127-128)
- b) " this objection to the idea of probabilistic explanation rests on a too restrictive conception of scientific explanation: for many important explanatory accounts offered by empirical science make quite explicit use of statistical laws which, in conjunction with the rest of the explanatory information adduced, make the explanandum no more than highly probable."
(Aspects of Scientific Explanation, op. cit., p. 391)

one milligram of polonium²¹⁸:

"Suppose that what is left of this initial amount after 3.05 minutes is found to have a mass that falls within the interval from .499 to .501 milligrams. This finding can be explained by the probabilistic law of decay for polonium²¹⁸; for that law, in combination with the principles of mathematical probability, deductively implies that given the huge number of atoms in a milligram of polonium²¹⁸, the probability of the specified outcome is overwhelmingly large, so that in a particular case its occurrence may be expected with 'practical certainty' ." 1

The point made forcibly here, is that genuine explanations may have a form that departs somewhat from the syntactic character of the strict deductive-nomological structure. Hempel thus appears to urge that the question of the form a particular explanation should adopt is not something that can be decided just in the abstract, but that reference is required as well to the kind of explanandum-phenomenon in question. From this important insight (made with reference, it should be noted, to explanations within the sphere of the physical sciences), much of value can be learnt as to the general nature of the process of explanation.

Whether a purported explanation of a specific occurrence possesses the form that an adequate explanation of an event of that sort should possess, is a matter that cannot be determined just by scrutinising the syntactic connections holding within the set of statements offered as an explanation. This matter can only be settled by showing that the structure of the candidate for an explanation is appropriate as regards the phenomena within the domain of inquiry with which one is concerned. But how does one show that a particular form of

1. C.G. Hempel, Philosophy of Natural Science, op. cit., p. 68

explanation is indeed appropriate for events that take place within the type of subject-matter under investigation? Unhappily, this is not something that one is ever able to demonstrate with certainty; in practice, one can only obtain 'good assurance' that a particular explanation has a general structure that is appropriate to the nature of its subject-matter. A possible indication that this is so is given if those seeking explanations as replies to their questions of inquiry ('Why did this happen?', or 'Why is this the case?') are to a considerable extent in inter-subjective agreement, both among themselves and with the person from whom they request the giving of explanations, as regards what metaphysical (i.e. basic or fundamental) presuppositions are true of the phenomena comprising the subject-matter in question. A second possible indication that one is in fact using an appropriate mode of inquiry, is given if one's actual investigations succeed in producing fruitful explanatory accounts - but here it needs to be recognised that how the term 'fruitful' is to be understood in this context may itself be a contentious affair. Nevertheless, although it is the case that assurance concerning the formal adequacy of any given explanation is either relative to the extent of agreement upon 'fundamentals' in the human context of its deliverance, or is dependent upon the partially pragmatic criterion of research 'success', we shall here assume that it is hardly open to dispute that many explanations possessing formal structures that are truly appropriate to the kind of phenomena with which they are concerned, are provided by both the natural and social sciences.

In the diverse and heterogeneous body of disciplines known as the natural sciences - physics, chemistry, zoology, taxonomic botany, geology, etc. and their many offshoots and technological

applications - one is dealing with subject-matters that contain purely physical (i.e. non-conscious and non-purposive) phenomena.¹

Within the natural sciences, it is a basic metaphysical presupposition that the objects and processes comprising the subject-matters are non-sentient and purposively 'inert', that is, they cannot be persuaded, reasoned with or treated in the way one treats deliberative agents. If one's aim is to explain the occurrence of (rather than merely to describe and classify) such phenomena, then it is methodologically appropriate to understand the question 'Why is this the case?' as meaning 'According to what (universal or probabilistic) nomological laws, and by virtue of what antecedent conditions does this phenomenon occur?' . As far as the natural sciences are concerned, if the general question of inquiry is taken to mean anything other than this, an inappropriate form of explanation will result. In the natural sciences, one explains the occurrence of a phenomenon by bringing it under a nomological law (or a number of them). How stringent the syntactic connections holding between the explanans and the explanandum need to be, is something that again depends on what particular sort of physical phenomenon is under investigation.

1. Strictly, this statement should be qualified slightly. All branches of biology are deemed (mistakenly?) to be natural sciences, and yet biologists, qua biologists, study, amongst other things, the behavioural patterns of organisms many kinds of which are undeniably conscious and whose behaviour is thus, supposedly, purposive to some degree. Therefore one must say that when biologists investigate 'lower' creatures as conscious beings living in animal 'societies', their methods must resemble, to put this no stronger, the modes of inquiry employed by social scientists. (What these methods are, will be discussed in due course.)

It is certainly the case that in some areas of the natural sciences, notably Newtonian mechanics and classical thermodynamics, one can produce explanations that successfully instantiate the strict formal structure demanded by the deductive-nomological theory. Explanations within classical physics can reach the methodological 'ideal' of a fully-fledged deductive-nomological explanation.¹ But within the natural sciences generally, the formal structure of explanations obtainable within classical physics should not be considered as paradigmatic. Areas of natural science such as elementary particle physics and Darwinian evolutionary theory (just to take two examples) have to use explanatory structures that are significantly different. In particle physics, as is well illustrated by Hempel's analysis of the appropriate way to explain the radioactive disintegration of a sample of polonium²¹⁹, one has to resort to a probabilistic (and thus non-deductive) form of nomological explanation. In evolutionary theory, the nature of the subject-matter is such that one cannot provide 'explanations of detail', but only 'explanations of the principle' upon which specific explananda-phenomena are produced. This kind of explanation is used in cases where one is dealing with areas of 'complex' phenomena, and for this reason 'explanations of the principle' do not show precisely why particular events occur, but show instead that what does occur falls within a wide range of events not prohibited as empirically impossible by the theory in use.²

1. This topic will be discussed in chapter III.

2. Again, this question will be examined in chapter III.

Even if one confines one's attention solely to the natural sciences and to the explanation of purely physical phenomena, the question of the formal manner in which an event should be explained cannot be answered if one remains entirely preoccupied with matters of syntactic structure; one can only decide what form a particular explanation should adopt, once one possesses a notion (or guess, or conjecture) as to the basic nature of the kind of subject-matter under investigation. And if this is the case (as it seems to be) with reference to just the various natural sciences, then it is surely a fortiori true when one turns to examine the appropriate way to explain occurrences lying within the distinctive field of actions and social phenomena.

(ii) Teleological Explanation and Social Causation

What are known collectively as the 'social sciences', consist of the individual disciplines of economics, sociology, social anthropology, political science and their variants. Although these are normally the only subjects which count as 'social sciences', it does no harm to add to this group human psychology¹ and history. It is to be recognised that this move may evoke vehement protest from both psychologists and historians; however, because a large number of methodological problems are common to both these two subjects and to the

1. For an analysis of the difference between human and physiological psychology, see chapter V of this discussion.

generally recognised social sciences, it is convenient to use the term 'social science' generically to cover all these disciplines together. The social sciences (considered in this slightly wider fashion) differ considerably among themselves in character, as do the natural sciences. Some of the 'internal' differences among the social sciences will be discussed in the following section; meanwhile in this section, attention will be directed at what the social sciences have in common, and what unites them together and renders them a distinct body of subjects as against the natural sciences.

It has already been stated that no analysis of the concept of explanation can be concerned merely with questions of form, to the exclusion of questions relating to substantive content; for questions of form only arise when one possesses some notion of what sort of subject-matter one is inquiring into. Since we have accepted that this is so, we are therefore duty bound to state openly what basic presuppositions need to be held if social scientific investigation is to make headway.

Whatever else may be their particular concerns, all the social sciences are involved, in some way or other, with explaining the actions of rationally purposive agents¹ pursuing their individual aims

1. It should be noted that if one is in a position to explain the action of an individual agent, then ipse facto one is able to explain the action of an institution. To do this, is simply to explain the actions of those individuals who are empowered or entitled to act in its name. It will be argued in chapter V that although institutions undeniably exist, they do not literally possess aims of their own. Since only individuals can be the operative causes of social change, all references to the aims of institutions are merely metaphorical ways of referring to the aims of the institutionalised individuals who represent or control the institutions in question.

within some societal or natural setting. The social sciences in dealing with deliberative social actors, have 'objects' of study which belong to a different order from the inanimate and inexorably determined phenomena which are the objects of study for the natural sciences; that is to say, all genuine social investigations are predicated on the fundamental presupposition that there is an absolutely unbridgeable chasm between the human and social sciences (which are concerned with meaningful actions and phenomena issuing from purposive agency) and the natural sciences (which are concerned with phenomena falling within the province of what is brought about wholly by nomic causation) . The numerous implications of this very crucial assertion will dominate not only the remainder of this chapter, but the entire discussion; the assertion will be defended in many places, and, in particular, the third section of the present chapter will argue that even those who overtly contend that things such as consciousness, self-interpretation and purposive agency are 'really' physical phenomena, cannot maintain this position with consistency when they themselves undertake first-order social inquiry.

Granted this fundamental conception of the nature of the phenomena studied by the various human and social disciplines, if what is

referred to is a freely initiated action, then it is appropriate to take the question 'Why did this occur?' as a request for a teleological explanation of some sort. (As used within the present discussion, the phrase 'teleological explanation' describes any explanation that makes reference to an end or goal which is sought or aimed at.) Since there are two ways (at least) of understanding the aforementioned question of inquiry, there are corresponding to these, two different kinds of teleological explanation that could be given as acceptable replies. Which of these two kinds should be given on any particular occasion, depends on which of the two interpretations the questioner has in mind.

The first sort of teleological explanation is given in circumstances when 'Why did this action take place?' is understood as meaning 'What aim or goal is the agent in question trying to achieve?', and in which the inquirer assumes it reasonable to hold that whatever the purpose may actually be, the agent considers that his action is necessary to bring it about. In such a situation, one successfully explains the action by informing the inquirer of the agent's reason for the performance of the action. (Here, the term 'reason' is used in the evaluatively neutral sense of 'the intention in the mind of the agent', and not in the justificatory sense of 'what the agent takes as his warrant or entitlement to act'¹.) What can therefore be called a

1. A reason in the sense of what entitles an agent to act may refer to one or other (or both together) of the following: (a) the agent's belief that the goal to which his action is directed is morally praiseworthy, or (b) the agent's belief that he is in possession of good evidence that his action will bring about his intended goal, whatever this may be. A reason in the justificatory sense of a warrant for an action presupposes the existence of a reason in the sense of an intention, but not vice versa.

'rational explanation' explains an action by revealing the reason (i.e. the intention) for which it was initiated, and hence to what goal it was purposively directed.

The second, and more commonly encountered kind of teleological explanation, is given when 'Why did this occur?' is taken not as requesting the aim of an action, but as seeking the answer to 'Why was this action initiated to achieve that particular goal?' . In this case, the explanatory reply should make it clear why the agent acted one way and not another to achieve his chosen goal. Such an explanation operates by showing that the action in question was the (or,

was an) ideally rational¹ thing for the agent to have done in the circumstances in which he found himself. One can call this kind of teleological explanation a 'rationality explanation'. The name is suitable because the general form of such an explanation employs a principle that treats agents as idealizations of rational men. The principle of rationality as employed by such an explanation, is the empirical assumption that a human agent will always choose the most technically efficient means (or, one of a number of equally efficient means) open to him in his situational circumstances to bring about his

1. To talk about the rationality of an action is to venture into an area bristling with philosophical problems. Does one mean the standards of rationality as conceived by the social scientist, or the standards of rationality as conceived by the acting agent in his own societal situation? In the social sciences of contemporary society (i.e. much of economics and sociology) this question does not pose a difficulty because both the social scientist and the agents in the society under investigation can easily reach agreement on what is a rational, and what is an irrational, choice of means to a given goal or hierarchy of given goals. However, this question has to be faced in a serious way by the social sciences dealing with primitive societies (i.e. social anthropology) and with societies of the past (i.e. history).

To deal with this problem one needs to distinguish the following: subjective rationality, ideal rationality and objective rationality. The first of these is possessed by a piece of behaviour if it contains any element (no matter how small) of intentionality, that is if it is planfully directed in some way (no matter how inappropriately) to the achievement of a chosen goal. The last of these three is what is rational according to the criteria of the observing social scientist. Ideal rationality, situated on a movable point on a continuous scale running between the other two, is attributed to an action if it constitutes a means that is thought to be objectively appropriate within the acting agent's own situational circumstances.

This topic will receive further treatment in chapter IV, section (iv).

desired end (or hierarchy of ends) .

The general schema for a rationality explanation is as follows:¹

(- Agent X aims to achieve goal G in)	
(situation S .)	
(- Agent X is ideally rational .)	
(- For an agent in X's situation (and)	<u>Explanans</u>
(with a full awareness of all the)	
(scientific knowledge in X's culture))	
(the/an appropriate means to adopt to)	
(achieve G, is course of action A .)	
	<hr style="width: 60%; margin: 0 auto;"/>		
	Agent X initiates action A)	<u>Explanandum</u>

One may wonder why here the explanandum is only 'informally inferable' rather than 'formally deducible' from the explanans. The reason is that one wants to be able to establish the truth of the explanans without having to refer to the occurrence of the explanandum-phenomenon, i.e. one wants the premises to be independently ascertainable. But if this condition is fulfilled, then it is a logical possibility (although not perhaps an empirical likelihood) that the explanans may be true and yet the explanandum false, because the agent may decide for no reason at all not to initiate the action in question. Since it is conceivable that an agent should so decide, unless one allows that it is possible for the truth of the explanans to be logically consistent with the falsity of the explanandum, the explanation harbours a petitio principii; part of the evidence that is used to establish the truth of the explanans is the occurrence of the explanandum-event itself.

1. The references and literature pertaining to this kind of explanatory structure (and its modifications) are to be found as footnotes in the second half of chapter IV .

Both rational and rationality explanations of actions involve reference to the intentions and intended goals of free agents. In this present analysis, the exposition of the way both these kinds of teleological explanation function, rests on the implicit suppositions that (i) an intention to achieve a certain goal is something quite distinct from the goal-directed action itself, and that (ii) it is possible to discover the details of an agent's intention on a specific occasion, without having to have knowledge of how he in fact does act at that time. A good way of investigating these two assumptions is to seek an answer to the question 'Are intentions (partial) causes¹ of actions, and can they be ascertained as such in particular instances?' . It should be obvious that an affirmative answer to this, can only be given if suppositions (i) and (ii) above are in fact correct.

The thesis that intentions can be, and indeed often are, causes of actions has been challenged by many philosophers, one of the best known and most recent of whom is Abraham I. Melden . He argues that the relationship between a motivating aim and an action, cannot be causal because one cannot describe the former in language which is conceptually unrelated to that which is used to describe the resultant

1. In the context of this question (because one is dealing with the realm of actions and not that of purely physical phenomena) one does not mean by the word 'cause', a 'neurophysiological process whose occurrence renders a set of material conditions sufficient to produce a reflex movement of the human body', but rather, a 'voluntarily entertained thought process that commences the (part mental, part physical) series of occurrences that terminates in the performance of a meaningful goal-directed piece of behaviour' . (The question of the meaningful import in actions will be discussed shortly in this section.)

action. Melden writes: "surely one lesson we can derive from a reading of Hume's discussion of causation (is that) the very notion of a causal sequence logically implies that cause and effect are intelligible without any logically internal relation of the one to the other"¹. Thus, according to Melden's line of argument, because an 'independent' description cannot be given of an intention on the one hand, and an action on the other, therefore the relationship between these sorts of things is logical or conceptual rather than causal.

The argument here is unsatisfactory on two counts. First, it is a plain fact that when an agent intends to bring about some goal, something is going on in his mind, namely, a conscious sustaining of his determination to obtain the chosen end; and it is this thought process, certainly something not identical with the resultant action, that one wants to say causes the 'aiming action' initiated as a means to the achievement of the desired goal. Secondly, Melden's analysis of what is necessary to constitute a causal connection is a strange one and against it one can endorse the criticisms of Donald Davidson . Davidson claims that Melden's argument fails because "events are often redescribed in terms of their causes"² (eg. 'he was injured because he was burnt') , and "to describe an event in terms of its cause is not to identify the event with its cause"³ . What events are causally connected is a question of what pertains in actuality;

1. A.I. Melden, Free Action, (Routledge and Kegan Paul, London, 1961, edit., 1967), p. 53 and p. 52
2. D. Davidson, 'Actions, Reasons and Causes' (1963), in A.R. White (ed), The Philosophy of Action, (Oxford University Press, Oxford, 1968), p. 86
3. Ibid., p. 89

how the events in question are identified and described is a matter of explanatory procedure. The status, however, of causal statements as synthetic or analytic depends on the second, and not on the first of these matters.¹ In other words, given a causal connection between two events, A and B, one could replace 'A', the original description of the antecedent event, by the description 'the cause of B'. If this were done, the statement 'the cause of B caused B' would be analytic, but from this it does not follow that the terms 'the cause of B' and 'B' are not understood to have separate referents between which a causal connection holds. Merely to point out that an intention and its resultant action are only describable in theory-loaded terminology is therefore no reason to think that they are not actually quite distinct things.

A different version of what is often called the 'logical connection argument' is advanced by Georg H. von Wright. Instead of following Melden in saying that an intention to do something cannot be the cause of an action because it cannot be described in words that are unrelated on the conceptual level, von Wright argues that an agent's intention is nothing over and above his 'aiming behaviour', because one cannot verify whether he has a certain intention apart from verifying that his action is in fact directed towards a certain goal.² For von Wright, to say that a person intends

1. D. Davidson, op. cit., p. 90

2. G.H. von Wright, Explanation and Understanding, (Routledge and Kegan Paul, London, 1971), p. 94ff

to do something is just a way of saying that a particular bodily movement of the individual in question is recognisable, by means of criteria governed by socio-cultural rules, as a piece of meaningful behaviour purposively aimed at the realisation of some goal.¹

It was stated as the first objection to Melden that it is simply false to identify an intention to bring about some end, with the subsequently initiated action. Nevertheless, even if this is accepted, one still has to face von Wright's contention that one cannot verify that a person possesses an intention apart from verifying that he does in fact act in a certain way.

But can one not directly ascertain an agent's intention just by asking him (or by inquiring of people who have asked him) ? This, surely, is the natural way to go about this. Von Wright however denies that this is a permissible method of establishing what an agent intends, for when one asks such a question the response is not just a set of sounds but a meaningful reply.² This cannot be taken to indicate an agent's intention, von Wright claims, because one has no way of checking that an agent means by his uttered sounds what one takes him to mean by them: "verbal behavior does not in principle afford more direct access to the inner states than any other intentional behavior"³.

Here, one is again faced with an exceedingly perplexing line of argument. It is of course true that an explanation of any kind of phenomenon whatever can be put into an infinite backward regress if the

1. G. H. von Wright, op. cit., p. 114ff

2. Ibid., p. 112f

3. Ibid., p. 113

inquirer so wishes. He can demand that one explain the elements of the original explanans by reference to further explanantia and so on without end. But, unless one is involved in discussion with a complete sceptic something substantial is always taken for granted, and one then explains the explanandum-phenomenon in question with reference to what the inquirer is prepared to accept without explanation. With human and social phenomena it is part of one's fundamental conception of the subject-matter, that one is concerned with meaningful phenomena, i.e. one supposes (virtually without question) that one is dealing with words and not mere sounds or ink scratches, goal-directed actions and not mere physical reflexes.¹ But once this is accepted as a presupposition (as it has to be if human and social inquiry is even to start), it is clear that one can ask an agent what goal he seeks to realise and then treat his reply as indeed providing a direct indication of the intention behind any subsequently initiated action. It is to be readily admitted that as a method of verification, this method does not and cannot give conclusive evidence (but where does this ever exist in empirical science?) because it is always conceivable that the agent is not telling the truth, or at the time of asking he may not be fully clear in his own mind as to what exactly he does want. But to say as much in no way appears to damage the claim that generally an agent's intention can be ascertained without having to witness the action initiated as a result of it.

1. This feature of human and social phenomena will receive extensive attention in section (iii) of this chapter.

Even if there is nothing philosophically objectionable in the thesis that intentions are causes of actions, when mention is made of 'human causation' (or 'social causation') what is usually meant, is not the relationship just discussed, but the relationship that holds between the environmental conditions (both physical and social-institutional) against which agents both develop intentions to achieve particular goals and also decide on the specific means of pursuing their chosen goals, and the actions subsequently initiated. In what ways is this form of causal connection similar to, and in what ways does it differ from, the sort of causal connection that holds between purely physical phenomena?

In both the physical and human realms, it is normally the case that the term 'cause' is used to refer to an event, an occurrence or a 'happening' of some kind. However, although this is undoubtedly what is generally meant, it is not correct to think that only 'happenings' can justifiably be called 'causes'. All explanations have to refer (explicitly or implicitly) to initial conditions, for without the presence of these the explanandum-phenomenon could not occur. Thus, provided that it is a relevant initial condition, a 'static' or unchanging state of affairs is just as much a cause of a particular phenomenon as the 'operative' event whose occurrence renders a set of circumstances sufficient to 'bring about' the effect in question.¹ Therefore, to give an example from the field of physical

1. This same point is to be found emphasised, in a slightly different context, in 'The Notion of an Historical Event' - a symposium by R. Gruner and W.H. Walsh, The Aristotelian Society, (Supplementary Volume XLIII, 1969), pp. 141-152 and pp. 153-164, respectively.

causation, the existence of oxygen in the air is, strictly speaking, a cause of a forest fire no less than, say, the operative event of the sun's rays shining through the glass of a broken bottle. And, similarly, to give an example from the area of human causation, the existence of the societal institution of language is, again strictly though pedantically speaking, a cause of an agent's action no less than, say, the 'happening' of another person's request that he do something.

It can thus be agreed that causation in the physical and human worlds is similar in that in both, the sufficient set of causes includes continuant states of affairs as well as operative events. In both areas, of course, when one picks out one particular element as the cause (i.e. the most interesting or important member, from one's own point of view, of the sufficient set of causes) this will be, more often than not, an operative occurrence rather than a static initial condition. But on occasions when a single event is so selected to be called the cause, one must not be led by the connotation of exclusivity associated with the use of the definite article to think that that one element on its own is causally sufficient to 'produce' the phenomenon in question.

Physical causation however differs in an extremely important way from human and social causation. In the physical world, given a causal law and the fulfillment of the appropriate initial conditions, the explanandum-phenomenon has to occur. One could say that it is 'compelled' to occur, or that it could not but occur; for it is a characteristic of physical causation that it involves 'necessity'. But it is precisely this feature of causation in the physical world that is lacking when environmental conditions (or particular changes in them)

cause agents to act in certain ways.¹ For this reason causing a natural event to take place (eg. causing a change in the volume of a gas by altering the temperature) is something rather different from causing the action of an agent to take place (eg. causing an individual to buy more of a good by increasing the purchase tax on a substitute good) . In the field of human and social affairs causal antecedents never 'necessitate' or 'compel' their consequents in this sense.

If one seeks the causes of particular actions, it must be recognised in advance that one is not attempting to explain the actions of 'abstract' or unsocialised agents. What one is anxious to uncover are the more or less immediate causes of the actions of individuals who possess minds that have been thoroughly moulded (not, of course, determined down to the last detail, since one allows that agents have a measure of autonomous free-will) by the social and institutional setting into which they were born and in which they continue to live. All forms of social inquiry, therefore, have to presuppose that the agents whose actions are under investigation have been affected (in ways whose details cannot be computed with precision) by some social environment.² If it is to be granted that one is dealing with socialised individuals acting within a set of societal conditions, one can then proceed to identify the kinds of factors that cause agents to pursue particular goals and to make particular choices regarding the

1. G.H. von Wright, op. cit., p. 69 and pp. 145-150

2. The denial that social inquiry has to presuppose socialised agents is the position known as 'methodological psychologism' . This will be examined and criticised in chapter V .

achievement of their goals. These factors consist of such things as the persuasion of others, the existence of opportunities for self-advancement, the normative pressures of a prevailing moral code, the threat of sanctions, the inducement of rewards or the fact that certain actions are just traditionally performed by occupants of particular institutional positions.¹ It needs to be emphasised again, that these kinds of factors, although they are correctly described as environmental causes of actions, do not 'force' an agent to act in the way that a billiard ball is 'forced' to move when struck by another. Social causes operate either by persuading an agent to pursue a particular objective, or by circumscribing the range of possible actions whose free initiation appears to an agent to be likely to result in the successful realisation of his desired goal.

When one person coerces another to do something (eg. when a criminal holds up a man at gunpoint and orders him to hand over his wallet) what is exemplified is not an instance of causal necessitation. If an individual is threatened with physical violence, he cannot be 'made' (in the sense of 'causally compelled') to act. He may of course decide to do what his assailant requires, but he will only do this if he thinks that the alternative (i.e. death or disablement) to doing what he is ordered to do, is not the preferable option. An agent, even when

1. There is an "enormous group of persons whose public actions are their own only in a significantly restricted sense ... (because they act) in situations where both the objectives to be pursued and the methods of pursuing them are largely determined for them" .

- W.H. Walsh, 'Pride, Shame and Responsibility', in The Philosophical Quarterly, Vol. 20, No. 78, January 1970, p. 7

under the most extreme pressure, can always in principle choose not to co-operate, and if he so chooses there is no way by which he can be 'forced' to act. If the limbs of an individual's body are physically moved against his will, the manipulator and not the agent performs an action. Thus, provided one is talking about the cause of an agent's action, what one takes to be the cause is only operative if it is mediated by the agent's own mind; unless an individual decides to do something, any movement of his body that may take place is not an action (and hence a fortiori cannot be a causally necessitated action) . In human affairs, environmental circumstances (physical, social or human) cannot 'compel' an action to take place; bodily behaviour that is causally necessitated by a physical stimulus (eg. the jerk of a leg after the knee-cap has been given a smart blow) is not an action at all, but a mere reflex.

On the topic of the distinction between human and physical causation, the sociologists Robert MacIver and Charles Page write:

"There is an essential difference, from the standpoint of causation, between a paper flying before the wind and a man flying from a pursuing crowd. The paper knows no fear and the wind no hate, but without fear and hate the man would not fly nor the crowd pursue. If we try to reduce fear to its bodily concomitants we merely substitute the concomitants for the reality experienced as fear. We denude the world of meanings for the sake of a theory, itself a false meaning which deprives us of all the rest. We can interpret experience only at the level of experience. Social changes are phenomena of human experience and in that sense meaningful." 1

In this citation the central point that is struggling to emerge is this: when particular environment conditions are identified as the cause of an

1. R.M. MacIver and C.H. Page, Society, (Macmillan, London, 1950, edit., 1964), p. 628 (authors' italics)

action, it is presupposed that it is possible to construct a teleological explanation to provide information on the intervening links in the chain of events that starts with the conditions and terminates with the action in question. In other words, the statement

'(the change in) conditions C caused action A'

presupposes a wider statement that provides either a rational or a rationality explanation of the action under consideration. That is, it presupposes, respectively, either

'in virtue of (the change in) conditions C, agent X adopted the intention to achieve goal G, and then tried to realise this by means of action A',

or

'in virtue of (the change in) conditions C, agent X decided that the appropriate way to achieve goal G was by means of action A'.

These last two formulations should make it clear that an environmental condition can only cause an action if the acting agent interprets it as meaningful¹, and hence relevant in some way as regards either his choice of goal or his selection of means to achieve some previously intended goal. This very point is to be found well emphasised in another work by MacIver, where he writes: "the chain of physical causation does not need mind except for its discovery. The chain of social causation needs mind for its existence."²

1. In the section immediately following, the important connection between social causation and the intrinsic meaningfulness of all human phenomena will be further discussed.

2. R.M. MacIver, Social Causation, (Ginn and Company, Boston, 1942), p. 263

On the occasions when a social scientist is confronted with a specific individual, he explains the agent's action by providing a teleological explanation of the sort required by the inquirer. When however the explanandum-phenomenon consists of (or is the social consequence of) the actions of a large number of anonymous but typically socialised individuals (eg. consumers, suppliers and investors who are understood to act within a certain sort of institutional setting), an explanation in causal form is a shortened and simplified substitute for an explanation overtly in teleological form. Suppose an inquirer wants an explanation of why the quantity demanded of a particular kind of commodity has increased, and in reply an economic theorist tells him that the phenomenon is caused by a price reduction.¹ This explanation makes no explicit mention of the aims and beliefs of the individuals whose actions are represented by the demand schedule of the kind of good in question. However, on investigation, the explanation will be found to rest on the supposition that if a commodity becomes cheaper in price relative to other goods, then normally this induces consumers to develop the intention to increase their consumption of it.

Although it is exceedingly common in the social sciences to find statements that deal explicitly only with the causal relations that hold between a number of theoretic 'variables' (eg. the level of demand, the level of supply and the market price) without making any reference to the conscious aims of individuals, if any such causal statement is fully unpacked a teleological explanation will emerge. For causal explanations which connect environmental conditions to actions,

1. See chapter II for an example of a fully laid out micro-economic explanation.

are based upon teleological explanations involving reference to the conscious purposes and aims of the agents who initiate the actions in question. And because of this, explanations that deal with phenomena which concern human and social causation, are justifiably considered to be of a different order from those that deal with phenomena in subject-matters where only physical causation prevails.

(iii) Scientism and Social Science

Up to now, as regards the field of human and social phenomena, attention has been directed at the proper way to formulate explanations of actions, those of either single agents or of a large number of individuals. It is however not true that the social sciences are concerned only with the explanation of actions. To a considerable extent (perhaps even for the most part), the social sciences are concerned to show why certain kinds of social interactions lead to the occurrence of certain sorts of social consequences. In many social investigations, it is simply assumed that agents have conscious aims and that they attempt to realise these rationally, while the main interest is concentrated on the question of whether the initiation of these actions in the given institutional context has any social consequences, and in particular whether it has any consequences that were not intended by the agents themselves. In general, therefore, when they are not involved with explaining actions, the social sciences are interested to ask and to answer the following question: 'From what purposive action, or set of interacting purposive actions, does this phenomenon arise as

the (intended or unintended) social consequence in these societal conditions?'¹ .

The precise way this question should be answered in practice depends upon what area of social phenomena is under investigation, and hence upon what specific social science is called upon to provide the desired explanation. Before one attempts to reveal the differences among the various social sciences, it will be helpful to consider what, if indeed anything, gives one the right to call the social sciences 'sciences' . To put this a little more exactly, is it the case that the study of men's actions, and the study of the general characteristics of the social order that result from the interaction of many men's actions, fall within the realm of what can legitimately be called 'scientific' inquiry?

In the English language the words 'science' and 'scientific' are profoundly ambiguous; happily however the ambiguity does not extend to the respective antonyms, namely, 'non-science' and 'non-scientific' on the one hand, and 'pseudo-science' and 'un-scientific' on the other. 'Science' in the first sense (i.e. contrasted with 'non-science'), means either a particular type of subject-matter or the use of a particular form of explanatory schema; 'science' in the second sense (i.e. contrasted with 'pseudo-science'), means a body of systematic knowledge (pertaining to any subject-matter whatever) that is gained by the use of a method of inquiry appropriate to the basic nature of the phenomena under investigation.

1. Chapters II and V of this discussion will elaborate and defend the methodological appropriateness of this general question of inquiry.

The first mentioned (and narrower) of these two senses has, on the whole, been favoured by 'ordinary' linguistic usage since about 1831 - the date the British Association for the Advancement of Science was founded.¹ In the popular mind, the term 'science' is used to denote either the physical and other natural sciences, or 'generalising' or theoretical disciplines. In the latter of these two cases, common usage follows the spirit of J.S. Mill's dictum: "Any facts are fitted, in themselves, to be a subject of science, which follow one another according to constant laws"². In contrast, the second mentioned (and, incidentally, the original and etymologically correct³) sense of 'science' has a wider meaning; provided a body of knowledge has been acquired by systematic application of a method of explanation appropriate to the subject-matter, it can be classed as 'science'. On the wider understanding of what constitutes science it is as unscientific to attempt to explain the actions of individuals by subsuming them under nomological laws, as it is to try to explain physical processes and phenomena by exhibiting their intentions and motives. Thus, it is as unscientific to fail to treat agents as agents, as it is to treat purely material objects as agents.

1. F.A. Hayek, The Counter-Revolution of Science, (The Free Press, Glencoe, Illinois, 1952, edit., 1964), p. 207
 2. J.S. Mill, *op. cit.*, p. 552 (i.e. Bk. VI, Ch. III, sec. 1)
 3. Robin G. Collingwood (1889-1943) reminds one that the use of the word 'science' in the narrow sense is "a slang usage, like that for which 'hall' means a music hall or 'pictures' moving pictures, (for) in the traditions of European speech the word 'science' means any organised body of knowledge" .
- R.G. Collingwood, The Idea of History, (Oxford University Press, Oxford, 1946, edit., 1966), p. 249

If one now has to make a choice on the matter, it is better to opt for the second rather than the first of these identified senses of the word 'science'. First, on the former sense the phrase 'social science' is fundamentally incongruous, while on the latter sense there is no awkwardness in holding that the social sciences are (or, more cautiously, can be made to be) genuine sciences. A second reason for deciding in favour of the wider meaning of the term 'science', is that its explication appears to relate closely to what Karl Popper and others have persistently argued, namely, that all scientific practice (social as well as natural) is united by a firm rejection of naive inductivism. No scientific inquiry proceeds by mechanically 'drawing out' explanatory accounts and theories from a 'theoretically uncontaminated' mass of collected facts, for the simple reason that whatever the area of investigation, there are just no such things as 'pure' or 'protocol' facts completely devoid of any theoretic import.¹ Even in order to ascertain the facts in terms of which the problem one is trying to solve is formulated, one must entertain (at the very minimum) some metaphysical view as to the fundamental nature of the phenomena before one. Since one never escapes the need to employ

1. K.R. Popper, The Logic of Scientific Discovery, op. cit., p. 59n et al.
- K.R. Popper, The Poverty of Historicism, op. cit., p. 121
- K.R. Popper, Conjectures and Refutations, (Routledge and Kegan Paul, London, 1963, third edit., 1969), p. 46 et al.
- N.R. Hanson, Patterns of Discovery, (Cambridge University Press, Cambridge, 1958, edit., 1961), especially Chapter I ('Observation') and Chapter II ('Facts'), pp. 4-30 and pp. 31-49, respectively.

ultimate presuppositions, is it not therefore natural to hold that scientific (as opposed to unscientific) forms of inquiry are characterised by the employment of appropriate presuppositions?

As well as classifying sciences as either natural or social, it is also open to one to categorise them as nomothetic or idiographic. In attempts to make clear the nature of this second division, it is often said that while nomothetic disciplines deal with kinds of events, idiographic sciences deal with singular or particular events. Although putting the matter this way is illuminating to a certain extent, it is also highly misleading; all sciences explain the occurrence of single events that are 'unique' in the sense that if all their properties (i.e. literally, all their spatial and temporal properties as well as all their substantial ones) were taken into account then they would each be found to be dissimilar in at least one discernible respect. To understand the difference between nomothetic and idiographic sciences it is necessary to realise that the phrase 'the occurrence of an event' may mean either,

- (a) the occurrence of a specific spatio-temporal phenomenon, or,
- (b) the occurrence of a specific spatio-temporal instance of a phenomenon of a certain kind .

All sciences are concerned to explain specific events. A science is nomothetic if, with reference to phenomena within its own domain of inquiry, it explains examples of (a) by showing that they are examples of (b), which are then explained (if physical events) by subsumption under a universal or probabilistic nomological law, or, (if social

events) by subsumption under a generalisation (deduced as a theorem from a social theory¹). A nomothetic discipline is therefore one that explains particular events by showing why they, qua events of a recurrent kind, occurred.² In contrast, a science is idiographic if

1. The term 'theory' is yet another ambiguous word that requires brief explication. First, there is the sense given it in ordinary speech (eg. as in 'this is pure theory, not proved fact') where the term refers to statements whose truth values have yet to be determined. In this sense also, in both the social and natural sciences, it means 'hypothesis' or 'conjectured system of causal connections'. Secondly, one has the sense where the word means 'principle of operative procedure', and this is then contrasted with actual working procedures. One would have something like this meaning in mind if, for example, one talked about what a piece of industrial relations legislation was supposed to lead to ('the theory'), and compared it to what one thought it actually would lead to ('the practice').

Thirdly, there is the important sense where 'theory' means 'formal theory'. Formal theories can be divided into two groups, the criteria for the division being the rigour or otherwise of the syntactic structure in which the empirical content of the theory is embedded.

(a) A rigorous formal theory consists of a small number of fundamental postulates from which one can deduce laws or generalisations as theorems. An example of such from the physical sciences, is the deductive system of statements that comprises the Newtonian theory of mechanics, while a social scientific example is micro-economic price theory. All syntactically rigorous formal theories, whether applicable to 'simple' or 'complex' phenomena (this distinction to be made a little further on in the main text), are amenable to mathematical exposition. (b) A less rigorous, or syntactically 'loose', type of formal theory, examples of which are Darwinian evolutionary theory and Freudian psycho-analytic theory, consists of a small number of related basic statements in terms of which 'explanations of the principle' upon which the phenomena in question appear, are given. It should be noted that while theories of sort (a) are found concerned with both 'simple' and 'complex' phenomena, theories of sort (b) are only found concerned with 'complex' phenomena.

An extensive analysis of the concept of a theory is given by:

- A. Kaplan, The Conduct of Inquiry, (Chandler, Scranton, Pennsylvania, 1964), pp. 294-310

2. Despite the etymological connection of 'nomothetic' with 'nomological', it is not satisfactory to define nomothetic disciplines as those that employ sui generis nomological laws. For if one says this, one is committed by a definition to the conclusion that no social science can be nomothetic. To take the term 'nomothetic' in the way suggested in the main text, allows for the possibility that disciplines that deal with phenomena not determined by nomic causation can be nomothetic sciences. Nevertheless, the linguistic disharmony is to be regretted.

it explains examples of (a) by showing that they were brought about by the occurrence of a unique configuration of specific elements interacting within a particular environment. If the idiographic science in question is a natural discipline, then each of these elements should be shown to be examples of (b) and then explained by the employment of a nomothetic natural science. If the idiographic science in question is a social science, then each of these interacting elements should either be explained teleologically, or be shown to be examples of (b) and then explained by the employment of a nomothetic social theory.

In the field of natural science, physics and chemistry are the outstanding nomothetic sciences, while the historical sciences of nature such as cosmology, geology, mineralogy, oceanography and physical geography are clear examples of idiographic disciplines. A geologist, for example, would not explain a specific mineral deposit by reference to a sui generis law of rock formation, but he would try to demonstrate how the particular situation before him resulted from a unique interaction of a constellation of single influences, each of which could be explained by the theoretical sciences of physics and chemistry. Certain natural sciences, such as zoology, are partly nomothetic and partly idiographic. A zoologist is a nomothetic scientist when he explains the homeostatic properties of an organism of a known species with reference to the functioning of its partial processes, but he is an idiographic scientist when he examines the ecological structure of a particular geographical region.

When one turns to the social sciences, history appears immediately as the very paradigm of an idiographic discipline. If an historian wants to explain, say, a revolution - that is a particular

revolution -- he does not explain it just in so far as it is a revolution (i.e. just in so far as it is the overthrow of a government by violent or other unconstitutional means) and not some other kind of event. He would try and explain why the specific event in question should possess all those properties he considers interesting and important, and not just why it possesses the aspect in virtue of which it is classified as a 'revolution'. Thus, if an historian were explaining the occurrences of the Russian Revolution of 1917, the Spanish Revolution of 1936, the Czechoslovakian Revolution of 1948 and the Greek Revolution of 1967, he would not do it by reference to a sui generis social 'law' concerning the occurrences of revolutions in general, in the way a chemist would explain four cases of the expansion of different volumes of gas with reference to the Boyle-Charles gas law. Instead, the historian would attempt to explain each of the above events by showing that they were intricate complexes of a large number of inter-connected elements, and that these complexes occurred within a particular space-time interval because the interacting agents concerned behaved in certain ways within the particular social environment prevailing at the time in question.

In addition to history, the idiographic social sciences include among their number the subjects of sociology, social anthropology, political science and their many variants. Human psychology is, perhaps, best regarded not as an idiographic science, but as one which hovers on the borderline between idiographic and nomothetic status. (Many of its practitioners claim that the sociology of contemporary society deserves a status similar to that accorded here

to psychology, but this claim brings in its wake strong and cogent opposition.¹⁾ However, whatever the correct classification for all the disciplines just mentioned should in fact be, at the present time the only social science that is incontestably nomothetic is economics. This discipline studies the actions by which scarce resources (both human and physical) are or might be allocated towards the alleviation of diverse and conflicting human needs and wants, and the direct societal consequences of the initiation of such actions²; hence, a phenomenon is described as 'economic' if it is involved in a process of valuation for the purposes of exchange. Because such valuations can be given in money terms (i.e. as market prices), the objects and activities upon which the market mechanism bestows a value can be measured quantitatively. Since economics is unique among the social sciences in having a notion of a precise respect in which its explananda-phenomena can be viewed as similar (economic phenomena are similar in that they are all possessors of some measure of monetary valuation), it is thus the only social science in a position to construct empirically applicable theoretical accounts of the ways the various types of phenomena it investigates are determined.

1. For example, supported by a battery of examples, Alfred R. Louch shows that to date all attempts to produce a nomothetic sociology have resulted in "triviality, redundancy and tautology".
- Explanation and Human Action, (Blackwell, Oxford, 1966), p. 9 and pp. 9-19
2. L. Robbins, An Essay on the Nature and Significance of Economic Science, (Macmillan, London, 1932), p. 15

That economics is a theoretical science is something that has long been recognised. In a well known passage in his The Scope and Method of Political Economy John Neville Keynes (1852-1949) urges his fellow economists not only to regard their subject as "a branch of ethics (seeking) to determine economic ideals" or "an art (seeking) to formulate economic precepts" , but also "to recognise as fundamental a positive science which is concerned purely with what is, and seeks to determine economic laws"¹ . For J.N. Keynes , this positive science constituted the "fundamental" part of his social

1. J.N. Keynes, The Scope and Method of Political Economy, (Macmillan, London, 1891), p. 36

science, and indeed he held that "the conception of political economy as a theoretical, abstract and deductive science"¹ was how the subject should be spoken of "in the main"². But granted that economics does employ (and has employed for a considerable time) formal theories, this is not sufficient to ensure that it is able to produce explanations of specific events equal in detail and precision to those produced by, say, Newtonian mechanics.

