To Alistair

Macroeconomic Thought

A Methodological Approach

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this set of theories admits of a particular range of policy proposals. Chapter 8 is devoted to exploring the policy proposals of each school of thought in three broad areas, but deals first with the approach of each school to deriving policy prescriptions from theory. Particular attention is then paid to the prescriptions generated for inflation/unemployment policy, techniques of money supply control, and international policy.

It is not uncommon for members of different schools of thought to come up with similar policy proposals, e.g. a recommendation to control the rate of growth of the money supply. But, because each makes the recommendation on the basis of a different view of the economy, there may be disagreement not only about the policy package of which it is a part b : also the techniques employed to implement it. It is the occasional *apparent* agreement on policy which makes continued theoretical arguments seem rather vexing. An understanding of the reasons for these arguments, however, could allow more effective communication between economists and policy-makers.

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Methodological Issues and Economics

2.1 Introduction

> Science tells us what we can know, but what we can know is little, and if we forget how much we cannot know we become insensitive to many things of great importance. ... Uncertainty, in the presence of vivid hopes and fears, is painful, but must be endured if we wish to live without the support of comforting fairytales. It is not good either to forget the questions that philosophy asks, or to persuade ourselves that we have found indubitable answers to them. To teach how to live without certainty, and yet without being paralysed by hesitation, is perhaps the chief thing that philosophy, in our age, can still do for those who study it. (Russell, 1946, p.14)

The formulation of theories in economics, as much as theories within philosophy, represents an attempt to deal with the fact that we can never attain a state of complete knowledge about the past, and even less about the future. Methodology is concerned with the way in which theories are formulated, the way in which knowledge is generated under conditions of uncertainty. It is thus concerned with theory formulation at a variety of levels. As individuals we must formulate theories (however subconsciously) about the environment in which we live in order to function at all, in order not to be 'paralysed with hesitation' (see Kelly, 1963). Economists in turn form theories about individual behaviour, and the consequences in aggregate of that behaviour. Methodologists variously form theories about how economists form their theories, establish prescriptions for how theory ought to be formed, and establish criteria for appraising and comparing theories. At each level there is an attempt to direct thought and investigation along lines which will generate as much knowledge as possible.

Methodology can be approached from a variety of standpoints, depending on the particular path chosen for generating knowledge. Here we will adopt simultaneously two conventional definitions of methodology, allowing it to span several levels: the study of 'the technical procedures of a discipline' as well as the 'investigation of the concepts, theories, and basic principles of a subject' (Blaug, 1980, p.xi). In other words, we will consider methodology as spanning the study of theory formulation at the technical level of model building and at the level of the underlying, and often implicit, world-view of the theorist. Indeed it is to be shown that the range of levels of thought involved are interdependent, that a particular world-view is generally associated with a particular technical approach to a subject.

Implicit in any methodology are criteria for theory appraisal, i.e. for prescription. We shall limit our enquiry to description of the content of different methodologies which correspond to different bodies of theory. Thus, rather than treating methodology as something which transcends all theory content, providing universal appraisal criteria, we will use it as a means of classifying bodies of theory.

In order to understand the issues addressed by methodologists, we have to go back one stage further to the very basic level of how people think, or believe they ought to think according to their implicit appraisal criteria. In the next section we identify two strands of thought which seem to have influenced methodologists as well as practitioners within disciplines like economics. Traditional methodology, or philosophy of science, has been more influenced by one of those modes of thought as the ideal for scientists. We explore the concerns of traditional methodology in the third section. Recent developments in the field of methodology along non-traditional lines can best be understood in terms of the other strand in Western thought on which we focus attention. The issues raised by this new approach are discussed in the following section. Finally, the nature of economics as a social science is explored to discover how the issues in both the traditional and new methodologies can be applied to economics.

The purpose of this chapter is to distil the breadth and complexity of methodological issues into those central issues which have ultimately influenced the development of thought in

economics. The cost of covering such a large literature in one chapter is related to its benefits: a great deal of detail must be sacrificed. This is necessary if we are to develop a view of the whole in such a way as to grasp the connection between mode of thought, methodology, theory and policy.

It is hoped that an understanding of some of the main issues will encourage the reader to pursue further areas of interest within the field. For recent treatments of methodology and economics, there is a range of texts, each with its own approach (see Hahn and Hollis, 1979; Blaug, 1980; Boland, 1982; Caldwell, 1982; Katouzian, 1980; Samuels, 1980). For a broader discussion at the level of philosophy of science there is a wider literature (see Toulmin, 1953; Nagel, 1961; Hempel, 1965; Suppe, 1974; Losee, 1972; Hacking, 1981; Newton-Smith, 1981). For those who find methodology somewhat inaccessible, the best introduction is Pirsig (1974); he uses a narrative, and thus relatively painless, approach to setting out his ideas on modes of thought.

2.2 Two Modes of Thought

By mode of thought is meant the way in which arguments (or theories) are constructed and presented, how we attempt to convince others of the validity or truth of our arguments. It is concerned as much with the rhetoric used as means of communication as with the logical structure of the argument. It is a broader concept still than 'methodology', and indeed influences our judgement as to what constitutes an acceptable methodological position.

At the general level of an argument on any subject, it may seem at first that there are simply good arguments and bad arguments. But a particular mode of thought is instilled in us from an early age, at least as being what we should aspire to. Education teaches us how to reason and how to organize our observations of the world. Unless we are already conscious of a conflict, say between the mode of thought which we prefer and another which we are being encouraged to adopt, it takes some effort to recognize our own mode of thought objectively and accept that there may be alternatives. Indeed some modes of thought more than others will limit the ability to admit the possibility of alternatives. Once it is admitted that there are alternatives, however, it must be accepted that what constitutes a good argument within one mode of thought may be a bad argument within another. At this stage we are not considering questions of the truth or falsity of arguments, or their correspondence with reality. We are concerned with the initial stage at which arguments are judged as to whether they are worthy of consideration at all.

Within the complex history of Western thought, we can identify in general terms two strands, or patterns, of thought which underlie the traditional and new approaches to methodology respectively, and which have echoes too throughout economic theory. These modes of thought encapsulate quite different ways of constructing arguments and of appraising theories. They are not opposites, nor are they all-encompassing; other patterns may also be identified. The purpose of focusing attention on only two modes of thought is to demonstrate the importance of awareness of mode of thought, as well as the other levels in the hierarchy (methodology, theory, policy prescription), if we are to understand debates in economics and use them constructively. Debates in macroeconomics have frequently stemmed from a lack of recognition that different participants were employing different modes of thought, or that there was a divergence between the mode of thought professed and the one used in practice.

The first, which we call the Cartesian/Euclidean mode of thought, is named, not by coincidence, after two mathematicians, the third-century BC Greek, Euclid, and the seventeenth-century Frenchman, Descartes. Both were path-breaking mathematicians who, through their geometric method, had a profound influence both on philosophy and on scientific method. The method involves establishing basic axioms, which are either true by definition or 'self evident', and using deductive logic to derive theorems, which are not self-evident. It is, however, only in mathematics that it is possible to establish incontestable axioms, because mathematics alone is a definitional system which can be pursued totally independently of observations of reality. The axiomatic method is aesthetically appealing because it allows a complete logical system to be constructed. Within this mode of thought, mathematics is thus regarded as the apex of scientific purity.

