

INTRODUCTION

I

It is now well understood that the macro structures of economies and societies and the causal mechanisms of their formation and history are beyond common sense understanding. Only a form of analysis and mode of understanding that penetrates to the obscured structural relations and imperatives of economies and societies can begin to reveal and explain the real history and powers of the organizational basis of social life. This, virtually a truism, seems to have been well understood by many social scientists for two centuries. Yet, surprisingly, there are still some in the history profession who believe that no special general concepts, methods, forms of explanation, or theories, apart from those intuitively absorbed from the prevailing form of "common sense" of the day, are required to grasp the histories of economies and societies or the reasons for human actions or the causes of events.

This dissertation is about how the historical processes of structural change in economies and societies are being conceptualised and explained and about how the main concepts and forms of explanation could be improved. The starting point is an awareness that there are major methodological and conceptual weaknesses in the explanations currently provided by many historians of economic and social structures. Empirical weaknesses in explanations will always be with us in the sense that more information about present and past economies and societies is constantly becoming available as a result of theorisation, research, and the process of social change itself. But methodologies and general concepts may be susceptible to permanent improvement as a result of analytical thought, including examinations of conceptual systems, of the logic of enquiry and reasoning employed by particular sciences, and of how some sciences have become more advanced than others. In particular, the crucial process of forming a coherent *domain of scientific enquiry* for economic and social structural history must be examined.

This examination is not overtly concerned with the empirical validity or strength of particular explanations of economic and social change. Empirical enquiry always takes place within certain methodological and theoretical frameworks. The value of these frameworks rests ultimately on their usefulness in helping us understand and explain the world but the relationship between the empirical power of explanations and their frameworks is not a simple matter. The complexity of that relationship and the use of frameworks in enabling and disabling explanation must be explored thoroughly before the

problem of empirical validity can be reconsidered. The history of science seems to indicate that the long-run value of frameworks, including general theories, is not dependent to begin with on simple empirical confirmation. As Descartes, Hume, Kant, and the Enlightenment tradition taught, the question of *how* we know is prior to *what* we know.¹

II

In spite of the radical critique of knowledge by post-modernist philosophers the will to truth and the consequent search for certainty remains a (perhaps the most) powerful motivator of intellectual endeavour. But this fundamental motivation and rationale of modern thought is under attack as rarely before during the era of Enlightenment and post-enlightenment. The general systematic activity of building explanatory social theories that has gone on, through various vicissitudes, since the 17th century (and started much earlier if we include medieval thinkers such as Ibn Khaldûn, and even much further back if we consider Plato and Aristotle to have been thinking about these problems), is being rejected by radical neo-Nietschkean critics of the epistemological enterprise. The task of explanation has to be justified anew, it seems. At bottom is the fundamental issue of how we are to live our lives -- in what social arrangements, by what norms, upholding what morality? Are there or should there be universal laws or rules or structures governing the possibilities of social life? But does it make sense to ask these questions or should we simply be getting on with living as best we can under the circumstances in which we find ourselves? Of course these questions have long since been asked. They are at the heart of religion and philosophy. It seems to be the fate of humans to be dissatisfied with daily life and its local conditions and seek, instead, for the meanings and the causes of life, social arrangements, and the nature of the world.

General understandings that human life takes place in ordered societies which constrain actions and beliefs and attempts to conceptualise and explain the nature and effects of these societies have been developed during the past couple of centuries. However, the debates between modernist and post-modernist modes of thought over the logic, meaning, efficacy, and relevance to social enquiry of scientific reasoning have reached new levels of intensity in recent years within western culture. The internal collapse of Eastern European Communism has hastened and been hastened by disenchantment with (perverted) forms of modernism. Marxism at its best is modernist and enlightened and was used as the (distorted) ideological and rhetorical legitimation of so-

¹ Cf E. Gellner, *Legitimation of Belief* (1974) p. 28.

called "modernization" in Eastern Europe and elsewhere on the supposed grounds of its scientificity. Similarly, in the western capitalist world other forms of "scientific" reasoning have been attacked lately. Everywhere now there are rejections of the modernist project of constructing a universal, rational scientific basis for natural, social, and historical knowledge and for political action, which Marx and Engels, among many others, did so much to advance.

The extended articulation and defence of scientific history in this dissertation tries to design and situate its fortifications so as to deflect the attacks of advocates of relativism, post-structuralism, pragmatism, and "common sense" historiography. The fortifications are built on a prominent outcrop of the territory of analytical philosophy of science, and are constructed out of materials gathered from scientific realism. To the advocates of hermeneutical relativism, post-structuralism, and pragmatism, arguments for a *science* of history are now atavistic and naive; and to the "common sense" practitioners of traditional interpretive history they are irrelevant. Attempts to conceptualise and *discover* the *real* hidden structures of society and the *real* processes of social structural change are unfashionable and outmoded to all these opponents. But I persist in holding that the structural histories of economies and societies, like the histories of the earth and the biosphere, proceed largely independently of beliefs, concepts, theories, ideologies, and philosophies about them. The emphasis should not be on the autonomy of discourse and language nor on the autonomy of the phenomena of the social world but on *discovering* the relative autonomy of the structuring and transforming social processes of societal evolution. Theorists and philosophers only try to conceptualise the world or merely interpret each other's imaginative theoretical creations; the point is to *explain* the origins and nature of the real structures of the world and their transformations.

Economic and social historians (or structural historians, as I shall usually call them) have been labouring at this knowledge-constructing task for two and a half centuries. In what ways do they go about their task, what success can they hope for, and how successful have they been? These are largely methodological and philosophical questions but ones that can satisfactorily be answered only by a careful analysis of the presuppositions, practices, and results of such historians, an analysis that should employ some of the conceptual and analytical tools of philosophy of explanation. Such an analysis does not imply that philosophy is the arbiter of practice and truth, only that it has a powerful set of tools to help with the job. An alternative view, that practice must be its own evaluator, secretes an *a priori* epistemological conception behind a supposedly pragmatic and humanistic exterior. The problem with hidden epistemologies is that they

can mislead practitioners into believing that "common sense" (for which we should read "the currently prevailing idea of naïve empiricism") or personal empathic insight or rhetorical persuasiveness are the only possible arbiters of interpretation and explanation. In that case the rational idea of "truth" is rejected in favour of pre-rational or irrational "understanding", and that cannot be shared widely. The rejection by many historians of any attempt by philosophers and methodologists to criticise their practices and arguments from some external methodological and historical point of view must arouse suspicion that they do not wish to be confronted with the logical and explanatory implications of their own assumptions and presuppositions and hence do not wish to have the strength of their own arguments and conclusions tested at all. The persuasiveness of explanations *is* at issue, the question being of *how* explanations persuade.

III

Economic and social structures are mysterious formations -- at once intangible, invisible, even somewhat incomprehensible, yet powerful and in many cases vast and very long-lived. Their very existence, let alone their nature and historical character, was long obscured to rational, systematic, enquiry. Gradually in 17th century Europe and more so in the 18th, thinkers began to construct concepts about economic and social structures, or about what they first called "political economy" or "civil society", and about how such structures have evolved through human history. For most political economists and sociologists of the 18th and 19th centuries there was no distinction between the study of societies and the study of their evolution -- that is, no historical/present methodological distinction. However, with the gradual development of the social studies in the 18th and 19th century there began the processes of separating the social enquiries from enquiries into political and military events and the activities of powerful individuals and elites, and of separating present-oriented enquiries from historical ones. The society/ politics/history (or economy/politics/history) intellectual distinctions have been apparent in one form or another in European thought since the mid-18th century. They have been attacked and defended down the centuries. For much of the 20th century history and sociology have staked out opposing territories on the spurious past/present dichotomy. But in recent decades there has been a strong move on a broad front by many historians and sociologists finally to do away with these distinctions and replace them with "interdisciplinary history", or "social history", or "historical sociology", or "historical political economy", or "historical economics". The original concern of sociology and political economy with large-scale, long-run social change has been re-emphasised. The advocates of all these, sometimes syncretic, unions usually rightly believe that there is no ontological or

methodological basis for the old society/history distinction. However, I shall argue that these necessary (but sometimes over simplified) attempts at unification often miss the real, defensible basis of *empirical/theoretical* and *event/structure* divisions. But these divisions, which are rational on heuristic as well as ontological grounds, must be seen as existing within a wider *unified* field of socio-historical concepts and methodologies because events (including actions) and the natures of structures can and must be explained at once separately on one level but together on another, deeper level. The need for and resolution of separation within a layered methodological unity will become clear as we proceed.

The writing of economic and social history is viewed herein as part and parcel of the social studies rather than as part of some *sui generis* historical discipline. There seems no longer any use in seeking to make a contribution to debates over the supposedly peculiar nature of historical knowledge or the place of narrative versus analytical reasoning in historical understanding. That there should be no fundamental distinctions between historically-oriented and present-oriented enquiries into economies and societies and between narrative and analytical reasoning are basic assumptions. All economies and societies are historical in two senses -- real and changing -- whether they exist now or have ceased to exist. Therefore, all enquiry in the social studies should be historically-oriented. This means that all eras and processes require a historical consciousness for their understanding. The old, and apparently fading, institutional and methodological distinctions between history and the social studies reflect philosophical, emotional, and psychological pre-dispositions rather than ontological or epistemological necessities.

IV

A central contention will be that the institutionalised "disciplines" of economic history, social history, historical political economy, and historical sociology should be considered together as one domain of enquiry - the *domain of social structural history*. This is because they all deal, or should deal if they are true to their self-designations, with the problem of history of *social structures* and not with the history of events and actions, even if those events and actions are supposed to be peculiarly economic or social. The basic operational distinction within the social sciences should be between the study of events and the study of structures. And structures include political systems, mentalities, and cultures as much as economic and social systems. The defence of an event/structure distinction and a merger of economics and sociology will require an argument about the common nature of economic and social structures and how they relate to events. This

argument is crucial because concepts of structure tend to license various approaches to explaining its history. The various "disciplines" just mentioned all adopt similar but different concepts of their object.

I shall argue that social structures (including economies) are neither patterns of events, actions, and behaviour nor reducible to social phenomena but have a form of structural existence that is at once relatively autonomous but not separate from the totality of phenomena that occur within them. Neither are structures holistic or completely autonomous. This is not a novel argument but apparently it still needs defending because it is not sufficiently widely or well understood.² I have argued elsewhere and do so again below³ that if social history is to be a distinctive field of enquiry it must be about the history of social structures and requires a methodology relatively distinct from the history of events. But some readers apparently have not grasped this essential point and so believe that all self-styled social history writing is included within the category "social structural history". Much of what is designated as social history writing is in fact about events rather than structures and so does not require a methodology any different from that for explaining any kind of event. In that case "social history" does not refer to a distinct kind of discourse or domain so it does not even require a separate label. That it has a separate label says more about the intent of the practitioners and labellers than about their methodology. It also indicates that the methodological self-understanding that practitioners of a discipline have is not necessarily a reliable guide to the real foundations, practices, and results.

Herein I examine the underlying individualist and holist methodologies (which are often only unexamined assumptions) for approaching the explanation of the history of economic and social structures. I argue that there exists a third alternative to individualism and holism - what I call "*methodological structurism*". Like the other two, this third methodology is interconnected with a concept of structure and a concept of structural change. But unlike the other methodologies, it has not been well articulated or extensively defended.⁴ Methodological structurism, I shall argue, now exists quite widely

² As shown by some of the uncomprehending reactions by social historians to my *Explanation in Social History* (1986). See for example, Asa Briggs', Review Article in *History and Theory*, Vol. 29, No. 1, 1990, where he actually goes to the extent of inventing "quotations" to support his unwarranted assertions and misunderstandings.

³ Lloyd, *Explanation in Social History*, Ch. 1. See also Section VIII of Chapter One below.

⁴ I have given an outline of it in *Explanation in Social History*. Here I shall develop a more extensive discussion.

as an unexamined assumption within the explanations of many historians. I shall try to articulate it and show why it is the most appropriate methodology for approaching the explanation of structural history.

Structurism can also be understood in a wider sense as an approach to social explanation that has methodological, sociological, and historical dimensions, all of which logically and conceptually reinforce each other. This reinforcement is a crucial component in making for the possibility of *scientific explanations* of structural history.

V

Most historians, many sociologists and economists, and many philosophers reject the notion of a "scientific approach" to society and its history, while a few embrace it enthusiastically. The embracers, such as the cliometric historians, have sometimes equated "science" with quantification but this idea is now well understood as seriously defective, as I show in Chapter Four. That there are large and sometimes mutually incomprehensible differences between approaches to explanations in the socio-historical disciplines is of course well-known. In Chapter Two I will survey the range of approaches to show the extent and variety of differences. Many historians have been happy with methodological pluralism, fuzziness, and idiosyncrasy, even seeing them as virtues, because they believe not only that human thought and the social arrangements that it is about are free and unconstrained in their development by objective structures but also that repugnant "scientistic" explanations lead to narrow "technocratic" considerations about social "engineering". There have long been many defences (including the currently popular post-modernist and pragmatist theories and the traditional "common sense" interpretism) of a humanistic form of enquiry into social life that is akin to artistic or literary interpretation and must not be forced into pre-conceived objectivist channels. For many such writers, social life is a multi-faceted "text" that must and can only be re-interpreted constantly from within particular discourses. For them there is no such thing as objective social structure but only fluid social life. Social life can be apprehended and *understood* from many points of view rather than objectively explained. Related to this argument but also more widely ascribed to is the view that social and political concepts and explanations are "essentially contestable" because of the multi-faceted, dynamic, and supposedly phenomenologically-constituted character of society and social relations. On the other hand, there are also strong traditions of self-styled "scientific" enquiry, including neo-classical economics and its Cliometric offshoot, and Marxism, which, while not necessarily seeing economic and social explanation as methodologically akin to

physical science, attempt to make truthful explanations (employing general concepts and theories, factual evidence, and logical inferences) of supposedly objectively existing social structures, events, and processes.

It is not necessary to choose between being either a partisan supporter of hermeneutical understanding or of scientific absolutism. The poles of possibility in social epistemology are neither so simply delineated nor determining of actual methodologies. The employment of hermeneutical interpretation and essentially contestable concepts; understanding society as at least in part phenomenologically constituted; making progress in explanations; and producing scientific results; are all compatible with each other, providing we understand the roles each should play in a piece of social research. Indeed, we should see them all as essential parts of social explanation. But in discussing the question of science, explanations that are developed in physics, chemistry, and biology are neither good nor bad models for social explanation. While discussions of the question of scientific versus non-scientific knowledge are not irrelevant, it is a question, rather, of defining what is meant by "social science" here in terms of possibilities and actual practices and showing its power as a description to delineate between the strengths of various explanations. The sciences do share certain fundamental characteristics that set them apart from other forms of discourse. The post-structuralist, post-modernist, post-Enlightenment modes of discourse that attempt to put aside questions of objectivity, truth, and progress of discovery would of course see this kind of reasoning as irrelevant. But relativism, however cleverly and attractively packaged is still relativism and so suffers from its inherent defect of tending toward nihilism or at least an avoidance of practical real-world issues and problems. One is reminded of the strictures of Marx and Engels on "critical criticism" in *The Holy Family* and *The German Ideology*, and their exhortations to put metaphysics back on its empirical feet again.

Several arguments for a science of history, ranging from positivism to structuralism to realism, have been made by adherents to the Marxist tradition. This tradition has been fruitful in terms of methodology and theory but its limitations are now very apparent and have to be transcended if a better conception of science and more adequate explanations are to be achieved. I will develop a concept of the "*domain of the science of social structural history*" that tries to do justice both to the objectivity and the subjectivity of social structures and their history and tries to show why a scientific methodology is different from and preferable to a non-scientific one. I will show how the concept of "domain" is very valuable as a means of theoretically constituting objects for enquiry as well as incorporating and doing justice to the history of science and its accumulation of knowledge.

A powerful argument can be developed now to show that the logical structure of explanations of physical and biological nature are not well reconstructed by empiricist and positivist philosophy. Why empiricism and positivism are completely inappropriate descriptions of social and historical science will be defended by articulating the close relationship between structuralism and the anti-positivist realist tradition in philosophy of explanation. Given the widespread attacks on empiricism and positivism of recent decades another critique of these epistemologies should not be necessary but unfortunately empiricism and positivism are still associated in the minds of many historians and economists with the idea of scientific history. Moving toward a framework for a science of social structural history constructed partly on the basis of a realist philosophy of explanation is one of the fundamental goals of this methodological dissertation.

In the end I hope to have established the following theses:

SUMMARY OF MAIN THESES ARGUED HEREIN

- 1) There can be formulated a scientific domain of social structural history.
- 2) Structuralism is the most appropriate basic methodology for the domain of structural history explanation.
- 3) Structuralist methodology and structuralist theory are mutually reinforcing.
- 4) Structuralism and realism are the proper foundations for a science of structural history.
- 5) Structural history should be part of a methodologically unified socio-historical science.

In order to try to establish these theses the dissertation has the following structure:

STRUCTURE OF THE ARGUMENT

- 1) Chapter One outlines the history of structural history writing; discusses the basic concepts of a science of society and of scientific domain; discusses the importance of sociological realism; and outlines the compositional and evolutionary problems of the putative domain of structural history.
- 2) Chapter Two critically surveys the existing approaches to economic and social history and argues that the realist-relational approach is based on the methodology of structuralism.

- 3) Chapter Three contains a detailed analysis of methodological structurism in abstract and as the deep foundation of the work of certain prominent historians, most notably Clifford Geertz and Emmanuel Le Roy Ladurie, who are discussed in detail.
- 4) Chapter Four contains a detailed defence of the proposition that structurism and realism are the proper foundations for the scientific domain of structural history.
- 5) Chapter Five examines the relationship between historical materialist theory and structurism and argues that structurism denies the possibility of an ahistorical general theory of history such as historical materialism.
- 6) Chapter Six explores the normative implications for practical action of establishing a science of structural history on realist and structurist grounds.

CHAPTER ONE

EXPLAINING THE HISTORY OF ECONOMIC AND SOCIAL STRUCTURES:

Themes and Theses on Historiography and Methodology, and Movement Toward the Establishment of a Scientific Domain

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The fundamental problem addressed herein is whether explanations of the history of social and economic structures can be improved. This task must involve, to begin with, examining the serious philosophical and methodological disagreements that exist between practitioners and how to develop a rational consensus on how the field or discipline should proceed. This opening chapter is concerned with outlining the general problems that exist with methodologies of social structural history and how we might move toward a resolution of these problems. Many issues are raised here but few are dealt with comprehensively. The following chapters deal in greater detail with particular issues. This chapter, therefore, opens up the problems and tries to set them in their overall context and relationships with each other.

I EARLY APPROACHES TO THE WRITING OF ECONOMIC AND SOCIAL HISTORY

The writing of the history of economies and societies in the west was a product of the 18th century Enlightenment and particularly of the coming into European consciousness of the differences between social organisations and material wealth of European states and those of the peoples of the Americas, Africa, and Asia. Giambattista Vico's *New Science* of 1725¹ can be seen as indeed new in western thought for he wanted to theorise, describe, and explain the history of whole peoples and their forms of social organisation. But even his radical work had some roots in the previous century's empiricist philosophies of Hobbes and Locke and the Enlightenment desire to see humankind as the measure of all

¹ Vico, *The New Science* (1968). There is now a vast literature on Vico. For an interesting discussion see L. Haddad, 'The Evolutionary Economics of Giambattista Vico' (1983).

things. The comments by Hobbes and Locke about the material attainments of "primitive" peoples were also influential on later writers, notably Smith and Turgot.²

However, there was at least one very important non-western predecessor of these writers -- the 14th century Maghrebian Ibn Kaldûn -- whose fundamentally important work of structural history, *The Muquddimah*, was lost to western thought until the 19th century and only fully appreciated since the 1960s. It is now clear that this great work on the foundations of Islamic civilization and society provides the first known attempt to write a theoretical and empirical work of structural history.³

Systematic ideas about the economy as a partially separate realm of action were emerging in Britain and France by the early 17th century. These ideas usually centred on the role of commerce in promoting the wealth of the state. And it was consideration of the importance of commerce *vis-à-vis* agriculture and the rise of manufacturing, combined with the idea of social and economic differences among peoples, that led to the development of the first systematic economic histories and historical sociologies in the mid-18th century. In other words, it was the beginning of rapid economic development and the possibility of individual economic and political freedom that helped prompt the emergence in Britain and France of thought about economic and social change. As Karl Marx was later to comment, because in earlier forms of society there was no possibility of development of the individual or society there was therefore no enquiry into the causes of wealth.⁴ The central contributor to the first general body of enquiry into the history of the causes of wealth (i.e. into what we would now call economic development) was Adam Smith, whose pioneering work began in the early 1750s. Other members of the so-called Scottish Historical School, notably John Millar and Adam Ferguson, also contributed to the development of an embryonic historical materialist account of economic and social structural change based on a theory of four stages of economic history.⁵

Remarkably, at about the same time or even a little before the Scottish approach was being developed there were parallel developments in France, especially by A.R.J.

² R. Meek, *Social Science and the Noble Savage* (1976), Chs. 1 and 2.

³ Ibn Khaldûn, *The Muquddimah: An Introduction to History* (1958). See also Y. Lacoste, *Ibn Khaldun: The Birth of History and the Past of the Third World* (1984), and E. Gellner, *Muslim Society* (1981), Ch.1, for illuminating discussions.