It will be argued at length in the third chapter of this discussion, that there is a third way of viewing scientific disciplines, namely, as those that deal with 'simple' and those that deal with 'complex' subject-matters. A 'simple' science (dealing with a 'simple' subject-matter) is one that is concerned to explain phenomena brought about by a small number of kinds of causal variables, while a 'complex' science (dealing with a 'complex' subject-matter) is one that is concerned to explain phenomena caused by a large number of different kinds of factors and influences. Although economics is a syntactically 'rigorous' nomothetic science (i.e. one that explains events of recurrent kinds by subsuming them under generalisations derived as theorems from a small set of theoretic postulates), to be such it has to limit itself to giving explanations that involve only the few most important kinds of determining causal factors at work in its domain of inquiry, because this is an area of complex phenomena. Thus, while both physics and economic theory employ rigorous mathematical formalisms, the

1. J.N. Keynes, op. cit., p. 9

2. Ibid., p. 15

former (as a simple discipline) is able to show in detail why a specific event of a particular sort occurs, but the latter (as a complex discipline) is able just to outline the principle upon which a certain kind of event occurs. In Friedrich A. Hayek's language, physics can provide 'explanations of detail', but economics can only give 'explanations of the principle'¹.

No science (whether it be natural or social, nomothetic or idiographic, simple or complex) is able to deal with all the properties of its explananda-phenomena, although clearly an idiographic science will concern itself with a substantially larger number of the properties of a specific event than will a nomothetic science. Since, needless to say, all sciences investigate only those selected aspects of particular phenomena that are deemed to be of general interest or are relevant as far as the explanation of their occurrences are concerned, all sciences must employ 'principles of identity' that provide inter-subjectively ascertainable criteria for calling specific phenomena the 'same'² as other specific phenomena. As a direct consequence of the differences

1. An analysis of F.A. Hayek's theory of explanation in complex phenomena, will be given in chapter III .
2. In order to avoid any possible confusion, it is best to state explicitly that the term 'same' is ambiguous: it has a 'qualitative' and a 'numerical' sense. In the former sense, when the word is applied to two distinct phenomena, it means 'similar in a certain respect' (and when applied to one phenomenon at different times it means 'unchanged in a certain respect over the period in question'). In the latter sense, when the word is applied to two (or more) purportedly different phenomena (eg. the Morning Star and the Evening Star) what is meant is that the two (or more) phenomena are in actuality just a single phenomenon (eg. the planet Venus). It should be obvious that in this discussion, the word 'same' is intended in the qualitative sense.

already mentioned between the subject-matters of the social and natural sciences, the social sciences require kinds of 'principles of identity' (to be employed with reference to the 'elements' whose descriptive terms enter into the explanatory accounts offered by these disciplines) of a quite diverse character from those used by the natural sciences.

The precise nature of the difference between social and natural scientific 'principles of identity' has been succinctly expressed by Peter Winch . (To cite Winch with approval on this particular point, does not commit one to upholding in its entirety his

general thesis¹ as to what properly constitutes social scientific activity.) In the third and fourth chapters of his controversial monograph, Winch makes much of the fact that "two things may be called 'the same' or 'different' only with reference to a set of criteria which lay down what is to be regarded as a relevant difference"².

1. P. Winch, The Idea of a Social Science and its Relation to Philosophy, (Routledge and Kegan Paul, London, 1958, edit., 1970) .

Winch's general aim (to which one can give considerable support) is to attack the notion that in the social sciences "we must follow the methods of natural science if we are to make any significant progress" . (p. 1) Winch argues that while natural science is characterised by the method of explanation that subsumes events under nomological laws, the social sciences cannot (and therefore should not try to) be concerned to seek causal connections. His overall thesis amounts to the position that there is a sharp contrast to be made between (a) causally determined events, and (b) rule-governed or rule-guided actions (pp. 51-52), and that social inquiry, per definitionem, is properly concerned just with (b) and not with (a). He argues thus: "to discover the motives of a puzzling action is to increase our understanding of that action; that is what 'understanding' means as applied to human behaviour". (p. 78, Winch's italics) And since "the notion of meaningful behaviour is closely associated with notions like motive and reason" (p. 45, Winch's italics), to learn "what a motive is belongs to learning the standards governing life in the society in which one lives" . (p. 83)

There is much in Winch that one can find to agree with. In criticism, however, two points are here in order: (1) the social sciences are not concerned simply to elucidate the meaningfulness of the actions of single individuals, but (presupposing actions to be meaningful) are concerned to explain social phenomena produced, intentionally or unintentionally, by the interaction of a large number of purposive actions, and (2) it is perfectly legitimate to claim that the social sciences deal with 'causally determined events' so long as one bears in mind that one is referring to instances of social and not physical causation.

An excellent, but little known, critique of Winch that both praises and takes him to task at the correct points, is :

- Karl-Otto Apel, Analytical Philosophy of Language and the Geisteswissenschaften, Foundations of Language, Supplementary Series, Volume 4, (D. Reidel Publishing Company, Dordrecht, Holland, 1967), pp. 38-57

2. P. Winch, op. cit., p. 103

Winch then proceeds to argue that the relation of a social scientist to his subject-matter is not at all like the relation of a natural scientist to his subject-matter. For the latter, because "the 'things' in question are purely physical the criteria (of identity) appealed to will of course be those of the observer"¹. However, for the social scientist, the subject-matter consists of human actions and the societal repercussions of the interaction of many actions. But because the operative causes of such events are conscious agents, they themselves entertain views, and formulate interpretations, of their own actions and of social phenomena that appear within the bounds of their own society. Therefore, if a social scientist is to treat such events as social phenomena (as, ex hypothesi, he must) then he :

".... has to take seriously the criteria which are applied for distinguishing 'different' kinds of actions and identifying the 'same' kinds of actions within the way of life he is studying. It is not open to him arbitrarily to impose his own standards from without. In so far as he does so, the events he is studying lose altogether their character as social events." ²

For a natural scientist, what are to count as his 'facts', are determined by prevailing theoretic considerations which lay down the physical criteria that are to be adopted in deciding what phenomena are to be taken as 'similar' and what as 'different'. But for a social scientist, 'principles of identity' can employ neither physical criteria nor any criteria (even if non-physical) that are simply those

1. P. Winch, op. cit., p. 108

2. Ibid., p. 108 (Winch's italics)

of the social inquirer's own choosing; the identification of the objects of the social sciences requires reference to the conscious intentions and interpretative ideas of those actually living in the society under investigation. Although social phenomena possess physical properties, they are not of relevance to social science in so far as they are physical, but only in so far as they are meaningful to the agents in whose society they occur.

The denial of the view that the social sciences, prior to any attempt to provide explanations, have to recognise in what way societal phenomena are understood as meaningful by those living in the society under study, is the position F.A. Hayek labels "scientism"¹. Before this is discussed further, let it be stressed yet again that in the social sciences what is important as regards the causation of actions and other societal phenomena, is not what the causes (whether these be actions or prevailing environmental conditions) are in physical terms, but what the acting agents think they are; also, one is not primarily interested in the physical characteristics of the explananda-phenomena in question, but rather in their intentional or ideational properties.

In direct opposition to the methodological procedures advocated by a scientific approach, Hayek argues that any truly effective attack by a social science upon a particular social problem, has to proceed by first recognising that human and social phenomena cannot even be identified, let alone explained, in either purely physical terms or in terms that are not intimately connected with the way the

1. F.A. Hayek, The Counter-Revolution of Science, op. cit., p. 15

agents concerned happen to think about them. Take, for example, concepts such as 'sale', 'purchase', 'crime', 'punishment', 'tool', 'instrument', 'word', 'sentence' or 'book'; it is indeed the case that "careful logical analysis of these concepts will show that they all express relationships between at least three terms, of which one is the acting or thinking person, the other some desired or imagined effect, and the third a thing in the ordinary sense"¹. It is in fact not possible to explicate social concepts "without using some terms such as 'suitable for' or 'intended for' or some other expression referring to the mental attitudes of men towards the things"² the concepts are used to denote. It thus follows that one is quite unable to "distinguish in physical terms whether two men barter or exchange or whether they are playing some game or performing some religious ritual"³, and therefore unless one "can understand what the acting people mean by their actions any attempt to explain them is bound to fail"⁴.

Since only what agents know or believe can shape intentions within the minds of human beings, an agent's action is not caused by the physical qualities of the actions, institutions and material objects comprising his environment, but by what appear to him to be the substantial properties of what he takes to be his environment. When, however, a social inquirer such as an ancient historian or an anthropologist desires to explain an action (or a social phenomenon

1. F.A. Hayek, The Counter-Revolution of Science, op. cit., p. 27

2. Ibid., p. 27

3. Ibid., p. 31

4. Ibid., p. 31 (italics added)

that results from the interaction of actions) within a society in which live individuals whose conceptualisation of the 'nature of the world' is very different from his own, it is often not an easy business to ascertain the ideational criteria by which the societal phenomena are to be identified in that society. Nevertheless, since (one assumes) the vast majority of human beings possess minds that are structurally the same and differ only in specific content, if the investigating social scientist perseveres long enough with his inquiry he will in time just come to understand the 'world outlook' of the agents whose general conceptual scheme differs so much from his own. It is worth noting that this particular matter does not constitute a problem (acute or otherwise) for all social scientists. For some, notably those economists and sociologists involved with questions relating to contemporary society, the 'principles of identity' which they employ in their non-professional moments are more or less the same as those used by the agents in the society whose features they are concerned, qua social scientists, to explain. Such social scientists are therefore most unlikely to encounter a conceptual clash between how they view the society in question, and how the acting agents see it.

An economist (or any other investigator of contemporary society) must not delude himself that he is a species of physical scientist just because he, unlike the historian or social anthropologist, does not have to employ a system of concepts for identifying social phenomena that is different from the one he is accustomed to use in his own private life. For a social scientist in this position the 'understanding' is merely presupposed, but the need for it does not thereby vanish. One can cite Hayek again: "that the

objects of economic activity cannot be defined in(material) terms but only with reference to a human purpose goes without saying. Neither a 'commodity' or an 'economic good', nor 'food' or 'money', can be defined in physical terms but only in terms of views people hold about things."¹. Thus, even though a social scientist may not have to undertake a conscious inquiry into the details of the meaningfulness of the phenomena he is concerned with, it still remains true that all social explanation is explanation of meaningful phenomena. This happens to be the case even if the precise nature of the 'meaning' is unproblematic and is thus either unrecognised or else implicitly taken for granted.

The position that denies that it is scientifically appropriate for explanatory purposes to treat human and social phenomena as meaningful, or to look upon them as different in any way from the sort of phenomena with which the physical and natural sciences deal, has its ardent advocates. One of the foremost² adherents to the 'physicalist' approach to social questions, is the sociologist George A. Lundberg. Despite the fact that Lundberg's methodological doctrines are obviously dated by their close attachment to inter-war positivism (they have been aptly described not only as "scientistic" but as "the 'last word' in simplism of this species"³), they are well worth examining as a clear articulation of a widely prevalent ethos.

1. F.A. Hayek, The Counter-Revolution of Science, op. cit., p. 31
2. A. Inkeles, What is Sociology?, (Prentice-Hall, Englewood Cliffs, N.J., 1964), pp. 39-42
3. F.H. Knight, On the History and Method of Economics, (The University of Chicago Press, Chicago, 1956, edit., 1963), p. 227

For although Lundberg as such may appear preposterous (even 'as dead as the dodo', as far as current first-order historical and sociological practice is concerned) , in some form or other the basic metaphysical tenets upon which he seeks to operate, pervade, and are deeply ingrained in, much of present day thought, especially popular thought.

Lundberg is highly contemptuous of MacIver's assertion (quoted earlier) that a fundamentally different kind of causation is exemplified when a piece of paper is blown by the wind and when a man is driven by fear to flee before a pursuing crowd. He considers that a scientific inquiry must recognise that "the principle of parsimony requires that we seek to bring into the same framework the explanation of all flying objects"¹ . Although, Lundberg readily admits, the idea that the same general laws may be applicable to both physical and social phenomena "may seem fantastic and inconceivable to many people"² , from the scientific point of view:

" a paper flying before the wind is interpreted as the behavior of an object of specified characteristics reacting to a stimulus of specified characteristics within a specified field of force. Within this framework we describe the man and the crowd, the paper and the wind. The characteristics of these elements, and they may be specified to any degree desired, would never be the same in any two cases of wind and paper or of men and crowds. But it is the faith of science that sufficiently general principles can be found to cover all these situations" ³

A little further on, Lundberg claims that as far as the explanatory procedures generally adopted by the social sciences are concerned, "the

1. G.A. Lundberg, Foundations of Sociology, (Macmillan, New York, 1939), p. 13 (italics added)
2. Ibid., p. 13
3. Ibid., p. 14 (Lundberg's italics)

error lies in assuming that the telic character or purposiveness which we like to attribute to societal behavior is an intrinsic character of the behavior rather than our way of describing it"¹. Thus, in short, Lundberg argues² that because the physical sciences made great advances when they abandoned the teleological form of explanation, it is simply the retention of this type of explanation that is preventing a similar advance within the social sciences. And therefore, Lundberg considers, it is very likely that the "next great developments in the social sciences will come not from professed social scientists but from people trained in other fields"³.

One can usefully begin one's critique of this scientific polemic, by remarking that the 'telic character' of human behaviour is not merely a property 'attributed' just by a 'way of describing it'. The truth of the matter - something surely less open to doubt than the fact that the Earth is not a saucer carried on the backs of four white elephants - is that men possess conscious minds and are capable of rational action. The 'purposiveness' of human behaviour is therefore an inherent and not an imputed characteristic. Secondly, it can be said that the positive explanatory procedure Lundberg suggests borders on the philosophically naive. If men's actions are to be explained as purely a 'response' to an environmental 'stimulus', then the question has to be raised of what kind of 'principle of identity' is to be employed in deciding what are to constitute the elements that are to be connected, namely the 'response' and the

1. G.A. Lundberg, op. cit., p. 21

2. Ibid., p. 8

3. Ibid., p. 13



'stimulus' .

If one is intent on employing a consistent physicalist method, then it is permissible only to use 'principles of identity' that rely on purely physical criteria. Instead therefore of concerning oneself with actions, institutions and other social phenomena, one is entitled, logically, to deal just with constellations of physical particles and to explain the movements of such configurations as causally 'necessitated' locomotion within a series of time co-ordinates. But this is exactly what Lundberg fails to do when he turns his mind to first-order social problems. In, for example, Chapters XI and XII (treating, respectively, the demographic characteristics of populations, and the spatial aspects of society) he discusses numerous topics including the various death rates¹ (due, among other factors, to accidents, suicides and homicides) , the rates for different sorts of crimes² , the rates for school truancy³ and the distribution of those receiving public relief from state funds⁴ . But phenomena of these kinds, as has been previously argued at length, cannot be identified by purely physical criteria, but only by reference to what the agents living in the situation under observation happen to think.

Although Lundberg explicitly vows to employ a physicalist explanatory procedure, by resorting to the use of 'principles of identity' that employ ideational criteria, he shows that he is in fact unwittingly taking for granted the intrinsic meaningfulness of his

1. G.A. Lundberg, op. cit., p. 442

2. Ibid., p. 483

3. Ibid., p. 484

4. Ibid., p. 486

explananda-phenomena. But to do this is implicitly to presuppose that the individuals in the society under investigation are purposive agents, and are therefore distinct (in this very notable respect) from the entirely 'inert' physical processes studied by the natural sciences. Thus, extraordinary to relate, Lundberg ends up affirming by his actual practice, a conception of the nature of social phenomena which he rejects when he engages in philosophical discussion on the explanatory procedures he thinks he uses in his own first-order investigations.

In a single sentence, what is it that differentiates the subject-matters of the social, from those of the natural sciences? The reply is that men, quite unlike physical phenomena, are rationally purposive agents and are moved to act because their minds interpret the behaviour of other men and their own environmental conditions in certain ways, and that hence their actions and the features of their various social orders, cannot be explained within the natural scientific framework of nomic causation. As has just been observed in the case of Lundberg, social scientists who cannot bring themselves to accept explicitly that there is this difference between the human and physical realms, nevertheless undertake their own concrete investigations on the presupposition that men are indeed deliberative, self-interpreting creatures. The reason why, despite any methodological protestations to the contrary, social scientists operate in this way is that in order to obtain any increment to social knowledge, the methods of investigation and procedures of explanation have to be appropriate. The penalty for methodological dogmatism (i.e. an unbending determination to apply some inflexible explanatory schema before one has contemplated whether or not it is appropriate to the type of phenomena under scrutiny) in

the social realm (or indeed anywhere), is a failure to achieve any pertinent and interesting results. Hence, it can be said that in the social sciences (as with other sciences) the subject-matter, in a metaphorical sense, 'forces' those inquiring into its features to treat it in a way that is in accordance with its basic nature. And granted this, it is thus understandable that those who explicitly advocate scientific procedures, do not put them fully into practice. It is also understandable, albeit very unfortunate, that when the disciples of scientism violate their own professed precepts in this fashion, any concrete results thereby obtained tend to be credited not to the methods of inquiry they in fact used, but to those they thought they used.

Since to be fruitful in social inquiry, one has to regard agents as agents (and not simply as non-conscious, non-purposive physical objects) and social phenomena as social phenomena (and not purely as electron-proton configurations), is it not more sensible to display openly the required metaphysical presuppositions, than to ridicule these and then to pretend that they can be successfully suppressed? Whatever may be achieved by this latter course of action, one can be certain that it will not be the avowed goal of making the social sciences more and more scientific by the progressive elimination of obscurantist features. For scientism itself leads to a form of unscientific obscurantism more penetrating (and far harder to dislodge) than any it may happen to replace. "The scientific as distinguished from the scientific view is not an unprejudiced but a very prejudiced

approach which, before it has considered its subject, claims to know what is the most appropriate way of investigating it." ¹

1. F.A. Hayek, The Counter-Revolution of Science, op. cit., p. 16

II PURPOSIVE ACTIONS AND RESULTANT SOCIETAL PHENOMENA

(i) Intended and Unintended Consequences

The general aim of the social sciences is both to account for the actions of specific individuals and institutions, and to trace the overall consequences for society that are produced by particular actions or the intersection of sets of purposive actions. As far as economics is concerned, one is interested, more narrowly, in devising formal theories which will enable one to demonstrate what social consequences will result when individuals (and households, firms and other institutions), operating within a particular type of societal framework, consciously attempt to satisfy their needs and wants by the allocation of scarce resources that have a number of competing potential uses. Economic theory assumes that individual agents and institutions each possess a specifiable hierarchy of conscious goals, and then attempts to work out the actual societal consequences of their rational pursuit. But how is the term 'actual', as used here, to be explicated?

Karl Popper asserts that "the main task of the theoretical social sciences is to trace the unintended social repercussions of intentional human actions"¹; and, in a similar vein, F.A. Hayek claims that the aim of the social sciences "is to explain the unintended or undesigned results of the actions of many men"². Why is it, one immediately wants to ask, that these two authorities appear to place all emphasis on the unintended, rather than, as seems strictly more accurate, the intended or unintended social consequences of men's actions? Since it is hardly plausible to maintain that no social phenomena occur more or less exactly as deliberately sought by the acting agent or agents, it would be quite arbitrary to lay it down that intended social phenomena do not come within the province of the social sciences. One should however beware of making too much of the wording of citations when removed from the context in which they are to be found. That Popper and Hayek in their extensive writings frequently omit to mention the intended consequences of actions, one must take to be their way of stressing that (owing to the immense intricacy and complexity of the inter-connections within the social world) the unintended rather than the intended societal consequences of human activities bulk larger among the phenomena worth investigating. And that also, it is the unintended rather than the directly intended results of action, which do not admit of an easy or obvious explanation but can only be accounted for with the aid of a penetrating

1. K.R. Popper, Conjectures and Refutations, op. cit., p. 342 et passim (Popper's italics)
2. F.A. Hayek, The Counter-Revolution of Science, op. cit., p. 25 et passim

theoretical analysis.

One of the great virtues of recommending that the social sciences should be constantly on the look-out for unintended social consequences, is that this acts as a firm bulwark against the opposing position that holds that every (or, perhaps, less strongly, every important) social phenomenon can be explained as the intended result of the action of some person, group or institution that consciously brought it about in order to achieve some desired goal. It is possible to identify at least two different forms, one far less subtle than the other, of the view that this is the basic mould into which all social explanations should be pressed. The crude variant is customarily called the 'conspiracy theory of society' ; this will be examined in just a moment. The more sophisticated version of this general thesis is often called the 'design theory of institutions' . This theory is characterised not so much by an uncompromising denial that unintended consequences of actions exist (although some expositions may involve just this), but rather the assertion that even if such phenomena do occur, they are of but little use for serving vital human purposes, and hence it is always rational to replace spontaneously grown systems of social order with societal structures that have been consciously planned to fulfil some specific function. But more will be heard of the design theory later.

For advocates of the conspiracy theory, the explanatory task of the social scientist is similar to that of the police detective. He has to regard every undesirable social event (i.e. every social event that is disliked and is thus unwanted by the majority of those living in the society in question) as caused by a criminal action, and his duty is

to find and expose the culprit responsible. If, on the other hand, the explanandum-phenomenon is socially desirable, the duty of the social scientist is equally clear. On such occasions he has to make sure that members of society fully understand and appreciate that the occurrence of the event is due to the beneficence of the government, dominant political party or whatever institution, group or individual he thinks deserves to be awarded the credit. As can be discerned, one of the main features of the conspiracy approach is its low level of theoretic import; whether an event is deemed desirable or undesirable, an explanation of its occurrence is held to be very simply obtained. All one has to do is to scrutinise the event's prominent attributes, and then discover who would want, welcome or benefit by its occurrence, and who therefore is most likely to have engineered it by direct design; for conspiracy theorists would argue that since whatever happens in the social world comes about as the result of someone's plan, once a person (or group) has been found who benefits from the event in question, who else but that person (or group) would have brought it about?

The example that Popper gives of a thoroughly disreputable conspiracy theory¹ is the one propagated by Josef Goebbels (1897-1945) on behalf of the German Nazi party. Goebbels

1. K.R. Popper, The Open Society and its Enemies, Vol. II, op. cit., p. 94ff

K.R. Popper, Conjectures and Refutations, op. cit., p. 123ff , p. 341ff

(Popper never claims that political or social conspiracies are not hatched. Such things obviously occur, but the important point to grasp is that as soon as conspirators start to transform their plans into action, this is liable to bring about repercussions they had not intended. These unintended results may prevent the consummation of the conspiracy, or ensure that the final state of affairs is far different from the one originally aimed at.)

believed he could prove that the defeat of Imperial Germany in 1918 and the subsequent economic depression, were misfortunes resulting directly from the calculations of International Zionist Capitalism, an allegedly powerful world-wide movement led by a mysterious group of individuals, known collectively as 'The Learned Elders of Zion'. This theory of the Zionist plot is, of course, nothing but a tissue of fabrications.¹ Nevertheless, it remains, for the record, not only the most notorious of all conspiracy theories, but also, not unsurprisingly, the most extreme. However, when a critique is directed against the version most marked by excess, there is always the possibility that the doctrine in question could be substantially rehabilitated in a weaker form that manages to escape some, if not most, of the original criticism.

1. An historical note.

The Protocols of the Learned Elders of Zion was a document, published in Tsarist Russia in 1903, which contained the purported minutes of twenty-four meetings said to have been held in Basle in 1897, the time of the first Zionist Congress. At these meetings, it was alleged, plans were made to overthrow all existing political order and then to establish a world government under Zionist control.

The utterly spurious character of The Protocols was clearly established in 1921 by Philip Graves, Constantinople correspondent of The Times. He demonstrated that the document was a clever forgery, compounded by the Russian secret police from two main sources: (i) a satire by Maurice Joly on Napoleon III, published in 1864 and entitled Dialogues aux enfers entre Machiavel et Montesquieu, and (ii) a weird novel, Biarritz (1868), by Hermann Goedsche. More recently, John S. Curtis, supported by a committee of prominent U.S. historians, published An Appraisal of the Protocols of Zion (1942), in which the document was subject to a thorough and critical analysis, and pronounced to have no claim whatsoever to authenticity.

For further details of this and other theories relating to the supposed Zionist world conspiracy, see :

- K.S. Pinson, 'Anti-Semitism', in The Encyclopedia Britannica, Vol. 2, (Encyclopedia Britannica, Inc., Chicago, edit., 1968), pp. 81-90

One must therefore ask whether showering scorn on the notion of a Zionist world conspiracy, is indeed sufficient to discredit entirely all formulations of all types of conspiracy theory. In particular, does this manoeuvre overthrow a re-interpretation of the general conspiracy thesis which advances not an all-embracing explanatory claim, but instead a partially normative claim that for every social phenomenon, someone (or some institution) can be found who is responsible for it (whether or not that agent (or institution) did in fact plan and/or cause it) ? To answer this question, one needs to examine more closely what is involved in calling a social phenomenon an 'unintended consequence' .

This matter can be usefully approached through the methodological investigations of the sociologist Robert K. Merton into the appropriate manner for sociologists and social anthropologists to explain the continuation or 'homeostatic' survival of whole societies or cultures. Merton suggests (quite acceptably) that when social anthropologists wish to account for the continued survival of a society, or the survival of a substantial aspect of a society, they ought to produce statements that describe: (i) the social processes or events which are 'functional' , i.e. the processes which constitute the (homeostatic) mechanism by which the culture in question remains intact, and (ii) the habitual and repetitive activities (eg. the continual practice of certain customs, rituals, magical acts etc.) which bring

about the said functional consequences¹. Such repercussions of the initiation of standardised actions are known as either 'manifest' or 'latent' functions; the former are "those objective consequences contributing to the adjustment or adaptation of the system which are intended and recognised by participants in the system"², while the latter, correlatively, are "those (societal consequences) which are neither intended nor recognised"³.

Merton's characterisation of latent functions as phenomena that are the unintended and unrecognised functional consequences of institutional activities, suggests strongly that it would be illuminating

1. It is very important to see that here the explananda-phenomena are the consequences which result from the practice of certain customs or rituals, and not these activities themselves, the occurrences of which are to be explained teleologically, i.e. with reference to the reasons, aims and professed goals of the agents who participate in the actions in question. At this point in the discussion the methodological propriety of what can be called 'strong' functionalism (which purports to explain the occurrence of institutional activities just by pointing to the fact that their societal consequences are functional) cannot be considered. The matter will however be taken up in chapter V - 'Individualism, Holism and Functionalism'.

2. R.K. Merton, Social Theory and Social Structure, (The Free Press, New York, 1949, 1968 enlarged edition), p. 105

3. *Ibid.*, p. 105

to set out a matrix to illustrate four different kinds of combinations.

Thus:

	Recognised	Unrecognised
Intended	a	b
Unintended	c	d

At once one can dismiss the possibility of the appearance of phenomena of type (b) - it is surely a conceptual absurdity to consider that any phenomenon could be intended and yet unrecognised. Also, there is no need to be concerned with phenomena of kind (a), for, in the context of the moment, these are quite unproblematic. It is however necessary to distinguish between those social phenomena that are both unintended and unforeseen, and those that are unintended but nevertheless foreseen.

A phenomenon is unintended simpliciter, if it is not deliberately sought, i.e. if it is additional to, or differs noticeably from, the result the actions were consciously initiated to realise. If the agents concerned were ignorant of the fact that their attempt to achieve a certain goal was liable to produce a different result or to cause other phenomena as well, then each of these would be correctly described as a consequence of sort (d) . However, if the occurrence of a particular unintended phenomenon did not come as a surprise to the initiators of the causative actions in question, then this would be an instance of a phenomenon of sort (c) .

Much of the practical value of social scientific inquiry, especially of economics, lies in the fact that once a social phenomenon has been successfully explained, society is then provided with the requisite knowledge either (if the phenomenon is deemed undesirable) to

reform the social conditions in such a way as to prevent its appearance, or (if the phenomenon is held to be desirable) to maintain those aspects of the societal structure that are necessary for its continued persistence. This sort of knowledge is particularly valuable when it enables governments to see (and thus, hopefully, to prepare their electorates to accept!) that the simultaneous conscious pursuit of a number of different goals may be empirically impossible within the prevailing social conditions, because the unintended consequence of the pursuit of one of them conflicts with the (full) achievement of one or more of the others. Now, granted all this, the door is open for looking anew at the worth of the conspiracy theory of society, when modified as a theory dealing with the allocation of responsibility for the occurrence of societal phenomena.

If a man intends to bring about some event, recognises that any action of his to this end will produce an unintended consequence in addition, and yet still decides to proceed with his action, then clearly he is responsible for the occurrence of that unintended result. A similar line of reasoning applies as far as governmental (or institutional) responsibility is concerned. If a government attempts to achieve something, foresees that this will bring about something else, and if that event then occurs as a direct result of the action to achieve the intended goal, then, although the phenomenon is unintended, the government is to be blamed (or praised, as the case may be) . Also, if a number of private individuals act together in a certain way, and if the government recognises that this form of activity will produce a (intended or unintended) socially detrimental effect, it is rightly to

be held responsible, if it neither forcibly prevents the actions altogether, nor tries to alter the relevant social conditions with the aim of preventing the unwanted phenomenon. When a government could have acted to prevent a disaster and yet did not, or when it itself acted in knowledge that the undesirable unintended consequence that resulted from its own actions was very likely to have occurred, is it all that mistaken to regard the government in the light one would if it had intended to inflict the unwanted phenomenon upon society at large ?

Here, one is entitled to hesitate to give a definite answer. What one can say on the general level, is that the growth of social scientific knowledge tends to widen the area for which responsibility can be assigned; for if (say) an institution has the requisite knowledge and authority to influence social events for the better or for the worse, then it cannot avoid some responsibility for what happens within society. This claim is phrased in cautious terms, because it is a grave mistake to think that perfect knowledge of all the detailed causes of social phenomena can ever be gained. Within fields of complex order, only 'explanations of the principle' on which its constituent phenomena appear, are obtainable.¹

However, whatever may be one's final verdict on the value of the normative variant of the conspiracy theory, one can agree, to return once more to the explanatory version, that while one may not be able to

1. As stated before, a critical examination of F.A. Hayek's conception of 'explanation of the principle' in complex phenomena, will be given in chapter III .

disprove formally¹ the statement 'every social phenomenon is the intended result of some consciously premeditated plan', one is quite unable to accept it as true. Actual social scientific practice 'forces' one to accept as a basic truth concerning the nature of the subject-matter, not the conspiracy theory's contrary ('no social phenomenon is the result of conscious design'), but its contradictory ('at least one social phenomenon is not the result of conscious design'). If one is interested in providing true social explanations, it is essential to realise that, in the words of Hayek, "the insight that not all order that results from the interplay of human actions is the result of design is indeed the beginning of social theory"². It is thus surely undeniable that the concept 'unintended consequence' is among the most crucial notions used by the propositions of any analysis purporting to give a comprehensive account of what happens within a given realm of social events. Therefore, when one is faced with a social phenomenon for which one wants to obtain an explanation, one should not ask oneself 'Who worked to bring this about?', but should ask instead a wider question that embraces this, namely, 'From what purposive action, or set of interacting purposive actions, does this arise as the intended or unintended social consequence in these initial environmental conditions?' .

1. The characteristic features of statements which are neither amenable to verification nor to falsification in a formal manner, will be discussed in chapter IV .
2. F.A. Hayek, The Confusion of Language in Political Thought, (The Institute of Economic Affairs, London, 1968), p. 10 (Hayek's italics)

To press home the point that the aforementioned is the scientifically appropriate question for social scientists to ask, sections (ii) and (iii) of this chapter will be devoted to giving two examples from economic theory of the way the interaction of the independent actions of many individuals can produce social phenomena that are not intended by the acting agents themselves. These examples are (a) the explanation of the frequent price instability liable to occur in free markets for primary products, and (b) the explanation of the Keynesian 'paradox of thrift' phenomenon .

(By way of parenthesis, a point of philosophical interest that is worth raising, is that the word 'consequence' (or 'repercussion'), as it occurs in the phrase 'unintended consequence' (or 'unintended repercussion'), is ambiguous in a way that does not appear to have been recognised by Popper or Hayek (or indeed by any of those traditionally associated with the general methodological approach that urges social scientists to pay particular attention to the undesigned results of men's actions) . As it operates within the phrase in question, the term 'consequence' may mean an unintended phenomenon that is a causal result of an agent's purposive action, or one that is a logical or conceptual result.¹ For the most part, certainly, unintended consequences are brought about causally (eg. the economic phenomena to be discussed later on) , but there is nevertheless, a small residue of social phenomena that appear as a matter of conceptual necessity. One can without much difficulty think

1. I owe this idea to Professor H.B. Acton (University of Edinburgh) .

up simple and non-technical examples of both sorts. An example of a causally produced unintended phenomenon may be observed to occur in an auction room where, say, valuable paintings are on sale. If a man enters and starts to bid for some article, this action is likely to cause the other eager bidders to raise the level of their own offers. Thus, although for the man in question it is no part of his intention to raise the price at which the work of art he wanted could be obtained, his action of entering the 'market place' as an active and determined participant is nevertheless likely to cause this to be the actual result. An example of a conceptually entailed unintended result occurs when a man decides on the most convenient time to obtain his lunch from a self-service cafeteria. If he decides not to go at one o'clock because the queue is then so long, but to go at half past one instead, he achieves his intended result (obtaining a meal without a long time-wasting delay) but his not appearing at one o'clock has the unintended consequence that many of those who do go at that time face a slightly shorter queue. But this is not something caused by the agent's action; for if he joins the queue at half past one rather than at one o'clock, it is a logical consequence that the queue at the earlier time contains one less person than it otherwise would have done.)

Not only are occurrences of specific single phenomena to be correctly accounted for as the unintended consequences of the interaction of many actions directed towards a variety of goals, but also whole societal structures and institutions (eg. language, common or private law, the market price system) owe their existence and persistence to the fact that they arose and continued to function

although none of the acting agents had intended that this should be the case. Situated diametrically opposite, however, to the view that "the independent action of many men can produce coherent wholes, persistent structures of relationships which serve important human purposes without having been designed for that end"¹, lies what was called earlier in this chapter the 'design theory of institutions'; this maintains that "no result of the action of many men can show order or serve a useful purpose unless it is the result of deliberate design"². Here, therefore, one has a straight confrontation between two conflicting analyses of social processes. On the one hand, there is the account which holds that a considerable amount of social order originally appeared, and was subsequently maintained, quite spontaneously as the unintended result of the many different actions of free men; and that, in time, such order proved not merely helpful, but actually indispensable for the realisation of much that civilised individuals came to desire. On the other hand, there is the approach that insists that whenever one finds important purposeful arrangements or organisations in society, these should be explained largely on the supposition that there was a time when they were consciously designed and then deliberately instituted to serve specific ends.

It is seriously to trivialise the question at stake here, to think of the controversy relating to the design theory as a purely

1. F.A. Hayek, The Counter-Revolution of Science, op. cit., p. 80

2. Ibid., p. 80

'academic' problem concocted by Hayek in 1942¹, and then developed and publicised by Popper two years later². (It is of course a plain historical fact that in recent times Hayek³ and Popper⁴ have been very much to the fore in leading the debate on this issue.) For one's view of how to settle this methodological controversy, carries with it far-reaching implications concerning the extent to which it is both rational and desirable to place economic and other social processes

1. F.A. Hayek, 'Scientism and the Study of Society' -
 - Part I in Economica, Vol. IX (New Series), No. 35, August 1942, pp. 267-291
 - Part II in Economica, Vol. X (New Series), No. 37, February 1943, pp. 34-63
 - Part III in Economica, Vol. XI (New Series), No. 41, February 1944, pp. 27-39

(The above are published as the first half (i.e. pp. 13-102) of The Counter-Revolution of Science, op. cit. .)
2. K.R. Popper, 'The Poverty of Historicism' -
 - Part I in Economica, Vol. XI (New Series), No. 42, May 1944, pp. 86-103
 - Part II in Economica, Vol. XI (New Series), No. 43, August 1944, pp. 119-137
 - Part III in Economica, Vol. XII (New Series), No. 46, May 1945, pp. 69-89

(The above are published as The Poverty of Historicism, op. cit. .)
3. F.A. Hayek, passim, but especially -
 - (a) 'Individualism: True and False' (1946), in Individualism and Economic Order, (Routledge and Kegan Paul, London, 1949), pp. 1-32
 - (b) 'Kinds of Rationalism' (1965) and 'The Results of Human Action but not of Human Design' (1967), in Studies in Philosophy, Politics and Economics, (Routledge and Kegan Paul, London, 1967), pp. 82-95 and 96-105, respectively.
4. K.R. Popper, passim, but especially -
 - (a) The Open Society and its Enemies, Vol. II, op. cit., Chapter 24 (i.e. pp. 224-258)
 - (b) 'Utopia and Violence' (1948), 'Towards a Rational Theory of Tradition' (1949) and 'Public Opinion and Liberal Principles' (1955), in Conjectures and Refutations, op. cit., pp. 355-363, 120-135 and 347-354, respectively.

under a system of unified central management.

Belief in a predominantly design theory of social explanation, engenders an 'engineering state of mind' which is favourably disposed to the view that societal institutions and processes are optimally serviceable to human interests (and hence worthy of retention), only if they have been deliberately created for the overt purpose of forming the means whereby specific human desires are to be satisfied, and if they then remain under conscious control to ensure that they continue to serve the particular functions for which they were intended in the first place. By way of contrast, acceptance of a mainly 'unintended consequence' type of analysis, inclines one to the position that provided spontaneously arisen systems of social order operate, on the whole and for the most part, more effectively than would any empirically possible alternative arrangements produced by direct design, they should be allowed to remain largely intact (but be subject to conscious adjustment whenever this is deemed necessary) .

Deciding between these two approaches need be neither arbitrary, nor a matter of blind, unthinking prejudice. If one considers the problem sensibly, the choice seems clearly to hinge on whether in fact an undesigned and spontaneously functioning system of social order is more, or is less, purposeful with regard to the efficient satisfaction of the needs and wants of society's members, than an overall structure consciously designed to fulfil the same general function. And to this, one is able to give an answer. Perhaps

the decisive reason, why a largely spontaneous "polycentric"¹ system of social order (i.e. a system of unintended order resulting from the actions of agents motivated by their own aims, and guided by their own understanding of their particular situational circumstances) is likely to be more effective in serving the vast number of diffuse and differing goals sought by members of society, is that such a system of mutually adjusting relationships can be sufficiently complex to be responsive to all the numerous and varying human wants. It can be precisely this, because it is able to draw upon and utilise the requisite knowledge distributed over, and only accessible to, the millions of separate individuals and institutions whose countless needs require satisfaction.² By comparison, a 'monocentric' system of social order (i.e. a system of consciously created and directed order resulting from the actions of agents motivated not by their individual decisions, but by the particular commands of a central agency) , can necessarily only be as intricate (and thus, adaptable and flexible) a structure as will actually permit of management by the controlling body.³

The first person to be generally credited with arguing that a social order which is mainly the result of spontaneous and unintended growth, may indeed be highly purposeful in serving men's varied interests, is the Dutchman Bernard Mandeville (1670-1733) . It was against the conception that a serviceable social order can only result from conscious

1. M. Polanyi, The Logic of Liberty, (Routledge and Kegan Paul, London, 1951), p. 170ff

2. Ibid., pp. 111-122 and 180-192

3. Reference will be made again to this point, in chapter III .

design, that Mandeville in 1714 posited the paradox of 'Private Vices, Publick Benefits' in his The Fable of the Bees. This book begins with a reprint of a work that Mandeville first published nine years earlier, a 433-line satirical allegory entitled 'The Grumbling Hive: Or, Knaves Turn'd Honest'. It is this short piece of verse, in which the description of the life within a beehive acts as an analysis of the economic life of early eighteenth century England, that first gives expression to Mandeville's profound insights into the operation and functioning of social processes.¹

The inhabitants of Mandeville's hive are all addicted to the vices of luxury, avarice, prodigality, pride and vanity, but though the underlying motives for their self-interested actions may be considered reprehensible at one level, the hive does not come to grief as a result of the sins of its numerous members; on the contrary, the overall result of all their actions produces the desirable state of affairs of an economically prosperous civilisation. The reason is that these vices promote material development, by creating desires which the bees see they can satisfy to their own benefit if they become

1. In his history of the doctrines given by those whom he takes to be the precursors of his own view regarding the danger of 'under-consumption', it is interesting to find that John Maynard Keynes (1883-1946) assigns Mandeville a high and honourable place.

- J.M. Keynes, The General Theory of Employment, Interest and Money, (Macmillan, London, 1936, edit., 1957), pp. 359-362

manufacturers and tradesmen. In this flourishing community, although :

" ... every Part was full of Vice,
Yet the whole Mass a Paradise;" 1

Mandeville, therefore, does not condemn the bees for their vices ; the prosperity of the hive as a whole depends on, and results from, these same self-seeking actions of its constituent members. Indeed, even :

" The Worst of all the Multitude
Did something for the common Good." 2

For, Mandeville argues, even the activities of criminals have the good effect that they provide lucrative work for many³ - lawyers, goalers, turnkeys, sergeants, tipstiffs and locksmiths - and they in their turn are then in possession of the wherewithal to indulge their appetites for luxury.

The general approach that Mandeville expounds in his own fanciful and picturesquely exaggerated manner, lies at the heart of all subsequent attempts to achieve a sound form of social inquiry. Later on in the same century, Mandeville's main theme provides the foundation for classical economics.

