A. THE PHILOSOPHY OF SCIENCE

1. The rise and fall of positivism
   (1) Observations are concept-laden.
   (2) Observations are hypothesis-laden
   (3) Observations are value-laden
   (4) Observations are interest-laden
   (5) Observations are laden with culture-specific ontologies

2. Current epistemological theories [Web 2]
   (a) Predictive instrumentalism
   (b) Conventionalism
   (c) Rhetorical analysis
   (d) Phenomenology
   (e) Evolutionary epistemology
   (f) Kuhn’s paradigm model
   (g) Lakatos’s methodology of scientific research programmes
   (h) The ‘strong programme’ in the sociology of science

3. Cognitive instrumentalism [Web 3]
   (a) Science, intelligibility, and public knowledge
   (b) Theories, facts, and empirical adequacy
   (c) The problem orientation of science
   (d) Science and non-science

B. THE STUDY OF SOCIAL PHENOMENA [Web 4]

1. Social science and natural science
2. Mentation, individualism, and holism
3. The problem of objectivity

B. THE STUDY OF SOCIAL PHENOMENA

According to Harry Elmer Barnes, the social sciences were created by the industrial revolution, which he describes as ‘the greatest transformation in the history of humanity’. This revolution ‘broke down the foundations of the previous social system’ and ‘out of the confusion, as an aid in solving the newly created social problems, ... to reconstruct the disintegrating social order’ sociology and the other social sciences came into being (An Introduction to the History of Sociology, 1948, pp. 47 f.). There is much to be said for this view, emphasizing as it does the relation of the social sciences to the social problems that attended the development of a much more complex system of economic and social organization. Nevertheless, the social sciences remained for a long time almost purely academic disciplines. Economic theory made some tentative appearance in the eighteenth-century discussion by businessmen of international trade and the monetary system, and political theory in the English constitutional debates of the seventeenth century, but, as we have seen, systematic social theory was largely a nineteenth-century
creation and, though it was clearly oriented to the discussion of contemporary social
problems, its main venue was the academy rather than the domain of practical affairs.

Indeed, up to the middle of the present century, professional economists, political
scientists, and sociologists found employment almost entirely in academic
institutions. Since then, the nature of the social science professions has undergone a
profound change. There are far more economists today in governmental agencies
and business firms than in the universities and, to only a lesser degree, professional
sociology and political science have experienced a similar transformation. If we take
the view that the willingness of practical men of affairs to spend good money for a
service certifies its value, the social sciences are today generously certified. One of
them, economics, has since 1969 even been endorsed by the highest court of science
itself, the Nobel Prize Committee.

Nevertheless, considerable doubt remains concerning the scientific credentials of the
social disciplines. Philosophers of science, and practitioners of the disciplines
themselves, continue to question their epistemic foundations, some castigating social
scientists for failing to adopt the proven methodology of science, others complaining
that they have been led to pursue incorrect methods by attempting to imitate the
natural sciences, still others contending that the very idea of a 'scientific' study of
social phenomena is a delusion, or an abomination. Moreover, some writers on this
question have radically altered their views. Alexander Rosenberg, a philosopher
whose initial work was on the epistemological foundations of economics
(Microeconomic Laws a
Philosophical
Analysis, 1976) declared the discipline to be truly
scientific in its methodology, even by comparison with physics, and strongly defended
the economist's use of mental entities, such as preferences and purposes, as causal
factors. But more recently he has declared that such entities do not have causal
status (Sociobiology and
the Presumption of
Social Science, 1980); that economics is
not really an empirical science but just a branch of mathematics ('If Economics isn't
Science, What is it?' Philosophical
Forum, 1983); and that the social sciences in general are not yet
sufficiently developed to permit a philosopher to subject them to epistemic analysis
('Philosophy of Science and the Potentials for Knowledge in the Social Sciences', in
Donald W. Fiske and Richard A. Schweder, eds.,
Metatheory in Social
Science, 1986).

I cannot undertake here to review the literature on the philosophy of the social
sciences, or even to do so comprehensively for one of them. In any case, many of
the issues that have been addressed, and the positions taken, parallel those
examined in the preceding section. I shall concentrate on three matters that seem to
me to require special attention: the relation between the social and natural sciences;
the epistemic status of mental states and the debate between individualism and
holism; and the problem of objectivity.

1. Social science and natural science

The notion that the social sciences must be judged by reference to the natural
sciences has been, and continues to be, the most prominent theme in the
literature of the philosophy of social science. It not only reflects the general view
that there is a 'unity of scientific method' but, more specifically, that the practices of
the natural sciences constitute the standards to which the social sciences are obliged
to conform. This notion has survived the controversies among philosophers concerning the philosophy of science. As we have seen, there is, today, no consensual view of this. Presumably, the social sciences are to be judged by reference to the natural sciences even though there is no agreement concerning the epistemic foundations of the natural sciences themselves.

Isaac Newton said, in reference to the methodology he practised: ‘if natural Philosophy in all its Parts, by pursuing this Method, shall at length be perfected, the Bounds of Moral Philosophy will also be enlarged’ (quoted from the Opticks in S. A. Grave, The Scottish Philosophy of Common Sense, 1960, p. 7). In the terminology of his day Newton meant ‘moral philosophy’ to include not only ethics but the study of human and social phenomena. By the eighteenth century Newton was revered as the greatest scientist of all time, not only on account of his specific discoveries in mechanics, but for having established the true method of scientific investigation.

Giambattista Vico and J. G. von Herder were the most prominent of the eighteenth-century writers who argued that the study of social phenomena must be methodologically different from the natural sciences (see above, Chapter 14 C), but their view did not carry the day. David Hume, despite having thrown a cloud of scepticism upon the methodology of science that has not been dissipated by the past two centuries of philosophical discourse, nevertheless embraced the unity of science thesis. Adam Smith, when destroying his unpublished papers shortly before his death, saved from the flames an early essay on the ‘History of Astronomy’ in which he argued that Newtonian celestial mechanics was the prototype of the method of investigation appropriate for all subjects. In the nineteenth century, leading philosophers (and social scientists) such as William Whewell, J. S. Mill, and W. S. Jevons held that all the sciences are united by a common philosophy of knowledge. Henry C. Carey, the most prominent American social scientist of the pre-Civil War period, contended in his Principles of Social Science (1858) that the study of social phenomena should parallel Newtonian mechanics, arguing (among other things) that Malthus’s theory of population was shown to be false by the law of the conservation of matter.

In 1878 Francis Galton proposed to the British Association that economics should be removed from its roster because it was not properly a ‘science’, but he was rebuffed on the grounds that economics was not inherently unscientific, just more difficult than the natural sciences. Economics, or ‘political economy’ as it was then called, was accorded scientific status, not so much because of its specific findings as on account of its method, which even at that stage resembled physics in its use of abstract modelling. Indeed, many of the strong mid-nineteenth-century opponents of political economy, such as the romantics, objected to it because it was scientific, applying Newtonian methods to phenomena which, in their view, belonged to a fundamentally different ontological category.

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The view that the sciences, or at least those disciplines that are truly sciences, are united by the adoption of a common philosophy of knowledge and the use of similar methods of investigation was a central principle of the Vienna Circle positivists, a notion that Otto Neurath, the social scientist of the group, promoted with uncompromising vigour. Carl Hempel, in his influential revision of early positivism, firmly sustained the unity of science thesis. This was the philosophical foundation of his attack on narrative historians that initiated the debate on the methodology of history which we examined in Chapter 14. That the social sciences deal with unique, non-recurring, phenomena (as historians claim) or with statistical or populational phenomena which must be treated in probabilistic terms (as other social scientists contend) does not, according to Hempel, indicate any epistemic difference between the natural and social sciences. The positivist theory of science, and the
revisions of it such as Hempel’s and Popper’s, have been abandoned, but the unity of science thesis still has considerable support among social scientists and philosophers. ‘The possibility of a social science in principle as perfect as physics,’ says Brodbeck, ‘remains the unexamined premise of the vast majority of present-day social scientists’ (Readings in the Philosophy of the Social Sciences, 1968, p. 1).

None the less, serious objections to the unity of science thesis have been expressed by a variety of writers. In our examination above of the ideas of Max Weber (Chapter 15 C) we encountered a theme that continues to punctuate the literature on the philosophy of social science. Weber argued that the study of social phenomena must be pursued in a fundamentally different way from the natural sciences. Social phenomena result from the rational, evaluative, and purposeful actions of individuals. The natural scientist cannot go beyond the construction of a body of knowledge based upon external observation; but the social scientist, who shares the property of consciousness with those entities whose actions make social phenomena, can, and must, present a more intimate, empathetic understanding of these phenomena. Weber’s concept of Verstehen has been variously interpreted by philosophers and social scientists but, in one way or another, it lies at the root of most of the claims that there are fundamental epistemic differences between the social and the natural sciences, as advanced, for example, by Frank Knight, Friedrich Hayek, Ludwig von Mises, and the modern ‘Austrian school’ in economics, and by Talcott Parsons and his followers in sociology. The word ‘scientism’ was coined by Hayek as a derogatory term for the view that social phenomena should be studied by the methods of the natural sciences (The Counter-revolution of Science: Studies in the Abuse of Reason, 1955). On the whole, philosophers have been hostile to the Weberian thesis, but Karl Popper seems to go a considerable distance towards it in contending that social scientists should pursue ‘situational analysis’, investigating the decisions of human agents in the situations in which they find themselves (see Douglas W. Hands, ‘Karl Popper and Economic Methodology: a New Look’, Economics and Philosophy, 1985).

Weber did not argue that the study of social phenomena is not scientific; on the contrary, he contended that the method of Verstehen is essential to making it so. But others have taken the view that the nature of the subject matter of the social disciplines, and the relationship between the investigator and his subject matter, are so fundamentally different from those of the natural sciences that the term ‘social science’ is an oxymoron, combining two notions that are inherently contradictory. Weber’s rejection of the unity of science doctrine is regarded by them as insufficient; the study of social phenomena is not, cannot be, and ought not to be regarded as in any way akin to the study of natural phenomena. In a widely discussed book (The Idea of a Social Science, and its Relation to Philosophy, 1958), Peter Winch argues that the study of social phenomena must be ‘philosophical’ rather than ‘scientific’, by which he means that the proper way to comprehend such phenomena is by conceptual analysis rather than by means of empirical research. The notion of ‘cause’, says Winch, does not belong to the domain of social phenomena; what is needed instead is a penetrating analysis of the concept of man as a ‘rule-following’ being. According to Winch, the relations between the individual members of a society are, at bottom,
the relations between ideas. Economists, sociologists, and
others should abandon their attempts to discover the causal determinants of social
phenomena and try to make society ‘philosophically intelligible’, or else give way to
philosophers, who are trained in conceptual analysis and understand the social (i.e.
‘rule-governed’) nature of language. A. R. Louch
(Explanation and
Human Action, 1966) agrees with Winch but
criticizes him for failing to see that, since social phenomena are the result of
deliberate individual actions, they are irredeemably moral in character. These moral
judgements are directly ascertainable and they should form the basic material of
social analysis. In effect, Louch tries to overturn two Humean doctrines - the non-
observability of causation, and the distinction between facts and values - and to
argue that because the social scientist, an inside observer of social events, can
escape Hume’s restrictions, his epistemic foundations are fundamentally different
from those of the natural scientist.