⁴ K. Marx, *Grundrisse* (1973), p. 487.

⁵ See the excellent discussion of Smith and the Scottish School in Meek, *op.cit.*

Turgot. He had been influenced by Montesquieu's book on *The Spirit of the Laws* (1748). Although Montesquieu had not himself presented a structural history he was concerned to trace what we would now call the socio-political origins of legal systems. Turgot developed a theory of the stages of economic development that was similar to that of the Scots.⁶

The emergence of these forms of historical political economy and historical sociology in Britain and France in the second half of the 18th century against the background of comparatively rapid economic and social change in certain regions had no counterpart in Germany. Instead it was the idea of Universal History that dominated there.⁷ This idealistic and rationalistic genre was increasingly practised throughout Europe from the early 18th century and it attempted to tell the general story of whole idealised peoples, empires, and states (rather than the structures of economies and societies and individual economic initiatives) from ancient to modern times. Kant and Hegel made notable contributions and this stream of work culminated in Germany in the writings of Von Ranke. He was a precursor of the modern academic general history that became fully institutionalised in Anglophone universities in the late 19th and early 20th centuries.⁸

Smith forms a link between the 18th century Scottish historical and totalising approach to economy and society and the later abstract, individualist, and ahistorical approach of utilitarian classical economics. The economy became increasingly abstracted from the social totality and from its own past as, under the growing aura of physics, the "science of economics" was developed, especially in Britain and Austria, by Ricardo and the Mills.⁹ By the late 19th century the marginalism of Menger, Jevons, Walras, and Marshall had completed the process of abstraction, deductivism, and ahistoricalism. Henceforth mainstream theoretical economics was divorced from history, sociology, and politics and was later based explicitly on logical empiricist (i.e. nomological-deductive and reductionist) epistemological notions. But, as I shall discuss in a moment, from the 1950s a school of "new" economic historians developed in America that employed neo-

⁶ On Turgot see Meek, *op.cit.* and P. Groenewegen, 'Turgot, Beccaria and Smith' (1983).

⁷ See H. Butterfield, *Man On His Past* (1969), Ch.2; and F. Von Schiller, 'The Nature and Value of Universal History' (originally 1789) (1972).

⁸ cf Butterfield, *op.cit.*

⁹ See D.C. Coleman, *History and the Economic Past* (1987), Ch.3.

classical theory to engage in retrospective (i.e. present-oriented) "historical" quantitative economic analysis in opposition to the more orthodox inductive "old" economic history of that time.

Meanwhile, in early 19th century France Saint-Simon and Comte made sweeping attempts to found a new all-encompassing philosophical approach to society and its history. Their aims were, in a fashion somewhat similar to each other, to combine their positivist philosophical notions with the powers of natural scientific thought and the new industrial class to found a new world order ruled by the social laws of development that they thought they had discovered. Comte was especially concerned to enunciate the supposed developmental laws of social organic evolution. "Positivism" and "sociology" (two of his coinages) were combined to produce the first thorough-going version of historical sociology. However, it was based on an *a priori* holistic concept of society -- a society supposedly driven through stages by evolutionary "laws" that could be discovered by a "scientific" method of observation -- rather than on an empirically developed model or theory of social structure. Comte did not undertake empirical enquiry in the more modern sense of the term but he did strongly influence the subsequent tenor of much of European social thought.¹⁰

The German Historical School of Economists, who began their work in the 1840s, were influenced by the positivist search for laws but were not fully fledged positivists in the way of Comte or the British evolutionary sociologists of the late 19th century. German Historical Economics was a school of thought which developed parallel to and in opposition to the growing ahistorical, rationalist, and abstract character of English and later Austrian economics. This was the first real *school* of ideas since the Scots of a century earlier and it presaged a pervasive development in modern social thought -- that of the coherent school of ideas based on certain foundational meta concepts and epistemological commitments, as well as institutionalised personal loyalties and obligations. Since the mid-19th century there have been many others.

The phenomenon of schools owes more to external (i.e. socio-cultural) than internal (i.e. logical and conceptual) influences on the development of proto-sciences and other

¹⁰ On the ideas and influence of Saint-Simon and Comte see G.G. Iggers (ed), *The Doctrine of Saint-Simon: An Exposition, First Year 1828-1829* (1972); A. Comte, *The Essential Comte* (ed S. Andreski) (1974); and F.E. Manuel, *The Prophets of Paris* (1962).

academic fields. As Pierre Bourdieu has argued,¹¹ fields of enquiry are spaces in which there is a struggle for recognition by peers. This often takes, when sciences are immature, the form of conflicts between social groups in each of which there are leaders and followers. Nevertheless, it is still possible for there to be progress in the field itself in terms of its contribution to knowledge. The history of the natural sciences in the 19th century would seem to bear this out for they too were characterised by schools out of which emerged inter-school consensuses, unified domains, scientific progress, and the corresponding decline in the strength and pervasiveness of schools in the later 20th century.

Like many schools, the German economists had an ideological as well as a theoretical foundation.¹² They were interested, as the earlier mercantilists had been, in promoting the health of the whole nation. This involved studying empirically the peculiar history, character, and circumstances of each nation but within a general theory of stages. Writers such as Roscher, Knies, and Hildebrand in the 1840s and later Bücher and Schmoller¹³ proposed evolutionary stage theories of economic development reminiscent of that of the Scots almost a century earlier. The German historical economists had a powerful influence on German nationalism, economic and social policy, and social thought generally, through the *Verein für Social Politik*. Gustav Schmoller, one of the members of the younger school in the 1880s, sparked off the *methodenstreit* with his attack on the deductivist and abstract character of Carl Menger's marginalist economics. Karl Marx was one of the early critics of the Historical School for what he saw as their failure to understand the structure and dynamics of the capitalist mode of production,¹⁴ and Max Weber was partly educated in their approach, conducting his early research under the auspices of the *Verein*, although rejecting their quasi-positivist and evolutionist methodology.¹⁵

¹¹ P. Bourdieu, 'The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason' (1975), and 'The Genesis of the Concepts of *Habitus* and of *Field*' (1985).

¹² For a discussion of the German Historical School see J.A. Schumpeter, *History of Economic Analysis* (1954) Part IV, Ch.4.

¹³ There is an excellent discussion of their work in Schumpeter, *Ibid.*, and B.B. Seligman, *Main Currents in Modern Economics, Vol One* (1962). Ch 1.

¹⁴ K. Marx, *Capital, Three Vols.* (1971), *passim*, and *Theories of Surplus Value* (1963-71), Part III, *passim*.

¹⁵ For Weber's relations with the Historical School and the *Verein* see the essays in Part I of W.J. Mommsen and J. Osterhammel (eds), *Max Weber and his Contemporaries* (1987). On the *methodenstreit* see Schumpeter, *loc.cit.*; and M. Weber, *Roscher and Knies: The Logical Problems of Historical Economics* (1975), and *The Methodology of the Social Sciences* (1949).

In contrast with the powerful influence and official acceptance in Germany of the Historical Economists Marx and Engels were for most of their lives marginalised outcasts, their work, apart from their more political pamphleteering and journalism, known only to a few fellow revolutionaries. It was not until the rise of the German Social Democratic Party from the late 1870s that their social, economic, and historical ideas came to have a wider prominence. But even then they were poorly understood. Their structural historical writings contained a more refined and sophisticated version of historical materialist theory than had hitherto been developed. They were particularly influenced by Hegel's idea of the dialectics of world history, by Adam Smith's political economy, and by historians of ancient societies such as Niebuhr and Maine, in their approach to the question of the origins and character of modern capitalism and what had preceded it. They were opposed to empiricism and holism and to evolutionary theories of historical change. They defended what we would now call a realist social ontology and epistemology.¹⁶ The importance of this philosophical basis was that it underpinned their presentation in the mid-1840s of the first well-developed theory of social structure and its internal dynamics, and its relationship to economic, political, and legal change. There had been intimations of theories of structure earlier, such as in the works of Millar, Ferguson, Saint-Simon, and Comte, but none of the earlier writers had a well-developed account and they all lacked the empirical base of Marx and Engels. The latter expressly abandoned conjectural, speculative thought in favour of careful empirical enquiry into the real origins and history of the economic and social structure of bourgeois society.

Evolutionism and holism, rather than materialism and realism, were the dominant influences on English sociology and anthropology in the second half of the 19th century. The general idea of evolution -- that systems and entities evolve through stages from lower to higher or from simpler to more complex -- was an ancient notion but by the early to mid-19th century it was being extensively applied to society as if society were analogous to a holistic organism. Perhaps the most important influences on the English evolutionary sociologists -- notably Spencer, Maine, and Tylor, and later Hobhouse¹⁷ -- were the positivism and organicism of Comte, the development of biological science, the geology of Lyell, and later Darwin's theory of biological evolution. But of course Darwin was himself influenced by earlier evolutionary ideas in economics and sociology, such as those of

¹⁶ On Marx and Engels as realists see R. Bhaskar, *Philosophy and the Idea of Freedom* (1991), Section 2.

¹⁷ English evolutionary sociology and anthropology are discussed in J.W. Burrow, *Evolution and Society* (1966) *passim*; and S.K. Sanderson, *Social Evolutionism: A Critical History* (1990), Ch 2.

Malthus. By the early 20th century some of these evolutionary sociologists were describing their approach as "historical sociology".¹⁸

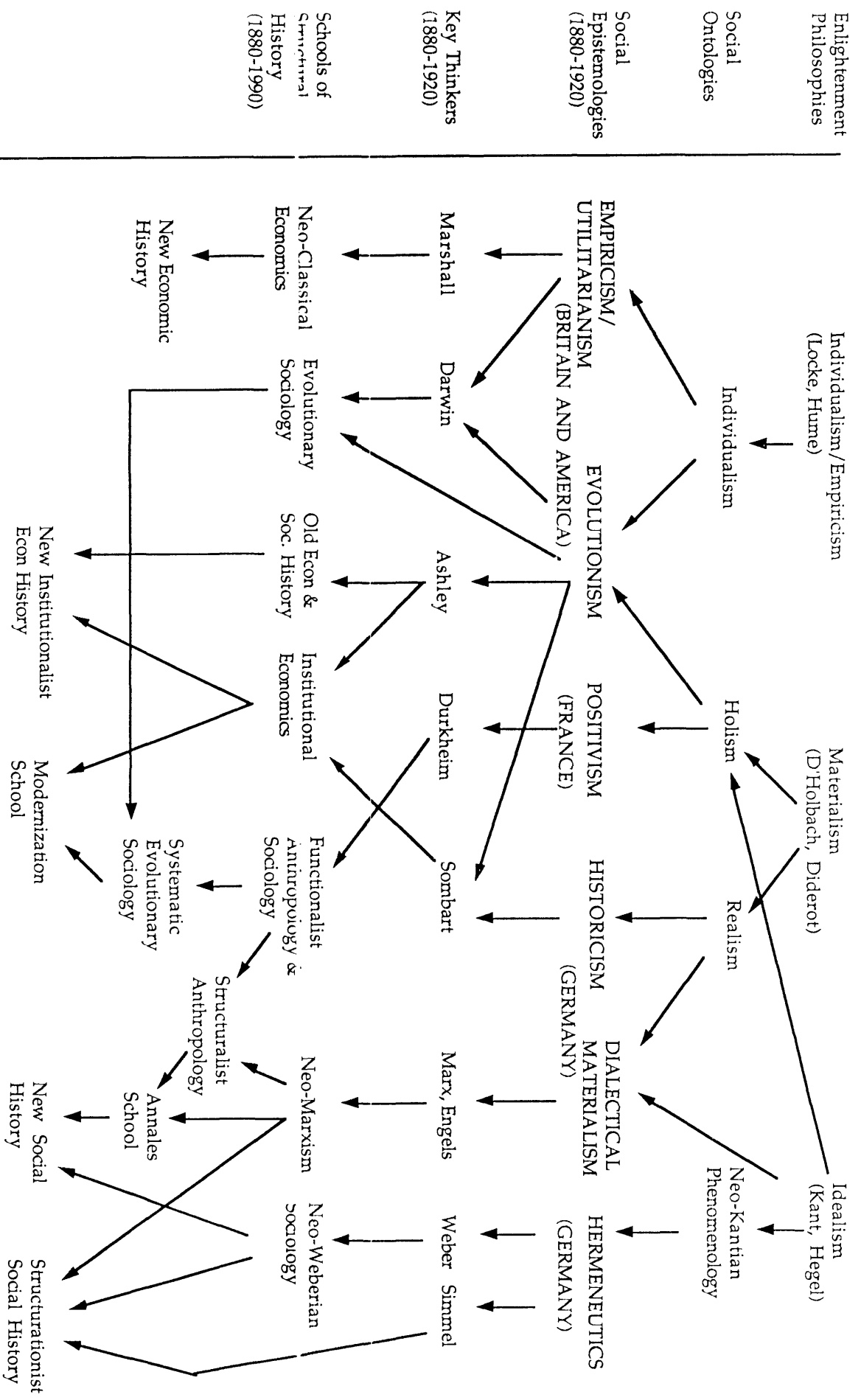
Contemporaneously with the evolutionary Historical Sociology School there developed in Britain a well-integrated school of evolutionary Historical Economics. Like the German Historical School, these writers, notably Rogers, Leslie, Toynbee, Ashley, Cunningham, Hewins, and Unwin,¹⁹ reacted against the abstraction, individualism, and deductivism of classical and neo-classical economics and proposed instead a relativist, inductive, and historical approach to explaining political economy. They had little lasting influence on economics but brought into being the modern "discipline" of economic history that thenceforth became institutionalised in Anglophone universities, especially in Britain and Australasia and to some extent in North America. The economics/economic history division that developed was based on a fundamental disagreement over philosophy and methodology -- an abstract versus historical split that, from the perspective of the late 20th century, seems to have been more harmful in the long run to economics than to economic history. The weakness of economic explanation springs partly from its lack of interest in social institutions, its lack of historical specificity, and from a misguided attempt by some economists to construct a positive science of society on the perceived model of physics.

It can be said with a high degree of justification that by the late 19th century there had developed four main philosophical or metaphysical conceptions of the nature of social structure, which had fundamental implications for approaches to its dynamics and its history. (At that time the later history/sociology distinction had not yet become significant.) The four conceptions were individualism, holism, realism, and phenomenology. Each of these was loosely nationally located and associated with a particular epistemology, as shown in Figure 1.1. The national differences can largely be traced to the influence of the national traditions of the preceding Enlightenment era. The development of structural historiography in the 20th century until the 1950s was broadly along the lines of the three main national/linguistic traditions -- Anglophone,

¹⁸ On English historical sociology see J.A. Hall, 'They Do Things Differently There, or the Contribution of British Historical Sociology' (1989).

¹⁹ On English historical economics see A.W. Coats, 'The Historist Reaction in English Political Economy' (1954) and 'Sociological Aspects of British Economic Thought (1880-1930)' (1967); D.C. Coleman, *History and the Economic Past* (1987); G.M. Koot, *English Historical Economics 1870-1926* (1987); A. Kadish, *Historians, Economists, and Economic History* (1989); S.K. Sanderson, *Social Evolutionism* (1990); J. Maloney, 'English Historical School' (1987).

Figure 1.1 The History of Structural Historiography



Enlightenment Philosophies

Social Ontologies

Social Epistemologies (1880-1920)

Key Thinkers (1880-1920)

Schools of Historical History (1880-1990)

Francophone, and Germanic. From the 1960s there began a blurring and a melding so that by the 1970s these distinct national traditions had largely disappeared.

In France Emile Durkheim was a dominant figure at the turn of the century and greatly influenced French social science for several generations. He had been influenced by Saint-Simon and Comte and developed an objectivist and functionalist account of social structure and social behaviour. Perhaps more than any other he was responsible for the institutionalisation and widespread propagation of sociology as a new theoretical and empirical positive science. His principle of functionalism, an explanatory teleological claim about the relationship of actions and structure, was widely adopted in 20th century anthropology and sociology, for example by Bronislaw Malinowski, A.R. Radcliffe-Brown, and Talcott Parsons.²⁰ Durkheim was also one of the influences on the development of structuralist anthropology by Lévi-Strauss and of the *Annales* School of structural history, which I shall come to in a moment.

In Germany, the revival of Kantian philosophy in the late 19th century by Dilthey, Windelband, and Rickert was powerfully influential on developments in social explanation. Max Weber's work from 1903 onwards contains an impressive attempt to transcend the *methodenstreit* debate between positivism, relativism, hermeneutics, and historicism and in so doing to develop a sociology that could combine objectivity and subjectivity, scientific enquiry and hermeneutics.²¹ His works of historical sociology and economic history, however, also reveal a strong influence from Marx although it is perhaps going too far to suggest that Weber employed historical materialism in those works. His emphasis on western rationalism, the cultural imperatives and constraints on action, the role of entrepreneurship in economic development, and the growing bureaucratisation of capitalist/modern society,²² are all themes that have reverberated and recurred in the burgeoning and spreading neo-Weberian and hermeneutical social science literature of the 20th century. As with all seminal thinkers, especially also Marx and Durkheim, who found new ways of thinking, Weber's work has been interpreted and appropriated in

²⁰ For a discussion of the functionalist tradition in socio-historical theory and research see W.E. Moore, 'Functionalism' (1979).

²¹ See bibliography for Weber's main writings. There are excellent general discussions of Weber in R. Collins, *Weberian Sociological Theory* (1986); S. Whimster and S. Lash (eds), *Max Weber, Rationality and Modernity* (1987); and D. Käsler, *Max Weber: An Introduction to his Life and Work* (1988).

²² Weber's historical methodology and theory are discussed in G. Roth and W. Schluchter, *Max Weber's Vision of History* (1979).

widely varying ways. Some of the recent works of historical sociology influenced by his ideas, such as by Benjamin Nelson, Ernest Gellner, Clifford Geertz, Albert Hirschman, and Charles Tilly,²³ have been amongst the foremost products of 20th century social science.

The French and German traditions of functionalism, structuralism, Marxism, historicism, phenomenology, and hermeneutics all saw a central role for grand theory that conceptualised the sweep of historical change and formed part of the framework of rationalist and idealist explanations. In contrast, in Britain and some other parts of the Anglophone world the emphasis was on supposedly theory-neutral (or theory-devoid) observation and fact gathering. This empirical (or more correctly labelled "naive empiricist") method ran across the ideological spectrum from left to right and across disciplines from macro historical or evolutionary sociology to "old" economic history to "old" social history and labour history and to neo-classical economics.

The "old" versions of Anglophone economic, labour, and social history of the early 20th century were closely interrelated in their empirical and non-theoretical methodology and in their opposition to the abstractions of neo-classical economics. A major split had occurred between theoretical and empirical social enquiry in Britain and some other Anglophone areas in the late 19th century that was not reflected in the other linguistic traditions. However, in America the situation was more complicated because there were influences from Germany and France as well as Britain. Institutional economics had gained a large following, as had empirical sociology and functionalist sociology. In the 1950s three significant new approaches to structural history were developed there. Cliometrics, or "new" economic history, attempted to unite a historical approach with the latest neo-classical economic theory to explain long-run change in series of economic data. This was a form of applied economics or historical economics that was not really interested in the complexities of the past *per se* but in devising empirical tests for modern theory.²⁴ Secondly, the new institutional economic history has attempted to apply neo-classical theory to explain the long-run history of real economic systems conceived in greater socio-political complexity.²⁵ And, thirdly, the functionalist-modernization school has tried to

²³ See the Bibliography for relevant works of these writers.

²⁴ For recent discussions of cliometrics see D. McCloskey, 'The Achievements of the Cliometric School' (1978), and *Econometric History* (1987).

²⁵ On the new institutional economic history see G. Hodgson, *Economics and Institutions: A Manifesto for a Modern Institutional Economics* (1988), and 'Institutional Economic Theory: The Old Versus The New' (1989); D.C. North, *Institutions, Institutional Change, and Economic Performance* (1990); A.J. Field, 'The Problem With Neo-Classical Institutional Economics: A Critique With Special Reference to the North/Thomas Model of Pre-1500 Europe', (1981); and K. Basu, E. Jones, and E.

employ Parsonian social theory to study the problems of industrialisation and modernization in the postwar world.²⁶

Meanwhile in France the most powerful and influential school of structural historians developed around the journal *Annales* and later at the *Ecole des Hautes Etudes en Sciences Sociales*.²⁷ The *Annales* combined a structuralist methodology, sophisticated forms of social theory, and rigorous empirical/statistical methods to examine the long-run history of macro socio-economic structures and cycles. The major rival to the *Annales* approach in France was the revival of Marxism in the 60s, which was actually a worldwide phenomenon. Marxism was also a major influence in the burgeoning development of the new social history in Britain and America. The only significant form of Marxist thought in Britain in the postwar years was in history writing where the Marxist history school was partly a continuation but with an explicitly theoretical orientation of a radical historiographical and political tradition stretching back to the 17th century.²⁸

In the 1970s social history became an amorphous all-encompassing movement in Anglophone historiography engulfing areas of cultural, economic, and political history writing. "Social history" came to mean almost all things to all people. Under this rubric were various individualist, historicist, and structuralist methodologies. More recently, "historical sociology" has also encompassed the same range of methodologies. In the next chapter I shall examine these developments. The 1980s has seen a breakdown of disciplinary boundaries so that the old history/theory division in Anglophone countries has begun to be seen as the absurdity it always was. One of the most significant results of the re-theorisation of history is the development of historical anthropology or the history of mentalities and cultures. We are also now seeing the disappearance of the old schools of structural history writing derived from positivist, functionalist, Marxist, and Weberian

Schlicht, 'The Growth and Decay of Custom: The Role of the New Institutional Economics in Economic History' (1987).