We will employ the term 'Cartesian/Euclidean' very broadly, to include all scientific thought influenced by the ideal of closed systems of axiomatic logic. The term is sometimes used strictly to refer to Descartes' or Euclid's mathematical method. But it is

retained here for much broader application on the grounds that this mathematical method has been retained as an ideal (not always explicitly) within traditional methodology. The fact that most discussion within traditional methodology has ranged around issues peculiar to applied science does not detract from the position adopted here, that these issues have been couched in terms of how to formulate a closed axiomatic system, however the axioms are arrived at, to apply to observable phenomena.

For applied sciences, the difficulty of identifying self-evident axioms is a perpetual problem, since they must have some anchor in observed reality. In economics, for example, the axioms of consumer rationality allow a wide range of theorems to be derived by deductive logic, but they cannot be regarded as self-evident as a universal representation of human behaviour (see Earl, 1983, pp.112–15). In the physical sciences, methodological developments have often been counterposed to the Cartesian method, in the sense that axioms were to be derived from empirical investigation, with all its attendant problems. But as long as the axiomatic systems approach is retained, we apply the term Cartesian/Euclidean. (See Losee (1972), chapter 8, for example, discussing Newton's method in relation to Descartes.)

The application of this axiomatic systems approach to generating knowledge within different disciplines has fostered some distinctive features of this mode of thought, which include the following:

1. Reductionism (or atomism). Because the entire logical structure depends on the basic axioms, it is important to make them as widely acceptable as possible, i.e. as close as possible an approximation to being 'self-evident'. As a result, propositions are broken down into their smallest components, such that one set of axioms can be identified from which all propositions can be derived by means of deduction. The term 'atomism' contains a clear reference to physics, at the stage when atoms were the smallest physical units to be identified. If all could agree on the nature of atoms, then the nature of molecules established by applying a logical chain of reasoning to the nature of atoms is likely also to attract agreement. Within economics, reductionism requires that basic axioms refer to the smallest unit of enquiry, i.e. the individual (although the application of psychology or microbiology might justify even further reduction to the determinants of human behaviour).

2. Dualism. Perhaps the most important feature, and one which permeates all aspects of analysis is dualism. Dualism is the propensity to classify concepts, statements and events according to duals, as belonging to only one of two all-encompassing categories: true or false, logical or illogical, positive or normative, fact or opinion, and so on. Reliance on duals has a strong tradition in the rather different debate within Greek philosophy (to which Euclid subscribed) as to whether mind/soul could be separated from matter, or the mental/spiritual from the material. Indeed virtue was associated with cultivating the mind/soul at the expense of matter. Since truth and logic were the products of the pure application of the mind/soul, their pursuit was thus also virtuous. To this day, arguments identified as deriving from matter (emotional, normative, opinionated or non-rigorous argument, for example) are conventionally regarded as being almost morally inferior to their opposites.

The second mode of thought which we consider has not represented an ideal within Western philosophy of science, and as such its origins, development and characteristics are less clearly established. Rather than using a linear system of logical deduction from basic axioms, this mode of thought starts from the view that it is impossible in general to establish watertight axioms and points to the way in which axiomatic error is compounded by each link in the deductive chain of logic. The alternative approach is to employ several strands of argument which have different starting points and which, in a successful theory, reinforce each other; any argument, therefore, does not stand or fall on the acceptability of any one set of axioms. Knowledge is generated by practical applications of theories as examples, using a variety of methods.

Various traditions can be identified as having something in common with the mode of thought described here (pragmatism and structuralism, for example). Its historical origins have been described variously as Babylonian (see Feynman, 1965, for the original usage, and also Wimsatt, 1981, and Stohs, 1983) as well as Stoic and Roman (see Macfie, 1955). The Babylonian tradition can refer both to the non-axiomatic style of mathematical reasoning (see Feynman, 1965) as well as to the rabbinical tradition represented by the Babylonian Talmud. More generally, this style of reasoning is found typically in theological and legal argument. We will use the term 'Babylonian' deliberately as a relatively new term

which does not pick up baggage from past debates. In fact, the dominance of the Cartesian/Euclidean style of reasoning has tended to colour the formulation and presentation of alternative modes of thought, particularly in perpetuating dualism. Babylonian thought is presented here not as 'not-Cartesian/Euclidean thought' (which would constitute a dual) but as something different.

A non-economic example which illustrates the differences between this approach and the Cartesian/Euclidean approach is the debate over the origins of the universe: what they are, and whether it is a useful question in the first place. Physicists and 'creationist' theologians who are concerned with identifying these origins are using reductionist, Cartesian/Euclidean thinking, presuming that all useful (physical or theological) results can be derived from the axioms which define the origins of the universe. Others who employ a more Babylonian style of thinking regard the limitations on knowledge as being so great that agreement on the origins of the universe would not be sufficient to allow conclusions to be drawn about contemporary events by means of deductive reasoning. The chain of logic would be so long, requiring so many ancillary assumptions, that any error would be seriously compounded by the time theorems emerged.

Rather, it is regarded as preferable to start with contemporary problems, choosing starting points for reasoning which are best suited to these problems. The evolution of the universe and God's rôle in it are regarded as so complex that it is inconceivable that human minds could capture it in a *complete* system of deductive logic. Each line of enquiry then is limited; there is no necessity for each to have the same starting point. This does not mean that Babylonian thought disregards logic. Rather, logic is applied within partial systems. Two lines of argument may have conflicting assumptions or conclusions. But the conflict is not a logical one if it simply reflects different choices as to which part of the system is chosen for enquiry. In Simon's (1955) terms, rationality for the scientist, as for the entrepreneur, is bounded. Knowledge can be generated by decomposing systems, but that decomposition is not universally applicable; it 'depends upon a limited range of environments and is likely to fail in critical instances' (Loasby, 1983, p.405). The scientist should thus be prepared to alter the decomposition (employ different chains of logic) in different circumstances.

While the examples which spring to mind of Cartesian/

Euclidean thought tend to be mathematical, the examples of Babylonian thought tend to be more 'applied'. For example, if one were debating the relative merits of cutbacks in financial provision for the university system, a whole range of arguments would be presented, referring to the benefits to society of higher education, comparisons with other countries, historical analysis of the role of the university sector, and so on. Indeed this is the way in which arguments are commonly conducted verbally, even by those whose papers written for academic journals take on a distinctly Cartesian/Euclidean air (see McCloskey, 1983).

Since the ability to agree on any one set of axioms is doubted within the Babylonian approach, there is no particular incentive to make the axioms as narrow as possible. Indeed, since Babylonian arguments or theories can draw on a range of facets of a system, depending on which is singled out for particular attention (physical, economic, political, or whatever) it is more useful to focus on the nature of the system as a whole. Rather than being reductionist or atomistic, this approach is 'holistic'. A Cartesian/Euclidean system of thought is bound together by the set of axioms from which all theorems are derived. Babylonian thought is holistic in the sense that the binding factor of theories is a general perception of how the system as a whole works. Different bodies of theory will reflect different choices as to which facets of the system to concentrate on, derived from different perceptions as to how the system works, no one body of theory purporting to present a complete, closed system. Thus some economists might see the whole in terms of market relationships, and study some particular aspect of markets, while others see the whole in terms of power relationships and consider different aspects of the economy in that light.

