TERENCE HUTCHISON'S 1938 CONTRIBUTION TO ECONOMIC METHODOLOGY

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by

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Abstract

Terence Hutchison's 1938 essay has been variously interpreted as introducing positivism, ultra-empiricism, and Popperian falsificationism into economics. Given Popper's well known anti-positivist stance, this state of affairs may seem puzzling. It might be presumed either that contradictions of some kind are involved in Hutchison's position, or that Popper's stance is not so far removed from logical positivism after all. In this thesis the latter option is adopted and Popper and logical positivism is viewed as part of a wider 'logical reconstructionist' pre-Quinean philosophy of science. Yet this move may not, and should not, resolve all disquietude on the part of the reader. For, to the extent that Hutchison adopted those aspects of Popper which clashed with logical positivism, there is an inherent contradiction between the view that Hutchison introduced positivism and the view that he introduced Popper into economics.

This provides us with the springboard we need for our thesis. For the contradiction is resolved once these views are recognised as turning Hutchison into a straw man. In the weak version of our thesis we argue that there has been an overemphasis on the positivist and Popperian elements in Hutchison's essay and a neglect of the extent to which it is concerned with economic methodology. In the strong version of our thesis we argue that Hutchison's essay is best viewed as a modern restatement of the inductivist-empirical-historical, as opposed to the deductivist-apriorist-formalist, approach in the long-standing *methodenstreit* in economics. In this restatement Hutchison draws on various elements of positivism and Popper to support a position that arises out of, and is specific to, the concerns of economic methodology, rather than to promote any particular philosophy of science in economics.

Survey chapters on the philosophy of science with special emphasis on logical positivism, and on aspects of the history of economic methodology, enable us to evaluate the nature of Hutchison's essay and to substantiate our thesis. Thereafter we highlight the shortcomings of the traditional interpretations of Hutchison's essay pointing to how these have limited Hutchison's influence in economics.

Key terms: Terence Hutchison, economic methodology, philosophy of science, empiricism, logical positivism, Popper, inductivism, hypothetico-deductivism, *methodenstreit*, historical method, institutional approach.

To the memory of
MIHKEL LEMMIT TRUU
1935-2001

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CONTENTS

INTRODUCTION

Dow (1997: 80) has distinguished between an old guard and a new guard in today's mainstream methodology. The old guard (Hutchison, 1938; Blaug, 1980) views the role of methodology as prescriptive, ie formulating rules for good scientific practice, especially the rule of submitting theories to empirical tests. The new guard (McCloskey, 1983) views the role of methodology as descriptive, ie as describing the rhetoric of how economists actually persuade each other. McCloskey showed how this practice differed from, what she termed, the official positivist methodology, so implying the latter redundant in practical terms. Dow (1997) also distinguishes between explicitly methodological writings and the methodology implicit in mainstream, or neoclassical, economics. This latter, she argues, is difficult to identify due to the belief of its practitioners that it has virtually no significance for the practice of economics (Hahn, 1992). Lawson (1994) argues that this belief results from the view that, provided the theory predicts successfully, no further methodological discussion is required. This, he argues, implies an underlying positivist methodology. Boland (1991) also identifies the methodology implicit in neoclassical economics as positivist, although McCloskey (1983) disagrees.[1]

Hutchison (1938) is generally regarded as having introduced positivism, ultraempiricism and Popperian falsificationism into economics (Knight, 1940; Machlup, 1955; Klappholz and Agassi, 1959; Rosenberg, 1976; Blaug, 1980; Caldwell, 1982; Coats, 1983; Hausman, 1992).[2] Given that Dow lists Hutchison (1938) as sole representative of the old guard mainstream methodology until 1980, and to the extent that Boland and Lawson are correct in their view that mainstream economists are simply unaware of the influence of positivism, it appears that we could

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[1] While in this first paragraph there is a balanced usage of both genders, for most of this thesis there is a preponderance of the male pronoun. This simply reflects the character of the historical period under discussion and should not be viewed as any modern-day gender insensitivity.

[2] The term positivism has come to be used in such a vague sense, and positivism bashing has become so widespread, that it is difficult to ascribe any sort of definite meaning to the term other than that it generally involves an attempt to attach some sort of pejorative meaning to the views so labeled (cf Hutchison, 1992: 49). In Chapter One we attempt to form a clearer idea of one form of positivism, logical positivism.

conclude that the influence of Hutchison (1938) on economics must have been comparable (at least in some way) to that of Keynes (1936) on economics. Yet the extent to which this is the case is far from clear. While some of the reasons will emerge in the course of the thesis, there are some immediately obvious factors.

One reason is that it may be a case of comparing apples with pears: the one involves the question of influence between two different levels of debate, while the other concerns the same level of debate, namely that between different economists theorising about economic relations. Another reason is that Hutchison largely left off active intervention in economic methodology until the late 1970s, devoting himself to the history of economic thought. Yet another reason arises from the phenomenal success of Friedman's (1953). This essay appears to have been more influential on practising economists than on economic methodologists. It was widely interpreted as contending that economists could simply bypass the issue of the realism of assumptions, provided the predictions of their theories met with empirical success.

Of course, citing Friedman's (1953) does not explain the previous fifteen years' apparent lack of influence. Where Friedman (1953) was applauded for setting out the methodology of positive economics, Hutchison's (1938) introduction of 'positivism' was roundly condemned by one of the leading economists of the time Knight (1940). Even in the 1950s it was once again rejected as ultraempiricism by Machlup (1955) who, at the same time, championed Friedman's introduction of positivism (Machlup, 1956: 485). This paradox is partly resolved to the extent that one accepts Boland's (1979) characterisation of Friedman as mainly instrumentalist, and Latsis's (1976) appraisal of (both Friedman and) Machlup as conventionalistic, defences of orthodox theory against falsificationism.[3] Yet puzzles remain. The rejection by Knight and

[3] There appear to be at least two very different senses in which the term 'conventionalist' is used. Latsis (1976) and Boland (1979) appear to use it in the sense in which Popper denounced instrumentalist and conventionalist attempts to both avoid the problem of induction and defend some particular dogma in the face of his fallibilist view of scientific knowledge. This contrasts with the widespread use of conventionalism in the philosophy of science (Poincaré, logical positivism, Popper himself – eg his rationality principle) to denote an anti-realist rather than a realist interpretation of concepts and (unobservable) terms in scientific theories extending to even scientific theories and laws themselves. For postmodernists it is only by convention that science itself is regarded as having any greater claim to knowledge than astrology: they are no more than two different conversations.

Machlup, contrasts with the seeming acceptance of Hutchison's views by the vast majority of practising economists. While Dow's distinction between economic methodology and the methodology implicit in mainstream economics needs to be taken into account, this widespread acceptance implies (as Dow has argued above) that Hutchison represented mainstream methodology. Yet Knight's and Machlup's reactions imply Hutchison (1938) was attacking orthodoxy.

Clearly the story of Hutchison's role in economic methodology is more complicated than that presented by Dow (1997). While this is partly due to the necessarily broad-brushed account of the field with which she is concerned, it is also partly due to the extent to which she subscribes to the consensus view of Hutchison (1938) as representing mainstream methodology and introducing positivism to economics. In this thesis we set out to challenge this consensus view in terms of both a weak and a strong version. In the weak version we argue that the positivist aspect of Hutchison (1938) has been overemphasised, while the extent to which it represents the concerns of economic methodology rather than any particular philosophy of science, has been overlooked. Just as Hutchison draws extensively on logical positivism, so his essay arises out of the on-going methodological concerns of leading economists in the history of economics eg Ricardo, Malthus, Senior and Cairnes amongst others. On the very first page of his essay he refers to Kaufmann, Mackenroth, Morgenstern, Myrdal and Robbins as participants in the then current discussions on methodological issues - he might have included Knight - all of whom feature significantly throughout his essay. In his first chapter, in which he proposes his Principle of Testability, *all* of his references are to writers concerned with economic methodology except for two. There is one reference to Carnap, another to Russell. The writer to whom he refers most of all in this first chapter is Schumpeter.

While we aim to defend this weak version of our thesis, we attempt on occasion to defend a strong version. The strong version is that Hutchison's essay is best viewed as another round of the long-standing *methodenstreit* in economics (between an inductive, empirical and historical approach and a deductive, a priori and formalist approach) rather than the introduction of positivism into economics. Hutchison's call is for a more inductive, empirical and historical approach in economics. Logical positivism, as the then latest version of empiricism, is used to support this call.

Although Hutchison draws extensively on logical positivist ideas, he tends to have more sympathy with the inductive, rather than the hypothetico-deductive, elements within logical positivism. And he indicates sympathy with 'the classic advocate of induction' when he refers to Bacon's dictum that wise questioning is only the half of knowledge (1938: 35-6). And he also refers (in Hart, 2002) to Bacon's distinction between light-bringing and fruit-producing. This accords with Hutchison's emphasis on economics as a discipline producing practical policy results.

Given this background, and having approvingly cited Hogben for one of his chapter six mottoes (1938: 160), it seems reasonable to infer that he would approve of Hogben's (1938: 26) citing of another of Bacon's dictums: 'It cannot be that axioms established by argumentation can suffice for the discovery of new works, since the subtlety of nature is greater many times over than the subtlety of argument'. Hogben goes on to protest against the view that 'exercises in draughtsmanship displayed in books on economics record the results of real measurements, as do curves in books on physics or biology' (ibid). With reference to Hicks (1932), Hogben comments:

> A curve which tells us the relation of resistance to heat exhibits a series of points each based on a Wheatstone bridge observation of the conductivity of a real piece of metal and a reading obtained from a reliable and tangible thermometer. The corresponding measurements of the employers' concession and the trade union resistance curves exist in the brain of Dr. Hicks (1938: 27).

Likewise, Hutchison's essay does not promote Popperian falsificationism in so far as his concern is more with empirically testing theories, than with specifically falsifying theories so as to avoid the problem of induction. (However, his essay certainly reflects Popper's fallibilistic view of science.) Hutchison's essay is grounded in, and emerges from, issues specific to the methodology of economics. It is this, rather than some ahistorical attempt to import a philosophy of science into economics.

Yet this strong thesis should not be pressed too far. Hutchison's essay shows a detailed knowledge of both the then latest philosophy of science, logical positivism, as well as earlier philosophy of science. Indeed, in drawing so extensively on the philosophy of science Hutchison appears to have set the norm for the methodology of economics from 1938 until the emergence of the new guard in the 1980s. This would

seem to reflect an enormous influence on the methodology of economics and accounts for Dow's (1997) listing of Hutchison (1938) as representative of mainstream methodology, ie methodology based on developments of logical positivism.[4] While there is truth in this view, Dow tends to underemphasize the *methodenstreit* in economics and does not even mention Robbins (1932, 1935).

It is true that she refers to the long-standing tension between empiricists or positivists and deductivists (Dow, 1997: 75). However, drawing on Caldwell (1982), she contends that the 1950s development of logical positivism did not 'necessarily entail any tension between empiricism and deductivism'. While this may be true of the situation in the 1950s, we argue in Chapter One that, from as early as the 1920s, a tension between an inductivist and a hypothetico-deductivist approach to scientific procedure characterised logical positivism. In line with our strong thesis we argue that the *methodenstreit* continued in economics with Hutchison (1938) replying on behalf of the inductivist camp to Robbins's (1932, 1935) statement on behalf of the deductivist camp. In drawing on logical positivism, Hutchison emphasised the inductivist aspects. Yet powerful factors led to the dominance of the hypothetico-deductivist approach in economics.

The inductivist approach as represented by the development of the full cost pricing principle, together with the theory of imperfect competition, proved threatening to the orthodox marginalist theory of the firm. This provoked the first powerful factor: Friedman's famous irrelevance-of-assumptions thesis in his (1953) instrumentalist defence of orthodoxy. Machlup skilfully presented Friedman's thesis as fully in line with the hypothetico-deductive version of logical positivism as outlined by Braithwaite (1953), so seemingly legitimising the old deductivist tendency to ignore the empirical realism of assumptions. Another powerful factor leading to the dominance of deductivism was the lack of experimental evidence in economics and the difficulty of empirical testing (Dow, 1997: 75). This, together with the problem of induction made Popperian falsificationism (which also drew on the hypothetico-deductive procedure so by-passing the problem of the realism of assumptions) seem

[4] The extent to which Hutchison's essay influenced the practice of economics is a different issue and is dealt with on pages 149-54.

increasingly attractive to many economists. In terms of both the hypothetico-deductive version of logical positivism and Popperian falsificationism:

> Methodology, therefore, guides theory construction only insofar as that construction facilitates empirical testing. Free rein has thus been given to deductivists to develop theory according to an internal agenda, with only token reference to empirical testing of the end results (Dow, 1997: 75).

While Dow links Hutchison, as representative of mainstream methodology, to this widespread deductivism, such a scenario clashes head on with the strong version of our thesis. This interprets Hutchison as emphasising the virtues of the inductivist approach in economics in 1938. While we will be substantiating this view of his 1938 essay during the course of this thesis (especially in Chapter Three), it is interesting to note that Hutchison's more recent writings appear to support such an interpretation. While Hutchison's first sustained return to methodological matters occurred in the late 1970s, it was only in 1992 that he delivered his first extensive criticism of orthodox methodology after that of 1938 (Hutchison, 1992). Drawing on Ward (1972), a book which he says he wished he had read eighteen years earlier (1992: 16), he endorses Ward's view that a methodological revolution more significant than the Keynesian revolution has occurred in postwar economics (1992: 17). This is the 'formalist revolution' (Hutchison, 2000, ch 9; cf Dow, 1998). Hutchison interprets its roots as being in the deductivist tradition following Robinson (1932, 1933) and Robbins (1932, 1935). Formalism prizes the development of the form, or technique, of the research method used, rather than the substance of the research itself. It has led to the development of increasingly sophisticated mathematical models and techniques of econometric modelling. Provided due conformity has been paid to these formalist procedures, further thought about the extent to which the analysis can be applied to the reality, eg taking into account the prevailing institutional and historical circumstances, does not even arise. This, of course, clashes head on with Hutchison's inductivism and his emphasis on taking account of the historical and institutional circumstances surrounding the research topic or problem.

Many factors may account for the development of formalism. Among these appear to be the postwar departmentalisation, professionalisation and internationalisation of economics (Hutchison, 1996, especially sections 5, 6, and 7). Hutchison (1992: 55)

dismisses Colander's (1990: 189-91) linking of formalism to positivism, since he interprets formalism as the polar opposite of positivism. While we concur with Hutchison (1998) that the roots of formalism are to be found in the ultra-deductivism of the Ricardo-Senior-Cairnes-Robbins tradition, it seems possible that the particular development of hypothetico-deductivist logical positivism within economics may have encouraged the growth of formalism. For instance, Ward (1972: 179) refers to 'the "positivist-formalist" who limits economics to those parts that can be formalised into the mathematical-statistical framework that is the positivist norm'. He points out that most inter-war economists conducted verbal rather than mathematical analysis. Those with a rationalistic bias steered clear of empirical facts, while the applied economist followed an historical approach (Ward, 1972: 40-44). The postwar period has seen a revolution which has swept this rationalist-historical mix aside and ushered in a mathematical-statistical one.

> The contemporary theorist is not only well-grounded in mathematics, but finds it the natural medium for expressing his professional ideas: the proof has replaced the argument. The well-regarded applied economist still knows his field of application, but his orientation is no longer historical. Instead, he seeks ways of formulating hypotheses which have the twin properties of being related to interesting modern theory and of being capable of statistical test. His aim is not so much to build a picture of events and their causes from historical documents as it is to test hypotheses of theoretical interest by using statistical tools and a pool of discovered or generated 'empirical' numbers (Ward, 1972: 41).

This passage from Ward enables us to form a clearer idea of the inductive, empirical and historical method that Hutchison supports. Hutchison (1988) traces, what he terms, the 'historical-institutional' approach to the eighteenth century Scottish Enlightenment and 'brilliant triumvirate' of Hume, Steuart and Smith. These ideas were next developed by the German historical school. While the rejection of Schmoller's support for the Prussian government as well as his academic empire-building is understandable, there has been a case of throwing the baby out with the bath water. There was no need, Hutchison argues, for the corresponding 'widespread rejection' of the 'vital role' of the historical and institutional factors in political economy.

Hutchison (1953) traces the historical school in England back to Ingram's (1878) and Leslie's (1879, 1879a) criticisms of classical political economy and earlier. He documents the influence of both the English and German historical schools on Marshall. Hutchison (1992: 125-6) has drawn attention to the extent to which Marshall was far closer to the historical method than many had realised until Coase's (1975) revealing article on the subject. Yet Marshall's dislike for methodological controversy and perhaps his over-sensitivity to Cunningham's (1892) criticism, led him to paper over the cracks of the long-standing divide between the deductivists and the inductive-historical school. Furthermore, Keynes's (1891) methodological treatise leaned more towards the deductivists than the inductivists. Hutchison's support for the historical method is clearly evident in his 1938 essay where he draws on Leslie's (1879a) in emphasising the importance of taking account of uncertainty when he exposes, in his chapter four, the extent to which the deductivist approach relies on the assumption of perfect expectations (1938: 89). Elsewhere he draws on Schmoller and Jones (1938: 59, 119).

However, just as it is wrong to interpret Hutchison (1938) as espousing no more than logical positivism, so it would be wrong to view him as espousing no more than the historical school's ideas. His methodology also draws on the classical empiricism of Hume, Locke and Berkeley. Hutchison (1953a) shows how Berkeley's approach to economic problems eschewed the deductivist approach. Concerning Berkeley's programme to relieve the unemployment in Ireland in the 1730s, Hutchison comments approvingly:

> Although the result is consistent and well-knit, Berkeley's programme is rather built upwards out of particular practical proposals suggested by the closely-observed problems around him, than deduced downwards from a set of formulae or generalisations. . . . Such analytical generalisations as Berkeley uses are thrown out ad hoc, the practical problem shaping and determining the 'tools of thought', as economists call them, rather than the other way round (Hutchison, 1953a: 53, 73).

Quinton (1993) distinguishes classical empiricism from two other forms of empiricism. The forms of empiricism are distinguished with regard to differences concerning the status of a priori *concepts* and *propositions*. He distinguishes two main types of a priori *concept*. First, there are the formal concepts of mathematics

and logic, eg, 'not', 'and'. Second, there are Kant's categorial concepts, 'categories' which the mind imposes upon experience, eg, 'substance', 'cause'. There are many kinds of a priori *propositions*, eg, definitional truisms and tautologies, logical and mathematical statements, and metaphysical claims. The strongest type of empiricism denies the truth of both a priori concepts and propositions. The second, or 'classical kind, denies the truth of categorial concepts, but accepts that formal concepts and propositions are a priori true. However, these pertain only to the relations between ideas, ie, to the conventional meanings given to words, not to matters of fact about the world. There are thus no 'synthetic a priori' propositions in Kant's sense. The weakest form of empiricism neither restricts a priori concepts to being only formal, nor to being only analytic. Consequently, unlike classical empiricism, there can be substantively informative propositions about the world that are nevertheless not empirical. Nevertheless, most concepts and propositions in this view remain empirical. Given their view that knowledge stems from experience, all empiricists stress the importance of inductive reasoning. We argue in this thesis that, to the extent that Hutchison (1938) draws on empiricism it is the second, or classical, form of empiricism that is more fundamental to his methodology than the logical positivism of the Vienna Circle.

Indeed recent revisions of the logical positivism of the Vienna Circle emphasise it as a synthesis of rationalism and empiricism involving 'a heterogeneous pluralism of views with regard to ethics, realism and verificationism versus falsificationism' (Stadler, 1998: 608). One of the bigger divides within logical positivism is represented in the protocol sentence debate of 1930-34 which we discuss as an appendix to Chapter One. Essentially this involved a split between Schlick who held to a phenomenalist empiricism and a correspondence theory of truth (and in this way closer to classical empiricism), and Carnap and Neurath who held to a physicalist empiricism and a coherence theory of truth which involved conventionalist (non-empirical) choices for empiricism itself (Friedman, 1998: 793). Carnap had originally sided with Schlick, but Neurath won him over in the course of the debate.

While Hutchison quotes from both the early and later Carnap, it appears that he is more influenced by Schlick, as we will see in Chapter Three. Rather than these philosophical debates, what was important about logical positivism for Hutchison is

the way in which it incorporated logic and mathematics (as a priori analytic knowledge) into an empiricist account of science. Apart from classical empiricism, it is the clear need for a vital role to be accorded to mathematics that distinguishes Hutchison's methodology from the historical method (as well as from the verbal formalism of the deductivists of the early twentieth century, ie Robbins and Mises). This explains Hutchison's choice of a passage from Pareto (1935) as the general motto for his entire book (1938: v). In the passage Pareto berates purely literary economists as 'numberless unfortunates' and praises the extent to which mathematical economics leads to some idea of the interdependencies amongst economic phenomena. Hutchison looks to economists such as Jevons, Marshall, Pareto and Schumpeter as symbolising the appropriate mix of deductive mathematical analysis and inductive historical investigation needed to deal with the particular subject matter of economics.

So far we have not considered the relationship between Hutchison's and Popper's methodology, apart from noting that Hutchison introduced Popperian falsificationism into economics and that his methodology is in accord with Popperian fallibilism. However, the methodologies differ in at least two areas. First, where Popper adopts the hypothetico-deductive accounts of scientific procedure, Hutchison's methodology is more sceptical towards hypothetico-deductivism and emphasises the extent to which inductivism-SM is useful in economics.[5]

> In this connection, it should be insisted that, as regards economics and the social sciences, the rejection or neglect of induction by strict hypothetical deductivists (like Popper and Hayek) also tends towards obscuranticism by insisting on excluding a method not used in physics, even when the material of economics requires induction if the aims and problems of the subject are to be tackled (Hutchison, 1992: 57).

Second, for Popper, prediction in economics must be based on laws. Hutchison disagrees and says that only 'trends . . . expressed in empirical or historical generalisations of less than universal validity, restricted by local and temporal limits' can be used in economics for purposes of prediction (1977: 19-21).

[5] Inductivism-SM is explained in Chapter One.

Both these characteristics of Popper's methodology are common also to the hypothetico-deductive version of logical positivism which developed into what Losee (1980, ch 12) describes as the 'logical reconstructionist' philosophy of science, perhaps best described in Nagel (1961). It understood the language of science to consist of a hierarchy of language levels with high-level statements referring to unobservables and low-level ones to observables (Braithwaite, 1953). Hempel and Oppenheim (1948) stressed the deductive nature of explanation which, in their account, involved a logical symmetry between explanation and prediction.

The fundamental requirement of both Popper and the logical reconstructionist philosophy of science for good scientific practice is that theories be subjected to objective empirical tests, whether these involved verification, confirmation or falsification. The objectiveness of the empirical tests derives from the view that the results of these tests are theory independent. However, in the late 1950s this logical reconstructivist position came under sustained criticism. The view that facts were theory-laden became increasingly emphasised and doubts were expressed about Hempel and Oppenheim's symmetry thesis. Amongst the more important critics were Quine (1951), Toulmin (1953), Hanson (1958), Feyerabend (1958), Polyani (1958) and Kuhn (1962).

* * *

In this introduction we have set out the project of this thesis and have attempted to provide some of the background against which to view Hutchison's 1938 essay. In order to assess the nature and influence of this contribution we will be concerned with a rational reconstruction of Hutchison's 1938 contribution to economic methodology, but will also attempt to take account of the intellectual climate of the 1930s (cf Torr, 2001). Given the need to make the thesis manageable, we decided to consider

Hutchison's influence only up to 1963.[6] This seemed a good cut-off date for three reasons. First, his 1938 essay comprises his only real methodological work (except for responses to criticisms of it) until Hutchison (1964). Up to 1963 it is thus only this work that can account for his influence on economic methodology. Second, 1963 is the year that Lipsey's best-selling textbook, *An Introduction to Positive Economics*, was first published. To the extent that Hutchison (1938) represents the introduction of positivism into economics - and we argue that this needs to be severely qualified - Lipsey's manifesto would seem to represent its final acceptance into mainstream economics. Third, it avoids having to take fully into account the extent to which Hutchison's methodological views changed and developed, and are still developing, during his long career. Hence, while we take account of certain revisions to his 1938 views that he made in the preface to the 1960 edition of his 1938 work, these are not especially relevant to our thesis. As it happens, we would contend that Hutchison's fundamental views remain much the same today as in 1938, but this topic falls outside the scope of this thesis.

Since Hutchison's essay has been mainly characterised as introducing positivism to economics, the main task of Chapter One will be to clarify the meaning of this term. We attempt to do this by examining in a fair amount of detail one particular form of positivism, logical positivism, since it is this form that is extensively cited in Hutchison's essay. We limit ourselves to discussing those aspects of logical positivism which are relevant to economics and the social sciences, rather than attempting a comprehensive account since this is best left to philosophers of science. Armed with this knowledge, we will be in a position to judge at least something of the extent to which the key ideas and propositions in Hutchison's methodology are drawn from this position. Hutchison refers not only to the writings of the Vienna Circle, but more widely to philosophers of science before the logical positivist era of the 1920s

[6] The outstanding assessment of Hutchison's methodology is Coats (1983). Coats (1983) formed much of the platform on which I started to develop this thesis so that I now no longer know if I first read of a certain idea or distinction related to Hutchison in Coats or not. Thus while I accord specific acknowledgement of Coats's appraisal at various points in the thesis, I can do little more than to acknowledge the enormous overall influence of his analysis on this thesis. (Coats appends a valuable bibliography of Hutchison's writings (1983: 265-70).) In more general terms, Caldwell (1982) likewise has exerted a fundamental influence. More than any other single text, it served to introduce me to the philosophy of science, the methodology of economics, and in particular to Hutchison's methodological contributions.

and 1930s. We therefore also examine such interventions for two reasons. Where they influenced the writers of the Vienna Circle this will aid our understanding of logical positivism. Where this was not the case, we will be able to form some idea of the extent to which Hutchison drew on a philosophy of science wider than that of logical positivism.

In Chapter Two we are back on home ground after our foray into the philosophy of science. Nevertheless, we limit our objective to looking at those aspects of the history of economic methodology that are relevant to a clearer understanding of the long-standing dispute between two competing approaches to investigating the subject matter of economics. The one emphasises the importance of inductivist methods and the other the importance of deductivist methods. Given that this is a theme dear to Hutchison, and explicitly brought out in Hutchison (1998), we need to guard against viewing the history of economic methodology too much through his eyes. In order to do this, as well as to gain a more detailed account of economic methodology in the period leading up to Hutchison's essay in 1938, we examine the methodological writings of three of the leading economists of the period, and indeed of the century: Robbins, J M Keynes and Knight. Given that it often helps in understanding a view to understand that to which it is opposed, a knowledge of Robbins's methodology should aid us when we turn to examine Hutchison's essay. Equipped with the knowledge gained about the history of economic methodology in Chapter Two, we are in some position to judge the extent to which Hutchison draws on, and is responding to, the topics and concerns of economic methodology rather than those of the philosophy of science.

We turn in Chapter Three to Hutchison's 1938 essay itself. We deal with it chapter by chapter examining it in terms of headings chosen by Hutchison. By withholding our own categories of interpretation it is hoped that it increases the chances of discovering what Hutchison himself regarded as important in his essay. Our main concern will be with the extent to which Hutchison is influenced by the issues and debates that arise out of economic methodology, as opposed to the extent to which his essay reflects an attempt to introduce the categories and propositions of logical positivism into economics. While Hutchison cites extensively from both the methodology of economics and the philosophy of science, the problem is to decide

which gives rise to the themes that are central to his essay. In terms of the weak version of our thesis, the main point that we hope to bring out in this chapter is the extent to which Hutchison's essay is the result of a wide-ranging examination of the methodology of economics, a point that has so far been overlooked. Instead his essay is conventionally viewed as an application of logical positivism to economics.

We now examine the way in which Hutchison's essay has been interpreted and, correspondingly, the way in which it has influenced the methodology of economics. In Chapter Four we focus on the immediate response to Hutchison's essay as reflected in the various journal reviews. These mostly highlight the positivist nature of Hutchison's essay, so starting what became the traditional way of viewing his intervention. By far the most important response to Hutchison (1938) was Knight's (1940) polemical review. Given that at the time Knight was about the most senior economist in the world and Hutchison only in his twenties and beginning his career in economics, the ferocity of Knight's response is puzzling. Although Knight made it clear he was attacking positivism in general rather than only Hutchison's essay, this still did not prevent the brunt of his criticism falling on Hutchison (1938). While positivism certainly was an issue, we suggest that the ferocity of his intervention is only fully explained in terms the *methodenstreit* in economics. Knight is defending the deductivist approach to economic analysis against a new and, seemingly backed by the latest philosophy of science, more threatening version of the inductivist approach to the investigation of economic problems. What Knight saw as being threatened was the dominance of the deductivist approach which, importantly, traditionally represented the official methodology of orthodox economics. Both Knight (and later Machlup) appear to have interpreted Hutchison's intervention as threatening not only the authority of the deductivist approach, but more especially, that of orthodox economics itself. Yet, Hutchison in 1938, we maintain, was not concerned with attacking mainstream economics per se. He only wanted it to follow what he saw as the procedure best suited to the subject matter and most likely to be 'fruit-producing'. To this end, he was championing the claims of the less influential inductivist approach while drawing attention to the limitations of the deductivist approach.

In Chapter Five we turn to Machlup's (1955) response to Hutchison (1938). The obvious question that arises is why Machlup took so long to respond to Hutchison's intervention. The answer has to do with the fact that Machlup dealt with Hutchison's essay as only one part of a wide-ranging defence of the orthodox theory of the firm. Machlup appears to have viewed his intervention in 1955 as an effort to further support Friedman's (1953) attempt to mount a methodological defence of the neoclassical theory of the firm which had been under sustained criticism throughout the 1940s from adherents of both the monopolistically competitive, and the full cost pricing, theories of the firm. We argue that, whereas Friedman's defence of orthodoxy followed mainly empiricist lines (ending up in instrumentalism), Machlup's defence of orthodoxy followed in the deductivist tradition (ending up in conventionalism). In seeking to deal effectively with criticism of the realism of assumptions (unlike Friedman who adopted instrumentalism) Machlup sought to portray such criticism as either methodologically naïve or extreme. Given Hutchison's criticism of the basic postulates of economic theory, he forced Hutchison (1938), Procrustean-like, to fit into this pre-conceived scheme. Hutchison was cast as philosophically well informed, but as representative of an extreme fringe of methodological opinion. Any potential critic of the realism of assumptions thus thought twice about being labelled an ultra-empiricist. Machlup thereby stunted the development of the inductivist tradition in economic methodology.

We end by examining in Chapter Six Klappholz and Agassi's interpretation of Hutchison's 1938 intervention. While they acknowledge Hutchison's great achievement as the introduction into economics of Popper's falsifiability criterion, they criticise Hutchison for not going far enough in the adoption of Popper's ideas in his methodology. We argue that Klappholz and Agassi (1959) represents an extreme interpretation of Popper's approach to scientific procedure. We have seen earlier in this introduction how Hutchison differs from Popper. This is mainly because Hutchison pursues a more inductivist approach than Popper. While we have argued that it is largely incorrect to view Hutchison's essay as an attempt to apply the ideas of logical positivism to economics, we argue that in the case of Klappholz and Agassi it is correct to view their intervention in 1959 as an attempt to apply (Popperian) ideas in the philosophy of science to economics. They were over ambitious in their attempt and tried to apply Popperian ideas too widely. By contrast, Hutchison in 1938 saw

that Popper's ideas applied to economics only in a more limited way. As with Machlup, Klappholz and Agassi's intervention had an unfortunate consequence in that Archibald and Lipsey were guided by Klappholz and Agassi rather than Hutchison. Hutchison's methodology was sadly ignored and Lipsey (1963) was led into adopting a methodological position that could not be sustained.

CHAPTER 1

THE PHILOSOPHY OF SCIENCE AND HUTCHISON'S INTERVENTION

In line with Knight's well known (1940) appraisal of Hutchison's 1938 intervention and other early reviews (Stonier, 1939; Whittaker, 1940), Hutchison (1938) has generally been regarded as espousing a positivist methodology. Although Machlup described Hutchison's position as ultra-empiricist, for him this is synonymous with positivism (Machlup, 1978: 486). In the introductory chapter of his influential microeconomics text, Ferguson (1969: 7) reproduces Machlup's characterisation in the form of a diagram with ultra-empiricism, represented by Hutchison, diametrically opposed to extreme a priorism. Others who interpret Hutchison as positivist include Caldwell (1984: 3) and McCloskey (1986: 39).

In Chapter Three we demonstrate that, in arriving at his philosophical and methodological position in 1938, Hutchison drew on a wide range of writings in the methodology of economics and the philosophy of science which was by no means limited to positivism. As far as the philosophy of science is concerned, while it is true that he cited many of the Vienna Circle writings, he referred also to Kant (1790), Mill (1843), Jevons (1874), Poincaré (1905), Einstein (1921), Wittgenstein (1922), Russell (1927), Ramsey (1931) and Popper (1934). Indeed, in reply to Knight (1940), Hutchison (1941) saw himself as drawing on the wider British empiricist tradition of Locke, Berkeley and Hume, and not simply on the Vienna Circle. Consequently in Chapter One we will aim towards an understanding of the philosophy of science somewhat wider than that of the Vienna Circle in order to encompass this aspect of Hutchison's 1938 tract. But the main focus of Chapter One will be directed towards acquiring an understanding of logical positivism so that we can judge (in Chapter Three) the extent to which Hutchison actually draws on logical positivism and therefore the extent to which portrayals of Hutchison as positivist or ultra-empiricist are accurate.

Consequently, in the following sections, we do not attempt the ambitious task of a full

philosophical appraisal of logical positivism and other positions, but instead limit ourselves to drawing out those aspects of the philosophy of science (particularly logical positivism) which help towards a more informed understanding of the methodology of Hutchison (1938). In line with this aim it will help to describe developments in two major areas which led up to the writings of the Vienna Circle: nineteenth century attempts to make philosophical sense of startling developments in the natural sciences, and the revolution in logic.

1.1 The revolution in science: the response of Mach and Poincaré

Partly due to the successes of natural science in the seventeenth and eighteenth centuries, the empiricism of Locke became more extreme in the later formulations by Berkeley and Hume (Lacey, 1995). Kant sought to rectify this imbalance by attempting a rationalist-empiricist synthesis. In his 1781 *Critique of Pure Reason* he argued that knowledge of the world could not be gained purely by reason alone. Likewise, it could not be gained from experience alone. Empiricists were wrong to view the objects of experience as existing independently of us. Rather it is only by reason that sense experience comes to be ordered and classified (in terms of twelve categories) so as to be coherently perceived. The order of nature does not exist independently in nature, but is imposed by the structures of our understanding. To Hume's two kinds of knowledge, relations of ideas and matters of fact, Kant therefore added a third: knowledge in the form of synthetic a priori statements. These could be known to be true independently of experience and yet they could give us new knowledge about the world. Among Kant's examples were mathematical statements such as the equation 7 + 5 = 12 and metaphysical statements such as 'Every event must have a cause'. For Kant, Newton's physics typified synthetic a priori knowledge in that it was based upon Euclidean geometry.

In the nineteenth century three major developments arose to challenge Kant's assertion of synthetic a priori knowledge: the rise of non-Euclidean geometry (eg that of Gauss, Bolyai, Lobachevskii, Riemann and Klein); the formulation of the conservation of energy and general thermodynamics; and the start of scientific physiology and psychology (Friedman, 1998: 790). In their efforts to make philosophical sense of these changes, two scientists exerted a significant influence on

the logical positivists of the Vienna Circle: Mach and Poincaré.

Mach's radical empiricism

Mach reacted to the interpretation of the conservation of energy (by Helmholtz) in atomistic terms, ie involving (metaphysical) elements which were not immediately perceptible. He developed a radically empiricist philosophy which was closely related to the work of Avenarius. Avenarius rejected the idea of a dualism between the physical and mental worlds and sought to replace this with a single 'pure experience'. He also made use of Ockham's razor to argue that it is legitimate to recognise only those things shown to us by experience: his famous principle of economy. 'Science is experience economically ordered, and its real content does not go beyond experience' (Kolakowski, 1972: 137).

To these ideas Mach brought sensationalist and phenomenalist theories of experience and knowledge. Here he argued in line with J S Mill (1843) who viewed physical objects as being no more than 'permanent possibilities of sensation'. For example, a stone consists of nothing more than a collection of various sensory qualities such as hardness, colour and mass. For Mach, 'these sensations are not in themselves illusory or deceptive, but, on the contrary, they are all that we know of reality' (Joergensen, 1951: 8). There is no underlying substance that has these properties or causes these properties. If the perceptible qualities were stripped away precisely nothing would remain. Scientific theories are therefore confined to the world of experience. Likewise any concept of causation, involving the noumena or ultimate entities of some ontological reality, was rejected. Causation was to be understood in Humean terms as describing no more than some regular connection, or constant conjunction, of phenomena.

Mach's theory of knowledge is biologically oriented. Just as the conditioned reflexes of animals arose as responses to their environment, so do the relationships of science. The only difference is that those of science are better organised, thanks to the development of human speech. Science simply constitutes a development of this process. 'Any assumption that the human conceptual system contains something more than the sense experiences from which it derives is completely unfounded: it is

merely more effectively organized' (Kolakowski, 1972: 147). According to Mach (1911: 49):

> In the investigations of nature, we have to deal only with knowledge of the connexion of appearances with one another. What we represent to ourselves behind the appearances exists *only* in our understanding, and has for us only the value of a *memoria technica* or formula, whose form, because it is arbitrary and irrelevant, varies very easily with the standpoint of our culture.

Mach also presented an instrumentalist view of scientific theories. At every stage of science knowledge is only provisional. It constitutes an essentially practical response to the solution of everyday problems, rather than any universal and eternally valid form of knowledge. Its generalisations are nothing more than contingent truths which may be confirmed or falsified by experience. This led Mach (1883) to revise Newtonian mechanics along phenomenalist lines, criticising metaphysical speculation about motions in absolute space and time as meaningless. Likewise he refused to ascribe any real existence to theoretical terms such as electric charges and atomic particles. Quinton (1982: 184) describes Mach as 'the chief philosophical stimulus for Einstein's great enterprises'. He goes on to point out his influence on Russell and contends that Mach's theory of experience ranks alongside Russell's logic as indispensable forerunners of logical positivism. However, Wolters has argued that Mach 'does not foreshadow the observation-theory dichotomy of logical empiricism, because he already emphasizes the theory-ladenness of observation' (2000: 254) (cf Mach, 1976: 120 [1905]).

Poincaré's 'conventionalism'

Poincaré responded to the rise of non-Euclidean geometries, in particular their implications for the status of geometrical propositions. According to Kant, Euclidean geometry represented a set of synthetic a priori statements. Poincaré (1902: 48) argued that if this were so, it would not have been possible to imagine the various post-Kantian non-Euclidean geometries that arose. Geometrical propositions thus cannot be synthetic a priori statements. He went on to reject the empiricist interpretation of geometry as an empirical theory of actual space. He pointed out that geometry has to do with exact truths while empirical evidence is only inexact.

> *The geometrical axioms are therefore neither synthetic a priori intuitions nor experimental facts.* They are conventions. . . . What, then, are we to think of the question: Is Euclidean geometry true? It has no meaning. We might as well ask if the metric system is true, and if the old weights and measures are false; . . . One geometry cannot be more true than another; it can only be more convenient (Poincaré, 1902: 50).

Since Poincaré believed Euclidean geometry is the most convenient, he claimed it would never be given up. He supported this claim by arguing along similar lines to his contemporary Duhem. Poincaré argued, that to test Euclidean geometry, we need to make assumptions in addition to the axioms of Euclidean geometry. For example, we need to assume a light ray travels in a straight line. If a geometry is not in agreement with experience, Poincaré argued that, rather than give up the geometry, some auxiliary assumption will be modified. This argument developed into the Duhem-Quine thesis (related to the problems arising with Popper's falsificationism): 'Since all testing must be carried out on sets of linked hypotheses, it is a matter of choice which of these hypotheses we regard as having gained support as a result of testing' (Stewart, 1979: 224).

Poincaré (1902: Ch VI) next turns to the laws of classical mechanics. He argues, using the same procedure as for Euclidean geometry, that these laws are neither a priori nor experimental truths but only disguised definitions, or conventions, which, because they are the simplest will never be given up in the face of empirical refutation. Gillies (1993: 92) points out that contrary to these claims, the development of relativistic mechanics did in fact lead to the abandonment of the view that Newtonian mechanics holds exactly in all circumstances. As 'one of the principal initiators of the twentieth-century revolution in physics', Gillies (1993: 93) points out that Poincaré was all too aware of such implications. This led him to radically revise his ideas of 1902.

The about-turn is documented in his 1905 work *The Value of Science* to which Hutchison (1938: 36, 51, 82, 129) refers. Here he gives up his 1902 view that the laws of Newtonian mechanics were conventions which would never be given up in the face of empirical refutation. This about-turn was largely as a result of experiments carried out by Walter Kaufmann in 1902-3. The results of these

experiments implied the falsity of Lavoisier's principle which in turn implied the falsity of Newton's laws.

Gillies (1993: 95), having described how Poincaré abandoned his view of the conventionalist nature of the laws of Newtonian mechanics, proceeds to point out that later in his 1902 *Science and Hypothesis* Poincaré regarded most of the remaining laws of science as 'genuinely empirical laws founded on induction from observation and experiment'. Even with respect to Newton's laws of motion he had maintained that they served not only as conventions but also as empirical generalisations. Initially laws are employed solely as experimental generalisations as, for example, a relation between terms A and B. Noting that this relation holds only approximately, a term C might be introduced which, by definition, has the relation to A which is expressed by the law. The original law now consists of two parts: an experimental law that states a relation between B and C, and an a priori principle which holds exactly between A and C and is a convention.

> But this is not to say that the choice of definition is arbitrary. Poincaré [1905] insisted that the introduction of conventions into physical theory is justified only if it proves fruitful in subsequent research. . . . Thus it would be incorrect to attribute to Poincaré the view that general scientific laws are nothing but conventions which define fundamental scientific concepts (Losee, 1980: 169).

This agrees with Harre's (1967) interpretation of Poincaré as espousing a *commodisme* different from the more popular doctrine called conventionalism. Poincaré's *commodisme* is that, in a certain sense, theories are linguistic conventions. Geometry is a way of representing spatial facts rather than a particular set of spatial facts:

> Choosing between Reimann and Euclid is like choosing between different projections for making a map of the world. According to the choice, the map will be different, but the continents are the same and bear the same relations to one another (Harré, 1967: 292).

According to Harre, in contrast to Poincaré's *commodisme*, conventionalism maintains that 'not only is the language of science arrived at by convention but so are the facts which that language is used to express. The laws of nature are not truths but mere conventions' (ibid).

> In repudiating this development of his opinions, Poincaré worked out his own position in more detail. The extreme conventionalist view implied that it did not matter which conventions were adopted; any one would be as good as any other. Of course, Poincaré was quick to point out, in science some conventions are better than others because some recipes are successful and others are not. There must be an empirical criterion which distinguishes good science from bad. The nature of this empirical criterion can be approached by noticing that although the facts which are ultimate criteria of truth may be the same, there are a great many ways of expressing them. A scientific fact differs from a 'crude' fact, according to Poincaré, in the same way that a statement in German differs from a statement in French. The laws of nature are, as it were, the rules of synonymy of the scientific language; but they can nevertheless be revised, for they depend upon concordances of fact. Should the concordance fail, scientific language would contain an ambiguity and would have to be revised. 'In sum, all the scientist creates in a fact is the language in which he enunciates it'' (Harré, 1967: 293)

The above quotation from Harré ends with a sentence from Poincaré. This same sentence is also cited by Hutchison (1938: 36). Here is a (small) example of the extent to which Hutchison was familiar with the work of Poincaré, a fact which makes it clear that Hutchison was far from being a naïve inductivist or ultraempiricist. It also serves as a good example of the extent to which Hutchison reached outside the writings of the Vienna Circle in formulating his 1938 tract. More broadly speaking, the attempts by Mach and Poincaré to make sense of the developments in natural science and mathematics exerted a decisive influence on the members of the Vienna Circle. For example, both Reichenbach and Carnap substituted Poincaré's concept of convention for the Kantian notion of the a priori (Friedman, 1998: 791).

1.2 The revolution in logic: Russell and Wittgenstein

Apart from developments in the natural sciences, the logical positivists were also significantly influenced by the revolution in logic. Kant had interpreted mathematical axioms as examples of synthetic a priori knowledge. Mill had declared them to be empirical generalisations. In terms of the logicism of Frege and Russell, since they could be subsumed within a system of logic, they expressed merely analytic truths. Carnap studied under Frege and, according to Friedman (1998: 792), Carnap (1928) was 'inspired by Russell's conception of "logic as the essence of philosophy" to reconceive philosophy itself on the model of the logicist construction of arithmetic . . .

by developing a "rational reconstruction" of empirical knowledge'. Likewise Wittgenstein (1922) interpreted the propositions of logic as entirely tautological or empty of empirical content. We now take a closer look at these developments in logic and the foundations of mathematics by examining the contributions of Russell and Wittgenstein.

Russell's analytical philosophy

Russell's early training in mathematics exposed him to Mill's empiricist account of mathematical knowledge (Gillies, 1993: 11). Dissatisfied with this view, Russell (1897) attempted a Kantian explanation of the foundations of mathematics. However, this was found to conflict with the geometry in Einstein's relativity theory. After this he turned to Hegel for a time until, under the influence of Moore, he abandoned this position. Russell now turned to a variety of Platonic realism (Quinton, 1971: 4). Re-thinking the foundations of mathematics, he was impressed with how Frege 'showed in detail how arithmetic can be deduced from pure logic, without the need of any fresh ideas or axioms, thus disproving Kant's assertion that '7 + 5 = 12' is synthetic' (Russell, 1924: 32). However, in terms of Platonic realism, terms such as point and number can only be meaningfully used if they refer to some real entity. Russell soon realised that the result would be an absurdly large number of problematic entities. This led him to further work on logic. He made a breakthrough after meeting the Italian mathematician, Peano, at a conference in Paris in July 1900. That October he sat down to write the first draft of the *Principles of Mathematics* (completed by the end of that year) in which he argued that 'all pure mathematics follows from purely logical premises and uses only concepts definable in logical terms' (Russell, quoted in Magee, 1971: 145). 'To sustain the thesis [of logicism], Russell needed to refashion logic, and for this he enlisted the co-operation of his old tutor, Whitehead' (Ayer, 1972: 16). This resulted in *Principia Mathematica* (1910-13), a detailed defence of the logicist thesis of the *Principles* (1903).

Russell's importance as a philosopher stems from his application of the discoveries he made in formal logic to the analysis of knowledge claims made in ordinary language. The chief characteristic of symbolic or mathematical logic is that the focus of attention is shifted away from the Aristotelian concentration on relations between

terms to a focus on propositions and relations between propositions. The power of focussing on relations between propositions is exemplified in his theory of descriptions. According to Russell (1905), to be meaningful a statement had to be capable of being either true or false. However, in ordinary language, some sentences (eg about Kings of France) could not be said to be true or false (because it does not refer: there is no such a thing as the King of France). Russell applied symbolic logic to show that, when analysed, their truth or falsity became apparent. The theory of descriptions provided a powerful new technique of analysis.

Until Wittgenstein arrived in Cambridge in 1912, Russell had been mainly concerned with a philosophical defence of mathematics by eliminating invalid logic and unnecessary or metaphysical assumptions. Wittgenstein, using Russell's own logic, convinced Russell that the propositions of mathematics did not refer to universals in some Platonic realm of reality, but were instead nothing more than tautologies, purely conceptual truths. Russell's reaction to this demonstration led his interest to change from a defence of mathematics to a justification of science (Quinton, 1982: 278).

After his rejection of Kant and Hegel, Russell adhered to some form of empiricism for the rest of his life (Gillies, 1993: 11). His notion of analysis arose from the empiricist tradition, eg Mill had defined 'cause' as 'invariable unconditional antecedent' (Hampshire, 1971: 21). According to Hager (2000: 408), Russell's method of philosophical analysis 'proceeds backwards from a given body of knowledge (the 'results') to its premises . . . (it is) a nonempirical intellectual discovery of propositions and concepts from which could be fashioned premises for the basic data with which the analysis had begun'.

> The old logic put thought in fetters, while the new logic gives it wings. It has, in my opinion, introduced the same kind of advance into philosophy as Galileo introduced into physics, making it possible to see what kinds of problems may be capable of solution, and what kinds must be abandoned as beyond human powers (Russell, 1914, quoted in Joergensen, 1951: 13).

Analysis enables science to expand to incorporate what was once philosophy. Russell is clear that the main source of knowledge advance is science, not philosophy. 'Whatever can be known, can be known by means of science . . . philosophers who

make logical analysis the main business of philosophy . . . refuse to believe that there is some 'higher' way of knowing, by which we can discover truths hidden from science and the intellect' (Russell, 1946: 788-9).

In a series of lectures in 1912, later published as *Philosophy of Logical Atomism* (1918), Russell presented a theory of meaning, knowledge and reality. For Russell, the process of analysis of the conditions necessary to give an ordinary sentence a definite meaning, finally terminates on a bedrock of elementary or atomic propositions which are unanalysable and upon which all our knowledge rests. The terms of these atomic propositions stand for distinct elements in our experience, the data of immediate experience, or 'sense data'. These 'logical atoms' constitute the facts of reality. They are not knowledge claims that need to be justified by an inference (Hampshire, 1971: 21). He calls the process of discovering the atoms 'logical analysis'.

According to Warnock (1971: 132), Russell held firm to his conviction that ordinary beliefs about the world are largely mistaken and that the aim of philosophy is to achieve understanding of the world. To do this required that natural languages be replaced with one which has a technical terminology which allows for greater precision than ordinary language (which Russell said incorporated the metaphysics of the Stone Age). Wittgenstein carried this project forward in developing the doctrine of logical atomism in the *Tractatus*.

> The purest atomistic vision concerning facts is provided in Wittgenstein's *Tractatus*, and is the view that all facts, or all basic facts, are atomic, and every atomic fact independent of every other (Sainsbury, 1995: 63-4).

Wittgenstein's theory of language

Russell's *Principles* had argued that mathematics was based on logic. Wittgenstein asked on what logic is based. To answer this question he developed a theory of language which showed up the nature of logical necessity - about what it is that makes some propositions necessarily true.

According to the *Tractatus*, the world is the 'totality of facts' (1.1).[1] These exist independently from us and from our language. They can be broken down via analysis into atomic facts. Unlike Russell, who identified these with sense data, Wittgenstein doesn't say what these facts are - only that they cannot be analysed into simpler facts. In language, the counterpart to these atomic facts are atomic propositions which are 'laid against reality like a ruler' (2.1512).[2] These are literally pictures of atomic facts.

Logic is like the scaffolding of the world. The world has to take shape around it. Logic is like a map or space of all possibilities, 'of everything that could conceivably be the case, and must therefore show us the limits of everything that can conceivably be (thought and) said. So in plotting the map of logic one is making manifest both the limits of (thought and) language and the limits of all possible worlds' (Magee, 1971: 35). Logic reveals the structure of language, and at one remove, the structure of the world. This is because 'the two structures are the same, like a man and his shadow'.

The theory that language mirrors the structure of the world is a theory about possibilities. Language reflects all the possibilities that can be said. Wittgenstein distinguishes between logical and factual propositions. He draws from Frege the notion that a factual proposition must say something absolutely definite, or positive. It is true or false with no third alternative. Since a factual proposition such as 'It is raining' has this definite sense, it follows that it rules out the negative case, ie, it is not raining. The logical disjuncture 'It is raining or it is not raining' must then be true: it covers the whole field of possibilities, every state of affairs. 'Logically necessary propositions are a kind of by-product of the ordinary use of propositions to state facts' (Pears, 1996: 687). They represent limiting cases of the essential nature of factual propositions. Since they cannot rule out anything about the world they are without the 'definite sense' of a factual proposition. It is in this sense that Wittgenstein refers to the propositions of logic and mathematics as tautologies: they are analytic statements which cannot tell us anything new about the world.

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[1] All references throughout this thesis to Wittgenstein's Tractatus are to the 1922 edition except where otherwise noted.
[2] The reference here is to the 1961 revised edition of Wittgenstein's Tractatus – see bibliography.

The view that factual propositions are the only meaningful propositions arises from Wittgenstein's thesis that language is fundamentally about the describing of fact. Upon reading a newspaper report of how, in a court of law, models had been used to represent the scene of a road accident, Wittgenstein was supposed to have exclaimed 'This is how language works!' (Quinton, in Magee, 1978: 79). This theory of how language works is often known as the picture theory of meaning. Sentences don't look like pictures, but if they are to have any meaning, they must be capable of being analysed into simpler propositions which literally are pictures. Wittgenstein thought of words as names of things or objects. In a factual proposition, the arrangement of names in the proposition corresponds to a possible arrangement of objects in the world. If that arrangement is indeed actualised in the world, then the proposition is true. If it is not, the proposition is false. If the names in the proposition are arranged in a way in which it is not possible for objects in the world to be arranged, then the proposition is meaningless. Propositions can either be meaningful in which case they are either true or false, or they can be meaningless (Magee, 1978: 81).