Winch and Louch do not supply their readers with concrete examples of how the
social sciences could be improved by following their prescriptions. Lacking such
demonstration, one is reasonably justified in taking the view that what they
propose is unlikely to give us a better understanding of our social world or enable us
to deal with its problems through social policy. Most philosophers continue to insist
that empirical phenomena, of whatever sort, must be investigated by procedures in
which empirical methods play a vital role. The views expressed by Winch and Louch
may perhaps receive a sympathetic response from some modern Marxist social
scientists, and some orthodox ones such as the disciples of Ludwig von Mises in
economics and those of Leo Strauss in political science but, generally speaking, social
scientists are now firmly committed to the view that the investigation of social
phenomena should strive to be objective and empirical, limited in this only by
technical feasibility.

Max Weber did not intend to drive his notion of Verstehen
in the direction taken by Winch and Louch. He insisted upon the firm separation
of facts and values, the need for scientific detachment and objectivity in social
investigation, and the indispensable role of empirical evidence. The philosophical
issue that is raised by Weberian Verstehen is the epistemic status of mental entities such as motives and preferences. Is it permissible
to construe such factors as causal variables in explaining social phenomena? This is
an important question which deserves specific attention. I defer discussion of it to
the next section.

The arguments put forward by defenders of the unity of science leave something to
be desired. A great deal of the literature on this proceeds as if the only natural
science were physics. But large areas of biology, geology, and other natural sciences
are very different from physics, more so perhaps than some areas of the social
sciences are. If one sets up physics as the standard there is no unity of science
within the domain of natural science itself. The early positivists adopted such a
standard but, as we have seen, philosophers of science have abandoned positivism,
and it has been succeeded by a large number of epistemological theories, none of
which has achieved general acceptance. Since there is no unity in epistemological
theory, how can it be claimed that there is epistemological unity in science?
However, if the reader is prepared to accept the argument advanced above that
‘cognitive instrumentalism’ is a more acceptable epistemology than the other
candidates, he will have no difficulty in embracing the unity of science thesis. This
epistemology sets up no specific science as representing the ideal, and makes limited
prescriptive demands that can, in principle, be satisfied by scientific investigation in
different areas. According to cognitive instrumentalism, theories and empirical data
function as complementary implements of investigation, and the only rules that must
be followed are that theories should be coherent and logically sound, and articulate
with observation data that are objectively obtained and properly processed. Except for those who reject empiricism, these are the rules that are, in fact, accepted as binding by social as well as natural scientists.

Nonetheless, there are important differences between the natural and social sciences. Epistemological unity does not mean homogeneity of substantive content, or homology of research procedures in all domains of scientific investigation. In understanding a scientific discipline it is necessary to comprehend not only what it shares with other disciplines but also wherein it is dissimilar. There are important differences among the social sciences themselves (as there are among the natural sciences), but to examine these in detail would extend the length of this book beyond reason, so I will confine the discussion to the more general differences between the natural and social sciences. This issue has been extensively discussed in the literature, some writers claiming that there are good grounds for regarding the natural and social sciences as categorically distinct, others saying that the differences are only matters of degree (see, for example, an excellent paper by Michael Scriven, ‘The Frontiers of Psychology: Psychoanalysis and Parapsychology’, in Robert G. Colodny, ed., *Frontiers of Science and Philosophy*, 1962). In the ‘Preliminary Remarks’ on this matter in Chapter 3 C above we noted that social phenomena are not as uniform, or as constant overtime, as natural phenomena are; that social scientists cannot carry out the kinds of experiments that natural scientists can, and test their ‘hunches’ in that way; that value judgements are more involved in the social than the natural sciences; that social phenomena reflect the operation of psychic entities while natural phenomena (or, at least, non-organic phenomena) do not; and that social scientists are less able to isolate particular causal factors from their general context than natural scientists. Further discussion of these and related matters will occupy our attention in the remainder of this chapter. They will not be discussed in terms of comparison between the natural and social sciences, for these issues must be addressed by the philosophy of social science quite apart from that comparison. But before we leave the subject of this section, we should consider a difference between natural and social phenomena which is an ontological difference, a categorical distinction of kind, not merely one of degree. Here and there in the preceding chapters we have had occasion to note the importance of social organization and the distinctiveness of the modes and mechanisms of organization in human societies. Now I want to reiterate this and emphasize its philosophical significance.

The Vienna Circle philosophers declared metaphysical propositions to be meaningless, but this only served to disguise from themselves and their followers that the positivist philosophy of science was based on the ontological postulate that all real existence consists only of material objects. They were on the right track in insisting on the epistemic unity of science, but not in presuming that it derives from the ontological homogeneity of the world. In order to sustain the unity of science thesis, one must show that the same basic principles of epistemology apply to the investigation of very different kinds of phenomena.

If one rejects the notion that there are non-material transcendental entities of the sort that idealist philosophers and theologians talk about, the positivist doctrine of metaphysical monism appears to be inescapable. All things, including organisms and their brains, are composed of molecules, and these in turn are composed of more elementary physical particles; all events, including social events, involve the operation of the primary physical forces. The metaphysical monism of the modern positivists reflects a theme that has a long history, going back at least as far as the pre-Socratic philosophers of ancient Greece. Parmenides, the leader of the Eleatic school, contended that all reality is composed of one substance. Phenomenal
diversity belongs only to the appearances of things; their true reality is homogeneous: the Many are, at bottom, One. But this assertion failed to satisfy other philosophers, who advanced the notion of a plurality of primary substances. This line of thought developed, in Greek philosophy, into the view that all reality is composed of four things: earth, water, air, and fire - a notion that was embraced by Aristotle and remained prominent in Western philosophy until the rise of empirical science.

One may tackle this issue by invoking the notion of emergent properties (see above, Chapter 3 B). According to this view, the existential diversity we observe empirically does not (necessarily) reflect differences in the fundamental constituents of things, but properties that become manifest at different levels of organization. With a new level of organization, new properties appear, which may be taken to reflect the coming into existence of new real things. Water, for example, is a different thing from the oxygen and hydrogen which compose it and we may speak of it without impropriety as having a real existence of its own. So also we may regard mountains, solar systems, organic cells, and termite colonies as real things, having properties that distinguish them from other things. Scientific investigation would get nowhere if it were to cling firmly to the notion that all things are made up of whirling particles and insist that all explanations be in terms of them. For every science, the 'laws' that it postulates are propositions that pertain to a particular level of organization, and causal explanation is, primarily, elucidation of how a particular type of organization works. Newtonian celestial mechanics, for example, describes the planetary motions in terms of the organization of the solar system, and explains that organization in terms of the operation of gravitational attraction, which is taken to be the dominant force at that level. Every science focuses upon a specific level of organization and the phenomena it investigates are the properties that pertain to at that level.

Existential phenomena are exceedingly diverse, reflecting the many different levels of organization that have evolved since the primordial beginning of the universe. But, in addition to the emergence of novel properties, on one planet at least among the billions that exist there have also evolved two novel forms of organization: organic systems and social systems have come into existence. Living organisms do not differ from non-living matter only in the emergence of new properties when the requisite chemicals are combined; a fundamentally different mode of organization is involved. And human societies do not merely have properties that come into existence when individuals live together in a co-operating fashion; their organization involves the operation of unique co-ordinating mechanisms. There are, one might say, three ontological categories of existence: the domain of matter and energy; the domain of life; and the social domain. Karl Popper attempted to make such a distinction in his proposition that there are 'three worlds' (see above, Chapter 15 B, Note 1), but he did not succeed in identifying the fundamental differences. He focused upon certain properties of life and society instead of considering their special modes of organization.

The difference between living and non-living phenomena has exercised the attention of philosophers since the dawn of philosophy in ancient Greece. A modern biologist would stress the capacity of some organisms to utilize the energy of sunlight to build up complex organic molecules from chemical elements, and the ability of others to do likewise by feeding on those that can perform photosynthesis. By this means organic structures are built, and energy is stored. This energy is released by the disassembly of organic molecules, and maybe utilized to power the processes of growth, movement, and reproduction, which phenomenally differentiate living from non-living things. The distinction between them does not appear to be categorical, however, since metabolism (the building up and breaking down of organic molecules) can be explained as chemical
processes that are not fundamentally different from those of the non-organic domain.

Dissatisfaction with what appeared to be a ‘mechanistic’ view led some late
nineteenth and early twentieth-century writers to postulate the existence of a special
entity that is uniquely present in living phenomena. Most prominent in taking this
stance were the French philosopher Henri Bergson (in a series of books, the most
popular being *Matter and
Memory*, 1896, and *Creative
Evolution*, 1907), and the German biologist-
philosopher Hans Driesch (*The History
and Theory of
Vitalism*, 1905). But Bergson, Driesch, and other writers
who followed this line of thought were unable to explicate the nature of the special
entity (Bergson called it the *élan vital*) that
differentiates the living from the non-living world in a fashion that would satisfy an
empiricist. The notion was embraced by some prominent literary figures of the
period (e.g. Samuel Butler and George Bernard Shaw), but scientists and
philosophers of science firmly rejected it. Understanding of the fundamental
difference between the organic and the inorganic world, in non-mystical terms, had
to await the development of modern genetics.

The DNA molecule, in chemical terms, is just a molecule. But its philosophical
significance is momentous. It has the capacity to encode information, which controls
the embryological development of an organism from a single cell into a complex
system of functionally specialized parts. After birth, the DNA-encoded information
controls the internal physiological processes of the organism and, for most species, it
totally controls the organism’s responses to the external environment. This
information is transmissible from one organism to another when reproduction takes
place and, at this point, changes in the information content of the DNA can occur,
making the progeny different from its parents. An organism is not merely a distinct
level of organization with emergent properties that are different from its chemical
constituents; it represents a different *mode* of organization, one in
which certain chemical constituents carry encoded information which creates and
governs the structure and function of an integrated and co-ordinated system.