It is precisely to the discovery that societal phenomena such as the market economy can arise which are not intended, but which may then prove the only feasible means by which men can achieve their

1. P. Harth (ed), Mandeville - The Fable of the Bees, (Penguin, London, 1970), p. 67
2. Ibid., p. 68
3. Ibid., p. 71

separate and conflicting goals, that Adam Smith (1723-1790) alludes, in his famous reference to the activities of the 'invisible hand' . Despite the ridicule and abuse that has been, and still often is, showered upon Smith , he, more than anyone else, deserves to be considered the originator of systematic social theorising. In the following passage, Smith advances the thesis that is foundational to all forms of sound social scientific investigation; namely, that frequently men unwittingly establish a spontaneous system of polycentric order, and then subsequently realise that to make use of this structure is a more effective way to achieve their many goals, than any alternative means available to them. Smith writes:

" every individual necessarily labours to render the annual revenue of the society as great as he can. He generally, indeed, neither intends to promote the public interest, nor knows how much he is promoting it. ... he intends only his own gain, and he is in this, as in many other cases, led by an invisible hand to promote an end which was no part of his intention. Nor is it always the worse for the society that it was no part of it. By pursuing his own interest he frequently promotes that of the society more effectually than when he really intends to promote it. " 1

Here, Smith contends that (for the Britain of his day), if the desired aim is to produce the highest national income possible ('to render the annual revenue of the society as great as possible') at the given stage of technological development, then the rational thing to do is to encourage extensive division of labour by permitting each individual to adjust his own actions, in whatever way he sees fit, to the spontaneous

1. E. Cannan (ed), Adam Smith - The Wealth of Nations, Vol. I, 1776, (Methuen, London, edit., 1961), pp. 477-478 (i.e. Bk. IV, Ch. 2)

price system arising (within the moral and legal framework) from the actions of all the other individuals. Smith's bold claim that a competitive market economy is the most technically efficient means known to man of providing for human needs and wants ('of promoting the public interest'), rests on the astute observation that the operation of a market price system does not require anyone to undertake the task of consciously co-ordinating the actions of each individual to the actions of all the others. In a free economy such co-ordination is achieved more or less automatically when each individual seeks 'only his own gain', not in the abstract, but in response to public indicators (eg. prices, costs, profit rates) which emerge as the unintended consequences of the like actions of all the other participants within the economy.

As well-informed commentators¹ have taken pains to stress, it is no part of Smith's argument that a spontaneous or self-generating social order is ipso facto a perfectly functioning social order. Smith in fact fully recognises that an unintended social structure may possess features that are unwanted, and that if this should be the case, men are fully entitled to make conscious alterations to the system to remove its

1. For example,

- E. Whittaker, A History of Economic Ideas, (Longmans, New York, 1940), pp. 158-159
- A. Gray, The Development of Economic Doctrine, (Longmans, London, edit., 1967), p. 143ff

undesirable characteristics.¹ Smith merely argues that a purposeful and well co-ordinated social order, does not necessarily imply the existence of an orderer (here, the price mechanism of a market economy provides a conclusive counter-example to the contrary claim of the design theory) ; and to say that specific social phenomena are produced by the actions of men, is not to say that they must therefore be the result of conscious design.

Although it is methodologically appropriate for all the social sciences to follow Mandeville and Smith in giving due recognition to the likelihood of both specific phenomena and purposeful systems of polycentric order arising quite spontaneously without any conscious direction, the clearest and most spectacular examples of the employment of the concept 'unintended consequence' are to be found within economic theory. The rest of this chapter will thus be devoted

1. Although Smith champions the use of the spontaneous price mechanism as the most effective means for achieving material prosperity, he is fully aware that an industrial society based upon the free division of labour has its own special (unintended) evils. He declares that when a man's employment comes to be confined to the performance of just a few very simple operations, his whole character and life are impoverished in certain ways. "His dexterity at his own particular trade seems ... to be acquired at the expense of his intellectual, social and martial virtues. But in every improved and civilised society this is the state into which the labouring poor, that is, the great body of the people, must necessarily fall unless government takes some pains to prevent it."

Smith then goes on, in his discussion of education, to give his firm support to State action undertaken for the explicit purpose of removing these unwanted consequences of labour specialisation, consequences, incidentally, which later generations were to name 'alienation' .

- E. Cannan (ed), Adam Smith - The Wealth of Nations, Vol. II, 1776, (Methuen, London, edit., 1961), p. 303 (above citation), and pp. 303-309 (i.e. Bk. V, Ch. 1, Pt. 3, Art. 2) .

to illustrating how social scientific explanations typically operate, by giving two examples taken from economic science. (It is to be observed again that although economics is distinguished from the other social sciences by being the only theoretical social science, its explanatory approach is similar to that of the other social sciences because it frames its explanations as answers to the same general question of inquiry.)

(ii) Example (a) - The Phenomenon of Price Oscillation

The scientifically acceptable way to account for a social phenomenon, is to trace back its causal antecedents to the set of consciously purposive actions from which it arose (as the intended or unintended consequence) within the prevailing societal conditions and circumstances. The first of the two examples illustrating explanations falling into this common structure, concerns the occurrence of certain instances of price instability. More precisely, the questions one is seeking to answer are: 'Why is it that in free markets, the prices of many agricultural goods and other primary products tend to oscillate between a high and a low point from year to year, and that these limits are liable over time to become increasingly separated? From what sort of behavioural interaction does this kind of phenomenon result?' .

A solution to this perplexing problem is to be provided by that branch of economic theory that deals with the way relative prices are determined within a free-market society. It is reasonable to

maintain that 'micro-economic price theory', as this part of economics is usually called, has no competitor as the most advanced and best developed of all social scientific theories. In his well known text-book, the economist Richard Lipsey pays it the very highest compliments. This theory, he asserts, "is beautiful in its simplicity and yet unusual in its wide range of real-world applications. For all its shortcomings, the theory of the determination of price by demand and supply is one of the finest examples of a theory that is both simple and powerful."¹ .

At the very heart of this social theory, one finds the fundamental premise that "when time and the means for achieving ends are limited and capable of alternative application, then behaviour necessarily assumes the form of choice"² . Individuals (and also households, firms and other institutions) cannot satisfy all their wants and desires to the fullest possible extent, because it is an empirical impossibility to supply all the goods and services that would be required to do this. In the face of this basic fact of scarcity, without which there would be no economic activity, the supply produced to satisfy the demand for one particular type of good, has necessarily to be limited, if other kinds of goods and services are to be supplied to satisfy other sorts of demand. Ceteris paribus (i.e. if there is no government intervention, and a state of free competition prevails),

1. R.G. Lipsey, An Introduction to Positive Economics, (Weidenfeld and Nicolson, London, second edit., 1966), p. 104
2. L. Robbins, op. cit., p. 13 (Robbins' italics)

the way the various factors of production (that is, the available 'units' of land, labour and capital) are used for productive purposes, is decided by the demand decisions of consumers expressed through the mechanism of the market price system. The final equilibrium price of one sort of commodity, that is the price at which neither a shortage nor a surplus develops, is determined both by the level of demand for it, and the level of supply of it. The total demand for a good - the total quantity demanded per time period - at a particular price, is the sum of all the separate demands of all consuming individuals and institutions. Likewise, the total supply of a commodity at a particular price, is the addition of the various supply flows per time period produced by all firms making that type of good.

Each individual's demand for a particular sort of commodity is dependent upon a number of variables, such as (i) the ruling or current price of the good, (ii) the ruling prices of other goods, and especially those of substitute and complementary goods, (iii) his own level of disposable money income, and (iv) his tastes and preferences. Similarly, each firm's supply of a good is a function not only of the first two and last mentioned of these factors, but also of (a) the current state of technology, and (b) the price and availability of land, labour and capital. But as far as the theory itself is concerned, one examines the general connections holding between the variables 'quantity demanded', 'quantity supplied' and 'ruling price' as if all factors influencing the levels of demand and supply remain constant over time.

Micro-economic theory assumes that the conscious aim of each consumer is the maximisation of his satisfaction, and that this is rationally achieved, when equal money expenditures on the last units obtained of each of the different kinds of goods purchased, yield equal amounts of satisfaction. Suppliers are likewise held to be rational; since each individual supplier is held to be anxious to maximise his profit, it is assumed that he will therefore produce goods up to the level at which the marginal cost of the last good produced equals the marginal revenue obtained from the sale of that last good.¹ Also, it is assumed that if the price of any one sort of good should rise, then some suppliers would substitute, in place of the kind of good they were producing, production of that relatively more scarce sort of good.²

From these assumptions about the actions of consumers and suppliers of goods, two important general conclusions about the

1. Thus, the upward sloping portion of any individual supplier's marginal cost curve can be regarded as his short-run supply curve. The market supply curve for a good of a certain sort, is the result of the horizontal summation of all the individual supply curves.
2. This statement is true, both when suppliers are operating in conditions of 'pure competition' and when they are in conditions of 'monopolistic competition' .

behaviour with which micro-economics is concerned, can be given:¹

- (a) Provided all other influences on demand remain constant, the total demand for any one type of product will vary inversely with any change in the ruling price - i.e. quantity demanded is a decreasing function of price.
- (b) Provided all other influences on supply remain constant, the total supply of any one kind of product will vary directly with any change in the ruling price - i.e. quantity supplied is an increasing function of price.

Graphical curves representing the schedules of the quantities demanded and supplied of any one type of good through the range of possible ruling prices, can be drawn to illustrate statements (a) and (b) .

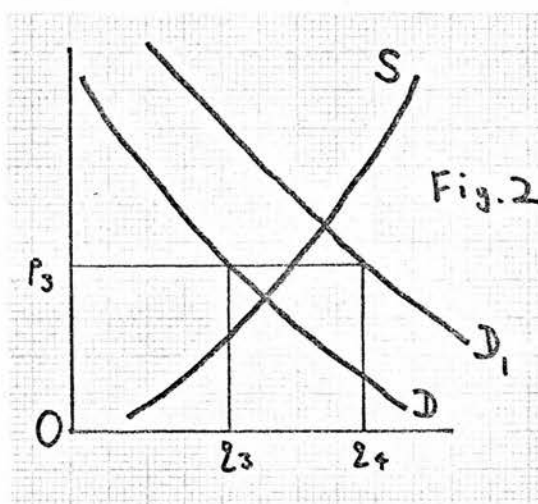
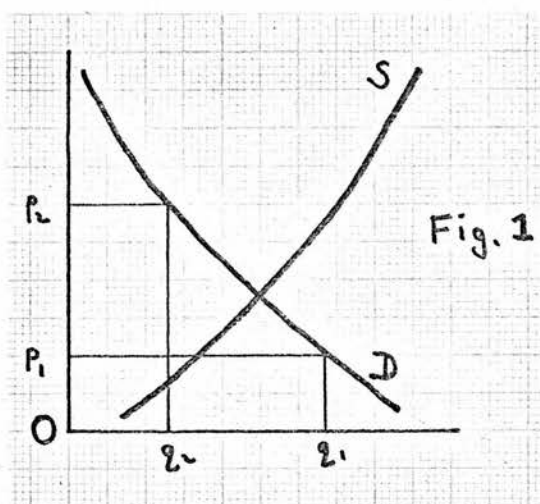
On any such graph, it is usual for 'quantity' to be measured along the abscissa (the horizontal axis) , and 'price' to be measured up the ordinate (the vertical axis) .

In figure 1. curve D , the market demand curve (produced by the horizontal summation of all individual demand curves), slopes continuously downward from the left to the right, while the curve S , the market supply curve (produced by the horizontal summation of the supply curves of all separate producers), slopes continuously upward

1. These conclusions offer good descriptions of the behaviour of consumers and suppliers as they act towards 'normal' goods. One does, however, meet 'abnormal' cases of demand and supply:

- (i) Demand is directly related to price when a consumer of low income has to purchase a minimum quantity of a good to stay alive. If its price rises, this may leave the consumer too poor to spend the remainder of his income on anything other than a few more units of the vital good in question.
- (ii) Supply is inversely related to price when a supplier attempts to maintain constant the absolute level of his total income, by supplying more if the price falls, and by supplying less if the price rises.

from the left to the right. One should notice that a change in the quantity demanded (or supplied), is to be distinguished from a change in the level of demand (or supply) . The former is a change due to a change in the ruling price. If (figure 1.) the price changes from Op_1 to Op_2 , then there will be a change in the quantity demanded from



Qq_1 to Qq_2 . The latter is change in demand (or supply) due to an exogenous¹ change in one of the variables, apart from the ruling price, determining the level of demand (or supply) . If (figure 2.) the ruling price remains constant at Op_3 a change in, say, consumer tastes could result in an increase in the level of demand, represented by a shift in the demand curve from D to D_1 , such that consumers would demand quantity Qq_4 instead of Qq_3 .

1. Economists usually refer to dependent and independent variables as, respectively, 'endogenous', and 'exogenous' or 'autonomous variables'.

One more major feature of market demand and supply curves needs to be mentioned, before one can attempt to give an adequate explanation of the phenomenon under investigation. The 'coefficient of price elasticity of demand (or supply)' is the numerical measure which indicates the percentage change in quantity demanded (or supplied), compared to the percentage change in price. Thus :

$$\epsilon_d \text{ (or } \epsilon_s) = \frac{\% \text{ change in quantity demanded (or supplied)}}{\% \text{ change in price}}$$

To condense the matter greatly, the demand for (or supply of) a commodity is said to have zero, unit, or infinite price elasticity, or to be price inelastic or elastic, if, respectively, the quantity demanded (or supplied) does not change as price changes, changes by the same percentage, is infinite at one particular price, changes proportionately less as price changes, or changes proportionately more as price changes.¹

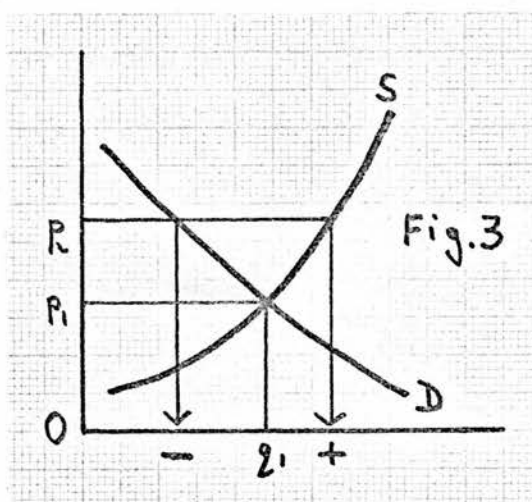
In a market situation represented by demand and supply curves, the price at which these curves intersect, if they do so at all, is the equilibrium price. If such a price emerges in a market, then that price is unique; a commodity never has more than one equilibrium price per market.² From the basic behavioural patterns assumed so

1. Strictly, since demand is usually inversely related to price, the coefficient of price elasticity of demand should, in normal cases, be negative. Convention, however, allows one to drop the '(-1)', with which the numerical measure is a product.

2. In certain abnormal conditions, one does have more than one potential equilibrium price per market, eg. when one has a backward sloping supply curve of labour, and this intersects the employers' demand curve for labour at two wage levels.

far - the lower the price the greater will be the total quantity demanded and the smaller the total quantity supplied, and, the higher the price the smaller will be the quantity demanded and the greater the quantity supplied - it follows that if there is an interaction and the ruling price is a disequilibrium price, then a position of equilibrium will tend to establish itself.

This tendency to gain or to restore an equilibrium position when in a disequilibrium position, results from a spontaneously functioning feedback mechanism. If (figure 3.) the ruling



disequilibrium price was Op_2 the quantity demanded would be less than Oq_1 , while the quantity supplied would be greater than this amount. The suppliers would therefore discover that at this price they would be left with an unsold surplus at the end of the time period in question. On the reasonable supposition that they would all prefer to sell at a reduced price than to risk not selling anything, they would each reduce the price from Op_2 . As the price was lowered so the quantity demanded would increase, and as the ruling price approached price Op_1 ,

so the difference between the quantities demanded and supplied would approach vanishing point. Thus, one can see that an unintended result of the interaction of consumer and supplier behaviour in a market for a particular commodity, is, at the minimum, a strong tendency for that market to be self-co-ordinating and self-adjusting; it will tend to 'correct' any disequilibrium price that emerges as the ruling price.

In the preceding argument, the word 'tend' has been used extensively. In the market represented by figure 3. it would in fact be the case, for reasons that will become apparent in a short time, that the spontaneous feedback mechanism is not only necessary but is also sufficient to ensure that from a position of disequilibrium, an equilibrium price would, ceteris paribus, be achieved. Nothing however in the construction of price theory, justifies the inference that in all cases of price disequilibrium the feedback mechanism (the undesigned effect of the intersection of the rationally purposive actions of consumers and suppliers) is sufficient to achieve an equilibrium price.¹ But the tendency for the market to 'correct' a disequilibrium situation, is always present, as an unintended consequence, when demand and supply actions are causally related to the level of the ruling price, in the way described.

On the assumption that the feedback mechanism is sufficient to restore a lost equilibrium, the method known as 'comparative static equilibrium analysis' enables one to 'explain in principle' the occurrence of a wide range of economic and related social phenomena. In

1. G.C. Archibald and R.G. Lipsey, An Introduction to a Mathematical Treatment of Economics, (Weidenfeld and Nicolson, London, 1967), pp. 271-302

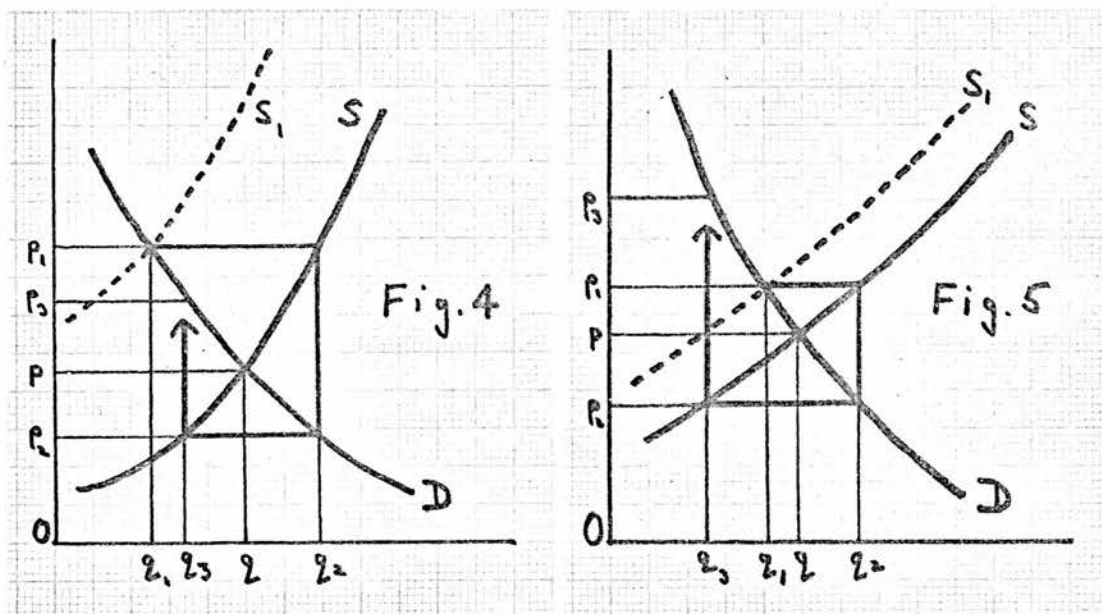
static analysis, one starts from one equilibrium position, hypothesises some change, and then compares the theoretic resultant with the old equilibrium position. From the differences between these two positions, one obtains an idea of what would happen in society, if the theoretically supposed change were actually introduced. In such cases, given the basic theoretical structure, one just thinks through the consequences of the proposed change by determining how the market mechanism would react.

However, to explain the phenomenon of price oscillation, static analysis, with its assumption of the sufficiency of the feedback mechanism, is inadequate. To account for an instance of this kind of explanandum-phenomenon, one requires to construct a slightly more complicated dynamic theory to show how the variables 'quantity demanded', 'quantity supplied' and 'ruling price' behave in between the introduction of the original change, and the subsequent attainment, if any, of a new equilibrium position.

As well as abandoning the sufficiency assumption regarding the operation of the feedback mechanism, one needs to adopt the additional premise that a change in the ruling price elicits a change in the total quantity supplied after a one period time-lag. As an assumption for a theory of the markets of most manufactured (or secondary) products this would be unsatisfactory, for often an increase in the level of demand for industrial commodities can be met almost immediately by an increase in supply due to the utilisation of spare capacity or by increasing overtime working. However, with other types of goods, notably agricultural and other primary products, the respective markets would in fact take a longer time to respond to an increase in

price, sometimes a whole year would be required.¹ For the theory relating to the markets of such kinds of goods, the assumption of this additional supposition is acceptable.

Assuming, therefore, a one period time-lag in the alteration of the quantity supplied, two main explanatory accounts are available. In figure 4. the original equilibrium is at price O_p , with quantity supplied Oq . But during time period t , one supposes, the level of



supply temporarily contracts because of a harvest failure. This is illustrated by movement of the supply curve, from S to S_1 . The new ruling price is O_p_1 , and the quantity supplied is Oq_1 . Given a one period time-lag between a price rise and the causal effect on the

1. G.C. Archibald and R.G. Lipsey, op. cit., p. 298

Also,

- R.G. Lipsey, op. cit., pp. 139-159

- P.A. Samuelson, Economics, (McGraw-Hill, Chicago, sixth edit., 1964), pp. 396-398 and pp. 399-423

quantity supplied, it can be seen that during time period $(t + 1)$, ceteris paribus, the quantity supplied, in response to the ruling price Op_1 of time period t , will be Oq_2 . But in period $(t + 1)$ in order to sell quantity Oq_2 , the price will have to fall to Op_2 . In time period $(t + 2)$, because of the fall in price from Op_1 to Op_2 in period $(t + 1)$, quantity Oq_3 will be supplied. But if that quantity is supplied, demand for it will bid the price up to Op_3 ... etc. . One can draw up a table to illustrate the various quantities and prices :

<u>Time period</u>	<u>Quantity supplied</u>	<u>Price</u>
t	Oq_1	Op_1
$t + 1$	Oq_2	Op_2
$t + 2$	Oq_3	Op_3
		<u>etc.</u>

In the kind of market represented by figure 4. , and whose time periods are detailed by the above table, the feedback mechanism is sufficient to renew an equilibrium once a previous one has been disturbed. The price oscillations will over time become smaller and smaller, and eventually quantity Oq will be supplied at price Op . The graphical representation of markets of this sort, is known as a 'convergent (or stable) cobweb' .

The explanation of why the feedback is sufficient to restore a lost equilibrium in markets of this kind, is that such markets are the unintended results of the interaction of actions comprising a level of demand that is more price elastic than the level of supply ; as price alters, the percentage change in quantity demanded alters more than the percentage change in quantity supplied. Thus, the percentage increase in price due to the supply of a shortage is less than the percentage decrease

in price required to sell a supplied surplus. But since each surplus is supplied following from the price increase due to a previous shortage, each oscillatory cycle starts from a ruling price nearer than the one before it to the equilibrium price. Therefore, the size of the price oscillation gradually gets smaller and smaller with the passing of time, as can be seen by inspection of the relevant graph.

The second and contrasting type of market, is illustrated by figure 5. . Here, the market is represented by what is called a 'divergent (or unstable) cobweb' . Suppose that the original equilibrium price is Op , and that quantity Oq is supplied. If, in time period t , this equilibrium is disturbed, the new quantity supplied will be, say, Oq_1 at price Op_1 . On the basis of a one period time-lag in supply, price Op_1 in period t would elicit the supply of quantity Oq_2 in period $(t + 1)$. But in this period, such a quantity could only be sold if the price were Op_2 , ... etc. . Exactly the same table as for the stable sort of market, outlines the time periods and price oscillations of this unstable type of market.

With divergent markets, the price oscillations grow larger and larger as time proceeds. The feedback mechanism, although operative, fails to be successful. The explanation why this should be so, closely parallels the previous explanation why convergent markets are stable. In an unstable market, the interacting behaviour of consumers and suppliers is such, that as price changes, the percentage change in quantity supplied alters more than the percentage change in quantity demanded. This means that the percentage increase in price following from a shortage, is more than the percentage decrease in price necessary

to sell a supplied surplus. But since each surplus is supplied following in time from the price increase due to a previous shortage, the ruling price oscillates each cycle further and further away from the equilibrium price.

Since this analysis of unstable markets is generally applicable to free markets for primary products (the supply of such commodities is both subject to periodic time-lags, and to unpredictable and sudden alterations due to adverse weather, or animal or plant disease, and the demand is liable to be less price elastic than the supply¹), one is thus able to use it to give an 'explanation of the principle' upon which price oscillations tend to pervade such markets. The phenomenon of price oscillation, to emphasise this point again, is not something maliciously engineered by somebody, but is an occurrence that appears as an unintended consequence when certain sorts of actions, each directed towards its own goal, intersect in a particular type of societal environment. But once this fact becomes known, that most influential of institutions, the government, is then in a position to act rationally if it wishes to take effective steps to neutralise the unwanted aspects of the resultant situation.

Indeed, it is because economists have successfully accounted for the explanandum-phenomenon in question, that governments in most western (i.e. non-communist) nations find it necessary to intervene actively in the free markets for agricultural and other primary products. This policy of intervention, is consciously designed to stabilise the

1. R.G. Lipsey, op. cit., p. 117 and 142

market by ruling out price oscillations and, as far as possible, changes in the quantity offered for sale (not necessarily the same thing as the quantity produced), from year to year. The desired stabilisation is achieved by such measures as government controlled marketing boards, guaranteed price support, direct subsidies to producers, variable import tariffs, and numerous kinds of programmes intended to limit or increase compulsorily the actual supply of certain goods at particular times.¹

(iii) Example (b) - The 'Paradox of Thrift' Phenomenon

For a second example of the way in which deliberate action can produce unintended consequences of a striking kind, we shall examine the phenomenon known to economists as the 'paradox of thrift'. But before this paradox can be reached, it is necessary for us to direct our energy to seeking an answer to the following two-part question: 'If there is some unused capacity in the economy, what would happen if, ceteris paribus, many individuals decided to save a larger proportion of their incomes than they are now doing? Are a large number of individuals able simultaneously to increase the absolute level of their saving if this should be the intention of each of them?' . To obtain a reply to this double question, one cannot employ the micro-economic price theory

1. P.A. Samuelson, op. cit., p. 405ff

that was used in the previous section.

The main task of micro-economics, is to explain the arrangement of the relative prices of those commodities and services, demanded and supplied within a market economy, that it is called upon to examine. But the question under consideration at the moment, is not concerned with the problem of the connections between the prices and commodities to be found in the various parts of an economy. The interest is rather in discovering the causal relationships (if any) which hold between aggregate variables such as total employment, total expenditure, the levels of saving and investment, and the average level of all prices. The formal theory that has been painstakingly developed over many years to deal with such high level societal phenomena, is called 'macro-economic theory'. It must not however be thought that because macro-economics deals with 'wholes', it operates in violation of the general principle that in any sound social inquiry all operative changes must be traceable to the deliberative actions of individuals, groups of individuals and institutions. The fundamental mode of theoretical analysis employed by macro-economics is methodologically identical to that used by micro-economics; indeed one "should think of micro and macro variables as representing a continuum of more or less aggregated data ranging from the expenditure of a single individual on a single commodity to the total expenditure of all households in the world on all commodities"¹. Macro variables are thus not anything other than simple aggregations of properties predicible of (anonymous but typically

1. R.G. Lipsey, op. cit., p. 538

socialised) individuals living in the society under investigation. For this reason, there may be times when it is as difficult to draw a clear-cut line between what is a micro- and what is a macro-economic phenomenon, as it is to tell of certain shades of colour whether they are more properly described as blue or as green.

Macro-economic theory obtains its explanatory ability by treating the whole economy as a circular flow of income. This flow runs from households and individuals to firms and businesses in return for goods and services purchased, and then runs back again from firms and businesses in the form of wages, salaries, rent and dividends for productive services rendered. The Gross National Income (equal to the money value of the Gross National Product) is the total flow of money income per time period paid to all individuals and firms. The GNI^1 (hereafter called simply the 'national income') in time period t is equal, ceteris paribus, to aggregate expenditure, or the total flow of money spent by individuals and firms, in time period $(t + 1)$. Aggregate expenditure at any time period is itself, ceteris paribus, the determinant of the national income at a later time period.

But other things may well not be equal. There may be additions to the circular flow of income, such as investment expenditure, government spending, and payments made from abroad for the purchase of exported goods. Likewise, there may be withdrawals such as saving, taxation, and money used to buy imported goods. If in any period

1. The Net National Income (NNI) is equal to the money value of the Net National Product (NNP). NNI equals the value of the GNP minus a sum deducted to the value of capital depreciation and indirect taxation.

additions to the flow should exceed withdrawals, this will cause, via the 'multiplier'¹, an increase in the volume of the flow in succeeding periods of time. Conversely, if in any period of time, withdrawals should exceed additions, this will cause a contraction in the circular flow of income in following time periods.

The whole economy is said to be 'in equilibrium', if there is nothing operating to cause the circular flow of income to increase or decrease in magnitude over time. This occurs when withdrawals from the circular flow equal additions in any time period. An economy is thus in equilibrium when national income equals aggregate expenditure over successive time periods, i.e. $Y_t = E_{t+1}$.

To turn this general conception of the economy into a serviceable explanatory account, one first makes the simplifying assumption that all additions and withdrawals, except for saving (S) and investment (I), balance, and that S and I are determined independently of each other. Secondly, one supposes that within the economy there is not full utilisation of all the factors of production of each sort, and that any increase in aggregate expenditure (which constitutes the total level of demand) will be met by increasing the total supply of goods by bringing into productive use previously idle, or underemployed, factors. From this second assumption, it follows that

1. The 'multiplier' addition (or subtraction) is the product of the original addition (or subtraction) and the reciprocal of the marginal propensity to save. Since the MPS is, by definition, (1 - the marginal propensity to consume), it can be seen that the 'multiplied' amount (to be added or subtracted) is obtained by using 'a/1-r' - the formula for a geometric progression to infinity where $r < 1$.

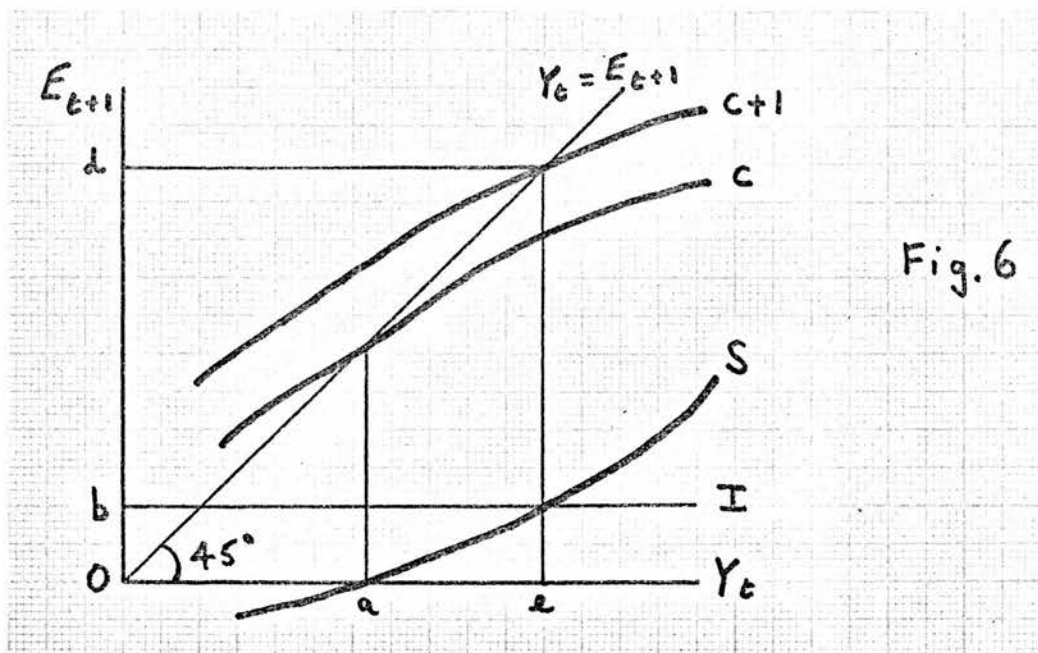
the level of employment, or use of labour factors, is a rising function of aggregate expenditure.

Within the structure of the theory, the level of aggregate expenditure in time period $(t + 1)$ is, by definition, made up from the investment expenditure in that period and from that part of the national income obtained in period t that is not withdrawn in period $(t + 1)$ but is spent, or consumed, on goods and services in that period. Thus, as far as the theory goes, it is analytically true that E_{t+1} is equivalent to $I_{t+1} + C_{t+1}$. It is also true by definitional identity that the national income in one period will either be saved or consumed in the next period, that is, Y_t is equivalent to $S_{t+1} + C_{t+1}$. On the understanding that it is a "fundamental psychological law, upon which we are entitled to depend with great confidence both a priori from our knowledge of human nature and from the detailed facts of experience, ... that men are disposed, as a rule and on the average, to increase their consumption as their income increases, but not by as much as the increase in their income"¹, one infers that as national income rises, the average propensity to consume (APC) - the percentage of total income that is not saved - falls.

In figure 6. the 45° degree line is called the 'linear aggregate supply function', and every point on it represents a potential equilibrium because at every such point $Y_t = E_{t+1}$.² On this

1. J.M. Keynes, op. cit., p. 96 (Keynes' italics). See chapter V of this present discussion for an analysis of what 'psychological' means in the citation in question here.
2. R.G. Lipsey, op. cit., pp. 545-593
P.A. Samuelson, op. cit., pp. 221-249

graph, and similarly with figure 7. , the level of the flow of expenditure in time period $(t + 1)$ is measured up the ordinate, while



the level of the flow of national income in time period t is measured along the abscissa. The level of investment expenditure is assumed in

the short run¹ to be autonomous -- it is represented by a horizontal line at level O_b parallel to the abscissa. Consumption spending, since it is an increasing function of the level of income, is represented by the curve C which rises upwards and to the right, but becomes flatter at the top in accordance with the demands of Keynes' 'fundamental law'. If national income were to drop below the level O_a , total consumption expenditure in the following period would exceed it, for many individuals would start to borrow or live off past savings to maintain something of their accustomed standards of living. At all levels of national income beyond O_a , the level of consumption in one period is less than the level of income of the previous period, since individuals start to save once their incomes rise beyond a certain point. The curve

1. In the long run the main determinants of investment expenditure are two in number, (i) the rate of interest, and (ii) the marginal efficiency of capital. The first of these depends upon the quantity of money (decided by the Central Bank) and the liquidity preferences of investors. The MEC is the rate of discount which would make the present value of the flow of expected returns just equal to the supply price.

The basic factors of the theory are spoken of in the following passage :

"we can regard our ultimate independent variables as consisting of (1) the three fundamental psychological factors, namely, the psychological propensity to consume, the psychological attitude to liquidity and the psychological expectation of future yield from capital-assets, (2) the wage-unit as determined by the bargains reached between employers and employed, and (3) the quantity of money as determined by the action of the central bank; so that, if we take as given the factors specified above, these variables determine the national income and the quantity of employment."

-- J.M. Keynes, op. cit., pp. 246-247 . For the account of the determinants of investment, see pp. 135-174

S represents the level of total saving, and its position is determined at each point by the vertical distance the curve C lies below the aggregate supply function. Finally, the aggregate expenditure function, $C + I$, is represented by a curve lying vertically above curve C by an amount equal to the level of investment. On the graph, the equilibrium level of income, which gives rise in the succeeding period to expenditure O_d , is O_e . At this level $S = I$ and, thus, because there is nothing to cause the flow of income per time period to increase or to decrease, $Y_t = E_{t+1}$.

The level of employment, as previously noted, is a function of aggregate expenditure or demand. To be more theoretically precise, and to use the words of Keynes himself, "the volume of employment is determined by the point of intersection of the aggregate supply function with the aggregate demand function"¹. Since one is interested in the connection between the level of saving and the level of employment, one must therefore investigate how changes in the former effect changes in the level of aggregate expenditure.

If the average propensity to save (APS) were increased for some reason, this would, per definitionem, mean a decrease in the APC.

1. J.M. Keynes, op. cit., p. 89

In figure 7., an increase in the level of total saving - not of course the same thing as an increase in the total quantity saved - is shown

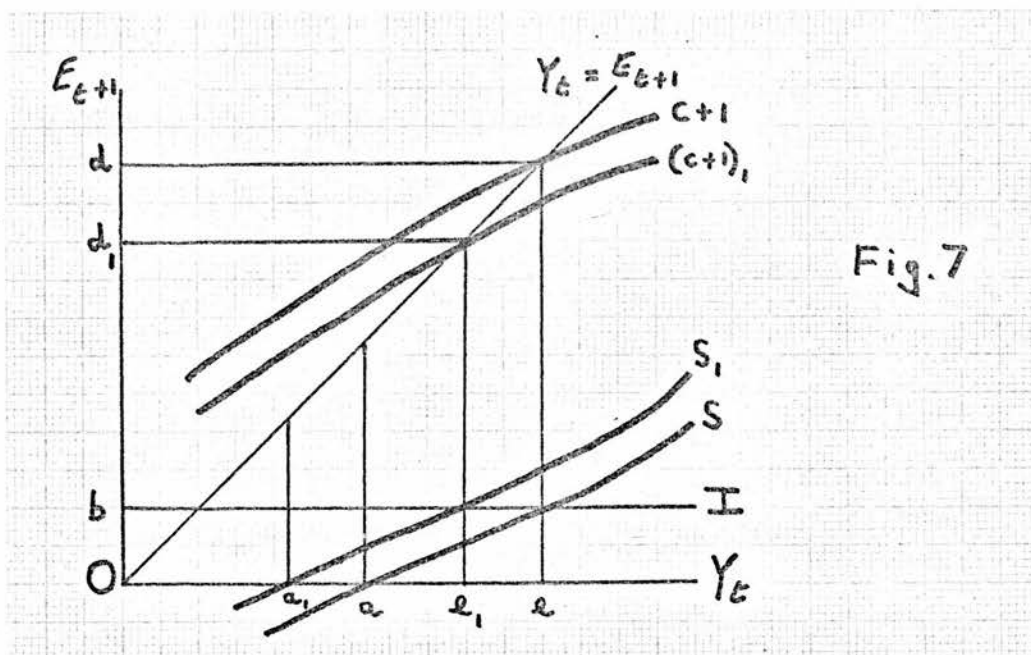


Fig. 7

by moving curve S to position S_1 . When this occurs, the consumption function (omitted) and hence the aggregate expenditure function must fall, in the latter case from $C + I$ to $(C + I)_1$. Consequent upon this, aggregate supply is reduced, and employment falls from the level associated with aggregate expenditure level Od to that associated with level Od_1 .

Thus, given the assumed conditions and circumstances, this analysis demonstrates that the level of employment is to be expected to rise as the level of total saving falls, and to fall as this rises. One now has the answer to the first part of the double question asked at the beginning of this section. The answer is that a change in the level of employment can be expected to result, if a large number of individuals

decide to alter (in the same direction) their private levels of saving ; and that, more specifically, the resultant change in the volume of overall employment, can be expected to take place in the opposite direction to the average alteration of all the individual levels of saving.

To answer the second part of the double question, one simply follows through the line of argument already started. Unless other things do not remain constant, it is not possible (except in the very short run) for a sizeable number of individuals simultaneously to increase the absolute levels of their private saving, by increasing the percentage amounts of their current incomes that are not, within a short time, spent on consumable goods and services. For as soon as a substantial fall occurs in the proportion of the national income that is consumed, this, ceteris paribus, has the effect of putting the circular flow of income into disequilibrium by making the magnitude of the current flow of withdrawals exceed that of the current flow of additions (i.e. $Y_t \neq E_{t+1}$ because $Y_t > E_{t+1}$ because $S_{t+1} > I_{t+1}$). Once the circular flow of income is in disequilibrium, this sets in progress a causal chain that, via the 'multiplier' effect, contracts the volume of the national income (and hence the quantity of employment) per successive time period ; this contraction in volume continues until a new equilibrium position is reached (eg. point Oe_1 , in figure 7.) at which the increased percentage of the national income saved, is equal, in absolute terms, to what it was previously, namely, a flow equal in magnitude, per time period, to the flow of investment expenditure.

If those who suffer (because of unemployment) the decrease in individual incomes consequent upon the fall in the level of national

income, are among those who originally decided to increase the absolute levels of their private saving, then the conscious attempts of those individuals to save more, produces the unintended consequence of their actually saving less in absolute terms. This then, at last, is the phenomenon of the 'paradox of thrift' - intended higher saving results in unintended lower saving.¹ Although its explanation requires a substantial amount of theoretical reasoning, it is hardly open to dispute that this paradox is one of the finest examples, if not the finest example, known to the social sciences, of a set of purposive actions, each one of which is directed towards the achievement of the same goal, producing an unintended repercussion. The consequence which actually results, is the exact opposite of what is deliberately sought by those who for their own reasons initiate the causative actions in question.

Once a social situation has been analysed, and an important feature of it explained as an unintended result of a certain kind of behavioural interaction, the acceptance of what at first may seem a rather surprising account, can have immense results for the general good of society. With reference to the second of this chapter's two examples, if most social scientists had properly understood the 'paradox of thrift' phenomenon during the inter-war years, much of the misery and human wretchedness of that period could, doubtless, have been entirely obviated. A realisation of the causal connections holding between the levels of overall employment, aggregate expenditure and total saving,

1. P.A. Samuelson, op. cit., p. 236ff

would have prevented economists from advising successive British governments to implement schemes to effect savings and cuts in public spending,¹ when unemployment stood as high as twenty per cent of the labour force.

However, even granted that the practical benefits to be gained by making use of social scientific knowledge to undertake judicious and thoughtful reform are not inconsiderable, the 'empirical success' of every sort of social inquiry is widely held to be 'inferior', when compared to the quality of explanatory ability that can be achieved by, say, the science of theoretical mechanics. It is, for instance, frequently asked why economics cannot predict (for a given societal environment) the precise effect on a particular price of a certain change in consumer tastes, in the way physics can predict (for a specified set of conditions) the exact velocity to be reached at a certain time by a particular moving body. Does this (admitted) inability on the part of the social sciences, indicate a deeply rooted deficiency in the methods of explanation hitherto employed, or is it that the idea that this inability is a deficiency is what requires to be corrected? This is the question upon which the discussion centres in the chapter immediately following.