As a corollary, no axiom is basic to the Babylonian system, and some theorems may be axioms in other parts of the system. If the same theorem crops up as a result of applying logic to axioms derived from different parts of the system, then the acceptability of that theorem is increased *because* it is not dependent on the validity of any one set of axioms. A parallel argument was used by Keynes (1973a) about conclusions to be drawn from statistical inference where observations reflect an absence of *ceteris paribus*. If a conjunction of events persisted *in spite of* a changing economic structure, the probability of a causal relationship increased (see Dow and Dow, 1985).

The resulting theoretical structure discourages the use of strict, universal duals. Rather than limiting analysis to one side of the dual of truth/falsity, logical/illogical, and so on, the Babylonian approach starts from the presumption that any chain of reasoning has shortcomings when applied to an existing reality. The Cartesian/Euclidean approach concentrates on the elimination of error; a system of thought is limited to one which eliminates error. The Babylonian approach designs a system of thought in such a way as to deal with error, where error is not logical error, but the result of the uncertainty with which most knowledge is held. Such an approach involves 'hedging', in order to reduce dependence on a particular line of enquiry, just as firms hedge with respect to production processes and product lines (see Kay, 1982). The result is less aesthetically appealing because the system cannot be closed. But while Euclid regarded usefulness as irrelevant to mathematics, the Babylonians, Romans and Stoics governed their enquiries by the uses to which conclusions, or theorems, could be put: usefulness here means the ability to deal with a wide range of practical problems, in spite of the difficulties in establishing axioms. The particular collection of intertwining chains of reasoning which are employed will differ depending on the problem at hand. While the Cartesian/Euclidean mode of thought is universal in terms of its structure, the Babylonian mode of thought is universal only in the sense of its adaptability in the face of different problems.

These observations of necessity (because of their brevity) have been selective in focusing on two broad strands within Western thought. But, as we turn now to discuss the philosophy of science, we shall see how influential these modes of thought have been. First we look at the traditional philosophy of science, which has generally formulated its ideals according to Cartesian/Euclidean principles. Then we look at more recent developments in the philosophy of science, which are grounded more in what we have called Babylonian tradition.

2.3 Traditional Methodological Issues

Methodology has traditionally been concerned with defining good scientific enquiry, and thus specifying a dividing line between science and non-science (note the dual). The Cartesian/Euclidean pattern of theorems logically derivable from a unifying set of axioms was taken as the basic requirement, whether the starting point of *enquiry* was observation or axioms. But in applying that framework in any area other than mathematics, the fundamental question was how to connect that chain of reasoning to reality. How were the basic axioms to be chosen? How were the axioms and theorems to be assessed as representations of reality?

The history of methodology in this mode of thought can be seen primarily as a debate about the relative merits of deduction and induction (another dual). We have already met deduction as the classic method of logic in the Cartesian/Euclidean tradition. It involves applying logic to some general law, or axiom, possibly in conjunction with some initial conditions, to derive particular theorems. It is the type of logic employed in arriving at *predictions*. Induction starts at the other end of the chain; particular conjunctions of events are observed to occur and, if these conjunctions are taken to be causally connected (and expressed as theorems), logic is applied to work backward towards the axioms. Thus, for example, if the velocity of circulation of money is observed to fall over a long period during which income is rising, an economist using the general equilibrium system, working back to the axioms of rational individual behaviour, could conclude that money is a luxury good. Induction, further, can provide a means of dealing with the difficulty in some sciences in establishing basic axioms as the first stage in analysis. In economics, observation of consistent negative relationships between demand and price, for example, can be used to *justify* the axioms of consumer rationality (whether or not any claim is being made as to the empirical validity of axioms themselves, other than their ability to explain observed conjunctions of events). Inductive logic is best suited to *explanation*, rather than prediction; the initial step is to hypothesize that conjunction of events involves causality.

Each method is subject to serious limitations. The problem of deduction derives from its dependence on the validity of the general laws or axioms from which the theorem is deduced. Validity of the deductive logic itself does not immunize the argument from the need for demonstrating its empirical validity. The problem of induction is that conjunction of events may not in fact reflect causality, so that different sets of observations could suggest different conjunctions.

Deductive logic applied to a simple monetarist model could predict that, given the assumptions of that model, and certain initial conditions being satisfied, a 10 per cent increase in the money supply this year will lead to 10 per cent inflation in, say, 18 months' time. But not all economists accept the assumptions of the monetarist model, or agree that the initial conditions are met, so there will be no agreement on the value of the prediction. And if the prediction proves to be incorrect, the 'axioms' of the monetarist model are so complex that it is generally impossible to identify which is the faulty axiom. This problem is inevitable with any applied discipline whose theorems refer to real rather than conceptual events. The organization of observations by which the validity of theorems is assessed is a product of the theoretical structure which generated the theorems (see Duhem, 1906; Quine, 1953). Thus, even if axioms are to be immune from assessment in the light of observation, there is no conclusive method by which theorems can be shown empirically to be true. They can only be shown to be confirmed by a particular set of observations.

Induction in turn is subject to a drawback arising also from the limitations to objective observation. Suppose inflation does correspond consistently to the rate of change in the money supply. Induction could suggest that something like the monetarist theory holds, but it does not *prove* that the monetarist theory is in some sense true. The conjunction of rates of inflation with similar rates of growth in the money supply could be explained by reasons totally unconnected with the monetarist model. The classic example of the problem of induction is to generalize from the observation of several white swans that all swans are white, although there are in fact black swans. Most uses of induction, however, require that a causal mechanism be imputed which would allow explanation of a conjunction of events (rather than simply a classification system).

Within the Cartesian/Euclidean mode of thought, in applied fields, induction and deduction are often combined to deal with the limitations on each. Observations are made with a particular system of deductive logic in mind. The observed conjunction of events is then explained in terms of that system, adapting it if necessary by means of induction. The adapted deductive system may then be employed to make predictions. Philosophers of science, on the other hand, have demonstrated a greater tendency to think in terms of duals. Given the acknowledged limitations on both induction and deduction, they have tended to opt for one or the other as the ideal for which scientists should strive. The inevitable problems resulting from such reliance on one would eventually justify a reversal in favour of the other.

The initial preference, arising from the Greeks, was for pure deduction. Plato maintained that the only true knowledge was available through the mind, not through the senses:

> there is nothing worthy to be called 'knowledge' to be derived from the senses, and .. the only real knowledge has to do with concepts. In (Plato's) view, '2+2=4' is genuine knowledge, but such a statement as 'snow is white' is so full of ambiguity and uncertainty that it cannot find a place in the philosopher's compass of thoughts. (Russell, 1946, p.163)

This preference for deduction persisted through the Middle Ages, reinforced by a theological preference for deduction from axioms which took the form of statements of faith. Scientific discoveries in the sixteenth century appeared to challenge some of the articles of faith, and provided the impetus for further questioning of the traditional scientific method of deduction. In particular, the argument that the world is round rather than flat, based on empirical evidence, raised new questions about the relation between theory and observation. Descartes' response in taking up this philosophical challenge was to question all axioms. He concluded (in accordance with Plato) that his senses were too imperfect a basis on which to rely; the only certain knowledge was his knowledge of his own thought: 'I think therefore I am.' From this position, Descartes reinforced the dualism between conceptual knowledge, and observation using the senses, between mind and matter.