To explain the organization of a mountain we need not resort to anything other than
the laws of matter and energy. To explain a living organism we have to recognize
the addition of a fundamentally different mode of organization that is mediated by
encoded information. This point has recently been strongly emphasized by Ernst
Mayr in some of the essays collected in his *Toward a
New Philosophy of
Biology* (1988; see especially chapter 1). Mayr is right in
rejecting ‘vitalistic’ explanations of organic phenomena and also in chiding
philosophers for their preoccupation with physics, but he also contends that biology
requires a fundamentally different epistemology from other natural sciences, and this
is

not sustainable. What is required is the delineation of an ontological difference
between living and non-living phenomena, based not only upon the emergence of
unique properties in the domain of life but, more fundamentally, on the emergence of
a unique mode of organization, one that is mediated by instructions encoded in a
form that permits their transmission from entities whose existence is limited to a
short time-span to their, similarly limited, successor entities. By this means,
*populations* of organisms may persist indefinitely
while the individuals who compose them have only a brief span of existence as
organized entities. One should stress, in this connection, that genetically encoded
instructions control organic *processes* as well as somatic
structure, and these processes are understandable as performing functions. In
describing an inorganic natural entity such as, say, the solar system, it would not be
meaningful to speak of a planet as performing a function that is necessary to the
dynamic equilibrium of the system, but the status of an organ such as the liver in a
living organism cannot be understood without reference to its functional role in
maintaining the organization of the organism.

A similar ontological distinction can be claimed for societies composed of organisms. Whether this applies to social systems other than those of the species *Homo sapiens* is problematic, and I will consider here only human societies. In addition to controls that are exercised by information encoded in the human genome, the behaviour of individuals is channelled in ways that permit the emergence, maintenance, and orderly development of social systems by instructions that are mediated by three other modes: established customs, traditions, and values; the exercise of coercive power by some individuals over others through hierarchically structured institutions; and the co-ordination of voluntary actions in the economic domain through the information carried by market prices. The oft-made assertion that ‘the whole is more than the sum of its parts’ does not refer only to the fact that the parts interact with one another and thus generate new properties. If that were so, the assertion would be little more than a cliché, or a way of stating what is obvious. The scientific analysis of wholes would require only that the ‘composition laws’ that govern the assemblage of parts into wholes be elucidated. But, in some cases, fundamentally new modes of organization, which are not obvious to casual observation or captured by composition laws, come into existence. These have special scientific and philosophical significance, which can be appreciated only by resorting, again, to the concept of function. The significance of customs, governments, and markets derives from the functions they perform in maintaining the organizational integrity of a social system. Because of the presence of such modes of organization, societies deserve to be recognized as distinctive ontological existents, significantly different from both material and organic entities. Like populations of organisms, societies persist beyond the life-span of their member individuals but, unlike populations, their persistence is not explicable solely in terms of genetic factors. Adam Smith initiated the scientific study of social systems as ontologically distinct phenomena by pointing out that the division of labour which increases productivity can be practised only if, and to the degree that, there are trading markets in existence. Herbert Spencer’s distinction between the ‘militant’ and ‘industrial’ types of societies focused upon the difference between co-ordinating mechanisms that operate through the exercise of coercive power and those that utilize market exchange. Emile Durkheim regarded the conscience collective as a co-ordinating mechanism that controls behaviour through enculturation and by placing pressure upon individuals to conform to established norms. However, the philosophical significance of such modes of organization was not recognized by these writers or, so far as I am aware, by subsequent ones. The doctrine of the unity of science is still widely held by philosophers and social scientists. I emphasize again, though, that recognition of the ontological distinctiveness of social systems does not mean that there is an epistemic difference between the social and natural sciences. Scientific explanation, in all domains, must utilize coherent and logically valid theories that model empirical phenomena, and treat relevant empirical evidence carefully and objectively. The social, biological, and physical sciences differ from one another mainly because they address phenomena that emerge in differently organized systems. (This point is amplified in my *Why does Homo sapiens Differ?*, *Journal of Social and Biological Structures*, 1988, and my lecture *How many Kinds of Things*...
2. Mentation, individualism, and holism

Max Weber contended that the methodology of the social sciences is fundamentally different from that of the natural sciences, and necessarily so, because the explanation of social phenomena requires an analysis of what individual persons do, not in terms of their physiological processes or their passive responses to changes in ambient conditions, but as active agents with the rational capacity to choose the means of achieving their objectives. It is easy to see why Weber thought that the social and natural sciences differ. Physical entities such as atoms and planets cannot be regarded as active agents in this sense; and, while non-human animals can be, the biologist is too far removed from other species to achieve an empathetic understanding of their behaviour. Weber was wrong in regarding these factors as calling for a distinctive epistemology of social science, and he failed to perceive that the existence of different modes of organization is what most significantly differentiates physical, biological, and social systems. But his insistence that the social scientist should regard social phenomena as resulting from the rational acts of individuals is less easy to dismiss. In Weber’s day, one of the social sciences, economics, was already dominated by theoretical models in which the central role was played by rational individuals. During his lifetime, this type of theory was entrenched still further by the development of neoclassical economics.

More recently, ‘methodological individualism’, as this has come to be called, has been extended beyond the specialized domain of economics to political science, sociology, and law. These developments have been accompanied by intense debate, among philosophers and social scientists, concerning the epistemic foundations, and the ethical and political implications, of this conception of social phenomena, with ‘methodological holists’ contending that it seriously misdirects social research and social philosophy. I cannot review this debate in detail here (see section 4 of May Brodbeck, ed., Readings in the Philosophy of the Social Sciences, 1968, for a small, but good, selection of representative papers), but the main issues involved warrant more discussion than has been devoted to them thus far. As a first step we must consider the epistemic status of the mental entities that occupy a central place in the individualist methodology.

A wide variety of terms is available for reference to mental phenomena. We speak of a person as having ‘desires’, ‘preferences’, ‘purposes’, or ‘intentions’, and as being ‘lonely’, ‘irrational’, ‘happy’, or ‘apprehensive’, to name only a few of the words that the English language supplies. For our purposes we may collapse this dictionary of mentation into three terms; ‘motives’, ‘tastes’, and ‘beliefs’. The first of these refers to that which is valued by the actor, the end or ends he hopes to attain by his actions. Two individuals may have the same general motives but differ considerably in their ‘tastes’ for specifics. One may rank brandy higher than beer and ballet higher than basketball, while the other may have the opposite schedule of preferences. Under the heading of ‘beliefs’ we include the individual’s views concerning the probability that an action will, in fact, serve to realize his motives; that is to say, the knowledge he considers himself to possess with respect to the relevant relations of cause and effect. The individual may also have moral beliefs, and these may affect his motives, but I will not take these into account in the immediate discussion here. A person may consider more than one thing to be worthy and there may be more than one way of attaining any particular end, so a fourth mental concept is useful: ‘choice’. Economic analysis tries to cut through the complexity of multiple ends and multiple means of attaining them by postulating that the individual seeks to maximize his ‘utility’. Thus economic theory has been described as ‘the logic of
rational choice', an explication of the choices that a rational utility-maximizing person
would make in a given situation. This way of looking at human behaviour, which has
characterized economics from its earliest beginnings as a systematic discipline, has
been subject to unremitting attack by other social scientists, psychologists, and
occasionally by economists themselves when engaged in methodological meditation,
but this will not concern us here. The immediate problem before us is not the
particular ways in which mental entities are used in explaining social phenomenon
but the justification for using such entities at all.

What is the source of our knowledge about mental entities? They are not observable
in the way that chairs, rivers, and other things are. They are objects

of perception, but the type of perception involved is what psychologists call
‘propiroception’, the perception that one has of one’s own physical body and other
aspects of oneself. Knowledge of one’s own mental state is a
form of proprioception usually called ‘introspection’. The argument has been made
that this knowledge is ‘subjective’ and, therefore, cannot be used in a scientific
analysis of social phenomena. One may perhaps explain the action of a particular
individual in terms of his own motives and beliefs, but such mental entities cannot be
used to explain things like crime rates and inflation. The central issue here is
whether information about mental states derived by introspection can be generalized.
In explaining the foundations of government Thomas Hobbes argued that
everyone desires security and believes that it can be obtained
by the centralization of coercive power. Max Weber’s concept of ‘ideal types’ rests
upon the view that, while it is not true that all people at all times have the same
motives and beliefs, the social scientist can employ the conception of an idealized
person for analytical purposes just as the physicist uses the concept of an idealized
mass.

The use of such idealizations as heuristic generalizations about human agents has
played a large role in the development of social theory, but not without considerable
concern over its methodological validity. In economics, the development, in the
1870’s, of the concept of ‘marginal utility’ to explain the market values of
commodities initiated a debate that has persisted down to the present. The notion
of ‘utility’, it has been argued, is irremediably subjective. One cannot compare the
utility of one person with that of another, and one cannot apply the concept of utility
to groups of individuals, for this would be equivalent to ascribing properties of mind
to populational categories such as classes, clubs, communities, or nations. This
argument has been accepted by most orthodox economists. Many textbooks in
elementary microeconomics and virtually all in more advanced ‘welfare economics’
tell the student that it is impermissible to make interpersonal comparisons of utility.
Nevertheless, economic theory continues to make heavy use of ideal type analysis in
which the postulated agent is represented as a utility-maximizing individual; the
recent extensions of economic theory to the analysis of political and legal phenomena
proceeds as if interpersonal comparisons of utility are permissible; and much applied
economics assumes that such comparisons can be made, and even estimated
quantitatively. In order to make the concept of utility instrumentally effective in
social analysis some relaxation of the ban on generalizing it is necessary. Pragmatics
overwhelms scholastics in social science as in other areas of scientific inquiry.

This does not mean, however, that the door is open to whatever mental entities one
may care to postulate a priori, or to the varied and
complex mental states that psychologists and psychiatrists deal with. In order to
render social phenomena intelligible, social theory must restrict itself to mental
entities that are very simple, and understandable in commonsense terms. The notion
that human agents seek to maximize their utility and pursue actions they believe to
be effective in promoting this end is such a simple and commonsense notion. So is, it seems to me, the ‘law of diminishing marginal utility’, which states that the satisfaction one derives from consuming additional units of a commodity declines as one’s rate of consumption of it increases. Mentational concepts such as, for example, those of Freudian psychology, may be more sophisticated, but they are unlikely to be serviceable in explaining market prices, or other social phenomena, in terms of mental entities. A Weberian ‘ideal type’ of human agent is one whose mentation is construed as consisting of simple motives, tastes, and beliefs that are immediately understandable by other humans. Economics and the other social sciences have demonstrated that reference to mental entities, if severely constrained, can be effectively used in constructing cognitively instrumental theories. Weber, and many of his followers, claimed that social phenomena cannot be analysed without the use of mentational concepts. This is too strong, since it makes the use of mentational concepts obligatory in all branches of social science.