According to Wittgenstein, if language is to be meaningful it must be concerned with describing the factual world. But much of what we say about the world, in metaphysics, ethics, aesthetics and religion, does not appear to have anything whatever to do with facts. This would seem to imply that philosophical propositions attempting to describe the essential natures of things or the metaphysical structure of the world are to be condemned as outside the bounds of sense, or senseless.

> Most propositions and questions, that have been written about philosophical matters, are not false, but senseless. We cannot, therefore, answer questions of this kind at all, but only state their senselessness. Most questions and propositions of the philosophers result from the fact that we do not understand the logic of our language. (They are of the same kind as the question whether the Good is more or less identical than the Beautiful.) And so it is not to be wondered at that the deepest problems are really *no* problems (*Tractatus* 4.003).

Wittgenstein (1922: 27 and 29) expressed his belief that, in the *Tractatus*, he had 'finally solved' the problems of philosophy. Traditional philosophy which tried to construct general systems about the world as a whole was misguided. Instead he argued, the method of philosophy should be that of analysis.

> Philosophy is not one of the natural sciences . . . The object of philosophy is the logical clarification of thoughts. Philosophy is not a theory but an activity. A philosophical work consists essentially of elucidations. The result of philosophy is not a number of 'philosophical propositions,' but to make propositions clear (*Tractatus* 4.1111, 4.112).

The *Tractatus* convinced members of the Vienna Circle that it was possible to draw a definite boundary around factual discourse and so a definite boundary around the enterprise of science. Whereas Wittgenstein had applied the method of analysis to philosophy, they set to work to apply it to factual, empirical propositions about the physical world to determine what was science and what was metaphysics.

It was the logicism of Russell and Wittgenstein that led the logical positivists to distinguish their kind of empiricism from earlier kinds. In particular, the emphasis on logical explication led to the view that 'the cognitive content of science can be fully and best expressed by identifying the formal structure of its theories; and that the epistemic evaluation of science requires reference only to the formal relation between data and hypothesis' (Kincaid, 1998: 560).

1.3 The revolution in philosophy: logical positivism

In August 1929 three members of the Vienna Circle set out the views of the Circle in the form of a pamphlet (Hahn et al, 1929). Dedicated to Schlick (born 1882) and written by Neurath (born 1882), it was edited by Hahn and Carnap (born 1891). The following month, in an effort to reach out to similarly oriented movements and 'a wider public', they published it on the occasion of the First Conference on Epistemology held in Prague, organised by the Ernst Mach Society and the Society for Empirical Philosophy (Berlin). In 1930 a further attempt to disseminate its ideas received a major breakthrough when Carnap, together with Reichenbach (born 1891) of the like-minded Berlin society, founded and edited the journal *Erkenntnis*. These views became known as logical positivism, a term first used in articles by Kaila and Petzall, two Nordic sympathisers, around 1930 (Mautner, 1999: 438) and by Feigl and Blumberg (1931). Ayer (1936) introduced logical positivism to the English-speaking world. For our purposes we will take the writings of Schlick, Neurath and Carnap as primarily representative of logical positivism. If we had to have chosen one further

representative it would have been Reichenbach. He and Carnap had met already in 1923 and it was he who introduced Carnap to Schlick (Internet Encyclopedia of Philosophy, 1999).

The aim of the pamphlet was to openly profess a new 'scientific conception of the world'. The ultimate goal was that of a unified science. The counterpart of this admiration for science was the radical rejection of all philosophy as it had traditionally been practised up until then.

Hanfling (1981: 31) points out that for a long time a lack of progress in philosophy had been noticed. The old questions (and old answers to them) kept being discussed again and again. By contrast, since the rise of modern science in the seventeenth century, 'science, using empirical methods, has been astonishingly successful in providing answers'.

In the light of this state of affairs logical positivists rejected the claim of philosophy to yield substantial knowledge about the world. Instead traditional philosophy should be seen as a futile attempt to answer the unanswerable questions of metaphysics regarding the ultimate nature of reality. Henceforth the discovery of substantive propositions about the world was to be the exclusive task of science. The new role for philosophy was to be drastically revised. Instead of masquerading as the superior or equal of science it was to be the servant, or as Ayer (1978: 97) more politely put it the 'handmaiden', of science. Both Schlick (1925) and Wittgenstein (1922) regarded the task of philosophy as limited to the clarification of basic concepts as stated in both ordinary and scientific language (White, 1999a: 592).

Friedman (1998: 791) has argued that Einstein's special and general theories of relativity 'directly stimulated Schlick, Reichenbach and Carnap to initiate a parallel revolution in philosophy'. Likewise Suppe (1977: 7) has cautioned at exaggerating the anti-metaphysical origins of logical positivism and emphasized the extent to which the Vienna and Berlin groups were scientists or scientists turned philosophers. He argues logical positivism stemmed from an attempt to interpret the then recent revolutionary developments in physics (cf Gillies, 1993: 20).

> Following Mach, the subject matter of scientific theories is phenomenal regularities; but theories characterize these regularities in terms of theoretical terms. Following Poincaré, these theoretical terms . . . could be mathematical . . . (and) are nothing other than conventions used to refer to phenomena, in the sense that any assertion made using them could be made in phenomenal language as well . . . (Therefore) . . . the laws of a theory . . . could be expressed mathematically . . . (and) are nothing other than conventions for expressing certain relations holding between phenomena (Suppe, 1977: 11).

Here we see that Suppe views the logical positivists as disregarding Poincaré's *commodisme* (which maintained that only the language of science is arrived at by convention) and instead interpreting his doctrine as maintaining that scientific laws do not express empirical truths about the world, but are mere conventions. We will return to conventionalism in section 1.3.5. At this stage Stadler's (1998: 608) point appears relevant: he maintains that, 'contrary to its popular reputation, a heterogeneous pluralism of views was in fact characteristic of the Vienna Circle' (for example, with regard to ethics, realism, and verificationism).

When Russell's (1903) contention that mathematics can be reduced to logic is incorporated into the synthesis of Mach's and Poincaré's views we have, what Suppe (1977: 12) labels, following Putnam (1962), the original version of the positivist 'Received View' on theories. He notes that its earliest published form appears to be that of Carnap (1923). The original version was concerned with solving the problem of metaphysical notions in science, ie, theoretical entities such as mass and force. With the publication of Wittgenstein's (1922) its scope was extended to language in general: the verification theory of meaning stated that all cognitively significant statements about the world must be empirically verifiable.

We shall now set out five features of logical positivism that appear relevant to the methodology of Hutchison:

1.3.1 An empiricist theory of knowledge

Logical positivism is to be understood as an empiricist *theory of knowledge*: knowledge claims about the world are to be justified by an appeal to experience. It is a positive theory in that it attempts explanations only in terms of experience - 'it

calls on one set of observations to explain another set, but never goes outside the observational framework' (Caws, 1965: 318). The main emphasis therefore falls on the verification of propositions in terms of experience. In logical positivism verification is connected with meaning: the verifiability principle of meaning. This principle gave the logical positivists a powerful tool to separate science from metaphysics, the criterion of verifiability (Hanfling, 1981: 31 ff). It is this criterion of verifiability which is the focus of section 1.3.1. Statements which were verifiable were deemed meaningful and therefore admissable to the project of science. Statements which were unverifiable were deemed meaningless and defined as metaphysics. Popper (1934), referred to by Neurath as the 'official opposition', adopted an alternative view. He substituted falsifiability for verifiability. Falsifiability acted only as a criterion whereby to demarcate science from non-science, not as a criterion of meaning. While this did not categorically rule out metaphysical entities from science, it still facilitated the empirical testing central to empiricism.

Kolakowski (1972: 10) has pointed out that, strictly speaking, positivism is not a theory of knowledge but merely a set of rules which express a 'normative attitude' about how we are to use terms such as 'knowledge' or 'science'. In following these rules, it limits itself strictly to the positive facts of experience. This is why Ayer (1959: 10) refers to Hume's polemical final paragraph of his 1748 *Enquiry Concerning Human Understanding* (1975: 165) - in which he consigns everything other than 'quantity or number' and 'matter of fact and existence' to the flames - as 'an excellent statement of the logical positivist's position'.

Logical positivism differed from previous positivisms in its use of the new symbolic logic that had recently become available. Where previous logic, dating from Aristotle had centred on concepts, the new logic focussed on the logical analysis of propositions. Where Wittgenstein had applied logical analysis to philosophy, the logical positivists sought to apply it to empirical science. Logical positivism also differed from previous positivisms in arguing that, not only knowledge, but in addition even the meaning of statements, is wholly constituted by experience. They proposed that they could demonstrate this by using the method of analysis (Hanfling, 1981: 44).

We will be concerned with the verification principle in so far as it gave the logical positivists a tool whereby to separate out metaphysics (as well as aesthetics, ethics and value judgements, and pseudo-science) from science. We will be concerned to point out that metaphysics is not knowledge, rather than that it is meaningless. In the next section we will be concerned with the verification principle as a theory of meaning. Having dismissed metaphysics as meaningless, the logical positivists were obliged to point out what was meaningful. In both sections the process of verification is given by the method of logical analysis. Indeed, Hanfling (1981: 44) describes logical analysis as their 'general approach'.

While the logical positivists belong to the empiricist theory of knowledge tradition, they brought to it a special concern with meaning. According to Hanfling (1981: 7), the question of knowledge became secondary to that of meaning. Whatever the case, he shows that the two questions are intimately connected. To ask how I know that something is the case I must first of all understand what that something means. Alternatively, to understand what something means involves understanding in what way, if any, I could come to know it.

The logical positivists of the Vienna Circle are popularly known by the verifiability principle of meaning. According to Carnap (1959: 146 [1957]), it was Wittgenstein who first formulated the principle. Arising out of the special relationship between Wittgenstein and Schlick, it was published for the first time by Waismann (1930-31). It is often stated in the following form: 'The meaning of a proposition is the method of its verification' (Hanfling, 1981: 7). Schlick (1959: 97 [1932-3a]) phrased it slightly differently as 'The meaning of a proposition is identical with its verification'. Ayer (1959: 13) states it as : 'the meaning of a proposition is its method of verification'. The principle thus moves from the general proposition (that to understand the meaning of something *involves understanding* the way in which we come to know it) to a stricter proposition: the meaning of something is *nothing other* than the way in which we come to know it. The verifiability principle constitutes this second proposition (Hanfling, 1981: 7).

The verifiability principle attempted a general theory of meaning. From this principle

it followed that empirical statements were meaningful since they were verifiable by observation and experiment. That they were considered meaningful does not mean to say that they were true. The process of verification might prove them false. But the fact that they were capable of being either true or false made them meaningful. By contrast, metaphysical statements were those statements which were neither capable of being true or false. The other kind of meaningful statements recognised by logical positivists were those of logic and mathematics. Kant had regarded these as synthetic a priori statements. Following Wittgenstein, the logical positivists regarded them as analytic, ie, they were viewed as not capable of yielding any new knowledge about the world. They were meaningful because they were capable of being true or false. If they were true they were true by definition, ie, tautologically true, while if they were false they represented logical contradictions.

While one concern of the logical positivists was with providing a general theory meaning, a more primitive concern was with combating metaphysics. For this purpose what was needed was a criterion of meaningfulness. Ayer attempted a weaker form of verificationism which avoids some of the difficulties of the verifiability principle. It consists of a criterion of meaningfulness. It is 'more modest than the principle; it is entailed by, but does not entail, the principle' (Hanfling, 1981: 33). According to Ayer (1946: 48):

> We say that a sentence is factually significant to any given person, if, and only if, he knows how to verify the proposition which it purports to express - that is, if he knows what observations would lead him, under certain conditions, to accept the proposition as being true, or reject it as being false.

According to this criterion a statement is meaningful if, and only if, it can be verified. 'It was this criterion which was thought to bring about a revolution in the history of philosophy' (Hanfling, 1981: 31).

Ayer introduced his weak sense of verifiability because he acknowledged that the stronger verifiability principle of meaning excluded many scientific statements which, though meaningful, were not conclusively verifiable. 'It seems to me that if we adopt conclusive verifiability as our criterion of significance, as some positivists have proposed, our argument will prove too much' (Ayer, 1946: 50). This was what

Popper referred to when he remarked that in their enthusiasm to exclude metaphysics, the positivists had ended up in ruling out science. Ayer's most important concern was that the stronger principle ruled out scientific laws since these necessarily take the form of general statements and general statements cannot be conclusively verified. However, his attempt met with criticism that left it largely crippled.

Far more successful was Popper's (1934) proposal that falsifiability be the criterion of demarcation between science and non-science. He pointed out that this was not a criterion of demarcation between meaning and meaningless statements. Indeed, he regarded the logical positivists' theory of meaning as a disaster for the philosophy of science (Hacking, 1983: 43). While Popper had several reasons for introducing his new criterion, the one that is of relevance to this section is that he did not suffer from the positivists' phobia towards metaphysics. While he accepted that metaphysics was not part of science, he was not willing to label it as meaningless. Indeed it could be useful to the project of science.

Nevertheless it was via the verifiability criterion that the logical positivists believed they had a weapon which enabled them to deal decisively with metaphysics. Carnap (1959b [1931-2]) points out that previously opponents of metaphysics had declared it to be false, uncertain or sterile. However, modern logic had made it possible to give 'a new and sharper answer to the question of the validity and justification of metaphysics' (Carnap, 1959b: 60 [1931-2]). Logical analysis yields the result that the alleged statements of metaphysics 'including all philosophy of value and normative theory . . . are entirely meaningless. Therewith a radical elimination of metaphysics is attained which was not yet possible from the earlier anti-metaphysical standpoints' (ibid: 61). Carnap (1959b: 80 [1931-2]) explains that he uses the term 'metaphysics' for 'the field of alleged knowledge of the essence of things which transcends the realm of empirically founded, inductive science'. He lists as examples of metaphysics the 'systems' of Fichte, Schelling, Hegel, Bergson and Heidegger. Ayer (1959: 16) gives the following examples of metaphysics: McTaggart's statement that time is unreal, Berkeley's contention that physical objects are ideas in the mind of God, and Heidegger's proposition that 'nothing nihilates itself'.

Ayer (1959: 10) points out that the logical positivists of the Vienna Circle did not go

so far as agreeing with Hume that all metaphysical works should be consigned to the flames since they might have 'poetic merit' or express an 'interesting attitude to life'. Their concern was that, following from the verifiability criterion outlined above, since metaphysical statements could not state anything that was either true or false, they could not add anything to knowledge. 'Metaphysical utterances were condemned not for being emotive, which could hardly be considered as objectionable in itself, but for pretending to be cognitive, for masquerading as something that they were not' (Ayer, 1959: 10-11). He goes on to note the well-known objection to the verifiability principle: that it is not itself verifiable. He resolves its status by contending that it should be interpreted as a convention. While it was to some extent descriptive 'it became prescriptive with the suggestion that only statements . . . that were capable of being true or false should be regarded as literally meaningful' (Ayer, 1959: 15). While he accepts that, so interpreted, it does not follow that statements that are neither true nor false are nonsensical, its merit is that it

> removes the temptation to look upon the metaphysician as a sort of scientific overlord. Neither is this a trivial matter. It has far too often been assumed that the metaphysician was doing the same work as the scientist, only doing it more profoundly; that he was uncovering a deeper layer of facts. It is important to emphasize that he is not in this sense describing any facts at all (Ayer, 1959: 16).

Ayer's interpretation of the verifiability criterion as prescriptive is in line with Kolakowski's (1972: 11) interpretation of positivism as a normative attitude towards what constitutes knowledge. Yet this outstanding, or revolutionary feature of logical positivism, the criterion of verifiability, was the weapon which apparently allowed them to deal a crippling blow to the status of metaphysics. More immediately, as far as economics is concerned, along with metaphysics it ruled out value judgements and normative questions as having any cognitive status: apart from analytic statements, only positive questions of fact had cognitive significance. It thereby re-enforced the importance of the need, long accepted in the methodology of economics (Hutchison, 1964), to distinguish between positive and normative statements.

1.3.2 An empiricist theory of meaning

Logical positivism is to be understood, via the verifiability principle, as a *theory of meaning* (rather than as a criterion for distinguishing science from metaphysics). Here we shall examine the verifiability principle (rather than the criterion). Here the method of logical analysis, or language analysis, is used in an attempt to reduce propositions in ordinary language to statements in terms of the empirical base. This involved a commitment to the analytic-synthetic distinction. The expositions by Schlick (1925), Carnap (1928) and Ayer (1936) drew on phenomenalism. Concerning the empirical base, Neurath later converted Carnap from a phenomenalist to a physicalist stance.

The idea that the meaning of something is to be found outside language is central to the verifiability principle. It is not saying that the meaning of 'It is raining' means the same thing as another set of *words* 'putting one hand out the window' which might describe the process of verification. It is rather identifying the meaning with the method of verification itself (Hanfling, 1981: 18). Wittgenstein (1922) had first outlined this idea in his theory of language in which propositions directly picture the empirical base: 'The name means the object. The object is its meaning' (*Tractatus* 3.203). The meaning of a proposition was to be found via logical analysis: 'To analyse a proposition means to consider how it is to be verified' (Waismann, in Hanfling, 1981: 45).

The logical positivists accorded meaning to only two kinds of statements: those of mathematics and logic (which were analytic a priori) and those which could be verified by experience (which were synthetic a posteriori):

> It is precisely in the rejection of the possibility of synthetic knowledge a priori that the basic thesis of modern empiricism lies. The scientific world conception knows only empirical statements about things of all kinds, and analytic statements of logic and mathematics (Hahn et al, 1973: 308 [1929]).

This reflected Wittgenstein's influence. He had earlier convinced Russell that mathematical statements were to be regarded as analytic rather than synthetic. The Vienna Circle's verifiability principle of meaning may be viewed as a particular

interpretation of Wittgenstein's ideas on meaning and language:

Following Wittgenstein, the logical positivists maintained that the meaning of an idea must be sought in the context of a proposition. Earlier Locke and Hume had linked the meaning of an idea to a word. Wittgenstein, following Frege, had said that a word only has meaning in the context of a proposition. Therefore to understand whether an idea was meaningful or not, required the examination of propositions, rather than words. The understanding of propositions in turn requires the use of truth-functional relations such as those of conjunction (p and q), disjunction (p or q), implication (p implies q) and negation (not-p). For example, say we want to understand the meaning of the statement 'This is a virtuous horse'. We can say that this statement is a truth-function of 'This is a horse' and 'This is virtuous'. The idea here is that the truth or falsity of the first statement depends on the truth or falsity of the latter two statements via a strict logical relation, ie, by a process of logical analysis. We should be able to break down the two latter statements into more primitive truth-functional components so arriving in the end at basic, or elementary, statements which cannot be broken down any further. In this way the meaning of the original statement would be clarified as well as what is involved in having knowledge of it (Hanfling, 1981: 11).

> Whenever we ask about a sentence, 'What does it mean?', what we expect is instruction as to the circumstances in which the sentence is to be used; we want a description of the conditions under which the sentence will form a *true* proposition, and those which will make it *false* (Schlick, 1936, quoted in Hanfling, 1981: 17).

It was over the nature of the basic or elementary statements that come at the end of analysis that there emerged a key difference between Wittgenstein and the Vienna Circle. To the verificationists of the Vienna Circle it was crucial that these statements were observation statements which facilitated a process of verification. For then it could be argued that the method of verification constituted the meaning both of the basic statements and of those derived from them by truth-functional relations. To Wittgenstein, however, their logical properties rather than any connection with verification defined the basic statements. Basic or elementary statements, for Wittgenstein, had crucially to be logically independent of each other. 'For example, if

"This is blue" entails the falsity of "This is red", the statements cannot both be elementary' (Hanfling, 1981: 11).

To the logical positivists, the elementary statements had to be observation statements. Following the verifiability principle, meaningful statements must be verifiable by reference to experience. They must be capable of being reduced to direct records of experience, or protocol statements.

> Since the meaning of every statement of science must be statable by reduction to a statement about the given, likewise the meaning of any concept, whatever branch of science it may belong to, must be statable by step-wise reduction to other concepts, down to the concepts of the lowest level which refer directly to the (empirically) given (Hahn et al, 1973: 309, cf p 306 [1929]).

The verifiability principle now gave rise to a key problem: what was to count as a statement of experience? What constituted the empirical basis, base, or ground? One attempt to answer this question is to interpret verification not in its usual active sense, but in a passive sense of being aware of phenomena. Verification in this passive sense is more usually known as phenomenalism (Hanfling, 1981: 47). For Russell this was the 'sense data' which figures in his theory of logical atomism. Schlick (1925), Carnap (1928) and Ayer (1936) followed Russell's (and Mach's) phenomenalistic interpretation of experience. However, Neurath converted Carnap from his phenomenalism to a physicalist interpretation of experience.[3] According to Halfpenny, Neurath developed his physicalist ideas out of Marx's materialism. For Neurath, physicalism avoided the metaphysical implications of materialism: '"mind" is not a product of "matter"; rather, both mind and matter exist for science only in so far as they are publically available space-time formations describable in the language of physics. Otherwise they are meaningless metaphysical concepts' (Halfpenny, 1982: 58). The phenomenalist and physicalist interpretations of the verifiability principle led to the well known protocol-sentence debate in the early 1930s. While this debate is relevant to understanding the extent to which Hutchison's (1938) contained phenomenalist and physicalist elements, these issues are best dealt with in an appendix to this chapter. In this way, we hope to achieve a shorter, and more focused, account of the major characteristics of logical positivism.

[3] The distinction between phenomenalism and physicalism is discussed in the chapter appendix.

At this stage, we want mainly to draw attention to the point that, while the logical positivists wielded the verifiability criterion (a statement is meaningful if and only if it can be verified) as a weapon against metaphysics using it to limit the sphere of knowledge to science, the principle of verifiability on which the criterion was based contained deep-seated problems that were in time to lead to significant criticisms of the logical positivism such as those of Quine (1951). In so far as the outcome of the protocol debate is concerned, what is of importance for the next section is that it was Neurath's physicalist stance that won out over phenomenalism. It was this physicalism that formed the foundation for the unification of science.

1.3.3 The quest for the unification of science

Logical positivism is to be understood as a quest for the *unification of science*. This quest needs to be viewed against the fact that in Germany in 1900 it was commonly accepted that a huge difference existed between the natural and social sciences. Neurath saw physicalism as establishing the unification of science.

In the nineteenth century Comte had proposed the view that all sciences could be integrated into a single natural system. This theme was taken up by the logical positivists of the Vienna Circle. In their manifesto of 1929 it had been explained that the ideas of the Circle consisted 'not so much of theses of its own, but rather its basic attitude, its points of view and direction of research. The goal ahead is *unified science*' (Hahn, et al 1973: 305-6, original emphasis). It is this naturalism of the logical positivists that makes their views relevant to economics and the other social sciences. They viewed science as unified in two senses (Kincaid, 1998: 559). First, it was unified in that there was one scientific method common to both the natural and the social sciences. Second, it was unified in that it was believed that the different sciences could and should be unified by being reduced to physics, that is, their positivism was reductionist rather than 'sociologistic' (Halfpenny, 1982: 55).[4]

Carnap emphasised the role of logical analysis in the unification of science:

[4] Comtean positivism was sociologistic 'because psychological study of human subjectivity was ousted by sociological study of sociological phenomena, which precede and constitute the individual psyche' (Halfpenny, 1982: 19).

> Thus, with the aid of the new logic, logical analysis leads to a *unified science*. There are not different sciences with fundamentally different methods or different sources of knowledge, but only *one* science. All knowledge finds its place in this science and, indeed, is knowledge of basically the same kind; the appearance of fundamental differences between the sciences are the deceptive result of our using different sub-languages to express them (Carnap, 1959a: 144 [1930-1]).

For logical positivists the start of this unification process was via a common language. This language was to be reached by a logical analysis, and thereby a clarification of the statements made in the various sciences. The verification principle of meaning was adopted to aid in this process of clarification (White, 1999a: 592). 'The logical positivists aimed to provide not only a unifying formal language: they also sought to establish a substantive unity. The primitives of the unifying formal language were to be based on experiences captured in one single descriptive language. This ideal arose from Neurath's (1931-2) observation that scientific propositions cannot be directly compared with experience, but only with other propositions describing those experiences. It is not sensations themselves that form the basis of science, but elementary or protocol sentences, sentences that are immediate records of experience' (Halfpenny, 1982: 55).

We have learnt in the previous section how the result of the Carnap-Neurath protocol-sentence debate appeared to nullify the hopes of grounding protocol sentences in experience. Nevertheless, it was their common belief that, despite the outcome of this debate, some solution would eventually be found. Although Neurath was the main force behind launching the *International Encyclopedia of Unified Science*, which he saw as a vehicle for achieving the unity of science, it was Carnap (1938) who provided the philosophical thesis about the unity of science. His thesis is that there is a basic kind of language to which the statements of all the various sciences are reducible. 'The core of their reductionist faith rests on the assumption that it will always be possible to reduce all empirical statements to more basic statements with clear-cut observational consequences' (Ray, 2000: 250).

Carnap (1953: 67-70 [1936-7, section 11]) writes about physicalism and a 'thing-

language'. A few years later, in writing about the unity of science, he refers to ' a unity of language in science, viz., a common reduction basis for the terms of all branches of science, this basis consisting of a very narrow and homogeneous class of terms of the physical thing-language' (Carnap, 1938, quoted in Hanfling, 1981a: 128). When Carnap says that the statements of science are reducible to a basic language, he is not using the term 'reduction' in the sense described earlier according to which a meaningful statement is analysable into a set of observation-statements. Rather, his view is primarily a thesis about words or terms. These, he maintains, must be reducible (via terms in the thing-language, eg, stone, water, elastic, soluble) to 'observable thing-predicates' by means of 'reduction statements'. Furthermore his reductionism, in its use of conditionals, is akin to operationalism and phenomenalism (Hanfling, 1981: 107-109).

Although this unity of language is far less effective than a unity of laws would be, 'it is a necessary preliminary condition for the unity of laws. We can endeavour to develop science more and more in the direction of a unified system of laws only because we have already at present a unified language'(Carnap, 1938, quoted in Hanfling, 1981a: 128). 'In such a system the laws of any particular science would be explicable from the laws of the science preceding it in a vast hierarchy. Thus, sub-atomic physics would explain atomic physics, atomic physics would explain molecular physics, molecular physics would explain chemistry and bio-chemistry, and so forth until the hierarchy even included sciences such as biology or economics or anthropology' (Musgrave, 1999: 579).

> Thus the Vienna Circle rejected the view, which many still hold, that there is a radical distinction between the natural and the social sciences. The scale and diversity of the phenomena with which the social sciences dealt made them less successful in establishing scientific laws, but this was a difficulty of practice, not of principle: they too were concerned in the end with physical events (Ayer, 1959: 21).

In this connection Musgrave (1999: 579) notes that 'the unity of science thesis is also associated with the doctrine that all sciences share the same methods - the 'unity of method' thesis'.

One of the consequences of the verifiability principle is that value judgements, as a

certain kind of metaphysical statement, were meaningless and outside the realm of science. A contrast was made between facts and values. One of the consequences of the unity of science thesis is that this exclusion of value judgements applies now not only to the natural, but equally to the social, sciences.

Ayer (1959: 22) points out that the logical positivists' stance on this matter followed on from the point of logic already made in Hume: that normative statements are not derivable from descriptive statements, that 'ought' does not follow from 'is'. However, an exception amongst the logical positivists to the exclusion of the ethical realm from science was Schlick (1930).

Even today, Hanfling (1981: 155-6) points out, 'the view that moral statements are non-cognitive is very widely accepted. It is often thought, in ordinary discussions, that merely to identify a question as moral is enough to show that it cannot be treated as one of fact, but must be left to the feelings of the individual. It is not easy, however, to produce arguments for this view'. This, he contends, was recognised by Ayer in a paper in 1949.

We will see in Chapter Three, that Hutchison (1938) followed Schlick's (1930) position on the exclusion of ethical statements in arguing that the distinction between positive and normative statements was essentially false. In 1938 he appeared to be more concerned with arguing against the view that there was a fundamental difference between the methods of the natural and social sciences, than for the view of a unified physicalist science. This may have been due to his regard for Schlick on this issue. Neurath's unified physicalist science envisaged science incorporating philosophy within science, whereas Schlick adhered to a dualism of science and philosophy recognizing a distinction between them (Stadler, 1998: 607-8).

1.3.4 Two theories of scientific method

Logical positivism is to be understood as incorporating a tension between two competing *theories of scientific method* or views about the empirical status of scientific laws. Firstly, there is the inductivist theory of scientific method. Chalmers (1982: 11) has characterized logical positivism as 'a particular brand of inductivism'.

With its roots reaching back to Francis Bacon and Mill (1843), this theory is closely in tune with empiricism, nominalism and anti-realist sentiments associated with logical positivism. Secondly, there is the hypothetico-deductive view of scientific method. This can be traced back to Herschel (1830), Whewell (1837) and Jevons (1874). More recent representatives are Popper (1934) and Carnap (1936-7). This view gives more scope to deductivism. It facilitates Popper's falsificationism which he argues avoids the problem of induction while retaining the empiricist demand for empirical testing. It is easily in tune with the view that central to science is the notion of laws which are described by general statements.

Before we look more closely at these two methods, it may be helpful to briefly clarify a related issue. This concerns the logical positivists' view of the formal structure of a scientific theory. Logical positivists viewed scientific theories as 'axiomatic theories formulated in a mathematical logic' meeting a number of specific conditions (Suppe, 1977: 16). As this Received View of the structure of a theory changed and developed, it came more and more to reflect the notion that science develops according to the hypothetico-deductive method.

> It is almost a platitude to say that science proceeds, more or less explicitly, by thinking of general hypotheses, of greater or less generality, from which particular consequences are deduced which can be tested by observation and experiment . . . For science, as it advances, does not rest content with establishing simple generalizations from observable facts: it tries to explain these lowest level generalizations by deducing them from more general hypotheses at a higher level (Braithwaite, 1953: ix).

Inductivism may be described as a theory of scientific method that emphasizes the importance to science of inductive arguments. Induction involves reasoning from the truth of particular statements to the truth of a general statement. For example, since on a number of occasions sodium when heated glows bright orange, we might conclude all sodium when heated glows bright orange. This principle of induction seems to allow us to predict that the next sample of sodium when heated will glow bright orange. However, Hume famously pointed out that there is no reason to expect that 'the course of nature will continue uniformly the same' (Jones, 1996: 574). This problem of induction applies equally to established laws such as the law of gravity. It may be accepted that while scientific laws do not give us absolutely certain

knowledge, they probably hold true. According to Salmon (1993: 288), Hume also challenged this belief. Even if a law has held reliably and regularly in the past, there is no logical reason why it should hold in the future at all. Furthermore, Chalmers (1982: 17) has pointed out that the attempt to replace the principle of induction by a probabilistic version does not overcome the problem of induction. Much of the philosophy of science since Hume is concerned with attempts to overcome the problem of induction. Popper (1934) proposed falsificationism. Conventionalism, instrumentalism and pragmatism are other positions which claim to avoid the problem of induction. The importance of the principle of induction had been recognized by Russell (1946: 699-700):

> Hume has proved that pure empiricism is not a sufficient basis for science. But if this one principle [induction] is admitted, everything else can proceed in accordance with the theory that all our knowledge is based on experience . . . without this principle science is impossible.

Gillies (1993: 3), like Chalmers (1982), also regards the logical positivists as supporters of inductivism (Gillies, 1993: 3). Although both authors make these statements within the context of chapters on inductivism as a theory of scientific method, it is ambiguous as to whether they mean that logical positivists supported particular inductivist methods within science or whether they mean that they supported inductivism as a theory of scientific method. To distinguish between these two notions, inductivism as a theory of scientific method will be referred to as 'inductivism-SM'.

We will take inductivism-SM to refer to the theory that it is by inductive reasoning that scientific theories and laws are both discovered and justified. This is what Losee (1980: 148) refers to as inductivism 'in its most inclusive form'. Inductivism-SM can be traced back to Francis Bacon's 1620 *Novum Organum*. Bacon proposed a general approach or method which, if followed, would result in the growth of scientific knowledge. According to this approach, science starts with the collection of facts via a process of systematic observation. It then proceeds to infer generalisations (laws and theories) from this factual data (Gillies, 1993: 5-6). Both laws and theories are confirmed or verified by comparing their predictions with 'all the observed facts, including those with which they began' (Blaug, 1980: 2). Mill's (1843) is often cited

as a classic description of inductivism-SM. 'Mill made extreme claims about the role of inductive arguments both in the discovery of scientific laws and in the subsequent justification of these laws' (Losee, 1980: 148). In his account Mill acknowledged his debt to Herschel (1830) and Whewell (1837).

Herschel (1830) distinguished between the 'context of discovery' and the 'context of justification'. Inductivism-SM is the view that inductive reasoning is crucial in both the context of discovery and the context of justification. Regarding the context of discovery, scientific theories and laws are interpreted as arising from inductive reasoning, that is, by induction from facts established by observation. Regarding the context of justification, scientific theories are viewed as justified to the extent that their consequences (predictions and explanations) - which may be clarified via deductive reasoning - are justified by a process of inductive inference. Herschel distinguished between the contexts of discovery and justification because he contended, contrary to Francis Bacon, that the method used to generate a theory is 'strictly irrelevant' to the question of its justification. 'A meticulous inductive ascent and a wild guess are on the same footing if their deductive consequences are confirmed by observation' (Losee, 1980: 116). Whewell (1837) broadly agreed with Herschel's hypothetico-deductive theory of scientific method. His debate with Mill led to 'the first great controversy' in the modern era of the philosophy of science (Harré, 1967: 289).

According to the hypothetico-deductive theory of scientific method, science begins with a theory, or hypothesis. This hypothesis may be the result of many different factors ranging from an inspired conjecture, or even a dream, to observations and the results of experiments. 'But the theory is not *inferred* by the scientist from any of these sources. The discovery of a new theory is a "happy guess", that may require the presence of certain causal conditions, but not a reasoning process subject to rules of inference' (Achinstein, 2000: 325, original emphasis). 'The point of the [hypothetico-deductive] model is that it employs no other rules of logical inference other than that of deduction' (Blaug, 1980: 4). It is in the context of discovery that unobservable terms, such as light waves, may be introduced.

In terms of the hypothetico-deductive approach to scientific investigation, it is only in

the context of justification that it is sensible to discuss the procedure of science. Nothing systematic can be said about the context of discovery. Logical positivists, following Reichenbach (1938), support this view. It is only in the context of justification that reasoning takes place in the attempt to defend or criticise a theory. This reasoning consists of a process of deductive reasoning in which conclusions are derived from the assumptions of the theory. If these conclusions are inductively verified by experience, the theory could be regarded as true. The problems with verification (outlined in the previous section) soon led logical positivists, notably Carnap (1953: 48 [1936-7]), to substitute the notion of confirmation for verification.

In an effort to clarify the notion of the (degree of) confirmation of an hypothesis, greater research was carried out in the field of probability and inductive inference (Caldwell, 1982: 22-3). Within the hypothetico-deductive tradition scientists (economists) put forward null hypotheses and used inductive statistics to refute those hypotheses and so confirm the alternative hypotheses (Halfpenny, 1982: 102). Much of this work on inductive reasoning has followed the lines Jevons (1874) initiated. 'In the works of J M Keynes, Richard von Mises, Hans Reichenbach, and Rudolf Carnap the probabilistic exegesis of the measure of inductive support has been developed' (Harré, 1967: 293).

A weaker version of the hypothetico-deductive method was outlined by Popper (1934). If a theory survives attempts to falsify it, it may be regarded as corroborated. Whereas the logical positivists test theories by looking to inductive support, Popper's process of testing by falsification allegedly does not require inductive reasoning. The problem of naïve falsificationism is outlined by the Duhem-Quine thesis: theories cannot be tested one by one, but only in groups. Since all testing must be carried out on groups of linked hypotheses, it is a matter of choice, or convention, which of these hypotheses we regard as having gained, or lost, support as a result of testing (Stewart, 1979: 224).

But common to both the inductivist verificationist and Popper's falsificationist approaches, 'a hypothetico-deductivist can postulate any unobservable entities or events he or she wishes in the theory, so long as all the observational conclusions of the theory are true' (Achinstein, 2000: 326). This sets alarm bells ringing for any true

blue inductivist-empiricist, that is, one subscribing to inductivism-SM.

According to Achinstein, not only Mill, but also Newton were avowed inductivists and opponents of hypothetico-deductivism, or the method of hypothesis. 'Mill cites as an example the wave theory of light, which postulates an ether':

> The existence of the ether still rests on the possibility of deducing from its assumed laws a considerable number of actual phenomena . . . Most thinkers of any degree of sobriety allow, that an hypothesis of this kind is not to be received as probably true because it accounts for all the known phenomena, since this is a condition sometimes fulfilled tolerably well by two conflicting hypotheses; while there are probably many others which are equally possible, but which, for want of anything analogous in our experience, our minds are unfitted to conceive (Mill, 1959: 328 [1843]).

Achinstein points out that many observed phenomena derived from the wave theory (eg reflection, refraction, and diffraction) were also derived from the competing particle theory. 'So the wave theorist should not be able to claim truth, probability, or even very much support for his theory from the fact that it entails these observed phenomena' (Achinstein, 2000: 326).

Problems such as this led Whewell (1840) to formulate a more sophisticated version of the hypothetico-deductive method, the 'consilience of inductions'. According to this version, it is not sufficient that the theory entail simply known phenomena that have been observed: it must also entail new ones. If it does so then it is certain, or according to a weaker interpretation, highly probable or strongly supported. Mill remained unconvinced. He argued that, given the above, there could still be an incompatible theory that entails the same phenomena. If so, Mill concluded, one cannot claim truth or even high probability for the (first) theory. According to Achinstein, hypothetico-deductivists have so far been unable to answer Mill. Gillies (1993: 39-53) describes two accounts of scientific discoveries (Fleming's penicillin and Farben's sulphonamide drugs) which, he argues, have occurred as the result of 'creative induction' in the case of penicillin and 'mechanical or Baconian induction' in the case of sulphonamide drugs. Both cases support the view that science progresses according to the inductivist method of procedure. Supporting this view, Hacking (1983: 149) contrasts the experimental method of scientists with

philosophers' tendency to 'constantly discuss theories and representation of reality'.

'Mill, by contrast to Whewell, proposes what he calls the "deductive method" for making inferences to scientific theories. It has three steps, the first of which is inductive and includes inferences to causes and laws. The second step Mill calls "ratiocination" which involves logical reasoning to determine the consequences of the causal claims. The third step is verification. What Whewell omits, says Mill, is the crucial inductive step at the beginning' (Achinstein, 2000: 327). The debate continued when Jevons (1874) criticised Mill's claim that justification of hypotheses is by satisfaction of inductive schema and re-asserted the importance of deductive reasoning (Losee, 1980: 158).

This section has aimed at combating the view, contained for example in Caldwell (1982), that the hypothetico-deductive method favoured by the logical positivists and Popper superceded their espousal of an earlier cruder inductivism. The matter is more complex. For example, Gillies has argued that, even in the natural sciences, there is support for the view that science progresses according to the inductivist method of procedure. When we turn to Chapter Three, we will see that Hutchison makes use of both inductivism-SM and hypothetio-deductivism. An obvious example of Hutchison's use of hypothetico-deductivism is his criterion of testability which draws on Popper (1934) and calls for the 'finished propositions' of science to be conceivably empirically falsifiable. Yet, Hutchison also refers to the inductivist and phenomenalist approaches of the early Carnap and Schlick. And, he espouses an even more inductivist-SM stance in his sympathy with the institutional and historical approaches of Schmoller and Cliffe Leslie. Hutchison then, may be seen as a drawing attention to the importance of inductivism in economics.

1.3.5 The status of theories and laws

Logical positivism is to be understood as involving views about the realist or non-realist *status of theories and laws*. Here we adopt and adapt classifications from Hacking (1983: 42) and Achinstein (2000: 328). Firstly, we will proceed to explain how the holding of realist or anti-realist views affects the status of theories and laws that deal with unobservable entities. It is here that the positivists' account (in the

Received View) of theories as no more than formal axiomatic structures or systems reflects their anti-realist views on this issue. These include instrumentalism, pragmatism and conventionalism. Secondly, we will describe how the holding of realist or anti-realist views affects our interpretation of the causal nature of scientific laws. Here Hempel and Oppenheim's (1948) account of explanation may be viewed as in line with Hume's view on causation. This is the view that there are no essential relationships between matters of fact. In other words, it involves the assumption that experiences are given individually or atomistically. It is this view that sharply distinguishes empiricists from rationalists and various kinds of neo-Kantians (Spurrett, 1998). The problem of attaching necessity to the laws in Hempel and Oppenheim's account has led to analysis of non-Humean views on causation (Halfpenny, 1982: 68) since 'Humean analysis of causation in terms of constant conjunction fails to distinguish laws from accidental regularities' (Halfpenny, 1982: 74).

Realism and anti-realism: how theories and laws about observables should be interpreted

These opposing views take many different forms. Williamson (1995) even contests the notion that they are views or positions. Instead they are better thought of as directions. 'To assert that something is somehow mind-independent is to move in the realist direction; to deny it is to move in the opposite direction' (Williamson, 1995: 746). In medieval scholastic philosophy realism was opposed to nominalism (only particulars not universals are real, ie, mind-independent) and conceptualism (universals are mind-dependent) (Mautner, 1999). Realism may also be opposed to idealism (only minds exist) (cf Torr, 1999). According to Chalmers (1982: 147), 'realism typically involves the notion of truth. For the realist, science aims at true descriptions of what the world is really like'. Hacking (1983: 21-31) describes what he labels 'scientific realism'. His account is broadly in line with that given by Achinstein below.

According to Achinstein (2000: 329), one prominent type of realism makes two claims: unobservable entities exist independently of us and theories which claim to describe are either true or false. By contrast, one prominent type of anti-realism

denies unobservable entities exist and asserts that only observable entities exist so that only the latter may be construed as making claims that are either true or false.

Harré (1967: 289) presents a 'grand opposition' between anti-realist and realist philosophers of science. The anti-realists are characterised by holding to (a) inductivism-SM, (b) Humean causality, and (c), aiming at the reduction of all theoretical concepts to functions of observables. In this camp have been J S Mill, Mach, Duhem, Hempel, Carnap and Braithwaite. He gives, as an example of the power of this view, the chemists' abandonment of the atom in the nineteenth century and an official prohibition on theoretical papers in the *Journal of the Chemical Society* (1967: 291).

Realists opposed to this tradition include Whewell and Campbell (1920). These hold that (a) theory construction is more complex than inductivism will allow; (b) the meaning of theoretical concepts is not exhausted by logical function and observational basis, and (c) that the actual procedures of science have more authority than formal logic has. A position that is difficult to classify is that of Poincaré who, as we have earlier seen, espoused *commodisme* rather than what has since been called conventionalism.

From the side of the anti-realist camp Mach's extreme reductionism combined with his sensationalism exerted a powerful influence on logical positivism.

> The net effect of this movement was a steady denigration of the power of theory in favour of logically ordered structures of empirical concepts. The marriage of Mach's views on science with Russellian logic initiated the era of hypothetico-deductivism, when a theory was thought to be an axiomatic structure, like formal logic or geometry (Harré, 1967: 291).

According to the *Internet Encyclopedia of Philosophy*, 'a scientific theory - in Carnap's opinion - is an interpreted axiomatic formal system'. As mentioned in the previous section, the Received View construes 'scientific theories as axiomatic theories formulated in mathematical logic' and comprising logical, observational and theoretical vocabularies. A set of correspondence rules (dictionaries, interpretative systems) explicitly define the empirical counterparts of theoretical entities in terms of

a physicalist language (Suppe, 1977: 16). 'The system gains empirical meaningfulness only when the system is given some empirical interpretation by means of interpretative sentences' (Caldwell, 1982: 25).

Caldwell points out that, as the Received View developed (Braithwaite, 1953; Hempel, 1958), it was accepted that theories be judged as entire systems: each theoretical term need not be explicitly defined. Theories are viewed as hierarchical systems with the higher-level hypotheses often referring to theoretical entities, while the lower-level hypotheses describe observable phenomena and are the propositions which may be empirically tested. An advantage of this development was that theoretical terms did not need to be tested directly: they gain meaningfulness indirectly when the lower-level propositions are empirically confirmed. This seems to allow a more normal role for theoretical terms in science. However, this more normal role turns out to still be a very hollow one.

Before expanding on this contention, a more general limitation of the Received View needs to be pointed out. The indirect testability hypothesis implies that, unless a body of non-theoretical terms can be independently distinguished from theoretical terms, the status of theoretical terms is ambiguous since no observational interpretation would be possible. There are three types of problems facing the drawing of this distinction: (1) distinguishing between theoretical and non-theoretical terms, (2) distinguishing between observable and non-observable terms, and (3), the question of passive observation (Chalmers, 1982: 24-8).

It turns out that the indirect testability hypothesis does not significantly change the Received View on theories which remains only marginally, rather than substantively different from Mach's fictionalist view of theories. In Mach's view theoretical terms had no meaningful role to play in science, being purely temporary devices which would be replaced by observational terms as science progressed:

> It would not become physical science to see in its self-created, changeable, economical tools, molecules and atoms, realities behind phenomena . . . The atom must remain a tool for representing phenomena, like the functions of mathematics (Mach, 1894: 206).

While Caldwell is quick to point out an advantage of the Received View (theoretical terms can now be accommodated in terms of the indirect testability hypothesis), he neglects to point out, as has Harré (1967), that in the Received View theories are reduced to 'empty shells' being no more than 'logically ordered structures of empirical concepts' - without any meaning whatsoever apart from a Procrustean empirical interpretation.

> The fact that axiomatic structures can never lead into the new and hitherto unknown, that they are, precisely because of their logical coherence, quite unfruitful, does not seem to have bothered the advocates of the hypothetico-deductive view. The logicians of this era were apparently not interested in the question of theory origin or theory growth, but only in the question of the best mode of formalizing theories that were already known (Harré, 1967: 291).

Here Harré's statement could be a little misleading when referring to advocates of the hypothetico-deductive view. Caldwell (1982: 26) is quite correct in contending that the hypothetico-deductive method is itself entirely neutral between realists and anti-realists. For example, Popper (1934), an avowed realist, sets out his falsificationist view of science in terms of the hypothetico-deductive method. Rather, it is the insistence by these particular 'advocates' and logicians that theories be construed as no more than axiomatic structures that is anti-realist. For example, those positivists who were instrumentalists (Mach, Schlick) were not interested in theory growth because they were not interested in truth but rather in usefulness (Leplin, 2000). (Neither would pragmatists be interested in theory growth because they seem to identify truth with usefulness.) (Mautner, 1999: 277; Worrall, 2000). This anti-realist nature of much of positivist thought has been discussed by Hacking (1983: 41-57). Fifty years earlier Planck had criticised the phenomenalist and instrumentalist aspects of positivism from a realist perspective:

> Of what value to the world are the sensory impressions of a mere individual? . . . No science can rest its foundation on the dependability of single human individuals. And the moment we have made that statement we have taken a step which puts us off the logical pathway of the positivist system. We have followed the call of common sense. We have taken a jump into the metaphysical realm; because we have accepted the hypothesis that sensory perceptions do not of themselves create the physical world around us, but rather that they bring news of another world which lies outside of ours and is entirely independent of us (Planck, 1932: 80-81).

When we turn to examine Hutchison's 1938 intervention in Chapter Three we argue that he adheres to certain aspects of the anti-realist orientation of the logical positivists, especially that of Humean causality. Yet he approvingly cites the realist views of Campbell (1920). He accepts that theory construction cannot be limited to inductivism-SM and, following Popper, that there is a need for employing the hypothetico-deductivist method. But, in turn, he has serious reservations with the extent to which in this method theories are interpreted as axiomatic formalised structures since this emphasises the logical coherence, rather than the empirical realism, of a theory.

Realism and anti-realism: how the causal and explanatory nature of theories and laws is to be interpreted

Anti-realists view science as a body of laws that explain and predict, but not in a way that would satisfy realists. Probably the most well known positivist and anti-realist leaning account of scientific explanation and prediction is given by Hempel and Oppenheim's (1948) deductive-nomological (D-N) model of scientific explanation. It is also known as the covering-law thesis (Newton-Smith, 2000). It states that a scientific explanation must consist of two parts: an explanans and an explanandum. The explanans, or premises, contain the antecedent conditions and the general laws. The explanandum is the sentence describing the phenomena to be explained. It follows from this account that both explanation and prediction involve the same logical procedure. This is not all. For an explanation to be sound three logical, and one empirical, condition(s) of adequacy must also be fulfilled: (i) the explanandum must follow logically from the explanans; (ii) the explanans must have general laws; (iii) the explanans must have empirical content. The empirical condition, (iv), is that the explanans must be empirically true (Hempel and Oppenheim, 1948, [1953: 321-24]). In this model explanation is deductive in form. Given the explanans, the explanandum *must* occur. Partly as a result of problems surrounding the concept of a general law, and partly to take account of the fact that many explanations in science make use of statistical laws, Hempel (1962) developed a second inductive-probabilistic (I-P) covering law model of scientific explanation. Both types of covering-law models have been subjected to three main realist criticisms.

The first revolves around the 'symmetry thesis', ie, the view that there is a logical symmetry between explanation and prediction. The idea is that every explanation must be a potential prediction. The only difference between the two is a temporal one: while explanation refers to events taking place in the past, prediction concerns future events. Critics have contended that explanation and prediction are not the same thing: a prediction requires only a correlation. Likewise explanation need not imply prediction. For example, Darwin's theory of evolution explains how species developed, but cannot predict what new forms will emerge. Caldwell (1982: 30) has argued that the covering law model is at odds with an instrumentalist view of theories since instrumentalism 'cannot claim an explanatory role for theories'. However, it should be pointed out that the notion of explanation involved in the covering law model is very 'thin'. It is not something that would satisfy many realists, since its conception of explanation is essentially Humean in that it draws on Hume's 'billiard ball' model of causation. According to this, causation is nothing but the constant conjunction of two events that happen to be contiguous in time and space. The earlier event is called the 'cause' while the later event is called the 'effect' although there is no necessary connection between them.

The second arises out of problems with the concept of a general law referred to above. The problem concerns the difficulty of identifying the necessity that characterises laws but is absent from accidental generalisations. The problem arises because the Humean analysis of causation, to which positivists subscribe, fails to distinguish laws from accidental regularities (Halfpenny, 1982: 74). One way out of this difficulty is to view laws as expressing causal necessity. 'Yet many early positivists were antithetical to causality because of its mysterious, metaphysical undertones. . . . Russell (1917) held that causal talk belongs to pre-scientific discourse, and "causality" featured on Neurath's *index verborum prohibitorum*' (Halfpenny, 1982: 68). Those not adopting the positivist anti-metaphysical stance, eg certain realists, can easily live with the notion that explanation involves a causal mechanism which guarantees that the relationship between the two events is a necessary one and is not just a chance occurrence. 'Explanation requires some sort of causal narrative. Whereas a prediction states that something will be the case, an explanation concerns how it comes to be the case' (Coddington, 1972: 5). For realists, such as Popper, explanation is a major goal of science. It therefore needs to be clearly distinguished

from prediction: 'I consider the theorist's interest in explanation - that is, in discovering explanatory theories - as irreducible to the practical technological interest in the deduction of predictions' (Popper, 1959: 61 n).

The third revolves around the contention that the covering-law models describe all legitimate explanation in both the natural and social sciences. Three other types of explanation will be mentioned here. Firstly, challenging the unity of science notion, it has been argued that in the social sciences motivational explanations are legitimate although they do not generally provide predictions. Secondly, it has been contended that another legitimate form of explanation is functional: the characteristics of some phenomenon are explained by reference to certain ends or purposes which the characteristics are said to serve. An example from botany would be: 'The function of chlorophyll in plants is to enable plants to perform photosynthesis'. Thirdly, non-positivist models of explanation have been presented by Harré (1960), Hesse (1966). Other non-positivists have put forward the notion of story-telling as a valid form of explanation in social science (McCloskey, 1989: 227; Ward, 1972, ch 12).

When we turn to Chapter Three, we argue that Hutchison in 1938 leans towards a Humean perspective on the problem of causality. He accepts Hempel and Oppenheim's (1936) view that empirical laws are central to scientific explanation (Hutchison, 1938: 64-5) and Popper's view that laws should be conceivably falsifiable. Yet, for Hutchison, such laws are inductive inferences (1938: 62). Popper (1960: 115), drawing on the hypothetico-deductive approach, argued that scientific predictions need to be based on laws which he distinguished sharply from trends. Hutchison (1977: 19-23) responded by arguing that, despite its 'obvious weaknesses', the 'nature of the material' in 'some important branches' of economics seem to allow for little more than induction of (mere) trends. We argue in Chapter Three that, despite his reference to Hempel and Oppenheim (1936), Hutchison in 1938 lent more towards emphasising the relevance of inductivism, rather than the covering-law thesis, for the purposes of 'explanation' and prediction in economics. Furthermore, he enthusiastically supports (Hutchison, 1992: 16-17) alternative forms of explanation in economics such as that of Ward (1972).