In Britain, however, there was the opposite response led, among others, by the eighteenth-century philosopher-economist Hume. Hume also emphasized the subjectivity of perception but, rather than concluding that only the mind and not the senses be consulted, he maintained that the only potential source of true knowledge was the observation of matter. Given the logical inability of induction to allow general laws of cause and effect to be derived from limited observations, however, Hume was extremely skeptical of any rational basis for general laws. But he influenced the emergence of empiricism, the use of observation and experiment as a basis for science. It must be noted that empiricism remained within the Cartesian/Euclidean framework. It simply changed the basis on which axioms were established; what was self-evident in an applied science generally required more explicit reference to the real world than did mathematics.

While empiricism and the inductive method were dominant in British philosophy of science until late in the nineteenth century, the deductive method was reinforced on the continent by the work of the eighteenth-century German philosopher, Kant. He divided knowledge into the dual of *a priori* knowledge and *a posteriori* knowledge. *A priori* knowledge is innate or self-evident knowledge, which is brought to the surface of the mind by observation; it is the source of the axioms, or general laws, of theory. This knowledge includes not only ideas such as that '2+2=4', but also causal relations; causation is the concept by which we organize our observations. *A posteriori* knowledge refers to empirical facts of which we can have no innate knowledge, e.g. that the rate of unemployment was at a particular level in a particular year. Such facts may constitute the predictions of theories, i.e. the theorems by which theories are tested. This approach provides justification for the deductive method as applied to axioms based on *a priori* knowledge (albeit elicited by empirical observation).

For the physical sciences, the necessity for extensive observation to elicit *a priori* knowledge in effect meant that induction from experimental information was still required. However, we shall see that in the social sciences, such as economics, the difficulty of collecting data by experiment was one of the factors which led to a continued emphasis on deduction. Within economics, debates between the inductivists and the deductivists could be extreme, where participants insisted that a choice be made between induction and deduction. A notable case was the methodological debate in the 1870s and 1880s between the Austrian subjectivists led by Menger (1883), who favoured deduction from subjectively derived axioms, and the German historical school, led by Roscher and Schmoller who favoured induction (see Schumpeter, 1954, pp.814–15 and section 3.3 below).

From Kant also came the notion that statements could be classified according to the dual of analytic statements and synthetic statements. The former are those whose truth or falsity depends on pure logic, and the definition of terms, such as 'all white swans are swans' or the Quantity Theory equation, MV = PT; all other statements are synthetic and their truth or falsity depends on their consistency with observed facts. It was thus concluded that meaningful statements must either be analytic, or be synthetic and *capable of* checking against facts, i.e. of being verified. This specification of what is and is not meaningful is what generally became known by the term 'logical positivism'. It is the logical consequence of the Cartesian/Euclidean mode of thought when applied beyond the field of pure mathematics (see Quine, 1953).

This 'verifiability principle' provided a criterion for identifying science (as opposed to non-science), as generating meaningful statements. These statements were generated by means of the hypothetico-deductive method: hypotheses were constructed on the basis of observations, from which theorems could be derived and tested against further observations. At a general level, this view of science was taken as an attack on any metaphysical, or normative, content in knowledge. It also threw up the symmetry thesis, that induction (from observations to hypotheses) is symmetrical to deduction (from hypotheses to testable theorems), where both are employed within the same axiomatic system. In practice, however, that symmetry seemed to be violated. Newton's theory of gravity, for example, provided an excellent basis for prediction of the outcome of falling bodies. But Newton's law of gravitation is a purely mathematical law; it does not yield a plausible explanation in the form of a mechanism of gravitation. In contrast, Darwin's theory of evolution provides an explanation of the survival of some species at the expense of others in terms of a mechanism: those species not suited to their environment die out. The theory does not allow prediction of which species will survive in the future. In principle, if we knew more about gravity and about natural selection, we could perhaps explain and predict with both theories. But we construct theories precisely because we do *not* know everything.

But, more important for the philosophy of science, logical positivism also highlighted the role of theory testing. If verification were the criterion for continuing to accept theories, absence of verification should suggest theory rejection. But, as Duhem (1906) (and, later, Quine, 1953) pointed out, it is not at all straightforward to know what to reject (see, further, Harding, 1976). Rather than theories being of the simple form 'if A then B', they are generally complex compendia of a range of axioms and initial conditions. In the simple monetarist example given earlier, a 10 per cent rise in the money supply was predicted to be followed by a 10 per cent rate of inflation 18 months later. If in fact inflation turned out to be 12 per cent, does that justify rejecting the entire monetarist model? It could be that the monetarist model is correct, but that the initial conditions of the hypothesis were not met; or that the money supply definition chosen was not the

correct one; or that the hypothesis of expectations formation is flawed; or that portfolio preferences cannot be represented by twice differentiable functions; and so on. This problem implied that only the simplest theorems were capable of verification, and thus meaningful by the logical positivist criterion. Duhem thus advocated relative simplicity as a means of choosing between theories; otherwise a retreat into deductivism might be implied.

The capacity to test theories was further thrown into doubt by the inescapable logical problem of induction; verification did not guarantee necessary causation. If the theory is of the form 'if A then B', and B is found to be true, then A need not be true; in fact C might be true instead where 'if C then B' is the case. The rescue of logical positivism was attempted in this respect by one of its critics, Popper, who in many ways spearheaded the new developments in methodology with which we deal in the next section. Whereas the prevailing, conventionalist approach was to adopt conventions for inferring propositions from induction in as satisfactory a way as possible *given* the problem of induction, Popper sought to provide an alternative solution which avoided the problem of induction. (See Boland, 1982, for a critical discussion of conventionalism.) Popper (1959) argued that the only watertight empirical test did not involve verification, but involved falsification. As long as observations are taken as true statements, the only real knowledge we have is that certain theories are *not* true; that if 'if A then B' is implied by deductive logic, and B is found not to be true, then A cannot be true either. (If at the same time B has been shown to follow from C, then C also is shown to be false.)

Falsifiability constitutes Popper's criterion for identifying scientific statements. He set out a series of rules of procedure for framing theorems for testing. First, theorems should be put forward on the basis of 'bold conjecture' rather than observation. The remaining procedural rules concentrated on precluding defensive strategies on the part of those scientists attempting to ensure a non-falsification result. For example, if a macroeconomic theory failed to account for the 1930s Depression, it was not legitimate then to exclude the Depression years from the theoretical statement. (See Hendry and Eriksson (1983), for a critique of Friedman and Schwartz (1982) along these lines.) If theories are to explain and predict, Popper urged that a falsification should provoke a modification of theory to incorporate this new evidence; science thus progressed by new theories (dealing with these anomalies) which of necessity conflicted with old theories. If tests were strictly specified, then an absence of falsification could be taken as corroboration (though still not proof) of the validity of the theory, although the problem of induction remains. Ultimately, a scientist should specify precisely what evidence would lead her to reject the theory outright.

For economics, this is a particularly exacting criterion for scientific behaviour, since observations are rarely if ever available in controlled circumstances. So many features of the economy are liable to unpredictable change of some degree that the range of possibilities is difficult to anticipate before a theory is tested. Nevertheless, some form of Popper's falsification principle is widely put forward in economics as the preferred method of testing. The relevance of the Duhem-Quine thesis, which questioned the ability of falsification procedures to deal with a complex body of theory, has been sufficiently strong to limit even further the applicability of Popper's principles to economics; Popper provides inadequate guidance outside the realm of falsifiable statements. Indeed, as a result, economic practice has diverged markedly from what is *professed* to be the preferred methodology (see Blaug, 1980).