Showing that such concepts are required in tackling some problems does not demonstrate that they are required in all. But Weber’s contention was quite unnecessary. From the standpoint of epistemology, the issue is not whether reference to mental states is obligatory but whether it is permissible in scientific explanation.

Some philosophers of science (e.g. Ernest Nagel, Karl Popper, A. F. MacKay, Daniel Hausman) accept references to mental states in scientific explanation; others (e.g. May Brodbeck) regard such references as dubious; while still others (e.g. Gustav Bergmann, Alexander Rosenberg in some of his more recent writings) firmly reject them. The central point at issue is whether social phenomena may be explained by construing motives and beliefs as their causes. The early positivists, as we have seen, attempted to eject the concept of causality from the domain of scientific inquiry. They were unsuccessful, but the restoration of causality leaves open the issue of what sorts of things may properly be accorded causal status in a scientific explanation. Resolution of this question would seem to rest upon the solution of a prior problem: the nature of causality. But philosophers have come to no agreement about this as a general issue in epistemology, so firm statements that motives and beliefs may be construed as causes, or that they may not, would seem to be at least premature. But we may have to wait a long time for philosophers to reach a satisfactory definition of the nature of causality. Meanwhile, science proceeds. If one is prepared to adopt cognitive instrumentalism as an epistemological theory, the question can be reformulated: does reference to mental states enable one to render an observed phenomenon more intelligible than it would be without it? Some primitive peoples resort to mentational concepts in explaining everything; ‘animism’ is the notion that all existence is characterized by the operation of entities of the sort that one knows by introspection. In more advanced communities the use of concepts like the Hegelian Geist or ‘divine will’ is not dissimilar, but the application of such concepts to physical phenomena has been generally rejected by philosophers.

The question is, are simple, commonsense, human mental states instrumentally useful in explaining human social phenomena?

Let us take a simple illustration. John Smith enters a shop carrying a loaded revolver, has a verbal exchange with Henry Jones, a clerk, whom he shoots and kills, and then departs with the contents of the cash register. One could expand this account by furnishing more details, such as the type of revolver, what organs of Jones’s body were injured, etc., but we cannot make the events more intelligible as a social phenomenon without referring to the motives and beliefs of Smith and Jones. The INUS model of causation has not been generally accepted by philosophers, but one of its merits is that it enables one to clarify the causal role of mental entities in such situations. According to this model (see above, Chapter 3 A 3) the requirements of necessity and sufficiency can be met by only defining a set of causal
factors, since no single factor by itself is sufficient, and sometimes is not necessary, to cause an event. Applying it to the above illustration, it is evident that the motives and beliefs of both Smith and Jones were both necessary to cause Jones’s death, since no sufficient set can be complete without them. If Jones had not chosen to resist Smith’s demand, he would not have been shot, so he was a partial agent in his own demise. On the other hand, if Smith had intended to leave no witnesses, Jones would have been shot even if he had not resisted, so reference to his mental state is not necessary to the completion of a causal set.

Let us alter the story when Jones refuses his demand, Smith does not shoot and flees empty-handed. In this case no murder, or robbery, takes place. But this does not mean that an explanation is not required. A satisfactory explanation can be reached by noting that the mental states of Smith and Jones were, in this case, incapable of completing any causal set sufficient to produce a murder or a robbery. For the strict behaviourist, who eschews reference to mental states, non-events are difficult to handle without resort to linguistic rephrasing that construes them as events. But in social phenomena, non-action can be as significant as action. For example, if the President of the United States refrains from signing a statute passed by Congress before the deadline prescribed bylaw, it is described as having been ‘pocket vetoed’ by him. But in fact we do not observe the President as doing anything with respect to the statute, and this is what requires explanation. It is difficult to see how such cases can be handled without reference to mental states. If Rosenberg were right in claiming that mentational concepts cannot be used in scientific explanation because they do not represent entities that are ‘natural kinds’, then it would be equally improper to use such concepts in considering everyday phenomena. But we do persistently use them in vernacular speech to make sense of our normal experiences. It is Rosenberg, not common folk, who is in epistemic error (see A. F. MacKay, ‘The Incredibility of Rejecting Belief-Desire-Action Explanations’, Philosophy of Science Association, Proceedings, 1982, Vol. II).

The physical sciences have no warrant for referring to motives, tastes, and beliefs, because such things do not operate within the phenomenal domain of their concern. In communicating with one another, however, scientists are engaged in a social activity. Alfred Schutz, a strong advocate of Weberian epistemology, points out that when one scientist considers the work of another he must know not only what the other did, but what his purpose was in doing it (‘Concept and Theory Formation in the Social Sciences’, Journal of Philosophy, 1954). Scientific papers are written as reports of observable events, using the passive voice, but if the reader wishes to understand what is going on he must reconstrue the text in terms of motives and beliefs. The text may say something like ‘the sample was centrifuged at 30,000 G and the supernatant fluid decanted’ but the reader must know what the scientist was hoping to achieve by this in order to make scientific sense of it. Despite his strong insistence that there is no place for mental entities in science, Alexander Rosenberg nevertheless refers to them without restraint in evaluating the work of other philosophers. Is philosophical discourse exempt from the rules it prescribes for other social phenomena? If one does not, and cannot, practise what one preaches, the normative prescription becomes dubious; as Hume succinctly put it, ‘ought’ implies ‘can’. The communication of ideas cannot be conducted without reference to mental entities.

This view has recently been aggressively attacked by a school of literary criticism called ‘deconstructionism’. The adherents of this view claim that the proper way to read a text is to take it as it is, without reference to the author’s thoughts, just as if it had been written by an inanimate being. Deconstructionists are manifestly unable to follow this rule when quarrelling among themselves, or with literary critics of other schools. The contention that no reference to mental entities should be made by the
reader of a text seems to be clearly untenable (even if it were possible) when the matter in hand is as personal and subjective as are poems, novels, and other forms of art, but it is equally untenable with respect to scientific texts.

The above illustrations show, conclusively it seems to me, that reference to introspectively known mental states is serviceable in rendering social phenomena intelligible. That is to say, the epistemological theory of cognitive instrumentalism permits the use of mental entities. But other epistemologies may not be able to accommodate them, or to do so easily. We should note especially Carl Hempel’s ‘covering law’ model of scientific explanation in this regard. Many philosophers who examined the scientific status of mental entities in the 1960s did so in terms of Hempel’s model (see Paul M. Churchland, ‘The Logical Character of Action - Explanations’, Philosophical Review, 1970). As we saw in discussing the debate over historical explanation in Chapter 14 A, Hempel argued that the use of mental entities is permissible if, and only if, they can be formulated as general laws, and chided historians for failing to do this. According to Hempel, when an historian explains, for example, why Henry IV of France rejected his Protestant upbringing and embraced Catholicism in 1593, he is obligated to state the operative covering law, such as ‘whenever anyone is faced with a choice between his religion and a crown, he will choose the crown’. Naturally enough, historians regard Hempel’s demand as placing an obligation upon them that they cannot meet, and any attempt to do so would only expose themselves to justified ridicule. A strict Hempelian would say that this simply shows that the work of historians is not ‘scientific’, and he might go on to extend this judgement to a wide range of social science, since it is not possible to state many empirically true general covering laws of human behaviour in terms of mental states. The difficulty, however, lies more with Hempel’s epistemology than with the practices of historians and other social scientists. Covering laws are explanatory, but they are not the only form of scientific explanation. In fact, physics and economics are the only sciences that persistently employ such laws. Economic propositions such as the ‘law of demand’ show that mental entities can sometimes be embodied in general law statements but this does not demonstrate that this is the only way in which they may be used in scientific explanation.

Social scientists who accept the causal status of motives, tastes, and beliefs frequently treat explanations in terms of them as complete. This can be defended in terms of the division of labour among scientists. The economist, for example, might say that he has reached the boundaries of his disciplinary domain of investigation when he has traced phenomena to the utility-maximizing motives of the actors. As Joseph Schumpeter put it, ‘the task of the economist is finished when his vessel grounds upon a non-economic bottom’. Further investigation may be undertaken by psychologists, or sociologists, or biologists, but so far as economics is concerned the explanation is complete. A stronger view is that the mental entities that the economist uses are not scientifically explicable, by economics or by any other discipline. Neither of these positions is easy to accept. The first appears to construe the disciplinary boundaries as if they were properties of the phenomenal world rather than as conventions, which they clearly are. The second appears to regard motives, tastes, and beliefs as belonging to an ontological domain that is categorically distinct from all other phenomena.

If we reject the notion that mental entities are ontologically distinct it does not follow that social scientists are obligated to furnish complete explanations of social phenomena. The contention that complete explanations are required is implicit in the argument of some philosophers and social scientists that, while motives, tastes, and beliefs may be construed as causes, they are only links in a causal chain, being themselves the effects of other causes, which must be elucidated. This seems to be
the root of the view adopted by sociobiologists that social phenomena should be analysed in terms of genetic factors; mental entities may be referred to, but only in the course of passage to the genes. But why stop there? Scientific investigation is not truly grounded on bottom until it has reached quantum mechanics or the Big Bang. The demand for such a radical reductionism is clearly not helpful in advancing the enterprise of science. The boundaries between the disciplines may be conventional, but they are also useful to the furtherance of scientific inquiry. The contention that the present boundaries should be redrawn can be certified only by concrete demonstration of the improvements in scientific inquiry that would ensue. That mental entities are unobservable is no argument for insistence that we go beyond them. There are lots of unobservable entities in science, including ‘genes’.

Emile Durkheim took a different tack on this issue. He opposed the notion that social phenomena should be explained by reference to mental states, but he also rejected the view that human behaviour is explained by reduction to biological factors ‘Social life,’ he said, ‘should be explained, not by the notions of those who participate in it, but by more profound causes which are unperceived by consciousness’ (quoted by Antony Flew, Thinking about Social Thinking, 1985, p. 46). These ‘more profound causes’, in Durkheim’s view, are the social factors that mould and govern the ideas of the individual members of a society. I defer discussion of this for a moment, until we come to consider the deficiencies of methodological individualism that some regard as calling for a holistic approach to social phenomena.