Conclusion

We started this chapter by looking at the responses of Mach and Poincaré to developments in nineteenth century natural science. Mach's response was radically empiricist, especially in terms of his phenomenalist or sensationalist theories of knowledge. Viewing science as a practical response to everyday problems, he rejected the view that it could lay claim to any universal timeless, and especially metaphysical, knowledge of the world. While he influenced the logical positivists and Hutchison, it should be remembered that he also influenced Einstein. We distinguished, with Harré, Poincaré's *commodisme* from other forms of conventionalism. According to this, *only* the language of science is arrived at by convention. In this regard, Poincaré's comment that 'all the scientist creates in a fact is the language in which he enuciates it' was explicitly noted by Hutchison (1938: 36). Following Gillies (1993), we have documented how Poincaré came, in his 1905 work, to regard the laws of science as empirical laws founded on induction and not simply merely conventions. What is important for our purposes is that Hutchison (1938) draws on Poincaré's 1905 rather than his 1902 views on the status of scientific laws. We see then, that both Mach and Poincaré acknowledged the importance of induction in science. This point will be relevant when fielding criticisms of Hutchison's sympathy towards inductivism.

We then turned to the revolutionary developments in logic and mathematics pioneered by Frege and Russell. While Russell showed how mathematics could be reduced to logic, it was Wittgenstein's demonstration of how logic reveals the structure of language that directly influenced not only the logical positivists, but also Hutchison, who learnt about the *Tractatus* in the early 1930s at first hand from his student friends at Cambridge who numbered amongst Wittgenstein's *amenuenses*. Wittgenstein revolutionised the conception of philosophy. Rather than a potential source of metaphysical truths about the world, it was to be limited to the activity of analysing and clarifying the concepts and propositions of science.

The logical positivists adopted and adapted Wittgenstein's verifiability principle of meaning. This principle gave them a powerful tool to separate science, or knowledge, from various forms of pseudo-science, chief amongst them metaphysics: a statement

is only meaningful if it can be verified. Popper replaced this criterion with one of falsifiability – which demarcated scientific from non-scientific statements. We will see in Chapter Three that Hutchison's (1938: 9) criterion of testability leans more towards Popper's criterion than that of the logical positivists.

The logical positivists also used their verifiability principle of meaning towards formulating a general theory of meaning. This involved attempts to apply logical analysis to language in order to clarify the empirical base upon which propositions rested. Schlick and the early Carnap (1928) adopted a phenomenalist approach to this question which was in keeping with an inductivist-empiricism which was linked to a correspondence theory of truth. Hutchison appeared to be sympathetic to this approach. But Neurath and the later Carnap adopted a physicalist approach which was linked to a coherence theory of truth. While physicalism facilitated Neurath's project for the unification of science, the acceptance of a coherence theory of truth fitted in with the view of a scientific theory as having a formal axiomatic structure in keeping with a hypothetico-deductivist approach to scientific procedure. As we argue in Chapter Three, Hutchison departed from this (dominant) form of logical positivism.

CHAPTER 1: APPENDIX

THE PROTOCOL SENTENCE DEBATE

'The protocol-sentence debate is about what kind of data science is supposed to be responsible to' (Cartwright and Cat, 1996: 81). Can the data be the sense data in the phenomenalist interpretation (Schlick and the early Carnap), or must we admit only data that are intersubjectively available (Neurath's physicalism)?

Phenomenalism is an empiricist theory of knowledge. It says that our knowledge of the external world is obtained via sense-experience. One of its earliest statements is by Berkeley who denied the existence of matter. This implies a phenomenalist stance since, for phenomenalists, we have no grounds for assuming that there are such things as material objects which exist beyond the immediate data of experience. Both Hume (1748) and Mill (1843) contain some phenomenalist tendencies. The latter famously referred to material objects as (no more than) 'permanent possibilities of sensation'. According to Kolakowski (1972: 11), phenomenalism denies any real difference between 'essence' and 'appearance'. But Caws (1965: 320) points out that by this denial phenomenalism, although very closely related to positivism, unlike positivism, involves a metaphysical claim about the nature of the world: 'that phenomena *are* the world - that the world of appearance is the only world there is'. By a 'short, simple and obvious step' we arrive at a 'substantial, although rather thin, world'.

> This position accounts for theoretical terms, not merely as calculating devices inserted between descriptions, but as logical constructions out of sense-data. . . . It enables one to talk about objects not merely when they are present but also when they are not, as sets of aspects or perspectives. Objects *are* phenomena, actual and potential (Caws, 1965: 320, original emphasis).

To avoid the ontological difficulties involved in this position, the logical positivists focussed on its linguistic aspects. Here the key idea is that, for a statement to be significant, it must be reducible to a statement about sense data. Strict phenomenalism goes beyond positivism in ruling out, unlike positivism, even the use

of 'empty symbols' as steps from one observational sentence to another (unless they can be reduced to sense data) (Caws, 1965: 321).

Influenced by Mach and Russell, Carnap (1928) adopted phenomenalism with its notion of the incorrigibility of the immediately given. As an introductory motto, he selected Russell's (1917: 155) famous line: 'The supreme maxim in scientific philosophising is this: Whenever possible, logical constructions are to be substituted for inferred entities'. Russell had applied this maxim to mathematics when he defined cardinal numbers as sets of sets.

> Carnap's *Aufbau* was a monumental effort to apply this maxim to all domains of scientific knowledge, and to 'construct' the natural world as we know it in a precise manner, using a single individual's experiences as substantive content, and employing the most powerful tools of symbolic logic to carry out the construction (Salmon, 2000: 234).

Reichenbach (1933) described it as fully presenting, for the first time, the Vienna Circle's scientific conception of the world. Influenced by the neo-Kantian philosopher, Bruno Bauch, Carnap had written his doctoral dissertation on the theory of space (Internet Encyclopedia of Philosophy, 1999). Richardson (1996: 310) aligns himself with recent revisionary work on Carnap which questions the strict empiricist interpretation of the *Aufbau* finding that it contains significant Kantian elements. He proceeds to expand on this theme distinguishing three phases of Carnap's movement from epistemology to the logic of science: his (1928), his (1931-2a), and finally his (1936-7). Cartwright and Cat (1996: 80) distinguish two central aspects of the *Aufbau*: 'one is the logical construction of science upwards; the second is the assumption that the foundations of the construction can be epistemologically secure' because of the incorrigibility of sense-data reports.

Physicalism, a term coined by Neurath (Mautner, 1999: 424), embraces the view that the language of science must be a language which refers to material, physical entities, and in which all basic predicates are physical. There are many interpretations of physicalism, not all of them positivist (Seager, 2000). Here we are concerned with pointing out that, rather than phenomenalism, 'Neurath, (who was) drawn to Marx's materialism and therefore opposed to the idealist flavour of phenomenalism, preferred

physicalism, where sentences about sense-data are analysable into sentences about the publically observable properties of physical objects: for example, "this object is emitting a high-pitched noise"' (Halfpenny, 1982: 89).

> Carnap had introduced the concept of protocol sentences for foundational sentences which incorrigibly report immediate sense-experiences and constitute a basis for all other knowledge. Neurath, in contrast, used the concept to characterise reports of particular observations of the physical world which, in his view, form the set of basic, but not incorrigible, sentences (Mautner, 1999: 382).

Neurath had outlined his physicalist views in his (1931, 1931a) articles. In his (1931-2) article he again presented physicalism as a non-metaphysical standpoint. Unhappy with the metaphysical overtones of the term 'world', he contended that it would be better to speak of a Vienna Circle for Physicalism rather than of one for a Scientific World-Outlook pointing out that 'world' is not a term found in science (Neurath, 1959: 282 [1931-2]). Later, he goes on to state:

> Since the views presented here are most nearly similar to the ideas of Carnap, let it be emphasized that they exclude the special 'phenomenal' language from which Carnap seeks to derive the physical language, which does not even seem to be usable for 'prediction' - the essence of science (Neurath, 1959: 290 [1931-2]).

In an article immediately following on from Neurath's, Carnap (1931-2a) concerns himself with the physical language and its relationship to the protocol language. In the *Aufbau* Carnap had been concerned with the question of how 'we achieve objective knowledge despite the subjective beginning of knowledge in private sensation' (Richardson, 1996: 330). In continuing this theme he distinguishes between two modes of speech: the 'material', or usual, mode and the 'formal' or correct mode. 'The first speaks of "objects", "states of affairs", of the "sense", "content" or "meaning" of words, while the second refers only to linguistic forms' (Carnap, 1934a: 38 [1931-2a]). In the material mode protocol statements 'refer to the given, and describe directly given experience or phenomena, ie, the simplest states of which knowledge can be had' (Carnap, 1934a: 45).

> Science is a system of statements based on direct experience, and controlled by experimental verification . . . Verification is based upon 'protocol statements' (Carnap, 1934a: 42).

However, when translated into the formal mode, 'we find a remarkable transformation . . . the crucial appeal to experience falls away' (Hanfling, 1981: 78-9). In the formal mode basic statements are not to be characterised by reference to (subjective) experience but instead by reference to inter-subjective terms and relations. In the formal mode protocol statements are 'statements needing no justification and serving as the foundation for all the remaining statements of science' (Carnap, 1934a: 45). 'Thus the crucial contact with experience is broken. The demands of empiricism are to be met by tracing logical relations between statements, and nothing more. Any reference to experience will be a confusion due to use of the material mode' (Hanfling, 1981: 79).

Neurath (1959a: 199 [1932-3]) contends that 'the fiction of an ideal language constructed out of pure atomic sentences is no less metaphysical than the fiction of Laplace's demon'. Again he stresses: 'There is no way of taking conclusively established pure protocol sentences as the starting point of the sciences' (1959a: 201 [1932-3]). Yet, he points out, Carnap (1931-2a) speaks of a primitive protocol language - an experiential or phenomenalistic language which requires no verification. Furthermore, Carnap maintains that 'at the present stage' this language cannot be 'precisely characterised'. This might lead 'younger men' to search for a protocol language and so to 'metaphysical deviations'. Therefore, as a means of keeping 'waverers in line', physicalism needs to be maintained in its most radical version (Neurath, 1959a: 201-2 [1932-3]).

To this end, Neurath proceeds to give a physicalist interpretation of protocol sentences. In doing so he rejects Carnap's thesis that protocol sentences 'require no verification'. Just as every physicalistic sentence is subject to change, so even protocol sentences may be discarded as useless or false. 'The fate of being discarded may befall even a protocol sentence. No sentence enjoys the *noli me tangere* which Carnap ordains for protocol sentences' (Neurath, 1959a: 203 [1932-3]). To illustrate this point he asked the reader to imagine an ambidextrous person writing down two contradictory protocols at the same time.

In the immediately following article, in Carnap (1932-3), 'the protocol-sentence debate ceases to be one over the facts about the protocol language and its relation to the physicalist language - it comes to be a series of proposals for the construction of languages for science' (Richardson, 1996: 324).

> Neurath opposes certain features of the view about protocol sentences I advocated in my article on the physicalistic language. He wants to contrast it with another view according to which protocol sentences are in different form and are manipulated according to other procedures. My opinion here is that this is a question, not of two mutually inconsistent views, but rather of *two different methods for structuring the language of science both of which are possible and legitimate* (Carnap, 1987: 457, original emphasis).

Here Carnap 'severs any and all connection' between protocol sentences and experience. Furthermore, any statement could serve as a protocol. 'One can always go beyond any given statement; there are no absolute starting-points for the construction of science' (Carnap, 1932-3, quoted in Hanfling, 1981: 82). It would appear that, if protocols do not refer to statements that 'describe directly given experience' (Carnap), and if they are not distinguished by any special kind of certainty (Neurath), there would be no the point in speaking of protocol statements at all (Hanfling, 1981: 81). According to Ayer (1959: 231), there is 'no more justification for it than there would be for making a collection of all the propositions that could be correctly expressed in English by sentences beginning with the letter B, and choosing to call them Basic propositions'.

With the severance of 'any and all connection' with experience, the question arises of 'what becomes of verification and truth?' One result of the Carnap-Neurath protocol sentence debate is that it appears to diminish the importance, if not the need, for distinguishing between analytic and synthetic statements. More importantly it leads to a version of the coherence theory of truth, 'according to which the truth of a statement is a matter of its coherence with other statements' (Hanfling, 1981: 83). Hanfling points out in a footnote to this comment that it is ironic that the coherence theory had previously been represented by non-empiricist thinkers. Another outcome of the Carnap-Neurath debate is that:

> Once a scientist decides to work within a given framework, intersubjective agreement can be delivered, but at a considerable price: the logical principles which provide the essential structure for the framework, and indeed the framework itself, are chosen, Carnap tells us, on pragmatic grounds . . . (In doing so) the judgements which they make are relative to the framework itself: what is 'true' or 'false' depends upon internal consistency within each separate framework. This resonates with Carnap's view that the whole system of physics must be taken into account when judgements are made. In this he is following Pierre Duhem's (conventionalism) (Ray, 2000: 249).

All this was too much for Ayer and Schlick to accept. 'Ayer argued that each rival and incompatible system might include the proposition that it was the only acceptable system' (Ray, 2000: 249). Schlick forcefully spelt out the consequences of accepting a coherence theory of truth and re-affirmed his commitment to experience:

> Anyone who takes coherence seriously as the sole criterion of truth . . . must consider any fabricated tale to be no less true than a historical report . . . so long as the tale is well enough fashioned to harbour no contradictions anywhere. . . . I would not give up my own observation propositions under any circumstances . . . (they) would always be the final criterion (Schlick, 1932-3, quoted in Hanfling, 1981a: 178 and pp 188-9).

Schlick appeared to believe that knowledge of empirical statements must be wholly reducible to corresponding experiences. It is their peculiar closeness to experience that endows his observation sentences with absolute certainty. In looking for such statements his motive was to vindicate empiricism (Hanfling, 1981: 95).

For our purposes, this review of phenomenalism, physicalism and the protocol sentence debate brings out at least three points relevant to our concern with Hutchison (1938). First, while we have examined the arguments of either side in the debate, what is important for our purposes is the logical positivists commitment to the importance of sense experience of one kind or another. Secondly, the clarification of both the phenomenalist and physicalist positions will prove helpful when we turn to examine Hutchison's 1938 tract in Chapter Three. Thirdly, the disagreement between Schick (who held to a correspondence theory of truth) and Carnap and Neurath (who came to accept a coherence theory of truth) will be helpful when we turn to logical positivist views on the realist or non-realist (conventionalist, instrumentalist) status of scientific theories and laws (in section 1.3.5).

CHAPTER 2

ECONOMIC METHODOLOGY AND HUTCHISON'S INTERVENTION

Hutchison drew not only on the philosophy of science, but also on the methodological writings of economists stretching back, for example, to those of Ricardo and Malthus. In order to evaluate Hutchison (1938) it is therefore necessary not only to understand something of the philosophy of science, but also something of the history of economic methodology. As with the philosophy of science, we do not attempt a comprehensive overview of the topic, but instead seek only to draw out those aspects of economic methodology relevant to Hutchison (1938). In doing so, we use as our organizing framework a distinction that Hutchison himself has drawn between two different approaches to the methodology of economics.

While the distinction is implicit in Hutchison's methodological writings from 1938 onwards, he conveniently sets it out explicitly in an article exactly fifty years after his seminal work on methodology (Hutchison, 1998). Here he distinguishes between 'the empirically minimalist, ultra-deductivist' approach of Ricardo, Senior, J S Mill, and Cairnes on the one hand, and the more empirical, inductivist-leaning approach of Smith, Jevons, Marshall and Keynes on the other.

Using this as our organizing framework has both an advantage and disadvantage. The advantage is that it will help us to understand Hutchison's interpretation of economic methodology and thereby give us an insight into the nature of his 1938 work. The disadvantage is that we will get too caught up in viewing the methodology of economics from Hutchison's perspective and thereby lose the critical distance necessary to form a balanced appraisal of his contribution to economic methodology. We hope to counter this disadvantage by drawing on many authors besides Hutchison thereby seeking a wide range of protagonists in the methodological arena. More importantly, we hope to counter it by providing a relatively detailed examination of the methodological writings of three leading economists in the period leading up to 1938: Robbins, J M Keynes and Knight, of whom two had dramatically opposed

methodological views to those of Hutchison.

In order to accomplish these ends we divide this chapter into four different sections. In the first we attempt to draw out those aspects of the history of economic methodology relevant to Hutchison (1938). Here we cover the period from Adam Smith to Cairnes (1875), who is often viewed as the last of the classical economists, before turning to the neoclassical period. This conventionally starts with the marginal revolution of the 1870s, which may be viewed as partly prompting the *methodenstreit* between Menger (1883) and Schmoller (1883). We end with Marshall (1890) and Keynes (1891). In the remaining three sections we deal respectively with Robbins, J M Keynes and Knight.

2.1 Economic methodology from Smith to the 1930s

Smith, Ricardo, Malthus and Senior

Hutchison (1978: 4) points out that Smith saw himself as a philosopher 'in a highly comprehensive sense' so that the *Wealth of Nations* is properly interpreted as part of 'a much broader study of society and human progress, which involved psychology and ethics, law, and politics' (p 5).[1] While he kept abstract reasonings on a very tight rein, he employed a system by which he meant an abstract deductive model. 'He very much doubted that abstraction could provide either understanding of the real world or, by itself, safe guidance for the legislator or statesman' (Viner, 1968: 327).

Hutchison (1978: 10) describes Smith as 'a leader of the Scottish historical school'. Smith referred to his model, as 'the simple system of natural liberty', or what we might call the freely competitive, self-adjusting market model. It is a historically dynamic model, in that it is concerned not only with a static, or ideal, criterion, but with the economic forces which make for progress (p 11). '*The Wealth of Nations* was not founded on abstractions, nor on the particular abstraction of economic actions and processes from their historical interdependence and interpenetration with social, legal and political actions and processes' (p 23).

[1] In this chapter unsupported page references are to the work cited immediately prior to such a reference, except in the case of section 2.2.1 where they are all to Robbins (1935).

Blaug (1992: 52) follows Hutchison in accepting that Smith generally used the methods of the Scottish historical school in the *Theory of Moral Sentiments* and the *Wealth of Nations*, except for Books I and II of the latter in which he followed the method of comparative statics adopted by Ricardo. In neither of these works did he make his methodological principles explicit. But in Smith (1799), written around 1750, he described the Newtonian method as one in which we lay down 'certain principles, primary or proved, in the beginning, from whence we account for the several phenomena, connecting all together by the same chain' (cf Redman, 1997: 220-27).

> The richness of Smith's method lies not in the beauty of a precise mathematical theory or in a system like Newton's, but in its *wide social emphasis*: the special stress on the psychological underpinnings and sociological aspects of political economy, the striving for breadth of understanding and overall grasp of the economy rather than specialized knowledge, and the view of political economy as an interdisciplinary pursuit entrenched in the moral, political, historical, psychological, and philosophical. . . . there would probably be no better antidote to the narrowness of economics's current methods than a greater appreciation of and revival of the Scottish (historical) approach in association with today's analytical, theoretical approach (Redman, 1997: 257-8).

For Hutchison (1978: 26), more significant than Ricardo's theory of distribution was his methodological claim that problems in political economy are problems of 'determining laws'. Such laws are to be determined by recourse to 'the method of extreme abstraction (or 'strong cases')'. Hutchison points out that Walter Bagehot regarded Ricardo as the 'true founder of abstract political economy'.

After detailing the influence of James Mill on Ricardo, Hutchison argues that Ricardo was not interested in abstract model-building as an end in itself but rather in the *laissez-faire* policy conclusions to which it gave rise: 'Ricardo did not buy a seat in Parliament simply to expound blackboard exercises or to read out articles for *Econometrica*' (p 45). Instead he was 'overwhelmingly interested' in abolishing the Poor Laws and removing the Corn Laws. Yet, Hutchison points out, Ricardo's approach relies on the assumption of perfect knowledge (p 48). In a letter to Malthus, Ricardo acknowledged this assumption as fundamental to his method:

> The first point to be considered is, what is the interest of countries in the case supposed? The second what is their practice? Now it is obvious that I need not be greatly solicitous about this latter point; it can clearly demonstrate that the interest of the public is as I have stated it. It would be no answer to me to say that men were ignorant of the best and cheapest mode of conducting their business and paying their debts, because that is a question of fact and not of science, and might be urged against almost every proposition in Political Economy (Ricardo, quoted in Hutchison, 1978: 48).

Hutchison proceeds to point out that Ricardo's assumptions regarding the speed of adjustment and efficiency of markets followed logically from his assumption of perfect knowledge. He cites Ricardo's argument that allowing free imports of corn would result in labour immediately being applied to the production of more profitable commodities. Hutchison comments: 'this seems to suggest a failure to distinguish between a hyper-abstract model and the real world . . . to something having gone seriously astray with regard to . . . the relationship between analysis and policy' (p 49). Ricardo's approach was more *laissez-faire* than Smith (or any other classical economist) due to 'political preconceptions', but especially to his adoption of extreme versions of Smith's assumptions particularly with regard to (1) natural wages and (2) the perfect knowledge assumption (p 51).

According to Blaug (1992: 53), Ricardo was a 'convinced advocate of what we nowadays call "the hypothetico-deductive model of explanation," vigorously denying that facts can ever speak for themselves'. Indeed, Schumpeter has labeled this propensity of Ricardo to apply abstract models directly to the complexity of the real world the 'Ricardian Vice' (1954: 472).

Redman (1997: 260) refers to the 'maze of myths' surrounding both Malthus and Ricardo. Ricardo is known for his logical, deductive theory stripped of sociological and historical aspects and condemned by Schumpeter for the 'Ricardian Vice'. By contrast Malthus is damned for his 'scurrilous' population theory, regarded as illogical, fuzzy-minded, the loser in his debate with Ricardo, but with a strong empirical-historical bent. 'Like all stereotypes, there is an element of truth in both characterisations' (p 260). She proceeds to re-examine the literature pointing out the numerous re-evaluations of both economists. Nevertheless her conclusion follows much the same line as that of Hutchison (1978). Redman (1997: 316, 319) contrasts

Ricardo's 'focusing on logical consistency and quick results' with Malthus's 'empirical, historical approach' with its emphasis on the realism of assumptions. She points out that 'orthodox neoclassical economics (especially mathematical economics) has evolved more in accord with Ricardo's design, while American institutionalism (especially Mitchell's version) more closely conforms to Malthus's methodological conception' (p 316).

Three years after Ricardo's death and ten years before Mill's (1836) essay Nassau Senior (1827) had, according to Hutchison (1998: 46), set out the first explicit account 'of empirically minimalist ultra-deductivism' in an introductory lecture delivered at Oxford in 1826. According to Senior, the 'theoretic branch' of economics rests 'on a few very general propositions, which are the result of observation or consciousness, and which almost every man, as soon as he hears them, admits as familiar to his thoughts, or at least included in his previous knowledge' (Senior, 1827: 7, quoted by Hutchison, 1998: 46; Bowley, 1936: 285). The most important of these propositions states: 'That every man is desirous to obtain, with as little sacrifice as possible, as much as possible of the articles of wealth (Senior, 1827: 30, quoted by Hutchison, 1998: 46; Bowley, 1936: 288). Hutchison (1998: 47), after noting Senior's condemnation of the importance which many economists ascribed to facts, points out that, according to Bowley (1937: 64), the approach of Mises 'is the same approach as Senior's'.

John Stuart Mill and J E Cairnes

From Chapter One (section 1.3.4) we learnt that Mill (1843) has generally been interpreted in the philosophy of science literature as a leading example of what we termed 'inductivism-SM'. De Marchi (1998: 313, n 2) points out that Mill's *Essay*, first written in 1831, formed the basis of Book VI of his 1843 *System of Logic*. According to Blaug (1980: 69-73), Mill (1843), 'after devoting almost the whole of his book to defending inductive methods' accepts in the closing section in which he turns to social science that inductive methods are generally ineffectual because there are so many causes at work. This, and the difficulty of conducting controlled experiments, means that it 'cannot be a science of positive predictions, but only of tendencies' (Blaug, 1980: 66-9). Mill therefore advocates different methods for the

social sciences. Political economy is an 'essentially abstract science' based on 'assumed premises which are not pretended to be universally in accordance with fact', but which instead are simplifications necessary for the a priori method to be tractable. According to Redman (1997: 340), the a priori method is the 'deductive method' of the *System of Logic*:

> It reasons, and, as we contend, must necessarily reason, from assumptions, not from facts. It is built upon hypotheses, strictly analogous to those which, under the name of definitions, are the foundation of the other abstract sciences. . . . The conclusions of Political Economy, consequently, like those of geometry, are only true, as the common phrase is, in the abstract; that is, they are only true under certain suppositions, in which none but general causes - causes common to the whole class of cases under consideration - are taken into account (Mill, in Redman, 1997: 340).

This 'sudden support for deductive methods after hundreds of pages extolling inductive ones . . . is well calculated to leave the reader utterly confused about Mill's final views on the philosophy of the social sciences' (Blaug, 1980: 72). Hausman (1992) has attempted to resolve this apparent inconsistency between Mill's empiricist views and his support for the a priori method in economics. According to Hausman, Mill's solution was to maintain that the basic postulates of economics are well established by introspection or everyday experience:

> These well-supported premises state how specific causal factors operate. If the only causal factors influencing economic phenomena were those specified in these premises, then the predictions of economic theory would be correct. But economic phenomena depend on many causal factors that are left out of economic theories. Consequently, the implications are inexact (Hausman, 1992: 214).

Verification is undertaken not to confirm the truth of a theory, but rather to ascertain whether or not there are 'disturbing causes' which have not been taken into account which might prevent it from being applied to particular circumstances, that is, verification determines whether the theory is applicable to the case at hand (Redman, 1997: 341).

According to Redman (1997: 326), while Mill (1843) was concerned with both natural and social sciences, his real quest was to determine the extent to which the

methods of the natural sciences could be applied to the social-moral sciences. This he attempts in Book VI of his *System of Logic* from what seems to be a naturalist viewpoint:

> The backward state of the Moral Sciences can only be remedied by applying to them the methods of Physical Science, duly extended and generalised. . . . (Social science) is a deductive science; not indeed after the model of geometry, but after that of the more complex physical sciences (Mill, in Redman, 1997: 328, 335-6).

Blaug (1980: 73 ff) points out that, despite empirical evidence in the first half of the nineteenth century falsifying Ricardo's predictions concerning the price of corn, the share of rent, the level of wages and the rate of profit, Mill 'retained the Ricardian system without qualifications'. Blaug concludes his discussion by supporting de Marchi's (1970: 266) contention that 'J S Mill's methodological position was no different from Ricardo's: Mill only formally enunciated the "rules" which Ricardo implicitly adopted'.

According to Blaug (1980: 77-8), Cairnes (1875), like J S Mill, continued to uphold the Ricardian system and the view that political economy is a deductive science. Hutchison (1998: 51) points out that Cairnes developed one of the main doctrines of 'ultra-deductivism', later called by Wieser 'the psychological method'. This 'claimed to achieve greater certainty for propositions arrived at by introspection; which were held to provide a more secure foundation for economic theory than was available to the natural sciences' (ibid: 51):

> The economist starts with a knowledge of ultimate causes. He is already at the outset of his enterprise, in the position which the physicist only obtains after ages of laborious research (Cairnes, quoted in Blaug, 1980: 78).

Hutchison (1953: 18) notes that when Cairnes 'claims that "the economist starts with a knowledge of ultimate causes", he is describing very closely the rationalist Cartesian approach of the Physiocrats'. Be that as it may, there appears to be reasonable evidence for Hutchison's (1998: 44) classification of Ricardo, Senior, J S Mill and Cairnes as representatives of 'the empirically minimalist, ultra-deductivist' approach to economic methodology. Hutchison notes that it was this approach,

particularly in the extreme form given to it by Wieser and Mises, that was to influence Robbins (1932). Yet Robbins (1935: 82) was to describe it as 'orthodox'!

Jevons, the English historical school, and the methodenstreit

Jevons (1871), along with Menger (1871) and Walras (1874), is generally recognised as introducing the marginal revolution. These works signaled the end of classical political economy and the birth of neoclassical economics. Jevons attacked the central pillars of what he described as 'the Ricardo-Mill Economics': the theory of distribution (the wages-fund doctrine and the natural wage theory) as well as the theory of value - and the policies derived from them.

Apart from this revolution in economic theory, Hutchison (1953: 77) has drawn attention to 'a much more fundamental' attack on the abstractions of the Ricardo-Mill Method. While some aspects of this attack can be found in Jevons (1871), it is in Jevons (1874) that his philosophy of science is more fully set out. According to Schabas (1998: 260-1), Jevons was

> a strict empiricist in the tradition of Bacon, Locke and Hume: scientific enquiry begins and ends with observation, guided by analogies. . . . Like Mill, Jevons maintained that what is now termed the hypothetico-deductive method of Newtonian physics was best suited to economics. He recognised that, as a species of induction, however, this method did not preclude subsequent revisions of a given hypothesis.

Hutchison (1998: 53) has noted how these empiricist sentiments revealed themselves in Jevons (1871). For instance, Jevons claimed: 'The deductive science of economics must be verified and rendered useful by the purely empirical science of statistics' (1871: 90). While he advocated the introduction of mathematics to economics, he insisted that it was only by obtaining empirical evidence that one could 'enhance the veracity of a given claim' (Schabas, 1998: 261).

Partly influenced by Darwinism, the 1870s saw a continued rise in the methodological criticism of classical political economy as found in Jevons (1871). Moderate criticisms came from Toynbee's lectures at Oxford and Bagehot (1877). Stronger criticisms came from Ingram (1878) and Leslie (1879), representatives of the English

historical school (cf Koot, 1975).

> Political Economy is not a body of natural laws in the true sense, or of universal and immutable truths, but an assemblage of speculations and doctrines which are the result of a particular history. [Leslie went on to argue for] the deletion of the deductive method of Ricardo: that is to say, of deduction from unverified assumptions respecting 'natural values, natural wages, and natural profits'. But we are not against deduction in the sense of inference from true generalisations and principles, though we regard the urgent work of the present as induction, and view long trains of deduction with suspicion (Leslie, in Hutchison, 1953: 20).

According to Fusfeld (1987), the *methodenstreit* began with Menger (1883) 'which made the case for pure theory based on assumptions about behaviour and antecedent conditions. Schmoller (1883) responded with a strongly worded review that argued for principles of economics based on empirical historical data and the inductive method'. While each agreed that both theory and empirical studies were necessary, they disagreed on their place and importance. For Schmoller, rather than starting with assumptions, the proper method is to start with historical-empirical studies and to induct general principles from them. Hutchison (1953: 145 ff) points out that in answer to Schmoller's criticism of his assumptions as unrealistic, Menger compared his assumption of 'pure self-interest' to chemistry's assumptions of 'pure oxygen' and 'pure hydrogen'. To criticise these as unrealistic is to misunderstand the procedure of all sciences. Hutchison (1953: 147) responds by asking whether 'it does not make a fundamental difference that practically pure chemical substances can actually be isolated, tested, and observed in a laboratory in a way in which pure self-interest and omniscience cannot be extracted, observed, and measured separately from the rest of human qualities'.

Marshall and John Neville Keynes

In his inaugural lecture of 1885 Marshall looked towards 'the possibility of a vast improvement in the condition of the working classes' (Pigou, 1925: 155). In both men this 'social enthusiasm', to use Pigou's term, was balanced by a disciplined adherence to scientific method, for they earnestly believed in economics as a science, although not one comparable to physics (Hutchison, 1981: 50-1). Hutchison (1953: 64 ff) points to the influence of both mathematical (Cournot) and historical (Jones and

Roscher) economists on Marshall. On the mathematical side he 'translated Mill's version of Ricardo's or Smith's doctrines into mathematics' (Pigou, 1925: 417). However, according to Pigou,

> though a skilled mathematician, [Marshall] used mathematics sparingly. He saw that excessive reliance on this instrument might lead us astray in pursuit of intellectual toys, imaginary problems not conforming to the conditions of real life: and further, might distort our sense of proportion by causing us to neglect factors that could not easily be worked up in the mathematical machine (1925: 84).

Indeed, Marshall believed that if the results of mathematical economics could not be translated into English they should be burned (Coase, 1994: 174 [1975]). Marshall's wariness towards the use of mathematics in economics extended to the use of 'pure theory' in economics as demonstrated by his letter to Edgeworth in 1902:

> In that old phrase you would perhaps take the kernel to be the essential part: I take it to be a small part; and, when taken alone, more likely to be misapplied than in the case of other sciences. In my view 'Theory' is essential. No one gets any real grip of economic problems unless he will work at it. But I conceive no more calamitous notion than that abstract, or general, or 'theoretical' economics was economics 'proper'. It seems to me an essential but a very small part of economics proper (Pigou, 1925: 437).

Whitaker (1998: 282) points out that, while deductive argument was central for Marshall it 'was to be chastened and constrained from building airy castles on shaky empirical foundations by remaining always in close contact with the observable and verifiable'. Marshall repeatedly denied any role in economics for 'long trains of deductive reasoning' (1920: 773, 781).

In addition to keeping 'theory' on a tight leash Marshall, in his 1885 inaugural lecture, displays an instrumentalist-leaning conception of theory. For him, it supplies (no more than)

> a machinery to aid us in reasoning about those motives of human action which are measurable . . . But, while attributing this high and transcendent universality to the central scheme of economic reasoning, we may not assign any universality to economic dogmas. For that part of economic doctrine, which alone can claim universality, has no dogmas. It is not a body of concrete truth, but an engine for the discovery of concrete truth, similar to say, the theory of

mechanics (Pigou, 1925: 158-9).

Hammond (1991a: 97) interprets Marshall's 'concrete truth' as facts 'which are time and place specific' and therefore not universal. 'Theory alone does not yield concrete truths about the world; this is possible only with theory informed by and applied to perceptual facts' (1991a: 98).

Marshall's critical attitude to theory and his reservations about the role of mathematics in economics may be partly explained by the influence of the historical economists mentioned earlier: Jones, the pioneer in England of the historical method and Roscher of the German historical school. Hutchison (1953: 66) cites Marshall as stating that Jones was among his early readings and 'gave a direction to a good deal of my subsequent thinking'. Likewise the German historical school, notably Roscher 'attracted him' (Marshall, 1920: 767, 773, 777, and 783). However, he rejected extreme historicists warning that

> it must then always be remembered that though observation or history may tell us that one event happened at the same time as another, or after it, they cannot tell us whether the first was the cause of the second. That can be done only by reason acting on the facts (Marshall, 1920: 774).

In keeping with Marshall, Keynes (1891) likewise adopted a conciliatory approach to the criticism of the historical schools. Rather than rejecting any role for historical forces in political economy, he accepted that these played a part and so wisely granted the historical schools' main point. However, he adroitly relegated their part to the application, rather than the genesis, of political economy. He did this distinguishing three concepts of political economy: (1) a positive science, (2) a normative or regulative science and (3) 'an art, or system of rules for the attainment of a given end' (Keynes, quoted in Deane, 1978: 102-3). In doing so, he neatly deflected the criticisms of the historical schools - which were to be viewed as more concerned with (2) and (3) - than with the core science of political economy itself. In both Dillard's (1968: 377) and Blaug's (1980: 82) view, Keynes was biased towards the abstract-deductive view of economics. This judgement receives support from Hutchison (1981: 53), who cites Marshall's own views on the matter in a letter to Foxwell:

> Most of the suggestions which I made on the proofs of Keynes' *Scope and Method* were aimed at bringing it more into harmony with the views of Schmoller. Some were accepted. But as regards method I regard myself as midway between Keynes + Sidgwick + Cairnes and Schmoller + Ashley (Marshall, quoted in Coase, 1994: 170-1 [1975]).

In the first section of this chapter we have tried to draw out, from the long history of economic methodology before 1938, those aspects which have a bearing on Hutchison's methodological intervention. In doing so, we have used Hutchison's own distinction between two competing approaches to the methodology of economics: the empirical-inductivist approach and the 'empirically minimalist ultra-deductivist' tradition. We have seen that each approach well established and supported by leading economists of the past. To provide some critical distance from Hutchison's interpretation, we have quoted and cited the views of other authors. In the light of this, it seems reasonable to conclude that Hutchison's 1938 should be seen as arising out of an ongoing *methodenstreit* in economics, rather than merely the equivalent in economic terms of Ayer's (1936) propagation of the logical positivist ideas of the Vienna Circle.

2.2 Economic methodology in the 1930s

We now turn to a relatively detailed examination of the methodological writings of three prominent economists in the 1930s. In this way we hope to give some depth to the so far rather sweeping review of aspects of economic methodology important to Hutchison's 1938 intervention. In addition, the examinations of these three authors will provide a perspective on economic methodology quite removed from that of Hutchison and so allow for the development of some critical perspective on the proper place and nature of Hutchison (1938). It will also allow us to evaluate claims such as Caldwell's that the dominant methodology prior to Hutchison (1938) stressed 'subjectivism, methodological individualism, and the self-evident nature of the basic postulates of economic theory' (1982: 99).

Already in 1930, in his inaugural lecture, Robbins had mounted a defence of deductivist-oriented economics. He rejected the arguments of 'so-called

institutionalists' who sought to replace deductive methods with 'historical and statistical inductions':

> By the application of the methods of higher statistical analysis to dense masses of statistics, laws of 'economic behaviour' are to be discovered which will put Economic Science on a basis of equality with the Natural Sciences (Robbins, 1930: 20).

And before Hutchison (1935), Robbins's views had provoked an implicit reaction in Kaufmann (1933). Kaufmann (1933: 381) traces the 'sterility' of methodological discussion to 'a lack of precision in the formulation of the problems involved'. To this end he attempts clarification of the questions concerning the scope and method of economics (pp 383-4). He rejects the view that we can formulate laws in economics such as are found in physics (pp 386-7). This poses severe methodological problems in economics as compared to physics. In the light of these problems it is important to note that, in an empirical science such as economics, the correct method cannot be claimed to be 'a priori recognisable', or self-evident since 'as a matter of principle no apodictical assertions whatever can be made regarding matters of experience . . . the incorrectness [of which] must always be allowed to be possible' (p 388).

Bernadelli (1936), in a review of Kaufmann (1936), adopts the 'neo-Kantian' view associated with von Mises: economic laws are essentially a priori and do not depend on experience for their validity. In reply Kaufmann denies that synthetic propositions a priori exist in economics: 'The death blow to this doctrine was dealt by Poincaré's conventionalist explanation of geometry and finally by Einstein's general theory of relativity' (1937: 338). In his 'Live and dead issues in the methodology of economics', Robbins (1938: 347) refers to the logical status of the general assumptions of economics as one of the live issues. Here he notes that the protagonists in the debate over their status are Mises and Bernadelli on the one side and Kaufmann and Hutchison on the other (p 348). On the side of the empiricists, Leontief (1937) criticised the deductive 'implicit theorising', not only of the 'neo-Cambridge school' of Robinson, Kahn, Hicks and Keynes, but also of Robbins (1932).

2.2.1 Robbins[2]

According to Peston (1981: 183), Robbins's famous *Essay* 'has usually been interpreted as a priorist and anti-empirical'. Nevertheless Blaug (1980: 87) describes Robbins's *Essay* in broader terms as a restatement of 'the Senior-Mill-Cairnes position in modern language along with some elements from the Austrian tradition'. And Caldwell rejects the a priorist tag since this may be read to imply that Robbins subscribed to von Mises's view 'that the status of the fundamental axioms is that of synthetic statements that are a priori true'. Yet, he points out, 'nowhere in Robbins's essay can one find the term "a priori"' (Caldwell, 1982: 104-5).

A factor that may help towards understanding Robbins's *Essay* is the extent to which it arose as a reaction to opposing methodological positions. From around 1900, American institutionalism had been on the rise, nearly becoming dominant in the USA during the inter-war years. Robbins (1971: 149), referring to his *Essay*, explains:

> This part of the book, more than any other, reflects the circumstances in which it was written. It is a reaction - doubtless overdone - against the ridiculous claims of the institutionalists and the cruder econometricians and an attempt to persuade Beveridge and his like that their simplistic belief in 'letting facts speak for themselves' was all wrong.

While institutionalism may have featured in the background, the immediate methodological position to which Robbins reacted appears to have been the more direct and personal one of his fundamental disagreement with Beveridge on matters methodological. In his *Autobiography* Robbins makes it clear how strongly he opposed Beveridge's views on economic methodology describing them as 'primitive in the extreme':

> He thought that even astronomy proceeded simply by the 'unbiased collection of facts'. No disciple of Schmoller or the extreme American institutionalists could have exceeded his denunciations of abstract theory . . . the bias of his preconceptions in this respect made nearly all departmental developments on the theoretical side something of a struggle, and on one occasion at least involved a severe impoverishment of our strength (Robbins, 1971: 136-7).

[2] In this section all unsupported page references are to Robbins (1935).

The Essay

Three major themes may be distinguished in Robbins's work: (a) that of generalising the scope of pure economics to cover non-material, as well as material, welfare; (b) that of separating out a body of economic science, or pure economics 'from those discussions of economic issues that involved value judgements' and, (c) that of clarifying the nature of the generalisations which comprised the scientific part of economics (Corry, 1987: 207). While the first two themes concerning the nature of the subject matter of economics became widely accepted amongst orthodox economists, the third has remained controversial. It was directed at supporting the view that the scientific part of economics is sound:

> The efforts of economists during the last hundred and fifty years have resulted in the establishment of a body of generalisations whose accuracy and importance are open to question only by the ignorant or the perverse (Robbins, 1935: 1).

(a) The scope and definition of economics

Robbins (1935: 64) points out that, traditionally, economists have viewed economics as being concerned with the causes of material welfare and proceeds to discuss the implications of defining economics in this way. He shows that it is not reasonable to limit the concerns of the subject to those of material welfare alone. He therefore proposes a 'formula to describe the general subject-matter of Economics' (p 3). This is 'a definition which is echoed in the first chapter of every textbook on price theory' (Blaug, 1980: 87): 'Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses' (p 16).

Robbins goes on to point out that the traditional materialist definitions of economics may be called 'classificatory' conceptions since they delineate those kinds of human behaviour that are concerned with material welfare. However, his definition is an 'analytical' conception: it does not attempt to isolate certain kinds of behaviour. Instead it looks at a particular aspect of human behaviour: namely, that which results from the influence of scarcity.

> It follows from this, therefore, that in so far as it presents this aspect, any kind of human behaviour falls within the scope of economic generalisations. We do not say that the production of potatoes is economic activity and the production of philosophy is not. We say rather that, in so far as either kind of activity involves the relinquishment of other desired alternatives, it has its economic aspect (p 17).

Traditionally, from Adam Smith's great work onwards, economists have professed to deal with the causes of wealth, or total social product, and its distribution. Yet, Robbins argues, the lasting achievements of economics have not sprung from such wide-ranging concerns since these involve notions that are too vague for the requirements of science. The development of laws has related to precise concepts as found in the theories of equilibrium, comparative statics and dynamic change which are not enquiries into the causes, or distribution of wealth.

> Instead of regarding the economic system as a gigantic machine for turning out an aggregate product and proceeding to enquire what causes make this product greater or less, and in what proportions this product is divided, we regard it as a series of interdependent but conceptually discrete relationships between men and economic goods; and we ask under what conditions these relationships are constant and what are the effects of changes in either the ends or the means between which they mediate and how such changes may be expected to take place through time (p 68).

While Robbins's definition appeared to widen the scope of the subject, in practice it excluded whole fields of economic enquiry concerned with broader macroeconomic, developmental and social issues (Deane, 1978: 147).

(b) The separation of value judgements from pure economics

Another major theme is Robbins's attempt to 'separate economics from ethics' (O'Brien, 1998: 424). He put forward the view 'that economic science could be clearly demarcated from those discussions of economic issues that involved value judgements' (Corry, 1987: 207). For Robbins, there are two important reasons for wanting to separate value judgements from economics. The first reason relates to Weber's notion of *wertfrei* and his views on objectivity in the social sciences. Robbins makes it clear that the practical significance of economics depends upon

being able to distinguish the propositions of pure science, which are strictly neutral between ends, from normative propositions which embrace value judgements (p 152). Economics, as a science, cannot help us with the problem of choosing between different ends, all of which may be desirable. This is a problem for the field of ethics. Instead, economics as a science can help us with ethical problems by clarifying the implications of the different ends that we are thinking of choosing. In other words it allows us, when choosing, to do so rationally, ie with a knowledge of what it is we are choosing as well as to choose ends which are consistent in the sense that they are mutually achievable.

The second reason for wanting to separate out value judgements from economics was Robbins's desire to debunk the 'scientific pretensions of Pigovian welfare economics (O' Brien, 1998: 424). 'By value judgements Robbins had especially in mind those evaluative statements of the form "better or worse" where inter-personal comparisons of utility were involved' (Corry, 1987: 207). Robbins argued that individual welfare was a fundamentally subjective matter which could not be objectively measured. This viewpoint had its origins in Austrian writings, in particular von Mises, with the idea that only ordinal, and not cardinal, comparative rankings were possible (O'Brien, 1998: 425).

(c) The foundations of economic generalisations

We look first at Robbins's views on the nature of economic generalisations, ie, economic laws or theories (his Chapter IV). O'Brien (1988: 34) refers to Robbins's position as 'essentially English' by which he means that it is characterised by 'elements of imprecision and compromise about the extent to which the basic premises of economics are hypothetical and a priori'. We then turn to his views on their relationship to 'reality' or the 'procedures for checking the validity of economic theory' (Corry, 1987: 207) (his Chapter V).

Robbins sets out the purpose of his Chapter IV: it is to discuss 'the nature and derivation of economic laws' (p 72). He goes on to make it clear that he is not concerned with the correct methods for discovering 'how economics should be pursued' – this, he regards, as settled. He is, rather, interested in ascertaining the

significance of its results. Taking the propositions of economics as established (the best established are those of the theory of value), the procedure is to 'inquire on what their validity depends' (p 73).

He answers this question by ruling out various responses. The foundations of economic generalisations cannot rest on an appeal to historical induction (p 74). Nor can they rest on the results of controlled experiment which cannot justify the wide-ranging propositions of the theory of value (p 75). Robbins then asks the question 'on what, then, does our belief in the general propositions of economics depend?' After discussing various general propositions, he concludes:

> The propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates. And the chief of these postulates are all assumptions involving in some way simple and indisputable facts of experience relating to the way in which the scarcity of goods which is the subject-matter of our science actually shows itself in the world of reality. . . . These are not postulates the existence of whose counterpart in reality admits of extensive dispute once their nature is fully realised. We do not need controlled experiments to establish their validity: they are so much the stuff of our everyday experience that they have only to be stated to be recognised as obvious (pp 78-9).

Robbins next points out that these self-evident facts of experience need to be combined with 'subsidiary postulates' (p 79). These subsidiary postulates are 'historico-relative' (p 80). Thus we must take care to 'be sure of the facts' before applying our general theory to a particular situation (p 81). But, although the subsidiary postulates of economics are historico-relative, this is not the case with the fundamental assumptions of economics. They have 'universal applicability'. Indeed, they carry greater certainty even than the generalisations of the natural sciences:

> In Economics, as we have seen, the ultimate constituents of our fundamental generalisations are known to us by immediate acquaintance. In the natural sciences they are known only inferentially. There is much less reason to doubt the counterpart in reality of the assumption of individual preferences than that of the assumption of the electron (p 105).

Robbins next deals with the behaviourists' contention that proper scientific method should follow the procedure of the physical sciences. Drawing on these ideas, economists such as Cassel and Pareto demand 'that we should leave out of account

anything which is incapable of direct observation . . . Our theoretical constructions must assume observable data' (p 87). Robbins responds by pointing out that were economists to do so, we should find it impossible to provide explanations of behaviour since these are ultimately based on individual's subjective valuations which may be understood, but not observed. These psychological elements, which form part of any adequate explanation, are one of the essential differences between the social and the physical sciences and explain why the social sciences 'can never be completely assimilated to the procedure of the physical sciences' (p 89).

Robbins goes on to argue, somewhat more controversially, that the assumption of rational behaviour does not even number among the fundamental assumptions of economics. This contrasts with today's view that ranks it amongst the most important assumptions of economics (Caldwell, 1982: 101). Instead, Robbins contends, the assumption of perfect rationality (as with the assumption of perfect foresight), properly interpreted, should be seen as no more than an

> expository device – a first approximation used very cautiously at one stage in the development of arguments which, in their full development, neither employ any such assumption nor demand it in any way for a justification of their procedure (p 97).

We now turn to look at Robbins's views on the relationship of economic theory to reality or the 'procedures for checking the validity of economic theory' – the topic of his Chapter V. Given Robbins's satisfaction with the orthodox body of economic thought, 'the question arises whether any of this body of theory requires confrontation with data. It seems quite clear that Robbins rejected both the attempts to arrive inductively at empirical regularities and also the direct testing of economic theories . . . for the simple reason that economic theories were necessarily true if they were arrived at by reasoning correctly from plausible premises' (O'Brien, 1998: 425).

Robbins begins by contending that economic generalisations, like scientific generalisations, refer to that which exists or may exist. 'If the premises relate to reality the deductions from them must have a similar point of reference' (p 104). He therefore rejects the view that economics consists of merely formal propositions such as those of logic and mathematics. Nevertheless, the propositions that have been

established in economics 'are very general in character' (p 106). While it would be useful to be able to attach numerical values to scales of individual valuation or to establish quantitative laws of demand and supply, 'a moment's reflection should make it plain that we are here entering upon a field of investigation where there *is no reason to suppose that uniformities are to be discovered* (p 107, original emphasis).

In a famous passage referring to a mythical economist called Blank attempting to estimate the elasticity of demand for herrings, Robbins proceeds to explain why we cannot hope to arrive inductively at economic generalisations:

> Is it possible reasonably to suppose that coefficients derived from the observation of a particular herring market at a particular time and place have any *permanent* significance – save as Economic History? . . . However accurately they describe the past, there is no presumption that they must continue to describe the future. Things have just happened to be so in the past (pp 108-9, original emphasis).

Here Robbins is voicing Hume's the problem of induction. For Robbins these problems were such that theories could not be specified in quantitative terms: 'What the economist possesses is merely a qualitative calculus, which of course may or may not apply in a particular case' (Blaug, 1980: 88).

Robbins then goes on to argue that if we cannot specify such elementary concepts as demand and supply functions in quantitative terms, then there is even less reason to hope that we can arrive at 'concrete' laws for more complex economic phenomena. Yet there has been a great multiplication of such attempts under the name of 'quantitative economics'. These are doomed to failure for

> The theory of probability on which modern mathematical statistics is based affords no justification for averaging where conditions are obviously not such as to warrant the belief that homogeneous causes of different kinds are operating. Yet this is the normal procedure of much of the work of this kind. The correlation of trends subject to influences of the most diverse character is scrutinised for 'quantitative laws'. Averages are taken of phenomena occurring under the most heterogeneous circumstances of time and space, and the result is expected to have significance (p 112).

According to Robbins, the recent multiplication of such attempts is nothing new: it is

no more than the continuation of a century-old revolt against the 'formal abstraction' of Ricardian economics. The arguments of the 'quantitative economists' echo those of earlier advocates of 'inductive methods', namely, the Historical School and Institutionalists. Since this movement has 'continually invoked a pragmatic logic, [it] may well be judged by a pragmatic test' (p 113): Despite this long time span, and despite them becoming 'a highly respectable band of expert authorities . . . and the directing functionaries of expensive research institutes . . . not one single "law" deserving the name, not one quantitative generalisation of permanent validity has emerged from their efforts' (p 114). Instead the substantial uniformities that have been discovered have flowed from orthodox theoretical analysis (p 115).

Our discussion of Robbins's methodology has dealt with Caldwell's misgivings about an over-readiness to label Robbins's *Essay* as a priorist and anti-empirical - as no more than a restatement of the Austrian economics. Robbins does indeed use doctrines beloved by Austrians such as the *verstehen* doctrine (Blaug, 1980: 88). For instance, he argues that we have better grounds for the generalisations of economics than the natural sciences because the assumptions are known to us by immediate acquaintance rather than merely inferentially (p 105). Yet, on this issue, Robbins refers the reader to Cairnes (1875: 81-90) rather than to an Austrian economist. Moreover, unlike von Mises, Robbins did not view the basic postulates as praxeological. Instead they involved 'in some way simple and indisputable facts of experience' (p 78). Furthermore, he embraces the need to separate out positive from normative statements so as to establish a value-free, objective science. And, as we have seen, he acknowledges that, at least part of his book, is a reaction to the institutionalists and Beveridge. For these reasons, it appears that Robbins's *Essay* is more accurately described as emphasising apriorism, but remaining within the Senior-Mill-Cairnes position.