1. A.J. Youngson, Britain's Economic Growth, 1920-1966, (Allen and Unwin, London, 1967), pp. 294-298

III THE EXPLANATION AND PREDICTION OF COMPLEX PHENOMENA

(i) Science and Complexity

One way in which it is philosophically illuminating to classify scientific disciplines is by reference to the comparative simplicity or complexity of their respective subject-matters. From the start, two important points need to be borne in mind. First, within this context the term 'comparative' must be strongly emphasised because no range of phenomena should be considered simple or complex simpliciter ; the distinction between simplicity and complexity should be seen instead in terms of relative positions in a continuum. And secondly, it should be noted that sciences , as opposed to their subject-matters , should be called 'simple' or 'complex' only derivatively ; thus, a simple science is a discipline that deals with a simple subject-matter, while a complex science is one that deals with a complex subject-matter.

Therefore in the first instance, simplicity and complexity should be understood as properties of phenomena, and not as syntactic features of theories developed to explain phenomena. To investigate the wider aspects of this distinction, this chapter has a two-fold task to accomplish : (i) to explicate the concepts 'simplicity' and 'complexity' (as they are applied to the various sorts of explananda-phenomena) , and (ii) to examine the methodological problems encountered in the scientific

investigation of domains of complex phenomena.

In analysing what is properly meant by the phrase 'field of irreducibly complex phenomena', one is concerned not so much with the intrinsic difference between social and natural phenomena, but with what distinguishes the phenomena studied by the 'exact' physical sciences (such as physics and chemistry), from those studied by both the social sciences and the 'inexact' natural sciences (such as meteorology and evolutionary biology). To accept that points of considerable difference are to be found between simple and complex phenomena, does not of course commit one to a scientific metaphysics; indeed, one of the reasons (as will be shown) that the subject-matters of the social sciences are so complex (even more complex than the subject-matters of the biological sciences) is that they deal with actions and other meaningful phenomena. Thus here, while continuing to recognise that human and social phenomena differ from all natural phenomena in the significant fashion stated in the first chapter, attention will be concentrated on the features that the subject-matters of the social sciences have in common with those of many of the natural sciences, and on the features that specifically differentiate such phenomena from those within the domains of inquiry of the exact physical sciences.

The first matter to be considered is the question of the precise way the central, but nevertheless rather vague, notions of

'simplicity' and 'complexity' should be understood. In the second of his two articles on explanation,¹ F.A. Hayek writes:

"The distinction between simplicity and complexity raises considerable philosophical difficulties when applied to statements. But there seems to exist a fairly easy and adequate way to measure the degree of complexity of different kinds of abstract patterns. The minimum number of elements of which an instance of the pattern must consist in order to exhibit all the characteristic attributes of the class of patterns in question appears to provide an unambiguous criterion." 2

It is clear that by 'abstract pattern', Hayek means 'typical structure of causal order', and that by 'minimum number of elements' he means 'minimum number of kinds of determining elements'. The fundamental criterion suggested here by Hayek is certainly not the only way by which the degree of comparative complexity of various sorts of causal formations can be estimated. However, the basic idea contained in this passage serves as an excellent initial position from which it is possible to elaborate further on the nature of the distinction in question. A more extensive analysis of the criteria by which systems of order can be characterised as 'simple' or 'complex', can be provided

1. F.A. Hayek, 'Degrees of Explanation', in The British Journal for the Philosophy of Science, Vol. VI, No. 23, November 1955, pp. 209-225, and 'The Theory of Complex Phenomena', in M. Bunge (ed), The Critical Approach to Science and Philosophy, (The Free Press, New York, 1964), pp. 332-349. Both articles are reprinted with additions in F.A. Hayek, Studies in Philosophy, Politics and Economics, op. cit., pp. 3-21 and pp. 22-42, respectively. All references will be to this last work.
2. F.A. Hayek, Studies, op. cit., p. 25

along the following lines. A domain of inquiry contains phenomena of the simplest degree, if it is the case that:

- (a) Every determining factor (of a specific instance of the sort of phenomena under investigation), is classifiable into one of a small ('conceptually manageable') number of kinds.
- (b) The relevant properties of the referents of each kind of theoretic variable are homogeneous; that is, as far as the theory is concerned, the interacting elements are qualitatively differentiated by the quantitative magnitudes of their pertinent properties (thus, any particular element is substitutable for any other of the same kind that possesses an identical quantitative magnitude) .
- (c) The constituent entities or processes of any aggregate structure are additively (or summatively) organised; hence, any such structure exhibits as its properties only those which can be exhaustively resolved into the quantitative 'relationships of extensionality' holding between its component parts.

By ascertaining how far these three criteria are true of the different types of encountered explananda-phenomena, one is able to produce an objective (i.e. an inter-subjectively acceptable) classification of subject-matters as more or less simple, or as more or less complex.

If these criteria are applied to the phenomena with which it is concerned, classical mechanics appears as the simplest of all sciences. This is easily shown. The first criterion of simplicity requires specific phenomena to be of such a nature that they are amenable to explanation by a complete theory containing a 'conceptually manageable' number of

different variables.¹ (A theory is said to be 'complete' if it takes into account all the various kinds of factors whose interaction is pertinent to the determination of each specific occurrence of the sort of phenomena under investigation.) In Newtonian mechanics, only three kinds of variables² are required, namely, 'mass', 'length' (or 'distance') and 'time'. If one desires to explain the movement of a physical body, then the explanandum-phenomenon is such that nothing other³ than determining factors classifiable into one or other of these

1. This must be modified slightly. In order to satisfy the first criterion of simplicity, phenomena must be of such a nature that they can be explained with a high degree of accuracy under a significant range of conditions by taking into account their dependence upon factors which can be theoretically classified into a small number of kinds, and by neglecting minor disturbances due to the overall influence of elements of other sorts. Thus one finds that even laws relating to simple phenomena are frequently stated with reference to idealizations. Actual deviations from what happens with 'pure cases' of the explanandum-event in question can be made negligible within specifiable circumstances; when deviations occur, however, if they are not due to unnoticed instances of the kinds of variables to be found within the law itself, then they are due to the operation of instances of the kinds of variables to be found within the formal theory from which the law is derived.

It is an unfortunate feature of this discussion that no sooner does one start to analyse one topic, then many others 'crowd in' and demanded to be attended to. One can however only examine one main topic at a time; hence the need to provide constant cross-references.

The use of ideal types in the natural and social sciences is a topic that will be examined in chapter IV, section (iii).

2. N. Feather, Mass, Length and Time, (Edinburgh University Press, Edinburgh, 1959, Penguin, London, 1961), pp. 1-55
3. It is precisely this fact that ensures that explanations within Newtonian mechanics (and other areas of classical physics) are unique among natural scientific explanations, in that they alone can successfully instantiate the strict formal structure demanded by Hempel's original formulation of the deductive-nomological theory of explanation. Therefore, as was mentioned previously in the first section of the first chapter, it is profoundly misleading to treat the 'logic of Newton's Principia' as paradigmatic of the methodological procedures of the natural sciences generally.

three kinds will be found to be at work. Similarly, the second criterion is well satisfied; each member of the set of the small number of variables employed by the science of mechanics, has as its referent a quantifiable and homogeneous element and no further differentiation into types of mass, length or time is ever required. Also, the only qualitative difference (as far as the theory needs to be concerned) between separate factors of the same kind, relates to the quantitative magnitudes of their relevant properties.

Finally, the phenomena studied by classical mechanics satisfy the third criterion of simplicity; it is indeed possible to resolve a mechanistic configuration into a set of constituent relationships, each member of which can be examined in detachment from the rest and then brought together again in a purely summative manner. Suppose one were analysing a system composed of just two celestial bodies; say a sun and its planet; in this case, the relationship holding between them (viz., that given by the inverse square law of gravitational attraction, $F = m_1 m_2 / d^2$) describes the pattern of order that is typical of the aggregate structure so formed. If further bodies were subsequently added, then the relationships between each of them and the rest would be given by the same law (plus the parallelogram rule for the addition of vector forces). Indeed, no matter how many bodies were involved, the general system of order exemplified by the aggregate structure so compounded, would be given by the law describing the relationship between just two bodies. In other words, the whole system would contain no properties that could not be comprehensively resolved into the set of external relations holding between its component

entities. In a mechanistic system, the 'internal' constitutions of the individual parts are not affected by (and hence in turn do not themselves affect) the wider structure of which the parts enter as components; in such a structure, the characteristic type of order that prevails is amenable to portrayal by a theory employing the same number of kinds of variables as required to account for the type of order present when only two entities or processes interact.

So much for the archetypal simple science of classical mechanics; one now has to inquire whether or not these three criteria of simplicity are satisfied by human and social phenomena. If the criteria are not satisfied, then the conclusion must be drawn that the social sciences do indeed deal with phenomena that are intrinsically and inherently complex. Before however one attempts to discern if this is the case, one is duty bound to examine a certain claim that is commonly argued with reference to this very point. The claim is that to suggest that there is any discipline which differs radically from the exact physical sciences in that its subject-matter consists of irreducibly complex phenomena, is to advance something thoroughly 'obscurantist' and 'unscientific'. But just how should a claim to this effect be treated?

Ernest Nagel argues that it is in appearance only that the fields of behavioural and social study are complex. He writes:

".... the complexity of a subject-matter is at best not a precise notion, and problems that appear to be hopelessly complex before effective ways for dealing with them are invented often lose this appearance after the inventions have been made. after Newtonian mechanics was developed, properly trained students were able to analyse motions of bodies that some of the best minds of preceding generations found too complex for human understanding." 1

Adolf Grünbaum is of much the same opinion. He likewise draws upon an analogy between the apparent complexity to those living before the modern era of the subject-matters of the physical sciences and the ease with which one can explain events in these areas now, and between the social sciences now and the social sciences at some future date.² Grünbaum likens the behavioural sciences, which have to cope with a vast proliferation of different factors, to the state of natural philosophy before the rise of modern physical chemistry. The idea that the human sciences deal with intrinsically complex subject-matters, is compared to the notion that it is impossible to produce a theoretical classification of the enormous and seemingly unsurveyable variety of physical substances in terms of a small number of basic atomic elements. Grünbaum's conclusion is thus a curt dismissal of the view that in actuality the phenomena studied by the social sciences are complex - "this argument rests its case on what is not known, and therefore, like all such arguments, it has no case"³.

1. E. Nagel, op. cit., p. 505

2. A. Grünbaum, 'Causality and the Science of Human Behavior' (1952), in H. Feigl and M. Brodbeck (eds), op. cit., p. 770

3. Ibid., p. 770

Although the appeal to the history of physics coupled with a very general prognostication of what will happen to the study of society in the unspecified future gives the argument of Nagel and Grünbaum a certain air of plausibility, the approach is nonetheless fundamentally unsound. In referring to the history of the exact physical sciences in this fashion, these two philosophers are guilty of begging the question at stake, namely whether in fact social phenomena are simple and whether it is (as they imply) only the 'immaturity' of the present state of social inquiry that prevents one realising this today. For unless there are reasons independent of the history of physics for thinking that social phenomena are not complex, the analogy from the development of this simple science to the future development of the social sciences is of little worth. Despite a great show of confidence in their position, Nagel and Grünbaum advance not a single reason (apart from their analogy which, as just stated, presupposes the conclusion to be established) for their judgment that it will be 'only a matter of time' before societal phenomena can be accurately explained by means of theories containing a small and completed number of kinds of variables. What in fact Nagel and Grünbaum do, is to prescribe a priori that all types of phenomena must be essentially simple in nature; but this is nothing more on their part than the forceful assertion of a dogmatic metaphysics. Since all that is known of social reality leads one to the opposite position (this remark will be substantiated in a moment), one is therefore bound to reject the claim advanced by Nagel and Grünbaum on the ground that no relevant evidence is presented in its favour.

Having said this, it is of course incumbent upon one to indicate the respects in which social phenomena fail to satisfy the

criteria of simplicity, and hence the extent to which there are rational grounds for considering such phenomena to be irreducibly complex. First, to take criteria (a) and (b) together, it is manifestly false that human and societal phenomena are brought about by causes which can be classified into a very small number of kinds, and that these factors are both homogeneous in character and always amenable to quantitative measurement. Suppose one takes human actions; events of this sort are brought about by such a multiplicity of separate factors (many of which are non-quantitative or, at any rate, have eluded extensive measurement up to the moment) that a theoretical classification of them into a few homogeneous kinds is just not possible. Moreover, it will be recalled that in the first chapter it was pointed out that the causes of actions do not function under nomological laws, but operate only mediately via interpretations in the minds of those notoriously idiosyncratic creatures, free agents. The fact that men's actions occur as the result of interpretations of environmental conditions rather than as the nomie effects of these conditions, adds immensely to the heterogeneity and immensurability of the determinants of human actions. Taking now societal phenomena, the operative causes of these are the significantly different actions initiated by individuals motivated by their appraisals of their own local situations; such phenomena are therefore not at all like simple physical phenomena which are brought about by homogeneous elements acting according to the parallelogram of forces. In the social sciences, even when (as in economics) one does have a formal theory that is able to classify many of the large number of different actions at work in a 'polycentric system of unintended order' into a small number of

kinds, the variables of the theory are neither complete (hence the constant reliance on a 'ceteris paribus' clause), nor, obviously are their referents homogeneous. Thus, whether one takes actions or societal events as one's explananda-phenomena, by their failure¹ to satisfy the first two criteria alone such phenomena are palpably complex.

Secondly, to examine the extent to which criterion (c) is applicable to social phenomena, it is not the case that the properties of a social aggregate are exhaustively analysable into the external relations between its component agents. For in contrast to what is true of an intrinsically simple constellation, reference to the overall social structure is required in order to explain those features of the constituent agents which dispose them to act the way they in fact do. Society is not composed of the additively organised extensional relations between unsocialised agents,² in the way that a solar system is composed of the additively organised extensional relations between elements whose substantial properties do not depend on the larger structure of which they form the parts. In order to account for the activities whose interactions bring about the overall properties of society, one has to refer to the societal conditions which affect the minds and personalities of those individuals who initiate the pertinent causative actions. The social relations that hold between individuals living together in a society, are therefore not purely extensional but are in addition partly

1. Much of the second and third sections of this chapter will be devoted to presenting a greater elaboration of the reasons why social phenomena fail to satisfy criteria (a) and (b), and to examining the implications of this fact for the study of human and societal phenomena.
2. Reference to this extremely important point will be made again both in the following section of this chapter, and in the second section of chapter V .

constitutive of the participating agents; for they would not be the individuals they are, if they were not part of the wider structure whose specific features form the explananda-phenomena. Indeed, since the relationships that hold between social phenomena are those of mutual interaction and mutual determination, the relationships between the constituent elements (i.e. single individuals and institutions) are no less important than the elements themselves for it is in their inter-relations that social phenomena are what they are.¹ This point does not have to be taken any further at the moment. One ought just to stress that by their failure to satisfy the third criterion of simplicity, social phenomena must again be pronounced 'complex'; the type of order that is characteristic of such phenomena is not resolvable without remainder into the set of external relations holding between its numerous components as these would exist in isolation.

If one accepts that the three aforementioned criteria adequately define 'inherent simplicity', then there can be no doubt that social phenomena are not simple. This conclusion cannot be lightly dismissed as 'subjective'; for provided the criteria are accepted, the intrinsic complexity of human and societal phenomena can be conclusively established.

Classical physics operates by seeking functional connections that can be fully described by two-, three- or four-variable equations; and within a limited sphere (eg. the subject-matters of terrestrial and celestial mechanics, optics, acoustics and

1. L. von Bertalanffy, General System Theory, (Allen Lane The Penguin Press, London, 1971), p. 54

'phenomenological' thermodynamics) the explananda-phenomena are of such a nature that this particular procedure is methodologically appropriate. That this is the case can be traced to the brute fact (for conceivably matters could have been otherwise) that events within these domains just are constituted by (and hence resolvable into) linear causal chains involving the behaviour of isolable atomic units. But on no account should it be considered that unless this state of affairs happens to prevail, scientific inquiry cannot proceed. In areas where phenomena prove to be characterised by an irreducible complexity of relatedness, the scientific approach does not attempt an a priori demonstration to the effect that in actuality no important sorts of phenomena could possibly be intrinsically complex, but openly accepts that many phenomena are indeed as complex as they first appear to be, and then sets about exploring the implications of this fact. And, with reference to social phenomena, it is precisely to this task that one now turns.

(ii) The Analysis of Social Complexity

If one is concerned to explain a specific event within a domain of complex phenomena, the 'empirical success' of one's efforts is likely to be less spectacular than would result if one were to direct one's activities to explaining an event within a domain of simple phenomena. For in order to gain scientific understanding of fields of complex phenomena, one has to work (as a matter of empirical necessity) with theories that have a considerably smaller factual content than

theories of essentially simple phenomena. In Hayek's terminology, which has passed into general philosophical currency, within fields of simple phenomena one is able to achieve 'explanations of detail'; but within fields of comparatively complex phenomena ("where the number of significantly interdependent variables is very large and only some of them can in practice be individually observed"¹) one is able to produce only 'explanations of the principle'². An 'explanation of the principle' gives information on the occurrence of the major instances of the most important kinds of factors that bring about a specific event of a certain sort, but does not (and cannot) provide information on the occurrence of all the separate instances of the complete number of kinds of factors that are relevant to the full determination of the (exact quantitative magnitudes possessed by the) pertinent properties of the specific phenomenon in question.³

Even though Hayek is probably correct to claim that with social phenomena "individual events regularly depend on so many concrete circumstances that we shall never in fact be in a position to ascertain them all"⁴, the division between an 'explanation of the principle' and an 'explanation of detail' is best thought of not as absolute, but rather as one of degree. But to say this, does not make the distinction nugatory; something that is a matter of degree is not, ipso facto, trivial or uninteresting. The mere fact that in borderline cases it is difficult to demarcate decisively these two types of explanatory

1. F.A. Hayek, Studies, op. cit., p. 8

2. Ibid., p. 11ff

3. Ibid., p. 14ff and p. 28ff

4. Ibid., p. 34 (italics added)

procedure, does not render this particular distinction illusory, trifling or unimportant. In practice, the division amounts to a considerable difference in the extent of obtainable 'empirical success'; namely, the difference between being able to explain a specific event only in so far as it is a member of a wide range of occurrences of a certain type, and being able to explain a specific event as a particular instance (or, more accurately, as an instance of an extremely narrow range of particular instances) of the class of events in question. (The parenthesis is required in the previous sentence because, strictly, even Newtonian mechanics cannot produce explanations of specific occurrences that are one hundred per cent accurate; even this discipline, the simplest of all sciences, can produce explanations of specific occurrences just in so far as they fall within a small quantitative interval.) It is, however, somewhat misleading to compare 'explanations of the principle' with 'explanations of detail' in this manner, as if they were in some sense direct competitors and one had to make a choice between the employment of one procedure or the other. 'Explanations of the principle' (which, by reference to just the main causal factors operating in the area under investigation, enable one only to explain "the appearance of a pattern of a certain class"¹ or "the general character of higher-level generalities"²) are not in meaningful competition with 'explanations of detail'. Although, to be sure, an 'explanation of the principle' would give far less in the way of empirical information than an 'explanation of detail' if applied to a simple phenomenon, in areas of

1. F.A. Hayek, Studies, op. cit., p. 24

2. Ibid., p. 29

complex order where 'explanations of detail' can have no application, 'explanations of the principle' constitute the only effective explanatory procedures.

Although the social sciences cannot produce 'explanations of detail', this should lead one neither to disparage the achievable 'explanations of the principle' as contributions to human knowledge, nor to underestimate their value for the successful direction of the actions of those who seek to produce social reforms in a rational manner. The illustrations of social explanations worked out in sections (ii) and (iii) of the previous chapter, demonstrate that the ability of such explanations to reveal the way the social world hangs together is considerable. Although, as will be remembered, the examples did not show how to account for the precise properties possessed by specific events, they showed that it is possible to trace with reasonable effectiveness the main sorts of purposive actions whose interaction gives rise to individual phenomena considered as instances of particular types.¹

Frequently social scientific explanations in indicating just the main instances of the predominant kinds of factors that regularly bring about occurrences of a given sort, pay only scant attention to the fact that phenomena in the social world are complex partly because their substantial properties are dependent, to a greater or lesser degree, on

1. While economic theories, unlike theories developed in other areas of social inquiry, can employ mathematical formalisms to express their empirical contents, this does not mean that economics deals with phenomena that are intrinsically simple. A formal rigorousness of syntactic structure is not to be identified with the ability to produce 'explanations of detail'; whether a science can produce these or not, depends on the intrinsic complexity of relatedness between the causal factors at work in its field of investigation.

the general system of inter-relations into which they enter. Little attention is paid to this aspect of social events, because it is held that to include a 'ceteris paribus' clause in the explanatory account legitimates inter alia the assumption that the extent of mutual determination is small enough to allow the event under consideration to be explained as if there were simply a relationship of linear dependence between it and its operative causes and initial conditions. But is this assumption ever unjustifiable, even taking into reckoning that what one is concerned to produce is an 'explanation of the principle' ?

The answer to this has a bearing of importance on the question of the degree to which 'orthodox' micro-economic price theory is in fact applicable to all the phenomena to which it is seemingly applicable. As will be recalled from the second chapter, when this theory is used to analyse what takes place in the market for a specific type of good when all the consumers and producers of it implement their plans to buy and sell, it is normally supposed that while the ruling price per unit of the good and the quantities bought and sold per time period are endogenous (or 'dependent') variables, such factors as consumer tastes and preferences, production possibilities, the availability of factors of production and all other prices (especially those of substitute and complementary goods) are exogenous (or 'independent') variables. The usual approach of micro-economic theorists - the analysis of a single market on the assumption that the price of the commodity in question can change without causing significant repercussions that reflect back on itself - is known as 'partial (or 'particular') analysis'. 'Partial analysis' (as is illustrated by example (a) in chapter II) proceeds by

abstracting a single phenomenon from the field of mutually determining societal phenomena, and then explaining the event as the primary and the more or less immediate social consequence of the initiation of some set of interacting purposive actions; any other repercussions these actions may have on the markets for other goods, and in particular any reflections from changes in these markets back onto the levels of demand and/or supply of the good whose market is under analysis, are ignored.

If the reverberations of a single change in price are small and widely diffused, then (with reference to the particular market under consideration) it is satisfactory to suppose that there is a rigid distinction between those factors which determine and those which are determined. But for each different market, the comparative adequacy of the 'partial' approach is a purely empirical affair; in order to gauge the degree of applicability of 'partial analysis', one has to have some idea of (i) the strength and range of the repercussions of an adjustment of the pertinent market on other sectors of the economy, and (ii) the ramifications that changes in other sectors have on the market in question. Since all goods are economic substitutes in the sense that they are all competitors for consumers' disposable money incomes, no market can adjust in complete isolation; any one change in price must disturb the prevailing price in at least one other market. But one can be sure that the smaller the effects that result from an adjustment within a market to processes outside it, and the smaller the 'echoes' back again, the more accurate (the 'less inaccurate') will be the 'explanations of the principle' provided by 'partial analysis'.

Because 'partial analysis' only devotes meagre attention to matters arising from the mutual determination of prices, this type of

approach is inadequate to answer the interesting question 'Is there a set of relative prices for an economy such that the total quantity demanded (per time period) of each sort of commodity, just equals the total quantity produced (per time period) of each?' . To reply to this question and to indicate how the details of the set of equilibrium prices might be discerned, one has to turn to 'general equilibrium analysis' (a 'system-analysis' type of approach to micro-economic phenomena pioneered by the economists Léon Walras (1834-1910)¹ and Vilfredo Pareto (1848-1923)²) . In contrast to what is of overriding concern to 'partial analysis' , 'general analysis' is not content to investigate merely the primary effects on a single market of some initial change, but seeks to explain economic phenomena as they occur within a field of price inter-relationships.³ To discover whether there is a set of prices (and if there is, what it is) at which the markets for all goods clear per time period, the 'general' approach aims to establish a system of simultaneous equations showing the mutual interactions holding between the relative prices of all commodities and

1. E. Roll, A History of Economic Thought, (Faber and Faber, London, edit., 1966), pp. 391-394

2. Ibid., pp. 408-414

3. The difference between 'static analysis' (i.e. where one assumes that the market feed-back mechanism is sufficient to restore or replace an equilibrium price lost after a disturbance) and 'dynamic analysis' (i.e. where one tries to show how the variables 'quantity demanded' , 'quantity supplied' and 'ruling price' behave in between the introduction of the original change, and the subsequent attainment, if any, of a new equilibrium position), is a distinction that cuts across the distinction between 'partial' and 'general analysis' .

factors of production.¹

The basic features of this form of analysis are suitably revealed by an examination of a simplified version. Let it here be supposed that (i) the total supply of each type of commodity is fixed and given, and that (ii) the total quantity of every sort of good varies not only with its ruling price, but with the ruling prices of all the other kinds of commodities. Therefore if there are n sorts of commodities, the total demand for any one type is mutually determined by the prices of all of them. If ' D_1, D_2, \dots, D_n ' stand, respectively, for the total quantities demanded of each kind of good and ' p_1, p_2, \dots, p_n ' for their prices, the following equations (or rather, equation-forms) of demand can be constructed:

$$\begin{aligned} D_1 &= f_1 (p_1, p_2, \dots, p_n) \\ D_2 &= f_2 (p_1, p_2, \dots, p_n) \\ &\dots\dots\dots \\ D_n &= f_n (p_1, p_2, \dots, p_n) \end{aligned}$$

By supposition (i), the quantities supplied of each type of good (represented, respectively, by ' S_1, S_2, \dots, S_n ') are known,

1. R. Dorfman, The Price System, (Prentice-Hall, Englewood Cliffs, N.J., 1964), pp. 105-125
- W.S. Vickrey, Microstatics, (Harcourt, Brace & World, New York, 1964), pp. 209-267
- W.J.L. Ryan, Price Theory, (Macmillan, London, 1958, edit., 1966), pp. 241-257

and hence if there is a set of equilibrium prices, they must be those that satisfy the following:

$$\begin{aligned}
 S_1 &= f_1 (P_1 , P_2 , \dots , P_n) \\
 S_2 &= f_2 (P_1 , P_2 , \dots , P_n) \\
 &\dots \dots \dots \\
 S_n &= f_n (P_1 , P_2 , \dots , P_n)
 \end{aligned}$$

But since one has now n simultaneous equations with n unknowns, this gives one a determinate solution; in other words, there is a set of relative prices such that if they prevailed, the whole economy would be in 'general equilibrium' (i.e. would be in that state at which neither a surplus nor a shortage of any sort of good emerged) .

To produce, however, a numerical calculation of what exactly constitutes the set of equilibrium prices is quite another matter. If one were able to collect all the relevant data (concerning such things as consumers' wants, tastes and preferences, the levels of all disposable money incomes, and advances in scientific technology and changes in production possibilities) to enable one to discover the precise properties of the relations ' f_1 , f_2 , \dots , f_n ', and if one were able to gather all this quickly enough to keep pace with the incessant change characteristic of social phenomena, then one would be able to obtain a numerical calculation of what prices would have to prevail if the economy at any particular instant in time were to be consciously placed in the state of 'general equilibrium' . But not only can these antecedent conditions not be satisfied, but also, as Pareto wryly remarks, "si on pouvait vraiment connaître toutes ces équations, le seul moyen accessible aux forces humaines pour les

résoudre, ce serait d'observer la solution pratique que donne le marché"¹. Thus, not only can the requisite numerical data not be collected, but even if one were in a position to formulate the details of the pertinent price equations, the only way they could be solved to ensure that they remained currently relevant to the rapidly changing conditions would be by observing the solution arrived at by the market price mechanism.

On the topic of 'general analysis', the economist Lipsey speaks scathingly. With reference to a theory involving demand equations similar to those just examined, he writes that "in practice such a theory will prove to be useless because we will not be able, within the foreseeable future, to obtain sufficient empirical knowledge of the way in which the demand for any good is influenced by the prices of most other goods"². This judgment however is much too harsh. Certainly, 'general analysis' in taking into account the inter-relationships between the prices in the economy as a whole, does not have the concrete applicability of the 'partial analysis' typically

1. V. Pareto, Manuel d'économie politique, (V. Giard & E. Brière, Paris, 1909), p. 234

The above is the French translation from the Italian (originally published in 1906), and is the most accessible edition of the book for English-speaking readers. The work is renowned because it introduces the use of indifference curves (which were not commonly employed by theoretical economists until the 1930s) to analyse what Pareto calls 'ophélimité' (i.e. the ability of commodities to render satisfaction). In the abovementioned edition, pages 539-671 contain the famous appendix where the theory of 'general equilibrium' is mathematically treated, and where the Paretian system of simultaneous price equations is presented.

2. R.G. Lipsey, op. cit., p. 502 (Lipsey's italics)

employed by 'orthodox' micro-economic price theory. But scientific theories are not only 'tools' ; one of their main sources of value lies in the extent to which they are true of the phenomena whose features they are meant to explain. Although 'within the foreseeable future' one will not be able to use 'general analysis' for explaining in detail the processes by which human actions determine relative prices and the methods and direction of production, the theory serves to illustrate the kind of coherent order that forms itself within the area under investigation. Indeed, 'general analysis' , by illustrating at a high level of generality how a system of ordered relationships forms itself on the principle that all prices influence the demand for any one type of commodity, is a paradigmatic example of a theory providing only 'explanations of the principle' . It thus makes a definite contribution to human knowledge and the scientific understanding of society, even if it cannot be employed in practice to explain specific economic arrangements as anything more than phenomena that do not fall outside the wide range of occurrences decreed by the theory to be empirically possible.

For the vast majority of social scientific investigations, the general field of inquiry is the complex system of order that arises when each individual (and institution) within society is moved to act not merely by his personal relationships with a few others, but, and far more important, by his impersonal relationships to the various resultants of the social interactions of people about whom he knows (and needs to know) nothing specific. It is by means of the enormous amount of information spontaneously generated and impersonally conveyed by

structures such as the price mechanism, that millions of separate individuals and institutions in seeking to satisfy their many different needs and wants unconsciously co-ordinate their various actions. It was maintained in the second chapter that from the point of view of technical efficiency a predominantly polycentric system of economic order in which each individual acts in response to the public indicators of prices, costs and profit rates that emerge as the unintended consequences of the like actions of the other participants within the economy, is superior to a mainly monocentric system in which the activities of each individual are adapted to the actions of those of all the others by a process of conscious co-ordination. This important point will now be discussed further.

If it is to be serviceable in any way, a system of economic organisation must provide some means for deciding three very basic matters, namely, (a) what commodities are to be produced and in what quantities, (b) by whom and with what resources, and in what technological manner goods are to be produced, and (c) for whom goods are to be produced. In order to solve what is sometimes called the 'central economic problem',¹ it is essential that there be some communication device for pooling the requisite knowledge possessed by, and for co-ordinating the purposive efforts of, the millions of diverse individuals within society. To investigate the question of whether a largely monocentric system of economic order is able to handle the task of gathering, collating and then transmitting the necessary information

1. P.A. Samuelson, op. cit., p. 14f

more or less effectively than a largely polycentric alternative, one will here compare the logically conceivable cybernetic capabilities of the respective idealisation¹ of both sorts of system. In other words, one will here undertake a comparative estimate of the complexity of relatedness that would hold between the participants in a completely monocentric system (i.e. a system that makes no use whatsoever, not even in a highly attenuated form, of a price mechanism) , with that which would hold between the participants in a purely polycentric system (i.e. a system in which no individual consciously co-ordinates or directs any actions other than his own) . Thus, in order to settle the question of whether a mainly monocentric system of order is likely to provide a more or less technically efficient solution to the 'central economic problem' than a predominantly polycentric system, one will examine whether the idealisation of the former sort of system would be able to function as a more or less effective cybernetic system than would the idealisation of the latter sort.

A fully developed monocentric system is to be conceived as a hierarchical structure in which information (relating to the various needs, wants and tastes of all individuals, and the available raw materials, human skills, productive resources and means of physical distribution) flows upwards, and in which instructions (relating to the details of how much and which of alternative goods and services are to be produced, their manner of production and from what materials, and in what way the national product is to be distributed among the different

1. As mentioned previously, ideal types will be further discussed in chapter IV, section (iii) .

constituent individuals) are transmitted downwards. At the summit of this hierarchy resides the 'commanding intelligence' that receives information from, and issues specific orders co-ordinating the actions of, his immediate subordinates; in turn, each of them receives information from, and gives appropriate instructions to, their immediate subordinates, and so on right down the pyramid of authority. At the bottom tier, the ultimate subordinates both feed information about their local situations and about their own personal wants to their immediate superiors, and perform the particular tasks these superiors assign to them. Thus, the solution to the 'central economic problem' that is chosen by the central agency after he has collated all the 'bits' of information passed to him through the recognised channels, is then realised by the consciously co-ordinated and directed efforts of the ultimate subordinates. (It is to be noted, that neither unauthorised mutual contacts, nor short-circuits in the upward flow of information, are possible. This means, for example, that no two agents are permitted to undertake man-to-man bartering of their allotted rations of the total output, in the way that soldiers in the army exchange cigarettes for bars of chocolate. Any grievance or dissatisfaction with the operation of the system (or indeed any relevant piece of information) that an agent may happen to possess can be conveyed only to his immediate superior, who can pass it only to his immediate superior and so on until the matter reaches the very top where it is duly considered. If this general condition is not observed, then the system ceases to be purely monocentric.)

In contrast to this state of affairs, under an entirely polycentric system individuals interact with each other on their own

initiatives, subject only to laws which apply uniformly to all. Their actions are not determined by the specific commands of a central authority, but by an impersonal and general form of guidance which operates on each agent when he finds that to earn a living or to obtain the commodities and services he needs and wants, he has to adjust himself to the various societal resultants that emerge spontaneously from the activities of everyone else. Thus in such a system, it is private preferences expressed through the price mechanism and their coming to the attention of profit-seeking entrepreneurs that constitutes the process which solves, more or less automatically and without central direction, the 'central problem' of what is to be produced, how it is to be produced and how the total output is to be shared. But which type of system is the more efficient, and precisely what is the connection between the relative efficiency and the relative complexity of a system of economic relationships?

Let it be supposed that c is the number (assumed to be finite) of distinct orders per time period that the central agency of a completely monocentric system is able to issue in response to information received, and that the 'span of control' (i.e. the 'relations adjustable per time period') of each superior subordinate in the system has as its capacity the number c also.¹ If this span of control is fully utilised throughout, each tier of the pyramid will contain c -times more people than the one above it, and $1/c^t$ of those in the tier below it. Furthermore, if the number of tiers is t , then

1. The argument of this paragraph borrows heavily from M. Polanyi, op. cit., pp. 111-122.

the total number of individuals (N) comprising the elements in the whole system, is given by:

$$N = 1 + c + c^2 + c^3 + \dots + c^{t-1}$$

The important fact however, is not the total number of agents but the total number of relations that are capable of adjustment per time period for each of those situated at the bottom of the chain of command. This number (r), as can be discerned easily by inspection, is given by the formula:

$$r = \frac{c \times (N - c^{t-1})}{c^{t-1}}$$

That is, the total number of relations adjustable per time period for each individual at the base of the pyramid is c-times the number of all those issuing directives of any sort, divided by the number of ultimate subordinates. If one lets c take the value 3, and t the value 4, then this gives a value of 40 for N, and 27 for c^{t-1} ; and if one then substitutes these last two values in the above formula, this gives a value of 39/27 for r. It is also the case that given a value of 3 for c, r takes a minimum value of 1 when t takes the value 2, and as the value of t increases r asymptotically approaches the value 3/2. The exact quantitative values are here unimportant, but to employ a set of them enables one to see that it is a characteristic of the administrative ability of a system of strict monocentric order, that an increase in its size "leaves the number of relations per capita which can be adjusted between the persons whose actions it ultimately

governs, practically unaffected"¹. In other words, as such a structure is extended in size the ratio of the total number of adjustable relations to the total number of ultimate subordinates remains virtually constant.

From the fact that the complexity of relatedness of the channels of contact between agents in a completely monocentric system of economic arrangements is limited by the number of conscious adjustments that the central agency (in response to information fed to him) plus his subordinates (in response to instructions given to them) can effectively make per time period, one is able to draw an inference regarding the operational efficiency of a predominantly monocentric system to the extent to which it is in fact monocentric. Namely, the larger such a system grows and hence the greater the variety of goods and services its members desire, the more inefficient an organisation it becomes, for every increase in size brings with it the necessity to suppress or ignore larger and larger amounts of critical information.

It is precisely because there is no upper limit to the complexity of relatedness that can prevail in a completely polycentric economic system employing a de-centralised method of co-ordinating the actions of its participants, that such a structure is not merely able to achieve a more efficient allocation of commodities and productive resources compared to a purely monocentric system of the same size, but is able to maintain its standard of cybernetic efficiency as it expands. For it is a feature of a fully developed polycentric order (and also of a largely polycentric order to the extent to which it embodies

1. M. Polanyi, op. cit., p. 117 (Polanyi's italics)

polycentric features) that the ratio of the total number of relations to the total number of its participants increases proportionately with the size of the system. This is easily shown. Even if a single individual cannot manage to make more than c conscious self-adjustments per time period, provided that he adjusts his actions rationally (as a consumer or as a producer, or as both) to the impersonal 'signals' of prices, costs and profit rates (that provide 'bits' of information concerning the relative scarcities and surpluses of every sort of good, resource and factor of production), he does in fact, whether he is conscious of it or not, co-ordinate with reasonable effectiveness his own activities with all those which played a part in the spontaneous generation of the 'information bearing indicators' in question. Therefore, although c may indeed be the number of the limit of an agent's acts of conscious adjustment, the total number of relations mutually adjustable per time period through the impersonal functioning of the price mechanism may be hundreds of thousands, or even millions, times c .

If a judgment is based on the degree of technical efficiency with which the vast number of different needs and wants of members of society can be served, it is hardly a matter of great contention to claim that a polycentric structure of social order is decisively superior to a monocentric system. Recognition however of this fact in no way implies, as was well stressed in the second chapter, that therefore one should not consciously strive to eradicate the particular shortcomings, deficiencies and undesirable phenomena that are admittedly present within an economic system based on the free operation of the market price mechanism. It was also emphasised in the second chapter that in order

for any programme of social reform to be rational, there must exist a well corroborated social theory to indicate from what sorts of actions the unwanted phenomena one desires to eliminate result as the (intended or unintended) consequences in the prevailing environmental conditions. But even when one has gained the requisite social generalisations, it should be remembered that the social sciences cannot provide, and never will be able to provide, knowledge of the exact details of all the causes of social phenomena.

If it is accepted that as a matter of fact society is composed of a large amount of spontaneous polycentric order, then, ipso facto, it ought to be clear that social inquirers are effectively prevented from providing 'explanations of detail' in the manner of physical scientists. For in a polycentric social order, the mutually interacting efforts of millions of different individuals (each motivated in the pursuit of his interests by his situational appraisal of his local circumstances of time and place), are too significantly diverse and the complexity of their incessantly changing inter-relationships too great, to be 'captured' in detail by a theory employing a 'conceptually manageable' number of kinds of variables. Social theories, therefore, have no alternative but to employ variables that refer to heterogeneous factors, and to treat the overall structure of relations in which these elements participate as if it were simpler than it in fact is. When all this is understood and the implications for the conduct of systematic inquiry fully grasped, is it not then almost self-evident that within the social sphere empirical necessity dictates that only 'explanations of the principle' can be produced?

(iii) Social Prediction and its Limits

It is a widely held view that the process of prediction is identical in respect of logical status to the process of explanation. That these two processes are formally symmetrical in character is, for instance, asserted by Hempel and Oppenheim who proclaim that "an explanation is not fully adequate unless its explanans, if taken account of in time, could have served as a basis for predicting the phenomenon under consideration"¹. Once the ambiguities of the term 'prediction' have been unravelled, there is indeed a sense of the word for which this claim is largely correct, but only as a matter of fact. For since, under the same meaning of 'prediction', there are occasions where one can explain certain phenomena but cannot predict them, the explanatory and predictive processes cannot be logically identical. This point will be made clear in due course.

An explanation informs one why a known event in the present or a recognised event in the past, occurred. A prediction gives information regarding a phenomenon which occurs in the present but which is as yet undetected, or about an event which is to occur in the future. It can also provide details of a past event whose occurrence has hitherto been unnoticed; a prediction of the past is called a 'postdiction',

1. C.G. Hempel and P. Oppenheim, op. cit., p. 323

or, more commonly, a 'retrodiction'¹. This section, however, will be concerned only to explicate the concept 'prediction of the future'. This notion is ambiguous because it may refer to either (i) a hypothetical prediction,² or (ii) a categorical prediction. If one prefers Popper's language, then one can call these two different kinds of prediction, respectively, a 'technological prediction' and a 'prophecy'³. The former is an assertion that if an antecedent event of a certain sort takes place within a specific type of initial conditions, then this will cause the occurrence of a consequent phenomenon of a particular kind (i.e. 'If p in S, then q'). A categorical prediction, on the other hand, is an assertion that since a certain type of situation is extant, therefore it will, with all likelihood, be followed by a consequent event of a certain sort (i.e. 'Since (if p in S, then q) and p in S, therefore q'). Because a hypothetical prediction is involved in the making of a categorical prediction, one will examine predictions of kind (i) first, and then later analyse predictions of kind (ii).

In the natural sciences, if by 'prediction' one means 'hypothetical prediction', then the explanatory and predictive processes are symmetrical. Thus, if one can offer an exact explanation of the occurrence of a specific event (as in classical physics), or if

1. W.H. Walsh, An Introduction to Philosophy of History, (Hutchinson, London, 1951, third (revised) edit., 1967), p. 41

2. The term 'hypothetical prediction' is taken from:

- M. Scriven, 'Explanation and Prediction in Evolutionary Theory', in Science, Vol. 130, No. 3374, August 28th 1959, p. 477f

3. K.R. Popper, The Poverty of Historicism, op. cit., p. 42f

one can offer a probabilistic explanation of the occurrence of a specific event (as in particle physics), or if one can offer an 'explanation of the principle' of the occurrence of a specific event qua instance of a wide range of possible events (as in evolutionary theory), then in each of these cases one can predict the same. In the social sciences, many predictions are identical in nature to the now familiar 'explanations of the principle'. When however, the predictor is part of the same society to which his predictions refer, he may find that in making ex ante facto

predictions he encounters phenomena which are ineradicably indeterministic¹ despite the fact that once they have occurred they can

1. Indeterminism is the position that determinism is false. Unfortunately, the term 'determinism' is highly ambiguous and stands for a variety of different doctrines, which include: (a) theological determinism, (b) logical determinism, (c) ethical determinism, (d) metaphysical or ontological determinism, (e) scientific or predictive determinism, and (f) methodological or programmatic determinism. The first three are beyond the brief of this discussion. The last is the recommendatory precept that one ought to seek until one finds the complete set of antecedent causes of the explanandum-phenomenon in question. Metaphysical determinism is the position expressed by the statement 'every event has some cause'; in chapter IV, section (i), the logical status of this and other 'mixed quantifier' statements will be examined.