Popper's thought reflects elements of the Cartesian/Euclidean tradition, specifying a demarcation between science and non-science, and viewing the philosophy of science as normative, setting out a code for acceptable scientific practice, in terms of formulating theorems for testing, if not the formulation of theories themselves. His notion of scientific progress is internalist, and rational; progress occurs according to scientific rationality (although hypothesis-by-conjecture raises questions about how far the *source* of conjectures is rational). But within this context, his enquiries led him in a direction which influenced others approaching the philosophy of science in a new way. First, he arrived at his code of behaviour in reaction to his observation of how scientists actually behaved, in particular that they were strongly motivated to protect their theories from falsification in a way which could inhibit the growth of knowledge. Second, part of that code of behaviour was that different scientists should maintain several competing theories at any one time as possibilities not yet falsified, on the grounds that the incentive to falsify competing theories would speed up the process of arriving at theories which resisted falsification, i.e. were closest to the truth.

Before proceeding to discuss the 'new' methodology, it is perhaps useful at this stage to summarize in point form some of the features of the philosophy of science as it developed under the influence of the Cartesian/Euclidean mode of thought. These points can be regarded as the views of a 'representative' traditional methodologist (although each point has been a source of considerable debate). (The list is based on Hacking (1981), pp.1-2.)

1. A line of demarcation can be drawn between science and non-science.
2. Science is cumulative, adding to knowledge over time (whatever counts as knowledge).
3. A distinction can be drawn between observation and theory.
4. Scientific concepts are precise in the sense of 'susceptible to mathematical expression' and scientific terms have fixed meanings.
5. The logical basis for justifying acceptance of a theory can be distinguished from the circumstances in which the theory was formulated.
6. There is one scientific method, so that in principle all sciences are part of a single scientific structure.

The final three features below refer to the connection between theory and observation. As we shall see in the fifth section, this relationship is of a different nature in the social sciences than in the physical sciences. But, since economics has been influenced so much by criteria set for the physical sciences, it is worthwhile to note these three features:

7. There is one best description of any aspect of the real world.
8. Observations and experiment provide the foundations for and justification of hypotheses and theories.
9. Theories have a deductive structure in their expression, if not in their discovery (which may be inductive); they are tested by deducing observation reports from theoretical postulates.

It should be noted that the philosophy of science, within this mode of thought, is itself frequently regarded as a science (or at least as a metascience, science on a different level). By principle 6 above, if it is part of the unity of science, it must itself display the other features listed above; any other type of methodology must be classified as unscientific.

2.4 Issues in the New Methodology

The 'new methodology' can be defined relative to the traditional methodology, differing over all of the above features of the traditional methodology, both as a prescription for scientific behaviour, and as a description of it. Along the lines of Babylonian thought, it was argued that the induction–deduction dual was subject to serious logical problems. But, in addition, the scientist could not be separated from her environment and thought-conditioning either to form universal general laws *a priori*, or to observe real-world events other than in terms of her mode of thought in general, and theoretical stance in particular. Further, the complexity of evolving systems is such that it is unlikely that universal laws could actually be identified, even in principle.

The skepticism from which this methodological approach emerged was fuelled by non-Euclidean developments in mathematics and by developments within physics which seemed to overthrow what had been taken as general laws: Einstein's theory of relativity, and Planck's quantum physics (see Canterbery, 1976, chapter 8). These new theories not only demonstrated that what seemed to be general laws at one time could be superseded by more general laws at a later date. But also their content raised questions about relativism in theory-formulation itself.

Laudan (1977) identifies the resulting change of view in the theory of knowledge (epistemology) as dating from the 1920s, due to the perception which became widely held, that knowledge was not certain or immutable. (The perception itself was not new, having been held by Hume and Adam Smith, for example.) At a sociological level, in Western Europe, the First World War initiated a new questioning of traditional ideas, particularly the notion of cumulative progress across a variety of spheres.

But it was not until the 1960s that a distinctive methodological approach developed out of the older notion of 'theory-laden facts' (see Hanson, 1965). The central figure in this group (who in turn stands on the shoulders, as it were, of Duhem and Quine) is Kuhn (1962), whose *Structure of Scientific Revolutions* sparked off further work in the area, some developing his ideas, others attempting to reformulate them within the traditional methodological framework.(see also Kuhn, 1974). Kuhn's theory is addressed to

the observation, unexplained by traditional theory, that falsification does not in practice lead to theory rejection. Indeed any individual theory is part of a theoretical structure; it is not separable as a testable entity. And yet, historically, theory structures *have* been overthrown, and replaced with new structures, for reasons other than falsification, which Kuhn undertook to explore.

Kuhn's style of reasoning has Babylonian features. He took a broad, 'system' approach to the questions at hand, exploring not only the 'internal, rational', scientific environment, but also the sociological, and above all historical 'external' environment, in which scientific discoveries were made. He showed in the process that the traditional duals (rational/irrational, and internal/external) are in fact inappropriate distinctions in this context. In other words, the process of scientific discovery, or of changing theoretical structures, is inherently a part of its broader environment. Approaching methodology more as a historian of science than as a philosopher of science, Kuhn emphasized the historical particularity of scientific developments, and concluded that it was not possible to identify any necessary trend of scientific progress over time, other than from the viewpoint of any one school of thought.

The central concept he employed was the paradigm, or 'disciplinary matrix'. This was a concept broad enough to encompass all aspects of a theoretical structure, ranging from practical techniques of analysis to the underlying world-view and mode of thought of the scientist. Indeed, his conception of a paradigm is commensurate with the definition we are using here of methodology. Its strength lies in its application simultaneously to several levels of a theoretical structure. Kuhn explained the paradigm concept by means of historical examples, demonstrating the range of possible applications. He was as a result widely criticized for vagueness (see the debate in Lakatos and Musgrave, 1970). According to the principles of traditional methodology listed above at the end of section 2.3 (particularly item 4), the paradigm concept could not be scientific. But the reaction to Kuhn, extending well beyond questions of pure logic, has amply demonstrated the power and range of the paradigm concept. Kuhn's reply to his critics itself provides an example of the necessary initial stage in a debate, of separating arguments of substance (expressed within a common framework) from arguments arising from lack of communication across frameworks.

Paradigms are identified with the community of scientists who practice them (although individuals may participate in more than one group; Kuhn's psychology is social rather than atomistic psychology). The common mode of thought and theoretical structure allow communication among members of the group, as well as a means of appraising scientific discoveries within the paradigm. The paradigm is transmitted by 'exemplars' or examples; far from relying solely on the persuasiveness of logic as laid down in traditional methodology, scientists in practice employ such means to convey the paradigm's way of approaching questions without necessarily making the underlying theoretical structure explicit. Activity within the paradigm consists of 'normal science', which involves incorporating new discoveries which are compatible with the paradigm.

'Extraordinary science', which involves questioning the basis of the theoretical structure, occurs as a prelude to crisis, when a new paradigm may supersede the old as a result of a revolution. This crisis occurs, not because of the falsification of any part of the old paradigm, but as a result of a widespread perception that it has failed to address an important problem. Because the new paradigm, by definition, consists of a complex structure based on a different world view from the old paradigm, using different techniques and language, there is no basis on which the two can be compared, or on which any decision can be made as to whether scientific progress has occurred or not. The *process* of revolution is not discontinuous in that the anomalies within the old paradigm must have been the subject of communication in terms of the language of the new paradigm. The discontinuity refers rather to the absence of a neutral 'paradigm', by which to compare the old and new paradigms.