²⁶ On the modernization school see B.F. Hoselitz, *Sociological Aspects of Economic Growth* (1960); B.F. Hoselitz and W.E. Moore (eds), *Industrialization and Society* (1963); and M. Nash (ed), *Essays on Economic Development and Cultural Change* (1977).

²⁷ The development and significance of the *Annales* school is discussed by, amongst a vast literature, F. Braudel, 'Personal Testimony' (1972); T. Stoianovich, *French Historical Method: The Annales Paradigm* (1976); L. Hunt, 'French History in the Last Twenty Years: The Rise and Fall of the Annales Paradigm' (1986); and P. Burke, *The French Historical Revolution, The Annales School, 1929-89* (1990).

²⁸ On the British Marxist school see H. Kaye, *The British Marxist Historians* (1984), and E.J. Hobsbawm, 'The Historians' Group of the Communist Party' (1978).

antecedents early in this century or from the 19th. Various new syntheses are being proposed, most notably the "structurationist paradigm" and the new cultural history, as I shall discuss in the following chapter.

II TOWARD A SCIENCE OF SOCIAL STRUCTURAL HISTORY

Many of the early writers of structural history, including Saint-Simon, Comte, Marx, Engels, Spencer, Durkheim, Sombart, and Braudel believed, and many contemporary writers, such as Fogel, Wallerstein, and Gellner believe today, that they were and are establishing or contributing to the development of a *science* of history. But understandings of what that means in abstract and entails in practice are remarkably various and often poorly analysed. One thing seems to be constant from the long history of discussions about and attempts to establish a science of society and history and that is the long shadow cast by the sciences of nature. Whether to live within the shadow or escape into a non-scientific meadow of luxuriant relativism has seemed to be the dichotomous (and too restrictive) choice for historical explanation.

The paradigm of explanation is indeed that of the mature sciences of nature. It has long seemed clear that these sciences have great power to *explain* rather than just describe the phenomena of the natural world. In the 20th century there have been vast strides in the success of the sciences as measured by the power of their explanatory knowledge to permit technology and engineering in natural contexts. The complexities of natural structure have been revealed to be opaque to common sense yet available to systematic rational enquiry and deliberate control through interventions. Those complexities, while often unpredictable in advance of discovery and therefore surprising, have been found everywhere to contain real, lawfully structured systems of elements, powers, and relations that can be expressed mathematically.

Of course the general picture of success must be tempered with, firstly, the knowledge that there have been many false turnings in the evolution of the sciences; secondly, an admission that there remain many areas of ignorance and falsity; thirdly, a realisation that there can be no warrant for believing in an approaching dénouement of scientific knowledge; and fourthly, a recognition of the many unintended, sometimes disastrous consequences of physical and biological engineering.

Looking back on the advances made by science in the past couple of centuries, philosophers and historians have attempted to develop an understanding of how it has

been possible for science to be so successful if, as Dudley Shapere put it in the quotation on p.vi above, "so many of our contemporary scientific beliefs could not have been anticipated by common sense, the suggestions of everyday experience, or pure reason". Appeals to some set of ahistorical metascientific rules of discovery will not answer this question because scientific knowledge has always been contingent and the rules have undergone occasional upheavals. I argue herein that the reasons for success lie in a combination of:

- (a) the implicit use of the idea of critical scientific realism, as recently articulated and developed by Richard Boyd, Clifford Hooker, Roy Bhaskar, Brian Ellis, Wesley Salmon,²⁹ and others, which argues *a posteriori* that discovery of the structures of nature and consequent successful interventions in nature could not have been possible without the *prior* mind-independent reality of the law-governed structures of nature. In other words, this is a transcendental argument from the obvious persistent success of science and engineering to a claim about what the world must be like in general terms for that success to have occurred.
- (b) the principle of "scientific internalization" to which Shapere appeals (see quotation on p.vi);
- (c) the rational construction of an interconnecting and reflexive network of reasoning that links theories, hypotheses about causal mechanisms and causal relations, and rigorous testing of those hypotheses through the use of models, analogies, experiments, observations and other data collections;
- (d) the three foregoing factors having allowed the development of theories that more or less accurately analyse the world into its actual natural kinds and the real causal relations that inhere in systems linking natural kinds. That is, scientific theories have made discoveries possible and earlier discoveries, using proto-theories, have made the refinement and development of theories possible. The mutually reinforcing relationship between theories and discoveries has been cemented by successful engineering in the laboratory and in nature.

These four features of science do not together amount to a definite and unrevisable model of "scientific method" which if followed can furnish us with positive knowledge. Our accumulated knowledge of entities, structures, and systems, which does not have to pretend to any absolute or even provisional finality or unrevisability, is the result, rather, of a complex and adaptable approach, rather than a fixed "methodology" in the usual sense of that word, that links ontological beliefs, general theories, linguistic devices,

²⁹ See the bibliography for the relevant writings of Boyd, Hooker, Bhaskar, Ellis and Salmon.

theorising about causes, constant testing, and revision of knowledge. (These ideas about the methodology and success of science will be discussed in greater detail in Chapter Four.)

The old problem of the scientific character or otherwise of enquiries into the history of economic and social structures is still a central issue for many practitioners and methodologists.³⁰ Unfortunately, progress toward delineating let alone resolving the problem has been bedevilled by an inadequate conception of scientific methodology and of the actual methodology of many social structural historians. Both the proponents and opponents of scientific history have usually adopted a simplistic empiricist-quantitative conception of science, which neither does justice to science nor allows the possibility of a truly scientific social history. No philosophical or historical student of scientific method seriously defends the conception of science that was advanced by the vulgar empiricist defenders of cliometrics and positivist economics. The historical, social, and psychological sciences have long been sidetracked by over-zealous scientific but ill-informed advocates of quantification and statistical techniques, as if they were talismans, or of behaviourism, or absolute objectivism, who lack a wider and deeper framework of concepts of a truly scientific nature in which to locate their superficial empiricism. It is no wonder that traditional historians, whether they deal with politics, culture, economy, or society, have felt unattracted by the "scientific" approaches being offered to them. Meanwhile, some historians of social structures, as well as other social scientists, have been developing genuinely scientific approaches to their explanatory tasks, usually without being very conscious of the scientificity of their methodology.

My (somewhat old-fashioned but still essential) tasks, then, in this regard, are to give a better account of scientific reasoning (including social science), to discuss the reasons for its superiority to other forms of explanation, to argue that structural history should endeavour to become scientific, and to show how structural history enquiry can be and sometimes is scientific. In Chapters Three and Four I will try to show in detail how scientific realism overcomes the problems of both empiricism and hermeneutical relativism and in so doing supports the methodology that I call "structurism" for structural history. Scientific social structural history, I shall argue, must weaken the claim of

³⁰ For recent discussions of the question of the scientificity of economic and social history see J.M. Kousser, 'Quantitative Social-Scientific History' (1980); J.M. Clubb, 'The New Quantitative History: Social Science or Old Wine in New Bottles?' (1980); A. Rutten, 'But It Will Never Be Science, Either' (1980); R.W. Fogel, 'The Limits of Quantitative Methods in History' (1975); R.W. Fogel and G.R. Elton, *Which Road to the Past? Two Views of History* (1983); S.P. Hays, 'Scientific Versus Traditional History' (1984); E. I. Monkkonen, 'The Challenge of Quantitative History' (1984); I. Winchester, 'History, Scientific History, and Physics' (1984); and L.E. Davis, 'Cliometrics: The State of the Science (Or is it Art or, perhaps, Witchcraft?)' (1987).

absolute objectivity that positivists have made while rejecting the relativist, pragmatic, and common sense modes of reasoning that hitherto have been the main alternatives. But hermeneutical interpretation must play a role within a scientific framework. The dichotomy between interpretive and scientific history was always false. The concept of truth must be retained but in a weaker and more complex form than a simple correspondence idea and in a complex realist rather than a simple coherence version.

III INTERRELATIONSHIPS OF PHILOSOPHY, METHODOLOGY, AND THEORY IN SCIENTIFIC EXPLANATION

In order to see, then, if it is possible for a science of social structural history to be developed which has at least a family resemblance to the sciences of nature we must know something of the character of scientific explanation as it exists. Essentially this means examining the relationship between metaphysics, methodology, and general theory in advanced science. Here I shall raise the main aspects of the problem that have to be considered, leaving the detail of the argument to Chapter Four.

Philosophy and Methodology

To explain something essentially involves giving an account of why it happened and/or why it is the way it is in terms of its supposed causal relations with other things. All explanations employ, whether deliberately or passively, general concepts and general theories, which are used to produce causal hypotheses, interpretations of evidence, and causal understandings of particular kinds of phenomena and processes. It is now generally well understood that these general concepts and general theories are parts of background frameworks or traditions of beliefs, ideas, knowledge, and assumptions that all explanations employ. These frameworks include philosophical and methodological assumptions, which are sets of ideas and beliefs about the entities and processes of the world and of how we can have knowledge of them.

The framework concepts and general theories that the advanced sciences employ pertain to what have been called "domains" of knowledge by some philosophers of science, most notably by Dudley Shapere.³¹ These domains, such as particle physics, astrophysics, organic chemistry, plate tectonics, genetics, virology, neurophysiology, insect population dynamics, and so on, are bodies of subject matter that have become delineated by the way in which the entities, powers, and systems of the world have been theorised and

³¹ For Shapere's work see the list in the Bibliography.

discovered to be naturally delineated and interrelated. Domains of science are the result of the history of scientific methodology, theory, and discovery over many centuries and even at a mature stage are in a process of constant piecemeal refinement and very occasional wholesale reordering and unification. There seems to be a strong long-run tendency to unify domains of subject matter so that general theories become increasingly comprehensive. Indeed, it seems that part of the essential character of scientific explanation is to reduce the number of domains and theories to fewer and more comprehensive ones. This is a main reason why there are occasional revolutions in thought.

The processes of constitution and unification of domains and the role that domains play in explanation are of vital importance to philosophers and historians of explanation and, as I shall presently argue, should also be to all those practitioners of a science or proto-science, such as social structural history, who wish to improve their explanations. (The character of domains will be discussed in more detail in Section V of this chapter.)

The historical process of inchoate enquiry whereby scientific domains came to be established in a piecemeal and unpredicted fashion must be distinguished from the process of reasoning that, *post facto*, reconstructs the logical connections between the assumptions and abstractions of domain frameworks and the ongoing empirical research that takes place within them. The process of discovery and of domain constitution is not the same as the logic of rational analytical reconstruction, formalisation, and criticism of scientific explanation. While here I am primarily concerned with making a rational reconstruction rather than with analysing the process of discovery I try not to lose sight of the historical and epistemological primacy of the latter process. Sciences are not established by deductive analytical reasoning alone but by practitioners gradually *discovering* how the world really is and what the appropriate methodologies and general theories are for making discoveries. However, scientists can only do that within a prior proto-scientific framework of an ill-defined and perhaps somewhat incoherent sort which provides them with some general methodological ideas and some concepts even if these are later refined and/or abandoned in the light of the discoveries they helped to establish. Observation, understanding, and discovery never occur except within some sort of intellectual framework. The difference between scientific and non-scientific understandings is not the development of a framework out of intellectual anarchy, as Kuhn seemed to think. On the contrary, science is but one among many kinds of coherent world view but it is vitally different from all the others in its explanatory power and ability to examine critically all other frameworks as well as reflexively examine itself. In this reflexive way the sciences

emerged from the pre-scientific world of early modern Europe.³² But when proto-scientific enquiry had reached and reaches a degree of refinement and explanatory power there then is possible a vital role for methodological analysis and criticism that is able to help sort the valuable from the valueless aspects of proto-frameworks and show sceptics that a new science is being born.

Thus analysis and criticism of proto-sciences that draws upon knowledge of the reasoning of the mature sciences only really became possible after the mature sciences had reached a certain degree of power in the 19th and early 20th centuries. Nevertheless, the wish to apply "scientific method" to all kinds of enquiry goes back to the early Enlightenment, as shown by Vico's desire to apply Bacon's prescriptions and Hume's belief that he was employing Newtonian principles to reconstruct the foundations of human understanding. The French Enlightenment of the second half of the 18th Century witnessed many attempts to establish sciences of the mind and human nature on supposed Newtonian mechanistic principles.³³ From the 1940s under the initial impetus of logical positivism there began an explosion of philosophy of science literature³⁴ analysing both mature and immature sciences. For critics of the social enquiries this literature has been appropriated, often piecemeal, often with a view to making their disciplines more scientific.³⁵ Thus the methodological critic of the immature or proto-science could be and was seen as the midwife of new sciences, drawing upon the reanalysed, reconstructed, and articulated understandings of existing sciences.³⁶ This seems to me to be a basically correct strategy

³² There is an excellent discussion of the relationship of science to non-science in E. Gellner, *Legitimation of Belief* (1974).

³³ There is a fascinating discussion of French developments in R. Darnton, *Mesmerism and the End of the Enlightenment in France* (1968), Ch 1.

³⁴ See the penetrating and comprehensive analysis of this literature in W. Salmon, 'Four Decades of Scientific Explanation' (1989).

³⁵ Literature in this vein includes M. Friedman, *Essays in Positive Economics* (1953); B.J. Caldwell, *Beyond Positivism: Economic Methodology in the Twentieth Century* (1982); D. Harvey, *Explanation in Geography* (1969); P.J. Watson, S.A. LeBlanc, and C.L. Redman, *Archaeological Explanation* (1984); M.H. Salmon, *Philosophy and Archaeology* (1982); R. Harré and P.F. Secord, *The Explanation of Social Behaviour* (1972); M. Harris, *Cultural Materialism: The Struggle for a Science of Culture* (1979); P.T. Manicas, 'Implications for Psychology of the New Philosophy of Science' (1983); W.L. Wallace, *Principles of Scientific Sociology* (1983); C. Lloyd, *Explanation in Social History* (1986); and the references to scientific history in note 30 above.

³⁶ The task and value of methodological enquiry has been hotly debated recently by some economists. See for example A.W. Coats, 'Explanations in History and Economics' (1989) and 'Disciplinary Self-Examination, Departments, and Research Traditions in Economic History' (1990); E.R. Weintraub, 'Methodology Doesn't Matter But the History of Thought Might' (1989); B.J. Caldwell, 'Does Methodology Matter? How Should it Be Practiced?' (1990); D.W. Hands, 'Thirteen

provided, crucially, that two important points are understood -- that the sciences are not uniform (that is, that each science takes its form and methodology from its subject matter rather than the reverse; a realist commitment, to be defended in a moment), and that we have adequate accounts of the structures of reasoning of the advanced sciences. This last point is, of course, a major problem -- how to develop adequate accounts of science.

Methodological enquiry into a science that has already reached maturity (assessed in terms of the coherence, completeness, theoretical fruitfulness, and explanatory power of its domain framework) has a role somewhat similar but different from that of the mid-wife of a new science. This role is the analysis and criticism of the reasoning of everyday theoretical and empirical enquiries -- a post-natal function, as it were. Here scientific framework building is no longer occurring except at rare times of framework crisis. The history of science can be read as indicating that such crises can be prompted by philosophical discussions of the nature of the entities, systems, and processes being examined and/or by methodological analyses of the reasoning of everyday enquiries. Such crises are usually resolved by unifying previously separate domains through the development of new methodologies and more encompassing general theories.

It is a central thrust of the Wittgensteinian, Neo-Nietzschean, post-structuralist, and post-modernist critiques of philosophy to reject all foundational and/or metacritical roles of philosophy and general theory. Philosophers such as Richard Rorty and Jacques Derrida, theorists such as Michel Foucault, Stanley Fish, and Jean-François Lyotard, and methodologists such as Donald McCloskey and Richard Harvey Brown³⁷ have all advanced the general idea that the role of methodological criticism is to articulate the pragmatic, linguistic/rhetorical devices of *persuasion* and the sociological/psychological/political *contexts* of discourses rather than to establish meta rules of assessment of the validity of theoretical and factual statements. Overarching metacriticism that draws upon some supposedly "privileged" level of presuppositions, concepts, and knowledge is held to be unsupportable because there can be no real foundation for the supposed foundation. All knowledge is constituted in and by forms of discourse or language. The role of the historian is then taken to be to analyse texts (or text-analogues) for their place in discourses and for what they reveal about the internal power structures of discourses rather

Theses on Progress in Economic Methodology' (1990); U. Mäki, 'Methodology of Economics: Complaints and Guidelines' (1990).

³⁷ See the Bibliography for relevant works of these authors.

than for what they tell us about any supposed objectively real structures of societies and history. "Historical reality" only has meaning from a standpoint within a discourse.³⁸

While relativists are right to reject untestable metaphysical and methodological assumptions, to emphasise persuasion, and to draw attention to the socially and linguistically relative contexts of truth concepts, they do seem to ignore the possibility of *reflexive* examination of all foundational metaphysical and epistemological propositions and hence the adoption or affirmation of certain *explanatory foundations*, such as those of scientific realism, only after rational enquiry and testing. Having stable philosophical and methodological foundations for explanation seems to be necessary but it does not mean that they are unrevisable or privileged. Indeed, in the mature sciences foundations have changed because of the scientific process itself. Their survival depends mainly on their long-run explanatory usefulness. Explanatory usefulness, in turn, is founded on the twin notions of *discoverable* order and the power of science to *intervene* in the order in predictable ways.

The fundamental problem with relativism is that it is unintentionally dishonest in the sense that, disclaimers notwithstanding, it does contain its own disguised assumptions or tacit commitments to "privileged" sets of notions or concepts which are in fact not "problematised". For example, the deep structures of grammar, semantics, and logic are not usually questioned and the relationship that the universal structures of natural languages and natural logic have within pre-theoretical and pre-scientific forms of reasoning are not questioned. The continuous rather than disjointed relationship of pre-theoretical and pre-scientific reasoning and understanding with modern theory and science has been well established.³⁹ In other words, there seem to be some universals of thought, or at least the relativists have not shown to the contrary.⁴⁰ If there are universal elements of human thought and language and these are a natural outgrowth of human life and thought in their interaction with the environment then there are limits to the kinds of social structure that can be developed and to how and what it is possible to think. Those possibilities and

³⁸ For discussions of post-structuralist concepts of history as discourse see D. Attridge, G. Bennington, and R. Young (eds), *Post-Structuralism and History* (1987); and H.A. Veveser (ed), *The New Historicism* (1989).

³⁹ cf M. Mandelbaum, *Philosophy, Science, and Sense Perception* (1964); W. Sellars, *Science, Perception, and Reality* (1963), and *Science and Metaphysics* (1968).

⁴⁰ cf E. Gellner, *Legitimation of Belief* (1974), including the important discussion of the significance of Chomsky's structural linguistics in Ch 5, and *Relativism and the Social Sciences* (1985), Ch.3.

limits are governed by the nature of the universe. Thought is parasitic upon nature in a general sense but is nevertheless very powerful in its capacity to conceptualise and construct theoretical patterns that supposedly describe and analyse the universe. Science itself has shown that the nature of the universe is the ultimate foundation of and reference for thought. Imagination is not unconstrained. The relativists have lost sight of this in their excited but naive "discovery" of the great power of language and imagination to mould our beliefs, understandings, and relationships. But that power is not unlimited. Language cannot mould physical and biological nature. How thought and its particular theoretical and conceptual products relate to the world is the age-old epistemological issue. The idea of thought or philosophy as edification and rhetoric is attractive but ultimately cannot advance the human quest for improving the quality of personal and social life. That requires *knowledge* of the real natural and social contexts of action and consciousness. It is knowledge that is ultimately persuasive because it allows scientific *interventions* from non-scientific motives.⁴¹

The romantic/hermeneutical/relativist stream of meta-thought about the social and human enquiries vigorously rejects the idea that they can and will become like the sciences of nature. This of course presupposes a certain conception of science as containing a fixed epistemological relation between external object and human enquirer and a conception of human action, consciousness, and social relations such that they supposedly cannot be studied in the same way as nature. However, the sciences do not have to have identical methodologies. The search for the ahistorical, correct methodology for all science has proven not only illusory but dangerous. Each science has pursued its own path toward maturity. Nevertheless while each science has its peculiarities each can be construed as scientific in a fundamental sense that I will try to articulate in Chapter Four.

The *a priori* rejection of a scientific approach to social structural history can be countered at first by showing that in fact the social sciences are indeed following their various peculiar paths of progress toward domain construction. If they are to be taken seriously in their *explanatory* aims, insofar as they have them, then they must do so. Adequate explanation and its improvement seem to require domain coherence and appropriateness of concepts to their object, as I shall further argue in a moment. In the social sciences the highly contentious process of *initial* establishment of domains of knowledge and their methodologies is still going on. This is the latest stage in a three-

⁴¹ cf I. Hacking, *Representing and Intervening* (1983). For a liberal ironist counter view to the idea of structural universals and social interventions see R. Rorty, *Contingency, Irony, and Solidarity* (1989).

centuries-long process. The construction and acceptance of social science domains has not yet reached the stage where there is a consensus on defining concepts, methodologies, and general theories.