Scientific or predictive determinism is however normally the doctrine at issue (as it is at the present moment) when the general word 'determinism' is uttered. To call a phenomenon 'deterministic' (in this sense) is to claim that it is logically possible to predict its occurrence with any desired degree of precision. This sort of determinism is of course at the basis of the celebrated ('notorious' would perhaps be a more appropriate word) view advanced by the French astronomer and mathematician, Pierre Simon de Laplace (1749-1827). He claimed that given the relative position of every particle in the universe and the laws of interaction between them at any one instant of time, an infinitely powerful mind would be able to predict (i.e. to predict categorically) every event within the bounds of the universe at any time in the future, and also to retrodict the same at any time in the past. Despite the fact that they embody the virtual apotheosis of scientism, Laplace's own words possess an hypnotic fascination that has dimmed little with the passage of time. He writes:

"We ought to regard the present state of the universe as the effect of its anterior state and as the cause of the one which is to follow. Given for one instant an intelligence which could comprehend all the forces by which nature is animated and the respective situation of the beings who compose it -- an intelligence sufficiently vast to submit these data to analysis -- it would embrace in the same formula the movements of the greatest bodies of the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past, would be present to its eyes."

- Pierre Simon, Marquis de Laplace, Essai philosophique sur les probabilités, (1814), trans. from the sixth French edition by F.W. Truscott and F.L. Emory, A Philosophical Essay on Probabilities, (Dover Publications, New York, 1951), p. 4

be satisfactorily explained.

Hypothetical social predictions are used to determine either (a) what (intended or unintended) consequences would result if certain actions were initiated within given societal conditions, or (b) what sort of actions would be required in the given conditions if a desired result is to be produced or if an unwanted situation is to be brought to an end. To begin with, let it be supposed that one faces no greater difficulty in making such predictions than one would if giving ex post facto 'explanations of the principle' .

A 'prediction of the principle' may be controverted on any occasion of application for one or both of the following reasons: (1) the particular social generalisation derived from the formal theory in question may be false, and (2) the initial social conditions explicitly assumed to prevail may not all have been satisfied, and hence either a required necessary condition is lacking, or at least one important operative cause not among those which the theory overtly takes into account is at work. Because social scientific predictions, like social explanations, have to be stated with reference to a 'ceteris paribus' clause, in the event of an apparent refutation the following problem arises. When one is using a theory which enables one to trace only the major instances of the main kinds of causal factors operating in a complex social situation, how does one decide, in the face of a discrepancy between the theory's implications and factual observation, whether to attribute the divergence to temporary causes allowed for by the 'ceteris paribus' clause, or to consider that the divergence falsifies (one or more aspects of) the formal theory employed to produce

the prediction in question? In order to decide whether a particular theory should continue to be accepted (as corroborated) or rejected (as falsified), one has to find a way of distinguishing refutations due to reason (2) from falsifications of an inadequate theory due to reason (1) .

It should be noted that this problem does not exist just for social inquirers (and for those who investigate comparatively complex natural phenomena) ; for even, as mentioned previously, within classical physics one cannot obtain one hundred per cent agreement between empirical observations and theoretical predictions. However, the problem is far more acute for social scientists than for physical scientists. In the social sciences, inquirers are restricted to the employment of 'predictions of the principle' , and this weakens their confidence that they are ever justified in firmly accepting or rejecting the theory from which their predictions were derived. On the other hand, in the exact physical sciences where inquirers can ascertain (and often control as well) all the instances of a small and complete number of kinds of variables, experimental results can frequently be used to give a clear decision on whether a given theory or hypothesis should stand or fall. With a simple physical theory, when one predicts that a specific event will possess a certain quantified property one also, as a matter of strict logical implication, predicts that nothing other than this will occur; thus, the number of possible falsifying instances is extremely large, and hence with a theory of such high empirical content it is not difficult to decide whether one's predictions are correct or incorrect. But in the social sciences there can be no crucial experiments. A social

scientific theory which predicts that an event will fall within a wide range of possible instances of a certain type, logically implies only that an occurrence of a kind outside the expected range will not take place. Here, the number of falsifying possibilities is far smaller, and therefore the social scientist (to a considerably greater extent than the physical scientist) has the difficulty of deciding what is to count as a significant test of his predictions.

A clear-cut solution to the problem of what constitutes a meaningful test of a social prediction would, no doubt, be desirable; unhappily, one has to admit that no answer can be produced that possesses the attribute of authoritative decisiveness. What can be claimed is that if phenomena habitually fall within the expected range, this provides justification for holding that the theory from which the prediction was derived correctly captures the main causal inter-connections operating in the domain under investigation. Similarly, it is justifiable to claim that a social theory is falsified if the divergences between its predictions and the subsequently observed events are persistent and gross. These claims are couched in indefinite terminology, but the lack of precision is unavoidable: it springs from the fact that in the social sciences one deals with 'predictions of the principle' which rely upon 'ceteris paribus' clauses that cannot be unpacked in detail. The inherent unspecificity of these qualifying clauses ensures that expressions such as 'habitual', 'persistent' and 'gross' (when employed to describe the concomitance, or lack of it, between a theory's logical implications and

empirically observed facts), cannot be given precise meanings which would command universal agreement. In the final analysis, therefore, whether a particular social prediction is held to be correct or incorrect is a matter of pragmatic judgment; for each individual case, one must decide by appraisal whether the set of divergences over a period of time from the theoretically expected is 'acceptable' or not. There is no definitive methodological rule.

A little earlier it was asserted that in certain circumstances social phenomena yet to occur are indeterministic, even though there is no difficulty surrounding their explanation after they have occurred. With the explanatory process, one attempts to account for the occurrence of a specific phenomenon by determining its antecedent causes. But since the causes of an explanandum-event lie in the past (either the very immediate past if the event in question lies in the present, or the more distant past if the event itself lies in the past), the set of causes is closed for all time. Even if one is never able to ascertain in detail all the causes of a specific event, anything that was a cause remains a cause, and nothing that was not a cause can subsequently become a cause. The causes of what has occurred or of what is now occurring are totally 'fixed'; thus, the explanatory process seeks to discover the settled causes, whatever they may in fact be, of particular social phenomena. However, since social events are not determined by nomic causation, a prediction concerning a future social occurrence has as its object a phenomenon whose set of causes may prove to contain a number of (social) factors of a quite novel sort.

Because of the openness of the kinds of factors determining future social phenomena, social scientists perpetually face the

possibility that their predictions may become 'self-reflexive'. In the extreme cases, such predictions are either self-refuting or self-fulfilling. With the former, the act of publicising a prediction to the effect that if certain sorts of actions are initiated within a given set of conditions then a certain consequence will emerge, itself becomes a causal factor such that when the antecedent actions take place the expected consequent does not appear, although it otherwise would have occurred. With the latter, the publication of a prediction becomes a causal factor such that when the antecedent actions are initiated the predicted event actually appears, although it otherwise would not have occurred.

Speaking of the ability to possess the property of self-reflexivity, the sociologist Merton claims that "this characteristic of predictions is peculiar to human affairs. It is not found among predictions about the world of nature So far as we know, the meteorologist's prediction of continued rainfall has until now not perversely led to the occurrence of a drought¹ (and) predictions of the return of Halley's comet do not influence its orbit."². In the first chapter of this discussion, good grounds were found for rejecting the scientific approach to social questions. It follows from the rejection of scientism that the reason why self-reflexive predictions are indeed 'peculiar to human affairs' is that the act of making and publicising a prediction is a social event, and as such is not the kind

1. R.K. Merton, op. cit., p. 183

2. Ibid., p. 477

of phenomenon that plays a part in the causal determination of a purely physical event. It is this point that is wholly neglected in the often cited attempt by Adolf Grünbaum to rebut Merton's contention that self-reflexive predictions are 'not found among predictions about the world of nature' .

Against the observations of Merton , Grünbaum asks one to consider a situation in which a computer predicts that, on its present course, a missile will not reach its target, and in which communication of this information to the missile in the form of a new set of instructions causes it to alter its course and thereby to hit its target, contrary to the computer's original prediction.¹ But by no stretch of the imagination is this an acceptable counter-instance, namely, an example of a self-reflexive prediction concerned with a purely physical phenomenon. If one is speaking accurately, then a computer (qua physical object) is no more able to make and disseminate a prediction than a cluster of celestial bodies is able to tell the time; publicising a prediction and telling the time are meaningful activities, and the ability to perform them is only ascribed to physical objects (and hence to human artifacts qua physical objects) metaphorically, and in any case only in so far as a human mind chooses to interpret their physical 'outputs' in a manner that relates to the purposive activity of men. In his final paragraph, Grünbaum concludes by saying that it is unavailing to object to his counter-example on the grounds that it involves the behaviour of an artifact, because "physical artifacts

1. A. Grünbaum, 'Historical Determinism, Social Activism, and Predictions in the Social Sciences', in The British Journal for the Philosophy of Science, Vol. VII, No. 27, November 1956, p. 239f .

fall entirely within the purview of physical laws"¹. But this remark is simply false. The notion of an artifact is only apparently purely physical, but is actually partly intentional. For a description of a machine is incomplete unless it specifies (a) its original purpose and what it can be used for, and (b) its operational and structural principles.² But to describe these, involves the use of intentional concepts which have no place within the formulation of physical laws; a machine, therefore, cannot be described by reference to just the laws of physics (although its functioning is of course founded upon physical laws) . Hence, even if a computer could be said literally to make predictions, Grünbaum's argument would still fail because in order to identify the referent of the concept 'computer' one has to employ ideational criteria, the use of which presupposes a conscious human mind.

If a social scientist makes a prediction from a perspective external to the society to which it refers, then (since the question of self-reflexivity does not arise) no barrier faces him that would not face him if he were intent on producing an ex post facto explanation of the phenomenon under consideration. But if a social scientist makes a prediction not ab extra, but publicly within the society to which the prediction refers and if its dissemination subsequently becomes an

1. A. Grünbaum, 'Historical Determinism, Social Activism, and Predictions in the Social Sciences', op. cit., p. 240 (*italics added*)
2. M. Polanyi, 'On the Modern Mind', in Encounter, Vol. XXIV, No. 5, May 1965, pp. 12-15
- M. Polanyi, The Tacit Dimension, (Routledge and Kegan Paul, London, 1967), pp. 34-42

integral part of the social situation and changes the circumstances under which the prediction originally held true, is there anything (apart from empirical impediments due to the complexity of the subject-matter) to prevent him making an allowance for the effect of the publication and then adjusting the prediction accordingly?

The answer to this is easily given. If the prediction in question and its adjustments are (as can sensibly be conceived) all of an enduring self-reflexive nature and if they are publicly disseminated, then it is the case that the phenomenon-to-be-predicted is indeterministic. Under the conditions just described, it is logically impossible to make a successful prediction. For it is conceptually incoherent to demand that a publicised social prediction take into account the effect of its own dissemination; once the original prediction has been adjusted, the revised prediction has then to be adjusted to allow for the effect of its publication, and so on ad infinitum.

This last point can be shown in a schematic fashion. Suppose that one produces a (first-order) prediction P_1 , thus :

$$P_1 = (\text{If } p \text{ in } S, \text{ then } q)$$

This states that if the set of actions p is initiated in social conditions S , then the consequence will be q . But (it is here

supposed) two assertions are true of P_1 , namely :

- (a) If p takes place and P_1 is not publicised, then q .
- (b) If p takes place and P_1 is publicised, then r .

However, by producing (a) and (b) one has, in effect, produced two (second-order) social predictions. If one reads ' \bar{u} ' as 'is not uttered in public' and ' u ' as 'is uttered in public', then these two predictions run :

$$\begin{aligned}
 P_2(a) &= (\text{If } p + \bar{u}(P_1) \text{ in } S, \text{ then } q) \\
 &= (\text{If } p + \bar{u}(\text{If } p \text{ in } S, \text{ then } q) \text{ in } S, \text{ then } q) \\
 P_2(b) &= (\text{If } p + u(P_1) \text{ in } S, \text{ then } r) \\
 &= (\text{If } p + u(\text{If } p \text{ in } S, \text{ then } q) \text{ in } S, \text{ then } r)
 \end{aligned}$$

But the matter does not stop here. For now it is open to one to express the two statements that on the supposition of persistent self-reflexivity, are true of each of $P_2(a)$ and $P_2(b)$. These are :

- (a) If p takes place and $P_2(a)$ is not publicised, then q .
- (b) If p takes place and $P_2(a)$ is publicised, then s .
- (c) If p takes place and $P_2(b)$ is not publicised, then r .
- (d) If p takes place and $P_2(b)$ is publicised, then t .

And if fully expanded, these four (third-order) predictions appear as

follows :

$$\begin{aligned}
 P_3(a) &= (\text{If } p + \bar{u}(P_2(a)) \text{ in } S, \text{ then } q) \\
 &= (\text{If } p + \bar{u}(\text{If } p + \bar{u}(P_1) \text{ in } S, \text{ then } q) \text{ in } S, \text{ then } q) \\
 &= (\text{If } p + \bar{u}(\text{If } p + \bar{u}(\text{If } p \text{ in } S, \text{ then } q) \text{ in } S, \text{ then } q) \text{ in } S, \text{ then } q) \\
 \\
 P_3(b) &= (\text{If } p + u(P_2(a)) \text{ in } S, \text{ then } s) \\
 &= (\text{If } p + u(\text{If } p + \bar{u}(P_1) \text{ in } S, \text{ then } q) \text{ in } S, \text{ then } s) \\
 &= (\text{If } p + u(\text{If } p + \bar{u}(\text{If } p \text{ in } S, \text{ then } q) \text{ in } S, \text{ then } q) \text{ in } S, \text{ then } s) \\
 \\
 P_3(c) &= (\text{If } p + \bar{u}(P_2(b)) \text{ in } S, \text{ then } r) \\
 &= (\text{If } p + \bar{u}(\text{If } p + u(P_1) \text{ in } S, \text{ then } r) \text{ in } S, \text{ then } r) \\
 &= (\text{If } p + \bar{u}(\text{If } p + u(\text{If } p \text{ in } S, \text{ then } q) \text{ in } S, \text{ then } r) \text{ in } S, \text{ then } r) \\
 \\
 P_3(d) &= (\text{If } p + u(P_2(b)) \text{ in } S, \text{ then } t) \\
 &= (\text{If } p + u(\text{If } p + u(P_1) \text{ in } S, \text{ then } r) \text{ in } S, \text{ then } t) \\
 &= (\text{If } p + u(\text{If } p + u(\text{If } p \text{ in } S, \text{ then } q) \text{ in } S, \text{ then } r) \text{ in } S, \text{ then } t)
 \end{aligned}$$

One does not need to continue this regress of adjusted self-reflexive predictions any further. It should be clear from an inspection of what has been set out, that provided the predictions are actually disseminated (and of course remain self-reflexive), it is indeed logically impossible to make a successful prediction. No ultimate public allowance for the effect of the dissemination of the original prediction can be made, because to achieve this involves the conceptually impossible task of constructing an infinite-order social prediction.

One must now consider the question of how far it is possible (i.e. logically possible) to undertake what was called earlier in this section the 'categorical prediction' of specific social phenomena. A prediction of this type, it will be recalled, is an assertion to the effect that since a particular state of affairs prevails at the present time, therefore it will be followed at a specific time in the future by a

certain consequent state of affairs. In the short-term, the categorical prediction of a particular phenomenon presents no problems in addition to those encountered in attempting to establish a hypothetical prediction of instances of its kind. Once one has established that 'If p in S, then q' is true and that p has come about, one is then in a position to predict categorically the emergence of q. But is long-term categorical prediction of societal phenomena, a suitable aim to recommend for the social sciences?

It has been maintained that unless the social sciences can produce long-term categorical predictions, they do not deserve the title 'sciences'. A statement of this position (to which of course Popper attaches his label 'historicism'¹) is well articulated by Charles A. Beard (1874-1948). 'If a science of society', he writes,

"were a true science, like that of astronomy, it would enable us to predict the essential movements of human affairs for the immediate and the indefinite future, to give pictures of society in the year 2000 or the year 2500 just as astronomers can map the appearance of the heavens at fixed points of time in the future. Such a social science would tell us exactly what is going to happen in the years to come and we should be powerless to change it by any effort of will." 2

An extremely puzzling feature of this passage is that it regards a categorical prediction as an entirely unconditional prognostication. But this is surely not a correct characterisation. In astronomy (to

1. K.R. Popper, The Poverty of Historicism, op. cit., p. 3, p. 41ff et passim
2. C.A. Beard, The Nature of the Social Sciences in relation to Objectives of Instruction, (C. Scribner's Sons, New York, 1934), p. 29. The passage is also cited in E. Nagel, op. cit., p. 460

take Beard's example of a 'true science') it is assumed that the orbital arrangements of the various celestial bodies will continue to hold in the future as they have done in the past, and that no new element (such as a stray comet) will enter the particular system under scrutiny and disrupt the prevailing pattern of movements. A categorical prediction of a specific event is not a forecast made without knowledge of initial conditions, but is a forecast that is conditional upon one's having good grounds for believing either that the initial conditions pertinent to the production of the event in question will remain undisturbed, or that the initial conditions change in ways which can be categorically predicted because they themselves rest upon a set of circumstances which remains unchanged over time. Thus, if one is interested in, say, the relative positions of the planets Mars , Jupiter and Saturn at the beginning of the first second of the first day of the year 2000 , this could be categorically predicted because astronomers have 'good assurance' that the established cyclical pattern of motion within the solar system is likely to remain permanently extant.

However, when it comes to social phenomena, one is unable to have this assurance regarding the initial conditions upon which specific aspects of society in the distant future depend; and one is unable to have this, for reasons that are additional to those stemming from the empirical fact of the inherent complexity of the social world. For once the full implications of the distinction between the social and the natural sciences are appreciated, the demand that long-term categorical prediction be made the aim of the social sciences reveals itself as a

demand that the sciences of society pursue a goal that cannot even be spelled out without conceptual incoherence.

In order to obtain a successful categorical prediction of a feature of society in the distant future (when the set of social conditions presently extant will have altered radically), it would, beforehand, be necessary to produce categorical predictions of what relevant conditions will prevail at the specified time. But because a large and important part of the determining circumstances of future social events will be the results of numerous intellectual inquiries,¹ in order to predict all the requisite initial conditions it is very likely that one would therefore be faced with the task of having to predict both of the following:

- (a) the time in the future that particular scientific discoveries will be made and novel philosophical ideas created, and,
- (b) the exact details of the substantive contents of these discoveries and ideas .

It goes without saying that at the very least such an endeavour is (and will remain forever) well beyond the bounds of empirical possibility. But is there any contradiction in the notion that a predictor (by predicting the brain processes of scientists and others) could learn that a piece of knowledge would be discovered at a certain date, and that its details would be such and such?

1. K.R. Popper, The Poverty of Historicism, op. cit., pp. v-vii
 G.L.S. Shackle, Epistemics & Economics, (Cambridge University Press, Cambridge, 1972), pp. 25-27, p. 272f et al.

Just for a brief moment, it will be assumed that (i) there is nothing incoherent in the concept 'the prediction of a discovery by the prediction of a brain state' , and that (ii) one is discussing a situation in which the predictor publicises his prediction within the social environment to which it refers.¹ If a predictor could predict both (a) and (b) , this would make him, at the time of the achievement of the prediction, the actual discoverer of the piece of knowledge in

1. K.R. Popper, 'Indeterminism in Quantum Physics and in Classical Physics', in The British Journal for the Philosophy of Science, Vol. I, No. 2, August 1950, pp. 117-135 , and Vol. I, No. 3, November 1950, pp. 173-195 .

In this double article, Popper argues that in a society of mutually interacting predictors (either human agents or predicting machines constructed by human agents) it is a logical point that "even if any prediction task is capable of being carried out by some predictor, there will be no predictor capable of carrying out every task" . (p. 193) For in such circumstances, "the future states of one at least of the existing predictors cannot be predicted by any of the existing predictors" . (p. 119) A predictor is necessarily unable to predict every future event within the society of which he is a part (no matter how much he may know of the initial conditions, and no matter how fast he may be able to calculate), because he is logically precluded from predicting (i) his own future predictions, and (ii) the future states of those predictors trying to predict his own future predictions. It is logically impossible for a predictor to predict his own future predictions, since they can only be made by him at the specified time ; they cannot be predicted now (and hence made by him now), and yet remain for him predictions to be made at the allotted time.

While one readily agrees with Popper's argument and conclusion, the situation he analyses is not precisely similar to that under present examination. At the moment one is attempting to discover whether it is logically possible for a predictor (who is not outside the society to which his predictions relate, but interacts with it to the extent that the publication of what he predicts now, affects subsequent developments) to predict not his own future states, but those of others. In these circumstances, the predictor plays no part in shaping the course of events beyond the making and publicising of his single prediction.

question (because he would know all its details before it otherwise would have been discovered). However, provided that the predictor kept this knowledge strictly to himself, no problem of conceptual coherency arises on the understanding that assumption (i) is conceded. But one wants here to assert assumption (ii) as well. And it is obvious that if made public, an initially correct prediction of a discovery (to be made by a member of the society under consideration) would, as a matter of fact, be self-reflexive; the individual whose brain-state had been predicted would learn of the prediction, and thus would himself be unable to produce the piece of knowledge as a discovery at the time the predictor predicted he would (or indeed at any other time). The assertion advanced by the predictor would then cease to say anything at all about the future of the society to which it referred; hence if the predictor should ever gain the opportunity to succeed, this, ipso facto, dooms his effort to ultimate failure. Notwithstanding this, under assumptions (i) and (ii) no conceptual absurdity appears to be involved in holding that the results of intellectual inquiries could be predicted and publicly disseminated, for it is a purely empirical matter that such predictions would become self-reflexive.

But suppose that assumption (ii) is not advanced, and that one is concerned with a situation in which the predictor stands absolutely aloof from the society whose future properties he is anxious to predict. What then is to be made of assumption (i)? Is it indeed even logically possible to obtain foreknowledge of future discoveries by predicting the brain-states of those involved in scientific research and other forms of intellectual investigation? Within a context of discussion different from that of the moment, this last question is examined in a recent

article by Paul Meehl . In this paper, it is envisaged that a "Utopian neurophysiologist studies the brain of a mathematician who is currently working on Fermat's Last Theorem "¹ . (By 'Utopian neurophysiologist' is meant "a man from Mars mysteriously possessed of such a Utopian knowledge of Earthling neurophysiology that he is able, by a combination of behavioral and microtechniques - such as single-unit stimulation and the like - to give a complete causal account, in neurophysiological terms, of all the activities and dispositions of any given member of Homo sapiens"² .) With reference to this imagined set of circumstances, Meehl writes as follows:

" the neurophysiologist in some sense could 'discover' a valid proof of Fermat's Last Theorem without understanding mathematics, by studying the brain of (the) mathematician I readily agree that this sounds counterintuitive. But I do not see anything contradictory about it." ³

The exact details of Meehl's problem and argument are not of concern; what is of interest, is his contention that there is no contradiction in the notion that a discovery could be predicted, if it were empirically possible to achieve a prediction of "the mere graphical residues of a molar class of finger movements"⁴ by means of a further prediction of the antecedent cerebral sequences in the mathematician's brain⁵ .

1. P.A. Meehl, 'Psychological Determinism and Human Rationality', in H. Radner and S. Winokur (eds), Minnesota Studies in the Philosophy of Science, Vol. IV, (University of Minnesota Press, Minneapolis, 1970), p. 351
2. Ibid., p. 349 (Meehl's italics)
3. Ibid., p. 354
4. Ibid., p. 353
5. Ibid., p. 351f

Against Meehl, one can say that his claim only possesses a semblance of conceptual coherence because his approach rests upon scientific preconceptions. But once scientism has been abandoned, it is easily perceived that any argument which has as its conclusion a statement referring to the occurrence of what requires to be identified by intentional criteria, and which has as its premises statements referring only to the occurrence of what requires to be identified by physical criteria, must be fallacious. Therefore, one cannot derive a statement (or set of statements) describing a discovery from a statement (or set of them) describing the purely physical results (eg. ink scratches on paper) of the effector movements of the muscles of a man's hand. In other words, when thoroughly explicated, the concept 'the prediction of a discovery by the prediction of a brain state' proves in its own fashion to be no more internally coherent than the famous concept 'the class of all classes that are not members of themselves'.

If it is logically impossible (as it appears to be) to predict the emergence and the substantive details of future intellectual inquiries, then it is logically impossible to predict the totality of environmental circumstances which might have an influence on social phenomena in the future. From this conclusion, it is an obvious inference that long-term societal prediction (of the sort advocated by Beard in the passage quoted earlier) is not a possible aim for the social sciences, because what is recommended cannot even be stated coherently. To accept, however, that it is methodologically misconceived to seek to produce categorical predictions of events that are to occur within societal conditions that are likely to embody features which necessarily cannot be predicted, in no way detracts from the propriety of formulating

hypothetical social predictions. Indeed, apart from cases where these prove to be persistently self-reflexive, there seems to be no reason to suppose that improvements in the precision of social 'explanations of the principle', should not receive correlative reflection in improvements in the accuracy of just such predictions.

IV PROBLEMS OF THEORETICAL AND IDIOGRAPHIC EXPLANATION(i) The Structure of Formal Theories

Social scientists (like natural scientists) are not interested in accumulating unconnected single facts ; it is part of the essence of scientific investigation that inquirers be concerned with systematic research into the causes of specific explananda-phenomena. One of the goals of social scientific activity, therefore, is to ensure that what are offered as explanations of particular events are related, even if only to the minimum extent of giving mutually consistent accounts of the individual phenomena within a given domain of inquiry. It has already been noted that economics is the only social science whose component statements are 'rigorously' related in that they form systems of deductive connections. This chapter, accordingly, will be devoted first to examining a number of central methodological problems which are pertinent to the theoretical explanations provided by economics, and, secondly, to discussing some further questions of philosophical importance which concern the idiographic explanations provided by the other social sciences.

At the present time, the most widely accepted position regarding the nature of scientific theories is the one Herbert Feigl

has recently dubbed "the standard account of the structure of scientific theories"¹. This influential view (which seeks to model empirical science on formal logic) is maintained by many philosophers, including Hempel², Nagel³, and Rudner⁴. In brief outline, the logical analysis of the structure of formal theories (whether natural or social scientific) promulgated by this account, is as follows. It is claimed that every scientific theory is properly regarded, in the first instance, as a purely syntactic calculus consisting of a deductively organised network of uninterpreted symbols. Each constituent symbol is either a 'primitive term' of the system, or is the subject of an implicit definition (i.e. is defined by reference to a number of the primitive terms). Although these symbols are totally devoid of any meaningful content, they are arranged into various permissible permutations called 'well formed formulas' (or 'wffs' for short) by preassigned 'formation rules'. A small number of wffs are designated as 'axioms'; and to these axioms 'transformation rules' specifying the accepted rules of deductive inference are applied in order to derive some (but not all)

1. H. Feigl, 'The 'Orthodox' View of Theories', in M. Radner and S. Winokur (eds), op. cit., p. 3
2. C.G. Hempel, 'The Theoretician's Dilemma', in H. Feigl, M. Scriven and G. Maxwell (eds), Minnesota Studies in the Philosophy of Science, Vol. II, (University of Minnesota Press, Minneapolis, 1958, edit., 1967), pp. 37-98
C.G. Hempel, 'On the 'Standard Conception' of Scientific Theories', in M. Radner and S. Winokur (eds), op. cit., pp. 142-163
3. E. Nagel, The Structure of Science, op. cit., pp. 90-105
4. R.S. Rudner, Philosophy of Social Science, (Prentice-Hall, Inc., Englewood Cliffs, N.J., 1966), pp. 10-53

of the possible wffs as 'theorems' . Finally, the syntactic calculus of uninterpreted formulas becomes an empirical theory when 'rules of correspondence' are found which link the primitive terms in the axioms (and in the formulas derived from them) with classes of directly observable phenomena.

The proponents of the 'standard' conception take care to stress that the account they give is intended as an analysis of the structural characteristics of existing theories, and should be construed neither as a description of the historical process of the formation of theories nor as a recipe for the construction of new theories.¹ This point is understood and accepted, but even as a hindsight reconstruction of the logical features of existing formal theories the analysis is highly unsatisfactory.

The 'standard' account, as is openly admitted by those who advance it, rests upon a "sharp distinction between the language of observation (observational language; O.L.) and the language of theories (theoretical language; T.L.) "² ; in other words, on this distinction depends the contention that an uninterpreted syntactic structure is conceptually prior to an 'empirical interpretation' , and that hence every explanandum-phenomenon of every theory can be identified by criteria which are completely 'uncontaminated' by conceptual notions stemming from the empirical theory which is to be used to explain the events in question. However, since there are and can be no such things as uninterpreted observations (one cannot simply observe, one must observe something; and in order to be able

1. H. Feigl, 'The 'Orthodox' View of Theories', op. cit., p. 13
E. Nagel, The Structure of Science, op. cit., p. 90
R.S. Rudner, op. cit., p. 18
2. H. Feigl, 'The 'Orthodox' View of Theories', op. cit., p. 7

to observe something, one requires some idea of what is significant and what is not), a rigid distinction between an O.L. and a T.L. cannot be substantiated. And granted this, it is evident that the 'standard' conception cannot be a correct account of the formal structure common to all empirical theories. For it is the case that numerous explananda-phenomena of many theories can only be identified after (a tentative version of) the theory in question has been formulated. Thus, although in the realm of formal logic it may always be possible to identify the 'elements' to be connected (eg. propositions, classes) by criteria extant in 'ordinary language', in the field of empirical science an account which allows for the employment of only those criteria of identification which can be found wholly apart from the theory to be established, cannot be accepted as an adequate analysis of the structural properties of scientific theories in general.

The rejection of the view that scientific theories are uninterpreted calculi which have had an empirical interpretation bestowed upon them, does not of course entail the rejection of the position that many theories are correctly viewed as systems of deductively related statements (and by 'statements' is meant 'meaningful assertions' or 'hypotheses'). It should be recognised however, that this characterisation is not true of all formal theories, for there are varying degrees of formalisation. In the terminology introduced in the first chapter, a 'rigorous'

formal theory (eg. Newtonian mechanics and micro-economic price theory) possesses a well articulated deductive structure, while a 'less rigorous' formal theory (eg. Darwinian evolutionary theory or Freudian psycho-analytic theory) has to depend for structural coherence more on informal connections between its constituent statements than on established deductive relations.

An analysis of the main components of a formal theory should commence by remarking that every scientific theory has to rest upon some ultimate metaphysical presupposition as to the inherent nature of the phenomena comprising the subject-matter in question, and hence as to the appropriate mode of explanation to be employed. There is no need to rehearse here the arguments amplifying this point, because they were brought forward in depth in an earlier chapter of this discussion. At the moment it is sufficient to repeat that if one is interested in constructing, say, an economic theory, then one's efforts must be based on the understanding that the explananda-phenomena under investigation require to be identified by criteria employing intentional concepts, and thus require to be explained in a manner appropriate to their nature as social phenomena. It may well be suggested that ultimate presuppositions are better regarded not as concrete components of scientific theories, but as necessary pre-conditions for the formation of theories. Whether this is so or not, is a matter of no great consequence; what is of importance is the fact that the business of theory-construction can only begin once an inquirer has adopted some initial theoretic conception (which may of course have to be abandoned in time in favour of some other) as to the kind of phenomena to be encountered within the given field of events.

If questions relating to the logical status of ultimate presuppositions are placed aside, the following types of statements (frequently confounded) can be discerned to constitute the axiomatic principles of a formal scientific theory: (a) fundamental nomological laws (if the theory pertains to natural phenomena), or basic social generalisations (if the theory pertains to human or societal phenomena), (b) analytic statements (true as a matter of logical necessity), and (c) foundational statements of 'mixed quantifier' form. From these three sorts of statements, there are deduced (if the theory in question is 'rigorous') or informally inferred (if the theory in question is 'less rigorous') the laws or social generalisations which are the main elements in theoretical explanations of specific phenomena.

With economic theories, the empirical postulates (statements of sort (a)) refer to human goals, to the means agents regularly choose to achieve their desired ends, and to features of the types of environmental circumstances within which the identified agents habitually operate. Examples of such statements are: 'consumers aim to maximise their satisfaction', 'suppliers aim to maximise their profits', 'human agents are rational and will pursue their desired objectives in the most technically efficient manner available to them', 'labour and capital factors of production are completely mobile' and 'markets are perfectly competitive'. (Inter alia, the following three sections of this chapter will examine a number of the central questions which arise in connection with the status and role of such statements.) An example of an analytic constituent (a statement of sort (b)) of an economic theory is the statement ' $Y_t = S_{t+1} + C_{t+1}$ '; it

will be recalled from the second chapter that this particular statement is to be found in the explanation of the 'paradox of thrift' phenomenon. Since, as far as the pertinent macro-economic theory is concerned, the assertion (implicitly) defines what is to be called 'national income', the statement 'the national income of one period is equivalent to the sum of the saving and the consumption expenditure of the following period' is a logically necessary truth. Provided that analytic statements such as this are not confused with statements of either of the two other sorts, their use within scientific theories presents no special problems. All that has to be understood is that statements of sort (b) do not convey anything factual, but function within a theoretical structure as (implicit) criteria of identification of what are to count as the referents of the terms they contain.

Somewhat more difficult to characterise than empirical and analytic statements, are statements of sort (c). If by the word 'empirical' one means 'testable', 'falsifiable' or 'incompatible with some conceivable observational report', then it is just not the case that the categories 'empirical' and 'analytic' are able to classify exhaustively every statement which may be found among the axiomatic principles of a scientific theory. For between statements which are empirical and those which are

analytic, there are a number which fall into a twilight zone which lies in the middle.¹ These are statements which are factual (for they are not consistent with the descriptions of all logically possible states of affairs) but which are nevertheless non-empirical (for they are consistent with all imaginable observational reports) . To this group belong 'mixed quantifier' or 'all-and-some' statements, examples of which are: 'every event has some cause' (the thesis of 'ontological determinism'), 'every social phenomenon is the intended result of some consciously designed plan' (the 'conspiracy theory of society'), 'every social institution serves some functional consequence' (the 'postulate of universal functionalism'²) and 'the quantity demanded and the quantity supplied of every good are some respective functions of the ruling price' (the skeletal structure of micro-economic price theory) . On account of their untestable nature, J.W.N. Watkins labels statements of this kind 'metaphysical'³ ; however, since this expression has been used previously to describe the ultimate

1. J.W.N. Watkins, 'Between Analytic and Empirical', in Philosophy, Vol. XXXII, No. 121, April 1957, pp. 112-131
 J.W.N. Watkins, 'Confirmable and Influential Metaphysics', in Mind, Vol. LXVII, No. 267, July 1958, pp. 344-365
 J.W.N. Watkins, 'When are Statements Empirical?', in The British Journal for the Philosophy of Science, Vol. X, No. 40, February 1960, pp. 287-308
2. The thesis of universal functionalism will be discussed in chapter V, section (iv) .
3. J.W.N. Watkins, 'Confirmable and Influential Metaphysics', op. cit., p. 344 et al.

presuppositions of scientific inquiries, statements of 'mixed quantifier' form will be called (for the purposes of this discussion) 'foundational statements' .

It is a characteristic of foundational statements that they can be neither conclusively verified nor conclusively falsified. Take the assertion 'every social phenomenon is the result of some design' . In the symbolism of the first-order predicate calculus, this is written as $'(x)(Ey)(Sx \supset (Py \ \& \ C(x,y)))'$ - that is, 'for all x there is a y such that if x is a social phenomenon, then y is a set of purposive actions and x stands in the relationship 'is the directly intended consequence of' to y ' . It can be seen by inspection that the symbolic translation contains both a universal and an existential quantifier; since whatever falls within the scope of the former is unverifiable and whatever falls within the scope of the latter is unfalsifiable, it follows that a union of what falls within the scope of each of these operators is both unverifiable and unfalsifiable. Nevertheless, despite the fact that if considered on its own as a single isolated assertion the statement under scrutiny (along with all other 'mixed quantifier' statements) cannot be formally demonstrated to be either true or false, it is open to an informal 'test of acceptability' when considered as an integral part of a formal theory which is itself open to empirical refutation. Although the statement 'every social phenomenon is the result of some design' cannot be formally disproved, if numerous empirical hypotheses of the form 'this social phenomenon is the intended result of the actions initiated at time t by agents a ' (constructed by circumscribing the existential component of the original assertion) are convincingly

falsified when tested, the fact of their refutation could not but cast grave discredit on the foundational statement itself. And indeed, it is because empirical conjectures of precisely this form cannot survive rigorous testing, that the conspiracy theory of society is held in wide disrepute. Since social theories employing the concept 'unintended consequence' have been shown to possess explanatory abilities vastly superior to those theories built around testable conspiracy hypotheses, one is quite unable to accept that the foundational statement lying behind such hypotheses is true (even though it itself cannot be definitely refuted) .

Similarly, it is the case that on their own both 'the quantity demanded of any good is some function of the ruling price' and 'the quantity supplied of any good is some function of the ruling price' are non-empirical and hence cannot be formally proved or disproved. However, these foundational assertions are subjected to a 'test of acceptability' whenever falsifiable hypotheses specifying in what way the quantities demanded and supplied of a particular sort of commodity are functions of its price are created and submitted to empirical tests. In this instance, because very many hypotheses of the form 'the quantity demanded of commodity c is a function f of the ruling price and the coefficient of price elasticity of demand is of value u ' and 'the quantity supplied of commodity c is a function f_1 of the ruling price and the coefficient of price elasticity of supply is of value v ' are well corroborated, their respective foundational statements are accepted as true.

Before this section is concluded, a final comment is required; namely, that statements of types (b) and (c) are only

of scientific value in so far as they are combined in a coherent fashion with statements of type (a) to form a comprehensive theory. A formal structure composed entirely of analytic and 'mixed quantifier' statements cannot possess any explanatory power because all observable states of affairs are compatible with it; since it 'forbids' nothing, it can explain nothing. To be effective as an explanatory account, it is necessary for a theory to contain at least one statement which is unambiguously of type (a). The point may be trivial, but its triviality does not prevent it being of crucial importance.

(ii) Testability and Economic Assumptions

Because its subject-matter is comprised of complex phenomena, an economic theory is able to indicate only the main instances of the predominant kinds of causal factors which regularly bring about (intended or unintended) consequences of certain sorts. Since this is so, in the event of a controversion of a (hypothetical, non self-reflexive) prediction as to the result of the initiation of a particular set of actions, how does one decide whether to attribute responsibility for the divergence to temporary causes allowed for by the 'ceteris paribus' clause, or to consider that the discrepancy falsifies one or more of the empirical postulates of the theory? This question was raised in the final section of the previous chapter, and there (as will be remembered) it was found that on account of the ineliminable unspecificity of the 'ceteris

paribus' clause qualifying every 'prediction of the principle', the matter admitted of no definitive solution. In practice, can this problem ever be obviated or side-stepped? More precisely, is there any impediment to prevent one testing an economic theory by testing the truthfulness of its empirical postulates instead of, or in addition to, testing the veracity of its logical consequences?

Before one can decide if there is any practical barrier, one must examine a more fundamental issue; namely, whether or not it is methodologically illegitimate to attempt to test a theory 'directly' (by testing its empirical postulates) rather than, or as well as, 'indirectly' (by testing its derived implications) because it is impossible in principle to test a theory by means of the former method. Prima facie, the claim that in principle a theory cannot be tested by testing its postulates seems to be patently incorrect. In the first place, it is a matter of simple logic that if a theorem (a law or a social generalisation) is validly deduced from a set of empirical postulates (in conjunction with a number of analytic and foundational statements) and if the postulates are tested and found to be true, then the whole theory has been tested and the theorem shown to be true even though the latter element has itself not been subjected to an empirical test. A second pertinent consideration is that no statement is a postulate or a theorem absolutely, but is such only with reference to the position it occupies within the framework of a given theory. Since a statement which is a postulate from the point of view of one theory may be a theorem from another and vice versa, the contention that a theory cannot in principle be tested directly entails an immediate and obvious paradox. Finally, since every statement

implies, and is implied by, itself, the claim that only a theory's logical implications but never its empirical postulates can be tested, appears to border on the incoherent.

In a very well known and frequently quoted essay entitled 'The Methodology of Positive Economics', the eminent economist Milton Friedman seeks to establish the proposition that "the only relevant test of the validity of a hypothesis is comparison of its predictions with experience¹ (and hence) a theory cannot be tested by the 'realism' of its 'assumptions' "². This claim is amplified in the following passage:

"The difficulty in the social sciences of getting evidence and of judging its conformity with the implications of the hypothesis makes it tempting to suppose that other, more readily available, evidence is equally relevant to the validity of the hypothesis - to suppose that hypotheses have not only 'implications' but also 'assumptions' and that the conformity of these 'assumptions' to 'reality' is a test of the validity of the hypothesis different from or additional to the test by implications. This widely held view is fundamentally wrong and productive of much mischief." ³

Since Friedman here contrasts the 'implications' of a theory with its 'assumptions', it is natural to take him to be using the latter expression to denote the empirical postulates from which a theory's numerous implications can be deduced. If the term 'assumption' is understood in this sense, then Friedman's contention is that a theory cannot be tested by ascertaining to what extent its (empirical)

1. M. Friedman, 'The Methodology of Positive Economics' (1953), in M. Brodbeck (ed), Readings in the Philosophy of the Social Sciences, (Macmillan, New York, 1968, edit., 1969), p. 512 (Friedman's italics)

2. Ibid., p. 523

3. Ibid., p. 516 (Friedman's italics)

postulates are 'realistic' (i.e. true) .