The term ‘methodological individualism’ was coined originally by Joseph Schumpeter to refer to what he viewed as the dominant methodological precept of orthodox economics. It has come since to be used more generally for the doctrine that social phenomena must be explained in terms of the behaviour of individuals. This contention is not based merely upon the view that societies are composed of individual persons; similarly banal composition propositions can be made about everything in existence. The proponents of methodological individualism favour reduction in scientific analysis but insist that this be carried to the appropriate level and no further. The appropriate level is construed to be the human individual, because it is at this level that mentation occurs and choices are made among alternative behaviour possibilities. As Herbert Spencer argued, the notion of consciousness can be applied to individual organisms but not to social groups as such. If social phenomena are to be explained in terms of mental states, it follows that the appropriate unit of analysis is the individual. There is a close connection between the debate over the causal status of mental entities and the debate over methodological individualism. But, as we shall see, it does not follow that anyone who grants causal status to mental entities is thereby committed to a rigorous methodological individualism in social science, and certainly not to the extreme form of it that some have advocated.

Methodological individualism has been strongly advocated by some sociologists and political scientists, but most notably by economists, and in the interest of brevity I shall confine the discussion here to the debate in that venue (see R. P. Dore, ‘Function and Cause’, in Alan Ryan, ed., The Philosophy of Social Explanation, 1973, for an excellent defence of methodological individualism in sociology). The standard treatment of microeconomic phenomena in the current economic literature is individualist. In explaining the determinants of market prices, for example, the market demand functions for
particular goods and services are derived by simple aggregation of the demand functions of the individual consumers. On the other side of the market, the supply functions of the several producing firms is similarly aggregated. The producing ‘firm’ is treated as if it were an individual person, neglecting the complexities of large corporate organization (or deferring them to subsidiary examination). Consumers and firms are construed to be the appropriate units of study because it is at these levels that ‘decisions’ or ‘choices’ are made on the basis of motives, tastes, and beliefs. Economists are, generally speaking, strongly committed to such an approach to the explanation of the phenomena in their domain and feel it necessary to construct models that rigorously adhere to the canons of methodological individualism. Macroeconomic phenomena such as unemployment and inflation have not been satisfactorily modelled in this way, and many economists have expressed the view that macroeconomic theory remains insecure until it has been furnished with ‘microeconomic foundations’.

The central thesis of methodological individualism was stated by John Stuart Mill in his System of Logic. Speaking ‘Of the Chemical, or Experimental, Method in the Social Sciences’, Mill declared:

The laws of the phenomena of society are, and can be, nothing but the laws of the actions and passions of human beings united together in the social state. Men, however, in a state of society, are still men; their actions and passions are obedient to the laws of individual human nature. Men are not, when brought together, converted into another kind of substance, with different properties; as hydrogen and oxygen are different from water... Human beings in society have no properties but those which are derived from, and may be resolved into, the laws of the nature of individual man. (Book VI, chapter VII)

The most influential formulation of this thesis in modern economics was contained in Lionel Robbins’s Essay on the Nature and Significance of Economic Science (1932), which, frequently reprinted, became almost a manifesto of orthodox economic methodology. Economics, said Robbins, develops its theorems by rigorous deduction from premises that state propositions concerning human nature. These premises are so simple and plain that, though they are derived by personal introspection, their universal truth is undeniable. Modus ponens logic therefore guarantees that the conclusions deduced must also be true. One cannot use introspection, or any other means, to obtain similarly ‘self-evident’ truths about social groups, so economic theory must analyse social phenomena in terms of the rational actions of individuals. Robbins did not, I think, intend to claim that the construction of such deductive models is all that economists are called upon to do. He was not, at least in principle, averse to empirical work in economics, though some critics (especially T. W. Hutchison) criticized him severely on this ground. Robbins never clarified his position on this matter satisfactorily, but he was, it seems, talking not about the methodology of ‘economic science’ but about that part of it which is economic theory.

None the less, so far as our present subject is concerned, he adopted a clear individualist stance, and it was largely this that was responsible for the widespread favourable reception of his book even in an era when empirical work in economics was rapidly developing.

Robbins and the ‘Austrian school’ of economists have often been treated as having similar methodological views, but this is incorrect. Ludwig von Mises and his followers argue that deducing conclusions from propositions about mental states is the whole of economics. Moreover, they regard such propositions as a priori truths; they are not furnished by an empirical procedure such as introspection. Robbins spoke of the mentational postulates of economic theory as ‘indisputable facts of experience’. The Austrians regard ‘experience’ as having nothing to do with economics. They reject empiricism altogether and contend
that economics, when properly conducted, differs from the natural sciences in being purely deductive. Economics is methodologically akin to Euclidian geometry and, like it, furnishes a body of apodictic truth. A small school that sometimes refers to itself as practising 'subjectivist economics' has developed under the inspiration of this methodological thesis. The main effect so far has been to bring into disrepute the notion that mental entities can be accorded causal status in a scientific explanation. But any idea can be made nonsensical by exaggeration, as the medieval scholastics, the philosophical ancestors of the Austrian school, amply showed. There is a great deal of difference between claiming that mental entities may be employed as causal factors in a scientific analysis of social phenomena; claiming, as Weber did, that they are necessary in scientific social inquiry; and contending that they, and modus ponens logic, are sufficient to enable one to discover the indubitable way of the social world.

If the only criticisms that could be mounted against methodological individualism were aimed at its extreme forms, the precept would remain undamaged. But there are others that must be more seriously considered. The most important of these is Emile Durkheim's. David Braybrooke notes that the fundamental position adopted by methodological individualism is that 'the only ultimately satisfactory strategy of explanation in the social sciences is one that moves from person facts to explain group facts and not the other way round' (Philosophy of Social Science, 1987, p. 33).

Durkheim's contention was that the 'other way round' is the most significant feature of human sociality.

Individual minds, forming groups by mingling and fusing, give birth to a being, psychological if you will, but constituting a psychic individuality of a new sort. It is, then, in the nature of this collective individuality, not in that of the associated units, that we must seek the immediate and determining causes of the facts appearing therein. The group thinks, feels, and acts quite differently from the way in which its members would were they isolated. If, then, we begin with the individual, we shall be able to understand nothing of what takes place in the group. (The Rules of Sociological Method, 1938, pp. 103-4)

This passage stands in sharp contrast to the one quoted above from J. S. Mill. In its wording Durkheim's position here is indefensible, since it seems to say that social groups possess the property of mind. Whether Durkheim did or did not embrace this highly suspect notion is debatable, but he did emphasize the methodological implications of something that cannot be empirically denied, or disregarded, in social inquiry: that the social group(s) to which an individual belongs play a strong role in determining his motives, tastes, and beliefs. This point has been highlighted repeatedly in this book, starting with the first chapter, where we noted that humans are exceptionally altricial animals who undergo a long period of enculturation. The biologist can disregard the properties of humans that are derived from enculturation, but the social scientist cannot, since the object of his inquiry is social phenomena. The causal connections between the mental states of the individual and social phenomena are reciprocal. The individualist and the holist argue for unidirectionality of causation, but in opposite directions. When moderately expressed as empirical statements, both positions are right; when expressed as hard methodological principles, both are wrong.

The methodological individualist construes the action of an individual as explicable in terms of mental states, but it is evident that reference to these is frequently meaningless without at least implicit reference also to the relevant social context of the act. If we observe that a person writes his name on a piece of paper 'in order to obtain currency from a bank', a social context is implied. But different social contexts
are relevant if the same act of writing is intended to identify the agent as confessing to a crime, or as the official authorized to conclude a treaty, or is an exercise in calligraphy by a student in a course on drafting. Within each of such social contexts we may generalize about what individuals do when they write their names, but any generalization about an act of ‘signing’ as such would be meaningless. Similarly, words such as ‘buy’ and ‘sell’ do not simply signify that money and goods change hands in opposite directions. Pieces of paper and metal that play a role in such exchanges in Sri Lanka may not do so in Hungary, or in the Amazon jungle. Most of the terms used in social science are replete with reference to a social context, and many of them are meaningful only within a particular context. Even leaving aside the fact that the mental states of individuals are the products of enculturation, the actions of individuals cannot be divorced from the relevant social institutions and cultural ambience. In speaking of the necessity for a social scientist to understand the ‘meaning’ of an act, Weber was referring to such factors.

The multisociality that characterizes many human societies makes recognition of the context of an action even more imperative. The individual may be described as seeking to maximize his utility in all his actions, but his specific acts may have different meanings in relation to his membership of a church, a professional association, a political party, or a tennis club, which cannot be disregarded in the analysis of social phenomena. If we focus upon the most fundamental problem in social science, the operation of the modes and mechanisms of social organization, we encounter again the fact that these function within particular institutional and cultural contexts. Even the market mode, in which the voluntary self-interested acts of individuals play the primary role, functions within a context of legal, business, and other social institutions, and is constrained by cultural norms. Human individuals are ontological entities, but this does not mean, as methodological individualists contend, that the scientific explanation of social phenomena must run exclusively in terms of mental states. Social wholes are ontological entities as well.

Among the many institutions that compose the general social ambience within which individuals are raised to maturity and live out their adult lives, those that generate and disseminate scientific knowledge are especially important. The social nature of knowledge poses no special problems for the natural scientist because the phenomena with which he deals are not affected by it. The orbits of the planets did not change as a consequence of the publication of Newton’s *Principia*. But new knowledge about social phenomena may affect the phenomena, by influencing human action. Even if such knowledge fails to alter anyone’s motives or tastes, it may well alter the beliefs that people hold concerning the actions that are most likely to attain the desired ends. A person who knows economic theory may act differently from one who does not. Some economists who adhere strongly to methodological individualism have recently postulated that everyone acts as if he were in command of the best economic theories currently available, everyone, in this sense, is said to be ‘rational’ in the expectations he holds concerning the effects of his own actions and those of others, including the actions that may be taken by governmental authorities to affect events. The scientific value of ‘rational expectations theory’ in economics is, to my mind, doubtful, but this is not the issue that concerns us here. It points to a special problem that arises in domains where mental states operate as factors in the causal chain of events.

Philosophers have called this the problem of ‘reflexive predictions’. In an excellent paper (‘Reflexive Predictions’, *Philosophy of Science*, 1963) Roger C Buck argues that though it is in principle a serious problem for the social sciences it is of no great concern in practice because it requires wide dissemination of knowledge. If a prediction is secretly
made, using a theory (which Buck takes to be the typical case, contrary to the
assumption of 'rational expectations theory'), the phenomena will not be altered by
the prediction. This is, I think, sometimes, but not universally, correct. If the few
who are privy to the knowledge have substantial power, their actions may be
sufficient to influence events. For example, if a central bank makes a prediction
about the future state of the economy, and acts upon it, it may affect events even if
the officers of the bank are the only ones who know about the prediction and the
theory on which it is based.