This dissertation is partly about how we might better constitute methodologically and theoretically the putative domain of social structural history and about how its foundations, or framework of methodology and concepts, may be defined and strengthened. We should not consider this domain of the history of social structures to be different from one that would study such structures as if they were static or existing only in the present. Social structures are *inherently historical* in the dual and paradoxical senses of temporally continuous and constantly changing. Therefore the abstract sociological and economic concepts that structural history employs should have an inherently historical and dynamic reference. From this point of view, one of the major defects of modern mainstream economics is its bracketing of dynamics in favour of building abstract models which then have to have dynamic variables somehow grafted onto them in order to study history. Model building is not itself a problem -- in fact abstractions are necessary to explanation; it is the ahistorical assumptions behind and static concepts of many models that must be criticised.

The view that the very *historicity* of society rules out scientific knowledge because such knowledge can be only of space-time invariant structures can be disposed of quickly. Because nature is ruled by universal, atemporal laws but society is not, goes the argument, there is a fundamental difference in the ontology such that the discovery of "laws" of social structural composition and history is impossible.⁴² But the counter to this is threefold. First, society is not the only historical system. The entire universe, including the planets, the geology of the earth, the biosphere, and the evolution of life forms are all historical in the sense of having changing processes. Second, science is not necessarily characterised by the aim of discovering laws. More basic characteristics are the form of rationality, the structure of reasoning, reflexivity and self-criticism, and empirical objectivity. There seems no *prius facie* reason why the study of the history of social structures cannot employ a form of rationality and a system of reasoning that resembles those of the sciences of nature. Third, it may turn out that the history of social structures is in fact law-governed in a way similar to the history of the geomorphology of the earth, for

⁴² I. Berlin argued in a famous article, 'The Concept of Scientific History' (1960), that history could not be a science. This idea goes back to Vico, who defended a distinction between natural science and human enquiry, and was later developed by the Neo-Kantians such as Dilthey, Windelband, and Rickert.

example, or perhaps, more plausibly, of a complex ecosystem. Shapere's Principle of the Rejection of Anticipations of Nature (see quotation on p. vi, and section IV below for further discussion) should be applied to society too. The structure of society and the mechanisms of its history are not immediately available to common sense perception and understanding. The idea that because we are "internal" to society we can intuitively grasp its structure is no more plausible than the idea that because we are "internal" to our own bodies or our own psyches we can intuitively understand them. Nevertheless, it seems clear from the history of the sciences that there is a chain of reasoning, with certain jumps in the sequence perhaps, that links common sense with scientific knowledge. But the natural sciences have moved a long way from common sense. Knowledge of social structural history has not moved as far yet. It seems that the problem lies partly in the inappropriate methodologies being employed by many practitioners. (See Chapter Two below for a discussion of existing methodologies.)

In a work of analysis of the methodologies of particular branches or domains of sciences and other intellectual fields the *first task* is to ascertain *how* those branches, domains, and fields describe and explain what they wish to describe and explain. That is, what is the framework of philosophical and methodological assumptions in which descriptions and explanations are made and what are the connections between framework, theories, and explanations? In fact, philosophical and methodological assumptions and concepts are both necessary to empirical enquiry and always present in one form or another. This is not to say that such assumptions are always easily recognisable or separate from empirical thought. But they always form a sometimes implicit, sometimes explicit level of general beliefs or vague ideas, which may or may not be expressed as concepts, about how the empirical discipline should proceed. They are closely tied to empirical enquiry but it is possible to separate them analytically. Separation and clarification of frameworks is the first task of analytical philosophical enquiry, upon which the second task depends.

The *second task* is to develop *criticisms* of the coherence, strength, adequacy, and plausibility of the methodologies and explanations, with a view to offering constructive assistance to the on-going process of empirical research. Not all the empirical researchers in a putative domain are at the same methodological level or employing the same methodology. Rational philosophical and methodological enquiry, then, is (or should be) the ally of empirical enquiry, especially in immature sciences.

In its heyday in the 1960s analytical philosophy of history was not much concerned with criticism but just with trying to analyse the writing of history. The task was seen as a more or less passive examination and articulation of the existing explanatory assumptions and practices of historians -- the counterpart to analytical philosophy of science. Many of these philosophers were concerned to show that historical knowledge is a distinctive and viable form of knowing with its own logic, standards, and rationale.⁴³ Since then this attitude has been eroded for several reasons. One is a growing realisation by many historians that they must become interdisciplinary and combine with the social and psychological sciences in various ways in order to construct explanations. Another is the crisis that developed in the 1960s in the philosophy of science that prompted much uncertainty about the nature of scientific and other forms of knowledge. Historians have to some extent been affected by these debates. However, that explicit philosophical and methodological criticism can be of assistance to empirical research is still not a popular idea among historians. Such criticism is much more popular these days among sociologists and probably is gaining popularity from a low level among economists. One of the determinants of the resort to methodological criticism is the degree of internal coherence of empirical disciplines such as economics or history in terms of shared beliefs about rationale, goals, and procedures. When this coherence is not present -- sometimes because of perceived failures in explanation or the creation of new approaches -- then the resort to philosophy and methodology for assistance becomes more acceptable. That is, questions about the methodological and philosophical assumptions of the discipline come to be raised by empirical practitioners because of perceived problems of empirical explanation. This is the main reason why there have been debates such as those between Lawrence Stone, Eric Hobsbawm, and Philip Abrams in *Past and Present* in 1979-80, and many other recent contributions to methodological discussions about economic and social history and historical sociology.⁴⁴

⁴³ Examples of analytical philosophy of history that defends the distinctiveness of historical knowledge include W.H. Dray, *Law and Explanation in History* (1957); W.B. Gallie, *Philosophy and the Historical Understanding* (1964); J. Barzun, *Clio and the Doctors* (1974); L.O. Mink, *The Autonomy of Historical Understanding* (1965), 'The Divergence of History and Sociology in Recent Philosophy of History' (1973) and 'Philosophy and Theory of History' (1979).

⁴⁴ L. Stone, 'The Revival of Narrative: Reflections on a New Old History' (1979); E. J. Hobsbawm, 'The Revival of Narrative: Some Comments' (1980); P. Abrams, 'History, Sociology, Historical Sociology' (1980).

There have been a lot of ostensibly methodological discussions about social history and historical sociology lately but unfortunately many of them do not really raise epistemological issues. For some of the more penetrating contributions see P. Abrams, *Ibid.*; G. Eley, 'Some Recent Tendencies in Social History' (1979); E. Fox-Genovese and E. D. Genovese, 'The Political Crisis of Social History: A Marxian Perspective' (1976); J. A. Henretta, 'Social History as Lived and Written' (1979); E. J. Hobsbawm, 'From Social History to the History of Society' (1971); R. S. Neale, 'Introduction - Social History' in *Class in English History* (1981); A. L. Stinchcombe, *Theoretical Methods in Social History* (1978); L. Stone, 'History and the Social Sciences in the Twentieth

The Nature of Theory

The separation of philosophical/methodological questions about explanation from substantive theoretical ones is important. The failure to do so is common and misleading. Lawrence Stone's *Past and Present* article of 1979 shows that he did not grasp the difference between "structuralism", which is part of a methodology, and "historical materialism", which is a theory of structural change. Problems of constructing particular theories and applying them arise *within* fields of enquiry that have a distinct and relatively coherent philosophical and methodological framework that delineates a distinct subject matter. Frameworks contain metaphysical beliefs (often well founded), general concepts about the nature of the objects of enquiry within the domain, general methodological principles, and a collection of linguistic explanatory tools, such as metaphors, analogies, similes, and source models. Scientific theories employ these beliefs, principles, tools, and models to construct putative causal explanations of types of phenomena and processes so that particular phenomena and processes can be explained.⁴⁵ However, this is only one of several understandings of theory prevalent in the social and human studies.

Century' (1976); C. Tilly, 'Two Callings of Social History' (1980); C. Tilly 'The Old New Social History and the New Old Social History' (1984); R. Vann, 'The Rhetoric of Social History' (1976).

Social history as history from the bottom up, or the history of lower class or ordinary people's lives, has been advocated in a series of articles by P. Stearns. See for example, 'Some Comments on Social History' (1967), 'Coming of Age' (1976), 'Toward a Wider Vision: Trends in Social History' (1980), 'The New Social History: An Overview' (1983), 'Social History and History: A Progress Report' (1985). See also the collection of articles in honour of George Rudé, in F. Krantz (ed), *History From Below* (1988).

The problem of the relationship of history and sociology to each other has been much discussed lately. Some general discussions, all of which argue for unification, are P. Abrams, *Historical Sociology* (1982); P. Burke, *Sociology and History* (1980); N. Elias, 'Introduction - Sociology and History' in *The Court Society* (1983); E. J. Hobsbawm, 'From Social History to the History of Society' (1971); G. S. Jones, 'From Historical Sociology to Theoretic History' (1976); D. Smith, 'Social History and Sociology - More than Just Good Friends' (1982); C. Tilly, 'Historical Sociology' (1980), *As Sociology Meets History* (1981), *Big Structures, Large Processes, Huge Comparisons* (1984), 'Retrieving European Lives' (1985), and 'Future History' (1988/89).

⁴⁵ The standard idea of scientific theories as advanced by, for example, Karl Popper sees them as making existential claims for empirical testing although there is a good deal of dispute about the logic and significance of theory generation and confirmation. See Popper, *The Logic of Scientific Discovery* (1972). For some recent valuable discussions see F. Suppe, 'The Search for Philosophic Understanding of Scientific Theories' and 'Afterword' (1977); W.C. Salmon, *Scientific Explanation and the Causal Structure of the World* (1984) and 'Four Decades of Scientific Explanation' (1989); P. Kitcher, 'Explanatory Unification and the Causal Structure of the World' (1989); R. Boyd, 'Realism, Approximate Truth, and Method' (1990); C. Howson, 'Fitting Your Theory to the Facts: Probably Not Such a Bad Idea After All' (1990).

A second understanding sees theory as the process of concept formation through which phenomena and processes are brought under taxonomic and explanatory classifications and other linguistic devices such as analogies and metaphors. A third and related understanding sees theory as model-building whereby ideal types, analogous descriptions, and sets of mathematical equations are devised for manipulating data and solving problems of interrelationships between sets of observations. These two understandings can be seen as subsidiary to the first idea of scientific theory mentioned in the previous paragraph, provided theory is seen in a realist rather than an instrumentalist sense.⁴⁶ In the classical period of social science, epitomised by the work of Marx, Durkheim, Weber, and Simmel, the task of social theory was seen as providing explanations of social relations, partly with a view to providing a basis for political action.

A fourth and quite different understanding of social theory has seen it as, in effect, imaginative, linguistic, rhetorical constructions of possible entities, episodes, or scenarios which are sometimes supposed to cast light upon a shadowy true reality that is never defined or directly studied. One form of this idea considers society to be a text that requires interpretation from some point of view but there is no one truthful interpretation. The difference between such a hermeneutical and phenomenological theory and imaginative artistic production seems minimal, as indeed some defenders of this approach have recognised.⁴⁷

Some practitioners of so-called "social theory" who engage solely in textual criticism produce what Pierre Bourdieu has called "theoretical theory" and contrasted with "scientific theory". The latter, he said,

takes shape for and by empirical work and gains less by theoretical polemics than by confrontation with new objects. Consequently, to truly side with science means making a choice, a rather ascetic one, to devote more time and effort to the exercise of theoretical findings by applying them to new research projects rather than preparing them somehow for sale by dressing them in the trappings of a meta-discourse -- destined less to verifying the thought than to publicizing its importance and value or to making its benefits immediately apparent by circulating it in the innumerable events that the jet age and the age of conferences provide for the narcissistic researcher....

⁴⁶ For defences of the scientific realist notion of social theory see R. Bhaskar, *The Possibility of Naturalism* (1979), and *Philosophy and the Idea of Freedom* (1991).

⁴⁷ For an example of the textual/interpretive idea of theory see the work of R.H. Brown, as listed in the Bibliography.

To treat theory as a *modus operandi* which directs and organizes practically scientific practice, means obviously that one has given up the somewhat fetishistic accommodativeness that "theoreticians" usually establish with it.⁴⁸

Unfortunately, too much of so-called "social theory" is of this "narcissistic" kind, tracing the genealogy of concepts and elaborating their internal coherence but having no empirical reference except to other texts within the "discourse".

Some other forms of "grand theory" (such as Marx's materialist theory of praxis and structural change, Durkheim's functionalist theory of religion, Parsons' functionalist theory of social evolution, Rostow's stages theory of economic growth, Olson's rationalist economic theory of the rise and decline of nations, Bendix's historical sociology of nation building, Bourdieu's theory of habitus and practice, Elias' theory of social figurations, Touraine's sociology of action, Mann's theory of social and state power, McNeill's ecological theory of world history⁴⁹) are constructions of very general concepts and theories that are supposed to apply to all or many social structures and situations. But sometimes they are so general that how they are supposed to explain particular cases is not at all clear. In fact some of these theories are rightly seen as being, rather, models or concepts that are part of an explanatory framework, having very little empirical content or reference. Insofar as they are empirically referred and exemplified, as are those of Marx, Olson, Bendix, Bourdieu, Elias, Touraine, Mann, and McNeill, for example, we can say that they are forms of scientific (or proto-scientific) theory. Some others, such as those of Parsons, Rostow, and Giddens, are closer in conception and value to the imaginative constructions of the "theoretical theorists" of Bourdieu's scorn. Of course it can be argued that the meta-theorising of the Parsons/Giddens kind provides fruitful and powerful methodologies, hypotheses, and concepts for other researchers to employ. Insofar as they do then the theorising is valuable.

Summarising the distinctions I have tried to make in this section, there are:

- (a) *Philosophical problems*, which concern issues about existence and explanation -- i.e. very general ontological and epistemological issues that remain tacit for most of the time and are only analysed by explicitly philosophical enquiry.
- (b) *Methodological problems*, which are more concrete in that they concern the delineation of domains and the actual explanatory practices and forms of reasoning

⁴⁸ P. Bourdieu, 'The Genesis of the Concepts of *Habitus* and of *Field*' (1985), pp. 11-12.

⁴⁹ See the Bibliography for references to the work of these writers.

of particular sciences or disciplines. Methodological issues have a more general currency than philosophical ones, particularly in times of crisis.⁵⁰

- (c) *Scientific Theories*, which are concepts, models, and statements of a *general* kind about the structural mechanisms, powers, and causal relationships between types, kinds, and classes of entities, events, and processes within a domain. Theories are directly used to explain particular events and processes. We should distinguish *general theories*, which attempt to encompass all the main structures, mechanisms, relationships and phenomena in a whole domain (e.g. general relativity, quantum thermodynamics, plate tectonics, Darwinian evolution, neo-classical economic equilibrium theory, historical materialist class theory, Freudian psychoanalysis) from theories of more *particular* events and processes. The latter are formulated employing general theories, concepts, and linguistic devices such as analogies, similes, and models, but are evaluated by bringing them into direct confrontation with empirical evidence, a procedure that may force changes in both the theory and the organisation, character, and meaning of the evidence.

Philosophies, methodologies, and general theories form the framework for formulating particular theories, research methods, and concrete explanations of a domain and are therefore somewhat remote from empirical questions. Whether the framework is explicit or largely tacit depends to some extent on the degree of advancement of the science and the degree of consensus among practitioners. Greater advancement usually means greater consensus, which usually leads to the framework remaining tacit. Methodological contention is sometimes a sign of immaturity. The evolution and survival of the framework is ultimately dependent in a general but indirect sense upon empirical explanatory adequacy and progress.

The bulk of the rest of this chapter is devoted to discussing philosophical and methodological issues involved in explaining structural history. By employing some of the points I shall make and the important distinctions between philosophical, methodological, and theoretical ideas just discussed, I shall go on in Chapter Two to construct a critical survey of the existing approaches to writing social structural enquiry. Some of these approaches are based upon a distinct set of philosophical assumptions and

⁵⁰ Methodological problems should not be confused with questions about research methods that deal with *technical* problems of conducting detailed empirical research, such as sampling techniques, survey design, archival data compilation, econometrical formulae, and so on. These are the counterparts of experimental laboratory and fieldwork techniques in physics, chemistry, and biology. Of course research methods are partly determined by methodological ideas.

methodological concepts but others such as Marxism, behaviourism, functionalism, and neo-classicism, are in fact just general *theories* and so do not or should not have the status of genuinely separate approaches. The philosophical and methodological foundations of approaches have to be articulated before their theories and explanations can be compared. Therefore this dissertation is not about theories as such although I do discuss aspects of the theory of historical materialism in Chapter Five because it has often been considered to provide a scientific approach to structural history. In Chapter Three I shall give a more detailed examination of the work of some examples of a structuralist approach to structural history because I want to argue for its superiority.

IV THE CONCEPT OF SCIENTIFIC DOMAIN

Now, in coming to examine the methodological problems of contemporary structural historiography the problem of the framework of explanation must first be considered. This is because the structure of explanation is not confined to a supposedly fundamental logic of enquiry or a fundamental theory/observation distinction, as many philosophers of explanation, including the logical empiricists in the 1930s-1950s period, long believed. A simplistic version of this belief was very influential in economics and parts of economic history, sociology, and psychology such that some practitioners were led to believe that only a nomological-deductive, reductionist, and abstract model of explanation was permissible for social enquiries that wished to be scientific and therefore genuinely explanatory.⁵¹ The logical empiricists concentrated on articulating what they saw as the universal logic of discovery and/or confirmation in science. They held that science should have no metaphysical presuppositions but there was a debate among them about the instrumental versus realist reference of theories of unobservables. Science, on the supposed model of physics, was thought to be quintessentially logical, objective, reductionist, and separate from other existing forms of so-called "explanation", especially from the relativist and holist hermeneutics that was thought to be the method of humanistic enquiry. But starting with Weber early in the century then Mannheim and Adorno in the 30s, Collingwood in the 40s, Popper, Scriven, Hanson, Feyerabend, Quine and especially Kuhn, in the late 50s and early 60s, there has been ever since a welter of discussion about the intellectual, psychological, institutional, and sociological organisation and interestedness of science and other forms of explanation. The rationality, logic, objectivity,

⁵¹ Classic examples of positivist social science are M. Friedman, *Essays in Positive Economics* (1953); O. Neurath, *Empiricism and Sociology* (1973); B.F. Skinner, *Science and Human Behavior* (1953).

coherence, validity, and separateness of the sciences have all been thrown into doubt by various relativist and realist arguments.

Perhaps the greatest advance in understanding the nature of explanation made in the post-positivist and post-Kuhnian era is the general realisation that methodologies, theories, and explanations are related to each other via extra-logical, historically variable constellations variously described as "background knowledge", "traditions", "paradigms", "research programmes", "fields", or "domains". We can call all of these "framework concepts". The major problem thrown up by this realisation is that of the relationship between frameworks and the objectivity and progress of knowledge. What are the connections between frameworks, research and explanatory activity, observational evidence, and reality? Do and must all the sciences have a similar form of reasoning that connects all these? Do the sciences have a superiority over other forms of explanation and understanding in the ways in which their frameworks relate to evidence and reality? Why do frameworks change? Are the sciences always objectively progressive in their discoveries?

Thomas Kuhn argued that sciences normally proceed in their research projects and activities within "paradigm" theories that provide the presuppositions for determining what is to be investigated, how, and the validity of the results. Paradigms are replaced in "scientific revolutions", not because objectively better ones have been developed but because the old ones are subjectively thought to be adequate no longer. Sciences are demarcated from non-sciences by the existence of consensual paradigms. The rationality of science is relative to the paradigm and so is historically specific rather than universal. Paradigms, he said, are formed and changed as much by social and psychological influences as by cognitive value.⁵²

Karl Popper's rationalist and realist response to Kuhn was to defend the idea of science being a rational and incremental enquiry aimed at making real discoveries through a trial and error process of hypothetico-deductive reasoning and objective empirical testing. The scientificity of a theory was to be judged by its falsifiability, which in turn was to be judged by its empirical power. Theories could be corroborated to a lesser or greater degree, thus allowing progress, but never proven absolutely true. The ideal of

⁵² Kuhn's original, now classical text, probably the most influential work ever written in the philosophy of science, is *The Structure of Scientific Revolutions* (1970 originally 1962). Kuhn has modified his views somewhat in more recent works -- see the Bibliography.

truthful discovery operates, he believes, as a powerful regulator of practice. The value orientation of science is its truth-seeking goal, a goal that it can never ultimately attain.⁵³

Building upon some of Popper's ideas, Imre Lakatos developed the concept of "research programme" to describe the relationship between theories in a science. The research programme is a set of, firstly, methodological rules, background knowledge, and basic laws, none of which is subject to testing, and, secondly, new hypotheses and models, which draw upon the background knowledge and are retained if empirically corroborated. Increasing degrees of corroboration of successive theories show that a programme is progressive.⁵⁴ Both Popper and Lakatos, like the logical empiricists, saw a strict separation between internal (i.e. logical) and external (i.e. sociological) dimensions of scientific activity. Not so, according to certain sociological, post-structuralist, and pragmatist critics of science such as Michel Foucault, Pierre Bourdieu, Paul Feyerabend, Richard Rorty, Barry Barnes, and David Bloor,⁵⁵ all of whom have rejected the idea of science as objective and progressive.