Having expounded the kernel of his thesis, Friedman proceeds immediately to outline exactly what 'mischief' he thinks has in fact resulted from the disregard of his own methodological precept. He continues:

"The theory of monopolistic and imperfect competition is one example of the neglect in economic theory of these propositions. The development of this analysis was explicitly motivated, and its wide acceptance and approval largely explained, by the belief that the assumptions of 'perfect competition' or 'perfect monopoly' said to underlie neoclassical economic theory are a false image of reality. And this belief was itself based almost entirely on the directly perceived descriptive inaccuracy of the assumptions rather than on any recognised contradiction of predictions derived from neoclassical economic theory."¹

Friedman believes that his methodological maxim is applicable not only to social scientific theories, but to scientific theories generally. However, since the theory of imperfect competition is the only example he actually provides of a theory which was created in order to satisfy principles he considers methodologically inadmissible, it is worth exploring in depth what Friedman has to say on this point. In the aforementioned passages (taken together) there are two separate claims: (a) the theory of imperfect competition was constructed because the central postulate of the theory of perfect competition was 'directly perceived to be descriptively inaccurate' , and (b) it is methodologically illicit to question the standing of a theory on the grounds that its premises present a 'false image of reality', because in principle a theory cannot be tested by testing the truthfulness of its

1. M. Friedman, op. cit., p. 517

assumptions. These two propositions will now be examined in turn.

The theory of imperfect (or monopolistic) competition is the branch of micro-economic price theory which traces the consequences of the actions initiated by producers to achieve profit maximisation in conditions where each producer faces a level of demand (for his output) which has a coefficient of price elasticity of less than infinity. The theory first appeared in 1933¹ with the publication of the book The Economics of Imperfect Competition² by Mrs. Joan Robinson (1903-) . Mrs. Robinson at Cambridge (where she had been a student under Arthur C. Pigou (1877-1959) , who had himself been a student of the economist Alfred Marshall (1842-1924)) was one of the two originators of the theory which is the direct object of Friedman's criticism. (The other originator, who developed a similar theory independently on the other side of the Atlantic, was the Harvard economist Edward H. Chamberlin (1899-) . His views appeared, also in 1933 , in a work entitled The Theory of Monopolistic Competition³, which was based on a dissertation presented to Harvard in 1927 .)

1. P.C. Newman, The Development of Economic Thought, (Prentice-Hall, New York, 1952), pp. 304-313
J. Oser, The Evolution of Economic Thought, (Harcourt, Brace & World, Inc., New York, 1963), pp. 297-308
2. Joan Robinson, The Economics of Imperfect Competition, (Macmillan, London, 1933, second edit., 1969) .
3. E.H. Chamberlin, The Theory of Monopolistic Competition, (Harvard University Press, Cambridge, Massachusetts, 1933, eighth edit., 1966) .

In her book, Mrs. Robinson gives her considered view of the theory of perfect competition. She writes:

"The traditional assumption of perfect competition is an exceedingly convenient one for simplifying the analysis of price, but there is no reason to expect it to be fulfilled in the real world. It depends, in the first place, upon the existence of such a large number of producers that a change in the output of any one of them has a negligible effect upon the output of the commodity as a whole, and it depends, in the second place, upon the existence of a perfect market. The first condition may often be approximately fulfilled, but the existence of a perfect market is likely to be extremely rare in the real world." ¹

Thus, for the assumption of perfect competition to be a true characterisation of a given economy at a particular time, there have to be (i) a large number of producers involved in supplying the aggregate quantity of each type of good, and (ii) a perfect market for each different type of good.² (The phrase 'perfect market' is a technical expression used by economists to describe the market for a particular commodity if it is the case that "the customers who make up the market all react in the same way to differences in the prices charged by different sellers"³ .) At the time of Mrs. Robinson's investigation, although the first condition was fairly well satisfied, the composite assumption of perfect competition was held to be false because the second condition was not even

1. Joan Robinson, op. cit., pp. 88-89

2. It is to be observed that under perfect competition (i.e. when conditions (i) and (ii) above are fulfilled), no individual producer is able by his own efforts to influence the selling price of the commodity he manufactures. That is to say, when perfect competition prevails any one producer is able to sell as much as he pleases at the current market price; if he lowers his price (by however little) he will be able to capture the whole market for himself, while if he raises his price (by however little) he will forfeit the whole of his sales.

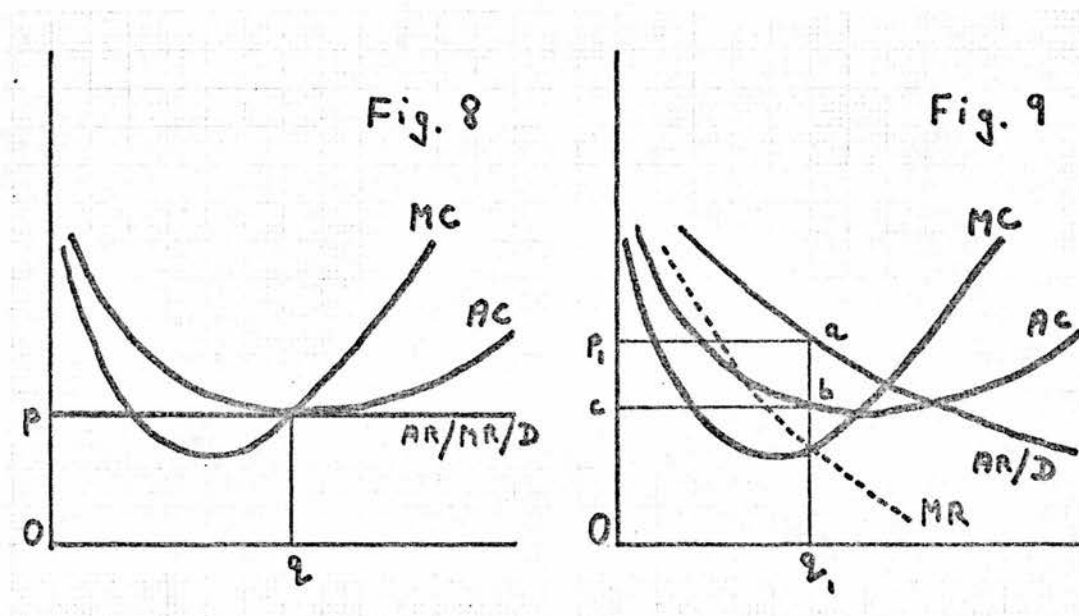
3. Joan Robinson, op. cit., p. 89

approximately fulfilled. The fact that Mrs. Robinson argues along these lines, appears at first sight to confirm Friedman's claim as to what originally prompted the creation of the theory of imperfect competition. However, Mrs. Robinson not only attacks the main premise of the theory of perfect competition on the ground that it is (to use Friedman's expression) 'descriptively inaccurate', but she also gives an additional reason why the premise in question should be abandoned. This second reason is to be found in the preface to her second edition (i.e. the edition of 1969), and will be examined shortly.

In both the theory of perfect and of imperfect competition, it is a theorem (derivable from the postulates 'suppliers aim to maximise their profits' and 'human agents are rational and will pursue their objectives in the most effective manner available to them') that every supplier will produce that level of output at which his marginal revenue is equal to his marginal cost. For a producer, this level is always the 'optimal' or 'equilibrium'¹ level of output. A producer's equilibrium level of output however, does not always coincide with the level of output at which the average cost per unit of output is at a minimum. An entrepreneur will only supply this latter level of output as his equilibrium level if he faces a perfectly competitive market, i.e. if he faces an infinitely price elastic level of demand for the units

1. In the theory of the firm, a single firm is said to be in 'equilibrium' when there is no tendency for it to alter its level of output; an industry (i.e. a group consisting of all the firms producing goods of the same sort) is said to be in 'equilibrium' when there is no tendency for the number of its constituent firms to change.

he produces of the type of commodity in question. This is the situation depicted by figure 8.¹ where quantity Q_0 is supplied at price P_0 per unit; here, the set of circumstances resulting from the actions of consumers and producers is such that for any



single producer the level of output at which MR is equal to MC, is also the level at which AC is at a minimum. In addition, if a supplier faces a perfectly competitive market, then: (a) he would bankrupt himself if he adopted any other level of output because, as can be seen by inspection, at any other level of output total revenue (ruling price x the quantity produced) is less than total cost (average cost x the quantity produced); and (b) his overall

1. In figures 8. and 9. : 'AC' stands for 'average cost', 'MC' for 'marginal cost', 'AR' for 'average revenue', 'MR' for 'marginal revenue' and 'D' for 'level of demand facing individual firm'; price/cost is measured up the ordinate, while quantity demanded/supplied is measured along the abscissa.

profit would be no greater than what is required to service his capital (for if his profit were any larger, other individuals would be attracted into his line of business; and the consequent increase in the aggregate supply of the good in question, would depress the market price until his profit margin ceased to attract further entrants into the industry) .

In the preface to her second edition, Mrs. Robinson points out that it is a deductive consequence of the theory of perfect competition that "any plant that (is) working at all must be working up to capacity"¹ . (By the expression 'up to capacity' , Mrs. Robinson means 'up to that level of output at which the AC per unit produced is at the lowest level'² .) But in the inter-war years (by October 1932 the script of the first edition had been completed) Mrs. Robinson felt compelled to construct an alternative theory of the firm "to explain the fact, in the world around us, that more or less all plants were working part time"³ . To account for this phenomenon, Mrs. Robinson had to reject the postulate of perfect competition (which requires, as is illustrated diagrammatically in figure 8. by the horizontal position of the demand curve, that every firm faces a situation in which the demand for its particular units of output is infinitely price elastic) . Thus, it is evident that Mrs. Robinson rejected the theory of perfect competition not only because she found that its central assumption was false, but also because she discovered

1. Joan Robinson, op. cit., p. vi

2. Ibid., pp. 96-97

3. Ibid., p. vi

that a number of its logical implications were empirically refuted.

In place of the assumption of perfect competition, Mrs. Robinson embraced the postulate 'all markets are imperfectly competitive'. Accordingly, she proceeded to revise the theory of the firm in order to accommodate what this new premise entailed, namely "the notion that every firm is facing a falling demand curve for its own product"¹. The theory of imperfect competition states that if the actions of consumers are such that each individual producer faces a level of demand which is less than infinitely price elastic, and if suppliers operating within such circumstances seek to maximise their profits (and rationally pursue this end by producing the level of output which equates MR with MC), then two consequences of importance follow. First, each producer's equilibrium level of output will be below that at which the AC per unit of output is at a minimum, and, secondly, the ruling price of each unit of the sort of commodity in question will be higher than it otherwise would be. Both these consequences are illustrated by figure 9.²; here, the producer's equilibrium position is such that the quantity supplied (i.e. Oq_1) is less, and the ruling price (i.e. Op_1) higher, than would be the case if the same producer were facing a level of demand which was infinitely price

1. Joan Robinson, op. cit., p. vi

2. Also in figure 9., the area p_1abc represents the 'extra' profits (i.e. those additional to the 'normal' profits required to service the capital employed in the process of production) which accrue to a firm if it has a semi-monopolistic position in the market for the kind of good it produces.

elastic (as can be checked by comparing the situation depicted by figure 9. with that depicted by figure 8.) .

To recapitulate: the assumption of imperfect competition was adopted not only because the assumption of perfect competition was found to be false, but also because the former premise permitted the construction of a theory which was able to provide, in Mrs. Robinson's words, "an explanation for a situation in which all firms could work their plants at less than full capacity (i.e. at less than the level of output at which AC was at a minimum) and still earn a profit"¹ . Friedman is therefore only partly right in the first of his two contentions distilled from the passages quoted earlier in this section. He is correct in that the theory of imperfect competition was developed on account of the fact that when the theory of perfect competition was tested directly, its central assumption was discovered to be unacceptable; he is incorrect in that (in contradiction to what he asserts) theorems can be derived from the theory of perfect competition which are not in conformity with the phenomena the theory is meant to explain, and in that the originators of the theory of imperfect competition were fully aware that false implications could be so derived. Thus, despite the fact that Friedman expressly and emphatically directs his criticism against the theory of imperfect competition, careful analysis shows his historical claim to be unfounded. For even if Mrs. Robinson had not tested the theory of perfect competition directly by examining

1. Joan Robinson, op. cit., p. vi

the truthfulness of its main postulate, she would certainly have abandoned the theory once she had discovered that a number of its logical implications were not in concordance with real-world phenomena.

One must now investigate the second of the two claims Friedman advances in the passages to which reference was made a moment ago. The second contention is that to attempt to test a theory by examining the veracity of its assumptions is an illicit move, because in principle no such test can be achieved. What argument does Friedman bring forward in support of this claim, which seems at first sight to be almost as erroneous as any claim can be (as mentioned previously) ?

Although Friedman is anxious to demonstrate the importance of his methodological precept to economic theories, he stresses that the relevance of his view is not restricted to theories of human and social phenomena but has application to all scientific theories. In order to emphasise the universal nature of his thesis, Friedman attempts to prove the proposition that a theory cannot be tested by ascertaining to what extent its premises are realistic, by arguing the case with reference to Galileo's law of falling bodies (i.e. 'for all x , if x is a body falling freely in a vacuum, the distance x travels in any specified interval of time is given by the formula ' $s = \frac{1}{2}gt^2$ '). At once, however, the mention of Galileo's law arouses suspicions as to whether Friedman is still debating his professed thesis; for while he was formerly concerned to analyse the relationship between the empirical postulates and the derived theorems of a formal theory, this example from classical physics involves a single law.

And indeed if awakened, any such suspicions prove to be fully justified. Without so much as batting an eyelid, Friedman asserts that "testing this hypothesis by its assumptions presumably means measuring the actual air pressure and deciding whether it is close enough to zero"¹. But at this juncture, as critics have not been slow to notice,² Friedman changes his tune. When he examines the theory of imperfect competition, Friedman employs the expression 'assumption' to denote the empirical postulates from which theorems can be derived; now, however, he is using the same term to denote the antecedent component (or components) of a statement in conditional form. Galileo's law is an assertion to the effect that if the statement 'this body is falling freely in a vacuum' is true, then the statement 'the distance this body falls is given by the formula ' $s = \frac{1}{2}gt^2$ ' ' is invariably true (when asserted of any body). The veracity of this conditional statement cannot of course be tested simply by ascertaining whether in actuality falling

1. M. Friedman, op. cit., p. 518

2. E. Nagel, 'Assumptions in Economic Theory', in The American Economic Review, Vol. LIII, No. 2, May 1963, p. 212

A. Rosenberg, 'Friedman's 'Methodology' for Economics: A Critical Examination', in Philosophy of the Social Sciences, Vol. 2, No. 1, March 1972, p. 21

bodies do or do not pass through a resisting medium. No statement in conditional form can be tested by seeing if its antecedent is true or false, but only by discovering if its antecedent is ever true while its consequent is false. If (as appears to be the case) this elementary logical point is what Friedman understands by his claim that Galileo's law¹ cannot be tested by assessing the realism of its assumptions, then this is something to which one very happily assents. But who has ever seriously sought to challenge the point?

1. The premises from which Galileo's law can be deduced (the first of which is an empirical assertion and which can certainly be tested as a statement in its own right) are:

- (a) All bodies in free vertical fall, travel with uniform acceleration (of value f)
- (b) $v = u + (e - u)/2$ (analytic statement)
- (c) $s = vt$ (analytic statement)

(In (b) and (c) above, 'v' stands for the average velocity of fall over a given period of time, 'u' for the initial velocity of fall, 'e' for the terminal velocity of fall, and 't' for the number of time periods.) Thus:

- (1) $e = u + ft$ From (a)
- (2) $s = t(u + (e - u)/2)$ Substitution of (b) in (c)
- (3) $s = ut + t(e - u)/2$ From (2)
- (4) $s = ut + t(u + ft - u)/2$ Substitution of (1) in (3)
- (5) $s = ut + t(ft)/2$ From (4)
- (6) $s = ut + \frac{1}{2}ft^2$ From (5)

If the initial velocity is zero, and if f is given the value of g (i.e. 981 cms/sec^2), then (6) becomes:

$$(7) s = \frac{1}{2}gt^2$$

Q.E.D.

Since Friedman uses the word 'assumption' in a grossly equivocal fashion, his argument for the highly contentious methodological thesis¹ advanced at the beginning of his essay simply collapses. Nevertheless, even though Friedman's methodological injunction cannot be cogently supported, a problem

1. The reference at this point in the main text is of course to the thesis regarding the testability of economic assumptions. It is to be noted however that in the same essay Friedman propounds a second major thesis, not altogether unconnected with the first. He writes:

"Consider the problem of predicting the shots made by an expert billiard player. It seems not at all unreasonable that excellent predictions would be yielded by the hypothesis that the billiard player made his shots as if he knew the complicated mathematical formulas that would give the optimum directions of travel, could estimate accurately by eye the angles, etc., describing the location of the balls, could make lightning calculations from the formulas, and could then make the balls travel in the direction indicated by the formulas. Our confidence in this hypothesis is not based upon the belief that billiard players, even expert ones, can or do go through the process described; it derives rather from the belief that, unless in some way or other they were capable of reaching essentially the same result, they would not in fact be expert billiard players." (M. Friedman, op. cit., p. 521; Friedman's italics)

In the above passage, Friedman readily admits that the assertion following the words 'as if' is a palpable falsehood. Despite his recognition of this fact, Friedman thinks that it is 'not at all unreasonable' to predict the actions of an expert billiard player by means of a theory which employs the said falsehood as one of its premises.

However, it is surely accepted that social scientists, no less than natural scientists, are not just concerned to obtain theories which are good instruments of prediction, but are also concerned to obtain theories which are bodies of true statements. If a postulate such as 'billiard players know and apply numerous mathematical formulas' were to be adopted as an integral part of a theory designed to explain the behaviour of an expert billiard player, then that theory would be incompatible with, say, psychological theories relating to the acquisition of human skills (which is what the ability of a billiard player is). And if one scientific theory is in logical conflict with another, then at least one of them must be rejected; to abandon this view (in effect, the view that truth is indivisible) is to retire from scientific activity.

associated with it remains. For even if there is no barrier in principle to testing a theory by testing the truthfulness of its empirical postulates, is there ever a practical barrier? The answer is clearly in the affirmative if one is talking about scientific theories in general. Suppose one takes, for example, the atomic theory of chemistry. Since the postulates of this theory refer to connections between 'microscopic' processes and entities which are far removed from what can be perceived by observational experience, it is only possible (that is, empirically possible) to test the postulates indirectly by seeing if their logical consequences are corroborated by the occurrence of the anticipated 'macroscopic' phenomena. But although there are certainly unsurmountable empirical impediments to the direct testing of many theories within the exact physical sciences, this does not seem to be true of theories within the human and social sciences (with the exception of psycho-analytic theories). The empirical postulates of, for instance, an economic theory appear to be no more difficult to test than their logical implications. But is this appearance in fact correct?

It must be borne constantly in mind that because an economic theory deals only with the main kinds of causal factors at work in a complex social situation, whatever form of test is administered it will hardly ever be able to provide a clear-cut indication of whether the theory does or does not capture the major causal inter-connections operating in the realm of events under investigation. The great difficulty of devising a significant test for a social theory when each of its 'predictions of the principle' is qualified by a 'ceteris paribus' clause which cannot be unpacked in detail, has been discussed previously; it is also the case,

however, that a precisely analogous difficulty is associated with any attempt to test an economic theory directly by testing the veracity of its empirical premises.

Suppose one takes as examples of empirical postulates the following statements: 'every producer aims to maximise his profits', 'every consumer aims to maximise his satisfaction' and "each individual acts in a sensible manner, in the circumstances in which he finds himself, from the point of view of his own economic interests"¹; and then one asks whether these assumptions are true or false. The answer (briefly, so as not to trespass too far into the province of the next section) is that these, and many other empirical postulates of economic theories, are completely true only of idealised individuals but are nevertheless approximately true of actual individuals. That is to say, when applied to actual agents such assumptions should in strict accuracy be preceded by phrases like 'for the most part' or 'on the whole' which are every bit as unspecific as the 'ceteris paribus' clauses qualifying derived 'predictions of the principle'. Certainly, if an empirical postulate is investigated and found to be manifestly neither true nor approximately true (eg. the assumption of perfect competition in the inter-war years), then any theory which employs it is false. However, on account of the ineradicable unspecificity of a phrase such as 'for the most part', the line of demarcation between a postulate being approximately true and being false is usually not easy to draw. Hence the occasions on which an economic theory can be falsified directly because one of its assumptions is found to be

1. Joan Robinson, op. cit., p. 15

indubitably false, are likely to be few and far between.

If an economic theory is tested directly and all its empirical premises seem to belong to the category 'approximately true', the theory should then be tested further in order to determine whether any of its infinitely many logical consequences can be falsified by the occurrence of phenomena falling outside the expected range of events; for only if the theory is subjected to an indirect test can it be discovered whether any postulate is in fact false, even though its falsity is not obviously apparent. In the social sciences, therefore, the method of direct test (which requires a penetrating inspection of a theory's small number of empirical assumptions) should be conceived as a helpful supplement, rather than as an alternative, to the method of indirect test (which requires a critical and systematic examination of as many as possible of a theory's derived implications) .

(iii) The Status and Function of Ideal Types

The component statements of a well developed scientific discipline cannot be expressed entirely in the language of everyday speech. Although investigation into a given field of events has necessarily to begin with ideas formulated in the terminology of ordinary language, in order to make headway a scientific inquiry must develop a technical vocabulary of terms whose meanings are clear, precise and free of the ambiguities and obscurities which habitually infect words in common usage. The technical vocabularies

of most sciences (both natural and social) include terms known as 'ideal types', which refer to idealisations (or 'pure cases') of the phenomena under investigation. Although the strict referents of such terms are hardly ever realised and encountered in experience, if idealisations should be approximated empirically by concrete phenomena, explanations employing ideal types can often be used to account (with an acceptable degree of accuracy) for what takes place within the sphere of actual events.

The question of the employment of ideal types in social scientific explanations was first explored in depth by the political economist and sociologist Max Weber (1864-1920). In his essay of 1904, "'Objectivity' in Social Science and Social Policy"¹, he states that an ideal type of an historical situation is a "construct which (is) arrived at by the analytical accentuation of certain elements of reality"². In amplification,

1. M. Weber, The Methodology of the Social Sciences, translated and edited by E.A. Shils and H.A. Finch, (The Free Press of Glencoe, Illinois, 1949). The 1904 essay forms Chapter II (i.e. pp. 49-112) of this work.
2. Ibid., p. 90

and in description of the way the ideal type 'medieval city economy' is formed, Weber writes:

" we construct the concept 'city economy' not as an average of the economic structures actually existing in all the cities observed but as an ideal-type. An ideal type is formed by the one-sided accentuation of one or more points of view and by the synthesis of a great many diffuse, discrete, more or less present and occasionally absent concrete individual phenomena, which are arranged according to those one-sidedly emphasised viewpoints into a unified analytical construct. In its conceptual purity, this mental construct cannot be found empirically anywhere in reality. It is a utopia." 1

(One notes, by way of parenthesis, that this quotation contains a confusion between the proposed ideal type and its possible referent. Weber does not mean, surely, that the mental construct cannot be found anywhere, but that its strict referent cannot be.) As originally envisaged by Weber, an ideal type is a concept (of a particular kind of historical or social situation) formed "by arranging certain traits into a consistent ideal-construct by an accentuation of their essential tendencies"². The concept itself designates nothing actual, but to its imagined referent a real set of circumstances can be compared in order that its socially significant features can be thrown into sharp relief for the purpose of historical analysis. Thus, speaking of the ideal type 'city economy', Weber writes that "historical research faces the task of determining in each individual case, the extent to which this ideal-construct approximates to or diverges from reality, to what

1. M. Weber, Methodology, op. cit., p. 90 (Weber's italics)
 2. Ibid., pp. 90-91 (italics added)

extent for example, the economic structure of a certain city is to be classified as a 'city economy' "¹ .

The methodological utility of creating ideal types of the sort just described has often been queried. In recent times the most damaging objection is that advanced by J.W.N. Watkins . In criticism of Weber , Watkins castigates the 1904 essay on account of the fact that the ideal type it proposes is 'holistic' in character.² Watkins' critique of the theory of holistic types rests on the contention that although it is, on the one hand, indeed possible to ascertain the macroscopic properties of a volume of gas while knowing nothing about the constituent molecules, it is, on the other hand, conceptually impossible to discern the essential traits of an historical or social situation considered as a whole while knowing nothing about the participating individuals.³ The question of methodological individualism versus methodological holism will be examined in detail in the following chapter of this discussion; in the meantime (anticipating a little the conclusions to be reached in the next chapter), it is sufficient to remark that Watkins' claim is correct. It is indeed the case that a social scientist is only in a position to comment upon the overall features of a given situation after he has elucidated (i) how the agents in the society under examination view their own societal environment, (ii) what institutional relationships hold between the individuals

1. M. Weber, Methodology, op. cit., p. 90

2. J.W.N. Watkins, 'Ideal Types and Historical Explanation' (1952), in H. Feigl and M. Brodbeck (eds), op. cit., p. 724f

3. Ibid., p. 727

within the society in question, and (iii) what social repercussions emerge as the (intended or unintended) consequences of the behavioural interactions of the said agents.¹ Granted the propriety of the 'individualistic' approach, it follows that one can only set about constructing a holistic ideal type after one has analysed the pertinent social circumstances by investigating the three points just mentioned. Hence, the ideal type can be of no assistance in the process of inquiry itself; or, as Watkins himself puts the matter (in a rhetorical question), "if the characteristics of a historical situation have already been charted before the ideal type is brought into play, why bother with ideal types?"² . Holistic ideal types, therefore, seem to amount to nothing more than arbitrary verbal definitions and consequently are devoid of relevance for explanatory purposes.

Weber himself, as is shown by Watkins³ , came to recognise the grave weakness of his original version of the ideal type. The later Weber (to be found in his 1917 essay, 'The Meaning of 'Ethical Neutrality' in Sociology and Economics'⁴ , and in his posthumous book entitled The Theory of Social and Economic Organisation⁵) abandoned his earlier conception of what ideal types should be and the way they should be constructed, and advocated instead the employment of individualistic ideal types. These types

1. J.W.N. Watkins, 'Ideal Types', op. cit., p. 725

2. Ibid., p. 726 (Watkins' italics)

3. Ibid., p. 727

4. The 1917 essay forms Chapter I (i.e. pp. 1-47) of M. Weber, Methodology, op. cit. .

5. M. Weber, The Theory of Social and Economic Organisation, translated by A.M. Henderson and T. Parsons, edited by T. Parsons, (Oxford University Press, New York, 1947) .

are constructed by determining what would be the rational courses of action for individuals to take to achieve their ends within a system of institutional relationships of the given sort, and then by working out which (intended or unintended) consequences of the interplay of these actions constitute the overall features of the societal circumstances being investigated.

There is little to criticise in Weber's own elaboration of the use of his second (and largely acceptable) version of the ideal type within the social sciences. As far as the theoretical social science of economics is concerned, Weber writes that "economic theory makes certain assumptions which scarcely ever correspond completely with reality but which approximate it in various degrees and asks: how would men act under these assumed conditions, if their actions were entirely rational?"¹. By employing a theory which makes use of the ideal type 'rational agent', an economist is able to discern "what course a given type of human action would take if it were strictly rational, unaffected by errors or emotional factors and if, furthermore, it were completely and unequivocally directed to a single end, the maximisation of economic advantage"². Since explanations which treat actual agents as idealisations of rational men are normally adequate to account for men's actions, an economic theory which assumes that men act in a rational fashion will therefore usually provide tolerably accurate analyses of how concrete phenomena are brought about by the interaction of actual goal-seeking

1. M. Weber, Methodology, op. cit., p. 44

2. M. Weber, The Theory of Social and Economic Organisation, op. cit., p. 96

activities. Ideal types can also play a useful role in the investigations of the idiographic social sciences such as history. On the subject of historical inquiry, Weber argues that an historian who is examining a political or military campaign will often find it of assistance to determine the rational course of action given the ends of the participants and good knowledge on their part of all the pertinent circumstances. For once an historian has worked out what constitutes the ideally rational course of action, it is possible by comparison with this "to understand the ways in which actual action is influenced by irrational factors of all sorts, such as affects and errors, in that they account for the deviation from the line of conduct which would be expected on the hypothesis that the action were purely rational"¹. (The following section of this chapter will deal further with the problems connected with the explanation of actions, and will examine the question of how deviations from the rational line of conduct are to be properly accounted for.)

An ideal type should not be confounded with an 'extreme type'. The former is a concept which denotes an idealised or pure case of a certain sort of phenomenon (an idealisation is an instance which either lacks entirely a number of the properties which are always or usually attendant upon phenomena of its kind, or possesses a particular property (viz., that of special concern) to the maximum degree conceivable). In the natural sciences, many ideal types are concepts which are not merely without a referent, but are concepts without a physically possible referent (an example of such a type

1. M. Weber, The Theory of Social and Economic Organisation, op. cit., p. 92

will be given shortly); in the social sciences, ideal types are concepts whose referents are in principle capable of exemplification but are, as a matter of fact, only occasionally found in actuality. An extreme type, on the other hand, is a concept which denotes an 'extreme instance' of a certain kind of phenomenon (an extreme instance is an empirically encountered end-point of a graded series of entities or processes each of which exhibits a particular trait to some degree) . Regardless of whether a discipline is natural or social, the referents of extreme types are, by convention, always to be found instantiated in reality (illustrations will be given in just a moment) .

In his analysis of typological methods, Hempel differentiates ideal types from extreme types in a second, but clearly related way¹; namely, by reference to the fact that only the former are used for explanatory purposes (often as components of laws and social generalisations) . Extreme types, however, are ordering concepts employed for the purpose of categorisation in conditions where any precise boundary line between two sorts of 'objects' would seem unnatural, because each 'object' possesses the property of concern to some extent. Hempel gives two examples of pairs of extreme types: that used by the 'scratch test' employed by geologists to distinguish between 'hard' and 'soft' minerals, and that used by the classificatory scale employed by psychologists to distinguish between 'extravert' and 'introvert' personalities. The two members of each pair "serve as conceptual points of reference or 'poles', between which all actual occurrences can be

1. C.G. Hempel, 'Typological Methods in the Natural and the Social Sciences' (1952), in Aspects, op. cit., pp. 157-158

ordered in a serial array"¹. Thus, if the term 'D' stands for the trait possessed to the greatest encountered extent by the referent of the extreme type 'hard mineral' (or, 'extravert personality'), any given 'specimen' (even if a concrete example of the extreme instance) would not be described as 'D or not-D' but as 'more or less D'. A second rock (or, individual) is to be correctly compared to the first, by applying the same classificatory criterion in order to determine whether it (or, he) should be characterised as 'more D than', 'less D than' or as 'as much D as' the first 'specimen'.

Ideal types, therefore, differ from extreme types in two ways: (a) the referents of ideal types are only rarely (never, in many cases in the natural sciences) fully exemplified in reality but may nevertheless be well approximated by actual phenomena, while the referents of extreme types are to be found in concrete form as a matter of convention; and (b) ideal types are employed for explanatory purposes, while extreme types function as merely ordering concepts.

The employment of ideal types for explanatory purposes within the social sciences has considerable affinity with their use to the same end within the natural sciences. While economic theories make use of postulates and generalisations containing terms such as 'perfect competition', 'perfectly rational agent' and 'perfect mobility of labour and capital', many physical laws (pertaining to macroscopic phenomena) are formulated with reference to idealisations and thus contain concepts such as 'ideal gas',

1. C.G. Hempel, 'Typological Methods', op. cit., p. 157

'perfectly rigid body' , 'perfectly elastic impact', 'frictionless pulley' and 'mathematical pendulum' . An example of a physical law stated with reference to an idealisation is the Boyle-Charles law for gases (' $p v = k T$ ') or, as it is more accurately called, the Boyle-Charles law for ideal gases. This law can be derived from the more comprehensive kinetic theory of gases in conjunction with the additional premise that the molecules constituting any mass of gas are volumeless¹ (and that therefore there are no forces of attraction between its component molecules) . The concept 'ideal gas' has of course no possible referent because there is not and never can be a mass of gas composed of molecules which answer to the description 'volumeless' . However, in so far as the strict referent of the term 'ideal gas' is approximated (in the relevant aspect) by any constant mass of real gas, the Boyle-Charles law designates with tolerable accuracy the actual relations of dependence between the temperature, the pressure and the volume of the quantity of gas being investigated. (Experimental results in fact show that the ideal gas law expresses satisfactorily the behaviour of a constant mass of a real gas only for a certain range of low densities; for only when the mass per unit volume is small, can the forces of attraction between the constituent molecules of a given quantity of gas be deemed empirically insignificant.²)

1. F.W. Sears and M.W. Zemansky, University Physics, (Addison-Wesley Publishing Company, Reading, Massachusetts, 1963, edit., 1967), pp. 446-447

2. Ibid., pp. 466-474

Because human and societal phenomena are intrinsically complex, social scientists are only in a position to provide 'explanations of the principle' of the specific events which fall within their domains of inquiry. Physical scientists, by way of contrast, deal with phenomena which are inherently simple, and hence they are able to give 'explanations of detail' of the events which concern them. The fact that laws relating to simple phenomena are often formulated with reference to idealisations, weakens neither the distinction between simple and complex phenomena nor the distinction between 'explanations of the principle' and 'explanations of detail' .

A subject-matter is correctly characterised as 'simple' , if inter alia it is the case that (i) the kinds of variables pertinent to the determination of each specific occurrence of the sort of phenomena in question are small ('conceptually manageable') in number, and that (ii) all the particular instances of these variables can be taken into account for explanatory purposes should this be desired. Because their domains of inquiry contain simple phenomena, physicists and chemists are not in any way obliged to use laws containing ideal types; they freely choose to employ laws formulated with reference to idealised cases of the phenomena being investigated whenever it is 'safe' to do so (i.e. whenever the deviations in reality from what is predicted by means of such laws, are negligible), because the manoeuvre permits them to streamline their numerical calculations. In the exact physical sciences actual divergences from what is asserted to happen in the realm of idealisations, are always precisely calculable and can always be accounted for in detail; for example, the discrepancies

between the behaviour of a real gas and that of an ideal gas which take place as the temperature of a fixed quantity (at constant pressure) of the former is lowered, can be computed exactly. It is thus not a matter of dispute that when ideal laws are not employed, physicists are able to provide 'explanations of detail' . But it is also true that on the frequent occasions when laws containing ideal types are employed within the exact physical sciences, inquirers in these fields do not cease to provide 'explanations of detail' ; for, as Michael Scriven aptly observes, "the essence of the success of the (exact) natural sciences is the possibility of finding simple laws referring to ideal cases that are or can be realised in empirical cases to an indefinitely high degree of approximation"¹ .

Social scientists are concerned with complex subject-matters, and hence (as was argued at length in the previous chapter) they cannot provide 'explanations of detail' which give information on the occurrence of all the separate instances of the complete number of kinds of variables that are relevant to the determination of specific events. As a matter of empirical necessity, social scientists have to work with 'explanations of the principle' which can give information only on the occurrence of the major instances of the most important kinds of variables that bring about their explananda-phenomena. Not only are social scientists unable to

1. M. Scriven, 'A Possible Distinction between Traditional Scientific Disciplines and the Study of Human Behavior', in H. Feigl and M. Scriven (eds), Minnesota Studies in the Philosophy of Science, Vol. I, (University of Minnesota Press, Minneapolis, 1956, edit., 1964), p. 338

ascertain all the individual elements which cause specific events within the social sphere, but in order to give their theories a degree of general applicability they are obliged (on account of the great heterogeneity of all human and societal phenomena) to treat the particular factors referred to by their 'explanations of the principle' as if these elements were more homogeneous than in fact they are. Thus, theoretical social scientists (unlike physicists) have no option but to use generalisations containing ideal types; in order to construct a formal social theory (such as micro-economic price theory) a social scientist has to concern himself with the actions of idealised agents operating within an idealised institutional setting. Also, the actual divergences from what is asserted by a social theory to occur within a realm of idealised phenomena are neither negligible (in comparison with the 'acceptable' discrepancies between the actual and the idealised in physics), nor amenable to precise calculation or detailed explanation. But this, however, is the price that has to be paid for theoretical advance in the fields of complex social phenomena.

(iv) Degrees of Rationality

It was claimed in the first chapter that whenever the object of reference is a freely initiated action, the question 'Why did this occur?' should be regarded as a request for a teleological explanation (that is, an explanation which mentions an end or a goal which is aimed at) . On that earlier occasion in this discussion, it was also pointed out that two forms of this

kind of explanation can be identified. A teleological explanation of the first sort is called a 'rational explanation', and is given in a situation where an inquirer seeks the aim or goal a particular agent is attempting to achieve by means of his action. The action is then successfully explained when the inquirer learns what reason or intention lies in the mind of the initiating agent, and hence comes to understand to what goal the action itself is directed.

A teleological explanation of the second sort is given not when an inquirer requests the end of an action, but when he seeks the grounds for which an agent with a given objective (or with a given hierarchy of different objectives) adopts the course of action he does, rather than some other, as a means of bringing about his desired goal. This type of explanation is called a 'rationality explanation' on account of the fact that it employs an important component known as the 'principle of rationality'. This principle is an assumption to the effect that human agents act rationally (i.e. adopt appropriate or fitting means) to achieve their chosen ends.¹ An explanation which employs this principle operates, therefore, by showing that the action in question is performed because it is the rational thing (or, perhaps, one of a number of

1. The rationality principle is not, of course, applicable only to deliberate actions. An habitual action can be called 'rational', provided that it is an appropriate means to the given end, and provided that it would have been adopted if the agent's choice of means had been consciously made.

equally rational things¹⁾ for the initiating agent to do in the circumstances in which he finds himself. The general schema for a rationality explanation was laid out in section (ii) of chapter I ; this present section is concerned to elucidate what is properly meant when reference is made to human rationality, and the precise role the principle of rationality plays in the explanation of a given action.

In order to be able to speak unambiguously on the topic of rationality, it is necessary to establish a three-way distinction between (i) subjective rationality, (ii) ideal rationality, and (iii) objective rationality . (It is to be noted that this terminology is peculiar to the present discussion, and does not therefore coincide with that used elsewhere in analysis of the same issue. As regards the particular topic in hand, there seems to be no generally accepted technical vocabulary.) If it is planfully directed in some way or other (no matter how ineffectively) to the achievement of a desired goal, any piece of behaviour constitutes a subjectively rational action. In this minimal sense of rationality, to be rational an action only has to be an action; thus, bodily movements which are not rational in this sense are

1. In his analysis of rational behaviour, Quentin Gibson writes: "(it is) .. an elementary logical point .. that, given certain evidence, there can only be one correct solution to the problem as to the best way of achieving a given end" . But this claim is completely false; there is no such logical point. If there happen to be a number of competing means to a given end, then it is a purely empirical matter whether one of them is more appropriate than any of the others, or whether several of them are equally appropriate.

- Q. Gibson, The Logic of Social Inquiry, (Routledge and Kegan Paul, London, 1960, edit., 1968), p. 162

involuntary reflexes produced by, say, extreme alcoholic or drug intoxication, or by physiological degeneration of the nervous system. In sharp contrast, objective rationality is possessed by an action if, within the environmental conditions of its initiation, it is the most appropriate choice (or, one of a number of equally appropriate choices) of means to achieve a given goal according to the criteria of an observing social scientist who bases his judgment upon the most recent scientific knowledge. In this optimal sense of rationality, to be rational an action must be the most technically effective means relative to the highest standards available at the present time. Finally, an action is ideally rational if it constitutes a means which is objectively rational according to the standards of technical appropriateness prevailing within the agent's own society. In this sense of rationality, to be rational an action must satisfy the criteria of what counts as objectively rational within the cultural environment of which the initiating agent is a socialised member. Thus, criteria of rationality form a continuous scale of degrees which stretches from the subjective at the bottom to the objective at the top; the criteria of ideal rationality occupy a position on the scale which either coincides with that of objective rationality, or is removed from the position of objective rationality by a distance whose length is inversely related to the

level of scientific advance attained by whatever society is under investigation.¹

The distinction between objective and ideal rationality is required because it is the business of many social scientists to explain actions (and phenomena which result from the interaction of many men's actions) which occur outside contemporary society. Although the rationality imputed to the individuals whose actions are studied by economics is virtually coextensive with what economic theorists hold to be objectively rational, it is not the case that the rationality attributed to the individuals whose actions are explained by social anthropologists and historians can be identified

1. P. Winch, 'Understanding a Primitive Society', in American Philosophical Quarterly, Vol. I, No. 4, October 1964 .

In the abovementioned essay, Winch advances the contention that it is illegitimate to speak of the intrinsic superiority of the standards of rationality found in a society which is profoundly influenced by the achievements and methods of modern science, over those found in a primitive society (such as the African Azande) which is deeply affected by a system of magical beliefs and practices. Winch states that he does not seek to challenge the view that ideas and beliefs are checkable by reference to an independent reality (p. 308), but that rather he seeks to deny that the concept 'independent reality' has meaning outside a particular universe of discourse (p. 309); in other words, he maintains he does not uphold, as he puts it, "an extreme Protagorean relativism" (p. 308), but nevertheless he strongly disputes the claim that the European notion of reality is correct and that of the Azande mistaken. In support of his position, Winch argues that "the conception of reality is not a conception which can be explicated in terms of what science reveals to be the case; for a form of the conception of reality must already be presupposed before one can make any sense of the expression 'what science reveals to be the case' " . (p. 315)

Since there can be no such thing as a 'protocol observation', all scientific inquiry has certainly to rest upon some ultimate presupposition as to the nature of reality. But this does not mean that any metaphysics is as good as any other. An initial theoretical/

Continued at foot of following page

with objective rationality. Both anthropologists (who are concerned with the behaviour of men within primitive cultures) and historians (who are concerned with the actions of agents in bygone eras) need a clear separation between what is objectively, and what is ideally, rational. If this distinction is not made, absurdity results. For it is palpable nonsense to hold that an agent is irrational if he adopts a means which is optimally efficient by the technological standards of his own society, but is downright inefficient by the technological standards in use within the social scientist's society. To avoid the crude error of calling the actions of people living within primitive societies 'irrational', social scientists, as the

theoretical idea is not something fixed and immune from criticism, but is something which may be abandoned if it does not lead to an appropriate conceptual framework within which true statements concerning the domain of inquiry in question can be formulated. And by 'appropriate' here, is not meant 'thought within a certain culture to be appropriate' but 'appropriate überhaupt'. A conception of reality therefore, is not just presupposed by, but is in addition supported by, what science reveals to be the case; for if under critical testing the empirical statements of a theory are invariably falsified, then this damages the basic theoretical presupposition.