2.2.2 J M Keynes

We turn next to discuss the views of Keynes. This might seem to require some justification. For one thing, Keynes's contribution to economic methodology in the 1930s is not nearly as voluminous as either that of Knight or Robbins. For another, his writings featured less significantly than either Knight's or Robbins's in Hutchison

(1938). Nevertheless, the overwhelming importance of Keynes as the economist *par excellence* of the twentieth century means that these factors pale into insignificance. Whereas prior to around 1980 Keynes's *Treatise on Probability* (1921) had been viewed as being limited to a technical account of probability, modern interpretations have regarded Keynes as having expounded fundamental philosophical views in his *Treatise*. Following Cottrell (1998) we distinguish, for purposes of exposition, between Keynes's philosophy and his methodology.

Keynes's philosophical position

Although the post-circa 1980 interpretation traces Keynes's philosophical views to his *Treatise*, this is as far as agreement goes. Much of this literature has been concerned with whether or not there was a fundamental continuity or discontinuity in the development of Keynes's philosophical thinking.

The discontinuity interpretation (Bateman, 1987; Davis, 1994) contends that Keynes abandoned much of his early philosophy. Just as the *General Theory* is traditionally taken to reflect a revolution in Keynes's economic thinking so, it is argued, Keynes's later philosophical views expressed within the *General Theory* are taken to be fundamentally different from his earlier views. According to Bateman (1991a: 105), these renunciations indicate that Keynes turned towards empiricism.

The continuity thesis contends that Keynes's renunciations of his early philosophical views should not be taken at face value. A less superficial understanding of Keynes, it is argued, reveals a continuity between his early and late views. While the idea of continuity derives from discerning an essentially unchanged philosophy, there are widely differing interpretations of the nature of this philosophy. Among these interpretations are those of Carabelli (1988) and O'Donnell (1989). O'Donnell (1989) argues that Keynes's philosophy is distinctly rationalist and that it must be viewed as a determining force behind his economics. For Carabelli (1988), Keynes's *Treatise* contains a methodology 'which contrasted with the nature and developments of Russell's logical positivism' (p 7) so that it should be placed in a 'third stream' between rationalism and empiricism (p 246).

The fact that some discern rationalist, while others see empiricist, elements in his position should not be a cause for concern. Indeed, it can be argued, that the main importance of the modern controversy is that it points to the richness of his philosophy in which rationalist elements co-existed with empiricist elements.

Keynes's views on probability and induction

We now turn to focus on Keynes's *Treatise*, in particular his theory of probability, and its relation to induction. Keynes's major development of the logical theory of probability was put forward in opposition to the frequency theory which was dominant then, as now, within the 'scientific tradition'. According to this theory, probability is defined as 'the limit of a relative frequency of a subset of events to an infinite series of realizations of the relevant event population' (Mirowski, 1998: 391). 'In this sense one could speak of the probability of drawing a king from a deck of cards, and this probability would refer to the number of times that this event would occur in repeated trials' (Bateman, 1990: 361).

The frequency theory of probability clearly has empiricist leanings: probability, in this approach, is a property of physical things or events. According to Black (1967: 475) the key idea in this theory is to 'deny the existence of any logical gap between frequency and reasons . . . This idea has great intrinsic appeal to empiricists who, hoping to interpret basic probability statements as contingent, have nowhere better to look than in the direction of observed frequencies'. By contrast, probability in Keynes's theory, instead of being defined as a property of physical things or events, is regarded as a logical relationship between propositions, (Keynes, 1921: 5).

> The theory of probability is logical, therefore, because it is concerned with the degree of belief which it is *rational* to entertain in given conditions, and not merely with the actual beliefs of particular individuals, which may or may not be rational (p 4).

Keynes introduces the topic of induction in the following way:

> I have described probability as comprising that part of logic which deals with arguments which are rational but not conclusive. By far the most important types of such arguments are those which are based on the methods of induction

and analogy. Almost all empirical science rests on these (p 241).

In the *Treatise* Keynes devotes two of his five sections to the topic of induction. He distinguishes between two types of inductive arguments: universal and statistical induction. While universal induction deals with arguments of the form 'all swans are white', statistical induction deals with arguments of the form 'most swans are white'. In examining the nature of statistical induction, Keynes (1921, part V) reiterated his main point that probabilities are degrees of belief rather than relative frequencies. Keynes criticised the early writers on statistical induction for assuming, rather than showing by induction, that one can validly infer probability from relative frequencies (p 400). In other words, they used only pure induction and ignored the role of analogy:

> To argue, without analysis of the instances, from the mere fact that a given event has a frequency of 10 percent in the thousand instances under observation, or even in a million instances, that its probability is 1/10 for the next instance, or that it is likely to have a frequency near to 1/10 in a further set of observations . . . is hardly an argument at all (pp 445-6).

Keynes distinguishes between two parts of an inductive argument: 'pure induction' and analogy. Pure induction refers to the number of observations we make, while analogy refers to the likeness that the various instances bear to one another. According to Carabelli (1988: 63), Keynes associated the 'traditional' view of induction with 'pure induction' which saw induction as being limited to 'number', ie quantitative and empirical matters. For Keynes, this was to omit the key role played by 'likeness' or analogy, ie qualitative and non-empirical matters. Positive analogy refers to the characteristics of observed instances being similar, while negative analogy refers to those that are different.

The importance of pure induction is strictly secondary to that of analogy. Such importance as it has arises only to the extent that further instances introduce new differences and so increase the negative analogy. 'For this reason, and for this reason only, new instances are valuable (Keynes, 1921: 259). A further instance which provides only a positive analogy will not raise the probability of the argument. Instead the probability is raised by increasing the negative analogy ie by diminishing 'the characteristics common to all the examined instances and yet not taken account

of by our generalisation' (p 260).

Keynes's emphasis on analogy rather than number in the problem of induction provides us with an instance of the extent to which Keynes regarded non-empirical, rather than empirical, concerns as fundamental to inductive arguments. As such it supports our earlier conclusion that points to the importance of both rationalist and empiricist elements in Keynes's philosophy.

Keynes's methodological views

In marked contrast to questions surrounding the degree of continuity in Keynes's philosophical views over time, it has been argued that there is a 'substantial' continuity in his methodological views. From the *Treatise* to the *General Theory* and after, Keynes repeatedly criticises 'what he regards as inappropriate attempts to quantify or formalize domains of study whose inherent nature makes such procedures problematic' (Cottrell, 1998: 263). For example, with regard to Keynes's misgivings about quantification, Carabelli (1992) has drawn attention to the fact that Keynes aggregated only quantities of employment and money, not 'real output', which he viewed as unquantifiable in principle (Keynes, 1936, ch 4). Keynes's misgivings about formalisation covered both statistics and mathematics. 'In his own economic work, he cites tables of data for various purposes but never does any formal statistics' (Cottrell, 1998: 264). And the mathematics in the *General Theory* is strictly limited (chs 20 and 21). Unlike his followers, Keynes did not attempt to describe the economic system in terms of a set of simultaneous equations:

> It is a great fault of symbolic pseudo-mathematical methods of formalising a system of economic analysis . . . that they expressly assume strict independence between factors involved and lose all their cogency and authority if this hypothesis is disallowed . . . Too large a proportion of recent 'mathematical' economics are merely concoctions, as imprecise as the initial assumptions they rest on, which allow the author to lose sight of the complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols (Keynes, 1936: 297-8).

Many of his reservations about formalisation of mathematical and statistical methods in economics are to be found in his criticisms of Harrod and Tinbergen (Keynes,

1973: 285-320). In his venture into methodology, Harrod (1938) somewhat crudely emphasises the role of observation and it is this emphasis which provokes a response from Keynes. For example, Harrod refers to the 'heroic attempts' made by Schultz to obtain quantitative laws of demand and argues that progress depends on 'empirical observations as make it possible to fill in the blank-forms of equations with quantitative data' (1938: 400-1). Later Harrod argues:

> The generalisation [falling prices associated with rising output] is a direct result of observation, an excellent example of the facts speaking for themselves. And if theoretical explanations have subsequently been woven round it, this must not blind us to the true source of our knowledge. If rather crude observational data can yield appetising morsels of this sort, may we not legitimately hope that when subjected to refined statistical treatment they will yield more fruit in plenty? (p 408).

While Keynes (1973) praises Harrod's address as 'the best for many years', he states that he has reservations about certain sections:

> It seems to me that economics is a branch of logic, a way of thinking; and that you do not repel sufficiently attempts a la Schultz to turn it into a pseudo-natural-science . . . *Progress* in economics consists almost entirely in a progressive improvement in the choice of models . . .
>
> But it is of the essence of a model that one does *not* fill in real values for the variable functions. To do so would make it useless as a model. For as soon as this is done, the model loses its generality and its value as a mode of thought. That is why Clapham with his empty boxes was barking up the wrong tree and why Schultz's results, if he ever gets any, are not very interesting (for we know beforehand that they will not be applicable to future cases). The object of statistical study is not so much to fill in missing variables with a view to prediction, as to test the relevance and validity of the model (pp 295-6, original emphasis).

While Keynes's scepticism regarding formalisation of mathematical and statistical methods in economics is clear, the deep-rootedness of his misgivings becomes evident in his criticisms of Tinbergen (1939). Keynes reviewed Tinbergen's book in the September 1939 *Economic Journal*. Tinbergen replied in the March 1940 *Economic Journal* and his reply was followed in the same issue with a comment by Keynes. Earlier, Keynes had drafted numerous comments on Tinbergen's work prior to its publication (Keynes, 1973: 285-320).

In analysing Keynes's criticism of Tinbergen we will follow Carabelli (1988, ch 10) in distinguishing four main features of the debate: a) the nature of economic theory, b) inductive inference in economics, c) characteristics of economic material and, d) the validity of economic theory.

a) Keynes (1973: 302) had no quarrel with Tinbergen in so far as his statistical investigations were concerned only with the descriptive function of verifying economic theory. However, Tinbergen was not content to confine statistics to verifying theory: statistics, via correlation analysis, should also deal with the problem of measurement, ie identifying not merely the causes that are operative, but measuring the individual strength of each of these causes. Keynes, as we have noted before (p 299), stressed that economic theory was essentially qualitative. Moreover, qualitative theory logically preceded measurement (pp 307-8). Keynes also clashed with Tinbergen's view of causation. Tinbergen's view was attuned to the Humean notion that restricted cause to no more than constant conjunction of events. Therefore, for Tinbergen, correlation analysis, insofar as it sought out empirical regularities, isolated causal relations. Economic theory was thus seen as grounded on empirical, quantitatively measurable relations. Keynes's view of causation, as propounded in his *Treatise*, was put forward together with his notion of induction as a logical, rather than an, empirical concept. Assuming he carried these views over to economics 'one can also say that Keynes's concept of economic explanation was far from that of causation implied in positive economics' (Carabelli, 1988: 180).

b) The next point about inductive inference is one that has been made by both Bateman (1990) and Lawson (1985). Keynes criticised Tinbergen for not having the grounds for making inductive inference:

> I have not noticed any passage in which Professor Tinbergen himself makes any inductive claims whatever. He appears to be solely concerned with statistical description. Yet the ultimate purpose which Mr Loveday outlines in the preface is surely an inductive one (Keynes, 1973: 315).

For Keynes, correlation analysis was not sufficient ground to enable the statistician to pass from statistical description to statistical inference. To take the step from describing the past to predicting the future one needed proper inductive grounds. For

Keynes, these were given by the logical theory of induction propounded in his *Treatise*. His main point was that Tinbergen's statistical inferences had been made in the absence of any inductive argument:

> I pass in conclusion to a different department of the argument. How far are these curves and equations meant to be no more than a piece of historical curve-fitting and description, and how far do they make inductive claims with reference to the future as well as the past? (Keynes, 1973: 315).

Lawson (1985: 129) points out that Keynes is not saying that empirical evidence is not relevant to economic analysis, nor does he reject the usefulness of inductive argument. His point is, rather, that while methods such as correlation analysis which depend on induction may be valid in the natural sciences, they may not be valid in the social sciences:

> Unlike the typical natural science, the material to which [economics] is applied is, in too many respects, not homogeneous through time (Keynes, 1973: 296).
>
> In chemistry and physics and other natural sciences the object of experiment is to fill in the actual values of the various quantities and factors appearing in an equation or a formula; and the work when done is once and for all. In economics that is not the case, and to convert a model into a quantitative formula is to destroy its usefulness as an instrument of thought (p 299).
>
> The pseudo-analogy with the physical sciences leads directly counter to the habit of mind which is most important for an economist proper to acquire . . . I also want to emphasise strongly the point about economics being a moral science. I mentioned before that it deals with introspection and with values. I might have added that it deals with motives, expectations, psychological uncertainties (p 300).

c) Turning to the characteristics of the economic material, Keynes 'makes it clear that he did not believe that the material of economics exhibits enough stability and homogeneity over time to license the application of formal statistical methods' (Cottrell, 1998: 264). Keynes argued that, in order for Tinbergen's method of correlation analysis to be validly applied, the economic material had to be homogeneous through time, numerically measurable and completely comprehensive. Keynes found Tinbergen's attempt to take account of these factors 'grievously disappointing'. His contention was forthright: 'In fact we know that every one of these conditions is far from being satisfied by the economic material under

investigation' (Keynes, 1973: 285-6). Although Tinbergen had recognised that non-measurable factors may play an important role, Keynes queried how, in terms of his analysis, he could take into account any possible influence they might have.

d) Carabelli's (1988: 191) last heading concerns the validity of economic theory. According to Tinbergen, although no statistical test could prove a theory to be correct, it could prove it to be incorrect. Keynes disagreed with Tinbergen's view that a statistical test could prove a theory to be incorrect. His disagreement arises from his notion 'that inductive inference was valid on logical rather than empirical grounds' and his 'view that theory always came first, before observation'. These two factors are in addition to his criticisms of Tinbergen discussed under a) to d) above. Keynes summed up his criticism of Tinbergen with the following rhetorical query: 'If the method cannot prove or disprove a qualitative theory, and if it cannot give a quantitative guide to the future, is it worthwhile? For, assuredly, it is not a very lucid way of describing the past' (Keynes, 1973: 308).

In this section we have distinguished, for purposes of explanation, between Keynes's philosophy and methodology. Concerning his philosophy, we saw how Keynes set forth his logical (cognitive) conception of probability and induction in distinct opposition to the then prevailing frequentist (physical) theory. The interpretation of his Treatise has led to a major debate. We concluded earlier that claims that both empiricist and rationalist elements are to be found in Keynes's philosophy reflects the richness of, rather than any latent inconsistency within, his philosophy. Concerning his methodology we have learnt that he was consistently sceptical of quantification and formalisation in both mathematics and statistics in economics. He accepted that qualitative, non-quantifiable factors play an essential role in economic explanation and that a difference exists between the methodology relevant for economics and that for the natural sciences.

2.2.3 Knight

To get to grips with Knight's views on the methodology of economics is no easy task. This is due partly to the fact that, having come to economics from philosophy, his philosophical position is unusually well-informed. Accounts of his views by less

philosophically sophisticated economists have therefore often led to confused and conflicting claims resulting in a fragmented portrait of him as an 'elusive mixture of scepticism and mysticism', a rationalist, an extreme a priorist, 'an institutionalist in a broad sense' and a maverick vacillating between orthodoxy and institutionalism (Gonce, 1972: 548). More recently, various attempts have deliberately aimed to counter this fragmented image.

Gonce seeks to do so by arguing that 'a tripartite system' - consisting of his own philosophy and ethics; his methodological principles; and his answers to the challenges of behaviourists, institutionalist and positivists - underlies his views. For Hammond, Knight's tendency to make claims that contradict each other, eg, that economics is a science and that it is not, may be resolved by interpreting him as presenting a highly personal and original 'anti-positivism' (1991: 371). He takes issue with Hirsch and Hirsch (1980), who interpret Knight as viewing economics as differing fundamentally from the natural sciences. He argues that Knight was not so much opposed to interpreting economics as a science similar to the natural sciences, as he was to the particular conception of science put forward by the positivists. Hands (1997: 196) accepts Hammond's view of Knight's position as anti-positivistic, but contends that he is also 'relatively hermeneutic and anti-scientistic about social science' (cf Latsis, 1972: 235, n 5). He argues that, for Knight, positivism leads to behaviourism where agents are reduced to physical or biological processes rather than having the motives and free will of human beings. As such, positivist human science is impossible.

In this section we seek to highlight various aspects of Knight's methodological interventions - those relevant to providing a better understanding of Hutchison (1938), the topic of Chapter Three. These aspects will also prove useful in Chapter Four when we consider Knight's (1940) famously fiery review of Hutchison (1938). As a unifying theme in this account we emphasise the extent to which Knight's interest in economics arises out of his ethics (Gonce, 1972). In the field of ethics Knight was deeply concerned with the problem of 'social control'. This has been described as the problem of 'understanding how by consensus based upon rational discussion we can fashion liberal society in which individual freedom is preserved and a satisfactory economic performance achieved' (Stigler, 1987: 58). According to

Gonce (1972: 550), Knight turns to economics for help in solving the problem of social control. But for economics to help, Knight argues we need to be clear about its nature and its limitations. He sets out upon such clarification in his *Risk, Uncertainty and Profit* (1921). For Knight, economics is mainly a pure science. The application of this pure science is limited and fraught with difficulties.

Economics as a science

According to Knight (1921), economics is the only social science with pretensions to being regarded as an exact science. In so far as it is an exact science (of which the exemplar is physics), this exactness comes at the cost of unreality. The method of exact science is the method of analysis and abstraction: these two terms are 'virtually synonyms' (p 3). This method allows us to generate laws which hold approximately, that is, they tell us what tends to hold true in a situation where the (less important) variables not taken into account by these laws are entirely absent. Such laws enable us to approach 'practical problems intelligently' (p 5).

Unfortunately economists are divided over 'the meaning and use of theoretical methods' (p 5). On the one hand, we find the extreme views of mathematical economists and pure theorists who insist on limiting scientific economics to 'a closed system of deductions from a very small number of premises assumed as universal laws' (p 6). On the other hand, we find those who insist on 'a purely objective, descriptive science'. Knight suggests a 'middle way' (p 6). Just as pure theory is fundamental to physics, so it is vital to economics. However, it should be viewed as no more than a necessary first step of the procedures in economics and its 'conclusions must be constantly checked with facts by observation and premises revised accordingly' (p 7). 'Where the data are too complex to handle in this way induction must be applied and empirical laws formulated' (p 6). However, for these to be significant, they must 'be shown to follow from the general principles of the science . . . we see that there is little divergence left between the two methods (p 7).

> Our knowledge of ourselves is based on introspective observation, but is so direct that is may be called intuitive. Its extension to our fellow human beings is also based upon the interpretation of the communicative signs of speech, gesture, facial expression, etc, far more than upon direct observation of behavior

> . . . Many of the fundamental laws of economics are therefore properly 'intuitive' to begin with, though of course always subject to correction by induction in the ordinary sense of observation and statistical treatment of data (p 7, n 1).

Knight immediately qualifies the above stating that it should not be interpreted as being concerned with 'philosophical problems'. He is, 'like Mill, an empiricist, holding that all general truths or axioms are ultimately inductions from experience' (p 7, n 1). While 'the scientific method of reasoning from simplified premises' can be applied to economics, it has been applied too uncritically: its proponents have failed to be clear about the unreal assumptions involved or that their conclusions refer only to tendencies. The extent to which these assumptions abstract from the complexities of life should 'be made as conspicuous and as familiar as has been done in mechanics' (p 9).

> The present essay is an attempt in the direction indicated above. We shall endeavor to search out and placard the unrealities of the postulates of theoretical economics, not for the purpose of discrediting the doctrine, but with a view to making clear its theoretical limitations (p 11).

So far we have emphasised the extent to which Knight is concerned with the problem of social control. However, economics is to play only a small part in 'this vast social undertaking'. 'The social problem is not one of means and end. It is a problem of values' (Emmett, 1998). His economics, to an extent, may be viewed as an attempt to help with the problem of social control. But, while he looks to economics, he is at pains to stress the limitations of the extent to which economics as a science can help. Before turning to these limitations (which arise out of problems in applying the scientific method to economics), it will prove fruitful to examine in more detail the limitations set up by the interaction between ethics and economics since these provide the background against which problems with the scientific method stand out more clearly.

Economics and ethics

In 'Ethics and the economic interpretation' Knight (1922) proceeds to explain why this part is small. It arises from the fact that, although economics and ethics both deal

with values, ethics cannot be collapsed into economics. This is because economics adopts a scientific approach, while any real ethics must be unscientific (Knight, 1922: 39). Economics treats human wants, desires or ends as data in an ultimate scientific sense, that is, as objective and measurable magnitudes that will remain constant, static or will 'stay put'. Such stability is necessary if they are to be regarded as causes of behaviour. If they were to chop and change it is unlikely that statements based on them would generally remain true after they were made.

But, by treating wants in this way, there can be no room for a separate ethics. If individual wants or desires are to be taken as given data 'in the ultimate sense', then there can be no debate as to the merits of these ends on ethical grounds. Any given end would be as economically rational to pursue as any other. 'The ideal man would be the economic man who knows what he wants and "goes after it" with singleness of purpose' (p 38). Instead, in the world in which we live, this kind of selfish behaviour is the object of moral condemnation: there is a separate ethics which stands apart from this economic interpretation.

There is a place for ethical theory in the world because economics is wrong to treat human wants as ultimate scientific data and their satisfaction as 'the essence and criterion of value': the fact is that the 'given conditions' in economics, the causes at work, 'are not really given . . . wants are not ultimately data' (p 35). Human wants are not only not ultimately given, they are also far from stable. They are forever growing and changing, always no more than simply means to new wants (p 23). This is not to say that wants cannot be regarded as causes of behaviour, only that behaviour cannot be understood as attempting to achieve the satisfaction of any given want. People do not expect to, and do not try to, satisfy any given desire (such as happiness). Rather the situation is one of shifting goal posts. The very process of satisfying desires leads to new or re-defined desires or objectives.

'A science of conduct is, therefore, possible only if its subject-matter is made abstract to the point of telling us little or nothing about actual behaviour. Economics deals with the form of conduct rather than with its substance or content' (p 36). For example, wealth only serves as a want because it is no more than an abstract term that embraces everything which individuals do actually (provisionally) want. To study the

concrete content of motives we must turn to history which is not a science.

Ethics has to do with ends. We have seen that the attempt to treat ends as scientific data - as economics does - breaks down under examination. These limitations of scientific explanation point to the view that any real ethics will be non-scientific (pp 38-9).

Economics is closer to ethics than mechanics

While mechanics is commonly conceived as being concerned with the action of forces, the scientist accepts that he is concerned only with the effects of these forces. The forces themselves are unknowable metaphysical entities with no real existence. They are no more than an aid to our thinking. Just as 'force' in mechanics 'explains' the movement of objects, so 'desire' in economics 'explains' the purchasing of goods. Yet the notions of force and desire, Knight maintains, are far more than an aids - they are indispensable to our thinking (Hammond, 1991: 360). Knight (1925) outlines two sources of information about desires in economics compared to only one source of information about forces in mechanics (the observed effects of these forces). In economics we are not limited to merely inferring desires from their behavioural effects: we can also feel desires within ourselves directly, and in others through language and social interaction. The wants which are the concern of the economist are quite different from causes of action analogous to those of mechanical forces (Knight, 1925: 81-86).

While science insists upon a sharp distinction between fact and desire, and between observation and inference, such distinctions cannot be made. With regard to economic demand, science views our knowledge of various desires as an inference from some kind of behaviour. Conscious desires are excluded from being scientific data. Consciousness is no more than a convenient assumption. But, not only do we infer consciousness directly from our own consciousness, 'we cannot perceive the objects themselves as real without making this inference to a certain extent, without reading our own experience into them' (p 93). Furthermore, economic wants are products of a social culture rather than some sort of objectively definable biological need. While much conduct is reducible to mechanical laws susceptible to the

application of objective technique, the really important part which stems from 'deliberate creative choice can never be distinguished and measured' (p 98).

> The purposes of men are inherently dynamic and changing; want-satisfying activity is not in the main directed toward gratifying existing desires sharply defined as data in the conduct problem; it is largely explorative in character . . . We do things to prove that we can, and to find out whether we like to; the problem is largely to understand the problem itself, and . . . understanding it largely carries the actual solution with it as a matter of course. As we know more, both the self and the world are enlarged, and this growth is life (pp 101-2).

The limitations of the scientific method in economics

Knight (1924) describes various limitations of the scientific method in economics. Here we highlight five. In terms of Knight's pragmatic conception of science, science is an 'instrumental' activity concerned with how to use given means to achieve given ends. However, as we have seen in the foregoing sections, instead of being content with satisfying given desires life is a constant striving for better values, for knowing ourselves. 'This fact sets a first and most sweeping limitation to the conception of economics as a science' (Knight, 1924: 105).

While the immediate purpose of science is to allow us to understand, in our scientific age this has been subordinated to a desire for control. As such science becomes no more than the technique of prediction, the process whereby we bring about a desired result. However, it should be noted that the complicated techniques of science are not used in reaching the practical decisions of everyday life. These are made by a process that is mostly unconscious. In the field of human behaviour the various prerequisites of science - the need for the data to be static and stable, the ability to classify the objects of experience into classes of manageable number, and the objective measurement of the data - do not hold good. The nature of the data thus sets a second limitation upon the scientific treatment of social and economic problems (pp 118 and 147).

Knight takes for granted the fact that, using common-sense methods, we are able to predict and control the behaviour of other people. Common sense does this by

connecting actions with feeling. However, according to behaviourism we should ignore consciousness in our fellow human beings when explaining conduct. This is a third limitation of using scientific method in economics: the scientist is restricted to inferring behaviour from previous observations. The 'enthusiastic' behaviourist may well deny that behaviour is related to consciousness, but in doing so he is turning away from science to philosophy. Instead the scientist should be concerned with the practical question of whether the notion of consciousness is useful in prediction. Knight argues that it is useful in science and inevitable in social science.

A fourth limitation of scientific method relates to the fact that, unlike the natural sciences, history plays an important part in the social sciences. For instance, the reaction of an individual to an object depends not only on the individual and the object, but also on the previous history of the individual (pp 122 and 130). Since each individual's history is essentially different, this explains why there are such varying reactions to given stimuli among human beings. These varying reactions are explained by the 'psychological', rather than the behavouristic, method.

While 'human phenomena are not amenable to treatment in accordance with the strict cannons of science' (p 129), nevertheless, scientific study is much more helpful than common sense in estimating probabilities so that we may hope for 'laws' of a statistical character. But apart from practical difficulties, turning to statistics generates a more general problem. 'It is only with reference to individuals in distinctly individual relations that differences cancel out or reduce to percentages': the same cannot be said for groups where group psychology and not statistics become relevant (p 132). This constitutes a fifth limitation of the scientific method in economics.

The extent to which the methods of the natural sciences may predict and control better than those of common sense or the intuitive processes of art appears doubtful. 'The kind of thing which human nature is is shown by the forms of language used in describing it' (p 134). And the form of language used is figurative rather than literal which means it cannot be subjected to the technique of analysis, nor to that of natural science. 'It seems to us that science is a special technique developed for and applicable to the control of physical nature, but that the ideal so constantly preached

and reiterated, of carrying its procedure over into the field of the social phenomena rests on a serious misapprehension' (p 133). This misapprehension is not only to do with the various limitations just reviewed, but has to do with moral questions concerning the notion that, while some social control is necessary, it should be minimized since the ideal is freedom.

Knight (1924) concludes that, in spite of all that has been said so far, there is a true, exact science of economics with universal laws similar to those of mathematics and mechanics. If economics is to develop further, there needs to be an appreciation of both its meaning and limitations. Economic laws, eg, the laws of diminishing utility and returns, refer not to the content, but to the form, of economic behaviour. They can be thought of either as 'intuitively' known or as arising from fundamentally 'necessary' Kantian facts of observation. 'The great fact that makes economic theory so vague and so difficult is the confusion already referred to as the relations between cause and effect, or the interpretation of the 'given' conditions' (p 141). In other words, this is the distinction between independent and dependent variables. This underlies the distinctions between static and dynamic, between short and long term, deductive theory and institutional economics. These are all matters of degree and all useful and necessary distinctions.

Conclusion

Since Hutchison (1938) draws on the methodological writings of economists as well as those of the philosophers of science, some understanding of the methodology of economics is necessary in order to evaluate his 1938 essay. In the first of the four sections of the chapter, we made use of Hutchison's own distinction between the empirically minimalist deductivist approach of Ricardo, Senior and J S Mill and the more empirical inductivist approach of Smith, Jevons and Marshall. To counteract the effect of approaching the history of economic methodology from Hutchison's viewpoint, we took a closer look at the writings of Robbins, J M Keynes and Knight.

We found a number of economic methodologists, including Blaug (1980), Coase (1994), Redman (1997) and Schumpeter (1954) in support of, at least aspects of, Hutchison's distinction. Our more detailed investigation of some early twentieth

century methodological writings revealed Robbins and Knight as belonging, broadly speaking, to the empirically minimalist line, whilst Keynes appeared to straddle both approaches. While it is of course possible to interpret particular economists, eg J S Mill or Marshall in ways different from Hutchison, the important point is that there is substantial evidence for the existence of a long-standing *methodenstreit* in economic methodology between rationalist-leaning and empiricist-leaning economists going back well before the famous exchange between Menger and Schmoller. That this point is relevant to interpreting Hutchison's methodology is borne out by the fact that Hutchison himself has drawn attention to its existence and longevity by referring to the famous debate between Ricardo and Malthus as perhaps the first *methodenstreit* in economics (Hutchison, 1998: 45).

While this *methodenstreit* may be regarded as a struggle between competing empiricist-inductivist and rationalist-deductivist approaches, such a conception is no more than a convenient analytical distinction. The fact is that many, if not most, leading economists have long recognized the need for elements of both approaches. The issue at stake is the relative role for each. The differences between the leading thinkers in the field are based on subtle and fine distinctions. For instance, although Kaufmann is closely linked to positivist views (Robbins, 1938; Blaug, 1980), as we saw earlier in this chapter he clearly maintained non-positivist notions, eg that the methods of physics cannot be applied to economics (Kaufmann, 1933). While the complexities of an actual methodological position such as Kaufmann's goes some small way towards explaining how both Hutchison (1938) and Machlup (1978) could regard Kaufmann with approval despite their differing approaches, it is even more significant to the extent that it can be interpreted as indicating that Machlup (1955) grossly exaggerated the differences between his and Hutchison's methodological positions.

In the next chapter we argue that Hutchison's 1938 essay needs to be viewed as arising out of this finely-tuned and long-standing methodological debate, and not simply as the result of a youthful over-enthusiasm for the leading theory in the philosophy of science of his day (as implied eg by Knight, 1940). In Chapters Four and Five we will see that the failure of Knight and Machlup to view Hutchison's 1938 essay within the context of this *methodenstreit* is one of the factors that leads to their

respective misinterpretations of Hutchison (1938). With some knowledge of the history of economic methodology, we are now in a position to appreciate the extent to which Hutchison was familiar with, and drew on, the methodological writings of economists as opposed to the literature on the philosophy of science.

CHAPTER 3

THE NATURE OF HUTCHISON'S INTERVENTION

Hutchison's (1938) essay has variously been viewed as introducing positivism, ultra-empiricism, and Popperian falsificationism into economic methodology (Knight, 1940; Machlup, 1955; Rosenberg, 1976; Blaug, 1980; Caldwell, 1982; Hausman, 1992). Hausman (1992: 152) contends that 'the first real change in accepted views of theory assessment in economics occurred in the 1930s'. The 'revolution in the methodological self-conception of the economics profession' that occurred in the 1930s was spearheaded by the 'positivist challenge' to the prevailing accepted 'abstract' deductive method presented by Hutchison (1938).[1] The question of the extent to which Hutchison (1938) set off a methodological revolution in economics is a matter of debate. Caldwell (1982: 115) contends that 'the movement towards positivism' was not the result of Hutchison's book - it only served to 'confirm changes' that were already taking place. By contrast, Hausman points out that Knight (1940) was 'right to worry' about its influence on the young since 'even the defenders of economics wound up fully accepting Hutchison's central philosophical premises' (1992: 155). While this issue is one that deserves further investigation, our concern in this chapter is primarily concerned with the extent to which Hutchison's work is accurately characterised as a 'positivist challenge' and, more broadly, with clarifying the nature of his 1938 contribution.

In this chapter we will argue that the general consensus that Hutchison introduced positivism into economic methodology has obscured the extent to which the views he espoused in 1938 had their origins in his quite separate engagement with the questions and issues of economics. In particular, two key aspects of Hutchison's intervention stand out quite independently from logical positivist ideas. The first is his concern with the historical and institutional nature of the subject-matter of economics. The

[1] Hausman (1992: 153, n 3) points out that 'positivist themes' by Kaufmann (1933, 1934, 1936) and Fraser (1937) had little influence compared to Hutchison (1938) - the 'positivist challenge that caught the attention of the economics profession' (p 153).

emphasis on this dimension is somewhat in the background in 1938. Nevertheless it is this, rather than any logical positivist notion, that lies behind his criticism of the 'traditional procedure of theoretical economists': the 'optimistic' approach (Hutchison, 1938: 73-76). Here, he points out, the analysis of equilibrium under conditions of perfect competition appears to be unable to advance from a static to a dynamic system 'where an analysis of change or causality can be introduced' (p 75)[2].

A second key aspect of Hutchison's intervention is his preference for the inductive-SM, rather than hypothetico-deductive, method as the scientific procedure appropriate to economics. As we learnt in Chapter One (section 1.3.4) both methods existed in logical positivism from its earliest formulations in the 1920s. However, over time, the so-called logical positivist 'received view' of the formal structure of a theory came increasingly to reflect the notion that science develops according to the hypothetico-deductive method (Suppe, 1977). By contrast, for Hutchison, inductivism is more appropriate to economics given its vital historical subject matter. This is why he places such importance on the role of induction and inductive generalisation (1938: 7, 25, 62, 164, 166). In particular, it should be noted that the 'conceivable falsifiability' that distinguishes scientific from non-scientific propositions, for Hutchison, is the characteristic of an inductive inference (p 25).[3]

It is against this background that we argue that it is incorrect to view Hutchison's (1938) as representing the wholesale introduction of logical positivist ideas to economic methodology. Instead, Hutchison's essay is better understood as part and parcel of a long-standing tension in the methodology of economics. Although Hutchison regularly refers to this tension in his 1938 essay, he most explicitly characterises it in an article fifty years later in which he distinguished on the one hand between the empirically minimalist 'ultra-deductivist' approach of Ricardo, J S Mill, Senior, Cairnes and Robbins, and the inductivist, empirical and historically-minded approach of Smith, Jevons, Marshall and J N Keynes, on the other (Hutchison, 1998).[4] But for reasons of modesty, he might well have included his own name after that of

[2] In this chapter all unsupported page references are to Hutchison (1938).
[3] By contrast, Popper valued the characteristic of falsifiability for its role in logically avoiding the problem of trying to inductively verify the truth of a proposition.
[4] This is to imply that our evaluation will involve mainly a rational reconstruction of Hutchison's intervention (Torr, 2001).

Keynes. This methodological tension famously erupted in the *methodenstreit* of the 1880s between Menger and Schmoller. Hutchison (1953: 66, n 1) points out that the historical movement of the 1870s in England was largely home-grown and 'was not an importation from Germany'. In the same way we argue that the most important arguments of Hutchison's essay are grounded in the (largely British) empirical economic methodological tradition rather than imported from Vienna.

3.1 Chapter I – Introduction

Hutchison states that he aims in his book to clarify the significance of the 'pure theory' of economics (p 1). The main problem in this regard is that it is not clear as to what are the basic concepts, assumptions and propositions of economics (p 2). This problem is fundamental since 'no advance in the elegance and comprehensiveness of the theoretical superstructure can make up for the vague and uncritical formulation of the basic concepts and postulates' (p 5). However, all attempts to clarify these foundations have resulted in inconclusive methodological and philosophical controversies (p 5). Hutchison suggests that, by distinguishing 'scientific' from 'philosophical' problems, we will be able to resolve the inconclusiveness of the discussions and so clarify the foundations of the subject (p 6).

While one can talk about the advance of science, the same cannot be said of philosophy (p 6). The advance of science is due to the fact that scientists accept certain intersubjective criteria for testing their propositions (p 7). This is in contrast to the fact that in two thousand years philosophers have never come to agreement. Thus, while two economists will very likely agree on whether or not the cheque system exists in Paraguay, it is very unlikely that two philosophers will reach agreement on this same issue (p 8).

At this stage let us pause to reflect on Hutchison's comments. His central concern is with the significance of the pure theory of economics. This topic is remarkably similar to Robbins's (1932, 1935) concern with the significance of economic science. Briefly, Hutchison's answer to this question is that while pure theory does have significance for economic science, it does not have nearly the significance that Robbins attributed to it in his famous *Essay*. There pure theory and economic science

are virtually synonymous. Hutchison is concerned to point out that it plays a far less important role as compared to that of the 'finished propositions' which Hutchison interprets as empirical statements.[5] Indeed, it is these propositions which are of central significance in any science. This reflects Hutchison's view of science which is clearly influenced by the logical positivism of his day.

Hutchison now explains his conception of science:

> The scientist proceeds by means of the two inextricably interconnected activities of empirical investigation and logical analysis, the one briefly, being concerned with the behaviour of facts, and the other with the language in which this is to be discussed (p 9).

His account of the nature of the scientific enterprise bears a striking resemblance to the formulation found in the 1929 manifesto of the Vienna Circle (Hahn et al, 1973: 309):

> We have characterised the *scientific world-conception* essentially by *two features*. *First* it is *empiricist and positivist* . . . *Second*, [it] is marked by the application of a certain method, namely *logical analysis*.

Again, when Hutchison goes on to state that it is 'largely the purpose' of his book to carry out 'logical analysis' (p 9), he reflects the logical positivist view of philosophy as the handmaiden of science, as an activity aimed at clarifying the concepts and language of science.

Out of this view of science Hutchison develops his Principle of Testability which states that the

> finished propositions of a science . . . must *conceivably* be capable of empirical testing *or be reducible to such propositions* by logical or mathematical deduction. They need not, that is, actually be tested or even be *practically* capable of testing . . . But it must be possible to indicate intersubjectively what is the case if they are true or false' (pp 9-10).

[5] He defines 'finished propositions' in expounding his Principle of Testability as propositions which must be capable of empirical testing and contrast them to logical and mathematical propositions (p 9). Together with his dichotomy (p 26) this implies that 'finished propositions' are synthetic empirical statements.

In a footnote he refers to the above as his Principle of Testability and goes on to say that the finished propositions of a science need not be 'empirically testable *directly*, but may be reducible by direct deduction to an empirically testable proposition or propositions' as for, example, propositions about electrons in physics (p 19, n 6, original emphasis). Such tests should not be viewed as finally deciding on the absolute truth or falsity of such propositions (p 9).

Hutchison points out that he is not suggesting that this principle is the best, or indeed some kind of ultimate or final, criterion for distinguishing science from non-science (p 12). These are general methodological issues which he leaves for others to discuss (p 12). Neither does he particularly want to advocate that this principle or criterion should be adopted (p 12). Rather, he is simply concerned to show the consequences for economics should such a principle be adopted (p 12). Indeed, his essay is mainly directed towards those who already accept that it is only by this principle that we have both 'a method of reaching agreement and a barrier against the pseudo-scientist' (p 13) and for excluding expressions of ethical or political passion, poetic emotion or metaphysical speculation (p 10). He accepts that his criterion implies that the methods of the natural sciences are applicable to the social sciences (p 14). Such a naturalistic conception would be rejected in Germany by writers such as Professor Sombart (1930). But then he appears to be left with no defence against the rising tide of propagandist pseudo-science (p 16).

Here we may pause to reflect on Hutchison's Principle of Testability. Hutchison is quite explicit about the point that the 'finished propositions' need not be capable of being tested directly, or practically now or in the future, so long as it is conceivable that it can be so tested, it passes muster as a scientific proposition. In qualifying his Principle he makes it clear he has in mind propositions in physics about unobservable entities such as electrons which cannot be directly verified. Despite these remarks, Machlup (1955) was to accuse Hutchison of ultra-empiricism because, according to Machlup, he insisted on the direct verification of basic assumptions (see Chapter Five). Hutchison's reference to propositions about electrons in physics is evidence that he is far from espousing a naïve inductivist position.

The importance of the Principle for Hutchison is that it provides a 'method of

reaching agreement' among scientists and as such is the critical factor enabling advance in science as compared to the inconclusiveness of philosophic discussion. It also provides a 'barrier against the pseudo-scientist', ie a criterion of demarcation between science and pseudo-science, 'quackery, prejudice and propaganda', as well as a barrier excluding ethical and metaphysical propositions (p 10). On both these points, but especially the second, Hutchison is clearly influenced by logical positivist thinking.

Likewise, Hutchison is aware that his Principle of Testability implies that the significant distinction is between science and non-science rather than between natural and social science. While, as we have seen from Chapter One, the unity of science is a major tenet of logical positivism, Hutchison points to the fact that it follows in the tradition of English writers such as J S Mill and Jevons who, unlike the German writers such as Sombart (1930), do not mention the idea of any fundamental difference between the methods of the natural and social sciences (p 14). Contrary to the view that emphasises Hutchison's links with the Vienna Circle, here is an instance in which, while he could easily have cited Viennese sources, he chose to limit his references to those of British empiricists. Besides which, from the way Hutchison phrases his remarks on this question, it appears he is more concerned with having some defence against the rising tide of 'propagandist Pseudo-science' in Germany than in enthusiastically advancing a tenet of logical positivism (p 16). Again, Hutchison's point that any tests should not be viewed as finally deciding on the absolute truth or falsity of a proposition appears to reflect Popper's fallibilistic thinking that knowledge is provisional, rather than logical positivist verificationism.

In putting forward this Principle and emphasising its crucial importance for any scientific investigation, Hutchison broke new ground in economic methodology. His proposal contrasted sharply with the then influential Robbins-von Mises a priorist methodology which denied that the scientific validity of economic theories could be empirically tested. In much the same way as Robbins (1932) provided what has since become the standard definition of economics, so Hutchison (1938) provided what has since become standard practice for research in economics. As noted by Coats (1983: 13), Hutchison's placing of testing on the methodological agenda is one of the 'greatest virtues' of his 1938 book.

Hutchison ends the chapter by outlining the scope and programme of his essay. To be useful to the scientist, the methodologist must accept the same criteria (of empirical testability and logical consistency) as the scientist himself who is working on the superstructure. Hutchison explain that sciences start at a common-sense level of tackling problems and then build upwards a superstructure of laws and relations and downwards their foundations (p 16). In working either on the foundations or on the superstructure, there is no role for the methodologist or philosopher separate from that of the scientist (p 16). In particular there are no metaphysical assumptions on which a science supposedly rests waiting to be discovered by the methodologist or philosopher (p 17). This is not to imply that economists should not bother about philosophical problems and confine themselves to tackling problems at the common sense surface level (p 18). The purpose of science is to overcome the crudities of common sense. Furthermore, it is the unsatisfactory state of the foundations of economics that is its most serious problem (p 18).

Hutchison began the chapter with the contention that it was difficult to discern the significance of the pure theory of economics because the foundations of the subject are not clear. This is due to the inconclusiveness of traditional methodological and philosophical discussion. In an effort to advance out of this state of inconclusiveness, Hutchison distinguishes between science and philosophy. Science advances because it has intersubjective criteria for testing its propositions, ie a method of agreement unlike philosophy. Hutchison attempts to formulate a version of this method of agreement, his Principle of Testability. The implication is that if economics is a science, then its advance depends upon the application of this Principle or some such similar version. Employing empirical testability and logical consistency rather than searching for metaphysical assumptions, the methodologist (in this case Hutchison) will be in a position to clarify the foundations of the science of economics and thereby discern the significance of the pure theory of economics.

The respect for science as opposed to philosophy, the view that there are no metaphysical assumptions waiting to be discovered in the foundations of the subject, and the conception of the project of his book as clarifying these foundations via logical analysis are all in line with logical positivist thinking. But, as we shall see as

we progress through Hutchison's essay, his methodology is by no means limited to logical positivist thinking. Instead we will show that he draws on a wide range of the philosophy of science as well as extensively on the methodology of economics literature. Hutchison is concerned with the methodology of *economics*, not with applying a particular philosophy of science to the subject.

3.2 Chapter II – The propositions of pure theory

Hutchison begins by distinguishing three kinds of propositions. The first takes the form 'if p then q' and holds with the same unconditional necessity and certainty as those of pure logic and mathematics. The pure theory of economics is made up of propositions of the kind (pp 23-4). Following Schlick (1925: 43; 1974: 45), he points out that it is difficult to distinguish them from a second kind of proposition since this also takes the form of 'if p then q' and so in ordinary language may be worded in the same way. Yet these propositions are completely different: they are inductive inferences 'won by experiential observation' (p 37). Unlike propositions of the first kind, they do not represent a logically necessary, but rather a conceivably empirically falsifiable, relation (p 25). A third kind of proposition, propositions of applied theory, may be distinguished. This kind takes a different form, namely, 'since p then q'. Here 'p' is asserted to be empirically true.

Hutchison now proposes 'an exhaustive twofold classification of all propositions which have "scientific" sense':

> Either a proposition which has sense is conceivably falsifiable by empirical observation or it is not. If it is not thus falsifiable it does not, if true, *forbid* any conceivable occurrence, but only a contradiction in terms (p 26, original emphasis).

Propositions which are conceivably falsifiable correspond to the 'finished propositions' of science, that is, to the second kind of proposition distinguished above (synthetic empirical). Those which are not falsifiable, correspond to the purely logical or mathematical 'accessory propositions', that is, to the first kind of proposition distinguished above (analytic a priori). But the price of the 'unconditional necessity and certainty' of such propositions is 'complete lack of empirical content' (p

27). Hutchison stresses that this classification can only be criticised for being inconvenient, not for being empirically false. In other words he is proposing a classification, rather than making an empirical proposition about the existence or non-existence of other conceivable types of propositions (p 27). For example, neo-Kantians might find it inconvenient since Kantian synthetic a priori propositions cannot be easily fitted into it (p 46, n 7).

At this stage we pause for comment on Hutchison's 'exhaustive twofold classification'. Coats (1983: 7) notes that this 'central distinction' of Hutchison's is 'basically Kantian', and can be found *inter alia* in Kaufmann (1936). Yet, the view that only these two kinds of statements have scientific sense draws on both logical positivism and Popper (1934), and Kantian synthetic a priori statements cannot easily be fitted into it (p 46, n 7). Since, in terms of the classification, all propositions which have scientific sense are either synthetic empirical and thereby empirically falsifiable or else are analytic a priori, Hutchison's twofold classification may be regarded as reflecting, although not explicitly, Popper's criterion of falsifiability for demarcating science from non-science. Hutchison's view that all the propositions of the science of economics can be classified exclusively into either synthetic empirical or analytic a priori statements has met with widespread criticism (Machlup, 1955; Klappholz and Agassi, 1959; Rosenberg, 1976; Blaug, 1980; Caldwell, 1982).

The chapter is entitled 'The propositions of pure theory' and, following from his introductory chapter, Hutchison's main concern is to use logical analysis (cf p 27) to clarify the significance of these propositions. This he has now succeeded in doing in this chapter by classifying them as analytic a priori propositions. They are analytic a priori because their truth is 'independent of all facts', although the fact of their applicability depends on their empirical truth (p 24). On this point Hutchison (p 25) cites Jevons (1887: 235): 'If a triangle be right-angled the square on the hypotenuse will undoubtedly equal the sum of the squares on the other two sides; but I can never be sure that a triangle is right-angled'. And, in one of his chapter two introductory mottoes, in this connection Hutchison cites Einstein (1921: 3): 'Insofar as the propositions of mathematics relate to reality, they are not certain, and insofar as they

are certain they do not relate to reality'.[6] In passing we note that these quotations from Jevons and Einstein constitute evidence for our thesis that Hutchison drew on far more than logical positivism in his 1938 contribution to economic methodology.

In a lengthy footnote explaining his ideas on pure and applied theory, Hutchison (1997: 144 ff) maintains that in 1938 he was trying to clarify the term 'theory' which was then being qualified by the two adjectives 'pure' and 'applied' by sharpening up, or clarifying, this terminology. The problem with these, we may remember, is that

> As I defined 'pure theory', or propositions thereof (1938: 23), they simply stated a purely logical (or mathematical) relationship between empirical assumptions which were not asserted as true, but which served as purely hypothetical postulates, from which empirical but hypothetical conclusions were deduced. Thus 'pure theory' was simply concerned with logical or mathematical relationships and could not be tested empirically; though, of course, both the postulates and the conclusions, usually could be so tested (unless laid down as definitions, or as somehow given a priori) (ibid).

He points out that he described pure theory as without empirical content, but not as thereby trivial, a criticism he had then described as 'completely fallacious' (p 32).[7] He goes on to compare his concept of 'pure theory' to 'the kind of instrumental concept vaguely indicated by all those metaphors about 'theory' as a 'box of tools', or as 'an engine for the discovery of concrete truth, rather than a body of concrete truth' (Marshall, in Pigou, 1925: 159).

Unlike pure theory, he defined applied theory as beginning with the assertion of an empirical synthetic statement (p 23). It is therefore 'quite inaccurate' of Hausman (1992, p 153) to claim that he (Hutchison) had regarded 'theoretical economics' as having no empirical content. 'I only regard, and define, "pure" theory as being without empirical content; though this, unfortunately, may constitute quite a large part of "theory"' (Hutchison, 1997: 145).

While the main thrust of this second chapter is to do with the significance of the propositions of pure theory, (the a priori analytic propositions of his twofold

[6] As translated by Hutchison (2000: 357).
[7] Hutchison (1997: 144) incorrectly gives p 37 as the reference.

classification), it is vital, in coming to grips with Hutchison's contribution, to understand what he is saying about the other kind of proposition contained in his twofold classification of propositions that have 'scientific' sense. While this kind has the same 'outward form' (ie in terms of ordinary language) as the propositions of pure theory, it is quite different in that it is an inductive, rather than deductive, inference (pp 25, 37). It is a '*conceivably* falsifiable, even if *in fact* not falsified inductive generalisation' (p 25, original emphasis). We noted that Hutchison referred to Schlick (1925: 43; 1974: 45) in distinguishing these propositions from the a priori analytic propositions of pure theory. In articulating his Principle of Testability, Hutchison had said that the finished propositions of a science must be conceivably testable. It is only now, in distinguishing deductive from inductive inferences, that Hutchison uses the phrase 'conceivably falsifiable' (pp 25, 26, 27, 62).

What needs to be brought out is that the importance to Hutchison of propositions being 'conceivably falsifiable' is that, despite such propositions having the same outward form as those of pure theory, by using this criterion we are now able to distinguish them from the propositions of pure theory. For Hutchison, falsifiability is a criterion for distinguishing inductive, empirical synthetic propositions from deductive, a priori ones. That is, it enables us to distinguish the 'finished propositions' of science from 'the accessory purely logical or mathematical propositions used in many sciences' (p 9). And, for Hutchison, these are inductive generalisations (p 25). We emphasise this point, for it is central to our thesis that Hutchison follows a more inductivist-SM-leaning approach to science than logical positivists in general who leant more towards a hypothetico-deductive approach to science.

We also emphasise this point because it is important in understanding the extent to which Hutchison may be regarded as introducing Popper's falsifiability criterion into economics (Klappholz and Agassi, 1959: 63; Blaug, 1980: 94; Coats, 1983: 8). Hutchison's concern with falsifiability is not put forward, as is Popper's criterion of falsifiability, in opposition to the logical positivists' criterion of verifiability. According to the logical positivists, as we saw in Chapter One, a proposition has meaning only to the extent that it is verifiable. Popper pointed to the problem of induction associated with this criterion. Whereas no amount of confirming instances

can ever be said to finally verify a general proposition, just one counter instance can falsify a general proposition. Popper therefore proposed falsifiability to as a criterion to distinguish between science and non-science (rather than the positivists' sense and nonsense) as a solution to the problem of induction.

Far from displaying Popperian concern with the problem of induction, Hutchison argues that it is inductive generalisations that are central to science. So, while Hutchison introduced Popper's demarcation criterion into economics, this does not imply that he thereby introduced Popperian ideas opposed to inductivism. Blaug (1980: 94) is therefore misleading when he implies that Hutchison recognised the significance of falsifiability as a solution to the problem of induction that dogged the logical positivists' verifiability principle of meaning, something that, Blaug says, even Ayer (1936) 'completely missed'.