Michel Foucault rejected the notion that there could be progress in epistemology and knowledge and attempted to construct an archaeological analysis of intellectual/cultural systems of orderings of the world -- "discursive formations" -- which were discontinuous one with another. Each discursive formation has its own deep set of rules, concepts, and governing statements that make possible the subsequent development of definitions of objects, concepts, and discoveries. Each formation has to be examined for its complex structure and not from some meta-discursive standpoint. The attempt to write the overall history of discourses should be abandoned in favour of the genealogy of each discourse's concepts and an examination of the power structures within them.⁵⁶

Pierre Bourdieu has analysed science as consisting of a series of "fields" of activity, each of which has a form of interest that is part socio-political and part intellectual. A scientific field is an objective space in which there is play of opposing forces struggling for scientific rewards. Neither a strictly epistemological/logical analysis of science nor an

⁵³ See especially Popper, *Conjectures and Refutations* (1972), Ch. 10, and *Objective Knowledge* (1972), Ch. 3.

⁵⁴ See the Bibliography for the most relevant of Lakatos' writings.

⁵⁵ See the Bibliography for main relevant works of these writers.

⁵⁶ See especially Foucault, *The Archaeology of Knowledge* (1972).

analysis of the social conditions of practice and knowledge can alone sufficiently grasp the content and power of a field. It is the field that assigns the function of the researcher, the choices of topics, methods, and so on. The goal of the scientist, according to Bourdieu, is to achieve recognition from competitor-peers. However, in pursuing this social goal the scientist can also further the progress of the science itself.⁵⁷ There is a two-way relationship between internal and external determinations of scientific progress, which is made possible by the relationship between the scientific field and what he calls the "habitus". The habitus is the principle of a form of subjective, implicit, practical knowledge that is unconscious and not requiring consciousness although it masters objective necessity thus enabling social competence. "The field, as a structured space, tends to structure the habitus, while the habitus tends to structure the perception of the field."⁵⁸ By seeing the "ontological complexity" of the two the subjective/objective dichotomy in explanation is overcome, in Bourdieu's account.

As all these arguments indicate, there are fundamental questions about the rationality, interestedness, power, and validity of the scientific enterprise and its products. Moreover, there is an increasing degree of consensus around the idea that, *contra* Kuhn, non-scientific discourses are like sciences in being characterised by paradigms as well. The question then becomes one of distinguishing scientific paradigms or frameworks from non-scientific ones. The great divide (if it is great) between the pre-modern, pre-scientific mode of cognition and the modern scientific mode has been explored extensively by anthropologists such as Lévi-Strauss, Bourdieu, Horton, and Gellner,⁵⁹ who have much more of significance to say about this than do ahistorical philosophers.

It is of course notorious that the modern social enquiries have not been able to develop consensual paradigms in spite of many attempts by thinkers from Comte onwards to do so. Is this because of the recalcitrance and complexity of the subject matter -- people and their shifting social arrangements -- or because of something lacking in the scientific mode of reasoning? Perhaps it is both -- is scientific reasoning incapable of grasping a subject matter that is humanly historical? But of course, as I have pointed out before, many

⁵⁷ P. Bourdieu, 'The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason' (1975).

⁵⁸ P. Bourdieu, 'Vive La Crise! For Heterodoxy in Social Science' (1988/89), p. 784.

⁵⁹ See C. Lévi-Strauss, *The Savage Mind* (1966); P. Bourdieu, *Outline of a Theory of Practice* (1977) and *The Logic of Practice* (1990); R. Horton, 'African Traditional Thought and Western Science' (1967) and 'Tradition and Modernity Revisited' (1982); E. Gellner, *Thought and Change* (1964), *Legitimation of Belief* (1974), and *Relativism and the Social Sciences* (1985), Ch.3.

of the sciences of nature also deal with historical systems. It seems to me that the problem has been methodological -- the attempts to construct a scientific framework for social analysis have usually been on the level of logic rather than socio-anthropological and practical. What makes a scientific discourse scientific is not its logic but its combination of rationality, orientation to the world, and practical application. The recent arguments of Dudley Shapere, including his concept of "scientific domain", provide an important but incomplete way, for reasons I shall try to indicate, of approaching these questions about scientific explanation. His arguments, especially about "domains", provide assistance in trying to improve approaches to social structural history.⁶⁰

Like Bourdieu, Shapere's fundamental argument is that there are no universal meta-epistemological or meta-logical criteria for judging good and bad explanations (or even what counts as an explanation) that are independent of the criteria established and employed by the advanced natural sciences. If we wish to understand the epistemological and methodological principles of scientific explanation we must carefully study the history and present practices of science itself. There can be no essentialist viewpoint of Kantian or logical empiricist or any other kind that is able to interpret and explain science independently of science according to some extraneous standard. All such standards, in Shapere's view, fail to grasp the actual reasoning employed by particular sciences. Thus the nature of scientific enquiry and the status of scientific explanations is an empirical question and, furthermore, the sciences differ from each other. Nevertheless, enquiries in the advanced sciences do have certain distinguishing features demarcating them from other forms of enquiry because, according to Shapere, the sciences have adopted in the 20th century two fundamental principles: "The Principle of the Rejection of Anticipations of Nature" and "The Principle of Scientific Internalization" (as defined in the quotation on p.vi above). Scientists seek and acquire knowledge which cannot be anticipated in advance. They do this through a process of building on those existing beliefs or knowledge which are free from reasons for doubt so that all the beliefs of science become part of science itself and are not external, unquestioned assumptions.

For Shapere, rationality is the central feature of science:

to count as a reason, a claim must be relevant to the subject-matter under consideration or debate. And thus the clear delineation of a subject-matter, and of the body of other claims relevant to that subject-matter, itself constitutes the development of a science based on reasons.⁶¹

⁶⁰ See Bibliography for Shapere's works.

⁶¹ D. Shapere, *Reason and The Search for Knowledge* (1984), p. xxii.

These two features -- a clearly delineated subject matter and a body of other claims -- he calls respectively "domains" and "background information". In his view,

it is precisely these developments -- the formation of domains and of background information relevant thereto -- that *constitute* the development of the rationality of science. For one of the most fundamental aspects of the idea of a "reason" in general is the following: to count as a reason, a claim must be *relevant to the subject-matter under consideration or debate. And thus the clear delineation of a subject-matter, and of the body of other claims relevant to that subject-matter, itself constitutes the development of a science based on reasons....*

The development of science thus consists in a gradual discovery, sharpening, and organization of relevance-relationships, and thus in a gradual separation of the objects of its investigations and what is directly relevant thereto from what is irrelevant to those investigations: a gradual demarcation, that is, of the scientific from the non-scientific. Indeed, to the extent that an area of human activity manifests the sorts of developments I have been describing, to that extent the area is considered paradigmatically "scientific". In other words, *this* is what we have come to call "scientific". In that development, science aims at becoming, as far as possible, autonomous, self-sufficient, in its organization, description, and treatment of its subject-matter -- at becoming able to delineate its domains of investigation and the background information relevant thereto, to formulate its problems, to lay out methods of approaching those problems, to determine a range of possible solutions, and to establish criteria of what to count as an acceptable solution, *all in terms solely of the domain under consideration and the other successful and doubt-free beliefs which have been found to be relevant to that domain; that is, to make its reasoning in all respects wholly self-sufficient.*⁶²

Shapere emphasises that the idea of "reasons" that he employs is independent of the specific character of reasons. Scientific rationality -- i.e. the reasons it has for holding its beliefs to be good ones -- has arisen through a complex process of observation of nature. But the character and meaning of observation is not a matter of passively recording sensory data. Observations are always interpreted within the context of the existing state of knowledge of a domain.⁶³

Science thus develops through a give-and-take interaction between the methods with which it approaches nature and what it learns about nature, or at least claims to know on the basis of the best reasons it has available. Included in that interactive development are, as we have seen, the subject-matter, the problem-structure, the standards, and the goals of science: in all these aspects, science is subject to change. And it has learned to make those changes, wherever possible, in the light of reasons -- reasons which, for us, consist of observations of nature..... The traditional doctrine of conceptualism was thus partly right: we do not extract our concepts of nature through observation of nature. But we do not simply "abstract" those concepts by perception and reflection thereon. Rather, learning through observation, we gradually forge concepts which reflect what we have found to be the case, or at least believe to be the case on the basis of the best reasons we have available; and then we seek to learn further about nature, and perhaps will be led to further revision of the concepts we have forged, and so on. Thus the present view may be termed "Bootstrap Conceptualism".⁶⁴

⁶² *Ibid.*, pp. xxii-xxiii (emphasis in the original).

⁶³ *Ibid.*, p. xxviii.

⁶⁴ *Ibid.*, p. xxxiii.

The basic problem with Shapere's argument is that while he rightly wishes to avoid any use of given and unalterable meta-scientific standards of what constitutes scientific practice and the legitimacy of results, it is in danger of slipping into the relativism and scepticism that he wishes to avoid. If the Internalization Principle is taken to an extreme it could be seen as a position very similar to those of Foucault and Feyerabend and the pragmatists. In their case, what constitutes a reason for holding a belief about nature is not that it corresponds in any, however complicated, sense with the reality of nature but that it *coheres* with the concepts and untested beliefs of the framework or discourse.

If relativism and scepticism are to be avoided then some notion of *realism* and the truthfulness of *correspondence* seem to be necessary. Therefore the most important point to glean from Shapere's approach is the concept of "domain" for with it we have a way of reconciling the rationality of science with realism. In the process of building a science, he says,

items of information come to be associated together as bodies of information having the following characteristics:

- (1) The association is based on some relationship between the items.
- (2) There is something problematic about the body so related.
- (3) The problem is an important one.
- (4) Science is "ready" to deal with the problem.⁶⁵

The association between the items is well grounded in domains such that the subject matter is seen as unified mainly in terms of the compositional and evolutionary theories that pertain to it. Compositional problems require theories and explanations of the constituents of the individual entities or elements of the domain and the laws governing their behaviour and interactions, and evolutionary problems require theories and explanations of the development of the entities or elements.⁶⁶

The process of domain constitution depends upon the dual processes of theoretical reasoning within scientific fields and gradual *discovery* of what Shapere calls relevance-relations among the objects and powers of the natural world. Those relevance-relations should be seen, I believe, as *structural causal* relations that are *naturally* delineated. Domain construction and development thus ultimately depend on the ways in which the world is discovered to be naturally structured and discoveries are dependent upon

⁶⁵ Shapere, 'Scientific Theories and Their Domains' (1974), p.525.

⁶⁶ *Ibid*, p. 534.

appropriate methodologies and theories. The appropriateness can only be determined historically *post facto* and thus methodologies and theories are contingent. The methodology of the domain is dependent upon the subject matter of the domain for it must be appropriate for making compositional and evolutionary discoveries at the level of the particular subject matter. But of course nature is not given in some simple, easily apprehensible form. Shapere is very right to draw attention to the inadequacies of common sense, and we should go further for nature has been found to be completely counter-intuitive in its complexity. Therefore methodologies evolve under the impact of further discoveries of the complexities of nature and the subject matter evolves under refinements, reappraisals, and reconstructions of the methodology. There is no external principle by which methodologies are appraised or improved. Revolutions *à la* Kuhn, in which the old methodology and form of understanding of the subject matter is abandoned, do not seem to occur. But the constant interaction between methodology and subject matter as the complexities of nature are further investigated leads to rearrangements and reconstructions within the domain. Incremental stages of long-term progress can be observed within sciences with the occasional change of direction and unification of separate domains. New understandings and knowledge within science are able to incorporate and reinterpret older understandings and knowledge such that contemporary theories and explanations can be seen as genetically related to earlier ones.⁶⁷

If social structural historiography is to develop a distinct domain of enquiry it is by being delineated through the interaction of the relatively unified nature of its subject matter with its methodology. For a domain to be said to exist a body of shared information has to be assembled about the composition and evolution of a class of entities such that the entities have relevance relations of a realistic character. The initial establishment of relevance relations is not a question of simple observation but of a complex process of metaphorical, analogical reasoning that I have discussed above and will do so again in Chapter Four below.⁶⁸ Therefore in order to be definable as such the domain must have a definable subject matter that can become the object of a distinct kind of enquiry. "Definable" here means "capable of being rationally abstracted from the totality of which it is a part". A rational abstraction is one that has some discovered or hypothesised foundation in reality and so is capable of being given an objective definition. In other

⁶⁷ For defences of the idea of scientific progress as the gradual refinement of theories and methodologies see H. Putnam, *Mind, Language and Reality* (1975), Chs.1-13; S. Kripke, 'Identity and Necessity' (1971) and *Naming and Necessity* (1980); and R.N. Boyd's works listed in the Bibliography.

⁶⁸ See also C. Lloyd, *Explanation in Social History* (1986), Ch.7.

words, there are good reasons, which can be shared by social scientists, for believing that the abstraction of social structures is based on features of concrete reality. If social structures have a form of existence that is relatively autonomous (or at least their reality can be studied as if it were relatively autonomous, that is, without the abstraction of structures from the social totality being too injurious to the totality) then they can be abstracted and studied as a domain of enquiry. All domains of enquiry are based upon such abstractions because the totality of all there is in the world is both a totality in the sense of being fundamentally *systemically integrated* but also being divided *naturally* into sub-systems and sub-structures. Discoveries of these more-or-less natural divisions have allowed and prompted the development of separate domains of enquiry. Of course the history of ideas shows that some abstractions on which domains or proto-domains have been based have been construed wrongly or, to put it more accurately, certain proto-domains can be incorporated, following further research and theorising, into other domains. As I indicated earlier, this is a central feature of the history of the sciences -- the tendency to reduce the number of domains by uniting them through the use of encompassing general concepts and theories that operate at a deeper and wider level. (I believe the supposed domain of economics can now be seen as in part falsely or at least too narrowly construed and should be included within the domain of social structural enquiry. Many economists themselves now believe this too, as I shall show in a moment.)

V IMPORTANCE TO SOCIAL SCIENCE OF A POLICY OF REALISM

Philosophical debates over the reality or otherwise of the objects of enquiry are clearly crucial to establishing the nature of scientific enquiry and knowledge and therefore for generalising from mature to immature sciences. As I just pointed out, Shapere's domain conception depends for its power on showing its congruence with realism. Philosophical realism is not quite the same as the social ontology of realism that I alluded to in Figure 1.1 and the associated discussion. But they are closely related in that philosophical realism provides epistemological underpinning for the social doctrine.

Philosophical realism is the broad and much debated doctrine, with many internal variants, which holds that knowledge is true or false (or perhaps plausible versus implausible) according to how the external world really is independently of our ways of knowing.⁶⁹ One variant is metaphysical or ontological realism, which asserts that there

⁶⁹ For general discussion of realism, see M. Devitt, *Realism and Truth* (1984); R. Harré, *Varieties of Realism* (1986); R. Miller, *Fact and Method* (1987).

is a fairly sharp division between on one hand, our explanatory frameworks, theories, and observations, and, on the other, the external, mind-independent world. It starts with the metaphysical presupposition of a certain form of real existence of external, ordered, discoverable, reality, which then becomes the dominant principle of explanation. Another variant -- epistemological realism -- starts with the methodological assumption that assertions about causation and reality have to be tested according to well established and seemingly successful procedures that over time, with the aid of a certain form of reasoning, have been found to furnish knowledge through discoveries about external reality. This is clearly a more plausible doctrine than ontological realism because of its grounding in the history of discovery rather than in an *a priori* assertion. Indeed, for the same general reason, epistemology has rightly come to dominate ontology as the core of philosophical argumentation. A third variant is transcendental realism, which regressively argues from the fact of successful science, as measured by ability to intervene in nature, to make a claim about the necessary reality of the structures and mechanisms of the world in order for science to be possible. Such a transcendental argument licenses, at least, a *policy* of realism as a commitment underlying judgments of the plausibility of theories.⁷⁰

Philosophical realism contrasts with rationalism (including idealism), phenomenology, and instrumentalism, all of which deny in various ways that the explanatory task can be to make objective discoveries about an independently real world. They all want to view subject matters as constituted in various ways through processes of belief, thought, and conceptualisation, and not as existing as real structures independent of mental processes. For rationalists, knowledge is the product of reason that determines and reflects upon experience. For phenomenologists, the world can only be discussed and understood from a particular viewpoint and when the viewpoint changes so do the phenomena. The process of observation, understanding, and concept formation is partly constitutive of that being understood. That is, the world is constituted as available for knowledge through language. For instrumentalists, the object of enquiry takes the form of data produced by a methodology or technique and they do not purport to examine "reality", which has no meaningful content for them.

In practice, philosophical realists have combined elements of metaphysics and epistemology. One thing that divides them is the question of wherein lies the essence of reality -- in the ordered appearances of the world or in some hidden counter-intuitive

⁷⁰ The leading exponent of transcendental realism is Roy Bhaskar. See the Bibliography for references to his work.

structure of the world of which the appearances are not a reliable guide? This is the basis for the division between empirical or common-sense realism and critical or scientific realism. Both say the universe is independently real but the first says that our senses are a good guide to its nature whereas the second says its nature has to be discovered or inferred by science in spite of the appearances.⁷¹

The pragmatic version of realism that draws on epistemological and transcendental presuppositions -- policy realism -- says that realism should be employed only as a broad policy that directs research rather than as a strong claim about reality and the truth or falsity of theories.⁷² The status of theories is, in this approach, more complicated and negotiable than the simple bivalence of True/False. Plausibility rather than truth is the aim. This is mainly because all knowledge is framework-dependent to some extent and so assertions about reality and causation always come from particular rather than universal standpoints. But unlike with relativism, it is accepted that frameworks do change and improve under the impact of discoveries about the world thus confirming that the external world has power to modify the ways in which we understand it, just as our knowledge is able to be applied to intervene successfully in the structure and operation of parts of the world. One advantage of policy realism over more metaphysical versions is its accordance with the history of science, which reveals a series of jerky forward movements in the direction of greater and greater discoveries, as confirmed by the increasing power of engineering in open systems. No claim about absolute truth is necessary to such a policy. All knowledge is provisional but some is more confirmed, plausible, and reliable than others.⁷³

A policy of sociological realism presupposes that society and culture are independently real entities that are neither artifacts of the theorist's or actor's creation nor reducible to characteristics of individuals or patterns of individual behaviour. Although social structures and cultures cannot be sensed they are deemed to exist in virtue of their causal powers to influence the behaviour, beliefs, and understandings of persons, and are knowable through the behaviour, products, and utterances of persons. But just what

⁷¹ Empirical realism is not the same as empiricism. The latter is related to subjective idealism and is based on the idea that sensory data are the basis of "knowledge" and knowledge is not a claim about "independent reality", which is supposed by some logical empiricists to be a meaningless proposition.

⁷² On policy realism see R. Harré, *Varieties of Realism* (1967).

⁷³ This has been well argued by Karl Popper in *Conjectures and Refutations* (1972).

forms the social and cultural real ties are thought to take is a matter for conceptualisation and/or for theory and research.

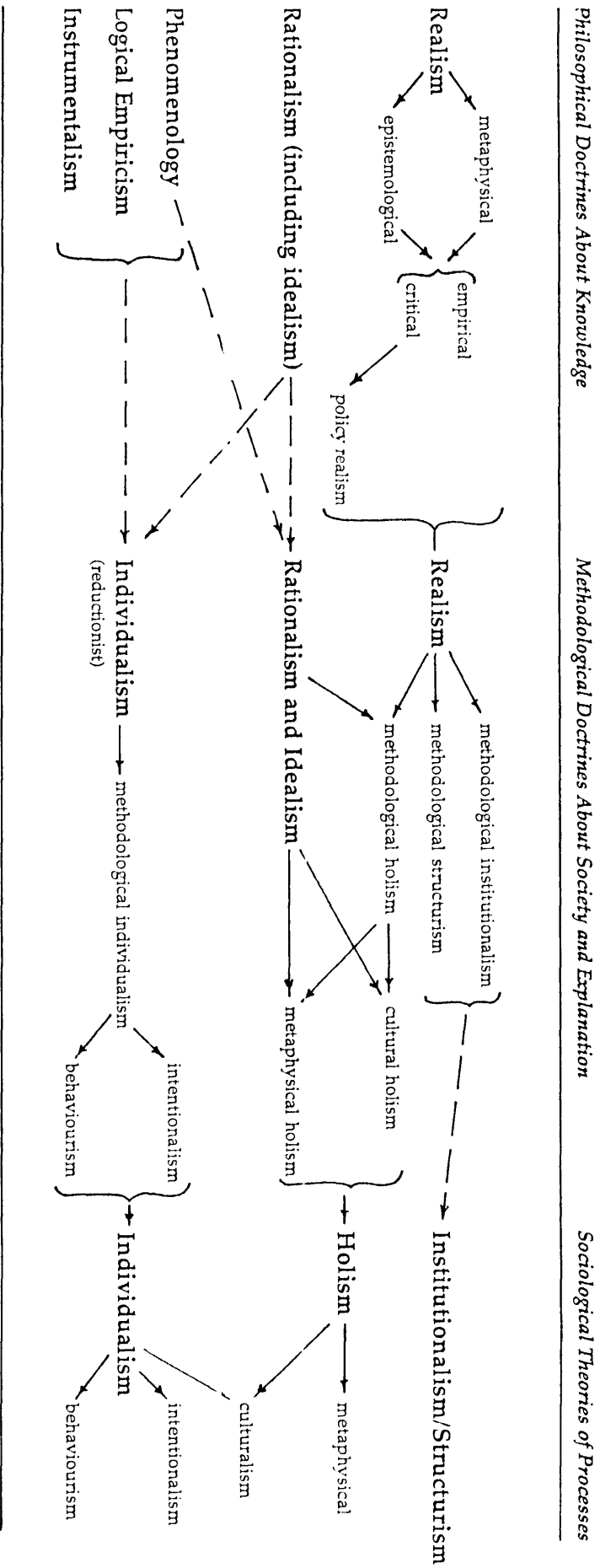
Sociological realism is not synonymous with sociological holism, although this mistaken equation has often been made in spite of cogent arguments that have been presented against it.⁷⁴ Holism is only one of two broad realist conceptions of society. A *holist conception* of society defines it as a self-contained and virtually autonomous unitary entity, sometimes endowing it with a spirit or quasi-mind of its own which exists through the thought and actions of people within it. But society can also be defined as a looser, less integrated *structure* of rules, roles, and relations, all of which exist apart from the particular people who employ or occupy them, and which has a dialectical and changing, rather than strictly determining, relationship with human thought and behaviour. Nevertheless, it is still real, although its reality is less autonomous than that of a holistic system. I shall try to show later that the decisive advantage of a *structuralist conception*⁷⁵ of social reality is that it enables the development of a plausible scientific theory of the origins and dynamics of social structures out of the historical intercausal relationships between society and human thought and action. Figure 1.2 attempts to sum up the interconnections of all these concepts.