As noted, Winch declares there is an 'independent reality' against which statements can be tested for truth or falsity. But since he holds this, is he not committed to saying that Azande beliefs containing the concepts 'magic' and 'witch' are amenable to test, and can therefore be pronounced true or false? And granted that such beliefs are false and that a choice of means to a given end is more rational when based on true beliefs than on false, it follows that the rationality of the Azande is objectively inferior to that of modern western civilisation. If a statement is true, it is true regardless of cultural conditions; and if a statement is false, it is false regardless of cultural conditions. To deny these sentiments is to espouse relativism. Thus, it appears that not only is Winch's central position erroneous, but that his overall position is internally inconsistent: if his thesis is true, he has to be a relativist; if (as he proclaims) he is not a relativist, his thesis has to be false.

anthropologist Siegfried F. Nadel (1903-1956) emphasises, "ascribe rationality to sequences of behaviour if they are analysable in terms of an intrinsic appropriateness of means to ends of which the actors are aware and which the observer, drawing on his empirical knowledge, can discern and verify"¹. In general, then, a cleavage between objective and ideal rationality is called for whenever an individual belongs to a cultural tradition in which what counts as a good reason for a particular choice of means, is not what the social scientist (who possesses more advanced knowledge than the agent being studied) would himself be prepared to endorse as a good reason.

The rationality principle states that men act appropriately to achieve their chosen goals in the conditions in which they find themselves. From what has been asserted so far in this section, it is surely evident that this principle has to be understood as an assumption that actual agents are ideally rational. Granted this point, two relevant questions arise: (i) What is the status of the principle within an explanation? , and (ii) Is it a reasonable supposition to hold on all occasions? . Both these questions can be answered in the context of an analysis of the main claims advanced by a recent article on the theme of rationality. In

1. S.F. Nadel, The Foundations of Social Anthropology, (Cohen & West, London, 1951, edit., 1953), pp. 266-267

his paper 'La rationalité et le statut du principe de rationalité', Popper puts forward three inter-dependent theses concerning the logical status and methodological function of the principle in question. These are: (a) "le principe de rationalité ne joue pas le rôle d'une proposition empirique ou psychologique, et s'il n'est pas traité dans les sciences sociales comme le sujet d'une catégorie quelconque de tests"¹, (b) "le principe de rationalité est faux"² but nevertheless if the theory of which it is an integral part collapses, "une bonne pratique méthodologique consiste à ne pas déclarer responsable le principe de rationalité, mais le reste de la théorie"³, and (c) "bien qu'étant faux, il est en général suffisamment proche de la réalité"⁴ and hence "nous employons le principe de rationalité simplement comme une bonne approximation de la réalité, tout en reconnaissant qu'il n'est pas vrai"⁵.

Popper construes the principle of rationality as the assertion "les individus agissent toujours d'une manière adaptée à la situation où ils se trouvent"⁶. (Rather idiosyncratically,

1. K.R. Popper, 'La rationalité et le statut du principe de rationalité', in E.M. Claassen (ed), Les Fondements Philosophiques des Systèmes Économiques, (Payot, Paris, 1967), p. 145
2. Ibid., p. 145
3. Ibid., p. 146
4. Ibid., p. 147
5. Ibid., p. 148
6. Ibid., p. 145

Popper incorporates the goals agents pursue into their situation; he writes: "la situation, dans le sens où j'utilise ce terme, contient déjà toutes les fins et toutes les connaissances réalisables qui peuvent être importantes, en particulier la connaissance des moyens possibles de réaliser ces fins"¹. The fact that Popper treats aims in this fashion appears, however, to possess no significance beyond itself; consequently, there is no need to refer to the matter again.) If taken exactly as it stands, Popper's formulation of the rationality principle is certainly (to use the language familiar to section (ii) of the present chapter) an 'unrealistic assumption'; that is, the principle views actual agents as if they were idealizations of rational men. Strictly considered, therefore, the principle is indeed false, as the first part of Popper's thesis (b) proclaims. However, even though the principle in the precise form just enunciated is not completely true, it is the case (as Popper clearly acknowledges in thesis (c)) that in reality men tend to be rational and, in the main, they strive for optimal appropriateness in the choice of means for the achievement of their goals. Hence, the qualified statement 'for the most part, agents act rationally in pursuit of their goals' can be confidently accepted as true. Human agents are sufficiently rational to enable explanations (and theories) employing the assumption that men are fully (i.e. ideally) rational, to account with satisfactory accuracy both for men's actions and for the many social phenomena brought about as the (intended or

1. K.R. Popper, 'la rationalité', op. cit., p. 144

unintended) consequences of the interaction of their various goal-seeking activities. (The question of just how well the rationality principle approximates the truth, will be discussed further in a moment.)

If Popper's thesis (c) is true (and there seems no reason to doubt that it is), then surely the rationality principle is an empirical proposition and therefore open, in principle at any rate, to overthrow by falsification. But these two consequential points are precisely those which thesis (a) disputes. As regards the first claim of thesis (a), Popper's position cannot be accepted as tenable; since the rationality principle is a meaningful, well-formed, non-analytic universal statement, it cannot be granted immunity on logical grounds from empirical test (in the way that, for example, a bald 'all-and-some' statement is immune from empirical refutation). Moreover, in a substantial number of cases the behaviour of human beings is such that the principle is reasonably well satisfied; but that this is so, is nothing more than a brute empirical fact (for conceivably matters could have been different). A solution to the problem of the status of the rationality principle is available along these lines: thesis (a) ought to be re-structured and combined with the second part of thesis (b) to create a more comprehensive thesis. This new contention would assert that although the rationality principle is an empirical assumption, social scientists should on methodological grounds look upon it for the most part as if it were non-refutable. In other words, the principle of rationality should always be the very last component blamed in the event of a social theory's failure; hence, the principle should be regarded as falsifiable, but should be held

accountable only in the final resort. The argument in favour of this revised thesis proceeds by reductio ad absurdum; unless the proposed methodological rule is adhered to, then any social inquiry is liable to end in a highly unpalatable way. Since a social explanation can only be overthrown in favour of another and since all social explanations contain the principle of rationality, to abandon this common principle is tantamount to making the disagreeable claim that the action under consideration is irrational (i.e. is merely subjectively rational). Thus, to repeat the chief points, one needs to recognise two things about the principle of rationality: (1) the principle is an empirical assumption, and is a good approximation to what is true, and (2) it is (virtually) non-refutable, not on logical but on methodological grounds.

To perform an action which is ideally rational, an agent has to adopt a means which is optimally effective relative to the most advanced technical standards of his own cultural setting. It is obvious that if a particular agent is not guided in his choice of means by a full awareness of all the knowledge available in his society or if he fails to evaluate correctly the information to which he does have access, then although he may think that he has chosen an optimal means to his desired end (or desired hierarchy of ends) his action may nevertheless fall short of what is ideally rational. When, therefore, a social scientist is faced with a moderate discrepancy between an actual action and an ideal line of conduct, he should attempt to discover what influences were responsible; such a deviation will usually be found to stem from factors like a mistaken assessment by the agent of his own situation and/or of the possible means open to him on

the occasion in question, or the possession by the agent of some peculiar personality trait. (It is to be observed that when an agent operates in conditions where it is logically impossible to predict each of the possible outcomes of his actions with certainty, an individual's choice of action cannot be explained without taking into consideration his 'gambling temperament', i.e. the extent to which he is disposed by character to act cautiously in the presence of risk and uncertainty.¹)

What is a social inquirer supposed to do when confronted with an action which is very unsuccessful? Is he permitted to account for the great divergence between the actual action and the ideally rational course of action, by hypothesising that the agent under investigation has taken leave of his senses? There can be no doubt that the answer to the second question is in the affirmative, for one cannot exclude a priori the conjecture that the agent suddenly became mad. However, by the methodological maxim accepted two paragraphs back, the hypothesis of insanity should be accepted only when all other attempts at explanation have failed; thus, if it is at all possible, an extreme discrepancy is to be accounted for in precisely the same fashion as a minor discrepancy. Therefore, if an historian is faced with an action which is so far from being ideally rational that it seems to possess only subjective rationality, he must see if he is able to revise (in

1. J.W.N. Watkins, 'Imperfect Rationality', in R. Borger and F. Cioffi (eds), Explanation in the Behavioural Sciences, (Cambridge University Press, Cambridge, 1970), pp. 184-188

a manner as little ad hoc as possible) the optimal course of action in such a way that the actual action appears ideally rational given that, say, the agent had just failed to notice something of great importance and such that it is extremely hard to understand, with hindsight, how any person could have overlooked it.¹ In other words, in cases where there is a very great divergence between an historical action and the constructed ideal course of action, an historian should: (i) work out what objectively mistaken situational appraisal could be attributed to the agent in question to show that he acted rationally enough (by the standards of his own society) given this perverse view of his own circumstances; and (ii) attempt to discover if there is any independent evidence (perhaps hitherto unnoticed) indicating that the agent actually held the situational appraisal conjectured in (i).

On the question of the way an historian should apply the principle of rationality, two (prima facie different) approaches are available. First, there is Collingwood's view that the explanation of the action of an individual who was attempting to find a solution to a particular problem, is achieved by "the re-enactment of past thought in the historian's own mind"². And secondly, there is Popper's position that an historian should explain an individual's action by means of 'situational logic'³; this form of analysis aims to demonstrate how the action of an

1. J.W.N. Watkins, 'Imperfect Rationality', op. cit., pp. 206-211

2. R.G. Collingwood, The Idea of History, op. cit., p. 215 et passim

3. K.R. Popper, The Poverty of Historicism, op. cit., pp. 135-143 and pp. 147-159

K.R. Popper, The Open Society and its Enemies, Vol. II, op. cit., p. 96ff et al.

historical agent corresponds to the course of action it would have been (ideally) rational for him to take while pursuing his chosen goal in his own specific situational circumstances. But is there any fundamental difference between these two methods? In his synopsis of British philosophy of history in the period 1945-1955, Watkins claims that these two procedures amount to virtually the same thing.¹ More recently, Popper himself has expressed precisely the same opinion.² But what does the disagreement which is alleged to exist, add up to?

Popper writes as follows:

"The main significance of the difference between Collingwood's re-enactment method and my method of situational analysis is that Collingwood's is a subjectivist method, while the method I advocate is objectivist. But this means that, for Collingwood, a systematic rational criticism of competing solutions to historical problems is impossible; for we can rationally criticise only conjectures or theories which have not become part of ourselves, but which can be put outside ourselves, and which thus may be inspected by everybody, especially by those who hold different theories. In contradistinction, the objectivist method of situational analysis permits the critical discussion of our tentative solutions - of our attempts to reconstruct the situation; and to this extent it is, indeed, much nearer to the true method of the natural sciences." ³

It is thus argued against Collingwood that his method of re-enactment, by placing all its emphasis on the re-thinking of the mental processes of an historical agent, only enables an historian

1. J.W.N. Watkins, 'Philosophy of History', in R. Klibansky (ed), Philosophy in the Mid-Century, Vol. III, (La Nuova Italia Editrice, Firenze, 1958), p. 167
2. K.R. Popper, 'A Pluralist Approach to the Philosophy of History', in E. Streissler (ed), Roads to Freedom, (Routledge and Kegan Paul, London, 1969), p. 197
3. Ibid., p. 198 (Popper's italics)

to analyse an action in terms of what the individual concerned happened to think was the intelligent (i.e. ideally rational) solution to the problem confronting him. On the other hand, it is claimed that situational logic "is not confined to the re-enactment of conscious thought processes, but makes allowance for the reconstruction of problem situations which were incompletely understood by the agent"¹; and that this is the case, because situational logic is not only "concerned with the situation as experienced by the acting subject (but also) with the objective situation as it actually was, and thus with the objective errors of the acting subject"².

What is one to make of Popper's comments? One should begin by noticing that Collingwood's picturesque metaphor of 're-enactment' cannot be taken literally³; one can only sensibly be said to 're-enact' another's thoughts in so far as one is able to consider in one's own mind what would constitute, in the given circumstances, the ideally rational solution to the problem the historical agent faced. But in order to construct the ideally

1. K.R. Popper, 'A Pluralist Approach to the Philosophy of History', op. cit., p. 199

2. Ibid., p. 200

3. Collingwood himself denies that to re-enact another's thoughts is to perform an act of thought numerically identical with that of the historical agent. He states quite plainly that for the historian the "only possible knowledge of the past is mediate or inferential or indirect". (The Idea of History, op. cit., p. 282) To disregard his explicitly asserted view on this point, is to take Collingwood to be recommending that historical events be investigated by an utterly impossible (and indeed nonsensical) method.

rational course of action for a given individual, an historian has to engage in an objective examination of both the agent's situational circumstances and the cultural tradition of the agent's own society; thus, despite Popper's contention, Collingwood's method is not, and cannot be, merely concerned with the 'situation as experienced by the acting subject' . Indeed, Collingwood in describing his method, explicitly asserts that "it is not a passive surrender to the spell of another's mind (for) the historian not only re-enacts past thought, he re-enacts it in the context of his own knowledge and therefore, in re-enacting it, criticises it, forms his own judgment of its value, corrects whatever errors he can discern in it"¹ . The significant phrase here is 'in the context of his own knowledge' ; for, to repeat the point made a moment ago, in order to establish the ideal line of conduct for a given agent and to be able to account for any deviation from it, an historian has no alternative but to undertake an objective analysis of the conditions in which the agent in question is placed.

It appears, therefore, that Popper is mistaken in his claim that 'situational analysis' is superior to 're-enactment' because only with the former method is it possible to stand back from an historical individual, and to view his activities in the light of modern knowledge. Re-enactment no less than situational analysis, permits an historian to ascertain how far an agent's action

1. R.G. Collingwood, The Idea of History, op. cit., p. 215
(italics added)

was ideally rational and to what extent the pertinent criteria of ideal rationality fall short of the criteria of objective rationality. Thus, if there is an important difference between the views of Popper and Collingwood on the application of the rationality principle to historical actions, it seems not to lie in the area Popper indicates.

V INDIVIDUALISM, HOLISM AND FUNCTIONALISM

(i) What is Methodological Individualism?

In the course of the present discussion, three examples of 'explanations of the principle' of economic phenomena have been worked out: (a) an explanation of why oscillations of the price of certain types of commodities occur in free markets, (b) an explanation of occurrences of the 'paradox of thrift' phenomenon, and (c) an explanation of why firms in conditions of imperfect competition will produce less, and charge a higher price, than they would if they were operating within conditions of perfect competition.

Although not mentioned previously, these three illustrations are in fact examples of what are properly called 'individualistic social explanations'; the reason for this, is that each of the explananda-phenomena in question were accounted for by providing an answer to the question: 'From what purposive action, or set of interacting purposive actions, does this phenomenon arise as the (intended or unintended) social consequence in the given societal circumstances?' . A more specific indication of what methodological individualism (as opposed to

methodological holism) involves, is provided by J.W.N. Watkins . The principle of methodological individualism:

" states that social processes and events should be explained by being deduced from (a) principles governing the behaviour of participating individuals and (b) descriptions of their situations. The contrary principle of methodological holism states that the behaviour of individuals should be explained by being deduced from (a) macroscopic laws which are sui generis and which apply to the social system as a whole, and (b) descriptions of the positions (or functions) of the individuals within the whole." ¹

It is clear from this quotation, that the principle under review is a methodological precept which recommends that social explanations be of a particular form. It urges social scientists, to cite Popper , to take the view that:

" all social phenomena, and especially the functioning of all social institutions, should always be understood as resulting from the decisions, actions, attitudes, etc., of human individuals, and that we should never be satisfied by an explanation in terms of so-called 'collectives' (states, nations, races, etc.)." ²

Because (as will be seen) misunderstandings and misrepresentations of the principle abound, it is necessary to stress with emphasis that it is a methodological rule governing what should be accepted, and what should be rejected, as a satisfactory form of social explanation; the principle is neither a theory of meaning concerning what concepts should be regarded as ultimately eliminable from social scientific discourse,

1. J.W.N. Watkins, 'Ideal types and Historical Explanation', in H. Feigl and M. Brodbeck (eds), op. cit., p. 729 (Watkins' italics)
2. K.R. Popper, The Open Society and its Enemies, Vol. II, op. cit., p. 98

nor is it a set of political maxims advocating, for instance, anarchism, egoism or laissez-faire capitalism.¹ Methodological individualism is a theory (or rather, a meta-theory) of explanation; it claims that if it is one's wish to explain a particular social phenomenon, then what one has to do is to trace its antecedents back to the action (or set of actions) of which it is the (designed or undesigned) social repercussion.

The debate on methodological individualism versus its rivals has been a recurrent topic in both the philosophy of the social sciences and the philosophy of history. In recent times, the question is universally acknowledged to have arisen with the authoritative writings of F.A. Hayek² and K.R. Popper³. Following on from the classical

1. On the question of individualism as a political doctrine, see:

- K.R. Popper, The Open Society and its Enemies, Vol. I, (Routledge and Kegan Paul, London, 1945, fifth edition, 1966), p. 100f

Here the contrast individualism/collectivism is clearly differentiated from the contrast egoism/altruism.

2. F.A. Hayek, Individualism and Economic Order, op. cit., Chapter I, (i.e. pp. 1-32) .

F.A. Hayek, The Counter-Revolution of Science, op. cit., Chapters IV, VI-VIII, (i.e. pp. 36-43 and pp. 53-86, respectively) .

3. K.R. Popper, The Open Society and its Enemies, Vol. II, op. cit., Chapter 14, (i.e. pp. 89-99) .

K.R. Popper, The Poverty of Historicism, op. cit., sections 7, 23, 24 and 31, (i.e. pp. 17-19, pp. 76-83, pp. 83-93 and pp. 147-152, respectively) .

expositions, a large body of literature¹ has developed over the years.

Of this literature, there can be little doubt that the most important

1. J.W.N. Watkins, 'Ideal Types and Historical Explanation' (1952), in H. Feigl and M. Brodbeck (eds), *op. cit.*, pp. 723-743
(This paper is a compound of:
 - 'Ideal Types and Historical Explanation' in The British Journal for the Philosophy of Science, Vol. III, No. 9, May 1952, pp. 22-43, and
 - 'The Principle of Methodological Individualism' in The British Journal for the Philosophy of Science, Vol. III, No. 10, August 1952, pp. 186-189 .)
- J.W.N. Watkins, 'Historical Explanation in the Social Sciences' (1957), in P. Gardiner (ed), *op. cit.*, pp. 503-514
- M. Mandelbaum, 'Societal Facts' (1955), in P. Gardiner (ed), *op. cit.*, pp. 476-488
- M. Mandelbaum, 'Societal Laws' (1957), in W.H. Dray (ed), Philosophical Analysis and History, (Harper & Row, New York, 1966) pp. 330-346
- E. Gellner, 'Explanations in History' (1956). Reprinted as 'Holism versus Individualism in History and Sociology' in P. Gardiner (ed), *op. cit.*, pp. 488-503
- M. Brodbeck, 'Methodological Individualism: Definition and Reduction' (1958), in M. Brodbeck (ed), *op. cit.*, pp. 280-303
- J. Agassi, 'Methodological Individualism' in The British Journal of Sociology, Vol. XI, No. 3, September 1960, pp. 244-270
- K.L. Scott, 'Methodological and Epistemological Individualism' in The British Journal for the Philosophy of Science, Vol. XI, No. 44, February 1961, pp. 331-336
- E. Nagel, The Structure of Science, *op. cit.*, pp. 535-546
- A.C. Danto, Analytical Philosophy of History, (Cambridge University Press, Cambridge, 1965, edit., 1968) Chapter XII, i.e. pp. 257-284
- W.H. Dray, 'Holism and Individualism in History and Social Science' in P. Edwards (ed), The Encyclopedia of Philosophy, Vol. 4 (The Macmillan Company & The Free Press, New York, 1967) pp. 53-58
- S. Lukes, 'Methodological Individualism Reconsidered' (1968), in D. Emmet and A. MacIntyre (eds), Sociological Theory and Philosophical Analysis, (Macmillan, London, 1970) pp. 76-83
- J.C. Harsanyi, 'Individualistic and Functionalistic Explanations in the Light of Game Theory: The Example of Social Status' in I. Lakatos and A. Musgrave (eds), Problems in the Philosophy of Science, (North-Holland Publishing Company, Amsterdam, 1968) pp. 305-321
- L. M. Lachmann, 'Methodological Individualism and the Market Economy' in E. Streissler (ed), *op. cit.*, pp. 89-103
- J.O. Wisdom, 'Situational Individualism and the Emergent Group-Properties' in R. Borger and F. Cioffi (eds), *op. cit.*, pp. 271-296

pieces of discussion are those contributed by Watkins in defence of the principle. His two main articles and his side in the justly famous Goldstein-Watkins¹ debate, comprise the main background to the writings on the topic.

This chapter, however, will be less concerned to examine in an historical way the contents of the various contributions to the controversy, than to discern the main philosophical points actually at issue and to see whether the principle of methodological individualism can in fact be defended successfully. In order to analyse the principle there are two important questions that need to be asked: (a) 'Can methodological individualism be distinguished from psychologism?', and (b) 'Why should one accept methodological individualism rather than (some version of) methodological holism?'. The first of these questions will be examined in the section immediately following, while the second will

1. L.J. Goldstein, 'The Inadequacy of the Principle of Methodological Individualism' in The Journal of Philosophy, Vol. LIII, No. 25, December 6th 1956, pp. 801-813
- J.W.N. Watkins, 'The Alleged Inadequacy of Methodological Individualism' in The Journal of Philosophy, Vol. LV, No. 9, April 24th 1958, pp. 390-395
- L.J. Goldstein, 'The Two Theses of Methodological Individualism' in The British Journal for the Philosophy of Science, Vol. IX, No. 33, May 1958, pp. 1-11
- J.W.N. Watkins, 'The Two Theses of Methodological Individualism' in The British Journal for the Philosophy of Science, Vol. IX, No. 36, February 1959, pp. 319-320
- L.J. Goldstein, 'Mr Watkins on the Two Theses' in The British Journal for the Philosophy of Science, Vol. X, No. 39, November 1959, pp. 240-241
- J.W.N. Watkins, 'Third Reply to Mr Goldstein', *Ibid.*, pp. 242-244

be looked at in the third section. Then, finally, in the last section we shall investigate methodological functionalism in sociology and social anthropology. There, an attempt will be made to demonstrate that when explicated appropriately, functionalism is perfectly compatible philosophically with the standpoint of methodological individualism.

(ii) Institutionalistic and Psychologistic Individualism

A considerable part of the foundations required to outline the precise tenets of methodological individualism was laid down in the second chapter. It will be recalled that there it was shown that the conspiracy theory of society - the narrowest species of individualism - was quite unacceptable; one is simply unable to hold that all social phenomena are the intended consequences of the conscious actions of some group of villains (or heroes, as the case may be). Since this is a statement of 'mixed quantifier' form, it cannot of course be shown to be definitely false: one however rejects it as incorrect because in practice it is unworkable. But all this has been discussed previously. Here, beyond stating that a social scientific theory that fails to recognise that not all repercussions of human actions are intended consequences lacks one of the most essential premises for providing true explanations of social phenomena, nothing more need be said about the conspiracy theory. There is, nevertheless, another false variant of individualism that needs to be tackled. This is something less narrow than the conspiracy theory, but still narrower than (in Agassi's nomenclature) institutionalistic

individualism,¹ that is, methodological individualism proper. This perverse species of individualism is known as 'psychologicistic individualism', or, more simply, just as 'psychologism'.

Although the conspiracy theory and psychologism, as well as institutionalistic individualism, operate from the ontological premise that only human agents are the operative causes of social change, the superstructure in each case built upon this common foundation is different. In the case of psychologism, it is argued that "society being the product of interacting minds, social laws must ultimately be reducible to psychological laws, since the events of social life, including its conventions, must be the outcome of motives springing from the minds of individual men"². This theory therefore claims that an adequate social explanation must be in terms of the psychological processes of individuals. As a methodological approach, psychologism recommends "reducing all social phenomena and all social regularities to psychological phenomena and psychological laws"³ because, in the final analysis, all social institutions, roles, rules and traditions are a 'reflection' of the psychological properties of the individuals who make up society.

J.S. Mill can be identified as a serious exponent of this totally reductionist explanatory procedure. He advances the opinion many times that "all phenomena of society are phenomena of human nature", and that "human beings in society have no properties but

1. J. Agassi, 'Methodological Individualism', op. cit., p. 244ff

2. K.R. Popper, The Open Society and its Enemies Vol. II, op. cit., p. 90

3. Ibid., p. 98

those which are derived from, and may be resolved into, the laws of the nature of individual men"¹. Immediately, the crucial question has to be raised of what methodological psychologists mean by the term 'psychology', and what the relation of psychology itself is to the social sciences. In other words, what is the nature of the discipline which is supposed to deal with the 'laws of individual man' with reference to which all social phenomena are to be explained and to which they are ultimately to be reduced?

Popper, in his own exposition of what individualism correctly conceived involves, takes great pains to differentiate methodological individualism from psychologism and to demonstrate that these two types of individualism can be clearly separated. An explanation of a particular social phenomenon is achieved once one has discovered the complex of actions of which it is the societal resultant. This does not mean that an explanation can be achieved without reference, implicitly if not explicitly, to the institutional situation in which the acting agents are placed. The purposes, plans, hopes, desires and knowledge of the individuals who initiate causative actions "are not ultimate data of human nature (for) they are, in their turn, explicable in terms of the social situation"². Institutionalistic individualism recommends that for the purpose of explaining a particular social phenomenon, the social environment within which the explanandum-event occurs should be regarded as given. Within a given situation (which constitutes a set of initial conditions) social scientists are urged to show how particular

1. J.S. Mill, op. cit., p. 572f, i.e. Book VI, Ch. VI, sc. 2, and Ch. VII, sc. 1

2. K.R. Popper, The Open Society and its Enemies, Vol. II, op. cit., p. 96 (Popper's italics)

social phenomena arise as the (intended or unintended) social effects of the behavioural interactions of a number of individuals, each of whom is consciously pursuing his own end for his own reasons. The reasons or plans which lie behind the actions of agents are not 'innate' psychological motives. If a man is motivated to act in a certain way because of, say, a 'speculative' notion concerning the structure of society, then this idea (no matter how erroneous it may in fact be) would have to be mentioned by a social scientist in an explanation of any phenomenon produced as the actual social result of the agent's action. Methodological individualism does not, emphatically not, require that adequate social explanations be ultimately in terms which use no social concepts at all.

Psychologism is to be distinguished from institutionalistic individualism by the fact that only the former requires that social explanations should, in the last resort, be in terms employing 'pure' psychological concepts (along with, one supposes, bio-physiological, neurological and physico-chemical concepts) . But what are 'pure' or 'innate' psychological dispositions?

Psychology may be considered to be either a narrow or a broad discipline. On the narrow conception psychology can roughly be said to be equivalent to physiology plus human biology. This discipline, physiological psychology, examines and explains men's behavioural patterns in so far as they result from basic instinctual drives and dispositions,

and from bio-genetic factors and physical environmental influences.¹
 On the other hand, psychology in the wide sense is what one may call 'human psychology'. The subject-matter of this discipline consists of behaviour as it results from the actions of individuals whose entire personalities and natures have been moulded (this is not the same as saying 'determined completely') by the social situations into which they were born and in which they live.²

The undeniable fact that "'human nature' varies considerably with the social institutions, and its study therefore presupposes an understanding of these institutions"³ is recognised by methodological individualists, but the fact of its acknowledgement seems to be ignored by critics of methodological individualism; it is indeed hard to comprehend how critics such as Ernest Gellner can claim that "the real

1. The subject-matter of psychology in the narrow sense can be identified easily by reference to any text-book of general psychology, eg.,

- H.H. Kendler, Basic Psychology, (Methuen & Co. Ltd., London, 1963) .

In this work, Chapter 5 - 'The Biological Foundations of Behavior' (pp. 80-107) - examines the rudiments of human biology. Chapters 6-9, dealing with the physiology of sensation (pp. 111-148), of conditioning (pp. 149-183), of perception (pp. 184-226) and of motivation (pp. 227-268), respectively, are concerned with what Howard Kendler himself describes as "basic psychological processes in their purest form" . (p. 269)

Chapter 9 (of the above work) discusses basic instinctual drives and dispositions. Among those examined, are the various 'appetitive' drives such as hunger (p. 232f), thirst (p. 235f) and sex (p. 236f), the 'aversive' drives such as pain (p. 241f) and the instinctive dispositions such as the need for air, maintenance of a constant body temperature and sleep. (p. 243f)

2. In H.H. Kendler (op. cit.), Chapters 11-15 fall into what may be broadly termed 'human psychology' or psychology in the wide sense. These Chapters are entitled, respectively, 'Verbal Behavior and Problem Solving' (pp. 338-388), 'Frustration and Conflict' (pp. 389-436), 'Personality' (pp. 439-493), 'Behavior Pathology' (pp. 494-545) and 'Social Behavior' (pp. 546-598) .

3. K.R. Popper, The Poverty of Historicism, op. cit., p. 158

oddity (of the doctrine advanced by Popper and Watkins) is that it seems to preclude a priori the possibility of human dispositions being the dependent variables when in fact this is what they often or always are"¹ in social scientific and historical explanations. To assert a claim like this in the face of what the methodological individualists actually say, is seriously to misunderstand what the doctrine in question involves. Institutionalistic individualism acknowledges that the social relations of one individual to another, and of individuals to societal institutions, are not just incidental factors which have to be taken into account only when an individual acts in a social situation. An individual is very largely what he is because of, and by virtue of, the fact that he enters into a system of social relations.² If it were possible to remove from a given individual literally everything which he owed to his status as a social creature (eg. his language and thus also his major thought processes) one would

1. E. Gellner, op. cit., p. 495

2. For precisely this reason one cannot agree with Watkins' contention that methodological individualism is analogous to the principle of mechanism in classical physics ('Ideal Types', op. cit., p. 730 and 'Historical Explanation', op. cit., pp. 504-505) .

Under mechanism, an aggregate constellation can be wholly analysed into its basic component parts and the external relations holding between them. But, as was argued at length in the third chapter of this discussion, the resemblance between the mechanistic approach and what takes place in the social sciences (and other realms of intrinsically complex phenomena) is only superficial. In areas of inherently simple phenomena (such as classical physics), the internal constitutions of the entities or processes comprising an aggregate structure are not affected by the system of relations into which the components enter. But the basic constituents of society - individual human agents - cannot be studied as individuals except in their status as socialised beings; for the social environment of which a human agent is a part largely determines his character. Thus, a study of social 'atoms' has to presuppose societal conditions, while a study of physical atoms does not have to presuppose any physical macro-configuration.

have as a final residue a biological organism which one would hesitate to call a 'person' or an 'individual'. It is just plainly false to assert, with Mill, that "men are not, when brought together, converted into another kind of substance, with different properties; as hydrogen and oxygen are different from water, or as hydrogen, oxygen, carbon and azote are different from nerves, muscles and tendons"¹.

If one reads through the reading list on methodological individualism one cannot but notice that the most frequently encountered 'refutation' of the individualistic approach is the one that first uncritically identifies methodological individualism with psychologism, shows why the latter is untenable and then in conclusion triumphantly claims the former to be overthrown. A blatant example of this line of thought occurs in the recent paper by Steven Lukes. He writes that methodological individualism is the doctrine that "facts about society and social phenomena are to be explained solely in terms of facts about individuals"². To justify this statement as to the programme of methodological individualism, Lukes produces the following quotation from Hayek:

"There is no other way toward an understanding of social phenomena but through our understanding of individual actions directed toward other people and guided by their expected behaviour." ³

But this is virtually a paradigm case of the tearing of a quotation from its actual context. The sentences of Hayek that immediately precede

1. J.S. Mill, op. cit., p. 573, i.e. Book VI, Ch. VII, sc. 1

2. S. Lukes, op. cit., p. 77

3. F.A. Hayek, Individualism and Economic Order, op. cit., p. 6

this one, and which Lukes fails to cite, run thus:

" (it is) the silliest of the common misunderstandings (to suppose) that individualism postulates, or bases its arguments on the assumption of, the existence of isolated or self-contained individuals, instead of starting from men whose whole nature and character is determined by their existence in society. If that were true, it would indeed have nothing to contribute to our understanding of society. But its basic contention is quite a different one; it is that " 1

And then follows the rest of the sentence, which is the part that Lukes seizes upon as his illustrative quotation. Lukes thus splits Hayek's passage in half, and then uses the citation of the second half to show that Hayek maintains an opinion which he in fact explicitly denies in the first half of the sundered passage.

Methodological individualism properly understood agrees that the actions of individuals not only have social effects, but are themselves the product of social conditions. While the social sciences study the former (i.e. the social effects, particularly the unintended social effects, of individuals' actions), human psychology studies the latter (i.e. the formative influences - social as well as genetic and physical - of human behaviour) . Social psychology is that branch of human psychology that examines and explains the behaviour of individuals as members of particular social groups (eg. families, work-teams, prisons, revolutionary political cells etc.).² If psychology is understood in the narrow sense as physiological psychology, then along

1. F.A. Hayek, Individualism and Economic Order, op. cit., p. 6
2. For an outline of the kind of topics covered by social psychology see, for example,

- F.A. Geldard, Fundamentals of Psychology, (John Wiley & Sons Ltd., New York, 1962) Chapters 20 and 21, 'The Individual in the Group', (pp. 357-371), and 'Group Behavior', (pp. 373-392), respectively.

with the physical and biological sciences it is presupposed by both human psychology and by the social sciences. There can, however, be no clear-cut division between human psychology and the social sciences - both employ explanations using a mixture of social and 'pure' psychological concepts. Human psychology and the social sciences should be thought of as two different but nonetheless closely interdependent kinds of studies with no clear boundary existing between them. Something very like this seems to be Popper's view of the relationship of (human) psychology to the social sciences. He considers that the former should be thought of "not as the basis of all social sciences, but as one social science among others"¹.

The division of psychology into two disciplines enables one to see that the psychological assumptions of economic theories are psychological in the wide sense and have little to do with what is psychological in the narrow sense. Empirical postulates such as Keynes' "fundamental psychological law .. that men are disposed, as a rule and on the average, to increase their consumption as their income increases, but not by as much as the increase in their income"², and the theoretical assumptions based upon his "three fundamental psychological factors the psychological propensity to consume, the psychological attitude to liquidity and the psychological expectation of future yield from capital assets"³, are by no means 'simple laws of human nature'. Although they are certainly in some way rooted in 'pure' psychological

1. K.R. Popper, The Poverty of Historicism, op. cit., p. 142

2. J.M. Keynes, op. cit., p. 96

3. Ibid., pp. 246-247

dispositions, as they are employed by theoretical economics they are empirical assumptions stating how (anonymous) individuals will act in a certain sort of social situation - namely an environment where such social phenomena as 'income', 'consumption spending', 'capital assets' etc. exist. The postulates of an economic theory presuppose a particular kind of social situation, and thus they are only fundamental in so far as the formal theory is concerned and not absolutely. Explanations in economics are therefore institutionalistically but not psychologically individualistic. The motives attributed to the anonymous homines economici are (idealised versions of) those that would typically be developed by actual individuals acting as economic agents within the social conditions to which the theory in question is conjectured to be applicable.

Psychologism, as expounded by Mill and as criticised by Popper, has to recommend (if it is to be consistent and not assume anything it wants to explain), that social phenomena in their entirety be explained with reference to the elementary and socially unaffected psychological laws of human nature. It must enjoin social scientists to produce explanations that refer only to the actions of 'individuals' who are motivated by 'pure' psychological drives and dispositions, and who possess minds that are tabulae rasae as far as societal concepts are concerned.

A determined methodological psychologist might retort that what has just been outlined is not the notion of an individual required by psychologism, but an extreme caricature. In defence of his position, he may well admit that social environmental factors are

important in explaining social phenomena as the result of the actions of individuals, but he would deny that the social situation of the acting agents can be regarded as simply given. The methodological psychologist would argue that all social situations are the result of prior sets of actions and thus if one goes back far enough it must be the case that all features of any particular social environment can be explained with reference to the 'innate' psychological laws of human nature. An institutionalistic individualist would have to agree, in reply to the remarks of this imaginary defender of psychologism, that a social environment cannot be considered as given if one means by this the ontological thesis that if all individuals were to be eliminated by a nuclear war, then society would survive them. He would also have to agree that there should be no part of any social situation that could not, in principle at any rate, be explained with reference to some actions of individuals residing in a temporally prior set of social conditions. But does this mean that after all, individualism proper and psychologism amount to the same position?

The opponents of individualism want its advocates to give an affirmative reply at this juncture. Gellner, to take an example of one such opponent, appears to argue that a methodological individualist has so to answer. He starts from the premise that individualism is "an attempt to 'eliminate' so-called 'holistic' concepts, or rather to show that these are in principle eliminable"¹; he then says (as was pointed out earlier) that since human dispositions are causally dependent on

1. E. Gellner, op. cit., p. 489

social circumstances, if a methodological individualist explains a social phenomenon with reference to actions within a social situation (or set of 'social facts') the occurrence of that situation in turn needs to be explained. Although the individualist "may in some cases account in some way for the social facts in terms of the interaction of individual decisions with prior 'social facts', any attempt to eliminate these altogether will only lead to a regress and possibly to an irrelevant genetic question of the hen-and-egg kind"¹. But the mistake here is the supposition that methodological individualists wish to eliminate all reference to social circumstances as initial conditions.² Methodological individualism, to repeat the matter, is a theory of explanation and not a theory of meaning about the kinds of concepts that should be regarded as

1. E. Gellner, op. cit., p. 499

2. If all social phenomena are to be explained psychologically, then it is the case that one is forced into the desperate 'historicist' position of having to operate with the notion of a 'beginning of society'. For, to avoid the infinite backward regress of having to explain social phenomena with reference to actions initiated in some societal initial conditions, one must be able to show how and why society itself appeared in some postulated pre-social epoch when 'men' were motivated only by basic instinctual drives and innate psychological dispositions. But, as Popper remarks, "this theory of a pre-social human nature which explains the foundation of society is not only an historical myth, but also, as it were, a methodological myth". (The Open Society and its Enemies, Vol. II, op. cit., p. 93) For man, or rather his biological evolutionary ancestor, was a social being long before he was ever human.

Strictly, it would thus seem to follow that the first emergence of societal organisation and language as an institution (the latter of these presupposes the former) are events that institutionalistic individualism is, by definition, unable to explain - there being neither human actions nor social initial conditions available with which to do it.

ultimately acceptable in a social scientific explanation.

Institutionalistic individualists need not in the least be perturbed about the existence of an unending regress of explanations. There is a radical difference between: (i) the institutionalistic claim that in any social environment any specific social phenomenon can be explained as the (intended or unintended) effect of the actions of human agents, and (ii) the psychologistic claim that everything in the social environment can be explained as the result of the actions of unsocialised individuals, motivated just by innate psychological dispositions. It is not the individualist but the psychologist programme that requires to assert the claim that "all sociological notions may be analysed without remainder into individualistic concepts or biopsychological concepts of a certain kind"¹.

All explanations (physical and teleological) have to be given, explicitly or implicitly, with reference to initial conditions. Institutionalistic individualists hold, as evidenced by the expository citations given so far, that all social explanations require reference not only to the (operatively) causative actions of individuals, but also to some sort of initial social situation whose existence has to be accepted as given, in the sense that it needs no explanation. Although it is commonly accepted to be hostile to methodological individualism, Maurice Mandelbaum's essay 'Societal Facts' contains nothing that should worry an institutionalistic individualist. (It is to be noted that Mandelbaum did not produce his paper as part of the philosophical dispute

1. L.J. Goldstein, 'Mr Watkins on the Two Theses', op. cit., p. 241

over individualistic explanation. He did not write to refute, or indeed with reference to, any who intentionally contributed to the debate.) Methodological individualists can readily agree with Mandelbaum that no social phenomenon can be explained without reference to features of an initial social environment. Mandelbaum discusses at length whether the functioning of an institution such as a banking system could be explained entirely in terms of the descriptions of the behaviour of individuals. He points out that the behaviour of individuals such as bank tellers is "unintelligible unless one views their behaviour in terms of their status and roles, and the concepts of status and role are devoid of meaning unless one interprets them in terms of the organisation of the society to which the individuals belong"¹. In other words, social explanations require societal initial conditions - but to assert this amounts to a denial of the psychologistic view that all social phenomena can be explained with reference to the psychological laws and processes of unsocialised individuals.

What specific phenomena social scientists actually decide to select for explanation is subject to no restriction whatsoever. But having chosen to explain a particular social phenomenon a social scientist commits himself to regarding some other social phenomena as unexplained initial conditions. The social circumstances which one social scientist decides to accept as his initial conditions, may be those a second social scientist is concerned to explain by reference to actions initiated in the societal circumstances which he is prepared to take for

1. M. Mandelbaum, 'Societal Facts', op. cit., p. 479

granted or to accept as given . Methodological individualism (as espoused by Hayek , Popper and Watkins) is thus not, to adopt the terminology of John Wisdom's essay, "collectively reductionist" but is "distributively reductionist"¹ . According to the former kind of doctrine social phenomena can be explained entirely in terms that employ no societal concepts; according to the latter kind of doctrine social phenomena can be explained only with reference to a societal background.

Thus, the claim that all social phenomena (i.e. any specific phenomenon) can be explained as the (intended or unintended) effects of human actions initiated within certain institutional circumstances, is clearly distinct from the claim that all social phenomena (i.e. every single social phenomenon) can be explained as 'reflections' of the instinctual actions of unsocialised individuals dwelling in a purely physical environment. For the former, institutionalistic individualism, references to social conditions are ineliminable; for the latter, psychologistic individualism, all references to social conditions are in principle dispensable.

This section of the discussion has argued that psychologism is overwhelmingly untenable - it either fails to realise that human nature is causally dependent upon social conditions, or it fails to see that unless the origin of society itself can be explained, social explanations require social initial conditions. Since methodological individualism proper can be differentiated from, and does not entail, psychologism, a rejection of this second doctrine is not a rejection of the first.