A notable case of reflexivity is that of Girolamo Cardano, a sixteenth-century Italian
physician and mathematician who was one of the founders of modern probability
theory. He was also a strong believer in astrology and became

famous for his medical horoscopes, casting one for Edward VI of England which
accurately predicted that he had not long to live. Cardano is said to have constructed
a horoscope for himself from which he predicted that he would die on a certain day
and, when that day arrived, he committed suicide. Does his death serve as
confirmatory evidence of his astrological theory? One would think not. But there
are other cases that are more problematic. The announcement that a bank is
insolvent and about to close its doors may cause a ‘run on the bank’ which forces it
to close. Should we regard the announcement as true? If the economic advisers of
a government predict an impending depression and the government, persuaded by
them, undertakes actions that succeed in preventing it, were the economists wrong?
Do the political events that took place in Russia in 1917, in which convinced Marxists
played a crucial role, certify that Marx’s theory of history is correct? It is difficult to
answer these questions with assurance. Apart from the immediate difficulties that
such cases pose for the social scientist, the problem of reflexivity throws additional
doubt on the view espoused by methodological individualism that the only way to
analyse social phenomena scientifically is in terms of the mental states of acting
individuals.

Reflexivity, or the ‘Oedipus effect’, as Karl Popper called it, is a unique problem in
human societies. It arises from the reciprocal connection between individual action
and social phenomena - in this case the crucial linkage is the social nature of
knowledge. The choice that an individual makes among alternative possible actions
is based upon his beliefs as well as his motives and tastes, and even when the latter
are stable, his beliefs concerning the relative efficacy of different courses of action
may change rapidly. Under certain conditions, which are not rare, the beliefs of a
large number of people may change at the same time and in the same way. In
recent years economists have paid a great deal of attention to the role of
‘expectations’ as mental causal factors in certain events. In macroeconomic
theory this has been especially prominent - for example, the important role of
‘inflationary expectations’ in the dynamics of inflation is now universally recognized,
and is not confined to rare events such as the German hyper-inflation of the 1920’s.
But dealing with public expectations within the epistemic rules of, as Braybrooke put
it, a ‘strategy of explanation... that moves from person facts to explain group facts
and not the other way around’ is obviously incapable of addressing such problems
effectively.

Many of the strong proponents of methodological individualism appear to embrace it,
at least in part, because of what they perceive to be the nefarious objectives of
holists. The main objects of attack are Marxists who regard Marxian theory as not
only scientifically true but as a potent political instrument that can be used to bring
its predictions to pass. In effect, reflexivity is construed, not as an epistemic
problem, but as a political opportunity. V. I. Lenin made this into Communist Party
doctrine by declaring that a cadre of professional revolutionaries, convinced of the
truth of Marx’s theory of
history, is necessary to bring about the predictions of that theory (What is to be Done? 1902). Not all holists can be accused of such a perversion of science, and it certainly is not an inherent property of a holistic methodology. The debate between individualism and holism as epistemic doctrines does not hinge upon which of them has been more abused and exaggerated by its adherents.

The main difficulties of methodological holism stem from the problems that are encountered in identifying the entities that are to serve as the 'wholes' in a social theory. In a complex society like that of the United States there is an almost limitless array of social entities to choose from. In Keynesian macroeconomics, the entities are defined in terms of types of 'effective demand'; for example, 'consumers' and 'investors' are treated as operative aggregates. In orthodox Marxism, the entities are 'classes' that are defined in terms of the ownership of the means of production. Political scientists talk about the 'black vote' or the 'anti-abortion lobby' or the 'farm bloc'. Friedrich Hayek and other strong individualists have argued that such notions are permissible if they are construed as theoretical concepts and not as real things; they cannot be real things because they are not 'natural' entities (see, for example, Hayek, The Counter-revolution of Science: Studies on the Abuse of Reason, 1955, Part One, chapter VI on 'The Collectivism of the Scientistic Approach').

This argument seems to me to be untenable. Social phenomena are not any less 'natural' than physical phenomena are, and the 'farm bloc' is at least as observable as is the electron. It is not 'naive realism', as Hayek calls it, to regard social entities as real. It is, indeed, 'naive positivism' to regard the concepts of any science as necessarily referring to observable physical entities that occupy a definite position in space-time.

Some methodological holists, however, have been led into highly questionable contentions by treating social entities as natural. If societies are natural entities, what kind of natural entities are they? We have argued above that they are distinct entities of their own, owing to the fact that their organization makes use of special modes and mechanisms. However, a long tradition in the philosophy of metaphysics holds that there are only two kinds of natural existents, mechanisms and organisms. Holists appear to accept this, and regard societies as belonging to the latter category. Since Plato, the conception of society as an organism has appeared over and over again in the history of social thought. Just as Bergson and Driesch argued that the property that distinguishes living organisms from physical matter is the existence of a 'vital principle', so some holists seem to believe that a society possesses an equally mystical property of its own, a Geist, an animating spirit, a vis vitae, or a teleological mission that is its historical destiny. The notion of societies as organisms appears to be supported by the 'functionalist' approach in social science, which treats the various social institutions in terms of the functions they perform in the operations of the whole, analogous to the way in which the physiologist explains the functions of the heart, liver, kidney, etc., in an individual animal. This leads by a short step, or a stumble, to the contention that societies have purposes and objectives of their own, and to the accompanying judgement that the welfare of a 'society' is different from, and has moral priority over, that of the persons who compose it. Societies are, by such reasoning, not only reified, but reified as entities that possess mentational and moral properties. To avoid this error it is not necessary to embrace the epistemic and ontological canons of methodological individualism. Acceptance of the notion that societies belong to a distinct ontological category, being neither mechanisms nor organisms, will serve. From this triadic standpoint, mentation is a property that only organisms possess,
but organization, operating through different modes, is a property of all three types of existents.

Part of the problem we encounter in the individualism-holism debate is semantic rather than philosophical or scientific. When Romeo exclaims, ‘Juliet is the sun,’ we know that he does not mean that she is a fiery ball 865,000 miles in diameter; and when a journalist writes that ‘the White House consulted the Pentagon’ we know that he does not mean that two buildings talked with one another. But when we say things like ‘the Catholic Church opposes abortion’ or ‘Hitler invaded the U.S.S.R.’ it is more difficult, but just as important to clear thinking, to recognize that figures of speech are being employed. Narrative historians, especially those who regard their craft as belonging to the domain of literature rather than social science, tend to use figures of speech with little restraint, apparently preferring them to straightforward descriptive locution. For example, J. J. Scarisbrick in his widely acclaimed biography Henry VIII (1968) often says ‘Henry’ or ‘England’ when he is really referring to the group of persons responsible for the formation of the government’s foreign policy; in the one case being excessively individualistic and in the other excessively holistic. It would be ridiculous to demand that history should be written like a scientific paper, without recourse to metaphors, metonymies, and other figures of speech. But some writers who adopt methodological holism use not only language but arguments in which collective entities are construed as having powers of autonomous action. In our day and age, the nation-state is the favoured collective entity for such endowment, but it is not at all uncommon to read popular, and academic, literature in which religions, cultures, socio-economic classes, language groups, or, with grand comprehensiveness, ‘society’ or ‘history’ are treated as if they possess such powers and have needs and aims different from, and superior to, those of individual humans.

The great debate between individualists and holists that was prominent in the literature of social science a quarter-century ago was largely initiated by Karl Popper’s The Open Society and its Enemies (1945). Popper argued that there is a close connection between epistemology and political philosophy. He traced the development of modern totalitarianism as exemplified by fascism in Germany and communism in Russia to ‘the spell of Plato’, which had been transmitted to the modern West by Hegel and Marx. As a philosopher of science Popper viewed the central error of these political doctrines to be epistemological: fascism and communism were, in his judgement, based upon methodological holism, an epistemic error. Individualism is not only the proper methodology of scientific inquiry in the social domain, but provides a philosophical bulwark against the enemies of democracy and personal freedom. At the time, this seemed for some of Popper’s readers to be a revelation of profound importance. The fundamental philosophical source of political totalitarianism had been identified; now it would be possible to recognize its nascent forms in social science, and extirpate them, by rational criticism, before they could undermine the fabric of a free society.

Popper was undoubtedly correct in perceiving an historical connection between the totalitarian philosophy of the state and the notion that a society or, rather, a nation, is a whole that is ‘greater’ than the sum of its members. One has only to pay attention to the language of modern romanticist political philosophy to become convinced that this connection persists. But Popper overstated his case. The rise of fascism and totalitarian communism in Germany and Russia cannot be ascribed solely, or ‘fundamentally’, to a holistic conception of society or the adoption of methodological holism in the analysis of social phenomena. Moreover, social scientists, especially historians and sociologists, have effectively used holistic concepts without slipping into the error of ascribing preternatural powers, teleological missions, and valuational capacities to social wholes.
In making a linkage between the political philosophy of democratic states and the
epistemology of social science we have to recognize the role of utilitarianism. As a
political philosophy utilitarianism developed a strong individualist orientation,
especially in the hands of John Stuart Mill and Henry Sidgwick. This focused, not on
the necessity of eschewing holistic entities in social science, but on recognition of the
fact that only individuals have the power of choice and moral judgement. The
companion of utilitarian political philosophy and ethics in the epistemology of the
social sciences is cognitive instrumentalism. This does not consider scientific
concepts in terms of their inherent nature but evaluates them in terms of their
capacity to connect with empirical information in ways that render social phenomena
intelligible, and manageable by individual and collective human action. How we
should act does not come within the orbit of instrumentalist epistemology, because
science and value judgements belong to different domains of rational discourse. One
cannot derive moral values from the findings of social science any more than one can
from physics or biology. But the social sciences have, unavoidably, a closer
connection with values than do the natural sciences. Clarifying that connection is a
major issue in the philosophy of social science, to which we now turn our attention.