Hutchison's (1938) chapter two developed out of his earlier 1935 'Tautologies' article. In this article he distinguished only two kinds of propositions - those of pure, and those of applied, theory. Although in 1935 he referred to 'inductive hypotheses', he did not distinguish inductive inferences as a separate kind of proposition characterised by the fact that they are conceivably empirically falsifiable. Coats (1983: 10) views Hutchison's 1938 development of this earlier classification as a 'sizeable step forward . . . toward Popper's concept of falsification'. From the foregoing discussion, it should be evident that this remark needs qualification. For it is accurate in so far as Hutchison supported falsifiability as a demarcation criterion between 'scientific' sense and 'non-scientific' sense, but not to the extent that it implies he supports falsifiability as an alternative to an inductivist-SM methodological programme. Rather than any move toward engagement with Popperian ideas, Hutchison's step in 1938 is better viewed as consolidating his inductivist views of 1935. In 1935 he had stated: 'The necessary fundamental assumption of all scientists is that there are some regularities about the facts of the world which allow of successful inductive hypotheses (Hutchison, 1935: 161). While this inductivist view was espoused by Schlick and the early Carnap (see Chapter One: Appendix), it was a view which most logical positivists, in line with the later Carnap, abandoned as they increasingly adopted the implications of the hypothetico-deductivist method of practising science.

In expounding his principle of testability, Hutchison did not refer to any philosopher or methodologist in particular (p 9). Later, with regard to his twofold distinction (p 26), when he distinguished deductive from inductive generalisations (p 25) he referred to Schlick (1925: 43; 1974: 45). And in explaining that the price of the 'unconditional necessity and certainty' of propositions of pure theory was 'complete lack of empirical content' (p 27) he referred to Feigl (1929: 11). In both his principle and twofold distinction, there is no direct reference to Popper (1934). Nevertheless this is not to imply that Hutchison did not introduce Popper's falsifiability criterion into economics. That he preferred to distinguish between 'science' and 'non-science' rather than between 'sense' and 'nonsense' (p 19, n 8) would seem to indicate awareness of Popper's departure from the logical positivist concern with meaning. We are simply explaining the way in which Hutchison can be said to have done so. This we attempt in the following paragraph.

The Popperian element in his twofold classification of all propositions is represented by his view that if a proposition has sense, and is not conceivably empirically falsifiable, then 'it does not forbid any conceivable occurrence, but only a contradiction in terms' (p 26). Hutchison goes on to explain that: 'Propositions obtain their empirical content simply in so far as, if true, they exclude, restrict, or forbid something (e.g. "This table is wooden", if true, forbids or excludes "This table is of iron" etc.)' (p 26). Much later, in his chapter four, in explaining how the empirical content of the 'fundamental assumption' had grown smaller and smaller over time (p 115), Hutchison referred to Popper (1934: 13, 43) for the relation between falsifiability and empirical content (p 126, n 52): 'Popper brings out very clearly that it is the function of a scientific law to "forbid" some conceivable types of occurrence: "Not in vain (or not inappropriately) laws of nature are called laws and they say all the more, the more they forbid".[8] A circularity or tautology "forbids" nothing. It is true whatever occurs, and therefore empirically empty'. With reference to the foregoing, Hutchison goes on to state:

> I thought that was clarifying and that is the essential sentence I picked up from Popper - there are one or two others. Well I came to like his whole approach –

[8] This sentence from Popper (1934) translated from the German by Hutchison, 10th July, 2000.

> *his* sort of 'rationalism' – and fallibility. Scientific knowledge is fallible and testable and may be knocked down at any moment. And being against certain things - like these people, such as Mises, who claimed absolute certainty for their 'laws' (Hutchison, in Hart, 2002).

So it is clear that, while Hutchison did not directly refer to Popper in formulating his Principle of Testability or his twofold classification, the notion that, what distinguished scientific statements from others is their characteristic of excluding, restricting, or forbidding some (empirically) conceivable type of occurrence, is explicitly traceable to Popper (1934) in Hutchison (1938). Therefore Coats (1983: 8) is misleading in stating that Hutchison's first published reference to Popper's demarcation criterion occurred only in the 1960 edition (p vii) of his 1938 book. Moreover, as we have seen, Hutchison first (p 25) referred to Schlick (1925: 43; 1974: 45) concerning this notion. And he also referred (p 61) to Mach's (1905 [1976: 352]) view that 'A law consists always in a limitation of what is possible'.[9] The notion of falsifiability, it seems, is not a uniquely Popperian characteristic, but is part and parcel of logical positivist thinking. Later Hutchison was to comment: 'I don't know if he [Popper] derived anything from Mach - he might deny that' (Hart, 2002).

In the second section of his chapter two, Hutchison sets about clarifying the source of the necessary truth of the 'analytical-tautological propositions of pure theory' (p 30). Tautologies have been taken to be synonyms for self-evident propositions. This is incorrect. To describe a proposition as self-evident or obvious is a purely psychological subjective judgement (p 28). By contrast, analytical tautological propositions are to be judged in terms of logic only. They need not be obvious or self-evident in order to be tautologies (p 28). Hutchison expands on this point showing logically that, by a process of assigning definitions, the proposition 'Under perfect competition firms are of optimal size' (which is far from self-evident or obvious) is in fact a tautology (pp 28-30). This is in line with Russell's (1927: 171) contention that propositions which 'can be proved by logic' are tautologies (p 30). He stresses he is emphasising their verbal rather than empirical nature, not in order to belittle pure theory (p 33), but because so much needless controversy has arisen in

[9] Cf 'A law always consists in a restriction of possibilities, whether as a bar on action, as an invariable course of natural events, or as a road sign for our thoughts and ideas that anticipate events by running ahead of them in a complementary manner' (Mach, 1976: 352).

economics as a result of people using the material, rather than the formal, mode and supposing that certain propositions are empirical when they are only verbal (pp 31-2). If discussion was restricted to the formal mode it would be clear at once that certain issues involved no more than questions of convenience (p 32).

Hutchison's next section examines the use and significance of the propositions of pure theory. Following a lengthy quotation (pp 33-4) from Schlick (1925: 35; 1974: 37-8), he proceeds to distinguish three roles for them. First, they can serve to draw out the implications of, and relations between, our definitions thereby providing a precise language with which to confront concrete empirical problems (p 34). Second, they enable us to pass from one empirical proposition to another (p 34). Third, they enable 'sharp and clear answers to be obtained from empirical investigation' (p 35).

Hutchison (1935) had already referred to the fact that it was a commonly held view that an allegation that a theory was tautological was tantamount to a criticism of it. Likewise tautologies were taken to be synonyms for self-evident propositions. Here he tries to put the record straight by outlining the then modern view of Russell, Wittgenstein and the logical positivists which saw, or defined, analytical a priori propositions as logically necessary statements. The source of their necessity is to be found in logic. Their falsity involves a logical contradiction. This again emphasises that these statements are propositions concerned only with the meaning of words, and have nothing to do with facts, of how the world is. This is not to imply that they are trivial: they facilitate logical analysis.

Hutchison now turns his attention to the hypothetical, or 'isolating' method used in economics (p 36). This method consists of investigating hypothetical, or simplified, problem cases such as perfectly competitive markets, rather than the real world. Despite this, it has been claimed (Bohm-Bawerk, 1924: 193; von Wieser, 1929: 19) that these 'hypothetical experiments' are 'full substitute[s] for the laboratory experiments of the natural scientist' (p 37). Hutchison dismisses this claim as 'fantastic' and applies his exhaustive twofold classification of scientific propositions to explain 'why the procedure of the so-called "hypothetical experiment" is

completely different' (p 38). The main reason is that, since no synthetic empirical propositions are introduced in these hypothetical experiments, any results remain limited to analytical tautological propositions. To the extent that the propositions of 'hypothetical analysis' are based on assumptions which 'are not asserted as facts', its propositions are propositions of pure theory (p 39). Nevertheless, such propositions have all too often been interpreted as if they had some empirical content (p 38), as if they were dealing with 'things' and not 'words' (p 40). This misapprehension has arisen partly through ignoring (his) distinction between the two kinds of proposition that have 'scientific' sense: analytical a priori and synthetic empirical inductive propositions (p 38) and partly due to pure theory being discussed in everyday language, that is, working in the 'material', rather than the 'formal' mode of expression (p 39).

In the above section, Hutchison uses his twofold classification of all propositions that have 'scientific' sense to argue that the propositions involved in the hypothetical method are properly interpreted as propositions of pure theory. They are concerned with words and not things. The implication is that (the Austrian) economists who insist on using such a method are not engaging in (empirical) science. His reference to the material and formal modes comes from Carnap (1934: 210, 225; 1935: 46-81). Here Hutchison displays his degree of understanding of logical positivism.

Rosenberg (1976: 153) presents a criticism of, what he takes to be, Hutchison's argument that 'microeconomic general statements' are all tautologies.[10] He claims that Hutchison (1938: 38) argues that the propositions of pure theory are tautological because they 'are arrived at by pure deduction' and proceeds to demonstrate the error involved:

> Merely because one proposition is deducible from a second, the first is not thereby analytic, even if the second is assumed to be true. For a proposition to be shown to be analytic in virtue of its deduction from a second, the second must be assumed to be not simply true, but analytically true (ibid).

[10] Hutchison (1997: 146), noting that both Rosenberg's (1976) and Hausman's (1992) concerns are with microeconomics only, asks whether they believe that their methodologies 'can usefully be applied to macro-economics and its problems?'

Rosenberg supposes that perhaps Hutchison is assuming the assumptions of microeconomics to be analytic. He contends that such an assumption does not generally seem to be true (ibid: 154). Rosenberg concludes that this argument refutes Hutchison's claim that the propositions of pure theory are all analytic.

However, Rosenberg attributes an argument to Hutchison that Hutchison did not make. Hutchison did not argue that the propositions of pure theory are tautological because they 'are arrived at by pure deduction'. Rather he was concerned to argue that it is fantastic to suggest that the same results can be achieved from a 'hypothetical experiment' as from a laboratory experiment since the former can be no more 'than a preliminary thought-clearing exercise' (Hutchison, 1938: 38). Referring to the hypothetical experiment Hutchison explains: 'Here certain simplifying assumptions are made, and then what we have called "propositions of pure theory" *without any empirical content*, are arrived at by pure deduction' (p 38, original emphasis). In this sentence the phrase 'without any empirical content' merely describes the nature of propositions of pure theory. They can only be 'arrived at by pure deduction' because the statements in hypothetical experiments are not asserted as empirically true of 'the world as it is' (p 36).[11] In such experiments we are rather concerned with Robinson Crusoe-type cases which are claimed to 'throw light on the real problems' (p 37).

Like Rosenberg (1976), Caldwell (1982: 113) also interprets Hutchison as claiming that the propositions of pure theory are analytical or tautological because of the 'deductive form' of the argument through which they are arrived. For the reasons pertaining to the criticism of Rosenberg above, this interpretation appears mistaken. However, according to Caldwell, Hutchison also argues that 'the terms used in such a deductive framework are only logical categories, and thus do not make references to real objects' (ibid). This argument does not, says Caldwell, establish 'lack of empirical content'. Caldwell explains that this is because the terms of a hypothetico-deductive theory gain empirical content indirectly when its predictions are tested against reality. While Caldwell is quite correct, this is a criticism that is only tenuously related to Hutchison's position, given his scepticism regarding the

[11] A hypothetical experiment describing 'some model community representing an extreme case . . . cannot be anything more than a preliminary thought-clearing exercise' (1938: 38). Although this sounds like no more than a thought experiment, Hutchison (1938: 49, n 29) distinguishes a hypothetical experiment from a *Gedankenexperiment*.

suitability of hypothetico-deductive method in economics (Hutchison, 1992: 129).

Hutchison ends the chapter by looking at the drawbacks of qualifying propositions with a ceteris paribus clause. Some ceteris paribus propositions are meant to be empirical, while others are meant to be analytical (p 40). In the case of those that are meant to be empirical, the purpose of applying the ceteris paribus clause is to prevent these propositions from being easily falsified. While some lessening of falsifiability may be acceptable, ceteris paribus clauses are usually so vaguely formulated that these empirical propositions either become empirically unfalsifiable (p 41), or are transformed into necessary analytical-tautological, propositions (p 42). Whether such propositions are empirical or analytical is revealed only by test cases when scientists are forced to choose between accepting or rejecting that a proposition (eg all gases expand on warming) has been empirically falsified. If they refused to accept that the proposition could ever conceivably be empirically falsified, then we would know that the proposition (all gases expand on warming) 'was not an empirical law at all, but an analytical-tautological definition which was always true because it was not allowed to be false' (p 43). Lastly, the ceteris paribus assumption has encouraged the fallacy, noted by Edgeworth, of 'treating as constant of what is variable'. It has come to be so used that it seems to mean only 'in many cases' something or other follows. If the something or other fails to follow, it can be maintained that the proposition still holds since this was simply one of the cases in which it did not (or ceteris paribus did not hold) (p 44). Hutchison analyses an example from Keynes (1936: 185) to show that use of the ceteris paribus clause makes it ambiguous as to whether propositions qualified by ceteris paribus clauses are empirical, or analytical. Furthermore, it imparts 'an air of some kind of precise and widely valid empirical content' to otherwise empirically vague or empty propositions. For these reasons, Hutchison recommends that it be used less frequently and with greater caution (p 162).

In their (1959) criticism of Hutchison's twofold classification of all propositions which have scientific sense as inadequate, Klappholz and Agassi focused on this section of Hutchison's chapter on ceteris paribus clauses. We propose to explain this aspect of their criticism in Chapter Six. At this stage we limit ourselves to pointing out that Klappholz and Agassi miss Hutchison's central argument. Hutchison is not so much concerned with presenting a philosophical argument about the untestability

of propositions with unspecified ceteris paribus clauses, as with recommending that propositions (irrespective of how well specified their ceteris paribus clause is) should be used less frequently and with greater caution (1938: 162). By contrast, Hausman (1992: 153, n 4) does not miss out on Hutchison's central argument in this section. He criticises Hutchison for imposing 'unreasonably stringent justification conditions' concerning the circumstances in which it is legitimate to employ ceteris paribus clauses. Here Hausman's criticism arises from his adherence to deductivism in economics, a position which, he notes, Hutchison 'forthrightly' rejects (1992: 154). From this perspective, it would be surprising if he did *not* find the demands of an inductivist empiricist such as Hutchison 'unreasonably stringent'.

3.3 Chapter III - The application of pure theory

Hutchison might more aptly have called this chapter 'The misapplication of pure theory'. His main concern in the chapter is to emphasise the extent to which conceptions of the subject matter, the laws, and the predictions, of economics are misconception since they reflect the dominance of the a priori rationalist tradition (pp 58, 77, n 17, 63) in economics rather than the inductivist empirical tradition appropriate to an empirical science.

Concerning the misconception of the subject matter of economics, Hutchison says that this matter is not to be dealt with by concentrating on defining the subject matter of economics (p 53). Instead, it is far more preferable to distinguish between scientific propositions and the propositions of metaphysics, poetry, politics or ethics which, since they are not conceivably falsifiable, are not scientific (p 54). Rather than explaining the benefits of distinguishing between science and non-science, Hutchison says he wants to point out how 'certain authoritative definitions' have served to limit the propositions of economic science to propositions of pure theory. He selects, as one of these authoritative definitions, that given by Robbins (1935: 38):

> The subject-matter of Economics is essentially a series of relationships - relationships between ends conceived as the possible outcomes of conduct, on the one hand, and the technical and social environment on the other. Ends as such do not form part of this subject-matter. Nor does the technical and social environment.

Hutchison contends that, if the technical and social environments are ruled out, this would seem to exclude the 'entire possible factual material for the social scientist' - and, in addition, the study of economic conduct, since this too is something that is taken as 'given' (p 54). Instead, the economist's task is limited to pure deduction from certain fundamental assumptions of pure theory. And he is said to 'venture outside his subject' if he concerns himself with facts. This reflects the classical tradition 'brilliantly summed up in Ricardo's contrasting of "questions of science" with "questions of fact"' (p 55).[12]

With economics defined in this way 'it is hardly surprising that every single central proposition and system of economic theory' since the Physiocrats has been criticised for assuming what it was meant to prove (pp 55-6). According to Wicksteed (1933: 569, 790), Ricardo's theory of rent said nothing more than that 'the better article commands an advanced price in proportion to its betterness'. Cairnes (1874: 21) criticised Jevons's theory of value as saying no more than that 'value was determined by the conditions which determine it' (p 56). The quantity theory of money has long been recognised as a tautology. These propositions were claimed to be about the empirical world, and still today 'circular' propositions of pure theory are claimed to have empirical content (p 57).

Hutchison's preference for distinguishing scientific from non-scientific propositions rather than being concerned with defining the subject matter of economics, is clearly an application of his Mach, Schlick and Popper-inspired demarcation criterion (pp 9, 19, n 6, 25, 26). Hutchison does not explain the benefits of distinguishing between science and non-science, but instead seeks to show how attempts to define economics have been wrong-headed. In particular, Robbins's (1935) definition has limited the propositions of economic science to those of pure theory! It does this by excluding the factual investigation of the technical and social environment, and even economic conduct itself. The thinking behind these definitions reflects the Ricardian tradition of reasoning by 'pure deduction' from fundamental assumptions of pure theory. Hutchison interprets Ricardo as implying that

[12] Hutchison's (1938: 121, n 6) reference to Ricardo is his letter to Malthus of 22nd October, 1811 cited in Bonar (1887: 17-19).

> economists are not to concern themselves with what actually happens in the economic world, as this is simply a question of fact, and not of science. The *scientist* assumes that people are omniscient as to their interests and are out to maximise their money returns, and deduces conclusions dependent on these and other such postulates (Hutchison, 1938: 121, n 6).

For an empiricist such as Hutchison, this is a fundamental misconception of the subject matter of economics: facts are central to any subject claiming to be an empirical science.

Hutchison now turns to examine the nature of the laws of economics. These are conceived of in much the same way, as we have seen, the subject matter is viewed. According to Schumpeter (1914: 43) and Halévy (1928: 267-70), the economic doctrines of the Physiocrats are part of a wider natural law system which had its origins in a priori rationalism. In terms of this system, laws are not viewed as empirical regularities, but as behavioural rules. The laws of Ricardo and those that followed in his tradition (Senior, Menger) 'were essentially of the same type', that is, deduced from postulates describing a 'natural' perfectly competitive community. In other words, they are, in Hutchison's terminology, 'propositions of pure theory' (p 58).

Although the above conception of laws was dominant, it was not the only conception held by economists. For instance, both Jevons (1874, vol. ii: 430) and Pareto (1935: 35) regarded their laws as empirical and comparable to those of the natural sciences (pp 60-1). But empirical laws were regarded as inferior, criticised for not being necessary and even not considered worthy of being called 'laws' (p 60).

In response, Hutchison cites Mach's (1905; 1976: 352) conception of a law as consisting 'always in a limitation of what is possible' (p 61). Yet, he protests, in terms of the dominant conception of laws in economics, such laws set no such limitation. 'They exclude or forbid no conceivable type of occurrence' (p 61).

Hutchison concludes that the orthodoxy of referring to propositions of pure theory as 'laws' is 'misleading and inappropriate, and appears to be a survival from eighteenth-

century rationalist philosophy and theology' (p 63). He recommends that the term 'law' should be used only to describe empirical generalisations. This implies not merely a terminological change, but rather a fundamental change in the aims and methods of economics. In this regard he cites Hempel and Oppenheim (1936: 102):

> The formulation of empirical laws is not just a special problem of the exact natural sciences but the central problem in the construction of all scientific theories, since empirical laws are the foundation for all scientific explanation.

As with regard to the subject matter, so with regard to the laws of economics, Hutchison is at pains to show the extent to which these are conceived of in the a priori rationalist tradition. As we have seen in Chapter One, there are two main characteristics of empiricism. First, knowledge is based on empirical fact. Second, there are no essential relations between matters of fact. Hume had said there were no laws, but only tendencies. Here Hutchison shows how he follows in this tradition by arguing that the term 'law' should be reserved only for empirical generalisations, ie for general trends, rather than any necessary relations (cf Hutchison, 1977: 19-21).

Hutchison's arguments and comments reflect the influence of a general empiricism, rather than any specifically logical positivist influence. For example, his conception of laws as empirical and comparable to those of the natural sciences need not necessarily reflect logical positivist influence. As, Hutchison himself notes, both Jevons and Pareto regarded laws in this manner. Yet neither were logical positivists. And when he seeks to illustrate the influence of rationalist philosophy as reflected in the prevailing tendency to call propositions of pure theory 'laws', he refers, in a lengthy quotation, not to a logical positivist, but to Campbell (1921: 47). As we explained in the Introduction, Campbell is a realist philosopher ranged against the anti-realism that typified much of logical positivism (Harré, 1967).

Before, we turn to the next chapter topic, it should be pointed out that Hutchison is concerned not just with arguing that economic laws are more appropriately conceived as empirical generalisations, but also with refuting the claim that propositions of pure theory are about empirical matters concerning the world 'as it is', that they have empirical relevance. 'Still today the discovery of what "determines" the level of employment . . . is claimed in "circular" propositions of pure theory' (p 57). Indeed,

it is the practical, possibly disastrous consequences of such a misconception of what are really propositions of pure theory, that are a central concern of Hutchison's (1938). This was aggravated by the view that such propositions were necessary and certain. Referring to his 1938 essay, Hutchison (1997: 146) has stated: 'What I attacked were dangerously misconceived misinterpretations of propositions of pure theory as proclaiming economic "laws" possessing certainty'.

As with the subject matter and the laws of economics, so with economic predictions Hutchison seeks to show up and combat the influence of the a priori rationalist tradition. He begins his section on 'prognosis and causality' by pointing out that another way of saying that the central concern of science is to formulate empirical laws is to say that the aim of science is to formulate predictions. As Knight (1921: 14) has well said 'The aim of science is to predict the future for the purpose of making our conduct intelligent'.

As with economic laws, so with economic predictions, purely due to the *necessity* of the propositions of pure theory, we find them being interpreted as having empirical content (Robbins, 1932: 111). Hutchison points out that the inevitability of the multiplication table also gives it predictive value, but this is not a prediction which has any empirical relevance (p 65). Nevertheless, it is insisted that predictions based on propositions of pure theory are not only empirically significant, but are also the only possible kind of prediction.

> The explanation of phenomena thus detected (by statistics) if it is to serve as a basis for forecasts of the future must in every case utilise other methods than statistically observed regularities; and the observed phenomena will have to be deduced from the theoretical system independently of empirical detection (Hayek, 1933: 37).

Such a view would seem to imply that prediction is based on deduction and occurs independently of empirical investigation (p 66). But, Hutchison points out, predictions in other social sciences, and in the natural sciences, are not given in the form of propositions of pure theory. Instead they are presented as conceivably falsifiable empirical generalisations, and, where the ceteris paribus assumption is made, it does not render the proposition unfalsifiable (p 67).

Hutchison proceeds to clear up two further misconceptions about the nature of predictions in economics. First, there is the misleading distinction between qualitative and quantitative prediction. The impression seems to be that while predictions can be reached via the 'qualitative' analysis of pure theory, little can be achieved by way of quantitative prediction. Although we can predict that, in a certain situation, price will rise, we cannot predict by how much it will rise. Hutchison contends that such qualitative predictions are no more than propositions of pure theory (p 68). He accepts that quantitative prediction is limited, but this point should not be made by using a false distinction (p 68). Secondly, there is the view that prediction in economics is impossible (Morgenstern, 1928, *passim*). The idea that we can predict reflects, to use Pearson's phrase, no more than 'the all too human love of certainty'. Such a view is fundamentally misconceived since all life, including economic life depends on at least *some* degree of predictability (p 69).

The impossibility of prediction in economics is closely related to the question of causality in economics. As with laws and predictions, so too in the area of causality the propositions of pure theory have been interpreted as if they had empirical content, that is, as if they stated causal relations (p 70). Hutchison explains that he is here using the word 'cause' in its everyday sense. However, a more precise concept is needed in science (p 71). While the concept may be useful in the early stages of a scientific explanation, its vagueness is liable to lead to controversies as to whether events are 'symptoms' or 'causes' and whether they are 'superficial' or 'real' causes (p 72).

The main point that Hutchison criticises in this section is the view that prediction in economics 'depends on deduction and must be independent of "empirical detection"' (p 66). In combating this view, we want to emphasise, Hutchison does not appeal to logical positivism. Instead he turns for support to one of the members of the English historical school, Cliffe Leslie. Leslie criticised (Robert Lowe's) view that while prediction was impossible in love, war, politics, religion or morals, it was possible in economics because here one could reason deductively from 'the two ruling passions of mankind - wealth and ease' (1879: 211). Leslie points out that it is precisely because these factors - love, war, politics, religion and morals - *do* powerfully

influence the behaviour of people with regard to wealth that the scope for prediction, based on reasoning deductively solely from 'mercantile motives', is severely limited in economics. 'What would be the worth of a treatise deducing the economy of Germany from the assumption that every man is occupied solely in the acquisition of wealth . . . ?' Leslie asks rhetorically (ibid: 210). Nevertheless, despite the influences of all these various factors, there are 'regular sequences' discoverable in society and hence limited prediction is possible (ibid: 211). Thus Morgenstern's (1928) view that prediction is impossible is mistaken.

Likewise, while Hutchison's comments on causality reflect the influence of logical positivism, they do not represent any strict adherence to it. Instead, he is quite willing to make use of the everyday sense of cause in the 'early' stages of a scientific explanation. And, as we will see in the next paragraph but one, Hutchison criticises closed deductive a priori tautologous systems for not being able to incorporate 'an analysis of change or causality' and so not being able to change from analysis of a static to that of a dynamic economic system (pp 75-6).

In the last section of his chapter Hutchison turns to consider the traditional method used in constructing equilibrium systems in economics. This has been variously termed that of 'decreasing abstraction', 'successive approximations', the 'isolating' one-at-a-time, or more recently, the 'optimistic', approach (Robinson, 1932: 6). According to this procedure, we start with deductions from 'simple' assumptions (perfect competition; 'neutral' money) and then gradually make the assumptions more nearly descriptive of present economic conditions (imperfect competition; 'non-neutral' money) (p 73). Such attempts to find more realistic assumptions would seem to require empirical investigation. However, since this procedure is primarily aimed at yielding 'significant chain(s) of deductive conclusions' from assumptions, the assumptions used must continue to be chosen for 'the possibility of deducing chains of conclusions from them - rather than for their correspondence with the facts' (p 74) - and so kept 'simple' rather than made more realistic. The 'optimistic' approach thus precludes any serious empirical investigation being designed to facilitate deductive a priori tautologous analysis.

Hutchison points out that for decades attempts to replace the traditional static

equilibrium system used in economic analysis by a dynamic system have failed because so far they have consisted of closed, deductive, and a priori tautologous systems where change or causality cannot be introduced (p 75). The question of why the attempt to change from a static to a dynamic analysis presents 'almost insuperable' difficulties for the traditional 'optimistic' procedure will have to left until after the following chapter, when the postulates of economic theory will be examined (p 76).

Two of Hutchison's chapter mottoes may serve as a conclusion to his chapter. The first, by Sidgwick (Marshall's predecessor), presents the view that in the social sciences, 'even powerful intellects' are liable to lapse into tautologous and circular reasoning (p 51). The second, by Myrdal, criticises the scientific tradition which regards abstract theory as having a more important role than simply clarifying empirical issues (p 52). While this tradition may appear 'realistic' by accepting that both the 'legs' of deduction and induction are as needed in science as in walking, in practice, it confines us to a state of theoretical absolutism stating general laws in a deductive way with induction used later only to provide practical examples.[13] Here Myrdal appears to have in mind, not a priori rationalism, but the hypothetico-deductive method of practising science. In terms of this procedure, general laws are stated 'in a deductive way' and both deduction and induction are used. This motto is evidence for our theme of demonstrating Hutchison's scepticism regarding the suitability of the hypothetico-deductive method in economics, and for his support for the inductivist-SM method.

3.4 Chapter IV – The basic postulates of pure theory

In this chapter Hutchison argues that the deductive method of deriving the propositions of economic theory from a few basic assumptions, or postulates, is 'more of less useless, because no relevant 'Fundamental Assumption' can, on our present knowledge, be made' (p 118). Using this method, whatever assumptions are made about the economic behaviour of individuals, or the condition of the economic system, unless they are empirically true, simply beg the issues involved, that is, they

[13] Here we have drawn on Hutchison's p 52 quotation from Myrdal as translated by Dr Sabine Marschall.

'assume what one wants to find out' which is what actually is the behaviour of individuals and condition of the system (p 113). In order to find this out, the appropriate method is instead extensive empirical investigation of these questions (p 114). Hutchison seeks to demonstrate the inadequacy of the deductive method by explaining the problems that arise in trying to deduce the propositions of economic theory from three main postulates: those concerning expectations, rational conduct and equilibrium.

Hutchison begins by arguing that, deeply embedded in the procedure of economics, is the notion that most of it can be deduced from some 'Fundamental Assumption' about human behaviour (p 83). In one of the introductory mottoes to his chapter, he quotes Morgenstern's (1935 [1976: 169]) remark that, although the theory of general economic equilibrium is the pride of theoretical economics, 'there can be found systematically compiled neither exact nor complete statements about the assumptions underlying the theory of general equilibrium' (p 81). Despite this vagueness, the 'maximum principle' (Pigou, 1935: 4), that is, the assumption that individuals aim to maximise their returns, formulated in various ways in the history of the subject 'from the profit-seeking Ricardian business man down to the "rational" consumer balancing marginal utilities', appears to be the leading candidate for being this 'Fundamental Assumption' (cf Robinson, 1933: 241-2). But, in line with Morgenstern's observation, there is little agreement about its precise formulation, whether it is analytical, synthetic, fundamental, 'or in fact used at all' (p 84).

Despite these disagreements, Hutchison draws attention to 'one remarkable characteristic' of the various formulations of the Fundamental Assumption: they require the further assumption that the expectations of individuals are perfectly correct (p 85). The assumption that it is 'rational', 'sensible', or 'natural' to maximise returns implies that there is no problem of *how* one is to maximise returns. This is only the case if individuals have perfect foresight, and therefore know for certain how to maximise these returns (p 85). However, we know that, in principle, uncertainty is everywhere present in our world. In this case one cannot simply 'act so as to maximise one's returns' (p 86). Rather one can only act according to one's *expectations* of what conduct would result in the maximisation of returns. Furthermore, it is only under these conditions that conduct can properly be described

as 'sensible' or 'rational'. Hutchison cites Knight (1921: 268) to the effect that without uncertainty individuals would not have to deliberate over various possible ways of adjusting to changes: instead all adjustments would be automatic and mechanical. In these circumstances one can no more describe their conduct as 'rational' or 'sensible' than one could describe the parts of a mechanical model as behaving rationally or sensibly (p 88).

Hutchison explains that this is why 'the usual Theory of Value' (as formulated for example by Robinson, 1933: 15) analyses conditions of such a 'problemless mechanical nature', that is, why it is 'confined to economic tendencies where there is one definite, unambiguous, and correct answer to the question "How am I to maximise my returns?"' 'It is inapplicable where there is any uncertainty about the answer to this question - which is, in principle, always the case in the world as it is' (pp 88-9). In a footnote at this point Hutchison contends that Leslie (1879a: 229) and later institutionalists (Veblen, 1919: 227) came close to highlighting this weakness of the 'orthodox' Theory of Value. He cites Dickinson (1922: 240-46):

> When we come to the market-place we find dealers absorbed in calculations which are reasoning, discovery, invention, rather than choosing among utilities. Their desire to make the largest profit possible, within the rules of the game, is fairly constant; the problem is *how* to make it.

Leslie (1879a) criticises both the economist Nassau Senior and the politician Robert Lowe for over exaggerating the extent to which laws may be deduced from the single assumption of monetary gain - laws which will allow them to predict human conduct.

> You may know that everybody you meet between Belgrave-square and the Bank loves wealth of some sort, and money as the means of purchasing all sorts; but what can you infer from that with respect to anyone's part or conduct in either production or distribution? Can you infer that the Duke of Westminster will, or that he will not, sweep a crossing for sixpence? . . . [Likewise] you can no more predict from their love of money what prices and profits (the) young men will get in their business than from their love of fair women what fortune they will get with their wives. . . . The orthodox, a priori, or deductive system thus postulates much more than a general desire for wealth. It postulates, also, (such) full knowledge . . . (Leslie, 1879a: 227-9).

Taking up Leslie's arguments, Hutchison emphasises that the working out of the maximum principle in these circumstances needs to be fundamentally distinguished from the way it works itself out in a world of uncertainty in which money is present, a world 'where people cannot conceivably *know* or *calculate* but can only more or less vaguely *guess*, which out of many possible lines of conduct will lead to the fulfilment of the principle' (p 87).

> It may perhaps be a broadly true generalisation 'that everyone desires to obtain additional wealth with as little sacrifice as possible' [Senior, 1836: 26-8]. . . . But this tells one nothing as to how, in fact, they set about fulfilling their desires, or, dropping the assumption of perfect expectation, how even it is sensible or rational for them to do so (pp 89-90).

Hutchison goes on to point out that it has been argued that the demand for capital goods depends on (uncertain) expectations of the future yield of capital (Keynes, 1936: 141). If we accept that consumption goods take time to consume, it can likewise be argued that the demand for consumption goods depends on (uncertain) expectations of the future utility of the consumption good. Such considerations make analysis based on the fundamental assumption 'only very roughly applicable' to the demand for capital or consumption goods (p 92).

At this point, it is convenient to pause and reflect on Hutchison's argument. He is concerned with highlighting the limitations of using a deductive procedure (arguing from a few basic assumptions) in economics. The major limitation he points out is that the fundamental assumption of rational conduct, or maximising behaviour, is sensible only if expectations are assumed to be perfectly correct. In this case analysis based on maximising behaviour (such as the orthodox theory of value) is inapplicable to the real world of uncertainty. Here, it should be apparent that Hutchison is opposing an inductivist empirical approach to the fairly dominant deductivist a priori tradition in economics. This inductivist empirical approach has little to do with the logical positivist philosophy of science and instead much to do with historical and institutional economics. The thrust of the main argument is that many factors (war, love, religion, politics) besides pecuniary gain influence economic conduct, and that, while this one factor does enable some prediction of human conduct, to 'isolate a single force, even a real force and not a mere abstraction, and to call deductions from

it alone the laws of wealth, can lead only to error, and is radically unscientific' (Leslie, 1879: 212).

After concentrating so far in this chapter on the fundamental assumption of maximising behaviour and the necessity for it to be combined with the assumption of perfect expectations, Hutchison now turns his attention to the third basic postulate of pure theory: that of equilibrium. As with the fundamental assumption, Hutchison focuses on the extent to which the assumption of equilibrium is bound up together with the assumption of (perfect) expectation. He points out that there are 'various possible conditions' in which an individual or community might be in a position of rest, or equilibrium (p 100). While he does not explicitly distinguish between Ricardo's notion of equilibrium as defined by a long run uniform rate of profit and the equilibrium of supply and demand, his argument about the extent to which perfect expectation is bound up with either appears to apply to both conceptions.[14] He disagrees, however, with Hayek's (1937: 41) view that perfect expectation is a defining characteristic of equilibrium and feels that the term is 'best reserved for the optimum maximum condition whether or not the individual or community has been led to it by perfect expectation' (p 104). He notes that individuals whose expectations are perfectly correct must be in this equilibrium position (p 103).

Hutchison begins his section on expectations and equilibrium by noting that expectations have been completely omitted in most accounts of the theory of value. Yet, those who mention it are divided about its importance for equilibrium. Several writers (Knight, 1921: 197; Hicks, 1933: 445; Pigou, 1935: 76) regard the assumption of perfect expectation as necessary for equilibrium theory. On the other hand, for Morgenstern (1935) it leads to a nonsensical result, 'the very reverse of equilibrium' (p 94). Hutchison attempts to clarify this situation. The perfect expectation assumption is not meant to describe the actual behaviour of individuals under conditions of equilibrium. Instead it is meant only to explain whatever behaviour might be since we are dealing here not with human beings, but with 'conceptual automata' whose 'behaviour' has been chosen by us when we defined the equilibrium

[14] Leslie (1879a: 231) notes that the main postulates of the Ricardian theory are that 'the advantages and disadvantages of all the different occupations are known, that competition equalizes the rewards of both labour and abstinence, and that the prices of commodities therefore are determined by the respective cost of production.

situation (pp 95-6).

> It is as though one was to sketch out the plans for, or actually construct (as one could actually construct a mechanical model of a community in static equilibrium) some piece of mechanism, say a cuckoo clock, and then ask the cuckoo whether it was because it had perfect expectation of the time that it appeared exactly at each hour (p 96).

Hutchison now proceeds to argue that the assumption of perfect expectation is compatible only with equilibrium arising from conduct in perfectly competitive conditions, that is, conditions where individuals act independently, taking no account of the behaviour of others. In particular, it is logically incompatible with conduct in monopolistically competitive conditions, that is, conditions where individuals do take account of each other's behaviour (pp 98-9). In these circumstances, endowing more than one person with perfect expectation will lead to an indeterminate, nonsensical result as, for example, in a game of chess where both players know the future moves of their opponent (p 97). Likewise, in the game of Old Maid - which according to Keynes (1936: 156) describes the business of speculation - profit-maximising individuals able to adjust their own behaviour and armed with perfect expectation would simply not play when they knew it was their turn to have the Old Maid (p 98).

Hutchison points out that equilibrium has always been the core concept of economic analysis. It is contrasted with disequilibrium, a purely temporary departure from normal conditions. Since, in principle, countless other conditions are possible, the justification for the equilibrium concept being centre stage must be that actual economic conditions 'tend' towards it (p 105). Indeed, Hayek (1937: 44) contends that it is only with this assertion that economics ceases to be an exercise in pure logic and becomes an empirical science'. However, for Hutchison, neither Hayek's assumption of a tendency towards equilibrium, nor J B Clark's (1931: 279, 408-9) question-begging analogy of water in a tank finding its own level after being disturbed, are satisfactory justifications for equilibrium to be the central concept of economics. Instead, justification requires the notion of a tendency towards equilibrium to be asserted as an empirically testable proposition, and that 'equilibrium is, in practice, regularly attained' (pp 105-7).

In his next section, Hutchison explains that the principle of subjective rationality is the principle that individuals behave in the manner they *expect* will maximise their returns, that is, utility or profits (pp 109, 111). This principle, or Law of Motivation (Schlick, 1939: 31-55), however, cannot tell us what we want to know, namely, how individuals actually form their expectations and so just how they will behave (pp 111-2). In other words, it tells us nothing about the actual behaviour of consumers and entrepreneurs. It only explains how individuals will respond when questioned about their behaviour (p 114). Just as no generalisation is possible about how, in terms of 'subjective' rationality, individuals actually form their expectations, so no generalisation is possible about how, in terms of 'objective' rationality, individuals actually form their expectations (p 113). Behaviour could only be objectively rational if we had an economic science which could predict with certainty the results of our various choices. Nor can probability theory help make our decisions more objectively rational for, as Keynes (1936: 162-4) pointed out, there are simply no grounds for any sort of calculation (p 110). Hutchison concludes:

> [W]hether and to what extent people's decisions are dominated by present prices as against the whole expected future course of prices; to what extent people's economic actions are taken on the spur of the moment, or according to a detailed plan; how far people come to any particular expectation at all or act unreflectingly according to habit; to what extent people learn from past economic mistakes and disappointments . . . are questions which cannot be answered by any general 'Fundamental Assumption' or 'Principle' . . . *ultimately* all such questions can only be decided by extensive empirical investigation of each question individually (p 114).

Since the marginal revolution of 1871, attempts have been made to formulate a 'maximum principle' to apply to consumers' behaviour as business behaviour is said to be guided by Ricardo's principle that businessmen's sole concern is to maximise money profits. Attempts over the years to broaden this maximum principle to include more, and exclude fewer, types of behaviour so as to make it less obviously false, have steadily reduced its empirical content until today it is regarded as no more than a definition (pp 115-6). Individuals are simply assumed to behave 'rationally' and 'rationally' is defined as how individuals actually behave (p 116). To define economic conduct as 'rational' appears superfluous, inappropriate and misleading. It is superfluous because, if our task is to examine economic behaviour, then simply defining this behaviour as rational does not help the examination in any way. It is

inappropriate because, in the everyday sense, much behaviour is irrational, that is, it is based on incorrect and stupid expectations. Finally, it is misleading because it may seem as if we have an empirical generalisation about economic behaviour instead of simply a definition (p 117).

Hutchison concludes that, to the extent that one views as unsatisfactory the omission of uncertainty since it is this factor that calls for economic as opposed to mechanical, technical behaviour, 'the method of deduction from some "Fundamental Assumption" or "principle" of economic conduct is more or less useless, because no relevant "Fundamental Assumption" can, on our present knowledge, be made' (p 118). The view of economics as a science based on a few fundamental propositions is shown to be 'entirely inadequate'. That the deductive method seemed applicable is only due to assumptions about expectations being tacitly made. Once these are made explicit, 'there come, quite rightly, accusations of "circularity", "begging the question", and "assuming what one requires to prove"' (p 118). For a more 'realistic' analysis we need an idea of 'the more realistic assumptions on which it is to be based' (p 119). While various objections have been made to employing empirical and statistical investigations in economics, these represent 'the only possible scientific method open to one' (p 120).

Hutchison's central concern in this chapter is to show that it is necessary to assume that individuals have expectations which are perfectly correct in order for them to be able to engage in the behaviour that will, in fact, maximise their returns (the fundamental assumption of orthodox theory) and so attain the optimum maximum condition, or equilibrium. He repeats, on several occasions, that this (drastic) assumption means that analysis based on it cannot be applied to the everyday world of which uncertainty is part and parcel: it effectively 'assumes most or all economic problems out of existence' (p 162).

According to Keynes, if orthodox economics is at fault, the problem has to do with its

assumptions (p 81).[15] By highlighting the crucial dependence of orthodox theory (ie theory based on the 'fundamental assumption') on the assumption of perfect expectations, Hutchison hopes to have exposed the extent to which it deals with a fictional (a world of 'pure theory'), rather than the everyday world in which uncertainty is present. It is the 'uncertainty factor' that makes money appear 'which can be construed as a sign that uncertainty is present, or even as a measure of its amount' (p 88) - just as the assumption of perfect expectation makes it disappear (p 105). The similarity between Hutchison and Keynes on this point is striking: the uncertainty factor was one of Keynes's (1937) two major disagreements with orthodox analysis. More broadly this chapter fits in with a central theme of Hutchison's essay, which is that the a priori deductive method based on a few fundamental assumptions is inherently defective.

3.5 Chapter V - Introspection and utility

Hutchison now examines a doctrine, called by Wieser (1929: 17) the 'psychological method', which is claimed to give economics an advantage over the other sciences. It can be traced back to Senior and Cairnes. According to Senior, the premises from which economic propositions are deduced consist of

> a very few general propositions, the results of observation, or consciousness, and scarcely requiring proof, or even formal statement, which almost every man, as soon as he hears them, admits, as familiar to his thoughts, or at least included in his previous knowledge (1836: 5).

Cairnes goes further:

> The economist starts with a knowledge of ultimate causes. He is already, at the outset of his enterprise, in the position which the physicist only attains after ages of laborious research. . . . [He is] already in possession of those ultimate principles governing the phenomena which form the object of his study . . . since we possess direct knowledge . . . of causes in our consciousness of what passes in our minds (1875: 83-90).

[15] 'To get back to Keynes. He did raise the knowledge question. He denounced the fundamental assumption, you see. That never got to be a part of Keynesianism which had much effect. Shackle made this point about ignorance, but he didn't push what the methodological consequences of that are, that is, that there isn't this one postulate here from which you can deduce so much' (Hutchison, in Hart, 2002).

According to Wieser:

> We can observe natural phenomena only from outside, but ourselves from within. [The employment of this inner observation is the psychological method] which finds for us in common economic experience all the most important facts of economy. . . . It finds that certain acts take place in our consciousness with a feeling of necessity. . . . What a huge advantage for the natural scientist if the organic and inorganic world clearly informed him of its laws, and why should we neglect such assistance? (1929: 17).

This doctrine has been given 'an important anti-empirical turn' by von Mises (1933) who argues that these general propositions 'logically precede all experience and are a condition and assumption of all experience', and by von Hayek (1937: 36) who refers to them as 'a priori facts'. Of these 'ultimate principles' from which it is claimed so much can be deduced, three appear to be among the most important: the 'fundamental assumption' or maximum principle; Gossen's Law or the law of diminishing marginal utility; and the principle of scarcity. Yet none of these, Hutchison contends, yields much of significance about human behaviour pp. 133-6). Furthermore, there is no indication of how they can be empirically tested.

While, for Wieser it is the employment of inner observation, or introspection, which is the source of the advantage of the psychological method, for Hayek it appears to be the existence of 'a priori facts'. This points to a confusion between the concepts of the a priori and introspection that occurs in discussions of this method, as well as to a lack of clarity concerning the empirical content of the Fundamental Assumption. Since this confusion, as well as the notion of a fundamental difference between the methods of the natural and social sciences, appears to hinge around Wieser's distinction between inner and outer observation, an examination of this distinction is called for.

The 'psychological method' appears to be a variation of the Ricardo-Senior-Mill empirically minimalist 'ultra-deductivist' approach. The variation involves the addition of, or emphasis on, the process of introspection. By referring to the 'anti-empirical turn' given to it by Mises and Hayek, Hutchison makes clear his opposition to a priori facts and the Kantian notion of synthetic a priori knowledge. Yet, while critical of such a priorism, Hutchison demonstrates the balanced and mature nature of

his empiricism (and his divergence from at least some versions of logical positivism) by accepting that although introspection does not involve externally observed and measurable empirical facts, it is nevertheless an invaluable aspect of scientific method.

Hutchison chooses the example of the law of diminishing marginal utility to illustrate the use of introspection in the empirical procedure of discovering empirical generalisations. At the same time he illustrates how the concept of introspection differs from the a priori. Using the method of introspection, the economist notices that the marginal utility of various amounts of money declines the larger total money income. He then notices that the results of this introspective exercise are correlated with his own external behaviour. It is by external observation that he concludes that his external behaviour concerning money is generally similar to everyone else's. He assumes, or concludes from this that everyone else has the same internal experience as himself. Hutchison leaves aside how this internal assumption may conceivably tested in order to emphasise the point that, while introspection is 'invaluable and practically indispensable' in deriving the empirical law of diminishing marginal utility, it is the observation of external behaviour 'which must furnish an overwhelming part of the evidence' (pp 140, 143). The two methods are not comparable, but are rather different procedures used at different stages in the enterprise of science. 'It appears, therefore, a misuse of terms, to put it mildly . . . to say that introspection yields 'a priori facts' (p 142).

This passage raises a point to which we refer regularly in this thesis. Hutchison's acceptance of introspection differentiates his position from that of logical positivism, or at least its leading characteristics as described in Chapter One. As with his view concerning the legitimate role of electrons in the natural sciences, Hutchison again shows that he quite accepts that unobservable entities are involved at a basic level, ie that the process of deriving the finished propositions of economic science does not rest exclusively on (external) observation as implied in criticisms of his view as espousing naïve inductivism or 'ultra-empiricism' (Machlup, 1955: 7-8).

Hutchison now turns to the concepts of expectation, utility and social utility which, due to their involving subjective valuation, have been called 'elusive' (Robbins, 1934:

4) and have even had their legitimacy questioned (Reddaway, 1936: 419). The legitimacy of a concept depends on one's criterion of legitimacy and definition of the concept: 'No sign or word is, as it were, somehow stamped from birth as illegitimate, "unscientific", or nonsensical; if it *is* this, it is because of the definition we choose or refuse to give it' (p 144, cf Schlick, 1936: 339-69; Wittgenstein, 1922: 129, para 5.4732). He uses this point to show that expectation and utility are easily legitimately defined according to his criterion of testability. For example, propositions such as 'A gets utility from a good' or 'A expects a rise in prices' are clearly conceivably empirically testable (p 145). In this vein he rejects the argument that, because such concepts are used in the social sciences, there is a fundamental difference in the methods of the social and physical sciences. If such concepts cannot be defined according to his criterion, he rejects them for any science, social or physical (p 146).

> There is no question of large parts of 'welfare' economics and Public Finance 'going by the board', or suddenly collapsing before a philosophical pin-prick. For the objections to 'welfare' economics consist in taking the word 'utility' out of its everyday use, giving it some kind of scientifically unusable 'definition' . . . and then concluding that since 'welfare' economics needs this concept it must be scientifically disreputable (p 153).

Discussions of welfare, social utility and comparisons of utility are often mixed up with controversies over normative and positive propositions and sciences. But, as Schlick (1930: 14; 1939: 17) has pointed out, the distinction between normative and positive sciences is false. Hutchison proposes 'only the criterion of conceivable empirical testability' (p 154).

Hutchison displays the influence of logical positivism in arguing that the meanings and definitions we give to words is essentially arbitrary in that they are chosen by us. In particular we can choose to define words or concepts in terms of conceivably empirically testable propositions or we can choose not to do so. Yet, in adopting Schlick's notion that the distinction between normative and positive science is false, Hutchison departed from the bulk of logical positivist thought which, as we saw in Chapter One, regarded the positive-normative distinction as basic to the development of an objective science.

3.6 Chapter VI - Conclusion

After presenting the main conclusions of his book, Hutchison lists 'even more tentatively' two further conclusions (p 163). Firstly, throughout his book he has been implying that scientific knowledge, aside from logic and mathematics, must be based on empirical regularities or generalisations. Progress in economics depends on discovering such empirical regularities, rather than on deductive analysis. In economics these will be regularities about how people actually behave. Although economic problems may be formulated in terms of wages, money, interest, etc, they are reducible to problems 'as to how people behave'. Yet 'Equilibrium Economics describes a community without economic problems because *so far as it affects him* everybody knows how everyone else is going to behave' (p 164, original emphasis).

Secondly, for some time now scientists have not claimed any certainty for their conclusions and have instead justified their work in terms of its practical usefulness. If economists are to follow suit, their conclusions should be extended to include political and sociological factors. For, following Weber (1922: 168-9), every policy problem involves both economic and political factors. Purely economic, or purely political, advice is equally to be condemned and is of academic interest only. It is noteworthy that great equilibrium economists, for example Pareto and Wieser, and great historical economists, for example Weber, have viewed their economic analysis as a prelude to 'wider sociological investigations'. It has been shown earlier that the 'optimistic' procedure of starting with extremely simplified assumptions, later replacing them with more 'realistic' ones, leads nowhere and that to progress empirical investigation is needed more or less from the beginning (p 166). Much the same can be said of the attempt to simplify social phenomena by separating them into political and economic spheres. 'Exclusively "economic" conclusions are vitiated by the same neglect of relevant factors as is "static" economic analysis' (p 166). Policy advice will prove helpful only if it is informed by both economic and political factors.

Hutchison concludes by applying his analysis and distinctions to the controversies surrounding the current problem of the trade cycle. Such a discussion forms an apt conclusion, he says, since these controversies reflect basic methodological disagreement, that is, disagreement about the fundamental procedure and assumptions

by which to mount any economic analysis. According to Hutchison, scientific disagreement can be only of two kinds and can always be removed. First, disagreement can be verbal. Currently trade cycle problems are discussed in a number of different languages, or 'sets of concepts'. Since concepts peculiar to one language, and hence not fully translatable between languages, are used to discuss critical aspects, it would seem to follow that much apparent disagreement could be reduced if economists came closer to employing one common language, or standard terminology (pp 167-9). Second, disagreement can be factual. Such disagreement has generally not been taken seriously in economics since factual questions have been regarded as no more than data or assumptions, not as questions of *science* (p 170). Consequently, not much more is expected of economic propositions other than that they be 'plausible' and 'tractable'. The 'optimistic' procedure can be relied upon to address any 'realistic' shortcomings of these propositions.

According to Hutchison, the fundamental disagreements surrounding the trade cycle are due to 'basic questions of fact' (p 170). He reminds the reader that, as he tried to show in his chapter four, such questions cannot be answered by deduction from some fundamental assumption. In cases where questions of fact are 'answerable by existing statistics', there is no room for further disagreement. In cases where it is not possible to obtain statistics, disagreement may remain and then the 'inconclusiveness of the position must be admitted' (p 171). Consequently, as far as the problem of the trade cycle goes, the economic scientist must stick to the facts and attempt to obtain 'regularities and correlations in the facts' (p 173). To venture beyond this will probably involve putting forward propositions which cannot conceivably be empirically tested. To obtain agreement on the problems of the trade cycle, the following is necessary: (1) a unified terminology; (2) statistical investigation of empirical fact; (3) avoidance of arguments not supported by statistical evidence. This conclusion is similar to that of Malthus (1820: 8) for whom disagreement in economics stemmed from a 'tendency to premature generalization' that militated against the willingness to submit theories to 'the test of experience' (p 174).

In this chapter Hutchison lists two final conclusions. First, progress in economic knowledge depends on discovering empirical regularities rather than the 'optimistic' procedure whose dependence on the assumption of perfect expectations has resulted

in it assuming 'most or all economic problems out of existence' (p 162). The empirical regularities that can be discovered will be 'very qualified and far from universal' (p 163). Here Hutchison nails his empiricist colours firmly to the mast: there are no essential relations between matters of fact. But, in doing so, he views himself as following in the British empirical tradition in the history of the methodological writings of leading economists of the past (notably Smith, Malthus, Jevons, and especially Marshall), rather than particularly attempting to introduce logical positivism into economic methodology. Indeed, to the extent that Hutchison accepts 'basic questions of facts' as unproblematic, he differs from logical positivists who, for instance, enter into (philosophical) discussions concerning the subjective and objective foundations of facts, as in the protocol sentence debate.