Kuhn's position was rendered ambiguous, however, when he set out five criteria by which theories may be appraised, referring to the following characteristics of theories: accuracy, consistency, breadth of scope, simplicity and fruitfulness (see Kuhn, 1977, p.297). Although he provided no rational justification for these criteria, the fact that they were put forward as something transcending individual paradigms appears to weaken the incommensurability of paradigms and at the same time opens the door to the *development* of a rationale for universal criteria. Implicit also in Kuhn's work is the perception of a universal goal for science which transcends paradigms. Newton-Smith (1981, chapter 5) on these grounds argues that Kuhn is not so far from being a rationalist (who adopts a universal goal of science and appraisal criteria) as he had at first seemed.

No matter how far Kuhn himself recognized any ambiguity in his own position, it is possible still to present a Kuhnian methodology which is both distinctive and internally consistent. First, it is possible (and indeed probable) that several paradigms will hold some appraisal criteria and goals in common. But as long as some criteria and goals differ, or are employed with different weights or with different interpretations, the paradigms are incommensurate; there is no extra-paradigmatic basis for comparison. Second, any methodologist will have her own preferred set of goals and appraisal criteria; any discussion must be coloured by that fact. The best that can be done is that these preferences be made explicit, with recognition that they are paradigm-specific. Kuhn has misled readers, to the extent that he had not always displayed that recognition with respect to himself. Johnson (1983) has pointed out that the features which define a paradigm should explicitly include a 'purposive function', or set of goals, to highlight this important feature of incommensurability.

To recapitulate, Kuhn's theory suggested that scientific practice has thrown up a multiplicity of scientific methods. One supersedes another for reasons other than what is traditionally regarded as scientific rationality, and it is impossible to identify objectively (i.e. independently of any paradigm) any one change as representing progress or regress. This latter conclusion was perhaps the hardest for many to accept, since it seemed to lead to relativism, or nihilism.

Lakatos's work can be interpreted as an attempt to bridge the gap between Kuhn and Popper, providing an empirical criterion for appraising theories. Lakatos's (1970, 1981) approach appears on the surface to have a lot in common with Kuhn. He explained the fact that scientists retained parts of their theories which had been falsified by Popper's criterion by means of the concept of 'research programmes'. These are theoretical structures, consisting of a hard core, the basic unquestioned principles of the programme, and the protective belt of theories derived from the hard core. Anomalies might persist until some new theory emerged to deal with them; activity within the programme is directed by its positive heuristic (its agenda of problems to be solved and the methods used to solve them) rather than by anomalies as such. New research programmes emerge, but rather than being incommensurable, they will generally incorporate much of the content of the previous one, allowing a more or less continuous evolution. The criterion by which one research programme is appraised is whether or not it deals with anomalies with new theories designed to explain them; thus a research programme is progressive if it deals with more novel facts than competing programmes, and degenerating if it protects the hard core from novel facts by *ad hoc* adjustments. The criterion is historically specific, in the sense that what constitutes a novel fact is historically specific.

Lakatos restored the traditional notion of continuity of scientific progress, which occurs by application of an internal criterion of scientific rationality. Checking Lakatos off against the list of features of traditional methodology identifies him as leaning towards the 'old' methodology. He had moved towards Kuhn by accommodating the persistence of research programmes despite anomalies. But, by aiming to specify what ultimately determines the decision to reject an anomalous programme, he joined with Popper in envisaging some universal, neutral set of criteria which transcended individual bodies of research. (See Latsis (1976) for a forum on Lakatos's approach as applied to economics.)

The Kuhnian approach appears still to be left with the problem of lacking a means of appraising different theories, i.e. of relativism. The absence of any universal criterion for appraisal is in fact welcomed by Feyerabend (1970, 1981) who has also emphasized the significance of theory-laden observation for the philosophy of science. It is customary for Feyerabend to be dismissed as an extreme Kuhnian; Blaug, for example, refers to Feyerabend's philosophy as 'the philosophy of flower power' (Blaug, 1980, p.44). It is important, however, to understand Feyerabend's stance as that of an advocate of academic freedom. He was conscious of the power (in sociological terms) of the dominating paradigm, and its capacity to be perpetuated through the educational system. He rejects the notion of universal methodology as much as universal laws. His position constitutes a plea for tolerance, an advocacy of an alternative mode of scientific thought. But he lapsed into traditional dualism by equating the absence of universal scientific laws with complete skepticism. Rather than universal appraisal criteria he went to the other extreme of no appraisal criteria.

Kuhn in contrast focuses on delineating those areas where skepticism is relevant and those where it is not. In Babylonian tradition, by accepting the inevitable difficulties attached to the traditional scientific method, he puts forward a methodology which takes account of those difficulties in a constructive way. He set out to describe rather than attack (or support) the existence of paradigms; he certainly denies forcefully the interpretation that he advocates the supremacy of normal science over extraordinary science (see Kuhn, 1970). Rather, he argues on methodological grounds that Popper's prescription of *exclusively* extraordinary science is unreasonable (see Popper, 1970). If scientists were continually *trying* to falsify the entire range of assumptions underlying their hypotheses, there would be little scope for scientific progress – not because arriving at correct assumptions would not constitute scientific progress, but because 'correctness' is paradigm-specific, i.e. there is no universal, ahistorical criterion for correctness. In line with the Duhem-Quine thesis, falsification procedures, as Popper prescribes them, are not feasible.

While Kuhn, like Feyerabend, denied the feasibility of universal criteria for appraising theories or paradigms with a common set of weights, all paradigms' weighting of criteria could coincide in particular contexts. Kuhn (1970) quite explicitly emphasized the common ground of logical argument. But the metaphysics from which theories are logically derived are rooted in history, and are transmitted by forms of persuasion which include, but also go beyond, pure logic. The choice of premises, the interpretation of observations, even the choice of questions to be addressed all provide scope for disagreements which evade settlement on the grounds of logic alone, even though they are transmitted or justified by reasoned argument. An argument can be logically watertight but still *unpersuasive*. A market economist may find a logical Marxian argument unpersuasive, for example, until it can be expressed in terms of markets; but such a translation may not be possible.

The number of paradigms which emerge is limited by the necessity for scientists to belong to groups within which they may communicate; in Kuhn's world, Feyerabend's methodological anarchy would, in fact, be unlikely to produce a proliferation of paradigms. In order to function within a discipline, any practitioner must belong, however loosely, to one or another paradigm, which has its own language, techniques, metaphysics and basis for theory appraisal. It will also have its own perception of its aims, of what problems it should be addressing. But the perception of problems themselves cannot be ahistorical, nor, by implication, divorced from the concerns of the prevailing paradigm and its competitors. It is possible to have reasoned arguments about the existence of those problems and how they are dealt with; scientific revolution arises from conclusions being drawn that the prevailing paradigm does not deal satisfactorily with what is widely held to be an important problem for the discipline, and that an alternative, more satisfactory paradigm is available. Thus Kuhn does not suggest that scientific development by paradigm revolution is irrational, simply that the relevant rational arguments cannot be understood except in the context of the particular use of metaphysics, language and appraisal criteria of the relevant paradigms.