1. J.O. Wisdom, op. cit., p. 274ff

(iii) Methodological Individualism versus Methodological Holism

In reply to the second of Leon Goldstein's onslaughts, Watkins attempts to make what he wants to claim unambiguously clear, by setting down the two theses to which a methodological individualist is committed. The first of these is an ontological assertion, while the second is a methodological prescription. In the form in which Watkins presents them, they run:¹

- (1) Human beings (together with their material resources and environment) are the only causal factors in history.
- (2) Explain all social events in terms of human factors.

In reply to Watkins' second rejoinder, Goldstein writes a third article; in answering this, Watkins emphasises that neither he nor Hayek nor Popper are interested, and are not obliged to be interested, in defending a third proposition, namely:²

- (3) Analyse all sociological concepts individualistically.

Section (ii) of this present chapter argued that while (3) is an integral part of the psychologistic programme, it has indeed nothing to do with institutionalistic individualism. One can therefore ignore Goldstein's misinterpretation of the position in question and hold, without further

1. J.W.N. Watkins, 'The Two Theses of Methodological Individualism', op. cit., p. 320
2. J.W.N. Watkins, 'Third Reply to Mr Goldstein', op. cit., p. 244

hesitation, that (1) and (2) correctly summarise the individualist position.

As is clear, (2) is not a categorical assertion, but a methodological precept. However, examination of this prescriptive rule will be helped if one is able to have at hand an expression of (2) in the categorical mode. Such is the following:

(2') Any social phenomenon can be explained as the (intended or unintended) consequence of the interaction of the purposive behaviour of a number of individuals (either specific, or anonymous but typical) acting within a certain institutional setting.

Statements (1) and (2') deserve further scrutiny. (1) constitutes the ontological foundation of methodological individualism. If it is amended slightly, it asserts that individual agents are the only operative causes in society; the societal conditions within which individuals act to pursue their own goals are causal factors not operatively but only as initial conditions. The social background is causally responsible both for affecting individuals' minds, and for constraining their initiated actions in certain ways.

Throughout the literature on this topic, statement (1) has been exhibited as the most important part of the individualist's case.

In his first paper, Watkins writes:

"The ontological basis of methodological individualism is the assumption that society is not some unimagined sort of organism, but really consists only of people who behave fairly intelligibly and who influence each other, directly and mediately, in fairly comprehensible ways." 1

In his second paper this ontological principle is again asserted - "the ultimate constituents of the social world are individual people who act more or less appropriately in the light of their dispositions and understanding of their situation"². This view is then put slightly differently a little later on:

"The central assumption of the individualist position is that no social tendency exists which could not be altered if the individuals concerned both wanted to alter it and possessed the appropriate information." 3

It is the denial of this essential premise that individuals are the only operative causes of social change, that is taken by Watkins to characterise the position of methodological holism. Thus, he claims that "if methodological individualism means that human beings are supposed to be the only moving agents in history, and if sociological holism means that some superhuman agents or factors are supposed to be at work in history, then these two alternatives are exhaustive"⁴.

However, this way of identifying the position of holism does not seem to be entirely satisfactory. If methodological individualism is

1. J.W.N. Watkins, 'Ideal Types', op. cit., p. 732

2. J.W.N. Watkins, 'Historical Explanation', op. cit., p. 505

3. Ibid., p. 506 (Watkins' italics)

4. Ibid., p. 505

is the advocacy of the precept (2), and if this is based on the truth of both (1) and (2'), then the rejection of this truth-functional conjunct can result in one or other of the following two positions:

- (a) The doctrine that (1) is true, while (2') is false, and hence that (2) is unacceptable.
- (b) The doctrine that both (1) and (2') are false, and hence that (2) is unacceptable.

(The theoretical fourth alternative in this 4-way truth-matrix, the falsity of (1) with the truth of (2'), cannot be advanced because the truth of (1) is obviously a necessary, but not of course sufficient, condition for the truth of (2').) For Watkins, what is here called position (b) amounts to social organicism, and this he explicitly identifies (as has been seen) with sociological holism; but he himself, in his second paper, gives two examples of where methodological individualism fails. Of these two examples, the first seems to be more illustrative of position (a) than of position (b). These examples of his will be examined in due course, but in the meantime one will regard both (a) and (b) as varieties of holism, while recognising that only position (b), which involves the denial of the ontological premise (1), appears to be overtly recognised by Watkins as holistic.

Since it is conceivable for methodological individualism to be false in two ways, one has two possible holistic doctrines on one's hands - holism_a and holism_b. The former, 'phenomenological holism', agrees with the ontological premise (1) but considers that there are social phenomena that cannot be explained in accordance with the procedure enjoined by precept (2). This view therefore holds that there are macro-phenomena in society which are sui generis (i.e. which cannot

be explained as either the intended or the unintended consequences of the purposive behaviour of interacting individuals), but which are not manifestations of the activities of any non-human agents. Holism_b, 'ontological holism', wants to assert that (2') is false, and that hence (2) recommends the adoption of an unworkable explanatory procedure, because (1) is held to be false. This is therefore the doctrine that society, societal institutions and other social 'wholes' or 'collectives' have aims, intentions, goals and purposes quite apart from those given to them by human individuals acting in particular societal or institutional roles.

What reasons can be advanced for accepting the two fundamental theses of methodological individualism? Two are suggested by Watkins, but he is careful to add that these only support the principle, they do not entail it.¹

The first reason is the generally disreputable character of scientism (or physicalism). It is the case that in all the social disciplines without exception, what count as the 'objects' of investigation cannot be recognised by their physical properties but only by their intentional or ideational properties. In other words, before one can get down to explaining anything at all in the social sciences, it is necessary to obtain a prior understanding of what things mean to the individual agents living in the society under investigation. One is unable to identify the referents of social concepts such as 'money', 'price', 'inflation', 'rate of interest', 'riot', 'revolution', 'state' and 'nation' using physical criteria in the way a botanist does to identify

1. J.W.N. Watkins, 'Ideal Types', op. cit., p. 729ff

particular taxonomic specimens in a herbarium. In F.A. Hayek's original analysis of methodological procedures in the social sciences, opposition to holism, or methodological collectivism as he calls it, arises mainly as a corollary to his opposition to scientism. On this he writes: "Closely connected with the 'objectivism' (i.e. physicalism) of the scientific approach is its methodological collectivism, its tendency to treat 'wholes' like 'society' or the 'economy', 'capitalism' (as a given historical 'phase') or a particular 'industry' or 'class' or 'country' as definitely given objects about which we can discover laws by observing their behaviour as wholes"¹. It will be recalled that in the first chapter of this present discussion the anti-physicalist position was accepted; therefore one can proceed without further comment to agree wholeheartedly with Hayek in his rejection of scientific holism. It is indeed true that the "belief that the total view will enable us to distinguish wholes by objective (i.e. physical) criteria proves to be just an illusion"². Holism, however, need not be scientific; to show that scientific holism is an unworkable methodology is not to refute, although it may help to discredit, a non-physicalist holism.

The second and more interesting reason given in support of individualism, was touched upon in section (iii) of the previous chapter when Max Weber's theory of ideal types was under discussion. Although the referents of many collective concepts (eg. 'demonstration', 'riot', 'battle', 'church service') do refer to easily observable social entities whose (intentional) properties are agreed upon by all those living in the

1. F.A. Hayek, The Counter-Revolution of Science, op. cit., p. 53

2. Ibid., p. 59

society studied, the exact referents of many others (eg. 'capitalism', 'democracy', 'communism', 'imperialism', 'war', 'nation', 'race') which consist of widespread collections of individual activities, roles, rules and relationships are not immediately discernible. In cases where, even after one has come to understand how the acting agents view particular features of their own society, one cannot find universally agreed criteria for identifying with any degree of precision the referents of far-flung collective concepts, one should not proceed to reify them. This is the error of naive collectivism which "mistakes for facts what are no more than provisional theories, models constructed by the popular mind to explain the connection between some of the individual phenomena which we observe"¹. Instead of vainly attempting to study crudely hypostatized holistic entities, social scientists must show how widespread social concepts can be made precise by the employment of theories that relate together the social connections that are discovered to exist between sets of actions and certain sorts of unambiguously identifiable social phenomena. Indeed, as Hayek continues, far-flung holistic concepts possess distinct, as opposed to merely vaguely comprehended, referents "only if, and to the extent to which, the theory is correct which we have formed about the connection of the parts which they imply, and which we can explicitly state in the form of a model built from those

1. F.A. Hayek, The Counter-Revolution of Science, op. cit., p. 54

Similar sentiments are to be found in the following:

- K.R. Popper, The Poverty of Historicism, op. cit., pp. 135-136, and p. 140
- K.R. Popper, Conjectures and Refutations, op. cit., p. 341

relationships"¹ .

This last point can be understood better if one compares a social scientist to a chemist. The latter, if in possession of the relevant criteria of identification, can undertake an examination of the macroscopic properties of a substance and can form functional laws between their magnitudes, without having to make mention of the constituent molecules. With far-flung social 'wholes', however, overall access to the precise properties is not immediately available. If one tries to identify the referents of such concepts by physical criteria, one falls into the quagmire of scientism. If, when faced with wild disagreement as to the precise criteria of identification on the part of those in the society under investigation, one tries to recognise the referents with non-physical criteria of one's own choosing, one's social inquiry becomes an arbitrary verbal game - the 'wholes' have the properties one says they have merely because one has so defined the denoting concepts. The only way to obtain criteria for identifying far-flung social collectives such that the criteria have a chance of becoming universally accepted among those living in the society in question, is to produce a true theoretical analysis. This will explain the occurrence of particular phenomena by tracing them back to the actions of which they are the (intended or unintended) effects. Once the actual connections between a number of directly observable social phenomena have been uncovered, one should be able to demonstrate clearly that, for instance, certain phenomena that were popularly imagined to be

1. F.A. Hayek, The Counter-Revolution of Science, op. cit., pp. 55-56

features only of a particular social 'whole' (eg. capitalism) were also salient features of another 'whole' (eg. socialism). But such illumination cannot be gained by direct or non-theoretical observation of widespread social collectives; it can only be obtained as the result of a careful examination of actions initiated in particular situations and their social effects - i.e. individualistic analysis.

To recapitulate briefly: the reasons given in support of methodological individualism are two in number. They are, first, that the subject-matter of any social science cannot be identified unless one knows what the acting agents think about their environment, and, secondly, that there are many social 'wholes' which can only be identified via an individualistic theory. But granted, as one is prepared to, the truth of both of these, they neither entail that there do not exist phenomena which can successfully elude individualistic explanation nor that individuals are the only operative social causes. Certainly, both reasons strongly suggest that individualism is the natural explanatory principle, but

nonetheless both are logically compatible with phenomenological and ontological holism.¹

In the second of his two main papers, Watkins admits that methodological individualism is not universally applicable since clear examples can be given of kinds of social phenomena which it cannot be used to explain.² The first sort of phenomena are probability-situations where capricious, accidental or irregular activities by

1. It should be noted that neither phenomenological nor ontological holism is what is subject to the main attack under the name of 'holism' in The Poverty of Historicism. As understood by the important section (23), holism is the doctrine that recommends that the social sciences investigate all (literally all) the properties of society, for to select certain features of the social structure and to explain only them is to destroy knowledge of 'reality itself' as it exists in all its richness of detail.

To rebut this approach, Popper writes that the term 'whole' is extremely ambiguous; it is "used to denote (a) the totality of all the properties or aspects of a thing, and especially of all the relations holding between its constituent parts, and (b) certain special properties or aspects of the thing in question, namely those which make it appear an organised structure rather than a 'mere heap'". (op. cit., p. 76) To say that a 'whole' in sense (b) is more than the sum of its parts is the trivial assertion that the 'thing' is not just a bare collection, but is an integrated structure possessing some aspect which one can recognise and see how all the component parts are organised. But the scientific propriety of examining 'wholes' in sense (b) cannot be traded upon as a basis for urging that society should be studied as a 'whole' in sense (a).

Clearly, no such study can be undertaken; all inquiry has to be strictly selective in character. Since a property is simply a respect in which one thing resembles or does not resemble something else, it is the case that the properties of any entity are infinite in number; thus one cannot (logically cannot) comprehend all the properties of even the most humble of objects such as a crust of bread, let alone all the properties of something as complex as a society. Therefore, to urge that social inquiry be unselective, is to recommend that social phenomena be studied by a manifestly absurd method.

2. J.W.N. Watkins, 'Historical Explanation', op. cit., p. 507f

individuals (eg. suicides, murders, motor accidents, the posting of unaddressed letters) all add up to fairly regular and predictable social aggregates. Secondly, methodological individualism does not work in cases where one is 'driven' by the nature of the phenomena to treat them as if they were organic entities. The example given of this kind of phenomenon is that of a wild crowd, as part of which individual agents are mindlessly carried along by a mob consciousness to do things they would not have done if they had paused for a moment to reflect on their behaviour.

These two kinds of phenomena -- both 'recalcitrant' as far as methodological individualism is concerned -- are very different in character, and will be looked at separately in greater detail.

The appearance of constant statistical regularities in society where no regularities can be detected at the individual level poses a problem for the individualist. It is to be noted carefully that it is not particular instances of such things as murders and suicides, nor the fact that the levels of such actions may alter as the general social environment alters, that are the problem, but the constancy per time period of the aggregate totals for all phenomena of one kind in any set of social conditions. The constancy of statistical aggregates cannot be explained, as methodological individualism requires, as either the intended or the unintended result of the rational behaviour of interacting individuals striving to achieve their own chosen goals. Variations in the levels of the aggregates in different societies may be explained by showing to what extent the differences in societal features dispose agents to act in the way in question. And these features may then be explained individualistically (as the result of goal-seeking behaviour within a

temporally prior institutional setting), but to do this is not to show why a constant total of irregular actions emerge in a given period of time.

But what follows from recognising the existence of 'irreducible' statistical tendencies? To make such an acknowledgement is neither a vindication of the thesis (as Mill triumphantly proclaims¹) that men's volitions are indeed governed by the 'law of invariable causation', nor does it mean that one must think that 'group-minds' or 'super-individuals' are at work in society. Probability-phenomena do indeed constitute an exception to methodological individualism in favour of phenomenological holism; but since the breach in the (categorical mode of) the principle is not very great - only proposition (2') is violated and not (1), the important ontological foundation - and only for a limited area, the existence of such social phenomena is no reason for the methodological individualist to become over worried.

If one now moves from these probability-situations to the second type of social phenomena to which the principle of methodological individualism is inapplicable, one is faced with occurrences which if their appearances were not very infrequent, would undermine the principle, or at best severely circumscribe its range of effective application. However, if one has to adopt an organicist approach only rarely, and even then only with reference to social groups whose

1. J.S. Mill states that instances of a "singular degree of regularity en masse, combined with the extreme of irregularity in the cases composing the mass, is a felicitous verification a posteriori of the law of causation in its application to human conduct". (A System of Logic, op. cit., p. 609, i.e. Book VI, Ch. XI, sc. 1; Mill's italics) The existence of probability-phenomena in society, does not, of course, establish any such conclusion as to the nature of human actions.

members are virtually physically contiguous with each other, then individualism can survive with honour and continue to be upheld as supplying, barring the few minor exceptions, the generally applicable pattern of explanation within the social sciences. But how often is one 'forced' by the intrinsic nature of an explanandum-phenomenon to explain its occurrence by postulating the existence of a 'group-consciousness' which 'wills' the actions of the individuals who are the members of the social collective in question?

One can start to answer this question, by trying to seek answers to two closely related sub-questions, namely:

- (i) How are the actions of social 'wholes' to be explained?
- (ii) Do social 'wholes' have minds of their own which 'decide' what their component members do?

On the supposition that (ii) can be answered in the negative, the answer to (i) can easily be given. If the 'whole' is a group, such as a crowd or a football team, whose members are in close physical proximity to each other, to explain its actions is just to explain the actions of its members consciously attempting to co-operate together in some common enterprise. If the 'whole' is a far-flung monocentric institution such as a trade union, an industry or a government, to explain its actions is to explain the actions of those (sometimes a very few) of its members who are entitled by socially accepted rules to act in its name. If the 'whole' is a far-flung polycentric structure such as a market economy, when one refers to its actions one is referring either to the many different sorts of actions of its members, or to the (intended or unintended) results of these activities. All this sounds

pretty truistic. But appearances here are not misleading: a football team (literally) does not win a match, but its members acting together do; a trade union does not go on strike, but trade unionists do; an economy does not respond to a budget, but economic agents do and their responses have wide social consequences. Thus, in normal circumstances there is no mystery surrounding the actions of social 'wholes'. There is no reason to think that an individualistic analysis is not perfectly in order.

This 'favourable' reply to (i) was obtained on the assumption that (ii) could be answered negatively. But is this the case?

One example of where just such a reply cannot be given has already been mentioned. If a closely knit crowd goes berserk, its members behave as if they were in a waking trance; they do not decide to go along with the collective frenzy, they just do. Hence, instead of explaining the actions of each member of the group, it is more appropriate to treat the 'whole' as a social organism with a life of its own and which positively 'wills' the behaviour of its component parts. However, such organism-like social phenomena are not every-day occurrences, and even when they do appear their life is seldom very long. Thus, while they constitute a second kind of formal exception to methodological individualism (this time in favour of ontological holism), they are too infrequent to be anything but unimportant.

More serious, however, is the question of whether far-flung collectives need ever be thought of as having minds of their own. Does one ever have to postulate that society itself 'thinks' for its individual members and 'induces' them to pursue certain goals, the

consequences of which pursuit serve, unknown to the conscious actors, the 'needs' of the 'whole' to which they belong? The answer, to anticipate what will be argued in the following section, is that while one cannot offer concrete disproof of the existence of long-lived 'collective minds', there are overriding reasons why such 'entities' need never be invoked as explanatory requirements.

(iv) Functionalism - 'Strong' and 'Weak'

It may be thought that some support for the holistic denial of the ontological proposition that human agents are the only operative causes of social change, can be drawn from the fact that in sociology and social anthropology the results of 'functional analysis' are presented in teleological or purposeful form. However, on closer inspection of the issue, the claim that support for ontological holism is forthcoming from this source simply evaporates. The question of such support can only appear when 'functionalism' is understood in an unacceptable way, and it just vanishes when the method is taken in the way its modern spokesmen urge it to be taken.

The three main figures responsible, historically, for the development of the functional approach were the anthropologists Bronislaw K. Malinowski (1884-1942) and Alfred R. Radcliffe-Brown (1881-1955), and the present day sociologist (to whom reference has previously been made) Robert K. Merton (1910-). An exceedingly important difference between the functional analysis developed by Merton for sociological

inquiry and the 'functionalist' and 'structuralist' approaches developed by his two respective predecessor exponents for anthropological research, is that Merton never asserts that his analysis constitutes a form of explanation. (The difference in disciplines here is only of verbal relevance; Radcliffe-Brown himself lends support to defining "social anthropology as that branch of sociology that deals with primitive societies"¹ .) But this is precisely what Malinowski and Radcliffe-Brown want to maintain, and it is just because they want to do this that the philosophical problem concerning ontological holism is pushed into prominence. Since much depends on the point that there is a 'right' and a 'wrong' way of viewing the functional approach, the matter deserves some elaboration.

In his well known and frequently cited article written for the thirteenth edition of The Encyclopaedia Britannica , Malinowski is eager to impress upon his readers that the functional approach to culture "supplies the right theoretical foundation for the practical application of anthropology"² . In describing what this involves,

1. A.R. Radcliffe-Brown, Structure and Function in Primitive Society, (Cohen and West, London, 1952, edit., 1963) p. 2
2. B. Malinowski, 'Anthropology', in The Encyclopaedia Britannica, Supplementary Volume I (The Encyclopaedia Britannica Company Ltd., London, thirteenth edition, 1926) p. 135

he writes:

"This type of theory aims at the explanation of anthropological facts at all levels of development by their function, by the part which they play within the integral system of culture, by the manner in which they are related to each other within the system, and by the manner in which this system is related to the physical surroundings. ... The functional view of culture insists upon the principle that in every type of civilisation, every custom, material object, idea and belief fulfils some vital function, has some task to accomplish, represents an indispensable part within a working whole." ¹

Let us leave aside for the moment what is asserted by the the second half of this quotation. A point of view similar to that expressed in the first half, is to be found advanced by Radcliffe-Brown ; in a particularly revealing passage, there is this fascinating methodological pronouncement:

"The postulate on which the (functional) method depends is that there are certain general 'physiological' laws, or laws of function, that are true for all human societies, for all cultures. The functional method aims at discovering these general laws and thereby at explaining any particular element of any culture by reference to the discovered laws." ²

However, in his systematic outline of the theoretical foundations of modern functional sociology, Merton austerey, and in direct opposition to Malinowski and Radcliffe-Brown , takes the "central orientation of functionalism .. (to be) the practice of interpreting data by establishing

1. B. Malinowski, op. cit., pp. 132-133 (*italics added*)
 2. M.N. Scrinivas (ed), Method in Social Anthropology, Selected Essays by A.R. Radcliffe-Brown, (The University of Chicago Press, Chicago, 1958), pp. 40-41 (*italics added*)
- In the above quotation from Radcliffe-Brown, one notes (in passing) that the belief in universally operative laws of society is strongly scientific in character. One should not, therefore, take seriously the claim that there are 'physiological laws' of social phenomena.

their consequences for larger structures in which they are implicated"¹ .

The entire problem concerning ontological holism in functional sociology arises out of what on the face of it seems merely a trivial point. But in fact, if functionalism is understood as a method of explanation (i.e. what will be called 'strong' functionalism) and not merely as a method of interpretative analysis (i.e. 'weak' functionalism) , then this does indeed endanger methodological individualism. For, in certain circumstances (to be outlined in a moment), to say 'the occurrence of actions A (eg. customary or institutional practices of some sort) is to be explained by the fact that they fulfil function F ' may be considered to invoke by implication the existence of a long-lived collective consciousness or group-mind, or some other non-human mental 'entity' .

In Merton's codification of functional procedures, the object of analysis has to be "a standardised (i.e. patterned and repetitive) item, such as social roles, institutional patterns, social processes ... etc."² . The set of actions which go to make up items of this sort may or may not have societal repercussions. With reference to

1. R.K. Merton, Social Theory and Social Structure, op. cit., p. 101
(italics added)

-- An interpretation of a phenomenon (as opposed to an explanation of it), is a description phrased in the appropriate manner; and by 'in the appropriate manner', is meant 'in the terminology of the theory (or group of theories) developed to investigate the realm of events to which the phenomenon in question belongs' .

2. R.K. Merton, op. cit., p. 104 (Merton's italics)

actions that do, Merton writes:

"Functions are those observed consequences which make for the adaptation or adjustment of a given system; and dysfunctions, those observed consequences which lessen the adaptation or adjustment of the system. There is also the empirical possibility of nonfunctional consequences, which are simply irrelevant to the system under consideration." ¹

To say that an action (or set of actions) is functional within a given social framework is to say that its initiation is sufficient to bring about consequences which serve either the vital biological or psychological needs of each individual member of the society in question, or which serve the 'necessities of group existence'. Functional consequences may be the intended or the unintended results of the initiation of certain actions; if they are the former they are known as "manifest functions", and if the latter "latent functions"².

If one is concerned with manifest functions, then it is in fact quite compatible with methodological individualism to regard functional analysis as constituting an explanation as well as just an interpretation of the actions which brought them about. To explain the occurrence of a custom by pointing to its manifest function (or functions) is equivalent to offering a rationality (or a rational) explanation of why the agents concerned acted as they did. They initiated their actions in order to bring about something which would be to the benefit of each of them individually, or to bring about something which would ensure some desired alteration to the structure of the society in which they lived.

1. R.K. Merton, op. cit., p. 105 (Merton's italics)

2. Ibid., p. 105

Thus, even if one should take functional analysis to be an explanatory procedure, if it is applied to actions and their manifest functions, it is, from the individualist standpoint, methodologically unproblematic.

But like theoretical economists, functional sociologists are more interested in discovering whether any unintended consequences of certain sorts of actions are produced, than in finding out whether the intended results of the acting agents are in fact achieved. "It is precisely the latent functions of a practice or belief which are not common knowledge, for these are unintended and generally unrecognised social and psychological consequences. As a result, findings concerning latent functions represent a greater increment in knowledge than findings concerning manifest functions"¹. Suppose one is examining the ceremonials of the Hopi Indians designed to produce abundant rainfall. If one confines oneself to the question of whether the intended result is achieved or not, the matter becomes more a problem for the meteorologist than for the anthropologist. But if one is prepared to extend one's inquiry beyond the occurrence of meteorological phenomena into possible repercussions of the rain-making ritual upon the groups that conduct the ceremonial, one may discover that this and similar activities "fulfill the latent function of reinforcing the group identity by providing a periodic occasion on which the scattered members of a group assemble to engage in a common activity"².

As far as methodological individualism is concerned, there is no problem connected with advancing any of the following: (i) the

1. R.K. Merton, op. cit., p. 122 (Merton's italics)

2. Ibid., pp. 118-119

occurrence of the Hopi rain-ceremony reinforces the group identity, (ii) the important significance of the Hopi rain-ceremony is that it reinforces the group identity, and (iii) the social cohesion of the Hopi is to be (partially) explained by the performance of the rain-making ceremony. However, because the occurrence of group reinforcement is a latent and not a manifest function of the rain-ritual, to assert the following is to raise a host of problems: (iv) the occurrence of the Hopi rain-ceremony is explained by the fact that it brings (or, helps to bring) about the reinforcement of group identity. With (iv) one is asserting that a certain sort of actions are performed not for the reasons given by the acting agents, but because their unintended consequences satisfy the 'needs' of the social 'whole' to which the agents belong.

The most trenchant of J.C. Harsányi's criticisms against functionalist explanations is clearly only relevant to the claim that social phenomena can be explained by reference to their latent functions, and not to the claim that such can be explained by reference to their manifest functions (where these exist). Harsányi writes:

"If functionalistic explanations are to have any explanatory value then it is not enough simply to assume that social institutions arise in response to actual social needs. Rather, one also has to be in a position to point out the specific social mechanism (or mechanisms) by which these social needs supposedly give rise to institutions satisfying these social needs - and functionalism plainly fails to do this." 1

But is such a 'mechanism' not provided if one postulates the existence of a group-mind which is to society what the human mind is to an individual

1. J.C. Harsányi, op. cit., p. 307 (Harsányi's italics)

agent? Under this theoretically hypothesised 'mechanism', the collective consciousness 'thinks' for the constituent individuals and 'makes' them initiate (for misleading or false reasons), actions that produce, as unintended consequences, social results which are functional for the adaptation or persistence of the 'whole' . But why is this conjecture so implausible?

In the case of a frenzied crowd it is legitimate to postulate the coming into existence of a short-lived mob consciousness. All the individuals are in close physical proximity to each other and their minds become 'fused into one'. They then act together to do things as a collectivity, but afterwards the agents are unable to provide any justification for their behaviour; literally, they were 'carried along with the crowd'. However, one cannot accept that a far-flung social collective has a mind which 'makes' prima facie free agents act to bring about consequences that are not of their own choosing. The only evidence for the existence of a group-mind would be the truth of a statement of the form 'the occurrence of actions A is explained by their having the latent function F ' ; but the truth of a statement of this form could only be established if some 'mechanism' of the requisite sort had been demonstrated to exist. In short, the group-mind hypothesis is quite ad hoc. The only evidence for it, consists of the very phenomena its existence is supposed to be used to explain.

Since one cannot show a non-contradictory uncircumscribed existential statement to be false, one is unable to produce a definite disproof of the existence of group-minds. But, nonetheless, one is entitled to reject the group-mind hypothesis as unacceptable, if no

satisfactory evidence is forthcoming. If one does this and can find no other suitable functionalist 'mechanism', one must therefore conclude that one cannot explain, as Malinowski and Radcliffe-Brown would have one believe, the occurrence of sets of societal actions simply by pointing to their functions regardless of whether these are manifest or latent. To discover that a set of actions has a latent function, or a number of latent functions, is not to explain the set of actions (although of course it is to explain the occurrence of the functional consequence, or consequences) .

In that they are interpretative and not explanatory of what produces the function, statements in the social sciences referring to latent functions are similar (in this respect) to functional statements in biology. To say 'the function of the heart is to circulate the blood through the organism' is not to explain the occurrence of the heart, but to interpret¹ the consequences of its operation for the organism of which it is a part.

Whenever a functional statement is used in the biological sciences, inquirers (implicitly) assume that the process to which it refers is an integral part of a complex and more extensive system such as an individual organism or an ecological structure. Such a system is self-regulating, by means of a homeostatic or negative-feedback mechanism, with regard to one or more of its properties or to its own continued existence as a self-maintaining structure of mutually dependent

1. In this instance, 'to interpret' means 'to describe in the technical vocabulary of the theories constructed to investigate the partial processes of biological entities' .

relationships.¹ When asserted against this sort of initially assumed complex system of order, functional statements in biology are true if the following are true: (i) a statement that a pertinent causal connection holds (eg. 'the pumping of the heart is sufficient to ensure the circulation of the blood through the organism'), and (ii) a statement or set of statements giving an account of the way the causal effects of the working of the particular component, contribute both to the continuation of its own operation and also to the operation or persistence of the rest of the general structure in question (eg. 'the circulation of the blood is necessary for the physical survival of the organism and hence for the continuing operation of the heart'). From this explicatory outline, it should be manifestly clear that functional statements in biology in no way require the existence of dynamic entelechies or vitalistic goal-seeking 'agents' behind either the system itself, or the particular part whose operation is sufficient to ensure

1. C.G. Hempel, 'The Logic of Functional Analysis' (1959), in Aspects of Scientific Explanation, op. cit., pp. 297-330
- E. Nagel, The Structure of Science, op. cit., pp. 398-428
- E. Nagel, 'A Formalisation of Functionalism' (1953), in E. Nagel, Logic Without Metaphysics, (The Free Press, Glencoe, Illinois, 1956), pp. 247-283

the fulfilment of the discerned organic necessities.¹ To point to the discovered function (or functions) of a particular component of a complex biological system, is not to explain the occurrence of the system; to explain the occurrence of the system, is the task of the theory of evolution.

Even if one agrees that in sociology and social anthropology, the appearance of institutionalised actions which give rise to latent functions cannot be explained simply by pointing to these functions, it is nevertheless the case that things like the Hopi rain-ceremony require explanation. A methodological individualist will explain such activities by uncovering the consciously held aims and purposes of the agents concerned. The Hopi ceremony is explained by discovering that it is practiced in order to produce rainfall. Whether or not the social scientist holds that the ceremony is an objectively rational choice of means to the Hopi's chosen goal, is beside the point as far as both

1. The claim that vitalistic agents can be successfully banished from biology, by no means entails the further claim that all biological statements (including those which refer to self-regulating systems and structures) can be translated without remainder into statements of physics and chemistry. Although all biological processes are causally dependent upon physico-chemical processes, this provides no support for the contention that the former are 'really' nothing but the latter; for the structural principles of (say) an organism cannot be described in language which uses only the concepts required by physics and chemistry. Since the concept 'organism' (at the very least) is ineliminable from theories of biological phenomena, the biological sciences cannot be 'reduced' to the physical sciences. (It is also the case that for similar reasons, chemistry cannot be 'reduced' to physics; and that within physics itself, thermodynamics (for instance) cannot be 'reduced' to mechanics.)

the general form and particular content of the explanation go; the divergence of the actual behaviour from the social scientist's own standard of rational behaviour, is to be accounted for by the empirical fact (let it be assumed) that the Hopi have not yet developed a critical approach to the matter of the acquisition and growth of knowledge pertaining to physical causation. However, by the appropriate criteria of ideal rationality, the rain-ritual doubtless appears highly successful: have there not been a very large number of convincing instances of the power of the ceremony to produce the desired rain? ; when rain is not forthcoming, is not this because mistakes were made in the performance of the ritual or because on that particular occasion it should have been performed more than once? . Thus, provided the social anthropologist argues generally along lines such as these (the substantive content of the explanation in each case would of course have to be decided empirically by field-research), explanations of the occurrence of sets of societal actions which in no way achieve their intended goals, but which lead to important unintended functional consequences, can be accommodated without difficulty in the methodological framework of the institutionalistic individualist.

It is patently false that a society is an organism. But how far is a society like an organism? The analogy between social and organic structures is clearly not entirely misconceived. A society is similar to a biological organism in that it is an inherently complex 'field' or 'area of happenings' that is made up of a multitude of interacting, inter-dependent and mutually self-adjusting relationships and dependencies. An organism, however, can be thought of as a highly integrated 'whole' in the way that a society cannot (or, at any rate, a

society that has advanced even a modicum of distance from the most primitive non-literate state) . With an organism the 'end' (or 'ends') which the 'goal-directed' behaviour of the system 'intends' to 'achieve' can be unambiguously established, because within an organism the functional consequence of the operation of any part is both functional for the working of the entire structure and also uniformly functional for the continued operation of all the other partial processes. With a society, however, this is not the case. The constituent individuals, groups and institutions within a society do not act together in such a way that the (intended and unintended) effects of the interplay of their actions fit harmoniously together to form a functionally unified 'whole' whose 'goals' can be discovered without much difficulty. A society, unlike an organism, is a highly differentiated structure; what is a beneficial societal consequence for one important component, may be an undesirable and adverse result as far as another is concerned.

As well as the important criticism that was discussed earlier in this section (namely, that one cannot explain a set of actions by pointing to its latent function), Harsányi's essay produces three other subsidiary shortcomings of the functionalist approach in sociology. These are:¹ (i) it assumes that every social institution serves some useful function, and thus it tends to overstate the efficiency of existing social institutions in serving social needs and to neglect any inefficiencies and detrimental social effects of these institutions, (ii) it plays down important conflicts of interests among different

1. J.C. Harsányi, op. cit., pp. 306-307

social groups which exist in all societies with the exception of the most primitive, and (iii) it is ideologically committed in virtue of an unduly conservative bias . A careful examination of these points shows, however, that they are no more applicable to the functional analysis developed by Merton , than is the major criticism Harsányi produces.

With reference to (i), one finds that Merton emphatically denies, for instance, what Malinowski vigorously asserts in the second half of the quotation given earlier from his article in Britannica ; namely, that functional analysis should work with the "postulate of universal functionalism"¹ (i.e. the foundational assertion 'every institutionalised action has some functional result') . Merton holds that it is, as a matter of fact, just not fruitful to study society on the supposition that this postulate is true. With reference to the second and third points from Harsányi's critique, Merton unequivocally rejects both the "postulate of the functional unity of society"² and the "postulate of indispensability"³ : functional analysis, as Merton develops it, neither assumes that any given society is a fully integrated or functionally unified organismic 'whole' (i.e. a social 'whole' with no serious or unresolvable internal conflicts) ; nor does the analysis hold either that a particular function may not be diversely fulfilled by alternative kinds of activities, or that indeed any particular function is crucially necessary for the substantial survival of the 'whole' .

1. R.K. Merton, op. cit., pp. 84-86

2. Ibid., pp. 79-84

3. Ibid., pp. 86-91

Although Harsányi mentions¹ Merton by name as one of those to whom his criticisms apply, Merton in fact advocates a functionalistic approach that escapes every one of the strictures Harsányi brings to bear on what he takes functionalism to be. His censorious critique appears to be pertinently directed only against an extreme organicist version of 'strong' functionalism. Thus, Merton's organismically tempered version of 'weak' functionalism can emerge unscathed. Merton's functional analysis in no way appears to be incompatible with methodological individualism as expounded by Hayek, Popper and Watkins.

Granted that when appropriately explicated functional analysis is methodologically unobjectionable, how suitable is it as a method for the social sciences generally? At first sight, it appears that functional analysis is applicable whenever the explicandum-phenomenon takes place within an initial environment which (with regard to one respect) can be viewed as a means-end structure and has effects that play a part in the continuing operation of such a system. It is therefore surprising to find that in economics, which of all the social sciences concerns itself with the workings of clearly identifiable feedback mechanisms, one only infrequently comes across statements in functional form.

It is not the case that true statements involving reference to unintended consequences which affect the operation of a means-end

1. J.C. Harsányi, op. cit., p. 305

system (i.e. consequences sociologists call 'latent functions'), and which result from the interplay of actions directed towards the achievement of economic ends, cannot be constructed. For example, take the following:

- (i) The function of the price system is to ensure that whatever is demanded is actually supplied, and whatever is supplied is actually demanded.
- (ii) The function of the activity of saving is to (help to) provide a fund from which, by the process of competition, the profitable industries can obtain necessary investment capital.

In principle, these are quite in order. The price system is not like an army; it is not a conscious creation (purposively designed to procure a number of pre-determined goals), but is a spontaneous system of unintended polycentric order arising as the result of many men, groups and institutions rationally pursuing their own different economic goals. It also succeeds, to a greater or lesser degree, in carrying out the function imputed to it. It is likewise the case that men do not save (i.e. abstain from immediate consumption) for the purpose of providing industry with investment capital, they do it to secure a monetary return in the future for themselves. But their actions to this desired end, nevertheless, have the unintended (but not, of course, unrecognised) consequence of supplying industry with a source of capital upon which it can draw for its investment requirements. The fact that the investment needs of industry can thus be met, ensures that many individuals in the future will be in a position to undertake acts of saving.

Despite their truth, statements such as (i) and (ii) are not often to be found in economic literature. The reason is that there is simply a difference in the emphasis of interest between sociologists and economists. The latter are primarily interested in discovering

social generalisations which will enable them to explain and predict the societal repercussions of the initiation of certain kinds of actions within a particular means-end structure (such as the price system), the existence and general properties of which are taken for granted. On the other hand, functional sociologists do not restrict their investigations to the task of establishing generalisations concerning phenomena which result from the rational pursuit of economic goals; they are concerned, inter alia, to learn what specific environmental conditions must prevail, in order that a particular means-end structure (eg. an economic, legal or political system) in a given society may persist through time.

What final assessment should be made of methodological individualism? To make sense of the principle (it will be recalled), it should on no account be viewed as the social scientific correlate of mechanism in classical physics; for if this is done, then the way is open for an identification of the principle with the highly unsatisfactory position of psychologism. If however the principle is correctly understood, then it indeed seems to be the case that (for the vast majority of societal phenomena), methodological individualism enjoins social scientists to adopt the appropriate explanatory pattern; that is to say, it recommends that every social scientific explanation should be formulated as an answer to the question: 'From the interaction of what set of purposive actions, does this phenomenon result as the (intended or unintended) effect in this institutional setting?' .

VI CONCLUSION

We shall now briefly bring together the main results of the present inquiry into the nature of the social sciences and their methods of explanation.

It has been argued that all social explanation is concerned (directly or indirectly) with the activities of human agents attempting to achieve their ends within a given institutional setting. The social sciences have nothing to say about the actions of unsocialised men or of individuals as they would be if unaffected by contact with any society. Human beings, indeed, are what they are and largely behave the way they do because their minds have been moulded by the society into which they were born and reared. In a clear sense, therefore, a society does not merely exist outside men but resides within them as well.

The process of explaining social phenomena takes place on two closely inter-connected levels. On the lower level, we are concerned to explain single actions. To show why a single action occurred, we give either a rational explanation (if our aim is simply to indicate the intention in the mind of the agent who initiated the action in question) or a rationality explanation (if our aim is to show why the agent acted one way rather than another to achieve his goal or goals within the circumstances in which he found himself). The central and most controversial component of the latter (and more commonly encountered) of these two types of explanation is the

'principle of rationality'. On the higher level, we seek to explain the societal consequences of the purposive activities and behavioural interactions of many men. To account for such phenomena (the main sort of interest to economics, sociology and political science) it is appropriate to employ an individualistic analysis. Within this kind of analysis, the concept 'unintended consequence' plays a major role; there is, however, no need to follow Popper and Hayek in virtually identifying the subject-matter of the social sciences with the results of human action which are not the results of human design. We can hold that the unintended repercussions of intentional human action bulk large among the social phenomena worth studying, without having to assent to the view that the undesigned results of human action are the only social phenomena worth studying.

Of the social sciences which aspire to the status of a theoretical science, economics is manifestly the most successful. This discipline examines the actions by which scarce resources are used to satisfy competing human needs and desires, and the immediate societal consequences (both intended and unintended) of these actions; thus, a particular phenomenon is called 'economic' if it is involved in a process of valuation for the purposes of exchange. Because such valuations can be expressed in money terms (i.e. as market prices), the diverse and heterogeneous objects and activities upon which the market mechanism bestows a value can be measured quantitatively and rendered amenable to linear addition. Since a clear conception is available of the respect in which all economic phenomena can be viewed as similar (i.e. as possessors of some measure of monetary valuation), a theoretical science of such phenomena is possible.

Although much of economic theory can be presented in mathematical form, this should not lead us to think that economics is able to give 'explanations of detail' of the events which fall within its province. All social phenomena are inherently complex; that is to say, all social phenomena are brought about by a large number of factors, only a few of which can possibly be taken into consideration for the purposes of explanation. But even though we should not expect to achieve 'explanations of detail' of human and societal events, we may hope to obtain 'explanations of the principle' upon which our explananda-phenomena are produced. An 'explanation of the principle' does not enable us to account for the precise properties possessed by a specific economic phenomenon, but it does permit us to show why the phenomenon in question is a member of a particular range of events rather than a member of some other range.

Finally (and perhaps most important of all), it should be mentioned that whenever reference is made to the causation of social phenomena one must not think that the use of the term 'causation' implies that human actions (and the societal consequences resulting from them) are produced in precisely the same fashion as physical events. Human individuals, unlike physical objects and processes, are rationally purposive agents who are moved to act because their minds interpret the behaviour of other men and their own environmental conditions in certain ways. Hence, social scientists are only in a position to identify the causes and effects extant in a given social situation after they have acquired an

understanding of how the agents under investigation view themselves and their society. That the social sciences deal with meaningful (as opposed to purely physical) phenomena, is the source of most of the special difficulties associated with the study of human and societal phenomena; for example, since social phenomena are in a sense constituted by thoughts, the possibility is ever-present that one's predictions of such phenomena will be controverted because the act of prediction has itself become a relevant causal influence. It is therefore reasonable to contend that a golden rule for social scientists is this: Remember that what is being studied belongs to a different order from what the natural scientist studies, and that accordingly questions asked of social phenomena should be appropriate to the basic nature of such phenomena.

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