We would go further and suggest that Hutchison's interest in methodology and philosophy is far from arising from, or being concerned with and limited to, philosophical issues, eg the rationalist-empiricist debate. Rather, the goal that motivated Hutchison in 1938 was that of transforming economics into a practically useful subject able to offer helpful policy advice. It is in line with this goal that the second conclusion listed in his final chapter is that, for policy advice to be useful, it must involve both economic and political factors. Here two remarks are in order. First, here again, Hutchison departs from logical positivism, and more particularly from the view that economics can, and must be, a positive science yielding objective advice free from normative political considerations (Friedman, 1953; Lipsey, 1963). While this might be said to reflect no more than Schlick's (1930) views, the influence of economic methodology is also evident, for apart from Weber (1922), he cites (p 56) from Myrdal's *Political Element in the Development of Economic Theory* (1931).

Secondly, it appears that the inductivist-empirical approach is more suited to engendering fruitful, practical results since it can so easily be extended to 'wider sociological investigations' needed for integrated policy advice than the 'optimistic' deductive procedure. Given this background, it is an understatement to say that Machlup (1955) quoted Hutchison out of context when he cited Hutchison's call, in this section, for beginning with 'extensive empirical investigation'. Here Hutchison is not calling for some naïve, or Machlupian 'ultra-empiricism' ie beginning with empirical investigation. Rather he is pointing to the need for 'wider sociological

investigations' necessary to include political and not purely economic factors in formulating fruitful policy advice. This is apparent in the very next sentence which Machlup fails to quote. Referring to his contrast between the 'isolated' abstractions of the deductive approach and that of 'extensive empirical investigation', Hutchison states:

> It is the same with attempts to simplify inextricably interconnected social phenomena by 'isolating' them in watertight 'political' and 'economic' departments. Exclusively 'economic' conclusions are vitiated by the same neglect of relevant factors as is 'static' economic analysis (p 166).

Whereas the crucial need for economics to be a policy-oriented discipline was not brought out clearly in his 1938 work, this aspect constitutes a key theme sustained in his later works (1964, 1968, 1992, 1994). In this connection it should be remembered that, against the background of the Great Depression, Hutchison gave up a classical (academic) training at Cambridge in order to switch to the newer, but more practically-oriented, subject of economics (Tribe, 1997; Hart, 2002).

Appendix: Some postulates of economic liberalism

According to the classical economists, economic science demonstrates that a *laissez-faire*, rather than a collectively planned, policy leads to the greatest possible returns. Hutchison calls this the doctrine of Economic Liberalism (p 177). He examines how this doctrine came to be held in order to further clarify his criticisms of economic theory and illustrate the general arguments of his essay. Hutchison argues that a large part of the reason as to why it came to be held was due to the assumption of perfect expectations being implicitly, rather than explicitly, made. For instance, von Mises (1927) contends that, in a capitalist economy, costs and revenues of various projects can easily be calculated to determine the most profitable project in contrast to the situation in the planning authority which could not begin to solve the problem. This, Hutchison complains, is a 'conspicuous example of assuming precisely what one has to prove' (p 182). It seems as if Liberals have been trying a priori to demonstrate some 'inner contradiction' in collectivist planning and have been supported in this endeavour by their methodology. 'It must always be remembered that *laissez-faire* and equilibrium doctrines had their origin in *rationalistic Utopia-building*' (p 184,

original emphasis). Since the assumptions of economic theory have often been ambiguous, the doctrines pertaining to this ideal world have become confused with the way the actual world works. Such theories need to be seen clearly as no more than Utopian constructions.

Hutchison is not in favour of planning and against *laissez-faire*. He is pointing out that, if one is to have a scientific evaluation of the two systems, the a priori approach suffers from two weaknesses. First, it facilitates prejudices (in this case of the 'old capitalism-socialism controversies') to be aired as 'confident generalisations' which have a ring of scientific authority about them. Secondly, the issue of planning versus *laissez-faire* is too complex an issue to be decided via the sweeping assumptions of the a priori approach. Instead a scientific evaluation of the issue requires detailed empirical investigation.

Conclusion

In conclusion we emphasise four points in Hutchison's 1938 essay. The first is one that most concerns Hutchison's relationship to logical positivism and Popper. In his chapter one Hutchison regards the distinction between scientific and philosophical problems as of 'fundamental importance for our discussion' - noting that 'these vague and highly ambiguous adjectives are being used here in a sharply and clearly separable sense' (p 6). It is this distinction, he argues, that is vital to making progress in economic investigation. This distinction is made in two stages. In his chapter one he puts forward his principle or criterion of testability: the 'finished propositions' of a science must be testable (p 9). Given that Hutchison is here distinguishing science from non-science, and that testability implies falsifiability, Hutchison hereby introduces Popper's falsifiability criterion to economics.

In his chapter two Hutchison turns to the second stage of his distinction between scientific and philosophical problems. His purpose is to clarify the meaning of the type of proposition of which the pure theory (as opposed to the applied theory) of economics consists. Propositions of this kind are analytic and deductive and follow with logical necessity if true. They are to be distinguished from empirical synthetic propositions which are inductively inferred and therefore may conceivably be falsified

(cf his principle of testability). One of Hutchison's examples of this kind of propositions is: 'If the clouds are grey it is going to rain'. In proposing that all propositions which have scientific sense consist exclusively of these two kinds of propositions, Hutchison adopts a logical positivist principle which appears to be modified by a reading of Popper (1934). The modification concerns Hutchison's qualifying the term 'sense' or 'meaning' by the term 'scientific' which he, still, as in his chapter one, puts in inverted commas. Thus, rather than implying that these two kinds of propositions are the only two meaningful kinds (the logical positivist notion), his 'exhaustive twofold classification' implies that 'science' is comprised of only these two kinds (closer to Popper's notion). This conforms to his view of the nature of science (p 9).

The second point is Hutchison's scepticism concerning the extent to which deductive, and his emphasis on the extent to which inductive, reasoning can be useful in science. This, in turn, reflects the extent to which he follows the inductivism-SM, rather than the hypothetico-deductivist, approach to scientific method. More recently, concerning the necessity for economists to induct from trends, Hutchison has emphasised:

> In this connection, it should be insisted that, as regards economics and the social sciences, the rejection or neglect of induction by strict hypothetical deductivists (like Popper and Hayek) also tends towards obscurantism by insisting on excluding a method not used in physics, even when the material of economics requires induction if the aims and problems of the subject are to be tackled (1992: 57).

In his second, third and fifth chapters Hutchison criticises various methods used in economic theorising for leaning too much towards the a priori deductivist and hypothetico-deductivist approaches to scientific method: the hypothetical method, the 'optimistic' approach, and the 'psychological' method. The hypothetical method is the most deductivist and a priori leaning. While introspection in the 'psychological method' is an added advantage in social as compared to the natural sciences, and is 'invaluable and in fact practically indispensable method' in suggesting hypotheses to be observed, it cannot of its own yield any 'finished propositions' of science: it cannot yield 'a priori facts' (p 142).

Hutchison reserves most of his criticism for the 'optimistic' approach, or traditional

method used by Clark, Marshall, Pareto, Wieser, and Wicksell in developing 'equilibrium economics'. It appears to be more realistic by accepting that both the legs of deduction and induction are as needed in science as in walking. Yet in practice it confines the analysis to a state of theoretical absolutism stating general laws in a deductive way with induction used only later to provide practical examples.[16] 'The postulates of the equilibrium system were specially chosen for their "tractability" . . . rather than for their correspondence with the facts: that is the essence of the optimistic procedure' (p 74). When Hutchison points to this tractability as making possible 'a fascinating display of mathematical or geometrical ingenuity' (p 120), he is putting forward one of his first criticisms of formalism in economics (Hutchison, 2000). In his fourth chapter it is the optimistic approach that he implicitly criticises. He does this by showing how difficult it is to formulate any basic postulates from which much of significance for economic behaviour may be deduced. That this equilibrium economics appears adequate is only because the basic postulates on which it depends tacitly involve the further assumption that people's expectations are perfectly correct. But in this case, we are dealing with a *Schlaraffenland* in which there is no economic problem.

The third and fourth points are more easily summarised. The third concerns an advantage of inductivism-SM (apart from its being a surer route to scientific knowledge than the hypothetico-deductive method) for Hutchison's approach to investigation in economics. This is that, by working 'more or less from the beginning with extensive empirical investigation', this method is better suited to the fact that social phenomena are 'inextricably interconnected' (p 166). It thereby fits in more naturally with, and is able to draw more easily on, the historical, institutional, sociological and political aspects of economics (Leslie, 1888; Weber, 1922; Pareto, 1935). This in turn is important for Hutchison for it means that the results of such investigations are more likely to yield policy advice which is of practical use in solving particular economic problems. From the introduction to this chapter, it will be remembered that the desire to further economics as a science able to dispense fruitful policy advice was one of Hutchison's motivations for switching from classics to economics. The fourth point concerns Hutchison's views on the realist status of

[16] Hutchison draws on this criticism of Myrdal (1933) in one of the mottoes to his chapter three (p 52). The motto translation is by Dr. Sabine Marschall.

scientific theories and laws. Here it is difficult to classify him as adhering particularly to any one realist or anti-realist view. In parts of the essay, and to the extent that he draws on logical positivism, he leans towards anti-realist views such as nominalism, conventionalism, instrumentalism and pragmatism (cf Torr, 1999). Yet, this is always in a cautious, qualified manner. For example, his 'conventionalism' is closest to that of Poincaré's *commodisme*. In other parts of his essay he leans towards the realism of Popper's fallibilistic approach and that of Campbell (1921).

Apart from these four points, our main conclusion is that the popular consensus that Hutchison (1938) served as the vehicle for the introduction of positivism into economics in something like the way Ayer (1936) introduced positivism to the English-speaking world, is largely mistaken. While logical positivism did indeed play a significant role in his essay, we have shown that so did other philosophies of science. For example, Hutchison quotes extensively in his chapter mottoes from Poincaré. At least as important as the part played by logical positivism is the extent to which his essay is informed and influenced by the methodological views of the leading economists dating back to Ricardo and Malthus. In this respect it is significant that Hutchison's closing passage is a quotation from Malthus (1820: 8) in which the clergyman bemoans the 'tendency to premature generalization' as one of the 'principal causes of error' in economics since this exacerbates the unwillingness to submit theories to 'the test of experience'. Over a century later Hutchison (1938) repeated this basic message.

CHAPTER 4

THE INFLUENCE OF HUTCHISON'S INTERVENTION: KNIGHT

The question of the influence of Hutchison's (1938) essay on economics begs the wider question of whether ideas, contained in books or otherwise, can or do influence the world of events. Keynes (1936) famously contended in his closing pages that it is the ideas (of economists and political philosophers) rather than vested interests that rule the world so supporting the view that the direction of influence runs from economic thought to policies and the real world. In terms of Marxist dialectical materialism, the influence runs in exactly the opposite direction with the material relationships determining the superstructure of ideas and ideology. With regard to Keynes's view, Robbins (1963) finds evidence in the effect of the English classical economists on the policy of free trade, in that of Marx's theories of history and in Keynes's influence on policy. However, Hutchison has voiced scepticism with regard to Keynes's generalisation:

> Roughly simultaneously, a new dawn gradually begins to break, cocks crow, and people get out of bed. Claims by the birds that 'the world is ruled by little else' than their crowing should be treated with reserve, without denying them any influence whatsoever, on particular occasions, on the course of events (1978: 282).

In this vein Hutchison holds that it is vital to distinguish between 'the different modes or channels by which economists may influence policy . . . and to distinguish between the political and economic content in what economists write, as well as between very different types of economic propositions' (1978: 283). In good empiricist fashion he concludes that it is unlikely that there is any 'short and simple' argument about the influence of theory that is valid. Instead each particular case must be examined individually.

Keynes's 1936 *General Theory* does appear to support the view that ideas can and do

influence policies and the real world. Keynes's book precipitated a policy revolution, if not a theoretical revolution. Textbook writers such as Harris (1947), Klein (1947), Dillard (1948), Samuelson (1948), and Hansen (1953) successfully popularised his ideas and led to the post-second-world-war generation in America being brought up on Keynesian macroeconomics. Keynesianism dominated economic policy for the two post-war decades (Canterbery, 1995: 179).

While in the case of Keynes the channels of influence seem clear, in the case of Hutchison things are quite different. Keynes had textbook writers popularising his *General Theory* and governments implementing his conclusions in their economic policies. Hutchison had none of this. The direct effects of the influence of Hutchison's ideas between 1938-1963 are instead limited mainly to the various debates in the economics journals of the period. These will be the topic of the remainder of this, and the following chapters.

The indirect effects of his ideas are a more difficult matter to discern. Here we must look at the extent to which his main methodological propositions were taken up by the economics profession. Caldwell (1982: 115) lists three propositions as being put forward by Hutchison: (1) that economists should search for 'conceivably falsifiable, though not practically falsified' empirical generalisations; (2) that various aspects of economic behaviour be empirically investigated; (3) and that economists abandon the psychological method or method of introspection as a means for evaluating or justifying their theories.

> These principles and practices advocated by Hutchison were to meet with nearly universal approval among economists in succeeding decades. Why were economists so eager to embrace the tenets of positivism? (ibid: 115)

Caldwell proceeds to answer this question:

> The movement toward positivism was not the result of any expressly methodological treatises; Hutchison's book did not cause the mathematization and quantification of economic theory. Methodological works taken alone seldom change the minds of readers, their purpose instead is to confirm changes that are already in motion (ibid: 115).

Caldwell ascribes the post-war changes in economics to three main factors. First were 'purely technical' factors. These involved the increased collection of statistics; the development of linear programming and progress in statistics and econometrics; and the mathematisation of consumer theory. Second were conceptual developments in macroeconomics, welfare economics and growth theories. Third, those opposed to the new post-war interventionist Keynesian policies clung to outdated notions such as subjectivism, introspection, and a priori true synthetic statements. For their arguments to be taken seriously, they needed to adopt positivist quantification and testing.

To go into the changes in theory and technique in more detail would take us too far into the realm of the history of the economics of this period – 'a matter on which discord rather than consensus currently seems to be growing' (ibid: 116). In this regard, Caldwell cites Shackle (1967), Deane (1978) and Hutchison (1978, ch 9).

It is no easy matter to demonstrate on the one hand the extent to which the various changes that took place in economics between 1938 and 1963 arose from the (indirect) influence of Hutchison (1938), and on the other the extent to which they were the result of factors such as those outlined by Caldwell in the paragraph above.

Consequently, in assessing Hutchison's influence in the period 1938-1963, we will limit ourselves to an examination of his influence as directly reflected and documented in the various debates in the journals of this period. While we will cast our net amongst the journals as widely as possible, the discussion will focus on three major debates. In this chapter, chapter four, we will focus on Hutchison's pre-war debate with Knight. In the next two chapters we will focus on his post-war debates with Machlup (Chapter Five) and Klappholz and Agassi (Chapter Six).

The question of the influence, both direct and indirect, of Hutchison's 1938 intervention is closely bound up with the issue of how his intervention was interpreted. Consequently, for the remainder of this introduction, we will consider this topic. In line with our thesis we argue that Hutchison's 1938 intervention is correctly interpreted as an attempt to redirect the nature of economic investigation

away from the a prioristic direction that Robbins (1932, 1935) was urging and towards a more empirical approach. Hutchison in 1938 was chiefly concerned with countering the then recent rationalist resurgence (Robbins, 1932, 1935). In this regard, Latsis (1972: 234 ff) correctly interprets Hutchison's intervention as advocating the adoption of a general methodological approach - although Hutchison himself claimed only to show the consequences of adopting certain key methodological distinctions (1938: 12).

Against the above, Knight (1940) and Machlup (1955) appear to have interpreted Hutchison's intervention not so much as proposing a general methodological approach, but rather more as an attack on the neoclassical programme - in particular on the theory of the firm - using methodological arguments. It is this latter (and incorrect) interpretation that, it will be argued, lies behind the polemical and furious response of Knight (1940) and behind the attempt by Machlup (1955) to position Hutchison on an extremist methodological fringe. Our argument, following Latsis (1972: 234), is that their chief goal was to defend the neoclassical theory of the firm rather than simply debate methodological issues with Hutchison. In the process, Hutchison's methodological position was misrepresented and the chance to steer economics towards embracing a more even mix of empiricism and rationalism was lost.

While Latsis correctly interprets Hutchison as concerned with the implications of a particular methodological and philosophical position, his further contention that Hutchison's main interest 'was philosophical, external to economics proper' is misleading (1972: 234). Hutchison was certainly interested in philosophy - witness his early reading of Wittgenstein (1922) - but his switch after his first year at Cambridge was from classics to economics, not to philosophy. Given this particular switch, it appears reasonable to argue that Hutchison was motivated by questions of immediate practical relevance, in particular those relating to the poverty and unemployment of the then current Great Depression. His switch also appeared to be in line with the 'social enthusiasm' of Marshall and Pigou. This 'social enthusiasm' rather than philosophy *per se* appears a better contender for capturing Hutchison's main interest. Such a view helps explain why he reacted strongly to the complacency

of his academic tutors, and in particular, to Robinson's (1932) reversal of the Baconian dictum when she argued that economists should be concerned with 'light-producing' rather than 'fruit-producing' investigations.

Hutchison viewed this reversal with alarm for he saw himself as belonging to the empiricist tradition in philosophy (Hutchison, 1941: 735). Nevertheless, contrary to Latsis, Hutchison's main interest was not philosophical. It would thus be wrong to view Hutchison (1938) as a work in the same vein as Ayer (1936), but directed specifically at economists. Rather, Hutchison's philosophical concerns were secondary to (or at least always closely intertwined with) a type of Marshallian and Pigovian 'social enthusiasm'.

It is this background that needs to be brought to bear on Latsis's (1972: 234) clarifying distinction which will be applied, subject to modification, to our discussion for the remainder of this chapter and the next. With respect to the discussion of economic methodology since the 1930s, especially as it relates to the theory of the firm, Latsis views the 'most interesting conflict' as that 'between a Popper-inspired criterion directed against different versions of the neoclassical programme on the one hand, and the apologetic defence of that programme on the other' (ibid). In the Popper-inspired camp 'the most influential figure' was Hutchison. In the apologists' camp were arraigned Knight, Mises, Robbins and, somewhat later, Friedman and Machlup. This, we hold, is the perspective from which the debates between Hutchison and Knight, and later with Machlup need to be viewed. While the issues in these debates appear to be concerned with methodological and philosophical questions, the motivating factor, we argue, for both Knight and Machlup (and Friedman) was their quest to defend orthodoxy, particularly the theory of the firm. In this quest Knight drew, mainly, but not only, upon a priorism, Friedman (1953) upon instrumentalism, and Machlup (1955) upon conventionalism.

Before adopting Latsis's distinction as central to our organising framework for this and the next chapter, it is in need of some modification. Latsis correctly describes Hutchison's interest in wanting to establish a general criterion to distinguish scientific from non-scientific statements in order to apply it to economics 'come what may'.

Latsis, however, also implies that Hutchison sitting writing his book in Nazi Germany in the late 1930s, was on the look out for a criterion to distinguish scientific from pseudoscientific statements when 'he came across' Popper (1934) 'and discovered in Popper's demarcation criterion the weapon he wanted' (1972: 235). If we are to interpret Hutchison's influence properly, we need to understand that the process of discovery was a good deal more complicated than Latsis would appear to suggest, and, more importantly, that, while Hutchison did discover Popper's criterion, this discovery constituted an important input, but not the only input into his own criterion, namely, the Principle of Testability.

4.1 The journal reviews of Hutchison's intervention

It was already in these early reviews that Hutchison was labeled a positivist. Yet, as we have argued, positivism is not central to Hutchison's methodological position. What is central instead is a thorough-going inductivist-empiricism. Hutchison drew on positivism to a certain extent, which was to be expected, since it was then the latest form of empiricism. Hutchison's empiricism led him to be critical of orthodox economics because much of it was interpreted as propositions arrived at by deduction from self-evident a priori assumptions. Intrinsic to Hutchison's interventions in the journal debates was this fundamental criticism. Yet Hutchison was not attacking and dismissing all orthodox economics. There were those parts which were empirically based and those parts which, while stated in a non-empirical form, could be re-cast in an empirical form.

By contrast Knight, and more so Friedman and Machlup, were concerned with defending orthodoxy. The irrelevance, or unimportance, of viewing Hutchison as a positivist is seen in the following state of affairs. Knight regarded Hutchison (1938) as a positivist attack on orthodoxy. Yet Friedman's 'positivist' (1953) is regarded as a classic defence of orthodoxy! To focus on Hutchison's positivism is to risk misunderstanding the influence of Hutchison as, not only critical of orthodoxy, but as presenting an alternative inductivist-empiricist methodology for economics. The influence of Hutchison's 'positivism' was paradoxically transformed via Friedman and Machlup into a defence of orthodoxy. In these terms, Hutchison's (1938) attack

appears to have had little influence. From being a potentially dangerous force, positivism is brought to the defence of the orthodox camp. Moreover, Hutchison gets blamed earlier for a position which is not central to him while Friedman gets credit for introducing positivism. The misinterpretation of Hutchison (as a positivist) results in his empirical-historical stance not being understood, with even less chance of it being influential.

The first economist to label Hutchison's intervention 'positivist' appears to be Stonier (1938) whose review in the *Economic Journal* has been described by Latsis as 'briefly contemptuous' (1972: 235, n 6). Stonier acknowledges Hutchison's 'extremely wide and accurate' knowledge of economic theory. He finds Hutchison's distinction between 'analytical deduction' and 'synthetic assertion' helpful. For example, it clarifies whether a particular version of the quantity theory of money is to be regarded either as true by definition or as a synthetic, verifiable statement. In this respect economists 'can learn much from Mr. Hutchison's book'. But, in general, Stonier criticises Hutchison (1938) for being 'an application of logical positivism to economic theory'.

According to Stonier, the problem with logical positivism is that it is 'anti-metaphysicalism'. This explains why Hutchison seeks to deny, or at least limit, the extent to which science depends upon metaphysical assumptions. Logical positivism ignores the relation between subject and object and so has no method of self-criticism. The problem that emerges concerns the meaning of verifiability. Stonier notes that, while Hutchison rejects behaviourism, he appears to argue that all economic theory can be tested by statistics, and that statistics

> are descriptions wholly from 'outside' of purely physical facts. This is not satisfactory, since all economic statistics imply some economic theory, and, on the other hand, some theory is subject only to introspective or intrapersonal tests (Stonier, 1938: 115).

Stonier misrepresents Hutchison's position. As we saw in Chapter Three, logical positivism is not of central importance to Hutchison. Rather, he draws on it to support a more broadly-based inductive-empiricism. He does not ignore the relation

between subject and object and indeed, argues that introspection is 'an invaluable and in fact practically indispensable method' in science (Hutchison, 1938: 143). His argument is rather that we cannot get anywhere in an empirical science by introspection alone. He accepts that theory plays a vital role in organising the facts, but would probably differ in his interpretation of theory from that of Stonier.

Stonier's labeling of Hutchison appears to have started the image of 'Hutchison the positivist'. Unfortunately, this interpretation forestalled the extent to which his more broadly-based inductive-empiricism may have influenced economists and economics.

Writing in the *American Economic Review*, Whittaker (1940) also interprets Hutchison as a positivist in philosophy (and a neoclassicist in economics). Yet, he argues, Hutchison attempts little justification of these positions. Similarly Hutchison argues that the goal of science is to discover empirical generalisations, or laws properly so called. Yet, he appears not to recognise the substantial difficulties involved.

From the discussion of Chapter Three, it appears misleading to describe Hutchison as a neoclassicist in economics. Hutchison's argument is that, to the extent that the propositions of neoclassical economics are interpreted as propositions arrived at by purely a priori deductive reasoning, and hence empirically untestable, it is not scientific. To the extent that propositions are formulated as propositions inductively arrived at, it is scientific. For Hutchison there are a number of such propositions: eg, the law of diminishing returns, the law of diminishing marginal utility or Gossen's law, Gresham's and Pareto's law (1938: 60, 64, 134, 135). Yet Hutchison also supports the non-neoclassical empirical-historical approach of Leslie (1888) and institutionalist economics of Mitchell (1928) - (see Hutchison, 1938, *passim*; 1998: 82-3). His position defies easy pigeon-holing.

Whittaker (1940) claims that many economists already accepted that the goal of science is to discover empirical generalisations. They did not need Hutchison to tell them. From our discussion of Chapter Two it would seem that Whittaker neglects the influence of Knight in America. Furthermore, as we have seen, in Europe and

England, Robbins (1935) and Mises (1934) were influential. Moreover, given the growth of formalism in economics, it appears that Whittaker's 'many economists' were not that concerned with discovering empirical generalisations. Hutchison's cautions about the need for economists to 'extend the range of their conclusions to include political and sociological factors' (1938: 165), and his call to notice that 'several great economists . . . have treated their work on Economics as essentially a preliminary to wider sociological investigations', were largely ignored (1938: 166).

It is unfortunate that the two reviews discussed above were largely negative since, being in the *Economic Journal* and the *American Economic Review*, they were highly influential. While the next two reviews were more positive, they were in less influential journals.

Shearer (1939), writing in the Australian *Economic Record*, describes Hutchison's 1938 as 'a patient and careful survey' of the foundations of static equilibrium theory. Hutchison's survey points to two conclusions. Firstly, propositions of 'pure theory' are put forward as being laws applicable to the real world. Yet they are not conceivably empirically falsifiable. In the past economic laws had empirical relevance because they were founded on 'realistic assumptions'. However, continuous refinement of these laws so as to facilitate deductive reasoning (by using the isolating procedure) has transformed them into formal propositions. Secondly, generalisations about economic behaviour should be derived by inductive reasoning, especially in the light of uncertainty in the world in which we live.

Shearer chides Hutchison for failing to tackle the problems of verification or to provide concrete suggestions concerning his call for more empirical analysis of economic behaviour. Instead, his discussion is 'too abstract and general' and is directed more towards the problems and status of theory in economics.

While Shearer presents a reasonably balanced summary of Hutchison's book, he appears to have overlooked Hutchison's (1938: 9) statement of the aims of his work. Here Hutchison explains that the scientist uses both 'empirical investigation and logical analysis'. 'It is this latter activity which it is largely the purpose of this book

to analyse and itself carry out' (ibid: 9). Hutchison can hardly be criticised for carrying out what he intended to do. At the same time, Shearer's criticism of Hutchison as 'too abstract and general' contrasts with the claims of those who have criticised Hutchison for underplaying the importance of theory. It also draws attention to the limitations of Stonier's depiction of Hutchison's (1938) as naïvely empiricist. However, it must be acknowledged that Hutchison's concern with 'logical analysis' is a clear instance of the influence of logical positivist thinking.

Shearer appears to be one of the few economists at the time who recognised the priority Hutchison accorded to generalisations about economic behaviour being derived by inductive reasoning, especially in the light of uncertainty in the real world. This is in tune with our interpretation of Hutchison in Chapter Three. It is one of the main aspects that significantly differentiates his position from being an 'application of logical positivism', as Stonier would have it. However, the influence of these views of Hutchison's remained lost behind the positivist portrait by which he came to be represented.

Dobb (1942) points to the resurgence 'in the last few years' of criticism of orthodox economics. Such criticism has ranged from general criticisms such as Hogben (1938) and Wootton (1938) to those of 'the Keynes school' concerning the validity of the fundamental assumptions of 'classical economics'. But, he argues, the most significant has been that of Hutchison (1938).

He describes Hutchison's essay as a logical positivist criticism of the 'language-habits of economists' (Dobb, 1942: 390). It is directed at those economists who have claimed that their science deduces a priori economic laws of universal necessity. Its alarming conclusion is that most economic principles are tautologies devoid of empirical content and hence any power of prediction.

According to Dobb, much of what Hutchison is saying compares with American institutionalism. Yet it is more radical for it implies that the method of deductive reasoning can yield little or nothing of value. For Dobb, it is quite clear that there is a role for general reasoning in economics. For example, conclusions about specific

features of capitalism can be deduced from its general character. Hutchison's error lies in rejecting the method of deductive reasoning rather than criticising its misuse. This means, for example, that he is unable to discriminate between Mises's and Marx's conceptions of economic reality (Dobb, 1942: 393).

Dobb traces Hutchison's error to his exhaustive classification of propositions into either verbal or empirical statements. He says he cannot understand how any set of propositions can be purely verbal, particularly those used in ordinary conversation and economics. It follows from the context of such statements that they are primarily about real world events. Controversy in economics is never simply about words, but about different interpretations of capitalism. In this case the crucial question does not turn on whether or not deductive or inductive methods of reasoning are used, but whether or not they give a picture of relations that hold in the real world. 'Our main quarrel, in other words, is not with a method . . . but with a false use of that method, creating a distorted picture of contemporary society' (Dobb, 1942: 395).

While Dobb makes some worthwhile points, his view of the 'relations that hold in the real world' differs sharply from neoclassical orthodoxy. Dobb's criticism of Hutchison relates to the fact that Hutchison does not share Dobb's Marxian conception of reality. Apart from this, Dobb appears to misunderstand Hutchison's twofold classification of propositions, for that classification relates to those propositions which have scientific sense, not those used in ordinary conversation. Furthermore, it appears perfectly possible for propositions to be 'purely verbal', the classic example being definitions, to which Dobb himself refers. Hutchison would no doubt ask Dobb how he could tell when the method of reasoning gave a picture of relations that hold in the real world. Hutchison's answer to this question is, of course clear. The proposition resulting from which ever method of reasoning would have to be conceivably testable and would have to be shown at least to be not empirically false. But, importantly for Hutchison, this would only show that it held tentatively. Following Hume's empiricism, at any moment in the future it is possible that it may not hold. For Hutchison, the problem is not one of avoiding a distorted picture of contemporary society, but the very existence and meaning of a conception of reality as general as Mises's or Marx's.

While Hutchison may be judged as undervaluing general reasoning and deductive methods, given his empiricist stance he would no doubt view it as vital to keep general reasoning on a short leash lest one is tempted into rationalist speculation. Nevertheless Dobb does point to a common criticism of Hutchison, namely, what is seen as his tendency to underplay the role of general reasoning and the deductive method. Furthermore, he draws attention to an important factor affecting Hutchison's influence on the practice of economics. This is the extent to which Hutchison's (1938) has wrongly been interpreted as primarily a criticism of orthodox economics when, in fact it is, rather the (careful) criticism of the a priori deductive method which, from Hutchison's point of view, pervaded too much of economics, especially when he wrote in the 1930s.

4.2 Knight's 1940 reaction

By far the most significant response to Hutchison's essay was that of Knight's. Described by Coats (1983: 18) as an 'outspokenly hostile diatribe', it nevertheless provided spectacular publicity for Hutchison. A few introductory remarks about Knight's position may help in understanding Knight's furious outburst. Given the complexity of his position (Chapter Two; Hammond, 1991; Emmett, 1998), we confine ourselves to two points. First, Latsis (1972: 235, n 5) draws attention to the extent to which Knight (1940) was influenced by hermeneutic philosophy, with several of its technical terms scattered throughout his review. Second, Hutchison has argued that 'over his long career, Knight's views on the philosophy and methodology of economics completely boxed the compass, from crude, extreme scientism, to crude extreme anti-scientism' (1997: 148). Blaug maintains that throughout his life they came 'straight from von Mises and company' (1992: 86-7). Certainly in 1940 Knight seemed to be in an anti-scientism stage. These remarks may help towards understanding certain aspects of Knight's review.

Knight immediately labels Hutchison a positivist (p 1).[1] He finds this philosophy

[1] All unsupported page references in this section (4.2) are to Knight (1940).

'particularly irritating' because it puts science on a pedestal and supposes that human beings can be studied as if they are objects of the natural sciences.

> Economics and other social sciences deal with knowledge and truth of a different category from that of the natural sciences, truth which is related to sense observation – and ultimately even to logic – in a very different way from that arrived at by the methodology of natural science (pp 5-6).

Knight begins by taking issue with Hutchison's claim that science is able to advance and progress because scientists have definite, agreed criteria for testing their theories, unlike the situation in philosophy. He refers to Hutchison's example of scientists settling the question of whether or not a cheque system exists in Paraguay by 'having a cheque before them'. He points out that it cannot be determined as simply as this, but instead requires knowledge of the history, laws and business usages connected with this single cheque, and the purposes and results of such a system. A cheque system involves more than a series of physical events and therefore questions about it cannot be resolved by exclusively empirical evidence. The agreed criteria that, according to Hutchison, mark out science from philosophy exist only for trivial, not for serious, issues.

While Knight might argue that positivists suppose that human beings can be studied as if they were objects of natural science, this is not what Hutchison is saying. As we saw earlier, he acknowledges the 'invaluable' role of introspection (Hutchison, 1938: 143). While Hutchison was sympathetic to the positivist notion of the unity of science, this was not a central feature of his 1938 argument as indicated by Hutchison's increasing acceptance of the importance of recognising differences between the sciences of degree if not of kind (1981: 276). A more sympathetic interpretation of Hutchison's Paraguayan example is that he is contrasting the availability of factual evidence in an empirical discipline with its scarcity in philosophical debates. As we have pointed out in Chapter Three, Hutchison, following Leslie (1888), acknowledges the importance of a knowledge of history, laws and institutions (1938, *passim*).

Knight now turns to examine Hutchison's claim that science comprises two fields of

knowledge: empirical investigation of the external world, and logical and mathematical truths. He makes two points about knowledge of the external world. First, because appearances are often deceptive, observations must be 'tested' before being accepted as true. Second, this testing of observations is a social activity which means that all empirical knowledge is a social phenomenon. This means that

> knowledge of external reality presupposes 'valid' intercommunication of mental content, in the sense of knowledge, opinion, or suggestion, among the members of a knowing group or intellectual community. A conscious, critical social consensus is the essence of the idea of objectivity or truth (p 7).

Knight explains that such a consensus 'is no matter of a majority vote', but depends upon honourable behaviour among scientists.

What is striking about the second field of knowledge, namely knowledge of the truths of logic and mathematics, is that it is knowledge simultaneously of the external world and of the way in which minds work. The propositions of logic and mathematics, even those regarding imaginary numbers and non-Euclidean space, are empirically verifiable. They are not mere 'forms of thought' but instead concern facts about the world of such a general nature that 'we cannot imagine a situation in which [they] would not be true' (p 10).

While positivism limits knowledge to these two fields, Knight identifies a third field: knowledge of human conduct. This field, into which economics falls, deals with human interests and motivation and constitutes a separate sphere of reality from that of the external world. Its problems are more subtle and complex that those of the 'sciences of (unconscious) nature' (p 12). Knowledge in this field is gained mainly from the process of intercommunication in social intercourse, and only partly from knowledge of physical reality.

According to Knight, Hutchison follows other positivists in claiming that knowledge of the content of human minds may be inferred from observations of their physical behaviour. Knight disagrees and argues that the process of inference in this third field of knowledge is so different from that in the first that is should be called 'sympathetic

introspection' (p 13). While it may be possible to infer thoughts and feelings from human speech or facial expression, what is heard or seen is not much more than what one understands. No matter how much knowledge of the physical world we have, it would not be possible to discover the interests and motivations of human conduct. The reason for this is epistemological. According to Knight, individuals are 'born completely ignorant, without minds,' and acquire knowledge via intercommunication with other selves (p 14).

> Thus our knowledge of the world and our knowledge of one another and of 'mind' in general form inseparable bodies of knowledge which must be studied in relation to one another, if we are to know anything about any of them, or talk sense about them (p 14.)

The intuitive nature of the part of human knowledge arising from the ability to see ourselves from within, and the fact that human behaviour involves intentions which are strictly unobservable, means that it is not possible for the social scientist to verify propositions about human (or economic) behaviour empirically.

Here, we can draw on our introductory comments for some help. Latsis (1972: 235, n 5) lists 'sympathetic introspection' (*einfühlung*) as one of the technical terms of hermeneutics. And so it would seem that the notion (that individuals are born completely ignorant, without minds, and acquire knowledge via intercommunication with other selves) is a concept or construct drawn from Knight's interpretation of hermeneutic philosophy. Note that Hutchison (1938: 140) states that he does not want to enter into methodological and philosophical discussion concerning the significance or legitimacy of the processes of *einfühlen* or *verstehen*. Knight's stressing of the point that individuals acquire knowledge via intercommunication with other selves appears to imply that this knowledge is more important than knowledge gained from physical reality, and so is contrary to empiricist and positivist philosophy. If this is the case, it would seem to clash with Knight's earlier espousal of 'radical empiricism' (1921: 199, n 1).

Knight now turns to the basic postulates of economics. The chief of these is the reality of economic behaviour whose meaning is intuitively known to us. In other

words, it is not possible to determine empirically whether certain behaviour is economic. Economics interprets the reality of economic behaviour by assuming that individuals know various economic propositions. According to Knight, we know these propositions with the same certainty that we know logical and mathematical truths, and with greater certainty than any empirical proposition.

> We know them in the same way that one knows he is writing sentences and not simply making dark markings on a white surface – or is reading versus seeing such marks – by living in the world 'with' other intelligent beings; we neither know them a priori nor by one-sided deduction from data of sense observation (p 17).

Knight's interpretation of the apodictic certainty of the basic postulates of economics appears to accept the existence of Kantian synthetic a priori propositions. Hutchison (1938: 46-7, n 7) pointed out that he did not want to claim that such propositions did or did not exist, only that they could not be fitted into his twofold classification 'without distortion'. While Hutchison's by-passing of the existence of synthetic a priori propositions may be interpreted as reflecting a positivist perspective, it may equally reflect an empirically moderate stance.

Knight goes on to point out that economic literature treats goods as if the qualities of the goods really inhere in the object and are measurable, resulting in the notion of utility. Yet, while some qualities inhere in the object, others are in the mind of the observer. Now this presents a paradox. On the one hand, utility does not actually inhere in the goods themselves. On the other hand, individual choice does depend on quantitative considerations. Yet utilities are not measurable as physical magnitudes. Without a technique of measurement it seems impossible to distinguish whether an experience (of utility) refers to the mind or to the external world. Furthermore, because of ignorance in the real world, as emphasised by Hutchison, the values at which goods exchange do not correspond to their utilities. Yet, if practice conformed to theory, the behaviour would no longer be economic or deliberate, 'and would become a mere mechanical response to a stimulus situation' (p 20). This difference between motive and result is further evidence that 'we do not infer the former from the latter'. Behaviour cannot be interpreted in positive terms because 'positive

science cannot in any sense be problem-solving, while this is the most important fact about human conduct' (p 20, n 13).

Knight makes two points here concerning the problems of analysing economic behaviour in terms of positive science. The first is that we have no way of knowing whether the utility which informs a choice derives from purely subjective considerations, or from attributes of the positive, external world. The second is that the key characteristic of behaviour is that it is problem-solving. As such it depends on (unobservable) motives which lead to (observable) results, not vice versa.

Regarding the first point, Hutchison maintains that Gossen's law or the law of diminishing marginal utility (as well as the law of diminishing returns and Pareto's law) were originally formulated as empirical laws (1938: 60, 64, 134, 135). It is only when they are reformulated as exact propositions of pure theory that they lose their empirical content. Reformulated in this way, conventional utility analysis of consumer behaviour cannot be applied to practical problems given real world uncertainty. Indeed, Hutchison (1938: 88) approvingly quotes Knight's (1921: 268) point (a point that Knight repeats in his review on p 20) that, without uncertainty, behaviour would no longer be economic, but mechanical, 'all organisms automata'. Hutchison (1938) frequently refers to Knight (1921) throughout his book and has stated that Knight's 1921 work probably exerted the most influence on him (Hutchison, 1997:147, n 7).

Knight points out that, once we attempt to apply economics to guide social action 'some theory of value, beyond factual preference, is necessarily involved' (p 22). One such attempt to guide social action is the policy of *laisser-faire* individualism. But, as a basis for this policy, it is impossible to accept individual preferences as absolutely final since, in the real world, individuals are brought up and educated to function in a society. The fact that everyone distinguishes between individual preferences and values assumed to be objective serves to remind us that 'no discussion of group action can be carried on in propositions which merely state what "I want"' (p 23). Yet Hutchison, in his discussion of the economic policy of liberalism, takes the individual as datum and fails to recognise that social policy rests on value judgements.

While Hutchison may not explicitly recognise that social policy rests on value judgements, he clearly states (referring to Weber, 1922: 168-9) that it involves politics 'and no separate economic advice or economic solution of a problem of policy is of any use until the modifications in it resulting from political factors have been worked out' (Hutchison, 1938: 165). On the question of value judgements, Hutchison (1938: 153-4), following Schlick (1930), considers the notion of a contrast between normative and positive sciences 'fundamentally false' and the associated controversy 'very nearly played out'. Hutchison (1960: xxii) however, accepts that his 1938 statement regarding the controversy surrounding the problem of value judgements as 'very nearly played out' does seem now to be 'not a little naïve'.

Knight contrasts a positivistic with a non-positivistic approach to interpreting human action. In the positivistic approach causal laws are interpreted in terms of the uniformity of phenomena. In the non-positivistic approach human action is interpreted as being problem-solving (where both 'problem' and 'solution' seem to be indefinable) (p 26). Knight points out that a strict positivist would have to stick to the realm of physical causality, ie uniformity of sequence, and dismiss interpretations of human behaviour in terms of motives as mystical (p 27).

Knight ends by emphasising the limitations of the possibility of prediction of economic behaviour (p 28). To him the control aspect implied in prediction is abhorrent to all humane thinking. Prediction depends on assuming economic behaviour can be specified in terms of a stable utility function. However, this is seldom the case since an individual's actions are rarely motivated by purely economic factors - and individuals, unlike physical objects, change their minds. Economic positivists and empiricists do not seem to have thought much about how we actually predict human behaviour. In the prediction of individual behaviour empirical observations play a minor role compared to insight into character and personality. While the law of large numbers applies where large numbers of human beings behave individually, this is not the case where they act as groups. Here the basis of prediction is 'social psychology' which, like that of individuals, has more to do with insight and interpretation than statistical extrapolation (p 30).

Instead the principles of economic theory can be applied to demonstrate what is wrong rather than what is right so far as any proposed line of action is concerned. In this respect answers depend on judgements of value and on insight into human nature and social values, rather than on the findings of any positive science. The need is for a broad social interpretative study (*verstehende Wissenschaft*). A sound investigation on this basis should prove practically successful (p 31).

Knight may be correct in stressing that empirical observations play a minor role in predicting an individual's behaviour, but he appears to be on thinner ground in arguing that predicting how large groups behave depends more on 'social psychology' than on 'statistical extrapolations' (Lipsey, 1963: 8). In any case, Hutchison is not recommending that prediction be based only on 'statistical extrapolation'. As we have seen, he also points to the need for 'wider sociological investigations' in attempts to apply economic theory to policy matters (Hutchison, 1938: 166). Whereas Knight here severely emphasises the limitations of prediction in economics, Hutchison (1938: 65) had originally quoted Knight's (1921: 14) contention that 'the aim of science is to predict the future for the purpose of making our conduct intelligent'. The paradox is perhaps resolved by noting that, for Knight, empirical observations play a minor role in prediction, whereas for Hutchison the possibility of prediction and 'the formulation of empirical laws' are merely two ways of saying the same thing (1938: 65).

4.3 Hutchison's 1941 reply and Knight's rejoinder

Hutchison opens by completely disclaiming any concern with Knight's question 'What is truth in economics?' saying that the questions formulated in his book were far less general and ambitious. He complains that the bulk of Knight's criticism is directed at the introductory comments rather than the central arguments of his book. Therefore his reply to Knight will be limited to three main points: further explanation of the arguments of his introductory chapter; answers to some of Knight's more detailed objections; and an appendix aimed at clarifying terminological differences.

According to Hutchison, Knight is annoyed because he fails to state his 'philosophical

position' in his book. But, Hutchison points out, the discussion of his philosophical position is deliberately brief because his book is about economics, and the validity of its main conclusions are in any case independent of his philosophical position. Moreover, such a view is the same as the one adopted by Knight himself in the first sentence of the following quotation:

> The text must not be taken as expressing any view whatever as to the ultimate nature of reality or any other philosophic position. The writer is in fact a radical empiricist in logic, which is to say, as far as theoretical reasoning is concerned, an agnostic on all questions beyond the fairly immediate facts of experience (1921: 199, n 1).

Furthermore, Hutchison is willing to accept the rest of Knight's description as accounting 'for the philosophical starting-point of my book' although 'they are not exactly [the words he] would have chosen [himself]' (p 735). Given our interpretation in Chapter Three, we argue that Hutchison would not have chosen to use the term 'radical' should he have used his own words. Moreover, the chances are that Hutchison 'went along' with the description mainly in order to dramatise the extent to which he accepted Knight's stated philosophical position, after Knight had implied that Hutchison's position must be very different to his own. This seems a reasonable supposition since Hutchison suspects that Knight is unlikely to find such a response satisfactory and he therefore proceeds to spell out his position more carefully.

Hutchison explains that he follows the Anglo-Saxon (as contrasted with the Teutonic) tradition of Locke, Hume and J S Mill and the development of this tradition by Russell, Wittgenstein, Mach, Schlick and Carnap. He notes that, among economists, Pareto, Schumpeter and Myrdal have emphasised the importance of empirical verification. Furthermore, Kaufmann (1936) reflects the point of view labeled 'positivist' by Knight (pp 735-6).[2]

This more careful explanation provides further evidence for our interpretation, in Chapter Three, that Hutchison's position is best described as a fairly broadly-based

[2] Unsupported page references in this section (4.3) from now until the subheading 'Knight's 1941 rejoinder' are to Hutchison (1941).

empiricism which draws on aspects of logical positivism. It is interesting that Hutchison approving cites the work of Pareto, Schumpeter and Myrdal. None of these has been described as positivist.

Hutchison now outlines his two main principles – suggested, as he emphasises, only as convenient classifications: (a) a sharp distinction between deductive and inductive propositions, the former being certain but without empirical content, the latter being empirical but only probable; (b) a sharp distinction between propositions put forward as empirically testable and those that are not, and the suggestion that both natural and social science (apart from their use of deductive theory) can concern themselves only with the former (p 736).

According to Hutchison, these distinctions derive from a two to three hundred year old Anglo-Saxon tradition so when Knight criticises him for recognising only two, rather than three, fields of knowledge, he is objecting to this tradition rather than to Hutchison. Moreover, Hutchison points out that he is not concerned in his book with the existence or nonexistence of fields of knowledge, but rather, first of all, with clarifying the distinction between deductive and inductive propositions. Inductive propositions assert something about the facts of the world and are therefore empirically testable or falsifiable. Deductive propositions assert nothing about the facts of the world and are therefore empirically untestable and unfalsifiable. Instead they assert that the relation between definitions is consistent and are tested by a process of mathematical or logical proof. As he pointed out in his book, most of the propositions of economic theory have been shown to be tautologies (p 737).

Here again, supporting our Chapter Three interpretation, we have direct evidence from Hutchison that he himself views his position as deriving from a two to three hundred year old tradition, rather than simply that of 20th century logical positivism. Here we also find a remarkably clear statement by Hutchison of the vital role of inductive reasoning in gaining knowledge about the world. As we explained in Chapter Three, the importance of the testability (or falsifiability) principle for Hutchison is that it allows us to distinguish inductive from deductive propositions. For Popper, its

importance is that it circumvents the problem of induction. One may ask why it is so important for Hutchison to distinguish inductive from deductive propositions. The answer, as we have argued in Chapter Three, is that Hutchison supports the inductive, rather than the hypothetico-deductive, method of scientific procedure. (Note that Hutchison accords a necessary role to deductive reasoning.) Finally, it should be noted that he is careful to say most, and not all, of the propositions of economic theory are tautologies. Hutchison (1997: 146) thus correctly rebuts Rosenberg's (1992: 244-5) claim that he 'derided' *all* of economic theory as a body of tautologies in his 1938 book.

Following from his second main principle, (b), Hutchison explains the importance of testing or verification. If a proposition claims to tell us something about the real world, 'and if it is questioned, but not confirmed by testing, it can remain only a hypothetical conjecture and not a scientific conclusion' (p 738). On the other hand, if it is denied that it can be tested or even that when someone questions it that it need not be tested, then such a proposition is not scientific. At various times in history propositions claiming to say something about the world have been held to be above all empirical testing. Only when it has been possible again to experiment has that science been able to go forward.

No matter how intuitively obvious are some propositions, to be scientific they must be testable and, if questioned, tested by anybody ready to take the trouble for himself. Without this we have no grounds for pronouncing Knight's propositions about 'snakes' seen by the delirium tremens sufferer false (1940: 7). For then the delirium tremens sufferer might claim that he knew by intuition that his propositions were true. While, as Knight suggests, science is impossible in a world of systematic liars, science is also impossible where there is no general acceptance of empirical tests. Yet Knight rejects the criterion of testability:

> It is not conceivably possible to 'verify' any proposition about 'economic' behaviour by any 'empirical' procedure, if the key words of this statement are defined as they must be defined to be used with relevance and precision (Knight, 1940: 15).

There are two points that call for comment. The first concerns Hutchison's statement that if a proposition that claims to tell us something about the real world is questioned, 'but not confirmed by testing, it can remain only a hypothetical conjecture and not a scientific conclusion' (p 738). This statement implies that were the proposition 'confirmed by testing' it would then qualify as a scientific conclusion. This is not a statement to which one could imagine Popper agreeing. For he famously rejects attempts at confirmation and instead emphasises the crucial importance of attempts at falsification. While this statement could be interpreted as reflecting (Carnap's) notion of confirmation in positivism, it is also consistent with our Chapter Three view that Hutchison adopted an inductivist approach to science.

The second point concerns Knight's contention that it is not possible to verify economic propositions by any empirical procedure. This contention reflects the extent to which Knight appears to be removed from even the most moderate, let alone radical, empiricism! Like Knight's (1940: 13) earlier remark that the process of inferring knowledge of human conduct should be called 'sympathetic introspection', it appears to confirm Latsis's interpretation of Knight's philosophy as hermeneutic, and Hutchison's (1997: 148) point that, in 1940 at least, Knight took 'his methodological views straight from Mises'.

According to Knight, not only is the criterion of testability 'fundamentally misleading and wrong', but,

> if one begins with confident and sweeping assertions about 'tests', one is under a corresponding obligation to make it unambiguously clear what sort of propositions do and what sort do not need testing and what tests are accepted as valid and not in themselves in need of testing. This follow-up is just what we do not find in Mr. Hutchison's essay (Knight, 1940: 6).

As regards Knight's question as to what propositions do and what propositions do not need testing, Hutchison replies that any proposition that is put forward as saying something about the facts of economic life is in need of testing. With regard to Knight's question as to what tests are accepted as valid and not themselves in need of testing, Hutchison argues in the same spirit as Popper there are no tests which will

finally decide whether an empirical proposition is absolutely true or false. The validity of tests must be, in a sense, relative. Science, like democracy, depends upon the willingness to accept certain rules for carrying out tests as well as upon the acceptance of the results of such tests. In view of this what tests we accept as valid is, in a sense, a matter of convention. While Knight (1940: 4) strongly disagrees with this notion of testing as conventional, he himself only a few pages later argues that testing is 'a social activity or phenomena' and that 'a conscious, critical social consensus is of the essence of the idea of objectivity or truth' (1940: 7).

Hutchison ends this section of his reply by reiterating that he did not claim any 'absoluteness' about the criterion of testability. He was merely calling attention to the two reasons for maintaining the criterion: first, as Mill urged, the social sciences can best advance by adopting the methods of the natural sciences, notably the constant empirical testing of their propositions; second, that the only ground on which to uphold the authority of science is 'the appeal to fact'.

> It is the task of the twentieth century to get the appeal to fact as widely and readily accepted in the field of social sciences as, since the seventeenth century, it has gradually become accepted in that of natural science (p 742).

Two points call for comment. The first concerns Hutchison's view that what tests are accepted as valid is a matter of convention. Here Hutchison appears to display the influence of Poincaré's notion of conventionalism. As we saw, Knight also pointed to the notion of testing as conventional. While this appears to show some overlap between Knight and Hutchison's positions, discussion of the question of conventionalism will be dealt with in the next chapter since there we will be able to relate the discussion to Machlup's conventionalist stand which differs from Knight's and Hutchison's positions. Second, Hutchison's call for the social sciences to adopt the methods of the natural sciences clearly reflects the influence of naturalism and positivism. However, as Hutchison explained, by 1960 his views were 'considerably' less naturalistic than they had been twenty years earlier:

> Differences between the natural and social sciences seem more important and ineluctable than they did then. Indeed, though quite ready, for the most part, to

> accept and rely on Professor Popper's anti-naturalist thesis in *The Poverty of Historicism*, I would not always want to go so far as he seems to go in denying significance to the differences between the natural and social sciences (1960: xi).