This Kuhnian approach is useful for the general task at hand here, which is to sort out the debates in macroeconomics according to schools of thought. In other words, since the primary aim is to classify and describe, the appropriate methodology is one which is designed to describe, rather than prescribe, and one which allows for different modes of thought. Implicit in that description will be an element of appraisal, since that is implicit in any paradigm. But by recognizing explicitly the methodological basis for differences between schools of thought, the criteria for appraisal within any one school of thought can be addressed on a rational basis.

2.5 Application of Methodological Issues to Economics

In attempting to apply the discussion of issues in the philosophy of science to economics, the assumption is implicit that these issues *are* applicable to economics in some way. Before making such an application, therefore, we will consider in this section, in general terms, three questions. First, is economics a science like other sciences? If not, in what ways does it differ? Finally, what is the relevance of these differences?

The question of whether or not economics is a science begs the question of how science is defined. Within traditional methodology, the demarcation was drawn according to adherence to the principles laid down for acceptable scientific practice. These principles differed from time to time, depending among other things on the prevailing view as to whether the mind or observation of matter was the source of truth. But aside from that difference, which determined the source of the axioms, or general laws, the structure of scientific argument (if not discovery) followed the broadly defined Cartesian/Euclidean model of a linear system of logic. This system produced theorems, or

predictions, which could then be compared with observations for confirmation of the truth of the axioms.

The problem of employing induction as a means of deriving general laws is common to all fields of empirical knowledge; no science can derive indubitable inductive general laws. The traditional methodological approach to economics thus posed the question as to whether economics qualified as a science in terms of how far it fell short of the ideal, relative to other sciences. There was, then, a perception that there were differences, possibly only of degree, between economics and other sciences, but not differences which justified necessarily an alternative code for scientific behaviour. Generally speaking, the consequence of acknowledgement of these differences simply strengthened resolve to approach as closely as possible the traditional scientific ideal.

Twentieth-century scepticism about traditional methodology raised the possibility of diversity of method for different disciplines. Within the field of scientific methodology itself, however, there was still need to specify its own scope, so that there was still a tendency to classify fields as science and non-science. Feyerabend (1970) was a notable exception, drawing parallels between reasoned argument in the structure of poetry and 'scientific' theories. While Kuhn suggested that method was paradigm-specific, he did discuss the degrees of scientific maturity of different disciplines. He identified maturity with the emergence of dominant paradigms, and with the specialization of knowledge (including language) within the discipline, precluding the participation of the lay person from its deliberations. While he has tended to exclude the social sciences from his enquiries, he has explicitly identified economics as having reached maturity in the second respect. There would seem to be little doubt that economics is 'mature' also on the grounds of generating dominant paradigms.

The reactions of economists to Kuhn's approach is determined to a considerable extent by their prior methodological stance. Thus an economist who views scientific progress as a continuing accretion of knowledge, according to rational criteria internal to the discipline, will find Kuhn's account of scientific revolutions with new and old paradigms incommensurate, an incorrect account of the history of economics; the account, in Kuhn's terms, does not fit into their paradigm (see, for example, Weintraub, 1979).

Among those whose methodological stance is receptive to

Kuhn's approach, however, there is debate as to its applicability to economics, as against the physical sciences (see Dobb, 1973, chapter 1; Coats, 1969; Bronfenbrenner, 1971; Blaug, 1976; Kunin and Weaver, 1971; and see Gutting, 1980, for papers dealing more generally with the social sciences). There is first the question of whether economics has in fact developed discontinuously, with periodic revolutions. In fact, Kuhn (1970) allows for continuity in the sense that *individuals* can be seen to have developed in their thinking from one paradigm to the next. The incommensurability of different paradigms stems from the fact that each paradigm uses concepts and language in different ways; once the new paradigm is established, the ideas of the old paradigm will be thought of in terms of the new paradigm, implying a continuity over time which disguises the fact of conceptual change. Even if this were not the case, the notion of a violent revolution is not essential to the application of Kuhn's theory to economics. As Deane points out:

> What the Kuhnian interpretation did bring out ... more effectively than any other, is the connection between the socio-historical development of professional schools of thought and the intellectual development in the theoretical content of the discipline.... [I]t is scarcely in dispute that there have been ruling paradigms in economics in that the textbooks describe a related set of theories, concepts and analytical techniques accepted as authoritative (though not necessarily as beyond criticism) by a majority of economists; and that there have been radical changes in the structure of economic doctrines which determine the generally accepted problem situation. (Deane, 1978, pp.xii–xiii)

The second question raised, by Kunin and Weaver (1971) for example, is whether the differences between a social and a physical science affect the applicability of Kuhn's framework of analysis to economics. Kuhn's own method consists of demonstration by means of exemplar, taken almost exclusively from the physical sciences. The problem of induction rears its head again in applying a theory based on the observation of one set of disciplines to another set. We turn now, therefore, to the second question concerning the nature of the differences between economics and the physical sciences.

The differences between economics and the physical sciences can be grouped into four categories.

Differences in source of observation

The standard difference to be pointed out in any methodological discussion is that the scope for experiment in economics is extremely limited. The difference, as Friedman (1953) points out, is one of degree; no physical experiment is truly isolated from its environment, and the application of the results of an isolated experiment to another environment requires additional hypotheses. But because the subject of economics (or any social science) is human behaviour, the inability to isolate particular features of behaviour and then to apply the results within a complex social environment is so marked as to rule out experiments as a general source of information. The only source of information, then, is observation of actual economic behaviour in its social environment. Economics is thus at a disadvantage in not having one of the tools of the physical sciences which facilitates the construction of general laws on which predictions can be based. The problems of testing these predictions 'in the real world' are the same for each, but the experimental physical sciences have access to short-cuts for deriving the initial axioms.

The significance of historical context

Even if experimental observation were available to economists, the capacity to generate predictions is hampered by the fact that the historical context in which theories are tested is necessarily different from that in which the axioms, or general laws, were formulated. A social science like economics must incorporate structural change in its theories, or at least arrive at a measure of structural change which can be used for adapting particular theorems or predictions to the environment in which they are to be tested. The basic subject matter of most of the physical sciences does not change historically (medicine being a counter example), whereas economic institutions and behaviour do change historically.

This difference has two important consequences. First, the scope for universal laws in economics is restricted by the capacity of the economic system to evolve over time; the majority of general statements must be conditional on the environment in which they are formulated. In particular, as Hicks (1979a) points out, the time element in statements of cause and effect becomes important if structural change can occur during that time period. In order to retain the causal statement, therefore, it must incorporate a statement about behaviour reacting to the structural change, as well as the initial cause. This requirement to account for historical developments further impedes the ability of an economist to conform to the traditional rules of scientific enquiry.

The historical dimension of economics alters also the application of the Kuhnian concept of scientific revolutions. If economics must adapt to take account of changes in the economic system, then there will be a continual source of fresh anomalies, and thus impetus towards paradigm shift for that reason alone. Rather than weakening the applicability of Kuhn's approach to economics, however, this additional feature of economics would seem to strengthen it. Paradigm shifts have perhaps been more frequent in economics than in the physical sciences. But the fact that, as Deane suggests, there have been identifiable shifts rather than continual evolution indicates that the dominant paradigm at any one time has been resistant to change, whether that change was warranted by its own shortcomings *or* by changes in the economic environment.