3. The problem of objectivity

Among the many properties that have been described as unique to the species
Homo sapiens, the possession of the mentational capacity for moral judgement has frequently been instanced.
Sociobiologists have argued that the

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performance of altruistic acts (which they regard as the fundamental nature of moral
behaviour) is no different in man than in other animals and is not, at bottom, a
matter of mentation. It is governed by biological imperatives that reflect the degree
of gene-sharing between the performer of the act and the beneficiary of it. But other
biologists reject this as a satisfactory explanation of human morality. Ernst Mayr has
recently argued (noting that other biologists have made similar contentions) that 'the
emergence of genuine ethics' has come about only with the evolution of an organism
that is capable of anticipating the consequences of alternative courses of action and
choosing between them on the basis of moral principles. 'Human beings,' says Mayr,
'have the capacity to make such judgements because of the reasoning power
provided by the evolving human brain,' and adds, in italics, 'The shift from an
instinctive altruism based on inclusive fitness [i.e. gene-sharing] to an ethics based on
decision making was perhaps the most important step in humanization' (Towards
a New Philosophy of Biology, 1988, p. 77). There can be no question that an
organism can do only what it has the biological capacity to do, and humans do
appear to make value judgements. The issue that concerns us here is not whether
this is a mere appearance that masks the operation of genetic imperatives, as strict
sociobiologists would claim. I will accept Mayr's view of the case, that humans make
rational choices which are influenced by value judgements. The problem we must
now consider is the connection, if any, between this and the other rational activity
that is an outstanding property of mankind, our ability to obtain objective knowledge
of the world in which we live.

If an empirical science of ethics could be constructed, the connection between these
two activities would be very close. Some biologists and philosophers have recently
argued that this is now possible, that we can explain not only the moral principles that men hold, but demonstrate what principles they ought to hold by construing such beliefs as having evolved, like the brain, by the process of natural selection (see Michael Ruse, *Taking Darwin Seriously*, 1986, chapters 3 and 6, for a good discussion of this). We have noted from time to time in this book the argument that ethical principles can be directly derived, not from biology, but from the social sciences (see, for example, the discussion above of David Ricardo’s theories of value and rent, Chapter 9 A and B). Such contentions amount to saying that, contrary to David Hume’s famous dictum, moral propositions can be derived from factual propositions. Though philosophers still debate this, no one has yet been able to provide even a hypothetical example of such a connection, and it seems to me that ‘Hume’s fork’ remains untarnished.

But this does not mean that there is no connection at all between moral propositions and scientific ones. On the contrary, we rely upon our scientific knowledge to supply the specificity to general moral principles that is necessary for action. For example, if we accept the general moral principle that the members of a society should have greater equality of opportunity, we rely upon empirical sociology and economics to tell us whether we ought therefore to adopt specific social policies such as the special taxation of inherited wealth and the financing of education from state funds. This kind of connection between values and science creates no problems that need concern us here. The connection we have to examine raises the question whether it is possible to construct a social science that is objective or, put differently, if scientific knowledge is defined as having the property of objectivity, is it possible to have scientific knowledge of social phenomena? The main contention that this is not possible rests upon the view that in the study of social phenomena there is such an intimate and unavoidable entanglement between the values held by scientists and their research procedures that objective findings are unattainable.

If the members of a community believe that a bridge would be desirable in a certain place, engineers can be relied upon to construct it by using objective knowledge. If engineering knowledge were itself so deeply affected by value judgements that, for example, the stress data for bridge trusses were dependent upon the engineers’ values, the community would be in great difficulty, even if its members were unanimous in regarding a bridge as desirable. Is this the case with the social sciences? Is the work of economists, sociologists, and others unavoidably entangled with value judgements to such a degree that there can be no reliable knowledge in this domain? In the discussion earlier in this chapter of the general epistemic argument advanced by Russell Hanson and others that empirical observations are ‘theory-laden’ (section A 1), this problem was dismissed rather summarily; now we must examine it more fully.

The examination of the sociological ideas of Herbert Spencer and Max Weber in Chapter 15 noted that, in their methodological writings, they regarded this problem as very important. The greater part of Spencer’s *The Study of Sociology* (1875) is devoted to it. In Spencer’s view the sociologist is likely to bring certain preconceptions to his study of social phenomena because he has been enculturated into, and remains a member of, a society with certain commonly held beliefs and values. He regarded this as a problem for scientific sociology, but not an insuperable one, since the conscientious sociologist can identify the biases he may possess and guard against them. It is perhaps significant that his book was frequently adopted as a text in introductory courses in sociology in American universities. Apparently American sociologists, at this early period in the development of their discipline, regarded Spencer’s warnings, and his advice on this matter, as salutary. In
Germany, Max Weber expressed a similar view even more strongly. The social scientist, he repeatedly insisted, must adopt a meta-value, the obligation to keep his work free of value judgements. Gustav Schmoller and his followers in the then dominant school of German social science held a different view with equal pertinacity. The social scientist, they contended, is morally obligated to promote values in his teaching and writing. Needless to say, they took it for granted that values which they themselves held should be the ones promoted. The issue of what Weber called \textit{Wertfreiheit} (value-freedom) became a matter of sharp controversy, which punctuated the meetings of the Verein für Sozialpolitik, the leading association of German social scientists. The controversy spread to neoclassical economics with the almost simultaneous publication of Gunnar Myrdal’s \textit{The Political Element in the Development of Economic Theory} (1930) and Lionel Robbins’s \textit{Essay on the Nature and Significance of Economic Science} (1932), which took opposite positions. Myrdal argued that economics is inextricably entangled with value judgements; Robbins claimed that economic theory, as a pure logic of rational choice, achieves the \textit{Wertfreiheit} that Weber demanded. Since then the controversy has continued unabated (for an excellent review of it see Mark Blaug, \textit{The Methodology of Economics}, 1980, chapter 5).

With the downfall of positivism the issue has also entered the domain of general epistemology. The ‘rhetorical analysis’ espoused by Donald McCloskey declares that all claims of scientific objectivity are a sham and that so-called scientific publications should be read as exercises in persuasion, which are strongly governed by the author’s own value judgements. A similar view is contained in the writings of the Edinburgh school. Less bluntly recognized, the view that science and value judgements are interlaced is implicit in the epistemological theory of conventionalism, Thomas Kuhn’s paradigm model, and Imre Lakatos’s methodology of scientific research programmes. On the other side, realists and cognitive instrumentalists defend the view that value judgements can be detached from pure science (except, of course, for the meta-value that it is better to know the truth than to be ignorant or misinformed), and that these two domains need be intimately connected only at the level of application.

A companion issue that intersects with the debate over \textit{Wertfreiheit} is the contention that the work of the scientist is influenced by his ‘interests’, that is to say, his desire to achieve objectives other than the advancement of knowledge. That scientists are not saints but, like common folk, are motivated by the desire for wealth, fame, power, respect, and admiration is scarcely to be doubted. That these motives, rather than the search for knowledge in itself, frequently dominate their activities, as scientists, is equally plausible, supported, for example, by James Watson’s frank account in \textit{The Double Helix} (1968) of how he and Francis Crick discovered the geometry of the DNA molecule, Nuel Pharr Davis’s \textit{Lawrence and Oppenheimer} (1969), David Hull’s \textit{Science as a Process} (1988), and numerous other books and articles recounting the ‘inside story’ of scientific discoveries, and by biographies of prominent scientists. The role of non-intellectual motives in scientific practice is, in fact, a matter of more interest to the social scientist than the issue of
since it raises the question of whether the institutional organization of science and the incentive structure it contains contributes to the advance of objective knowledge or retards it. But let us defer that issue for a moment.

Ernest Nagel has surveyed the question of objectivity, with specific attention to the social sciences, in his *The Structure of Science* (1961). Focusing on his discussion will serve our purposes, since he considers the main arguments that have been advanced against the notion of *Wertfreiheit* in social science and gives counter-arguments in defense of it. (In this summary, and the following discussion, I will draw upon material in my paper 'Social Science and Value Judgements', *Canadian Journal of Economics*, 1977.)

The first argument examined by Nagel is the contention that social scientists do not study all social phenomena with equal intensity; they select among the potential specific topics of study and, it is claimed, their selection is determined by value judgements. Nagel rejects this argument, on the ground that no scientist can study everything, even within the limited domain of his discipline, and that social scientists are no different from natural scientists in selecting among potential topics. This seems to me to be an inadequate defence of social science, for two reasons. First, one cannot acquit one branch of science from a charge of questionable procedure simply by showing that other branches are also culpable. Secondly, and more important, there is the question whether the social and natural sciences are equally culpable. One cannot measure this quantitatively, but there would seem to be good reason to believe that the selection of specific topics for investigation is governed by value judgements to a greater degree in the social sciences. Social research is strongly oriented towards social problems and social scientists may differ greatly in how they evaluate such problems. For example, one economist may regard the distribution of income between workers and property owners as important in assessing the quality of a society, another may think that this is better indicated by the size distribution of income regardless of its source, and a third might regard the number of people whose income is below the 'poverty line' as the relevant indicator. Natural scientists may select among lines of research on grounds of their comparative potentials for social betterment, but they are less strongly impelled to consider the practical applications of their research than social scientists are. In the area of pure science, topics are selected for research by considering the comparative potentials of different lines of research in yielding scientifically important results. In doing this scientists are making value judgements. But this is not the kind of value judgement that is relevant to the assessment of scientific objectivity, for either the social or the natural sciences. To claim that a scientist is not being objective when he chooses one line of research as more promising, scientifically, than another, would lead to the ludicrous contention that the claim to objectivity could be sustained only if scientists were to allow their research topics to be determined by a process of random selection!

Nagel makes a distinction between 'characterizing judgements' and 'appraising judgements' and contends that some of the arguments made against the notion of *Wertfreiheit* in social science rest upon a confusion of these. Every scientist must characterize the specific phenomena he observes and investigates as belonging to a certain generic class. The biologist, for example, uses a definition in order to determine whether a particular phenomenon belongs to the classification 'respiration' or 'photosynthesis'. In doing so he is making characterizing judgements, but he is not making any evaluative
appraisal of the phenomena. In the social sciences, says Nagel, equally immaculate characterizing judgements may be made. In principle, this may be correct, but in practice there are great difficulties. If an attack is made on a village by a group of armed men, is it an act of 'terrorism', or 'liberation from tyranny', or a 'bandit raid'? Such characterizing judgements are obviously infused with values, and the implicit values govern the research undertaken to explain them. In the social sciences there are many concepts, such as, for example, 'money supply', 'voting', 'congressional committee', and 'professional association', that permit purely characterizing judgements to be made, but there are many that cannot be detached from valuational connotations: 'unemployment', 'crime', 'drug addiction', 'log-rolling', to cite just a few examples of the many that could be instanced. The value-loading of such concepts may be a matter of language, due to the fact that ordinary language is used for scientific purposes in economics, sociology, and the other social disciplines. But it is doubtful that any language, even an artificially contrived one, could be preserved from contamination by values, when used to examine social phenomena.

Nagel notes the contention that the social sciences cannot be objective because prior values govern not only the choice of specific topics for investigation and the concepts employed, but are injected into the analysis itself and effectively control the conclusions reached. This amounts to saying that social scientists are prone to employ warped logic and improper treatment of empirical data in order to support views they held prior to the investigation. I think that Nagel is right to dismiss this as a problem specific to the social sciences, since it is simply bad scientific practice tout court. Such practices may be easier to conceal in the social than in the natural sciences, but the contention that the possibility (or even the probability) of this destroys the claim of objectivity is not warranted.