Still later Hutchison accepted that his 1938 book 'expressed a highly "naturalistic" view' and emphasised the importance of recognising the differences between the natural and social sciences (1981: 300, 276). As mentioned earlier, this positivist issue was not central to Hutchison's stance.

Hutchison now proceeds to answer some of Knight's more detailed objections:

He rejects Knight's claim that he was exalting science above non-science. Instead he was simply concerned with distinguishing between the two.

He rejects Knight's claim that he did not stick to his own criterion for scientific propositions, ie that of testability. For example, in his chapter five where he discussed the concepts of utility and welfare he made it clear that these concepts need not be viewed as metaphysical, but could be defined as empirically testable.

According to Hutchison, Knight puts forward three propositions. First, economic behaviour involves allocating limited means among alternative uses. Since nothing is said about what means are available or how they will be used, this seems to be no more than a very obvious generalisation. Second, different behaviour leads to different results. Hutchison comments that if this is not a tautology it might very often be false. Third, one particular allocation of means will yield 'more' than all the others. Hutchison contends that these propositions do not tell us anything at all about concrete economic problems such as the trade cycle, unemployment or monetary conditions. They are no more than intuitions unverifiable by any empirical procedure.

Hutchison says his appendix was a discussion of some postulates of economic liberalism. Yet Knight criticises it as if it were a discussion of economic policy. This procedure epitomises Knight's overall method of attack in his review. Knight claims that the author sets out to treat some wide, general subject (eg What is truth?) when

the author is instead concerned with a much narrower purpose. Knight might well have thought Hutchison's concerns trivial, but he could have said this in 32 lines, not 32 pages.

In this section Hutchison rejects Knight's objections and proceeds to criticise Knight's three propositions as unverifiable, and therefore, according to the criterion of testability, unscientific. He also criticises Knight for criticising him on broad general issues which he never attempted in the first place. This quest on the part of Knight to discuss broad general issues such as 'What is truth?' and the rejection on the part of Hutchison of discussion of such issues shows up the differences between Hutchison's empiricist outlook and Knight's 'anti-positivism' (Hammond, 1991).

In an appendix Hutchison lists some of Knight's propositions which he finds 'glaringly false' since this might help to show the extent of the intervening gulf between them:

a) The validity of any interpretation of economic behaviour in terms of motives depends on the possibility of error or uncertainty. But the conception of any process as problem-solving is rejected by positivism as metaphysical (Knight, 1940: 27). Hutchison says he wouldn't be concerned to argue on behalf of positivism but for the fact that Knight appears to apply this term to his views in general. He says he does not understand the sense in which Knight uses the term 'problem', but it seems that the proposition quoted is a tautology. Hutchison explains that the sense in which he uses the term 'problem' coincides with the ordinary everyday sense in which one wants to know whether or not certain practical actions will achieve certain practical results.

b) The positivist might well ponder the fact that no objective definition can be given of 'work' and 'play' fundamental as these concepts are in economics (Knight, 1940: 25). Hutchison replies that he can give such definitions.

c) Surely no one thinks that from knowledge of the physical world it would be possible to predict the interests of intelligent beings living in it even if knowledge of

psychology is included? (Knight, 1940: 14). Hutchison replies that psychology should be able to tell us something about their interests in 'a particular physical environment'.

d) Changes of mind upset predictions based on observation of previous behaviour (Knight, 1940: 29). Hutchison asks whether changes of mind cannot be predicted on the basis of previous behaviour about which there have been changes of mind?

e) Where there are agreed criteria for testing there can be no serious intellectual or methodological problems (Knight, 1940: 3). Hutchison asks whether there are no criteria in the natural sciences the adoption of which has coincided with their growth and progress?

f) Hutchison completely ignores intercommunication (Knight, 1940: 13). Hutchison says that Knight quotes him referring to 'spoken and written words, . . . tone of voice or facial expression'.

Hutchison's bringing together of the propositions that most divide his views from those of Knight conveniently highlights the extent to which Knight's views are influenced by rationalism and Hutchison's by empiricism. *a)* Knight views problems as mental phenomena, whereas for Hutchison they are empirical phenomena. *c)* and *d)* For Knight we cannot acquire knowledge about human behaviour simply from knowledge of the physical world, whereas for Hutchison we can. *d)* Here Hutchison displays his inductivist orientation. *f)* Knight emphasises mental communication more than Hutchison. Finally, in *a)* Hutchison explicitly states that he is 'not concerned to argue on behalf of positivism'. This once again provides support for our argument in Chapter Three that Hutchison should not be interpreted primarily as a positivist.

Knight's 1941 rejoinder

In reply Knight says that, apart from reviewing Hutchison's book, he was concerned with a school of thought as well as general methodological issues. This is why he

takes 32 pages and not 32 lines. In his review he was mainly concerned with calling attention to the inconsistencies which arise from trying to reduce the fundamental concepts of economics to terms of empirical fact.

The central issue of debate concerns the view that the propositions of economic theory must be testable, because only testable propositions have any place in science. Literally speaking, this is a matter of the definition of science which hardly seems worth arguing about. Knight says he is not particularly concerned whether or not economic theory is regarded as a science. What he is concerned about is that it should be clearly understood that it is not a science in Mr. Hutchison's sense, that is, a natural or physical science. While the propositions of the natural sciences are indeed testable, Knight emphasises they are testable only within limits (p 751).[3]

This is because agreements on observations and logic exist 'in a community of discourse' only amongst persons who are competent and trustworthy judges. Such judges are identifiable only by mutual recognition. Furthermore, physical science contains concepts of a purely interpretive character which are not sense data, eg force, energy, and matter. While physics has been defined as the science of measurement, it should be noted first, 'that at the margin of accuracy, measurement is itself a matter of estimation' or judgement and second, that physics cannot state in purely empirical terms what is being measured (p 752).

Here two points call for comment. First, Knight reiterates the extent to which testing depends upon mutual agreement within a specialised community. He had made this point before when speaking of testing as a social activity (1940: 7). On this issue Hutchison agrees with Knight: both the rules for carrying out tests and the acceptance of the results of such tests are clearly acknowledged by Hutchison to depend upon the willingness of scientists to accept such rules and results, that is, depend upon accepting a convention (1938: 145, 152). Yet, on this earlier occasion, Knight (1940: 4) had criticised his stance for implying that 'truth is merely a game in which the players are free to make any rules they please'! (see Hutchison, 1941: 741). Second,

[3] Unsupported page references for the remainder of this section (4.3) are to Knight (1941).

according to Latsis (1972: 235-6), Knight, in stressing that testing depends upon agreement within in a 'community of discourse', anticipated the view that 'there can be no explicitly formulable demarcation criteria even in natural science'. Yet, one wonders, why did Latsis not extend such foresightedness to Hutchison who had already made much the same observation in his 1938 book? Perhaps, he realised that Hutchison, while agreeing that a conventional element is involved in testing, would not accept such a conclusion. Nevertheless, when Latsis goes on to point out that Knight lapses into authoritarianism in insisting that those qualified to judge on the validity of tests are 'identifiable only by mutual recognition', he expresses a criticism that would appeal only too readily to Hutchison's rejection of dogmatism and authoritarianism in any discipline purporting to be scientific.

Given this conventional element in science, Knight argues that there is no ultimate difference between theoretical economics and theoretical physics. However, a difference remains and it is so great that it is a difference in kind rather than one of degree. The fundamental concepts of economic theory cannot be reduced to the same meaning-content as those of physical science. The principles of economics cannot be verified in the same sense as the laws of physics or the propositions of mathematics. Economic magnitudes are not measurable, but at the most are estimated, and then only by the individual subject.

The difference is so great because economic propositions relate to purposiveness in human behaviour. Such purposiveness cannot be inferred from the observation of behaviour. The fact that human behaviour is affected by error leads to a divergence between descriptions of its purpose and observation of actual behaviour. 'And in this contrast centers the primary interest in economic principles' (p 752). Economic propositions attempt to describe an ideal, not the reality. They are subject to no test other than the agreement that stems from the intercommunicative life of rational individuals. Consequently, Knight says, when he opposes his formulation of ultimate economic principles to that of any other writer, he is only giving his opinion; and it remains no more than that 'except in so far as it is confirmed by a general consensus of competent and trustworthy students' (p 752).

Knight ends by saying that he was also interested in pointing out in his review article that the fundamental concepts of economic theory must also be contrasted with a third type of proposition, namely that which states a value judgement. Men do not behave like machines, but instead value some ends above others. And 'the primary value or end which ought to be pursued is telling the truth' (p 753). While any proposition reflects a merely individual state of mind, rational discourse consists of propositions which state truth of a superindividual character or objectivity.

According to Knight, his debate with Hutchison concerns the principle, or criterion, of testability (p 751). While Hutchison maintains that the propositions of economic theory must be empirically testable, Knight maintains that this criterion is inapplicable to economics. Economics is concerned with purposive behaviour in terms of motives. Such propositions are to be understood in terms of introspection and cannot be inferred from (nor tested by) the observation of behaviour. Even on this central issue there appears to be room for agreement. For Hutchison, as we have seen, quite accepts a role for introspection in suggesting hypothesis and regards introspection as invaluable. And Knight, in view of his 'radical empiricism' (1921: 199, n 1), would surely agree that at least some statements in economics are empirically testable.

According to Knight, Hutchison views economics as a science not significantly different from the natural sciences. Knight points out that even in the natural sciences propositions are testable only within limits. That is, the concepts and relations within these sciences cannot be reduced to purely empirical terms. In any event, economics is so different from the natural sciences that it involves a difference in kind rather than one of degree. Again, on these issues a *rapprochement* is possible. As has been argued in Chapter Three, the extent to which Hutchison adheres to the reductionist thesis of logical positivism is debatable. Hutchison (1960: xi) explains that his views have become less naturalistic and admits to important differences between the natural and social sciences. (He still views the differences as those of degree, rather than of kind (p xii).) Nevertheless they 'amount to such a considerable degree as to constitute a profound contrast' (Hutchison, 1981: 276). Rather, the divisive issue is over the extent to which the natural science method of subjecting statements to empirical testing is useful in the social sciences.

4.4 Kaufmann's 1942 response

Kaufmann (1942) argues that the problem of explaining why the achievements of the social sciences hardly compare to those of the natural sciences is not to be resolved in terms of choosing between the naturalist and the anti-naturalist viewpoint. Rather, such an explanation concerns, first, ascertaining the methodology common to all empirical sciences, and second, distinguishing between the different methods of the natural and social sciences within this common methodology. Kaufmann intends to apply this procedure to examine the nature of the basic postulates of economic theory. His chief point is that an ambiguity concerning the concept of law underlies the methodological controversy between Hutchison and Knight (p 384).[4] This controversy goes back to Schmoller and Menger and even Ricardo and Malthus (p 383).

Kaufmann examines the ambiguity concerning the concept of law by extending Hutchison's analysis of the role of the ceteris paribus clause in economic theory. He argues that further analysis 'reveals that the ceteris paribus clause is a way of formulating a heuristic postulate, a methodological program' (p 388). For example, in trying to explain changes in the amount demanded of a given commodity, we start by investigating empirically whether or not changes in demand are preceded by changes in the price of the commodity. If it turns out that this is not the case, we look at other relevant factors such as changes in income or the price of substitutes. The programme is not complete unless all the 'disturbing factors' are discovered. Kaufmann argues that errors that arise as a result of employing this procedure are the result of incorrect interpretations of the programme and not, as historical and institutional economists argue, the result of the procedure itself.

> The method of isolating a small number of factors, analyzing the relations between them and making these relations the backbone of a theoretical construction is indeed common to all empirical sciences which are not merely descriptive (p 389; cf Kaufmann, 1944: 216).

[4] All unsupported page references in this section (4.4) are to Kaufmann (1942).

This is the method employed by Bacon, Galileo and Newton. They have been as much attacked for being too abstract as today's theoretical economists. Yet, Kaufmann emphasises, the method of isolation needs 'a method of synthesis'. The dangers of its being an incomplete programme on its own are best avoided by distinguishing carefully between heuristic postulates and empirical laws (p 389).

Such a distinction is at the centre of the controversy about the nature of laws in economics. Kaufmann asks how Galileo's law of falling bodies can be said to be valid if empirical tests show that falling bodies generally do not behave in accordance with it. The usual answer is that it is true only under certain conditions, namely that of empty space. This goes against the notion that a law is a universal proposition stating that certain facts will occur when other facts occur. To say that a proposition is true only under certain conditions could properly be interpreted as saying that it is generally false.

> This leads us to the realization that the so-called empirical testing of physical laws of the type of Galileo's law of falling bodies is in fact a testing of more restricted laws. The restrictions are implied in the additional conditions which the experimenting physicist considers relevant for the test (p 390).

Even were this more restricted law empirically falsified it would not imply that the more general version of the law 'loses all significance for scientific procedure'. Kaufmann points out that the ceteris paribus clause

> may still indicate a certain line of research to be followed, that is, it may still function as a heuristic postulate. But we should never forget that a heuristic postulate as such is a convention, not a proposition belonging to the corpus of science. Such a convention may have to be replaced by another if it does not lead to the desired results, and accordingly it may be affected by the negative outcome of an empirical test. But it cannot be directly falsified by it (p 390).

Since it is not possible to falsify the principles of economics directly by an empirical test, Kaufmann suggests that they are to be generally understood as heuristic postulates. That this has not been recognised is due to 'wishful thinking', by a desire to justify whatever method is employed on ultimate grounds, eg the principle of the uniformity of nature is used to justify induction, a principle of causality to justify the

notion that all events are explainable, and a principle of free will to justify the notion that not every fact in psychology or in the social sciences is explicable.

In orthodox economics the problem is to explain or predict a particular kind of human behaviour, namely that involved in exchanging goods. We do this by assuming, with J S Mill, that men 'prefer a greater portion of wealth to a smaller one in all cases'. 'Much has been written to show that this principle, if understood as a statement about reality, does not hold universally' (p 391). But, this need not imply 'the approach of theoretical economics is inadequate'. While the profit motive does not hold universally, it plays a significant enough role in most market transactions to give us good enough reason for assuming that behaviour is governed by this single motive alone. Nor, Kaufmann continues, do we need the assumption of perfect foresight to justify assuming behaviour is governed by the profit motive alone. While Hutchison, following Morgenstern (1935), has correctly stressed that the assumption of perfect foresight leads to contradictions, this does not mean the method of economic science is wrong. Rather the contradictions result from misinterpreting the method. This misinterpretation has resulted from the desire to give a priori reasons for the adequacy of the method.

According to Kaufmann, Hutchison may agree with the above, but Knight will probably disagree by pointing out that the argument above is based on the misguided assumption 'that economic theory deals with factual behaviour' (p 392). For Knight, economic theory is a normative science like ethics, grammar or jurisprudence and as such deals with ideal or rational behaviour. The problem in a normative science is to find out whether given (ie actual or ideal) behaviour is in accordance with certain norms.

> The underlying idea is that if both a rational end of economic behaviour and the correct means to be applied to the promotion of it are predetermined - with the result that every deviation from these standards, either in setting goals or in the attempt to reach them, can be regarded as an error attributable to human weakness - then the method of economics dealing with ideal, perfectly rational behaviour is a priori justified. These standards are to be discovered by an analysis of motives which are given with absolute certainty by introspection (p 395).

For Kaufmann, this theoretical framework may have its merits, but 'only so long as one does not attempt to press empirical science into the procrustean bed of such a preconceived scheme' (p 395). This does not mean that we need to go to the extreme of conflating economics and physics. There are significant differences between these subjects. But, while there are these differences, nevertheless there is a 'structural similarity', a basic layer of methodological problems common to all empirical science.

It is now time to sum up the implications of Kaufmann's intervention for the issues raised in the Hutchison-Knight debate. Kaufmann's article represents an important turning point in this debate which, until then, had been conducted largely as one between empiricism and a priorism. The main result of Kaufmann's intervention is to introduce a pragmatic note into the debate which may be said to anticipate, in certain respects, the instrumentalism and conventionalism of Friedman (1953) and Machlup (1952, 1955).

Kaufmann is clearly more sympathetic to Hutchison's view of economics as an empirical science dealing with factual data than to Knight's view that economics is concerned with describing an ideal - an ideal concerned with the purpose or motive of economic behaviour - rather than the reality (Knight, 1941: 752). He rejects attempts to base a science such as economics on 'ultimate grounds conceived as self-evident truths' and 'cannot accept von Mises's argument' (Kaufmann, 1944: 1, 226). Yet, he argues, Hutchison 'has not come to the root of the methodological problem involved' (p 387). This is because economic laws are not to be understood as empirical statements (Kaufmann, 1944: 213), but rather as heuristic postulates which describe a method of scientific inquiry without referring to 'the very nature of the objects of inquiry' (p 392). Such postulates are to be interpreted as a methodological programme. For example, the assumption that individuals are motivated solely by the profit motive may yield useful conclusions. However, in making this assumption we are not making any claim about the actual behaviour of the individuals. If our conclusions are empirically falsified it means only that our heuristic postulates are inadequate as tools, not that some proposition about economic reality has been falsified (Lowe, 1942: 432). Here Kaufmann appears to adopt an instrumentalist

interpretation similar to Friedman's (1953) later stance.

Kaufmann's instrumentalism can be traced to Dewey's (1938) influence. Kaufmann himself (1944: vii) explains that his 1944 book is 'a very different book' from his *Methodenlehre der Sozialwissenschaften* (1936) due to his being influenced by Dewey's analysis of scientific procedure in his *Logic: The Theory of Inquiry* (1938). According to Gouinlock (1998: 47), the core of Dewey's pragmatism - or as Dewey himself called it, instrumentalism - is his *Theory of Inquiry*. 'The logic of inquiry is not a set of norms existing independently of and prior to our cognitive efforts' (ibid). Rather, 'the philosophy of science starts from the fact that science is already an ongoing social activity' (Ryan, 1999: 139). 'Inquiry is inevitably theory-laden - or, in Dewey's parlance, meaning-laden' (Gouinlock, 1998: 48). Gouinlock goes on to argue that 'Dewey repudiated the archaic idea that knowledge is a correspondence between an object and a mental image' (ibid).

It would appear that this influence was already making itself felt in 1942 in terms of Kaufmann's instrumentalist view that his 'heuristic postulates' are to be interpreted as tools and not as propositions about reality. This leads to his statement that his approach proceeds 'along a different path' from the one that analyses methodological issues in terms of opposed philosophical doctrines, eg rationalism-empiricism, realism-idealism and subjectivism-objectivism (1944: 2). Here we are clearly far removed from the rationalist-empiricist debate between Knight and Hutchison.

> Discussions concerning the nature of the most general principles of classical physics, particularly the principle of the conservation of energy, by conventionalists like Henri Poincaré or Eddington and their opponents reveal a striking similarity to the controversies concerning the nature of the principles of economics (p 390).

Kaufmann criticises Hutchison for interpreting economic principles or laws as statements that can be empirically falsified. Since in general it is not possible to falsify such principles or laws empirically, they are better understood as heuristic postulates. This conflicts with Hutchison's (1938: 62, 64) view that scientific laws are viewed in most sciences (except economics) as inductive inferences which are

conceivably empirically falsifiable. Kaufmann interprets Hutchison's position as 'radical behaviourism' or physicalism (p 383) or, more generally, as a form of sensationalism (Kaufmann, 1944: 7, 151):

> But the rejection of pseudo-philosophical 'ultimate' grounds for the validity of the laws of the market, which are supposedly derivable from the nature of man, is not bound to lead us to the thesis that they have no other basis than actually observed behavior in the market. Analysis of human motives plays a significant part in their foundation (Kaufmann, 1944: 216-7).

In other words, Kaufmann argues, the rejection of Knight's thesis does not necessarily lead us to adopting (what he views as) Hutchison's sensationalism. Yet Hutchison (1938: 143) explicitly denies he adopts the doctrine of behaviourism. As we have argued in Chapter Three, Hutchison adopts an empiricist position broader than logical positivism or behaviourism. Instead he is much influenced by Poincaré's (1905) 'conventionalism' and would deny a naïve realist position that scientific inquiry gets to 'the very nature of the objects of inquiry'. But, like Poincaré, Hutchison keeps his conventionalism in check. While principles and laws contain a conventional element, the test of empirical evidence is still relevant.

The importance of Kaufmann's intervention for our thesis is that here we have what appears to be the first attempt to respond to Hutchison's arguments by introducing the pragmatist stance of instrumentalism. It seems strange that Friedman's (1953) essay which is concerned with defending the neoclassical orthodoxy against Hutchison's criticisms, should make no mention of Hutchison at all. It may reasonably be conjectured that Friedman's (defensive) approach was influenced by Kaufmann (1942). In that case we have a possible link between Hutchison (1938) and Friedman's (1953) essay via Kaufmann (1942). Even if this were not so, Kaufmann still provides an important link between Hutchison and the later interventions of Friedman and Machlup. In attempting to refute what he saw as Hutchison's radical empiricism (sensationalism) and Knight's radical rationalism (supremacy of reason over sense), Kaufmann (1944: 15) paved the way for Friedman and Machlup to introduce a modified rationalism and conventionalism in terms of which empirical testability was considered to be relevant only to the predictions of a theory.

Hutchison's view, that the empirical testability of the assumptions was relevant, was conveniently ignored. This was because the methodological procedure adopted allowed both Friedman and Machlup to defend traditional economic theory (à la Robbins) yet seemingly adopt the new 'scientific' language of empirical testability. Hutchison's consistent empiricism involved too critical a stance towards the fundamentals of orthodox economic theory. It called for 'a fundamental alteration' in the orthodox approach to economic phenomena (Hutchison, 1938: 64). Kaufmann (1944: 228) denies that such a 'radically different' approach is needed. His response was to set the trend away from the more radical implications of Hutchison's thesis towards a defence of economic orthodoxy.

According to Coats (1983), Kaufmann's views are constructive and could have helped resolve the later dispute between Machlup and Hutchison. This would have involved interpreting Kaufmann's heuristic postulates as a third type of proposition, ie in addition to the analytic and synthetic types of proposition of Hutchison's twofold classification. Yet as Lowe (1942: 434) pointed out, to treat economic laws merely as heuristic postulates 'would simply enable the economist to avoid the implications of empirical tests by modifying his postulates' (Coats, 1983: 22). While such an option would be rejected out of hand in terms of Hutchison's methodology, it fitted in well with the instrumentalist and conventionalist approaches of adopted by Friedman and Machlup.

Conclusion

Four major points emerge as a result of the discussion of this chapter. First, much of the influence that Hutchison exerted on post-1938 methodological debate arose not so much because of his alleged positivist ideas, but rather because his 1938 intervention was interpreted as too critical of orthodox economic theory. Second, given Knight's forthright a prioristic stance in 1940, what is notable is the extent to which he and Hutchison came close to agreeing on several methodological issues. Third, after the Hutchison-Knight debate, the issues raised by Hutchison ceased to be cast as those between rationalism and empiricism. Fourth, there is a sense in which the Hutchison-

Knight debate reflects a re-emergence of the long-standing *methodenstreit* in economics between rationalism and empiricism.

The first point refers to our main organising principle of this and the next chapter. This involves a modification of Latsis's (1972) classification. Two interpretations of Hutchison's 1938 are at issue. On the one hand, from the side of the apologetic defenders of the neoclassical (research) programme (Knight, Friedman and Machlup), Hutchison was viewed as too critical of orthodox economic theory. This issue, rather than purely methodological considerations, motivated their interventions. Against this is the interpretation adopted in this chapter that Hutchison's 1938 is to be viewed as primarily concerned with methodological issues, in particular applying an empiricist demarcation criterion to all areas of economics 'come what may'.

The interpretation of Hutchison as primarily a critic of economic orthodoxy simply does not hold water. Hutchison (1938) agrees with the views of many orthodox economists (eg Smith, Jevons, Marshall and Knight!) and in his 1941 reply to Knight singles out Pareto, Schumpeter and Myrdal for approval (1941: 735). In a recent article he commented:

> I have come, more and more, in recent decades to see Frank Knight's *Risk, Uncertainty and Profit* (1921) as one of the most (or perhaps the most) important and valuable works of this century regarding the more profound, or methodological aspects of micro-economics. *In fact, in Hutchison (1938), I think I probably drew more inspiration from this work than from any other work on economics (as my citations might suggest)* (Hutchison, 1997: 147, n 7, emphasis added).

As Hutchison (1998: 67) points out, in 1921 Knight was 'then, apparently in an "ultra-empiricist" mode or mood'. Whether in 1921 or 1940 Knight remained in orthodox economics mode or mood. Apart from Hutchison's approving citation of leading neoclassicalists, Hutchison accepts various key aspects of orthodox economics such as Gossen's law or the law of diminishing marginal utility, the law of diminishing returns and Pareto's law - as they were originally formulated as empirical propositions (1938: 60, 64, 134-5). It is their interpretation as a priori propositions that Hutchison rejects. Yet it was the extent to which leading apologetic defenders of

neoclassical orthodoxy chose to interpret Hutchison as attacking orthodoxy that threw Hutchison into the limelight and so brought the attention of the profession to focus on his methodological views.

Second, we have pointed out several areas where Hutchison and Knight in 1940-1 come surprisingly close to agreement. Knight himself refers to the central issue of his debate with Hutchison as 'the view that the propositions of economic theory must be testable' (1941: 751). While Knight has been at pains to emphasise the extent to which economic propositions are testable only within limits given that testing is a social activity and hence requires social intercommunication and consensus (Knight, 1940: 7), Hutchison is far from espousing a naïve empiricist or falsificationist stance regarding testing. Drawing, perhaps on his reading of Poincaré, he fully accepts that testing involves a conventional element (1938: 145, 152). (Hutchison (1938: 65) approvingly cites Knight's - this time in his 1921 empiricist mode - statement that 'the' goal of science is prediction).

Another issue where Hutchison and Knight come closer to agreement than one might expect is on the extent, or significance, of the difference between the natural and social sciences. Knight is clear that the difference is so great that it is one of kind rather than degree (Knight, 1941: 752), while Hutchison (1938: 14-15) rejects this anti-naturalistic view. However, since 1938, Hutchison has revised his stance on this point going so far as to state that, while the differences may be only those of degree, they 'amount to such a considerable degree as to constitute a profound contrast' (1981: 276). One may reasonably infer that this was not one of the central props of Hutchison's position in 1938 so that the naturalistic-anti-naturalistic divide was narrower than may have appeared at the time.

This is not to suggest that major differences between Knight and Hutchison did not exist in 1940-1. One only has to think of Knight's insistence on the existence of a third field of knowledge: that of human conduct which involved unobservable motives leading to observable results and not vice versa. Our point is that Knight was motivated to reply to Hutchison - and this accounts for the fierceness of his response - out of a desire to defend neoclassical orthodoxy against what he regarded as an

attack on its leading propositions. The casualty in this battle was the depiction of Hutchison as occupying an extremist methodological position and the chance for a *rapprochement* in the on-going *methodenstreit* between rationalism and empiricism in the history of economics.

This brings us to the third point. The Hutchison-Knight debate was one in which the main dividing methodological issue was that between rationalism and empiricism. As mentioned above, given the calibre of the two representatives of these positions, they both showed more understanding of each other's positions than might ordinarily be expected in such an exchange. Nevertheless, Knight's insistence on the existence of a third field of knowledge lay behind his view that the social sciences should be exempted from Hutchison's Principle of Testability, something to which Hutchison could never be expected to agree. After the Hutchison-Knight debate the clarity of the issue as one between rationalism and empiricism was lost. As Latsis has pointed out, Friedman and Machlup recast the terms of the debate.

> They - unlike Knight - do not wish to apply peculiar standards to the social sciences. They wanted what I call the neoclassical research programme to come out as satisfactory by general methodological standards (1972: 236).

Indeed, Friedman (1953) presented his position as representing the 'methodology of positive economics'! In this vein, he appeared to accept Hutchison's Principle of Testability showing how empirical testing could, and should, be applied to the propositions of economic theory. In case anyone should decide not to restrict empirical testing to predictions and extend it more generally to include assumptions, Machlup (1955) made it clear that the adoption of such a procedure would represent an extremist, ultra-empiricist stance. Hutchison was chosen as the sole representative of such a position. In this way Machlup could both defend the neoclassical programme and isolate the all too independent and critical Hutchison.

This brings us to the fourth point. With reference to the exchange between Hutchison and Knight, Kaufmann comments:

> We have here before us, if somewhat revised, a methodological issue which has

> troubled economists since the days of Karl Menger and Gustav Schmoller, even since those of Ricardo and Malthus (Kaufmann, 1942: 383).

We would suggest that Kaufmann's point reflects a more accurate description of the perspective that should have been brought to bear in appraising Hutchison's (1938) contribution. Instead, in the process of attempting to defend the neoclassical programme, Friedman and Machlup succeeded in generating a quite wonderful degree of methodological confusion in economics. Unfortunately after making this helpful point Kaufmann (1942), under the influence of Dewey (1938), went on to suggest that the way to resolve the rationalist-empiricist debate in economics was to apply Dewey's pragmatism to the issues involved. As discussed earlier, this suggestion may possibly have led Friedman to his instrumentalist stance in his famous 1953 essay.

CHAPTER 5

THE INFLUENCE OF HUTCHISON'S INTERVENTION: MACHLUP

No sooner had Robbins (1932) presented economics as a discipline with a body of propositions about which all reasonable individuals were in agreement than Chamberlin (1933) and Robinson (1933) disrupted this picture of scientific consensus. Robbins (1935) responded to the changed mood. The next year Keynes's *General Theory* caused an even bigger disruption by seeking to replace classical theory on the subject of employment. Two years later Hutchison's (1938) directly challenged Robbins's complacency concerning the status of economics as a science. Then Hall and Hitch (1939) questioned the relevance of the neoclassical theory of the firm, while Triffen (1940) attempted to carry further the work of Chamberlin (1933) and Robinson (1933).

In this revolutionary decade of the 1930s Hutchison's methodological challenge may have been the last straw for Knight. Whatever the reason Knight's (1940) furious response represented the last attempt to explicitly represent and defend orthodox economics on a prioristic grounds. Hutchison (1938) had been launched on a wave of empiricist sentiment that was to push through until well into the 1960s. This trend increasingly made Knight's 1940 defence appear outmoded and called for a 'modern' reply to Hutchison that would show not only that neoclassical theory was quite consistent with the new methodology of science, but would also rebut Hutchison's criticisms using the new methodological framework. This was a tough call and it took thirteen years until Friedman (1953) delivered the next major methodological statement in economics. Although Friedman did not refer to Hutchison, his famous essay can be read as an indirect response to Hutchison's challenge. Machlup's (1955) more philosophically sophisticated piece not only shored up Friedman (1953), but sought to deal directly with the challenge posed by Hutchison (1938).

Hutchison's (1938) was directly concerned with the basic postulates of economic theory, a topic that had formed the point of departure for the new theories of

monopolistic competition. He contested the method of arriving at economic propositions by deducing them from a few self-evident assumptions. Not only did he argue (in his chapters two and three) that using this method rendered these propositions empirically untestable, he also directly challenged the basic assumptions of this 'pure theory' in his chapter four. In this key chapter he challenged the 'fundamental assumption' of maximising behaviour; the assumption of perfect knowledge and the assumption of a tendency to equilibrium. Robbins (1932, 1935) had distinguished between the fundamental assumptions of economics (scarcity, more than one factor of production, scales of valuation) and the 'expository devices' of maximising behaviour and perfect knowledge.

> Hutchison changed all this by placing the construct of the rational, maximising economic agent who operates with full and perfect information on center stage: this construct, in his mind, is the 'fundamental assumption' of economic theory. The status and importance of this 'unrealistic' assumption, its role in economic theory, and the nature of its testing are questions that were to dominate economic methodological debate in the 1950s (Caldwell, 1982: 117).

In fact they were to become the focus of debate much sooner than the 1950s. The very next year Hall and Hitch (1939) challenged the very assumption that Hutchison had so recently shifted to centre stage. They found that the majority of firms in their survey set prices by adding a mark-up to average cost in a 'rule of thumb' way which would result in maximum profits only by chance. They thus questioned the neoclassical assumption of maximising behaviour. Prices were not set by equating marginal cost and revenue, but according to a full-cost principle. After the war this controversy was continued in England by Andrews (1949) and Robinson (1950) and, in America, by Lester (1946) and Machlup (1946). Meanwhile, in addition to Hall and Hitch's findings, there was the challenge to orthodoxy presented by the theory of monopolistic competition.

Taking up our theme from Chapter Four we will, following Latsis (1972), continue to cast Friedman and Machlup as apologetic defenders of the neoclassical programme. Although the first direct influence of Hutchison arises only in his 1955-6 exchange with Machlup, in order to evaluate this debate it necessary to understand the background to Machlup's (1955) intervention. To this end it is necessary to examine the period between Knight (1940) and Friedman (1953) in an effort to understand why

it took thirteen years for Friedman's 'modern' methodological reply to Hutchison (1938) to be forthcoming. In this intervening period the kinds of discussion are not clearly methodological or clearly economic, but are rather a mixture of the two. This is because we argue, in line with Latsis, that Friedman and Machlup were concerned primarily with defending orthodox economic theory – methodological considerations are used as back up when needed.

The chapter divides the material prior to the Hutchison-Machlup exchange into three sections. In the first section we focus on Friedman's initial response to the theory of monopolistic competition. Here we note the similarity of his methodological views with those of Hutchison. The second section focuses on Machlup's engagement with the re-emergence of the marginalist or full-cost pricing controversy in the 1940s and contrasts his methodological views with those of Hutchison. The third section examines Friedman's (1953) showing how he begins to depart from Hutchison's views and, in doing so, adopts instrumentalism. The fourth section finally takes up the Hutchison-Machlup debate showing how Machlup adopts conventionalist stratagems to further shore up Friedman's defence and to explicitly and finally deal with Hutchison. We hope to arrive at a clearer understanding of how Hutchison (1938) was interpreted, and the way in which it influenced the debate in economics, in the late 1940s and early 1950s.

5.1 Friedman's pre-1953 methodology and Hutchison

What we aim to do in this section is to show the extent to which Friedman's response to monopolistic competition embodied an empiricist approach remarkably similar to that of Hutchison. But first the key difference. In his desire to defend neoclassical theory Friedman adopted instrumentalism (Boland, 1979). In doing so he parted company with Hutchison who subscribed to Popperian fallibilism. According to Niiniluoto (1998: 181), 'fallibilism as an epistemological doctrine was born as a middle way between dogmatism and scepticism'. Apart from this, Friedman implicitly follows Hutchison's methodology, as described in Chapter Three. Given that Hutchison was not against economic orthodoxy *per se*, but only against its formulation in terms of an a priorist-deductivist-leaning methodology, and that his approach formed part of the early twentieth century resurgence of empiricism, such a

correspondence appears much more likely than Machlup's later depiction of Hutchison (1938) as representing an extremist fringe of methodological opinion.

As Hammond points out (1998: 197), Friedman studied under Burns, Viner, and Mitchell. Friedman was thus directly influenced by the two leading American institutionalists of his day. Furthermore, under Burns and Viner Friedman was introduced to another economist whose methodological views were in line with those of Hutchison: Alfred Marshall. In the following we argue that Friedman was close to Hutchison's approach in three directions (at least as compared to Machlup). First, and most importantly, Friedman sympathised with Hutchison's inductivist-empiricist approach as opposed to the hypothetico-deductive-empiricist approach. Secondly, they both wanted theory to be of practical assistance for policy guidance. Thirdly, their interest in methodology arose from their interest in economic issues rather than from the philosophy of science. Here we look at two examples of this similarity.

The first example concerns the development of econometrics: Friedman's (1941) criticism of Tinbergen's (1939a). For Friedman, the statistical equations in Tinbergen's model are 'an analogue of the Walrasian equations of general equilibrium' (1941: 658). He points out that Tinbergen's variables have been selected because they yield high coefficients of correlation (ibid: 659). Against this Friedman cites Mitchell's (1928: 266-7) criticism:

> A competent statistician, with sufficient clerical assistance and time at his command, can take almost any pair of time series for a given period and work them into forms which will yield coefficients of correlation exceeding +- .9 . . . So work of [this] sort . . . must be judged, not by the coefficients of correlation obtained within the periods for which they have manipulated the data, but by the coefficients which they get in earlier or later periods to which their formulas may be applied.

Friedman contends that Tinbergen makes no such attempt. These and other points are reasons why Friedman rejects Tinbergen's claim to provide 'an empirically tested explanation of business cycle movements' (1941: 660). While the formalisation of econometric techniques, boosted by Haavelmo (1944), led to Koopmans's (1947) depiction of Burns and Mitchell's (1946) as embracing 'outdated' quantitative techniques, Friedman (and Hutchison) would not have agreed.

The second example concerns developments in general equilibrium theory. Here we have several instances of Friedman's criticisms. Friedman (1941a) explicitly rejected of Triffen's (1940) attempt to set the theory of monopolistic competition in a general equilibrium framework. (Friedman also rejected the view that the more realistic assumptions of monopolistic competition gave rise to benefits which exceeded those of working with the admittedly less realistic assumptions of Marshallian theory since it meant giving up the benefits of the industry as the unit of analysis.) Friedman's reason for rejecting the move Triffen suggested was that it conflicted with his (and Hutchison's) methodological priorities. Like Marshall, Friedman's priority was on keeping hypotheses close to a factual basis and able to generate predictions (Hammond, 1998: 199). Triffen's suggested formalisation of monopolistic competition substituted this priority for another: abstractedness, generality and mathematical elegance (ibid).

In the same vein a few years later Friedman (1946) criticised Lange's (1944) work published by the Cowles Commission as being over formal and under empirical, two themes dear to both Mitchell and Hutchison. Friedman investigates the problem of why, despite Lange's 'brilliant display of formal logic', his 'analysis seems unreal and artificial' (1946: 277). Furthermore he points out that, according to Lange, 'only under very special conditions does price flexibility result in the automatic maintenance' of full employment (ibid: 281). Despite this, Lange holds that these 'very special conditions' were realized from the 1840s until 1914. This apparent contradiction emphasizes 'the fundamental weakness of [Lange's] kind of theorizing' (ibid: 282). He proceeds to outline three main criticisms of Lange's analysis.

Friedman's first criticism concerns Lange's theoretical approach. In the approach to theorizing in the physical sciences, the theorist starts with observed facts (ibid: 282). The theory is used to derive generalisations about the real world. 'A theory that has no implications that facts can contradict is useless for prediction' (ibid: 283). Lange's analysis does not start with observed facts, and it ends up with conclusions 'no observed facts can contradict'. Lange concentrates on logical consistency, not empirical application or test. 'The theory provides formal models of imaginary

worlds, not generalisations about the real world' (ibid: 283). This first criticism could have been taken directly from Hutchison (1938).

Friedman secondly points to the weaknesses of Lange's type of formal theorizing. In order to consider an indefinitely large number of variables, it has to oversimplify to such an extent that it loses empirical meaning. For example, while his classifications appear to have meaning, in order to apply them to his entire analysis, he is forced to define them in a way that eliminates their empirical content. Friedman finally points to 'errors of execution' fostered by Lange's approach. Some arise from a desire to simplify by ruling out possibilities purely by asserting they are unrealistic without presenting any empirical evidence. Others arise from a desire to be more realistic. But this increased realism is gained only by the sacrifice of logic. For example, although there is no place in Lange's system for time lags, or uncertainty concerning expectations about future prices, Lange attempts to introduce these (Friedman, 1946: 286).

Friedman concludes that Lange's analysis consists of unsupported empirical statements and theoretical conclusions not very relevant to the real world. The lack of relevance that derives from oversimplification and formal classification is concealed by the 'errors of execution' enumerated above. While the correction of these errors would make the analysis formally correct, it would make it clear that the analysis has only the remotest bearing on problems of policy (ibid: 299).

> The basic sources of the defects in Lange's theoretical analysis are the emphasis on formal structure, the attempt to generalize without first specifying in detail the facts to be generalized, and the failure to recognize that the ultimate test of the validity of a theory is not conformity to the canons of formal logic but the ability to deduce facts that have not yet been observed, that are capable of being contradicted by observation, and that subsequent observation does not contradict (ibid: 300).

As with the first criticism, Friedman's further criticisms and his conclusion above could all have been taken directly from Hutchison (1938).

Yet another instance of Friedman's concern with general equilibrium analysis is his (1947) review of Lerner's (1944) *Economics of Control*. Lerner analyses the problem

of maximising economic welfare. He is concerned with deriving the formal conditions for an optimum and the institutional arrangements for achieving these conditions. While it appears as if the book contains a programme for economic reform, the institutional proposals are 'almost entirely irrelevant' to the formal analysis (Friedman, 1947: 301). Friedman's chief criticism is that Lerner writes as if it is possible to base conclusions about appropriate institutional arrangements on the formal conditions for an optimum. 'Unfortunately, this cannot be done' (ibid: 316). It is possible to construct institutional arrangements which would permit the formal conditions for an optimum to be satisfied. However, this would not constitute a realistic appraisal of the economic problems involved.

While Friedman's criticism of Lerner and Lange's over-formal and under-empirical analysis is no doubt influenced by his work under Mitchell and by his reading of Alfred Marshall, we have been at pains to show how closely in line it was with Hutchison's approach. Not only did Friedman and Hutchison both pursue an inductivist-empiricist approach that was critical of abstract general theoretical systems, they were both interested in the ability of economic theory to provide practically relevant predictions for policy guidance. Finally it was this interest that led to their excursions into methodology. While we have not shown any direct influence running from Hutchison (1938) to Friedman's various writings in the 1940s, the closeness of the similarities appears to support a case for at least some indirect influence. More importantly, we are now in a position to show how, when we turn to consider Machlup's methodological interventions, Machlup's position contrasts with that of Friedman, and not only with that of Hutchison. This supports our argument that Machlup misrepresented Hutchison in their 1955-6 exchange. With these considerations in mind, we now turn to examine the nature of Machlup's intervention focusing on his exchange with Lester.

5.2 Machlup, the marginalist controversy, and Hutchison

Hutchison (1938) had disagreed with Robbins's categorisation of maximising behaviour and perfect knowledge as subsidiary rather than the fundamental assumptions of economics. In his key chapter four he had presented the maximum principle, perfect expectations and a tendency to equilibrium as 'the basic postulates

of pure theory'. The fact that these assumptions became the centre of two major controversies, the monopolistic competition and the marginalist or full-cost pricing, debates suggests Hutchison's indirect influence. This should have lent greater authority and stature to Hutchison's work. That it didn't was partly due to Hutchison's all too independently critical methodological stance regarding economic orthodoxy, but also partly to Machlup's (1955) portrayal of Hutchison's methodological position as extremist.

In this section we contrast Machlup's position with the three points held in common by both Hutchison (1938) and Friedman's 1940s interventions. We argue that Machlup is decidedly out of sympathy with the first point: their inductivist-empiricist stance. Instead he leans much more than Friedman towards hypothetico-deductivist empiricism. His priorities place abstractness and generality before the need to anchor hypotheses closely to fact. Regarding the second point, although he accepts the need for empirical testing, he is forever at pains to emphasise just how difficult it is to test predictions given the nature of the subject matter with which economics has to deal, and how difficult it is to apply economic theory to the world of experience (Machlup, 1939: 233-4). Regarding the third point, in contrast to either Friedman or Hutchison, Machlup explains how he was brought up in the deeply philosophical atmosphere of the Vienna associated with Wittgenstein, the Vienna Circle, Popper 'and wrote about methodology from the early 1920s' (Machlup, 1978: x). His background and orientation is more methodological and philosophical than either Friedman or Hutchison. For instance, it is difficult to imagine Hutchison in 1978 writing a fifty odd page chapter on 'What is meant by methodology' (Machlup, 1978, ch 1). Machlup used this background to mount a defence of neoclassical orthodoxy. In doing so he chose to cast Hutchison as an extremist.

In seeking to bring out these aspects of Machlup's methodological stance, we will focus mainly on his exchange with Lester since this allows us, in addition, to understand and appreciate how Machlup's (1955) engagement with Hutchison fits into, and arises from, the marginalist controversy.

The Lester-Machlup exchange of 1946

The Lester-Machlup exchange of 1946 that marked the re-emergence of the marginalist controversy appears to be the major direct factor leading not only to Machlup (1955) but also to Friedman (1953). In this section we will concentrate on Machlup's role in this controversy, leaving Friedman (1953) for the next section.

Lester (1946) points out that protests that firms do not operate on the marginal principle have failed to shake the confidence of textbook writers who have devoted more and more space to 'complicated graphs' (p 63).[1] A gap, however, exists between the marginal theory of the firm and general theories concerning employment, money, and the business cycle 'which may not mention the principle at all' (p 63). His paper represents a step towards bridging the gap. The conclusions of his paper are based mainly on 'written replies by 50 odd concerns to questions concerning the relative role of different factors in determining their employment . . . and probable adjustments to an increase in [relative] wages' (p 64). 'It is clear from numerous interviews that most business executives do not think of employment as a function of wage rates but as a function of output [demanded]' (p 67). Like Harrod's (1939) study, his study also found that entrepreneurs do not think in terms of marginal variable cost. They seem convinced that profits increase with output until capacity and have no faith in the validity of U-shaped marginal cost curves (p 70). According to the data collected, methods of manufacture do not readily adjust to changes in relative costs of productive factors (p 73). It is questionable as to whether wage reductions will lead to more employment. Specifically, according to the questionnaires, lower wages in the South have not led employers to use more labour and less machinery compared to the North (p 75). 'Unlike economists, business executives tend to think of costs and profits as dependent upon the rate of output, rather than the reverse (the rate of output as dependent upon the level of cost)' (p 81). His paper, he says, raises grave doubts as to the validity of conventional marginal theory and the assumptions on which it rests.

[1] Unsupported page references in this paragraph are to Lester (1946)

Lester (1946) is very much an empiricist intervention showing little patience with abstract theory and 'complicated graphs'. He challenges the neoclassical view that entrepreneurs attempt to maximise profits by adjusting output until marginal cost equals marginal revenue. It is simply not applicable to the world of business. His conclusion is based on the inductivist technique of studying the responses to questionnaires handed to business men. It represents a much more 'ultra-empirical' stance than Hutchison's (1938). Let us see how Machlup responds.

Machlup (1946) starts by pointing out that while critics revolt against marginalism, as the logical process of finding a maximum, it is clearly implied in the so-called economic principle - striving to achieve with given means a maximum of ends. The critics of business behaviour are not correct. The alleged inapplicability of marginal analysis is often due to a failure to understand it, or to a mistaken interpretation of findings (p 520).[2] Machlup divides his article into two sections: marginal analysis of the single firm, and empirical research on the single firm.

Regarding marginal analysis of the single firm, Machlup says that any attempt to test marginalist theory through empirical research presupposes full understanding of the theory. He argues that it does not give a complete explanation of the determination of output, prices, and employment. Rather, it explains the effects which certain *changes* in conditions may have upon the actions of the firm. The concept of equilibrium is a tool in this theory of change; the marginal calculus is its dominating principle (p 521).

Machlup now examines the relationship between marginal revenue and cost of output. Costs, revenues and profits are all subjective. 'Marginal analysis of the firm should not be understood as implying anything but subjective estimates, guesses and hunches' (p 522). While, for the business man, the range of possibilities for price and output variation is much narrower than the typical curves an instructor draws on a blackboard, this does not alter the principles of marginal analysis. In view of attempts to derive statistical cost curves from accounting data - which refers to the past - it should be noted that marginal cost and revenue concepts refer to expectations of future conditions (p 523). Marginal analysis rests on the assumption that the firm

[2] Unsupported page references for the remainder of this sub-section are to Machlup (1946).

attempts to maximise its profits (p 525). This is not to deny other non-pecuniary considerations. However, it is methodologically sounder not to reduce them to money terms since then whatever a business man does is explained by the principle of profit maximisation. The analysis would then acquire the character of a system of definitions and tautologies, and lose much of its value as an explanation of reality (p 526).

Machlup now turns to examine the relationship between marginal productivity and cost of input. The determination of output on the basis of factor cost and factor product is merely the reverse side of the above analysis, except that the significant magnitudes are units of factors and units of product. Almost everything regarding the analysis of output holds true, *mutatis mutandis*, in regard to the meaning of marginal productivity and marginal cost of input (p 533). Only the process by which marginal productivity may be derived seems so formidable that an analogy will help explain the apparent contradiction (p 536). Machlup's analogy is that of the automobile driver trying to overtake a truck. The explanation of his action must often include steps of reasoning which the acting individual himself does not consciously perform. 'To call, on these grounds, the theory invalid, unrealistic or inapplicable, is to reveal failure to understand the basic methodological constitution of most social sciences' (p 535). Equipped with this understanding of the meaning of marginal analysis, we may proceed to a discussion of the empirical findings which purportedly fail to verify it.

Let us examine the nature of Machlup's response. He contends that critics of marginalism fail to properly interpret or understand the theory. Machlup provides a more complex account of what the theory is about. This concern with the meaning of the theory appears to reflect a philosophical predisposition. His main point is that the theory deals with subjective rather than objective empirical factors, and furthermore, with expectations of these subjective magnitudes. He readily defends the abstractness of the theory using his automobile analogy. It should be apparent that this response is far from the inductivist-empiricist orientation of Friedman or Hutchison. A further point is that Machlup accepts that marginal analysis rests on the assumption of profit maximisation. This is to accept Hutchison's rather than Robbins's stress on this assumption as fundamental. While it is difficult to say if this reflects Hutchison's influence, it is interesting that Machlup also warns against the dangers of analysis

based on this assumption becoming tautological, a well-known theme of Hutchison (1938).

Regarding empirical research on the single firm, Machlup says the impressions gained from questionnaires which appear to form an empirical basis for doubting marginal analysis is due to a naïve acceptance of rationalisations in lieu of genuine explanations of actions (p 536). The vast majority of business men have never heard of expressions of such as elasticity of demand, marginal revenue or cost. How then can they be supposed to think in such terms? This does not imply that marginal analysis is unrealistic since it is usually possible to translate the terms. Answers to questionnaires are likely to be rationalisations in terms that make the actions appear plausible and justified to the inquirer (p 537).

Regarding average cost and price, the marginal calculus may be followed without pronouncing or knowing any of the terms in question. The average cost figures, despite their prominent place in our business man's statement, had no place in his actual decision which was based on the profitableness of the added business. Since accountants have emphasised the point that selling price must cover average cost, 'is it then surprising that business men try to explain their pricing methods by average-cost considerations?' (p 540). Contrary to the critics many of their findings confirm rather than contradict marginal analysis (p 545).

Whereas the average cost theory has been advanced as a substitute for the marginal theory, no substitute has been put forward from those who decried marginal productivity and wage theory. In any case, statistical studies of the relationship between wage rates and employment would be nearly useless because we have no way of eliminating the simultaneous effects of several other significant variables, especially those of a psychological nature (p 548). Lester's questionnaires on employment, variable cost and adjustments all suffer from many weaknesses.

Machlup concludes that the marginal theory of business conduct of the firm has not been disproved by recent empirical tests (p 553). Furthermore, empirical research cannot assure useful results if it employs the method of mailed questionnaires, if it is

confined to direct questions, and if it aims at testing too broad and formal principles rather than narrowly defined hypotheses (p 554).

What is of interest here is Machlup's response to the charge that businessmen say they do not consider marginal revenue or cost in their business practice. He claims that they do, but do so unconsciously. The reason they say they do not is that they are unfamiliar with these technical terms of economic theory. The replies to the questionnaires can therefore be interpreted as confirming the marginal theory of the firm. Here Machlup takes issue with Lester not by resorting to empirical evidence of any kind. Instead he argues along a priori lines that Lester wrongly interprets the responses to his questionnaires. In doing so, Machlup is at the same time arguing in favour of the realism of the assumptions of the theory, but on a priori and introspective, rather than empirical, grounds (1955: 17; Blaug, 1980: 105). Friedman (1953) was to change this.

We are not attempting a thorough interpretation of Machlup's complex methodological position in so short a space. Rather, we are simply drawing attention to the extent to which there are differences between his and that of Friedman's (1953) responses to criticism of the marginal theory of the firm. Our aim in this has been to show that, while Friedman and Machlup were equally keen defenders of neoclassical theory, the pre-1953 Friedman was considerably more sympathetic to empiricism than Machlup. Machlup's criticism of Hutchison needs to be seen in this light. We now turn to Friedman (1953).

5.3 Friedman's 1953 essay and Hutchison

In noting similarities between Friedman's pre-1953 methodology and that of Hutchison, we focused on the extent to which Friedman shared Hutchison's inductivist-empiricism and his view that theory should be practically useful. This led to their being critical of formalist tendencies in economics: abstractedness, generality and mathematical elegance pursued as ends in themselves. By contrast we have seen in the previous section that Machlup was far more at home in explaining the need for abstraction and generality in economic theory.