Excellent philosophical support for a realist conception of society can be found in Maurice Mandelbaum's *Purpose and Necessity in Social Theory*. He shows how the dichotomies of individualism/holism, purpose/necessity, and chance/choice, have to be transcended in order for there to be a social science. All of these concepts are necessary to social, psychological, and historical explanation and, as many other structuralists have argued, their combination precludes the use of ahistorical theory based on rationalist rather than empirical reasoning. Mandelbaum's invaluable contribution to the question of establishing the parameters of the domain of the science of structural history will be discussed in detail in Chapter Four.

⁷⁴ Cf Karl Popper's classic discussion of holism in *The Poverty of Historicism* (1961) where he seemed to equate holism with all kinds of macroscopic concepts and theories. For the necessity to distinguish between tightly integrated social wholes and loosely integrated social structures see, for example, M. Brodbeck, 'On the Philosophy of the Social Sciences' (1973); E. Gellner, 'Explanations in History' (in *Cause and Meaning in the Social Sciences*, 1973); and my *Explanation in Social History* (1986), Ch 8. In later writings Popper acknowledged the existence of social institutions which have a "logic" of their own independent of individual people. See 'The Logic of the Social Sciences' (1976).

⁷⁵ My term "structuralist conception" or "structuralism" will be articulated in greater detail later. It has been influenced by the writings of Rom Harré in, for example, 'Philosophical Aspects of the Micro-Macro Problem' (1981) and 'Architectonic Man: On the Structuring of Lived Experience' (1978).

Figure 1.2 Basic Types of Social Methodologies and Theories



VI EVENTS, ACTIONS, AND STRUCTURES: ON THE METAPHYSICS OF THE SOCIAL WORLD

I contend, given the foregoing realist epistemological argument, that in order to establish the validity of the domain of structural history there has to be established the validity of employing a general conception of social structures as being *non-phenomenally real* and a conception of events (including actions) and structures as constituting a symbiotic *duality* rather than a dichotomy. The difference between dualist and dichotomous sociological conceptions is that in the former individuals and society are both relatively autonomous, with their own irreducible powers yet being inseparably linked, whereas in the latter they are opposites and almost mutually exclusive in their designated powers in that the autonomous reality of one excludes the autonomy of the other. Social realism and a dualist conception of events, actions, and structures should imply each other.

This is an argument about social metaphysics, the resolution of which is fundamental to methodological debates. But, as I have pointed out, a realist ontological position that is counter to empiricism, idealism, and phenomenology was arrived at only after a long process of proto-scientific and scientific activity so that the metaphysical argument is a reconstruction of results made possible by inductively generalising from a process of research and theorising. (I shall say more in Chapter Four about this difference between the process of discovery and the logic of arguments about abstract metaphysics.) Nevertheless, all ongoing social enquiry, whether subscribing to realism or not, must employ some general ontological notion about the fundamental nature of the object.

Is social structure merely a statistical *aggregation* of singular events and individual persons and their actions, that is, an artifact of a technique (perhaps expressed as an instrumental concept); or perhaps merely a taxonomic category, having no intrinsic properties and powers of its own? Or does it have irreducible properties and powers? If so, are they such as to constitute it either as a *holistic* closed system with the power to control its own history and effect changes in other entities? Or is it a less integrated *structure* of elements, relations, and processes that depend upon persons for their production, reproduction, and history, but that causally influence the actions of agential persons?

Basic Social Ontologies

An *individualist ontology* directs attention almost solely to the role of the natures, powers, and behaviour of individuals. In this ontology, society is not something extra-individual in the sense of relations, rules, shared understandings, and meanings that are not reducible to individuals. Society, rather, is either a set of observable positions that

individuals create and occupy, such as work positions and family roles, and which cease to exist when not occupied; or it is just a pattern of individual actions and events.⁷⁶

A *holist ontology* emphasises autonomous structural existence and evolution. Society, in this account, is supposed to be a supra-individual organic entity or organisation of rules, relations, and/or meanings, the sum total of which has properties and powers greater than its parts, particularly the powers to maintain and reproduce itself through dominating the choices and actions of individual people within it. The various kinds of holism all rest on the idea that society, or culture, or economy, or nation, are somehow objectively existing, external entities, which can be conceptualised and studied as though they are more or less unchanging things. The role of individuals here is as the passive carriers of collectively generated social forces. Change somehow mysteriously comes from the social whole and is implemented by individuals. This ontology is the least coherent of the three because it cannot generate a viable account of the genesis and history of structures.⁷⁷

A *structurist ontology* directs attention to the *structuring interactions* between (on one hand) individual and collective human beliefs, intentions, choices, and actions and (on the other) the externally real enabling and constraining structural conditions of thought and action. In this model *social structures are the emergent ensemble of rules, roles, relations, and meanings* that people are born into and which organise and are reproduced and transformed by their thought and action. It is people who generate structures over time and initiate change, not the society itself, but their generative activity and initiative are socially constrained. This ontology denies the legitimacy of the action/society polarity that the others are based on and attempts to conceptualise action and society as being an interpenetrating *duality* in the sense advocated by Jean Piaget and Anthony Giddens. There is a duality of causal power in this model -- humans having structuring power and structures having enabling and constraining power.⁷⁸

⁷⁶ Examples of social theorists who employ an individualist social ontology include James Coleman and George Homans. The Bibliography contains references to their work. I have discussed their work in *Explanation in Social History*, Ch. 11.

⁷⁷ Holist ontologies in social theory take three main forms -- wholes of the "spirit of the era" and "national character" kinds, deep structures of the Lévi-Straussian and Althusserian kinds, and systemic cybernetic structures of the Parsonian kind. See my *Explanation in Social History* (1986), Chs.10 and 12.

⁷⁸ Structurist ontologies in social theory take many forms, including Piagetian structuralism, structurationism, network theory, and figurations. Examples of their use include the work of Jean Piaget, Anthony Giddens, Philip Abrams, Norbert Elias, Charles Tilly, and Roy Bhaskar -- see the

The tacit or deliberate adoption of one or other of these ontologies tends to determine the concepts of society and action and the general methodology for examining structural history that are adopted. This is not to say that general ontological concepts are always prior to concepts of social structure and action and to methodologies. All of these are usually closely related and should conceptually reinforce each other. Together they afford the construction of substantive theories but are not themselves theories, strictly speaking, as I have pointed out in Section III of this chapter. Theories are general, putative, causal explanations that are in need of testing and confirmation in particular instances and may never reach the stage of being well confirmed. Theories are constructed using general concepts, models, and previously well-confirmed knowledge in order to try to deal with a domain of subject matter that has certain problems about structure, causation, and evolution.

General Sociological Concepts -- The Macro-Micro Problem

The relationship between the macro (structure) and micro (action) levels of society is perhaps the fundamental problem for social theory.⁷⁹ Dichotomous approaches to concept formation emphasise the primacy of either the macro or the micro over the other. That is, one or other pole is seen as the site of crucial determination. Micro determination is generally synonymous with individualist conceptions of social phenomena. These have a long history in the social enquiries, going back at least to Hobbes⁸⁰ and now take several forms in sociology, economics, and history. Perhaps the most prominent and important are rational action, behaviourist, and exchange theories, all of which are closely related to each other.⁸¹ Being individualist, they emphasise instrumental individual rationality and purposive behaviour. People are conceived as more or less autonomous individuals and the imperatives to action are sought largely within the natures, dispositions, consciousness, and decisions of people. Social structure, insofar as it exists, is conceived as

Bibliography and the relevant chapters in B. Wellman and S.D. Berkowitz (eds), *Social Structures* (1988).

⁷⁹ For general discussions of the macro-micro problem in social theory see R. Collins, 'On the Microfoundations of Macrosociology' (1981), 'Interaction Ritual Chains, Power and Property: the Micro-Macro Connection as an Empirically Based Theoretical Problem' (1987) and 'The Micro Contribution to Macro Sociology' (1988); J.C. Alexander *et al.* (eds), *The Micro-Macro Link* (1987) *passim*; N. Wiley, 'Macro vs Micro Interpretation' (1983) and 'The Micro-Macro Problem in Social Theory' (1988).

⁸⁰ cf Mandelbaum, *Purpose and Necessity in Social Theory* (1987), Ch 3.

⁸¹ See my *Explanation in Social History*, Ch 11.

the phenomenal outcome of large numbers of individual interactions and takes the form of collective patterns of observable behaviour. Social events are conceived as collective behavioural phenomena. Structure is ontologically dependent on rather than prior to action. This kind of conceptual scheme underlies neo-classical economics and is well articulated in the work of James Coleman who has developed, refined, and applied it over a thirty-year period.⁸²

Holist concepts of actions, events, and society start from the pole opposite from individualism; that is, from the pole of the social whole. Here society, or social reality, or the social totality, is conceived as an integrated unity with temporal, geographic, and cultural cohesion and integration. Humans have no individual autonomy or identity apart from that bestowed on them by the whole. Thus people are the personifications of or the carriers of categories such as "nation", "race", "class", and so on. Holist conceptions have taken many forms in social and historical theory ranging through functionalist social systems theory, structuralism, and phenomenological conceptions of the social life-world. In all these cases, the totality constitutes, motivates, and/or gives meaning to individual and collective action.

Dualist or structurist conceptions of the social realm have been developed as conscious attempts to transcend the older polarised individualist and holist traditions. These conceptions have an ancestry going back over a hundred years, as I shall briefly indicate in Chapter Three, and in recent years there have been several new and extended articulations. Three of the most important conceptions are those of structurationism, networks, and figurations. All of these conceptual schemes focus on the decentred person as the *agent* of reproduction and transformation of *pre-existing* social institutions that in turn structure semi-autonomous action and consciousness.

The event/structure duality (rather than dichotomy) and the dynamic structuring process mutually implied by it form the necessary structurist ontological basis for the *history* of social structures fundamentally because this general conception has a central role for a causal agent within a structured, irreducible, but evolving social context. This will be discussed extensively in Chapter Three.

⁸² See Bibliography for Coleman's writings.

VII THREE ALTERNATIVE METHODOLOGIES FOR STRUCTURAL EXPLANATION

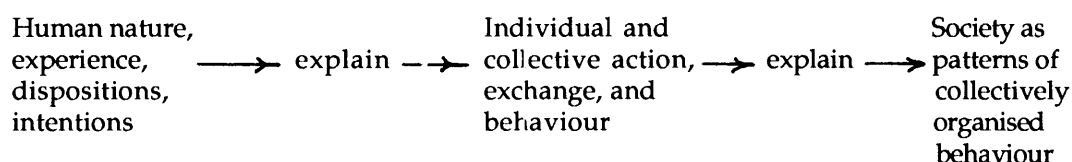
Unfortunately, what should be an event/structure duality has long and pervasively been considered a dichotomy. By emphasising either the decisions and activities of persons or the determining powers of social entities the social studies have given rise to two main competing explanatory methodologies -- individualism and holism. While these are not necessarily consciously or coherently adopted they can still be found strongly influencing the explanations of many or most practitioners in the socio-historical sciences.⁸³ Fortunately, there is a third possibility -- structurism -- which attempts to transcend this dichotomy.

Pulling together the foregoing discussion of ontologies and general concepts we can articulate the following methodologies.

Methodological Individualism

This attempts to explain social phenomena and processes, behaviour, and consciousness by reference to individual motivations and actions. That is, it attempts to explain what are conceptualised as macroscopic phenomena and entities by attributing their causation and even existence to their microscopic components. This does not necessarily rule out the existence of the macroscopic *per se* but it does deny it any inherent causal power on its own level thus calling its ontological status into question, at least. That is, reductive explanation is not necessarily reductive ontologically but it at least points in that direction.

Figure 1.3 Methodological Individualism



Reductive explanations are common in the physical sciences but even there they are far from universally applicable. Many macroscopic physical systems can only be explained as

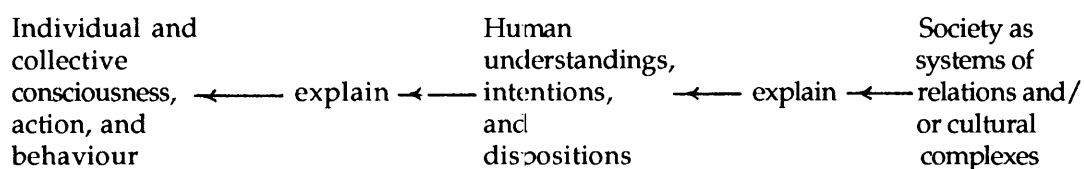
⁸³ See Mandelbaum, *Purpose and Necessity*, Ch 1.

systems and the behaviour of their components can only be explained as parts of systems. This is even more the case in the biological and ecological sciences. The fundamental barrier to successful attempts at reductive explanation, which is also evident in the social studies, is the irreducibility of certain macroscopic systems with emergent properties and powers.

Methodological Holism

Thus the opposite to individualism is methodological holism, which attempts to explain structures, behaviour, consciousness, and social change by reference to holistic categories such as "social system", "epoch", "culture", "nation", "race", "class", and even "spirit of the age". The supposed entities referred to by these categories are attributed with causal powers of their own, whether exercised pervasively through the consciousness of the actors who supposedly embody them or through some functional, teleological feedback relation upon action. Methodological holism is not ontologically reductive for it recognises the discrete existence of both wholes and parts, unlike methodological individualism, but its explanations do not go in both directions for the micro is seen as causally dependent.

Figure 1.4 Methodological Holism



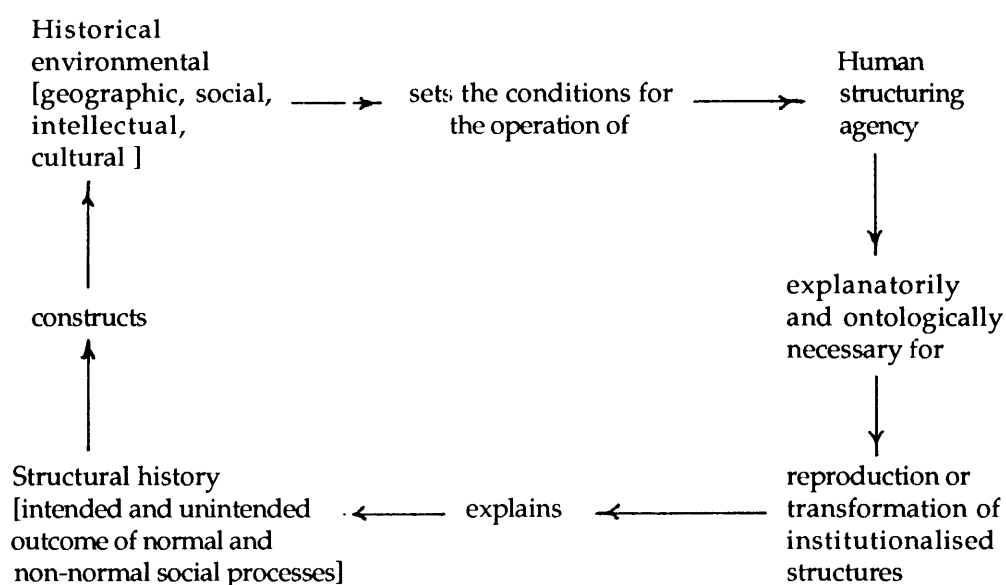
Being methodologies, however, neither individualism nor holism is necessarily committed to a corresponding social ontology and set of concepts. Nevertheless, there is usually an implicit ontological assumption in each methodology. That is, methodological individualism usually implies sociological individualism, in the sense that social entities and processes are reducible to the individual people, events, and actions that supposedly constitute them; and methodological holism implies sociological holism, in the sense that social wholes have some form of real existence independent of individual people and

events. Individualist methodology tends to overemphasise human autonomy and event distinctiveness while holism tends wrongly to impute autonomous power to social entities. Individualism does provide a coherent way of approaching the genesis and transformation of society but its implied concept of society is impoverished. Holism cannot provide a coherent approach to social change because there is no place for an agent.

Methodological Structurism

This third alternative says that social and behavioural explanations both have to be approached from the *dual* perspectives of action and structure. Methodological structurism approaches explanation by developing concepts of the separate real existence yet mutual *interdependence* of individuals and institutional structures. That is, structures *qua* structures have structural properties such that those properties are not merely the aggregate of the powers and behaviour of the individual people who are supposed to constitute them. On the other hand, those structural properties are not independent of the structuring practices of people. Thus methodological structurism is explicitly based on an ontology of the social that recognises two nodes of causal power. However, the kinds of causal power that persons and structures exhibit are different, as we saw in the previous section. Persons have *agential* power, structures have *conditioning* power. The conception of persons as agents is the fundamental difference from the other two methodologies.

Figure 15 Methodological Structurism



The term "methodological structurism" is not widely employed but the more common term "institutionalist approach" seems to mean almost the same thing.⁸⁴ In fact there have been earlier intimations of and versions of structurism going back more than a century, as I shall briefly indicate in Chapter Three.

This grid of ontologies and methodologies can be summarised as in Figure 1.6

Figure 1.6 Table of Methodologies⁸⁵

	Ontology	Methodology
Individualism	Only individual events and people and their actions and beliefs are real. Society is an aggregate of individuals. The term "society" is only instrumental.	Aggregative -- builds up an analysis of society by studying individuals and their motivations for action.
Holism	Society is a closed, supra-individual system with powers of self-regulation. It dominates individuals who receive their life-courses and beliefs from the whole, which acts through them.	Conceptualises and studies the whole as a totality that structures everything within it. Searches for the internal determining mechanisms and/or essential meaning of structural evolution.
Structurism	Society is a real structure of rules, roles, relations and meanings that has to be produced, reproduced, and transformed by individuals while causally conditioning individual actions, beliefs, and intentions.	Conceptualises and studies the structuring process over time by examining the causal interactions of individuals, groups, classes, and their structuring social conditions, beliefs, and intentions.

⁸⁴ See Mandelbaum, *Purpose and Necessity*, Ch. 5, where he discusses institutionalism.

⁸⁵ Lest it be thought all sociologists and historians are now tacit or overt structurists thus leaving the other four boxes in the table empty, I point out at this stage that this is far from the case. It is possible to identify many prominent individualists and holists, some examples of whom will be given in Chapter Two. Of course they do not necessarily subscribe to the pure models just outlined.

VIII EVENT HISTORY AND STRUCTURAL HISTORY

If economies and societies can be understood as dynamic, non-phenomenal, yet real structures then all those who study the history of economies and societies (defined in a wide sense to include families, firms, markets, communities, political systems, languages, and mentalities) are, *ipso facto*, social structural historians. If social structures are not being directly studied then this label should not be used. That is, if the objects of enquiry are primarily events, actions and/or the behaviour of groups, then that is not social structural history in the proper sense of the term but is event history. However, structures and events are not somehow ontologically separate things, which is a mistake that tends to be made by some structuralist historians and sociologists, such as Fernand Braudel, Immanuel Wallerstein, and Talcott Parsons. The basic problem, as I have indicated previously, is to try to establish the ontological relationship between structures and the phenomena of the social world. Each is completely dependent on the other but it is possible to construct a social methodology that emphasises one or the other for explanatory purposes. Structures have a suprahuman, non-phenomenal existence through time, even for centuries, and are the context and object of events, actions, behaviour, and thought. Structures can be conceived as the systems of social rules, roles, relations, and symbols in which events, actions, and thought occur and lives are lived. But structures have to be reproduced continually in thought and through action and cannot exist apart from collective thought and behaviour. The division of labour in their study should be within a methodological structuralist explanatory framework that emphasises the symbiotic duality of event/structure rather than a dichotomy.

This argument about structures and events entails that "historical sociology", "societal history", "social history", and "structural history" should all be alternative names for the same discourse, whereas "action and event history", even if it is mistakenly called "social history", is distinct from but dependent upon structural history for its explanations, just as the converse is true. At times it seems that most historians and most kinds of history writing are now claiming to be "social" because it is fashionable to be so. But mere labels and rhetoric are not sufficient. "History from the bottom up" is intrinsically no more "social" than "history from the top down".