Human will and purposefulness

Human beings as an object of study differ from physical objects in having their own will, and by acting purposefully rather than being passive. At one level, the capacity to conduct experiments is limited by the capacity for human subjects to behave purposefully in experiments, possibly wilfully subverting proceedings. At another level, human beings are so complex in their behaviour, particularly in reacting to different economic environments, that it is very difficult to encapsulate that behaviour in a few axioms which define 'economic man'. The representation of human beings as deterministically following set *rules* of behaviour appears to conflict with a generally held view as to the nature of humanity, which combines stable behaviour patterns with creativity. But if economics is to conform to the traditional criteria for science, reductionist axioms depicting the universal features of human behaviour must be formulated, and, given the argument of the previous section, formulated in such a way as to incorporate the human impetus to institutional change and the behavioural reaction to such change.

In addition, the human content of economics influences the

scope of the discipline. The conscious attempts around the turn of the century to make economics more scientific focused on the distinction between normative and positive economics, advocating concentration by economists on the latter. Political economy was thus replaced by economics, moral science by mathematical science. (It is significant that the terms 'moral' and 'science' were used together, implying that they were viewed at that time as not necessarily incompatible.) But the question of how far economics *can* be made value-free is central, given its subject-matter.

The verstehen *principle*

While the differences noted so far have impeded the conformity of economics to the traditional view of science, this last difference provides some counteracting assistance. The *verstehen* principle (from the German 'to understand') refers to the capacity for an economist to have knowledge of human behaviour from introspection. A physical scientist cannot have an innate understanding of her subject-matter in any comparable way. Indeed, if one takes the results of introspection as indicators of 'true' human behaviour, then economics is rescued from the problem of induction. Like Kant's *a priori* ideas, which have independent existence in our minds, but require observation to bring them to the surface, these ideas can be used to formulate axioms. *If* these ideas are indeed true, and the deductive logic is correct, then economics is a purely deductive system to rival mathematics.

A Kuhnian approach would question the universality of application of observations of oneself, on the grounds that each individual is at least partially a product of her environment. Indeed, if introspection is simply detailed observation of one person, and does not tap the essence of human behaviour, then it too comes up against the problem of induction. On these grounds, Mises (1949) argued that the predictive power of economics was limited to individuals' own prediction. Nevertheless, it would be accepted that this additional source of observation generates further understanding of the individual input into economic relationships, within a particular economic environment.

While we saw that perceptions of methodology within the physical sciences underwent something of a revolution during the early years of this century, the above discussion still suggests that there are some significant differences between economics and the physical sciences. The final question we must address, then, is what significance these differences have. Do these differences preclude economics from the sciences? Or is economics a science requiring an alternative scientific method? Indeed, the case has been made that, if the unity of science is rejected, then economic methodologists should not be concerned with the history of scientific methodology (see, for example, Boland, 1982, Introduction).

Interpreting the differences between economics and the physical sciences from the standpoint of traditional methodology, the first three differences are differences of degree, and do not preclude the retention of the Cartesian/Euclidean ideal of scientific behaviour. Only the last difference, in its extreme form of relying exclusively on introspection, breaks with the empirical element in traditional methodology. Yet this *a priori* approach can be viewed as consistent with an older element in this tradition, on a par with mathematics. From the Kuhnian standpoint, the greater problems facing economists in interpreting observations place even more weight on the conceptualization process, and thus on the metaphysical, linguistic and metaphorical content of a paradigm. The paradigm concept thus seems to be even more powerful when applied to economics.

But it has been the result of attempts to apply traditional scientific principles, as developed for the physical sciences, which has governed much of the development of both the methodology and content of economics. It is still the case that alternative schools of thought are judged within the orthodoxy by the standards of traditional methodology (whatever actual methodological practice is within the orthodoxy). It is therefore necessary to understand that background, and to understand the particular problems economists have faced in attempting to conform to those standards. The tenacity of traditional methodology as an ideal within economics is contrasted by McCloskey (1983) with a practice which, as he describes it, has more in common with what we call here the Babylonian mode of thought. But McCloskey falls into a traditional dualism by arguing that an awareness of the divergence between principle and practice would put an end to concern with principles. Rather, if economists were more aware of the priciples which *implicitly* govern their practice, and of those which govern the practice of economists within other schools of thought, the constructive discussion of principles would be possible.

2.6 Conclusion

The traditional methodology of science has conformed to the broadly defined Cartesian/Euclidean mode of thought, setting standards for good scientific behaviour. The eventual overthrow of what had been regarded as universal laws in mathematics and physics, combined with the recognition that inductive logic was incomplete, posed a threat to the validity of those standards. The outcome was, on the one hand, scepticism of any attempt to impose standards, and on the other, a tenacious holding on to the traditional standard as the only other possibility allowed by this mode of thought.

Nevertheless, it is compatible with scepticism about universal objective scientific standards to develop methodological principles to suit the particular problems faced by any one discipline, such as economics, and to suit the historical context within which problems are selected and addressed. Kuhn's paradigm concept captures the process by which theoretical structures are built on a particular methodological base, and then replaced by another paradigm when that structure fails to deal with what is perceived as a major conflict between theory and observed reality.

Kuhn has been widely dismissed as relativist, in the sense of removing any universal criteria for theory appraisal, i.e. he has been dismissed for falling short of scientific criteria established within the Cartesian/Euclidean mode of thought. Rather, by stressing the uncertainty attached to knowledge noted by Russell in the quotation which introduced this chapter, Kuhn provides an alternative framework to prevent the 'paralysis of hesitation' which extreme scepticism brings about. It is a relativism which allows for reasoned argument (which may not be conclusive) rather than nihilism. By promoting awareness of the significance of world views for determining the inputs to logical arguments, Kuhn points to the boundaries of reasoned argument on the one hand, and the feasibility of the co-existence of incommensurate paradigms on the other. The observational problems in economics, and the pervasiveness of its moral or normative content, pose particular problems if economists insist on striving for the traditional scientific ideal. In economics, a Kuhnian starting-point can lead to a conclusion in favour of methodological diversity. Kuhn in effect presents a 'second best' argument: if the 'first best' of traditional methodology is unattainable, the 'second best' need not be to strive to come as close as possible to the first best outcome.

In the next chapter we explore how economists have interpreted scientific methodology for application to economics. By rejecting the fifth postulate of traditional methodology (see p.25 above) that the context of justification can be isolated from the context of discovery, we will consider developments in economic theory in terms of the environment in which they occurred.

3

The Historical and Methodological Development of Schools of Thought in Macroeconomics

3.1 Introduction

The dominant ideal methodology for economics has generally been that which has prevailed for the physical sciences. This ideal has influenced the way in which the discipline of economics has responded to the problems which it has addressed, as well as the choice of problems to address. But, in turn, the particular features of economics outlined in the last chapter have required a particular interpretation of the 'ideal' methodology, and indeed have meant that economics has often had policy-related problems thrust upon it, whether or not they were regarded as falling within the scope of economics as traditionally interpreted. Thus the development of economics has reflected the continuation of ideals as represented by traditional modes of thought, in combination with the historical environment in which economists have found themselves.

According to traditional methodology, scientific development consists of an inexorable accretion of knowledge which brings scientists closer to objective truth about general laws. There is thus no history of science, only a history of pre-science; science is encapsulated in the current stock of knowledge (see Canterbery, 1976, chapter 2). In contrast, according to the 'new' methodology which we employ here, the current state of knowledge cannot be understood without an appreciation of the environment which generated particular ideas in the past; such knowledge is indeed essential for an understanding of the current coexistence of several schools of thought. It is with a view to contributing to that understanding that this chapter is devoted to tracing some of the