There is, however, a problem in the treatment of empirical data that cannot be reduced to a matter of sound or unsound practice, which Nagel considers but, in my view, underrates. This problem was first noted by Jerzy Neyman in his early papers that led to the development of the Neyman-Pearson model of statistical inference in the 1930's and emerges clearly in Abraham Wald's formulation of statistical inference as a process of decision-making under conditions of uncertainty. The heart of the problem is concisely stated by Richard Rudner, a philosopher, in a paper unambiguously entitled 'The Scientist qua Scientist makes Value Judgements' (Philosophy of Science, 1953). When the data used in a scientific investigation are statistical, as is usually the case in the social sciences, one rarely finds that they provide categorical answers to the question at issue. Take, for example, the 'law of demand' in economics. This stipulates that people will purchase more of a commodity at a lower price than at a higher one, other factors affecting purchases held constant. If we wish to ascertain whether this is consistent with empirical evidence, we might collect the relevant data by means of a sample survey of households and compute the general mathematical relationship between quantities purchased and prices. Leaving aside the technical difficulties in doing this, suppose we find that the relationship between prices and quantities purchased is indeed negative as the law of demand states. Our data, however, are merely a sample, not the whole population of households in the community. What econometricians do in such a case is to calculate the probability that such a survey will yield the results it did by chance. Let us say that this tells us that such results could occur by chance four times in a hundred if we were to do the exercise over and over again. Should we conclude that the data support the postulated law? If we do so we risk some danger of accepting the law when in fact it is not true. If, however, we conclude that the law is not supported by the data, we risk rejecting it when in fact it is true.

There is no objective way of determining when we should accept and when reject: is a 4 per cent chance of being wrong 'small' or 'large'? The economist might say that,
in this particular case, he is prepared to accept such a chance of being wrong. But suppose the data yielded a positive relationship between prices and quantities purchased, not the negative one postulated by the law of demand. In this case the economist might well say that, since the law plays such a vital role in the general structure of microeconomic theory, he is unwilling to accept the result of the survey and reject the law even though there might be a 96 per cent chance that the law of demand is indeed false. What probability of being wrong would convince him? Mathematical statistics deserts one here. A value judgement must be made, and such judgements might well differ among scientifically conscientious economists.

The above illustrations indicate that a scientist might be unwilling to accept the results of an empirical study, even if the mathematics of inference indicated that there was only, say, a 4 per cent probability that the results were untrue, when such acceptance would do grave damage to a central pillar in the general structure of the science as presently constituted. One of the merits of the epistemological theory advanced by Imre Lakatos is in pointing out that certain propositions in a ‘research programme’ are more vital than others, being part of the ‘hard core’ of the programme. Scientists are reluctant to accept empirical evidence that is inconsistent with the hard core, for to do so would require the abandonment of the programme, and no viable alternative may be available. The judgement involved here concerns the scientific, not the moral consequences of regarding contradictory evidence as conclusive. That is, it is not the kind of judgement that involves values other than the scientist’s general belief that the advancement of scientific knowledge is a worthy aim. Indeed, the insistence that one must eliminate all values from science would amount to the destruction of scientific inquiry. Mark Blaug points out that the acceptance of empirically true statements rests upon the view that they ought to be accepted (The Methodology of Economics, 1980, p. 131). If this were the only value judgement involved in science, its claim to objectivity would be secure against attack by all but the most determined scholastic pedant.

Social scientists have a heuristic interest in protecting their hard core propositions, just as natural scientists do, but choosing between the risk of accepting a false hypothesis and rejecting a true one also enters when statistical data are used to provide empirical foundations for decisions on matters of social policy. The judgements involved here are not devoid of valutional content. For example, if the data show that there is a positive correlation between the level of the legal minimum wage and the amount of unemployment, with, say, a 4 per cent chance of being due to chance, should one advise the governmental authorities to lower the minimum or repeal the law altogether? Undoubtedly, economists employed by labour unions and ones employed by business firms will differ on this; but even economists who are not interested parties may differ, for the decision hinges upon value judgements. This problem applies to all applied sciences. An engineer might, for example, accept a 90 per cent safety probability for a coal-fired generator but insist on a much higher one for a nuclear power plant. The social sciences, however, are much more dependent upon non-experimental statistical data (some of which are quite soft) than the natural sciences are, and much more of their work is directly oriented to making assessments of social policies.

These are the main arguments that one finds in the literature on the issue of Wertfreiheit in the social sciences. Nagel’s general conclusion is that the contention that the study of social phenomena cannot be objective is not sustainable or, at least, that the problems one encounters in the social sciences are no more severe than those that face natural scientists. I have tried to show that, with respect to most of these issues, Nagel’s view is questionable; value judgements do enter in significant ways into all domains of scientific inquiry but
they do so to a greater degree in the study of social phenomena and the application to social policy. Does this mean the notion of objectivity must be abandoned? Not by any means. In our general review of the epistemology of science in the first section of this chapter we found that the notion of *absolute certainty*, which the early positivists embraced, must be abandoned in any realistic view of scientific knowledge and its potential for further development, but that does not mean that it is illusory to believe that we have some objective knowledge of the world and that we can improve that knowledge. Perfect insulation of science from value judgements is not possible, but this is merely one of the reasons why we must regard our knowledge as contingent. The instruments of scientific inquiry cannot furnish apodictic truths about the world, but they can enable us to obtain limited and tentative knowledge about it and, in some areas, that knowledge is sufficiently reliable to serve practical purposes.

Objectivity, then, like certainty, must be regarded as a philosophical ideal rather than a characterizing property of scientific knowledge. Most philosophers of science, including most of those who have abandoned positivism, hold that it is desirable to make our knowledge of the world more objective and more certain. This is, of course, a value judgement, but it is one that serves the process of scientific inquiry rather than rendering it problematic. That these ideals cannot be attained is not a reason for disregarding them. Perfect cleanliness is also impossible, but that does not serve as a warrant for not washing, much less for rolling in a manure pile. All knowledge is human knowledge and humans are imperfect beings, who can only cope with the problems they encounter as best they may. Scientists are human too. That they make value judgements in selecting problems for investigation, in framing theoretical concepts, and in drawing inferences from empirical data means only that science requires the use of informed judgement as well as the application of formal logic and the rules of empirical methodology.

If objectivity is regarded in this way, the important issue is the pragmatic one: by what means can the degree of objectivity in scientific work be raised, or prevented from declining? Herbert Spencer advocated that social scientists should be conscious of the biases they may harbor owing to family background, education, and the general norms of their culture, as well as their personal interests. Insisting that all aspiring social scientists should take a course in professional ethics modelled after Spencer’s *The Study of Sociology* would probably do no harm, but one may be sceptical that this would suffice to assure that social research would be pursued with a degree of objectivity sufficient to preserve it from gross contamination by value judgements and other biases. Max Weber argued that *Wertfreiheit* requires consciousness of potential bias, and recommended honest and public admission by the social scientist of the values he embraces, but he also made the much more important point that the accumulation of reliable knowledge depends as well upon the social organization of science. Ernest Nagel echoes Weber’s point in emphasizing ‘the self-corrective mechanism of science as a social enterprise’ that operates when scientists are free to criticize one another and pursue their activities in a regime of friendly competition.

The importance of the social organization of science with regard to the issue of objectivity can be illustrated by comparing two cases of extreme lack of objectivity: the outright fabrication of empirical data. Several years ago a scientist named Summerlin, at the Sloan-Kettering Institute in New York, claimed to be able to make successful skin and corneal transplants in mice. This would have been an important contribution to scientific knowledge, and possibly to the Institute’s programme of cancer research, if it had been valid. But Summerlin’s data were fabricated. Other scientists tried to reproduce his results, with lack of success, and the fraud was...
quickly discovered. The story is reported in Joseph Hixson, *The Patchwork Mouse* (1976). In reviewing this book, P. D. Medawar (*New York Review*, 15 April 1976) gave an excellent discussion of the problems posed by cases of data fabrication in science but came to the conclusion that ‘no great truth about scientific behavior is to be learned from the Summerlin affair except perhaps that it takes all kinds to make a world’. On the contrary, there is an important lesson to be learned from it: that when free, independent inquiry is permissible, fraudulent practices maybe quickly exposed, and negligible harm done to the body of scientific knowledge or its practical applications. By contrast, there is the case of Trofim Lysenko in the Soviet Union, who fabricated data on genetic transmission in plants in the 1930’s. As president of the Lenin All-Union Academy of Agricultural Sciences Lysenko was in charge of Soviet agricultural research and its applications. He was also head of the Institute of Genetics of the Soviet Academy of Sciences, and a member of the Supreme Soviet, the highest legislative organ in the political system of the U.S.S.R. Other scientists were not free to dispute Lysenko’s theories. Those suspected of harbouring other views lost their posts, some went to prison, and some were executed. The consequences were that Russian work in genetics, which had been in the forefront of world research before the rise of Lysenko to power, was utterly destroyed, and remained locked into the Lamarckian theory of inheritance that Lysenko embraced, until the fall of Khrushchev in 1964. During this period immense harm was also done to Soviet agriculture, which was obliged to follow Lysenkoist prescriptions. If science in the U.S.S.R. had been organized differently, been more pluralist, less subject to the control of political authorities, it would have undoubtedly been more objective. Political ideology, however, is not the only thing that can generate gross departures from the ideal of objectivity. Any form of organization that seriously constrains free competition in the domain of scientific research can have similar effects, including control by scientists themselves. Francis Bacon’s notion that a ‘Solomon’s House’ of science should be established, in which scientists would be brought together as a unified authoritative agency to speak with one voice, is a prescription for the destruction of science, not for the furtherance of scientific knowledge and its reliable application to practical problems.

The social sciences are more heavily involved with value judgements, political ideologies, and other contaminants of objectivity than are the natural sciences and, lacking the ability to make closed-system experiments, they are less able to contend with fabricated data and other abuses of empirical evidence. But mainstream social scientists are committed to an epistemology of empiricism, and where they work in a pluralist environment of intellectual independence, the ideal of objectivity can be approached, even though never attained. In his famous essay *On Liberty*, John Stuart Mill argued the case for intellectual freedom on utilitarian grounds, as a form of social organization that promotes the advance of knowledge. The thesis he advanced need not be defended as a matter of faith or liberal political ideology it is certified by the historical experience of science in all its domains.