In short, I argue that if the study of social history is to be seen as having a distinct object of enquiry, as many of its supporters have strongly advocated, then that object must be the history of society as a definite real, continuous, structural entity. Some so-called "economic" and "social" historians, however, do not in practice study economies and societies *as real structures* and so do not rightly fall into this category. And some political,

cultural, and intellectual historians are in fact primarily studying structural change so they too should rightly be seen as structural historians. Indeed, political and cultural structures should be included. The analytical division should not be between the economy, society, and class structure, on one hand, and politics, ideas, beliefs, psyches, and so on, on the other. There should not be a material/mental methodological division, rather a structure/event heuristic division. The material, social, and mental are all structured. Events and actions of all kinds are motivated, impelled, channelled, and organised by structures, partly toward reproducing and transforming them. If social history is to have an object of enquiry and methodology distinct from the traditional history of events and actions it has to be possible both to study structures relatively separately from the actions and events that they determine and which in turn determine their history and to study events and actions in new, non-traditional, theoretically informed ways.

I emphasise that I am arguing *against* the complete collapse of all the socio-historical studies into each other. There needs to be a *rational division of labour* between the domains of event history and structural history within the entire broad field of socio-historical enquiry. In other words, there is a sound basis for some separation between branches of the field; but the existing dichotomies of history/theory and past/present are not soundly based, and the separations should not be dichotomous.⁸⁶ These

⁸⁶ The position adopted here on the question of the fields of social enquiry has certain similarities with that developed recently by Paul Veyne. He said that his book *Bread and Circuses* (1990) was a work of "sociological history", in the sense of "sociology" developed by Weber. The difference between sociology and history was described by Veyne thus:

"A historical fact can be explained and consequently described, only by applying to it sociology, political theory, anthropology, economics, and so on. It would be useless to speculate about what might be the historical explanation of an event that could differ from its 'sociological' explanation, its scientific, true explanation. In the same way, there can be no astronomical explanation of astronomical facts: they have to be explained by means of physics.

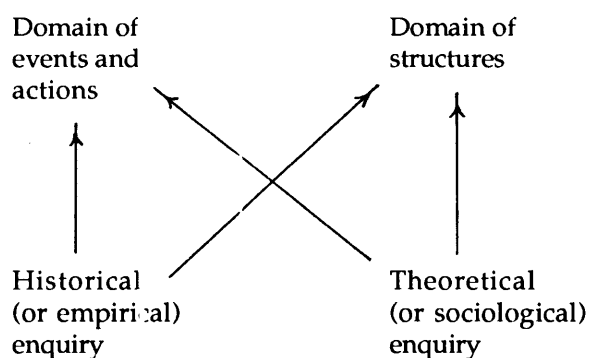
And yet a book about astronomy is not the same as a book about physics, and a book about history is not quite the same as a book about sociology -- although the difference in this case is less than traditional historians are wont to claim. ... They both seek to explain the same events in the same way, but whereas sociology deals with the generalities (concepts, types, regularities, principles) that serve to explain an event, history is concerned with the event itself, which it explains by means of the generalities that are the concern of sociology. In other words, one and the same event, described and explained in the same way, will be, for a historian, his actual subject, whereas, for a sociologist, it will be merely an example that serves to illustrate some pattern, concept or ideal type (or will have served to discover or construct this).

The difference, as we see, is slight, in the main. From one angle, we have an act of euergetism explained and conceptualised by the ideal type of political science and, from the other, these same ideal types illustrated or discovered by means of an example, namely, an act of euergetism. The flavour is the same, the potential readers are the same, and, above all, the knowledge required of the historian and the sociologists is the same, except for the division of labour implied. Since the 'facts' do not exist (they exist only through and under a concept, otherwise they cannot be conceived), a sociologist needs to know how to constitute them, while a historian has to be able to find his way around in

event/thought/structure distinctions that I am advocating are ultimately based on the realist argument (outlined in the previous section) about the ontological status of events, thoughts, and structures, and the possibility of a correspondence truth relation between theory and evidence. I shall develop this philosophical argument in detail later.

These considerations lead to a summary description of the connections between all the socio-historical studies, which sees the broad field as consisting of four interrelated "quadrants" or branches, as in the following diagram. This shows that there are two domains of enquiry as delineated by the fundamental ontology of the social world. The establishment of this ontology is the outcome of two hundred years of enquiries. Those enquiries have been of two basic kinds -- historical and theoretical.

Figure 1.7 The Field of the Social Studies



A rational division of labour in the social studies would see enquiries *concentrating on* one or more of theoretical (i.e. sociological) or empirical (i.e. historical) enquiries into events or structures without ever losing sight of the influence of the others and seeing each enquiry as contributing to the overall understanding of a total historical social process. The best social scientists, such as Clifford Geertz, Barrington Moore, Paul Veyne, Eric Hobsbawm, Emmanuel Le Roy Ladurie, Alain Touraine, and Ernest Gellner combine in their work the perspectives of all four "quadrants". In that case we should call them "social scientists", rather than any of the more particular labels of "historian", "sociologist", "historical sociologist", "anthropologist", or "social theorist". Indeed, they usually reject

sociology, to estimate its relevance and, where necessary, to create it. History leads to sociological discoveries being made, while sociology solves long-standing historical problems, and also poses fresh ones." (pp. 2-3)

such labels. But even in their work it is possible to identify *analytically* the four "quadrants" as separate kinds of analysis. In fact this identification is possible for the work of a great many historians and sociologists but some of the "quadrants" are often poorly developed in their work. The rich development of all four makes the work of the aforementioned social scientists compelling and powerful as explanations.

IX PROBLEMS OF EXPLANATION OF STRUCTURAL HISTORY

Having now discussed at a very general level the philosophical parameters of the putative or proto domain of structural history I turn now to a consideration of some of the problems that are at issue in constructing explanatory accounts in structural history. Some of these problems arise because of the as yet vague state of the philosophical and methodological framework of the domain and others are inherent in all explanatory accounts.

Causal explanation

Some historians have been ambivalent about whether establishing causation is their aim and, furthermore, there is a glaring lack of agreement about what the establishment of causation involves. This ambivalence and confusion spring ultimately from a lack of agreement among philosophers about the nature of causal explanation and the relationship between generalisations and particular cases. However, as I argued above, there is a growing current in the philosophy of explanation that rejects the characterisation of scientific inference as conforming to the canons of deductive logic and attempts instead to develop more complex and empirically grounded accounts of the logic of causal explanation in different sciences. Scientific explanation, it is now argued, employs a complex structure of theoretical hypotheses, empirical generalisations, particular descriptions, analogies, metaphors, and models. It makes intuitive leaps and unsupported assertions. (These points will be developed further in Chapters Four and Six.)

The point here is that the interrelationship of hypotheses, analogies, models, general theories, and so on, to evidence is a central methodological problem for the *causal explanation* of structural history, as in any form of empirical enquiry. If causal explanation is not thought to be the goal, then the problem of giving a convincing account of an alternative goal is seemingly insurmountable. It is difficult if not impossible to see what another goal could be because however the commencement of empirical enquiry is justified (such as by a desire for "understanding") it must boil down to a desire to answer

"why" and "how" questions as well as "what" and "when". The understanding of what and when cannot be divorced from temporal and structural relationships of a *causal* kind. So-called "hermeneutical understanding" and "interpretive description" should be seen as incomplete forms of the complex form of causal explanation just mentioned. If they simply provide chronologies then perhaps they do not explain but this is rarely if ever the case. Even supposed non-explanatory descriptions in fact involve some explanatory element. So it is not a choice between explanation or understanding or description. These are all parts of the one enquiry. The problem is to try to penetrate to the causal relations between the moments of complex social structures in order to make a judgment of the relative strength of these relations. This involves a process of theoretically informed and organised empirical research. Construction of the problems and objects for enquiry takes place within theories, which are within domains, but theories must be responsive to empirical findings that they help uncover and interpret. As Max Weber pointed out long ago (but has too often been ignored),

the formulation of propositions about historical causal connections not only makes use of both types of abstraction, namely, isolation and generalization; it shows also the simplest historical judgment concerning the historical "significance" of a "concrete" fact is far removed from being a simple registration of something "found" in an already finished form. The simplest historical judgment represents not only a categorically formed intellectual construct but it also does not acquire a valid content until we bring to the "given" reality the whole body of our "nomological" empirical knowledge.⁸⁷

Conceptual and Theoretical Thoroughness

Ideally, causal explanations should not leave loose, unexplained ends. A basic working assumption should be that all phenomena and aspects of society are interrelated and so in principle explainable by the one general explanatory structure of theory and analysis. This means that in order for sociologists and historians adequately to explain any of the "moments" and "levels" of social totalities -- actions, utterances, events, productions, behavioural patterns, cultures, structures, and so on, and changes in patterns, cultures, and structures -- they first need concepts and theories of all of these and of how they causally relate to each other in very general terms.⁸⁸ However, that is not to say much because even when such concepts are present it is the content of the concepts and theories that is obviously crucial. For example, we can see that the behavioural approaches of James Coleman and George Homans, which are forms of individualist

⁸⁷ M. Weber, *The Methodology of the Social Sciences* (1949), p. 175.

⁸⁸ Max Weber's excellent discussion in *The Methodology of the Social Sciences*, *Ibid.*, pp. 164-188, is still highly relevant to this problem of adequate explanation.

theory, and the structural-functionalism of Talcott Parsons, which is a form of holism, all offer, in different senses, a complete approach to social and historical explanation in that they have theories and explanations of all these aspects. But their conceptualisation and theorisation are inadequate to the complexity of society.

Conceptual and Theoretical Adequacy

Conceptualisation and theorisation must be adequate to their objects. That is, *all* the "moments" and "levels" of a particular totality must be treated as causally efficacious. It is here that individualism and holism are fundamentally flawed by their explanatory concentration on one or other component of the *structuring process*. One or other of the components of individuals or structures remains causally passive or even worse -- epiphenomenal. Behaviourism attempts to explain social phenomena solely by reference to the motivations of individual behaviour; while structural-functionalism, although purporting to be an action theory, in effect explains action solely by reference to its supposed functional relation to a holistic social system.

Against these, structurism tries to tie the micro and macro levels of social analysis together, without subordinating either to the other, by giving an account of how human personality, intentions, and actions interact with culture and structure causally to determine each other and social transformations over time. In order to do this it is essential of course that there be a model of humans as social agents. Agential persons have innate causal powers to affect intentionally and unintentionally their own actions and bring about changes in the world. This conception thus leaves no epiphenomenal danglers while incorporating all aspects into the explanation. Individualism and holism have impoverished conceptions of persons.

Subjectivism versus Objectivism

There is no doubt that social enquiry has a subjective element in that we partly rely upon the verbal and written statements of actors for our knowledge of society and social enquirers cannot divorce themselves from society to the same degree that natural scientists are external to their subject matter. Society is to a degree phenomenologically constituted by both actors and observers. What this means is that our subjective point of view of society affects the way we and others act and our actions in turn help to constitute the way society is. Because society is far from being absolutely objective in its existence there is a constant interaction between our socially-constituting thought and actions and the pre-existing social structure. Nevertheless, social scientists are not natives of the societies they are studying except their own local communities and institutions and so can and must

have a degree of detachment. The distinction between observer and native is always present for no matter how much we try we cannot become native of that which we are not so the possibility of an entirely empathetic understanding is ruled out. Social explanation cannot be a matter of getting inside the natives' skins and telling it as they see it but rather is a matter of enquiring into the natives' points of view as well as examining the structures in which they live and of which they may in any case have a very imperfect and distorted understanding.

The difference between natural and social reality has been seen as crucial by the advocates of the necessity of subjective interpretation since Vico at least. Society requires, these people argue, a subjective and phenomenological approach. On the other hand, the objectivity of society has been emphasised by several theoretical schools, including positivism, structural-functionalism, Marxism, and Francophone structuralism. The task, as many historians and sociologists now realise, is to try to reconcile and transcend this age-old polarity of objective structural determination and human subjective and socially-constitutive action that structures the world within existing structural limits. As Abrams and others have pointed out,⁸⁹ and as I show in the following chapters, there are now valuable attempts being made to overcome this polarity.

The Presentation of Historical Accounts and Arguments

We must distinguish, as Karl Marx and Max Weber did, between the rhetorical or artistic presentation of results and the logical structure of enquiry.⁹⁰ The structure of socio-historical analyses, as I shall argue in detail in a moment, is no different in essence from that of other sciences but the presentation is usually different because of the role played by story telling or narrative. There is a false dichotomy sometimes drawn (as Lawrence Stone did) between narrative and analytical-statistical (sometimes misleadingly called "scientific") presentations of histories.⁹¹ Both always have a part to play, their roles depending upon the task in hand. Narratives are more or less putative and incomplete causal accounts of a colligatory kind and statistical accounts are incomplete structural causal explanations. Both are analytical as well as descriptive to varying degrees. In fact, narrative seems to be unavoidable for all historians because of the

⁸⁹ P. Abrams, 'History, Sociology, Historical Sociology' (1980); R. Bernstein, *Beyond Objectivism and Relativism* (1983).

⁹⁰ Cf Marx, 'Afterword to the Second German Edition of *Capital*' (originally 1873), in *Capital*, Volume One (1971) pp. 128-9; Weber, *The Methodology of the Social Sciences*, (1949) p. 176.

⁹¹ L. Stone, 'The Revival of Narrative', (1979).

temporal dimension of historiography. Events and actions follow events and actions in a continuous jerky flow of time that takes place within a gradually evolving structural context that enables them to happen. None of the *Annales* structuralists and not even the economic cliometricians entirely eschew narrative in the way that Stone seemed to believe. A glance at their work shows this. And as their work also shows, description is unavoidable. Perhaps some descriptivists do manage to avoid giving causal analyses, in which case they are offering only chronologies or shallow journalism. But even the shallowest of journalistic reporting implies some explanatory causal sequence. The examples of early narrative that Stone cites, such as Thucydides, Gibbon, and Macaulay, and biographies, all contain putative or elliptical causal analyses. Moreover, the "revival of narrative" insofar as it exists is not a return to simple narrative, as Hobsbawm and Abrams pointed out,⁹² but contains an increased awareness of the epistemological scope of narrative by combining explanatory colligation with theoretical explanation. Philosophers of history have long discussed this question and many have shown convincingly how narrative can be explanatory.⁹³

The problem for historians in this regard, as many of them now recognise and deal with, is how to combine structural analyses (employing theories, statistical data, and qualitative evidence) with narrative presentations. Structural historians cannot avoid examining aggregate patterns of events, actions, and utterances of all kinds because it is only through them that structures of rules, roles, relations, and meanings become available for analysis. The task is not to reject *histoire événementielle*, as some *Annalistes* have advocated (but not practised), but first to grasp the ontological relationship between structures and events. Neither is reducible to the other but neither can exist without the other. Structures eventuate in events and are reproduced and transformed through events. So the presentation of structural history must take a partly narrative form and in fact always does so. Lawrence Stone's own work contains good examples of this.⁹⁴

⁹² Hobsbawm, 'The Revival of Narrative', p. 4; Abrams, 'History, Sociology, Historical Sociology', pp. 9-10, and *Historical Sociology*, Ch 10.

⁹³ There is an excellent discussion of narrative explanation in W.H. Dray, 'Narrative Versus Analysis in History' (1985). He refers to much of the relevant literature and defends at length the position advocated here regarding the complexity and unavoidability of narrative, even in "analytical" history. See also M. White, 'The Logic of Historical Narration' (1963); L.O. Mink, 'Narrative Form as a Cognitive Instrument' (1978); C.B. McCullagh, 'The Unifying Themes of Historical Narratives' (1989); and T.C. Jacques, 'The Primacy of Narrative in Historical Understanding' (1990).

⁹⁴ See the Bibliography for examples of Stone's work.

The Role of Comparison

If socio-historical explanation requires a complex web of different kinds of inference between analytical constructs and empirical evidence then there should be a central role for comparison of cases. In fact comparison is central to all the macro historical sciences -- including astronomy, geomorphology, botany, zoology, anthropology, and sociology -- where the subjects of study are complex, highly differentiated, and evolving structures that share features as well as having unique aspects. In these sciences every structure has to be compared either with ideal-typical classificatory categories and models or directly with other cases in order to be comprehended. Comparison can be all-encompassing, multiple, or just pair-wise. There has to be a comparison between cases, categories, and models in order to arrive at an adequate explanation of any case. Comparisons can draw upon analogies, similes, and even metaphors, as well as simple descriptions. No two cases are exactly alike and no case exactly fits a model so any attempt to explain deductively from a general law or principle is bound to be misleading. Nevertheless, *general* concepts, categories, hypotheses, analogies, and models are still necessary in all these sciences in order to think about, classify, and compare structures and kinds of entities and thus to begin to explain structures, functions, and history.⁹⁵

X RELATIONSHIP OF STRUCTURIST METHODOLOGY TO STRUCTURIST THEORY

As I have argued, a methodological doctrine does not logically have to imply or entail a sociological theory but in practice there is a close coherence between the two levels of concepts. That is, methodological individualism usually mutually implies sociological individualism in the sense of a general theory of society as consisting fundamentally or only of individual people whose behaviour is impelled by internal causes of an intentional and/or psychological kind. Methodological holism usually mutually implies a general theory that society is determinate on some macro level and within which people have little or no autonomy. Neither methodological doctrine would cohere easily, if at all, with the opposite social theory but methodological individualism would not necessarily

⁹⁵ On the role and structure of comparative method in socio-historical explanation see T. Skocpol and M. Somers, 'The Uses of Comparative History in Macrosocial Inquiry' (1980); the editor's contributions to Skocpol (ed), *Vision and Method in Historical Sociology* (1984); V.E. Bonnell, 'The Uses of Theory: Concepts and Comparisons in Historical Sociology' (1980); and P. McMichael, 'Incorporating Comparison Within a World-Historical Perspective: An Alternative Comparative Method' (1990).

rule out a form of sociological structuralism. This at least seems to be the argument of Jon Elster.⁹⁶

Methodological structuralism mutually implies the general theory of sociological structuralism. As I indicated above with some examples, this type of theory rejects the poles of individualism and holism as concepts of social reality and the social process. For structuralist theorists, society exists in a *dual* sense as agential people and the institutional structures that constrain people and which are the products of people collectively. In other words, this is a general theory that is resolutely *historical* in that it refers to the dialectical process in which the structure of society's institutionalised system of rules, roles, and relations is produced, reproduced, and transformed through the human thought and action which over time it enables and constrains. Society is both a structure and a *historical structuring process* of structurally-oriented (rather than individually-oriented) action.

Thus, without a structuralist theory of society and action there is no proper place for history and without history there is no genuine sociological explanation. There can be many particular forms of the general theory of sociological structuralism, depending on the explanatory tasks and objects for which they are required. However, there is a lack so far of *well confirmed* basic or pure theoretical propositions about the powers, causal relations, and history of the entities of the domain of structural history. Neo-Classical Economics does have some interesting basic propositions but they seem of little use to structural historians because of their abstractness, concern with behaviour rather than structures, and lack of historical confirmation. On the other hand, Institutional Economics has been a rich source of appropriate general historical hypotheses as found, for example, in the work of Marx, Weber, C.P. Kindleberger, Douglass North, Mancur Olson, and others. Some historical sociologists and social anthropologists, such as Max Weber, E. Gellner, M. Mann, J.A. Hall, C. Geertz, B. Nelson, B. Moore (to name but a few), have attempted to establish basic theoretical propositions but none has been well confirmed and all are now in eclipse. At the moment theoretical pluralism, even anarchy, reigns and is seen by many structural historians, sociologists, anthropologists, and even a few economists, as a virtue, even a necessity.⁹⁷

⁹⁶ J. Elster, 'Marxism, Functionalism, and Game Theory: the Case for Methodological Individualism' (1982).

⁹⁷ One of the main advocates of methodological pluralism has been Bruce Caldwell --- see the Bibliography for relevant writings.

XI TOWARD A SCIENTIFIC DOMAIN FOR STRUCTURAL HISTORY

In the context of this wide range of metaphysical, methodological, and theoretical issues an outline of a putative domain framework for social structural history can now begin to be constructed. The first task in establishing the specifications of the domain is to establish a broad conception of the domain entities -- i.e. historical social structures. I contend that this involves, among other things, showing the validity of including both economic and social history as correctly construed. My basic claim here is that the study of economic and social history should be one domain of enquiry fundamentally because economies and societies are not separate kinds of entities but are the same kinds of relational structures. Thus, the concepts of "economy" and "society" should be considered merely as *definitions* of parts of the same broad subject matter rather than descriptions of substantive natural differences in the entities and structures being dealt with. Can these alternatives indeed be transcended or perhaps synthesised to create a unified domain?

That is, the fundamental questions about the domain's entities concern

a) *composition*:

- the nature of economic and social (or socio-economic) structures;
- the relationship of structures to actions, events, and consciousness;
- the degree of autonomy of structures from patterns of events and from conscious understandings;

b) *evolution*:

- the diachronic and synchronic powers and tendencies of structures;
- the agents of structural history;
- the real history of structural evolution.

Good answers to these questions can only really be developed through empirical research and theory building, some examples of which I will discuss later, but all research can be conducted only within a framework of concepts and methodology. The relationship of a domain's specifying concepts and methodology to empirical enquiry is obviously crucial. As I have indicated, establishing a coherent and consensual domain framework is the result of a long process of empirical enquiry, theory-building, and concept formation. The process of discovery of the nature of structures, action, and historical processes cannot be the same as the deductive argument that reconstructs the logical relationship between the domain framework, theories, and empirical findings once the process of their establishment has occurred.

A coherent and consensual domain framework for and a well-confirmed general theory of structural history have not yet been established but answers to the above fundamental questions should take into account the following general considerations.

The Nature and Interrelationship of Societies and Economies

There have been and are many schools of thought about the fundamental natures of and connections between societies and economies. I have already mentioned the theoretical diversity with regard to social structure, which has been conceived in many individualist, holist, and structurist ways (see Section VI above). In economics, the dominant neo-classical tradition has conceived of the economy primarily as an autonomous realm of choices, action, and behaviour. Such a conception has led economists to try to model and/or explain the phenomena of production, distribution, exchange, and consumption as if they were organised patterns of individual and collective decisions, actions, and behaviour in which transactions or exchanges occur between rational sovereign individuals and groups in order to try to maximise satisfaction of material desires. In the tradition of classical liberalism, people are taken to be freely moving monads, devoid of institutional, social, ideological, or cultural prior determination. As such, being based on a form of methodological individualism, neo-classicism suffers from the attendant problems of such an approach. In particular, the major problems have been of how to account for the observed macro socio-behavioural pattern by reference almost exclusively to individual choices (and a narrow range of those), and how to account for structural change. Individualism has adopted theoretical assumptions of rationalist and behaviourist psychological postulates and so directed its empirical attention to series of data about behaviour and collective phenomena. The instrumentalist epistemology that underlies this methodological individualism was derived from 1950s logical positivism and, in keeping with the tenor of that era in most disciplines, drove out any realist structural alternative. Theories are constructed as abstract, formal, mathematical models which are often not intended to be descriptive or analytical of the real economy.⁹⁸ The power of unobservable but real structures of institutional relations and intellectual-cultural complexes; the complexities of human choice, decision-making, agency, and social relations; and the historicity of economies are all ignored in neo-classical theory. Behavioural postulates are axiomatic rather than problematic.

⁹⁸ On positivist economics see B.E. Seligman, 'The Impact of Positivism on Economic Thought' (1969); B. Caldwell, *Beyond Positivism* (1982).

The major alternative to liberal individualist economic orthodoxy over the past two centuries has been a broad stream of realist sociological and institutionalist theorising. This stream has conceptualised the economy either as an organic-like system of relations and interactions or as a social structure consisting of positions, rules, roles, and relations in which economic behaviour occurs. The major problems here have been of how to account for the actual connections between systems or structures, decisions, and behaviour, and to explain systemic or structural change.

The German Historical Economists of the second half of the 19th century had a realist institutionalist conception of the economy in which national economic and political interests, institutions, and policies played a prominent role. They opposed the abstraction, deductivism, and ahistoricalism of Menger and other Austrians of the late 19th century. However, their account of action and human agency was severely underdeveloped. Similarly, the contemporaneous English Historical Economists also wished to study the real world of institutions and their history. But again they lacked a viable theory of action that was alternative to the utilitarianism of classical economics and the emerging neo-classicalism of their time.

Marx's version of socio-political economics conceived of the economy as a structure of social relations centring on historically specific forms of property ownership and the power that flows from property. Economic phenomena and technological change were determined by the structure of socio-political institutions, including property relations and social classes, which in turn were the result of prior technological, institutional, and political developments. He had a well-developed concept of human agency in which the key role was played by classes acting as collective, self-conscious entities to further class interests. An ongoing transformational process was at the heart of Marx's theory so that for him there was no distinction between theoretical abstractions and the historicity of societies.

Like Marx, Max Weber had a relational conception of the economic structure but his theory of motivation and action went further than Marx's socio-political model of persons to incorporate cultural determination. For Weber, people are rational actors whose individual decisions must be studied by the sociologist and the economist but rationality is socio-culturally determined and not exogenous to the analysis. This approach has inspired a large following, including the Modernization School of economic development theorists, who have studied economic transformation as being a complex psychological, socio-cultural, and political process.

Institutionalist economics was long overshadowed or marginalised by neo-classicalism but has recently been revived.⁹⁹ The American Institutionalists of the late 19th and early 20th centuries, notably Veblen, Commons, Mitchell, and Ayres, also rejected the abstraction and simple behaviourism of classical and neo-classical economics and opted for studying the legal, ideological, socio-political, cultural, and organisational framework of economic behaviour. For them, the economy was the institutional framework and not some thing separate from it. In recent decades new versions of institutionalist economics have been developed, firstly by Karl Polanyi in the 1940s and 50s.¹⁰⁰ He defended and employed in a series of empirical and theoretical works the idea of the economy as an instituted process that was embedded in society and had significance only through these wider structural and functional relationships. The non-economic context is as important for the operation of the economy as the economic activities and relationships themselves.

Secondly, what is now called by some commentators the "New Institutional Economics" tries to combine aspects of the old version with neo-classical rational action theory. This version is therefore best called "Neo-classical Institutionalism". Douglass North and Mancur Olson,¹⁰¹ for example, draw a distinction between economic behaviour and the institutional structure -- that is, the institutions affect the economy but are not the economy itself. This means that the economy is defined, as in neo-classicalism, as a realm of individual rational decisions and actions but which are shaped by the institutions of ideology, property, and socio-political systems. Economic institutions are for them the rules of individual and collective rational behaviour and not sociological structures of positions, roles, relations, or classes.¹⁰² This rational action/institution distinction is incoherent because it assumes that action somehow occurs separately from the institutional structure. But if institutions exist and play a role in moulding or determining behaviour then behaviour must be considered as at least in part institutionally oriented rather than

⁹⁹ For discussions of the revival of institutionalism see the works of R. Langlois and G. Hodgson in the Bibliography, and P.D. Bush, 'Reflections on the Twenty-Fifth Anniversary of AFEE: Philosophical and Methodological Issues in Institutional Economics' (1991).

¹⁰⁰ See Bibliography for writings by Karl Polanyi. For discussions see M. Hechter, 'Karl Polanyi's Social Theory: A Critique' (1983); D.C. North, 'Markets and Other Allocation Systems in History: the Challenge of Karl Polanyi' (1977); A. Martinelli, 'The Economy as an Institutional Process' (1987); K. Polanyi-Levitt, *The Life and Work of Karl Polanyi* (1990).

¹⁰¹ See the Bibliography for the works of North and Olson.

¹⁰² cf North, *Institutions, Institutional Change, and Economic Performance* (1990), p. 3. He also draws a distinction between institutions and society.

utilitarian and then the behavioural foundation of neo-classical economics would be undermined.

Thirdly, an ultra new version of institutionalism that can best be labelled "Structurist Institutionalism" has recently been proposed, mainly by Geoffrey Hodgson,¹⁰³ who has rejected the polarity between action and structure that is inherent in neo-classicalism. He wishes to replace "rational economic man" with a more complex theory of motivation and action drawn from cognitive psychology which includes a role for ignorance, irrationality, habits, customs, and traditions. In his approach, economic systems of institutions are produced and reproduced in a dynamic evolving manner through the behaviour that grows out of this kind of sociological motivational complexity. People are social beings rather than narrow economic beings.

The economics 'discipline' has depended for its autonomy on assuming either that the economy is a natural kind of entity or system with relative autonomy from the wider society (i.e. an abstractionist move) or that all kinds of behaviour can be subsumed under or explained by concepts and theories drawn from economic discourse (i.e. a reductionist or a subsumptionist move). In both cases economics is taken to be the key social science around which all the others gravitate. This is part of the ruling ideology of the modern capitalist age. The fundamental problem with this ideology is that it rests on a contradiction, which the neo-classical institutionalists attempted to remedy but could not. The contradiction is that economics is in fact not a social science but a *behavioural* science (or putative science) and so cannot provide the core for a *social* science. Neo-classical economics brackets or rejects social analysis in favour of trying to explain collective *behavioural phenomena*. But it fails to explain satisfactorily those phenomena for it lacks adequate social and psychological causal theories, a defect the neo-classical institutionalists understood but could not overcome for they too retained the unjustified behaviour/institution distinction. Most practising economists rely upon largely unexamined behavioural postulates or assumptions and concentrate on making statistical correlations of behavioural aggregates rather than genuine socio-structural causal analyses. To make such analyses requires a framework of structural concepts and general theories of action and structure and their dynamic, historical interactions.

¹⁰³ G. Hodgson, *Economics and Institutions: A Manifesto for a Modern Institutional Economics* (1988), and 'Institutional Economic Theory: The Old Versus the New' (1989).

I am not claiming that there is no such thing as economic activity or economic relations or economic systems but rather that they should be understood as a sub-set of *social* activity, relations, and structures. Economic behaviour, like all behaviour, cannot be but social in its origin, orientation, and determination. The concept of the splendidly isolated, heroic (Robinsonade) actor moulding the world through rational choices and actions, although meant to be abstract, has been an irrational and unwarranted abstraction. Action is always structurally located, oriented, and impelled. Economic behaviour and structures have to be studied *sociologically* and not in abstraction from the wider social structure. Methodological individualist and reductionist economic theory has flourished, nevertheless, firstly because of its support for the egoism and aggressive material acquisitiveness of capitalism, which has successfully broken down or moulded to its own purposes all non-economic institutional barriers to economic accumulation, and, secondly, because of its apparent theoretical success in building an operational paradigm that seems to explain aggregate economic growth. But these aggregates that it examines are of course statistical artifacts of the economist's theory and methodology. In themselves such aggregates pose no necessary problem for the construction of a science of structural history providing they are not taken to represent *structural* reality. However, the explanatory success of orthodox economics is illusory because of the persistent failure to predict structural change and explain past structural change. The desire for prediction is not matched by results and the attempts at *post hoc* explanation are in fact *ad hoc* as well as radically incomplete because of the abstraction of the economy from the wider socio-cultural-political context.¹⁰⁴

One of the central problems resulting from economic abstraction and the desire for positive science is the *static* conception of its subject matter. The inherently dynamic and historical character of economic and social structures and behavioural patterns is bracketed in favour of static equilibrium analysis. This is not to deny the development of neo-classical, Schumpeterian, and Keynesian growth theories. But the problem of growth in the orthodox framework has been admitted largely by relaxing certain central assumptions of the equilibrium model rather than by being made the centre of analysis. Long-run structural change remains as a great difficulty for orthodox economics as underlined especially by the curiously ahistorical attempts to deal with it by the neo-classical institutionalists. The historicity that the 18th century Scots, 19th century

¹⁰⁴ As Ernest Gellner perceptively pointed out, "like physics, economics is more or less closed to the layman through being technical and counter intuitive; unlike physics, however, it does not repay this loss by presenting reliable and agreed results, or indeed by being manifestly superior in its judgments to those of the layman." *Thought and Change*, (1964), p. 175.

Germans, and late 19th century English Historical School understood as being central was lost in the 20th century attempt to establish a positive science. Economic history became for a time a separate discipline. Now, unfortunately, it is being reincorporated into economics as a form of applied theory rather than economic theory being subordinated to historical analysis. The North/Olson type of work shows the dangers of simply looking at the past from a standpoint resolutely in the present, trying only to explain outcomes rather than the realities of historical episodes and processes.

The failure of economic approaches to explain economic and other social phenomena and structures and their dynamics leads to the conclusion that there has to be a socio-institutional-historical approach to and theory of the economic sub-structure. I have already mentioned the important revival of institutional economics. There have also been several conspicuous attempts from the 1950s onwards to merge economics and sociology, that is, to develop a sociological economics and an economic sociology.

Versions of Sociological Economics

There are two basic strategies for unifying the discourses of economics and sociology, that is, incorporating either into the other to produce economic sociology or sociological economics. These are certainly not the same, although many commentators tend to confuse them. Economic sociology, strictly speaking, should attempt to apply the forms of reasoning of orthodox economics to socio-political phenomena and processes. That is, the utilitarian rational action model, market exchange theory, methodological individualism, and deductivism are all brought to bear on what is usually considered as non-economic behaviour such as voting, marriage, family relations, education, and crime.¹⁰⁵ The effect of this approach is actually to conceive of all behaviour as economic -- the whole of social life is examined as if narrow "economic rationality" prevailed everywhere.

Sociological economics, strictly speaking, should study economic phenomena and processes in a sociological manner. That is, the economic realm is considered as part of the social structure and so is characterised by the same kinds of institutions, organisations, social relations, social interactions, and historical social processes as the wider society. Nevertheless, there is considered to be an economic sub-structure or sub-system that is oriented toward certain kinds of material production and which can be abstracted to some extent from the totality but not ultimately explainable apart from the totality. Just as

¹⁰⁵ Cf G. Becker, *The Economic Approach to Human Behavior* (1976).

there is no dominant paradigm in sociology, unlike economics, so there is no broad consensus on methodology and theory in sociological economics.

Disregarding the misleading labels sometimes used, it is possible to identify several approaches to construct a sociological economics. As with so much in social science, Karl Marx's was probably the first sociological economics in that he developed a social relational conception of the economy but he was influenced in this by Adam Smith and the other members of the Scottish School. Economic relations and interactions were characterised by inequalities of social power that were institutionalised as class and property relations. Max Weber also had a sociological conception of the economy in which culture, class, and status dominated individual decisions and actions. Both Marx and Weber have inspired and continue to inspire many forms of neo-Marxist and Neo-Weberian sociological economics.

Perhaps the first major new version of sociological economics after Marx and Weber was that of Talcott Parsons and Neil Smelser whose *Economy and Society* (1956) and Neil Smelser's later work developed a theory of the relationship of the economy as a subsystem to the wider social system. The goal-seeking orientations of actors was the key causal element. This attempt was very influential and inspired a strong movement in the sociology of development from the 1950s centred mainly on the Modernization School and the journal *Economic Development and Cultural Change*. These people, including Bert Hoselitz, Wilbert Moore, and S.N. Eisenstadt, drew on Weberian, Parsonian, Schumpeterian, cultural-anthropological, and orthodox economic sources to construct their approach.¹⁰⁶ One of those influenced by this approach but who moved away to develop a more anthropological and cultural approach to understanding the economy, was Clifford Geertz, whom I shall discuss in detail in Chapter Three.

In recent years, Arthur Stinchcombe has systematically developed a new sociological economics, which he and other commentators call "economic sociology".¹⁰⁷ He has drawn on Marxian, Weberian, Polanyian, ecological, and other sources to construct a

¹⁰⁶ See the Bibliography for the works of these writers and my *Explanation in Social History* (1986), Ch.10, for a discussion.

¹⁰⁷ A. Stinchcombe, *Economic Sociology* (1983). For discussions of the new "paradigm", see R. Swedberg, 'Economic Sociology Past and Present' (1987) and *Economics and Sociology* (1990); A.. Martinelli and N.J. Smelser (eds), *Economy and Society* (1990); and S. Zukin and P. DiMaggio (eds), *The Structures of Capital: the Social Organization of the Economy* (1990), especially Chs.1, 2, and 3, which contain excellent overviews of the new "paradigm".

new paradigm that he and others hope will supersede both orthodox and institutional economics.

So, the argument I am trying to make here is in essence that the structural reality of the social world is not captured in theoretical conception by reductionist economic approaches or by any other form of reductionism or methodological individualism. Structures should be understood as *social* -- that is, as the shared rules, roles, relations, and meanings in which people necessarily live their lives and which, as generalised structures, pre-exist individuals and groups and organise their behaviour. Actions, events, and patterns of behaviour cannot be understood and explained except by reference to such structures as well as to problematic individual intentions, beliefs, and psychological imperatives. "Social structure" in general subsumes so-called economic and political structures; but within social structures there are of course many types of action and sub-structure, including economic and political ones, which are concerned with more specific objects, productions, and goals.

The Relationship of Structures to Events and Actions -- The Micro-Macro Problem

Thus any approach to explaining the history of social structures should deal with the crucial issue of micro-macro interdetermination. Economists and anthropologists have long seen the micro-macro nexus as crucial and developed general concepts of the micro and macro levels of their subject matter and of their interconnection. Some sociologists and political theorists have been deficient in this area and traditional historians have been on the whole very deficient. But now there is a great interest in the question from many quarters of the social studies for the problem has come to be seen, quite rightly, as central to all social enquiry.¹⁰⁸ The question is crucial because it bears directly upon the problem of social dynamics. What is the source of dynamism -- of innovation, change, evolution, history -- in societies?

Fundamentally, there are three broad approaches to providing theoretical solutions to the problem of the micro-macro connection -- individualist, holist, and structuralist. Individualists have defined the macro in ways that either deny its ontological reality or place it in a completely dependent relationship with the micro. For them the micro level consists of powerful, autonomous actors -- whether they be persons, integrated small groups, or legal entities such as firms -- who interact with and strive to make

¹⁰⁸ Discussions of the micro-macro problem in social science include the excellent collection of articles in J.C. Alexander *et al.*, *The Micro-Macro Link* (1987).

exchanges with other actors in order to satisfy wants. In doing so they individually and collectively bring into being structured macro patterns or organisation of behavioural interaction. In the simplest theory, when action and interaction are not occurring the macro pattern does not exist and so the structure does not exist. Strictly speaking, few theorists if any subscribe to such an ontological view but this pure form of reasoning is implicit in the work of some individualists. More common is the idea that methodologically only the micro individual level is studied in order to grasp the macro, which is dependent for its very existence on the constant creativity of individuals. Common in economics is the idea that while the macro phenomena of the whole economy must be studied they can only be understood as a collectivity of micro decisions and interactions by individuals who have stable dispositions and preferences. The source of dynamism in such an approach obviously has to come from the power of creative individuals who recreate the social world on a constantly ongoing and moment-by-moment basis. Any change in their pattern of behaviour comes from internal mental processes and conscious decisions to alter behaviour. The collective behavioural outcome is the result of a large number of decisions. The problem with this is that the account of how beliefs, understandings, and decisions are arrived at is deficient without an account of prior mental structures or forms of consciousness and understandings, which have to be shared and linguistically based. Once that is conceded, there arises the problem of the social origins of language and consciousness. "Social origins" implies prior existing social conditions that affect individuals. "Social conditions" implies a form of social power, which cannot be incorporated into an individualist approach.

On the other hand, holistic-social reasoning is fixated on the nature of social power. The macroscopic social system is enormously powerful in the holist orientation, so much so that it determines the micro level more or less completely. That is, actions, events, and the mental processes of individuals derive their existence and character from their place and role in the system. Dynamism within such a system is aberrant for the system has a powerful tendency toward static equilibrium. Actions and consciousness function to maintain the system and have significance only within that context.

With a structurist approach the problem of dynamism and history is central to the very conceptualisation of the macro and micro levels and their nexus. The macro structure is conceived as the rules, roles and relations that are the intended and unintended production *over time* of the conscious routine action and interaction of people conceived as social agents. Both macro and micro levels have power to influence the other but only people are genuine agents -- i.e. they have power to mould and alter the structural world.

The Degree of Autonomy and Temporality of Structures

Given the foregoing definitions, it follows that in the structuralist conception structures have a form of relative autonomy from events, actions, and consciousness and therefore also relative temporal continuity. In the individualist approach, structures have virtually no autonomy or temporality, and in the holist approach they have enormous autonomy and temporality. If the study of social history is to develop a coherent framework which delineates structural history from action history then the relative autonomy of structures and their spatial and temporal continuity have to be adequately conceptualised. Of course there has been much progress toward achieving this, examples of which will be discussed in the next two chapters.

The Powers and Tendencies of Structures

If social structures, as the structuralist methodology contends, are emergent systems of social rules, roles, and relations into which people are born and which must collectively be reproduced and occasionally transformed by human agents then they themselves are not agents. They do not have power to act to try to bring about derived outcomes and they do not have consciousness. However, in the cybernetic sense, they have a "memory" in that they have a powerful tendency toward maintaining temporal and spatial continuity. The fundamental power that they have, which is the basic index of their reality, is to mould and/or condition human action and thought. Humans cannot exist, act, or think except within structural contexts that enable and constrain their very existence, as well as their action and thought. Structures, not being agents, cannot make their own history, although they are all historical. Their historical powers and character arise through the actions and thought of human agents. Structures have within them a continuously shifting balance between diachronic and synchronic forces.

The Agents of Structural History

Thus humans individually and (much more so) in groups, classes, and institutions, are the agents of social structural history. This may seem very obvious but unfortunately structural history is sometimes studied and "explained" as if humans are the mere carriers and/or victims of social forces completely beyond their ken and control. This is a mistake, for it ignores the collective and unconscious structuring power that all people possess in all times and places in virtue of the social co-operative arrangements in which they live. The *self-consciousness* of agency and the *deliberate* exercise of structuring power collectively to build new institutions and social organisations may be rare, at least before the advent of modern society, but the exercise of agential power does not need to be conscious. People

necessarily structure the social and geographical environment in their daily lives. Agency does not imply political action for the latter is but a heightened, conscious form of the former.

The Real History of Structural Evolution

Finally in this sketch of components of the domain of structural history is the part devoted to studying the real history of structural evolution. It is here that general empirical concepts and scientific theory have their place, all the foregoing being concerned with methodology. Scientific domains are characterised by, among other things, encompassing theories that are able to explain, to the satisfaction of most people, the entities in the domain. Social structural history does not yet have a well confirmed, widely-accepted general theory. Rather, there are many competing theories that do not share the same general concepts and hypotheses of causal relationships.

Most of what follows in the dissertation is concerned directly or indirectly with elaborating the claims made for structurism and therefore establishing the general concepts and methodology of the domain of structural history.