According to Hammond (1998), Friedman's methodology involved (1) a rejection of the (Walrasian) formalisation of theory and (2) a rejection of the assumptions of monopolistic competition as representing progress in economic theory. We have described how Friedman responded to the first point in his pre-1953 phase. We note in passing that Friedman (1955: 908) again criticised Walras for favouring form over substance. He suggested that, like Marshall, we favour substance over form.

We now turn to describe how Friedman parted company with Hutchison on that long-standing issue in the history of economics: the realism of assumptions. It is widely agreed that the central theme of Friedman (1953), 'the centerpiece of postwar economic methodology', concerned the 'irrelevance-of-assumptions thesis' (Blaug, 1980: 103). Many also (including Friedman himself) regard this central argument as involving a mainly instrumentalist methodology (Boland, 1979; Caldwell, 1982). Boland has pointed out that conventionalist and inductivist elements are also to be found in Friedman's essay (1979: 507-8). In this section we will be concerned with the extent to which, apart from his instrumentalism, Friedman's (1953) is in agreement with Hutchison's methodology. The view of Hutchison's (1938) as representing an extremist position needs to be revised, and to be seen as the viewpoint of someone, namely Machlup, highly sceptical of empiricism.

Friedman's Essay

In examining Friedman's essay, we focus on two aspects. Concerning the first aspect we argue that Friedman still follows a path broadly similar to Hutchison. It is with regard to the second aspect that they part company.

A. The first aspect relates to Friedman's characterisation of positive economics. Friedman states that his essay is concerned with the 'problem of how to decide whether a suggested hypothesis should be tentatively accepted as part of [the positive science of economics]' (p 3).[3] This echoes Hutchison's problem in 1938. Given the confusion surrounding Keynes's (1891) distinction between economics as a positive and normative science, Friedman attempts to clarify the relation between these

[3] Unsupported page references in this sub-section are to Friedman (1953).

branches. The task of positive economics 'is to provide a system of generalizations that can be used to make correct predictions' (p 4). Positive economics 'is, or can be, an "objective" science, in precisely the same sense as any of the physical sciences' (p 4). While there are differences on issues of policy, these differences derive from positive rather than normative issues and so can be resolved by progress in positive economics.

Hutchison would be in enthusiastic agreement with Friedman's concern with predictions as the goal of economic science. However, as we discussed in Chapter Three, even in 1938 Hutchison never went as far as Friedman in his adherence to the naturalistic, or unity of science, thesis. Later, Hutchison described Friedman's adherence to this thesis as 'thoroughly "naturalist"' (1960: xii). Here Friedman merits the extremist tag far more than Hutchison, a point missed by Machlup. Hutchison (1954) considers that Friedman is rather 'optimistic' in his view that policy differences derive from the positive, rather than the normative side. (Regarding this well known distinction, Boland (1979: 507) points to its inductivist origins.)

Friedman goes on to describe theory as both a 'language' and 'a body of substantive hypotheses designed to abstract essential features of complex reality'. As a language it is no more than a set of tautologies designed 'to serve as a filing system for organizing empirical material' (p 7). As a body of substantive hypotheses, it 'is to be judged by its predictive power' (p 8). Only factual evidence can show whether it is to be tentatively 'accepted' or 'rejected'. Since the number of hypotheses is infinite while the number of facts is finite, there will generally be a number of hypotheses consistent with the facts (p 9). Choice between these hypotheses will be based more on considerations of 'simplicity' and 'fruitfulness' than on logical consistency (p 10). While the inability to conduct controlled experiments hinders the testing of theories by the success of their predictions, the real significance of non-experimental evidence is that it is 'far more difficult to interpret' (p 10). This makes the 'weeding-out of unsuccessful hypotheses slow and difficult' and fosters 'a retreat into purely formal or tautological analysis' (p 11).

So far Friedman could well have been reflecting the methodology proposed by Hutchison (1938). They both view the ability of a theory to yield predictions as vital

if the theory is not to 'retreat into purely formal or tautological analysis'. Friedman's distinction between theory as a language and a body of substantive hypotheses corresponds, if only very roughly, to Hutchison's distinction between pure and applied theory. On this point Caldwell (1982: 176) interprets Friedman's reference to theory as a language as his way of describing theory as a hypothetico-deductive system. Thus interpreted a theory has no meaning until certain empirical counterparts are designated (presumably via the indirect testability hypothesis). This, Caldwell claims, is a step Hutchison failed to take. We have argued in Chapter Three that Hutchison retained a sceptical stance towards hypothetico-deductivism. We are not dealing here, as Caldwell seems to imply, with someone who does not properly understand the role of the indirect testability hypothesis. Rather we are dealing here with someone who maintains a healthy dose of inductivism is needed in scientific investigation. In line with this interpretation, Friedman's emphasis on the importance of factual evidence would be enthusiastically endorsed by Hutchison. His seemingly inductivist point that empirical evidence is important in constructing and not only in testing hypotheses likewise would meet with Hutchison's, but not with Machlup's, approval. At this stage Friedman expands on his earlier irrelevance-of-assumptions thesis and in doing so parts company with Hutchison.

B. Earlier, Friedman had stated his controversial irrelevance-of-assumptions thesis: 'the only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience' (pp 8-9, original emphasis). Friedman now points out that a further difficulty of gathering evidence for testing implications (ie apart from the 'retreat into purely formal analysis') is that it tempts one 'to suppose that hypotheses have not only "implications" but also "assumptions" and that testing these against "reality" amounts to a test which is *different from* or *additional to* the test by implications' (p 14, original emphasis). Such a view is fundamentally wrong. 'In so far as a theory can be said to have "assumptions" at all, and in so far as their "realism" can be judged independently of the validity of predictions, the relation between the significance of a theory and the "realism" of its "assumptions" is almost the opposite of that suggested by the view under criticism . . . in general, the more significant the theory, the more unrealistic the assumptions (in this sense)' (p 14). (The converse does not hold: assumptions that are unrealistic do not guarantee a significant theory (p 14, n 12).) Rather than be 'descriptively realistic', assumptions should be 'sufficiently good

approximations for the purpose in hand' and this depends on whether the theory yields sufficiently accurate predictions. 'The two supposedly independent tests thus reduce to one test' (p 15). Disregard of the above led to the development of the theory of imperfect competition and to the discussion on marginal analysis in the *American Economic Review*. Later (p 30), he again points out that his essay relates directly to criticism of orthodox economic theory as 'unrealistic' citing Veblen (1898) and Oliver (1947). On this issue he could well have cited Hutchison (1938).

Hutchison (1960: xiii) admits difficulty in trying to make sense of Friedman's seemingly radical solution to criticism of the assumptions of orthodox theory as unrealistic. He points out that Friedman himself (p 23) draws attention to ambiguity surrounding the concept of assumptions.[4] Yet, despite failing to clear up this ambiguity, Friedman proceeds to 'blast' attempts to test theories by examining the 'realism' of their assumptions. Moreover, Hutchison contends, Friedman appears to reintroduce the concepts of 'assumptions' and 'realism' under different names. For example, Friedman stresses that 'full and comprehensive evidence' is vital 'in constructing hypotheses' (pp 12-13). Hutchison argues that when Friedman later (p 23) outlines the positive roles that assumptions play in theory, he is implicitly concerned with the realism of the assumptions of a theory (1960: xiv).

What follows from our discussion of Friedman's essay, especially the first aspect (A), is the somewhat surprising extent to which Hutchison is in agreement with Friedman's methodology. Regarding the second aspect (B) of Friedman's essay Hutchison's major reservation concerns the irrelevance-of assumptions thesis. Here his conclusion is that his disagreement with Friedman may be purely verbal. Elsewhere he complains that Friedman's defence of the maximisation-of-returns hypothesis is apt to come 'perilously close to presenting theory as a pure tautology' (Hutchison, 1954). In particular, Hutchison notes, Friedman appears to reject the procedure of using questionnaire studies of business behaviour as a possible way out of this peril.

[4] Here Musgrave (1981) would have done much to clear up this ambiguity.

In terms of our adoption of Latsis's classification, Hutchison's disagreement with Friedman is not verbal, but rather stems from Friedman's apologetic defence of existing price theory. In this defence Friedman resorted to instrumentalism. Hutchison, by contrast, follows Popper's fallibilistic approach which underlies his critical attitude to 'existing price theory'. This is why Hutchison (1960: xiv), for example, finds Friedman's account of explanation or prediction inadequate. As Blaug has noted, Friedman is 'not really interested in testing the maximisation-of-returns hypothesis and is instead seeking to confirm it (1980: 117). We end this section by noting Friedman's acknowledgement that his methodological essay was a direct response to challenges posed by the theories of monopolistic competition and full cost pricing to orthodox price theory (pp 30-1). This provides evidence for our view that Machlup's 1955-6 response to Hutchison is properly interpreted as an integral part of Friedman and Machlup's defence of orthodoxy.

5.4 Hutchison's 1955-1956 exchange with Machlup

In this section of the chapter we aim to show how the a priori-leaning nature of Machlup's defence of the orthodox theory of the firm is continued in his 1955 article on verification in economics. In this article we argue that, while Machlup sought to present his own position as a sensible middle of the way one between the extremes of a priorism and empiricism (Machlup, 1955: 17, n 42; 1956: 485), it is in fact much closer to a priorism than he makes it out to be. To the extent that this is true, it points to the view that the exchange between Machlup and Hutchison is more accurately represented as yet another instance of the long-standing *methodenstreit* in economics between empiricism and rationalism. We argue that Hutchison should be viewed as a reaffirmation of the empiricist stand in the wake of the then recent resurgence of rationalism in the form of Robbins (1932, 1935). Machlup misrepresents Hutchison as an extreme empiricist when, as we have seen in the last few sections, he is no more extreme than Friedman. Yet Machlup (1955), far from distancing himself from Friedman, embraces his 1953 essay and defends it in his own 1955 intervention (1955: 17, n 42). While Machlup presented the disagreement regarding the 'problem of verification' as one between methodological extremes, issue at stake also concerned the apologetic defence of neoclassical theory. As such Machlup's sophisticated methodological arguments were the means to this end.

Before turning to examine these arguments, we take a more general look at Machlup's methodology. As mentioned before, we do not attempt the ambitious task of interpretation, but instead limit ourselves to such further clarification as will help us in understanding his 1955 attack on Hutchison. To this end, we briefly examine Latsis's (1976) classification of Machlup's position as 'conventionalist'. We drew attention in our Introduction to the two very different senses in which the term 'conventionalism' is used. Latsis (1976) and Boland (1979) appear to be primarily concerned with the extent to which conventionalist stratagems can be used to defend some particular dogma in the face of Popperian falsificationism.

According to Latsis (1976: 9), 'conventionalism grew out of Kant's apriorism'. While it agreed with Kant that the mind interprets experience within a framework, this framework was not fixed according to Kant's categories, but could be adjusted to interpret any experience. Pareto (1909), following Poincaré (1902), introduced conventionalism to economics. In their defence of the 'neoclassical research programme', Machlup (1946, 1952, 1955) and Friedman (1953) have presented conventionalist methodologies.[5] Latsis identifies Machlup's view - that 'qualitative indirect testability' rather than (empirical) falsifiability is the appropriate criterion for appraising a theory - as an example of conventionalism (1976: 10). As such he [Machlup] 'repeatedly argues that counter-intuitive and apparently refuted assumptions may nevertheless be valuable for explanation and prediction in neoclassical microeconomics' (ibid: 10). For Latsis, Machlup's response to the potential empirical falsification of a theory nicely reflects 'the defensive attitude of conventionalist methodology':

> When there is an apparent conflict between observations and the theory they are supposed to test, the observations can usually be disqualified as of uncertain reliability; and where this will not do the conflict can usually be reconciled by means of auxiliary hypotheses (Machlup, 1952: 73, cited in Latsis, 1976: 11).

[5] Latsis (1976: 12) identifies Friedman's conventionalist methodology as corresponding to what Samuelson (1963: 232) has called the 'F-twist': 'to be important . . . a hypothesis must be descriptively false in its assumptions'. This is a stronger version of Friedman's irrelevance-of-assumptions thesis.

It is this unwillingness in principle to allow empirical observations to 'disqualify' theory that offends Popperian-minded economists such as Latsis and Boland, and, long before them, Hutchison.

While Machlup (1978: 460) happily accepts Latsis's classification of him as a conventionalist, he interprets such a label in the very different, and more general, sense of conventionalism referred to earlier. Conventionalism in this sense is a nominalist rather than a realist interpretation of concepts, unobservable terms, and propositions in scientific theories. We saw in Chapter One that, while Poincaré applied a conventionalist interpretation to concepts and unobservable terms, he was far more cautious about extending such an interpretation to propositions and laws. By contrast, Machlup is quite happy to label, what he specifically terms 'basic propositions' as no more than conventions. This, we venture to say, is probably because his methodological position, as we have mentioned before and attempt to highlight when we turn to his 1955-6 exchange with Hutchison, encompasses significant a priorist elements. And it is these a priorist elements, rather than Machlup's conventionalism, that we argue underlie his attack on what he appeared to regard as the then most significant statement for the inductivist-empirical-historical side of the *methodenstreit.*

Machlup's problem of verification in economics

In our examination of Machlup (1955) we consider two main points. First we focus on his classification (and criticism) of (extreme) methodological positions in economics. Second we turn to his argument that not all assumptions, and particularly not 'fundamental' assumptions, need to be verified.

A. Machlup's avowed aim is to settle the controversial issue of what kinds of propositions can be verified, and whether or not all scientific propositions should be verified or at least verifiable. He presents two extreme positions concerning, what he terms, 'the problem of verification in economics'.

At one end of the spectrum economic science is regarded as

> a system of a priori truths, a product of pure reason, an exact science reaching laws as universal as those of mathematics, a purely axiomatic discipline, a system of pure deductions from a series of postulates, not open to any verification or refutation on the ground of experience (p 5).[6]

Writers in this mould include von Mises (1949), Knight (1924), Weber (1949), Robbins (1935), and even further back by Cairnes (1875), Senior (1836) and John Stuart Mill (1836). While the position as described above might sound 'provocative', Machlup (somewhat strangely!) assures the reader that such (a priorist!) writers were quite happy to test the predictions of economics and objected only to testing the assumptions of economic theory in isolation (p 7).

Here we note Machlup's immediate defence of this extreme position: it is, he argues, not really 'provocative'. Indeed, it appears to coincide remarkably with the so-called moderate view in which Machlup casts himself which accepts testing of predictions, but not independent tests of assumptions! The same treatment is not meted out to 'extremists' at the other end of his spectrum.

This is the position of ultra-empiricism, or the view that propositions at any level of analysis must be independently verifiable. This extreme empiricism, associated with William James's (1912) radical empiricism, is the one usually discussed and criticised in philosophy and reflected in the historical, institutional and quantitative schools of economic thought (p 8, n 26). The assumptions of economic theory are criticised as unrealistic and the hypothetico-deductive theoretical system built upon such assumptions as tautologous, without empirical content, predictive or explanatory significance and therefore without application to the real world (Hutchison, 1938: 166 and 120). The ultra-empiricist rejects indirect verification of hypotheses, that is, verification of the results deduced from these hypotheses combined with certain factual assumptions. Instead, he insists on the direct verification of assumptions at every level of analysis (p 8).

Interestingly for our thesis, we note that Machlup associates 'ultra-empiricism' with historical, institutional and quantitative schools of thought. This contrasts with Quinton's (1993) description of the most extreme form of empiricism (see

[6] Unsupported page references in this sub-section are to Machlup (1955).

Introduction, pp 8-9). Further examination of this matter would take us beyond our topic. If Machlup is happy to label (rather than merely associate) historical, institutional and quantitative schools of thought 'ultra-empiricist', we would be only too willing to accept his classification of Hutchison as 'ultra-empiricist' since this would accord entirely with our view of Hutchison and also with our view that Machlup stands opposed to the inductive-historical-empirical tradition in economics. Yet Machlup seems to imply that Hutchison's position is empiricist in the logical positivist 'scientific' sense and it is here that we differ sharply from Machlup. As an aside it should be noted that Machlup neglects to include Knight (1921: 199, n 1) as one of the adherents of radical empiricism. While, as we argue throughout, he incorrectly cites Hutchison as his lone example of an ultra-empiricist, he correctly refers to Hutchison's scepticism concerning the hypothetico-deductive system, but neglects to say that this scepticism particularly concerns the extent of its application to economics. As we shall see, when Hutchison later replies, he cites the passage in his 1938 book in which he explains that propositions need not be directly testable (Hutchison, 1956: 476-7).

Machlup criticises ultra-empiricists for not distinguishing between hypotheses on different levels of generality. While specific assumptions are intended, fundamental assumptions are not intended, to be independently testable. The fundamental hypotheses are also known as heuristic principles, basic postulates, or useful fictions. They are acceptable so long as they generate successful predictions. According to Einstein, Newton's laws of motion rest upon assumptions which cannot be independently tested (Einstein and Infield, 1938: 33). Instead, the theoretical system is tested by testing the lowest-level hypotheses (p 10). Following from the above, the fundamental assumptions of economics such as rational maximising behaviour do not themselves need to be empirically tested. This assumption is better understood as an idealisation or even as a 'complete fiction with only one claim: that reasoning *as if* it were realised is helpful in the interpretation of observations' (p 11). Fundamental assumptions are inextricably bound up with the theoretical system which they support and are rejected only together with such an entire system. ' A theory is only overthrown by a better theory, never merely by contradictory facts' (Conant, 1947: 36).

Here it is interesting to note that Machlup argues more strongly than Friedman, by using an analogy from natural science, that the fundamental assumptions of economics are properly understood as useful fictions which facilitate 'as if' reasoning. Friedman (1953) had argued along similar lines, with one key difference which we will explain in our comment on the next part (B) of our examination of Machlup's paper. At this stage we wish only to remind ourselves that viewing assumptions as useful fictions is not in accordance with the logical positivist interpretation of the hypothetico-deductive method. The indirect testability hypothesis, which is formulated in terms of this method, says that assumptions need not be directly testable, not that it is of no concern if they are obviously empirically false (Caldwell, 1982: 177).

B. Machlup now proceeds to his main argument designed to show that not all assumptions need to be verified. In doing so he draws upon the hypothetico-deductive theory of scientific method. He does this by presenting a model of an analytical system with various types of assumptions. The system is viewed as a machine (Pure Theory) with the input being an Assumed Change and resulting output a Deduced Change. The machine helps find a cause for explanations and an effect for predictions. The parts of the machine are made up of assumptions or hypotheses of different degrees of generality. The fundamental assumptions represent fixed parts of the machine. Assumptions about the conditions under which the Assumed Change must operate (the Assumed Conditions) represent exchangeable parts. Machlup now proceeds to discuss the observational status of the various assumptions and the requirements of verification (p 12).

While the theoretical system can be applied where only *either* the Assumed or the Deduced Change are identifiable, to verify the entire theory *both* changes must be identified. A more casual approach to verification of the Assumed Conditions will suffice for most types of problems. Machlup lists three types of Assumed Conditions (type of case, type of setting and type of economy) in order to show why strict verification is not required. For example, with regard to type of case the analyst may assume perfect competition. If the results of his model accord reasonably well with observed conditions, he may retain this assumption even though he knows it is

contrary to the real situation since it avoids the complications of a more realistic hypothesis (p 16).

Machlup now turns to what for him is the important part of his model. This is the part that consists of the Assumed Type of Action, or Motivation which forms the fundamental postulates of economic theory and therefore does not require independent verification. The fundamental postulates have been termed the 'economic principle', 'maximisation principle', the 'assumption of rationality' and so on. Their logical status has been characterised as 'self-evident propositions', 'axioms', 'a priori truths', 'facts of immediate experience' and so on. While these various characterisations appear to be inconsistent, there is little agreement as to the relation between such terms in the natural sciences, and even less in the social sciences 'where man is both observer and the subject of observation!' (p 16). The essential difference between the natural and social sciences is that in the latter the data of 'observation' are 'themselves the results of interpretations of human actions by human actors' (p 16). This means that in the social sciences the types of action used in various models must be 'understandable' in the sense that it is conceivable that a sensible man could act according to the type of action assumed. From this perspective, the fundamental assumptions of economic theory should be *understandable* rather than independently empirically verifiable (emphasis added). According to Machlup, the only serious flaw in Friedman (1953) is that he disregards this requirement (p 17, n 42).

Here we can now comment on the difference between Machlup's and Friedman's proposals to view the fundamental assumptions of economics as useful fictions which facilitate 'as if' reasoning. We saw that such a view cannot be justified in terms of the logical positivists' indirect testability hypothesis. Friedman, we saw in our previous section, justified his irrelevance-of-assumptions thesis by recourse to an instrumentalist interpretation of theories. Machlup's approach to this question differs from both Friedman's and the logical positivists'. He rejects the straw man view that such assumptions be independently empirically verifiable. He does not go along with Friedman's instrumentalism. Instead, he maintains, while the fundamental assumptions are properly interpreted as useful fictions, they should be 'understandable' in the sense that it is conceivable that a sensible man could act

according to the type of action assumed. (Remember, for Machlup, Friedman's only serious flaw is that he disregards this requirement.) This fits in with Machlup's view that the data of the social sciences are 'themselves the results of interpretations of human actions by human actors'. As such the fundamental assumptions are not open to objective empirical verification, independent or otherwise.

Machlup's emphasis on this point appears to be a red herring. It diverts attention from his adherence to a priorism, subjectivism and the *verstehen* doctrine. As Blaug (1980: 105) has noted, assumptions can be realistic in terms of the *verstehen* doctrine, that is, in terms of ascribing motives to economic actors 'we' can understand. Friedman, unlike Machlup, rejects this interpretation. Instead he adopts instrumentalism and so argues that it is valid to impute 'as if' motives to economic actors 'that they could not possibly hold consciously' (ibid). The important point that follows from all of the above is that Machlup in fact adopts a far more subjectivist, a priorist and *verstehen* stance than the middle of the (methodological) road position he pretends to in his 1955-6 articles. Instead of Machlup's view of himself in the middle of his spectrum and Hutchison at the extreme, we hope to have shown that it is Machlup's position that is extreme, and Hutchison's that is more moderate.

Hutchison's reply

Hutchison (1956) begins by pointing out that Machlup distinguishes between two schools of thought, a priori and ultra-empiricist, on the subject of verification in economics. He characterises the latter as insisting on the independent verification of all assumptions at any level of analysis, and as rejecting any indirect verification. Although he claims he could give dozens of examples of the ultra-empiricist position, he chooses to single out only Hutchison (1938).

Hutchison contends that he has been completely misinterpreted by Machlup. Far from espousing ultra-empiricism, his viewpoint 'explicitly denies' such a position. In his book, he makes it clear that, by no means, is he insisting on the verification of all assumptions. Indeed, he is arguing only that the 'finished propositions' of economics 'must conceivably be capable of empirical testing *or be reducible to such propositions* by logical or mathematical deduction' (1938: 9). Regarding the fundamental

assumption, he states that it does not matter in principle whether it is tested directly or indirectly by working back from tests of the conclusions (p 481).[7]

Here Hutchison clearly protests against Machlup's characterisation of his position and demonstrates that he is quite aware of the limitations of naïve inductivism from which Machlup implies his position suffers. Despite later (in his 1956 rejoinder) claiming only von Mises as representative of extreme a priorism, in 1955 Machlup, as we have seen, listed quite a few more representatives including Robbins and Knight. Furthermore he assured readers that, despite appearances, such an extreme position was more reasonable than it sounded. By contrast, Hutchison remained the sole representative of extreme empiricism which Machlup made no attempt to defend as at all reasonable.

Hutchison proceeds to point out that Zeuthen (1955: 8) argues that direct or indirect testability is a necessary requirement for statements to be scientific. Zeuthen cites Samuelson (1947: 4) and Hutchison (1938: 9) in support and he 'makes it clear that he is quoting [Hutchison] in diametrically the opposite sense to Machlup's [interpretation]' (p 477). It would help to clarify Machlup's classification if he (Machlup) explained whether he regarded Zeuthen (1955), Samuelson (1947), Lange (1945-6), Little (1950) and Friedman (1953) as ultra-empiricists or as a priorists (p 478).

Hutchison complains that the trouble with Machlup's ultra-empiricist category is that his only example (namely, Hutchison) falls outside it, while the trouble with his a priorist category is that it is so broadly defined, ranging from J S Mill to Mises, that it is hardly significant or an extreme position. Indeed it seems as if this latter position covers the spectrum from extreme a priorism through varying degrees of empiricism right up to ultra-empiricism. However, it does not appear that making a distinction turn on whether or not 'indirect' testing is accepted 'could be at all serviceable'. For example, having measured two sides of the 90 degree corner of a triangular piece of ground, Hutchison suspects that there would be no, rather than Machlup's dozens of, ultra-empiricists who would insist on 'directly' measuring and testing the Pythagorean

[7] Unsupported page references in this sub-section are to Hutchison (1956).

proposition concerning the length of the third side. Moreover, no serious methodological error appears to be involved if an 'ultra-empiricist' actually measured and tested it (p 479).

We have already referred to Machlup's wide-ranging definition of extreme a priorism and will defer further comment until we turn to Machlup's 1956 reply. Meanwhile Hutchison's contention that there would be no ultra-empiricists lining up to test the Pythagorean proposition by direct measurement points to the extent to which Machlup's ultra-empiricism describes a hollow position.

Having dealt with Machlup's criticisms, Hutchison now examines Machlup's positive thesis, in particular, his conception of 'fundamental assumptions' or 'high-level generalisations' *in economics* (original emphasis: 479). Machlup's only example is that 'people act rationally, try to make the most of their opportunities, and are able to arrange their preferences in a consistent order; that entrepreneurs prefer more profit with equal risk' (Machlup, 1955: 10-11). These are all variations on the fundamental assumption of maximising or rational action. Unless Machlup can provide further examples of fundamental assumptions, the point at issue would appear to turn on the status and nature of this proposition about maximising behaviour.

Machlup describes this assumption as 'empirically meaningful'. This requires that the empirical content, or significance, somehow be specified. Yet, he contends, it requires 'no independent empirical tests but [instead] may be [a] significant step in reaching conclusions which are empirically testable'. While Machlup maintains that the conclusions are 'empirically testable', he does not show how these can be deduced with logical inevitability from 'empirically meaningful' assumptions about human actions (p 481).

> In short, while admitting the principle of indirect verification, we cannot agree to the loose and sweeping appeal to it which Professor Machlup seems to be making. Much more particularity and precision seems to be desirable (p 482).

For example, if we take a proposition of practical importance such as Walras's conclusion that 'free competition procures within certain limits the maximum of utility for society' exactly the reverse procedure to Machlup's appears to be required.

That is, one needs to work back from this conclusion to the assumptions involved, especially to the fundamental assumption of maximising or rational actions and enquire what would constitute a test of this assumption. The variant of the fundamental assumption that Machlup mentions, that 'consumers can arrange their preferences in order', was arrived at thanks to the efforts of a long line of economists to make the fundamental assumption testable. Yet, when Machlup describes the fundamental assumption as the notion that people act rationally it is not clear how this assumption can be tested or even whether it is testable. Nevertheless economists such as von Mises have used this assumption in arriving at 'wholesale political conclusions' backed up by the authority of economic science. Not only are Machlup's doctrines on verification in economics questionable in terms of economics, they may be used to defend a kind of politico-intellectual obscurantism that seeks to avoid the empirical testing of its dogmas (p 483).

Two points arise for comment. First, Hutchison criticises Machlup for not addressing the question of how he can be sure that the conclusions he is happy to test do in fact follow from 'empirically meaningful' assumptions. This may sound as if Hutchison is either citing a naïve inductivist objection or that he does not properly understand the indirect testability hypothesis. Indeed, Machlup's reply (to which we shortly turn) proceeds along these lines. Instead, we argue, Hutchison's criticism is another instance of his scepticism concerning the extent to which the hypothetico-deductive method can be applied in economics and hence that inductivist-leaning procedures in economics should not altogether be ignored. Second, Hutchison makes it clear that he particularly dislikes the authority of science to be used to support conclusions in the political arena when the science that is involved is not amenable to empirical verification. As in economics, Hutchison is willing to apply his demarcation criterion 'come what may' to both von Mises's 'scientific' grounds for supporting laissez-faire capitalism (Hutchison, 1938, appendix) and (later) to Marx's 'scientific' grounds for supporting socialism (Hutchison, 1981). Paradoxically it is this even-handedness of Hutchison that earned him more than the usual amount of criticism for his critics came from both, or all, sides. Hutchison's candour goes quite far in explaining the interpretation and influence of his 1938 intervention.

Machlup's rejoinder

Machlup (1956) responds by saying that, although Hutchison appears to reject some, he retains many, aspects of ultra-empiricism. For example, Machlup accepts that Hutchison rejects the position which he called ultra-empiricism in his 1955 article, that is, one which requires the direct empirical testing of the fundamental assumptions of a theory. Machlup notes that Hutchison requires only 'the *conceivable* testability of the deduced *consequences*', rather than the direct testing, of the fundamental assumptions of economic theory (1938: 9; 1956: 476). Yet, Machlup argues, Hutchison retains aspects of ultra-empiricism by effectively repudiating this position by much of what follows in his 1938 book and his 1956 note.

Machlup suspects the crucial misunderstanding between them is that Hutchison does not accept his interpretation of indirect testing. According to Hutchison, if a proposition is not directly testable then it must be 'reducible by *direct* deduction to an empirically testable proposition or propositions' (Machlup's emphasis). The way this is expressed implies that the consequences of any single proposition must 'be tested independently of those other propositions with which it is conjoined to constitute a case' (p 484).[8] But this goes against the essence of indirect testing. According to this, if assumption A cannot be tested, but A and assumption B together yield proposition C, and if C is empirically tested, then A has been indirectly tested.

Machlup 'suspects that Professor Hutchison does not accept the validity of indirect verification in this sense' (p 484). He now re-defines as ultra-empiricists those who reject the above formulation of the indirect testing of the fundamental assumption and insist that the assumption of profit maximisation is tested independently of the other propositions involved (pp 484-5).

Hutchison argues that his 1938 formulation of the Principle of Testability does, in fact, take account of Machlup's 1956 reformulation of what is involved in indirect testing:

[8] Unsupported page references in this sub-section are to Machlup (1956).

> That is, one can leave one assumption at a time, or one part of a hypothesis at a time, to be 'indirectly' tested, by testing a conclusion that follows logically from it, *and* any other assumption with which it is combined to yield this conclusion (1960: xv).

Here Hutchison appears to accept Machlup's interpretation of indirect testability arguing that Machlup is mistaken in viewing this as the crucial misunderstanding between them. However, the situation is not quite as straightforward. This is because Hutchison adopts an empirically stricter interpretation of indirect testability than does Machlup. In particular, he rejects completely Machlup's (1955: 7) view that 'we need not worry about independent verifications of the fundamental assumptions' (Hutchison, 1960: xv, n 1). Hutchison appears to interpret Machlup's statement as saying that we need not worry *at all* about independent verification - *at all* since, for Machlup, the fundamental assumptions are useful fictions - they could well be *empirically* false. Such an interpretation is consistent with Machlup's position as explained earlier. It is not a viewpoint acceptable to Hutchison:

> It really will not do for economists now to claim it as a demonstration of superior methodological wisdom, rising above the naïve demands of ultra-empiricism, to regard the generalization as scientifically corroborated whenever a rise in price is actually followed by a fall in the quantity demanded, without 'worrying or being very particular' about what had happened to consumers' objectives, tastes, expectations, incomes and other prices, that is, without any attempt at 'testing assumptions' (1960: xv-xvi).

With respect to Hutchison's charge that his classification of 'extreme a priorists' extends from the extreme through the middle ground right up to his classification of 'ultra-empiricists', Machlup replies that very few fall into this extreme category, and cites only von Mises. (In his 1955 article von Mises was joined by six other well known economists, among them Robbins.) In the middle ground Machlup cites Zeuthen, Samuelson, Lange, and Friedman and implicitly includes himself (cf Blaug, 1980: 109). Machlup maintains:

> none of them holds that no conceivable kind of experience could ever cause him to give up his theory, and none of them wants his fundamental assumptions tested independently of the propositions with which they are combined when the theory is applied (p 485).

Nevertheless, those occupying the middle ground hold very different and conflicting methodological positions. We have seen earlier in this chapter that Friedman (1946) had ten years before castigated Lange (1944) among others for the remoteness of the connection between his theory and experience. Later Samuelson (1963) was to deliver his famous F-twist criticism of Friedman. Aside from this, in formulating his descriptivist methodology (termed the poor man's version of instrumentalism by Blaug (1980: 113)) Samuelson came much closer than any previous economist to demanding that all assumptions be tested independently. Latsis (1972: 242, n 1) labels Samuelson's methodology 'classical inductivism' which he defines as 'the requirement of proving one's theories from facts'. Samuelson, in what Latsis aptly describes as his 'one or two too many' methodological contributions, appears to be the only, or at least the leading, candidate qualifying for Machlup's ultra-empiricist award.

Machlup now criticises Hutchison's two-fold classification of scientific statements into empirical and tautological. In particular, with regard to the maximisation postulate, Hutchison appears to regard only empirically falsifiable, and purely definitional, propositions as scientifically legitimate. If this is the case, then he rejects an intermediate category of propositions, 'the heuristic postulates and idealised assumptions', which are neither a priori nor a posteriori.

> Such propositions are neither 'true or false' nor empirically meaningless. They cannot be false because what they predicate is predicated about ideal constructs, not about things or events of reality. Yet they are not empirically 'meaningless', because they are supposed to 'apply' or correspond broadly to experienced events. They cannot be 'falsified' by observed facts, or even be 'proved inapplicable', because auxiliary assumptions can be brought in to establish correspondence with almost any kind of facts; but they can be superseded by other propositions which are in better agreement with these facts without recourse to so many auxiliary assumptions (p 486).

Machlup says that, while he regards the fundamental assumption as empirically meaningful, as an heuristic postulate it is not falsifiable (pp 486-7). It neither need, nor can be verified independently of the uses to which it is put in economic theory. Some economists hold that it need not be verified because common experience tells us it is self evident. However, common experience only tells us that we can, and usually, seek the highest returns, not that everyone always acts in this way. While this

assumption is bound to be sometimes disconfirmed by the facts, the problem is that we do not know the significance of such disconfirmations. The solution is to regard maximising behaviour as an heuristic postulate and to justify this postulate in terms of the successful predictions, or applications, of the theory (p 488).

As we saw in the previous chapter, the solution of viewing the fundamental assumptions as heuristic postulates had been mooted by Kaufmann (1942). According to Caldwell (1982: 145), it also emanates from Schlick. This is somewhat ironic given Machlup's antipathy to Hutchison's positivist 'ultra-empiricism'.

Machlup now makes the following statements in reply to Hutchison's various questions about the fundamental postulate. The assumption does have empirical content if it is 'to apply to empirical data of a certain class'. It applies to large numbers of, rather than particular, households or firms. It is considered tested if it, together with its theory, yields better explanations or predictions than any other assumption and theory. It would be regarded as disconfirmed if another theory not using this assumption 'worked equally well for a wider range of problems'. It is far from being superfluous: 'Never could a behavioristic approach provide all the millions of "entrepreneurial behaviour functions" which would be needed to do the job that is now done by the simple postulate of profit maximisation' (p 490).

Although Hutchison (1956: 480, n 5) believes he has found an ally in Friedman, he insists on direct testing of fundamental assumptions and so places himself in direct odds with the thrust of Friedman's methodological essay. According to Friedman, they should be tested indirectly by testing the predictions of the theory (p 491).

The above two paragraphs each give rise to a point for comment. Concerning the first, to the extent that Machlup implies that Hutchison adopts a behaviouristic approach, he is mistaken: this is a position Hutchison explicitly denied (1938: 143). Indeed, Hutchison's argument implies that there are plenty of stopping places within empiricism before behaviourism. Concerning the second, we have argued at length in this chapter that Hutchison is correctly viewed as much closer than Machlup to Friedman's methodological position.

Finally, Machlup deals with Hutchison's charge that his theory is tautological. According to Machlup, this may mean many different things. Machlup examines two: 'that the theory constitutes an internally consistent and closed system; that some of the assumptions are empirically empty'. Regarding the first meaning, Hutchison (1938: 36) himself recognises that 'pure theory' is necessarily tautological. Regarding the second, the assumptions that consumers and producers maximise expected utility and profits have been viewed as 'empirically empty' because (a) we cannot know whether or not agents really believe they are acting in the way which will maximise their returns; (b) whatever they do can be interpreted as maximising behaviour; and (c) 'we cannot deduce any particular way of acting from the assumptions standing by themselves' (p 492). Machlup contends that the issue is that the assumptions do not stand by themselves. When they are combined with others they 'may become of definite empirical significance'.

In dealing with this second point Machlup reiterates his main criticism of Hutchison: Hutchison does not accept his interpretation of indirect testing. In this interpretation the fundamental assumption is inextricably bound up with other propositions and it is the consequences only of this combination which can be tested. We have already described Hutchison's inductivist-empiricist reasons for being sceptical about separating the fundamental assumption by such a long way from direct empirical testing. While it appears that Machlup is appealing to the (respectable) indirect testability hypothesis of hypothetico-deductive empiricism, he is in fact not doing so. Rather, as we have seen, he appeals to a conventionalist interpretation of the fundamental assumption as a heuristic postulate (Kaufmann, 1942), or as an 'ideal type'. According to Caldwell (1982: 145) this move by Machlup was unnecessary given the indirect testability hypothesis. The question then is why Machlup makes this move. In terms of our interpretation, it is because Machlup's position, contrary to Caldwell (1982: 165), does not represent some sort of moderate hypothetico-deductivist empiricism (lying between the a priorists and the ultra-empiricists). Instead, his move is better explained in terms of Machlup's conventionalism.

Conclusion

In this chapter we have shown the remarkable extent to which Friedman's methodology follows in the tradition of Hutchison. While Friedman may not have been directly aware of Hutchison's (1938) arguments, it at least seems likely he read of his old teacher's (Frank Knight) 1940-1 exchange with Hutchison. We have emphasised Friedman's criticism of various formalist tendencies in both economic theory and econometrics in the period before his 1953 essay. We have also attempted to demonstrate the extent to which Friedman's (1953) essay is compatible with Hutchison's position. Indeed, far from Hutchison being an extremist and Friedman adopting a moderate position, on certain points Friedman represents a more extremist position than Hutchison (eg economics is an 'objective' science in exactly the same sense as the natural sciences (1953: 4)).

Hutchison's point of departure arises from Friedman's commitment to defend the neoclassical programme. He appears to believe that to do this he has to resort to his instrumentalist irrelevance-of-assumptions thesis. Yet, on this point Friedman has suffered something similar to the fate of Hutchison at the hands of Knight and Machlup. Blaug (1980: 111-2) has pointed out that Friedman is in fact not guilty of the extreme version of the irrelevance-of-assumptions thesis condemned by Samuelson (1963: 232-3) as the F-twist. Instead, if one reads Friedman's essay carefully he 'has only asserted that unrealistic assumptions are "largely" irrelevant for assessing the validity of a theory' (ibid). Nevertheless, it is the extent to which the irrelevance-of-assumptions thesis reflects Friedman's resort to instrumentalism in order to defend the neoclassical programme, that has most concerned his critics.

Apart from this traditional criticism, Friedman's view that *only* predictions should be tested implies that theories can be neatly separated into different entities (assumptions, predictions etc). This is too methodologically naïve (Blaug, 1980: 120, but see Caldwell, 1982: 148 for a contrary view). Machlup (1955) was thus obliged to step in and provide more sophisticated support for Friedman's essay. Machlup's intervention, we argue, simply furthered confusion surrounding the nature and possibility of empirical testing in economics. For example, it stands in contrast to the kind best exemplified in later years by Musgrave (1981) who showed that, while the

realism of some assumptions (eg heuristic) is meant to be ignored, the realism of other kinds of assumptions (eg domain and negligibility) cannot afford to be ignored.

This brings us to the role of Machlup in his co-defence with Friedman of the neoclassical programme. Despite the fact that Machlup casts himself alongside Friedman in the middle of the a priorist-empiricist spectrum, we have sought to demonstrate in this chapter the extent to which his interventions differed methodologically from those of Friedman. Machlup (1978: 460) himself has accepted Latsis's (1976) classification of his position as conventionalist 'in the sense of one who accepts as meaningful and useful basic propositions that make no assertions but are conventions (resolutions, postulates) with regard to analytic procedure'. While we do not take issue with this, we have emphasised in this chapter the extent to which Machlup's position contains elements of a priorism, the verstehen doctrine and subjectivism. This supports our view that the Hutchison-Machlup exchange represents another round in the long-standing *methodenstreit* in economics.

This contrasts with Caldwell's view of Machlup. Caldwell (1982: 163) agrees with Machlup's representation of his own position as in the middle between the a priorists and the ultra-empiricists. He points out that Machlup does not insist upon direct testing of the basic assumptions nor, like a priorists, does he claim that neoclassical theory is true by definition or everywhere applicable. We have argued that Machlup's position lies far closer to a priorism than Caldwell allows. In this respect Blaug's (1980) interpretation is more in line with our argument. For example, Blaug points out that 'Machlup, while urging the importance of empirical research in economics, is nevertheless keen to underline the inconclusiveness of all tests of economic hypotheses' (1980: 114). Blaug's response to these difficulties of testing in economics is to encourage economists to increase their efforts to surmount these problems:

> It implies that economists should concentrate their intellectual resources on the task of producing well-specified falsifiable predictions, that is, assigning less priority to such standard criteria of appraisal as simplicity, elegance, and generality, and more priority to such criteria as predictability and empirical fruitfulness (1980: 115).

However, Blaug points out that Machlup orders his priorities the other way round. Indeed, he has been 'singularly ingenious in discounting' empirical tests. Yet, Blaug protests, there is little point in commending empirical work, as Machlup certainly does, 'if it never really makes a difference to the beliefs one holds (1980: 115).

In contrast to the above interpretation of Machlup, Caldwell (1982: 158) gives credence to the significance of Machlup's notion of ultra-empiricism by contending that 'empirically minded economists' who insist on testing the rationality postulate should be labelled ultra-empiricists. If such economists do not accept the rationality postulate as an untestable hypothesis, they should either 'denounce their ultra-empiricist methodology, or reject the rationality assumption as metaphysical' (ibid). This is in line with Caldwell's verdict that, with respect to a priorists and positivists, Machlup's position is the most 'methodologically sound' (1982: 158). It should be clear that we do not accept such a conclusion.

Machlup has made out Hutchison to be the 'big bad wolf' naïvely insisting on extreme empiricism when, as we have argued, his methodological position is instead an eclectic and nuanced empiricism. In apparent reference to Hutchison, Blaug denies that there really ever was a big bad wolf who insisted on directly and independently testing the fundamental assumptions of economic theory:

> What critics of Friedman have argued is (1) that accurate predictions are not the only relevant test of the validity of a theory; (2) that direct evidence about assumptions is not necessarily more difficult to obtain than data about market behaviour used to test predictions; (3) that the attempt to test assumptions may help the interpretation of predictions; (4) that if testing theories by predictions only is all we can hope for, theories must be put to extremely severe tests (1980: 110).

Two points concerning the above passage arise for comment. First, the passage would be more accurate if Blaug were to substitute Machlup for Friedman since it is Machlup, as we have seen, who has been fielding the bulk of the criticisms. Moreover, the criticisms are more relevant to Machlup's than to Friedman's position. Second, the original and main critic of Machlup in this respect is Hutchison. Blaug's four points detailed above provide a far more accurate reflection of Hutchison's

methodological position than Machlup's attempt to represent him as an ultra-empiricist.

We have also pointed out that Caldwell sympathises with Machlup's 'methodologically sound' position. It is therefore interesting to note his description of Machlup as 'a contemporary representative of the dominant view [up to the 1930s!] of the role of empirical studies' (Caldwell, 1982: 166). This role is to determine whether or not a theory is applicable, rather than whether or not it is falsified (1982: 168). Caldwell hereby casts Machlup as a modern day representative of Robbins. Such an interpretation directly supports our view of Machlup. In particular it supports our contention that Machlup incorrectly labelled Hutchison an ultra-empiricist partly by misleadingly representing himself as occupying the moderate methodological ground. Rather than Machlup's representation of Hutchison as an ultra-empiricist, the 1955-6 exchange between Machlup and Hutchison should be interpreted as another round of the long-standing *methodenstreit* in economics. But, whereas before Hutchison's opponent Robbins explicitly presented himself as a priorist-leaning, this time Machlup obscured his a priorist tendencies with the cloak of a modern moderate empiricist stance.

CHAPTER 6

THE INFLUENCE OF HUTCHISON'S INTERVENTION: KLAPPHOLZ AND AGASSI

In the last two chapters we have viewed Hutchison as ranged first against Knight's, and then against Friedman and Machlup's, apologetic defence of orthodox economics. In this chapter we are mainly concerned with Hutchison's influence on the group of economists that attended the 'LSE Staff Seminar in Methodology, Measurement and Testing', or 'M^2T', in the late 1950s and early 1960s. According to de Marchi (1988: 141), the group, led by Lipsey and Archibald, wanted to replace Robbins's (1932, 1935) methodology, still dominant in economics especially in Britain.[1] We have already described this methodology in Chapter Two, but will briefly highlight certain aspects relevant to the group's response to Robbins as noted by de Marchi: the style of the Robbins seminar 'was the then common one of analytical dissection'; theory was viewed as a way of classifying the totality of possible cases; and 'models were examined for the realism of their assumptions and for internal consistency' (p 143).[2] On this point de Marchi quotes Harry Johnson's (1978: 158) remark about Cambridge in the 1950s: 'the examination of the realism or unrealism of analytical assumptions as a test of the validity of a theory . . . provided a basic technique of British theoretical discourse in the 1930s and on well into the 1950s'.[3] Theory was assessed in terms of whether or not it resulted in the shedding of light on a problem, rather than in terms of an empirical test requiring quantification of specific economic magnitudes.

In their search for an alternative to Robbins's methodology, the M^2T group was attracted to the ideas of Popper. Popper had joined the philosophy department at the LSE in 1946. While some members, eg Peston, indirectly picked up on Popperian ideas via an undergraduate logic and scientific method course given at the LSE, the

[1] Besides Archibald and Lipsey, the group included, amongst others, Klappholz, Foldes, Peston, Lancaster, Corry, and Steuer (de Marchi, 1988: 141).

[2] All unsupported page references in this introductory passage are to de Marchi (1988).

[3] Johnson presumably referred to realism in the Robbins sense of 'self-evident' or facts of everyday experience, or, in Lipsey's (1997: 213) description 'intuitively plausible'.

group as a whole was directly exposed to Popper via Agassi, a graduate student of his who was keen to proselytize. Agassi was introduced to the M^2T group by Klappholz. He tutored the group and, over about a six-month period, its members 'learned, and came to accept, much of Popper's views on methodology' (Lipsey, quoted in de Marchi, 1988: 148; cf Lipsey, 2001a). It was only after this Agassian experience that the group adopted a more formal approach along with the M^2T title (p 148).

It is clear that the M^2T group wanted to move away from Robbins's a prioristic approach and towards a more empirical one especially, they argued, if economics was to be practically applicable. Already in 1938, as we have seen, Hutchison had directly criticised Robbins's approach. He pointed out that the assumptions of much of economic theory were formulated, and interpreted, in an a priori way. As such it represented pure theory that was, and remained, analytical and tautological until joined to empirical propositions when it became applied theory. While he reserved a vital role for pure theory, Hutchison called for the adoption of a more empirical approach to economics.

In addition to challenging Robbins's methodology, Hutchison had also introduced Popperian ideas to economics. As de Marchi has pointed out, he should have been regarded as a natural ally by the M^2T group (p 146). Yet the group failed to build on Hutchison's early foundations. De Marchi identifies four reasons that, he argues, led these 'potential supporters' of Hutchison to largely neglect his intervention (pp 145 ff). The first was the perception that Hutchison was urging some kind of naïve inductivism and the second that his position rested on logical positivist foundations. The perception of Hutchison as a naïve inductivist arose, according to de Marchi, due to his apparent concern with unrealistic assumptions, and his stress on using ceteris paribus only together with strongly verified empirical generalisations (Hutchison, 1938: 46, 119). However, we argued in Chapter Five that Hutchison's concern with unrealistic assumptions was far from naïvely inductivist. Furthermore we have argued that, while Hutchison was indeed influenced by logical positivism, it was not central to his approach which drew more broadly both on the philosophy of science and on the history of the methodology of economics. This latter led him to recognise the significance of historical and institutional elements in economics. Consequently these first two reasons put forward by de Marchi are not very convincing.

By contrast the third and fourth reasons advanced by de Marchi carry more weight: Hutchison's intervention provided 'little guidance in showing how economics could be done in an empirical manner'; and the appearance of Friedman's 1953 essay. Regarding the third reason, de Marchi argues that 'Samuelson's *Foundations*, with its discussion of ways to make equilibrium propositions operational, held out a promise of getting beyond both Robbins and Hutchison' (p 146). It especially influenced Lancaster (1962) and Archibald (1961). Regarding the fourth reason, Friedman's 1953 essay, he suggests, appeared to release the group from concern with assumptions: only the testing of the implications of a theory was necessary. This view is supported by Lipsey (1997: 217) who has remarked on how many of the M^2T group were unhappy with the 'idea that we should argue about the plausibility of assumptions'. This move by Friedman was therefore not only popular, but also appeared to circumvent the problem of induction. 'To clinch the appeal, Friedman not only addressed methodological concerns, he also showed - much more so than Samuelson - how theory could be combined with quantification: *A Theory of the Consumption Function* [1957] was quickly embraced by the economics profession as an exemplar of quantitative economic analysis' (p 147).

While these reasons put forward by de Marchi go a long way towards accounting for Hutchison's relative neglect by the M^2T group, we want to draw attention to two further factors that, we argue in this chapter, played an important role in accounting for his lack of influence on the M^2T group. The first was Klappholz and Agassi's (1959) extreme, or ultra, Popperianism. In terms of their purist interpretation of Popper, they were against any methodological rules or prescriptions whatsoever (p 155). They accordingly criticised Hutchison for daring to propose a methodological rule: his criterion of testability. Yet, in terms of Latsis's (1972) distinction between those concerned with defending orthodox economic theory and those determined to subject it to Popperian-inspired methodological criticism, their broad sympathy with Hutchison (Klappholz and Agassi, 1960: 161) was far more important than their disagreement on a point internal to a Popperian-oriented methodology. Instead of putting these matters of economics and economic methodology first, they chose to pursue a primarily philosophical issue. That they did so appears to reflect the fact that Agassi, as a philosopher, was probably more concerned with philosophical issues for

their own sake than for economics. Nevertheless, their criticism sent out a misleading signal to the M^2T group of economists directly curbing Hutchison's influence:

> When we formed the M^2T seminar we discovered Popper and learned about him, and other methodological writings from Agassi and Klappholz. They were quite critical of Hutchison and for that reason I never read his [1938] book (Lipsey, 2001a).

As we will see later in this chapter, Archibald (1959) too, seems to have been influenced by Klappholz and Agassi in his critical stance towards Hutchison. It appears then that Klappholz and Agassi's views may have helped contribute to Archibald and Lipsey's overly ambitious attempts to apply a particular philosophy of science (Popper's) all too directly to economics (Archibald, 1961; Lipsey, 1963) - attempts that were abandoned after only a few years (Archibald, 1966; Lipsey, 1966). Had Hutchison (1938) received a more sympathetic hearing, his moderate interpretation of the extent to which Popper could be applied to the subject matter of economics might have prevented these unfortunate results.

The view, that much of the neglect of Hutchison by the M^2T group can be traced to Klappholz and Agassi's (1959) influence, fits in well with our Chapter Five discussion. Here we concluded that the influence of Machlup (1955) accounted for widespread misinterpretation of Hutchison as fixated on the realism of assumptions. Importantly for our argument, de Marchi points to the possibility that Machlup's (1955) labelling of Hutchison as 'ultra-empiricist' may have influenced the way he was interpreted by the M^2T group. Yet he does so only in a footnote (p 146, n 6). This, we argue, is to miss the significance of Machlup's influence on the M^2T economists and may be due, partly, to de Marchi not recognising the extent to which Machlup misrepresented Hutchison's position.

The second factor that affected Hutchison's influence on the M^2T group (and more widely) concerns a much broader and more fundamental issue: the difficulty of empirical testing in economics. (This relates to de Marchi's point that Hutchison provided 'little guidance in showing how economics could be done in an empirical manner'.) Hutchison (1938) had argued that, if economics were to be regarded as an empirical scientific discipline, its finished propositions needed to be testable. But, if

we grant Hutchison's point, this raises the issue of just how we are supposed to conduct empirical testing in economics. Two separate responses to this problem may be distinguished. One response is to narrow the scope for empirical testing. The obvious example here is Friedman's dictum to test only predictions. The other response is to widen as far as possible the scope for empirical testing. This appears to be Hutchison's response. The fact that the M^2T group, along with many other empirically-oriented economists, adopted the narrow rather than the wide approach, we argue, was an important factor in limiting Hutchison's influence. It also seems to have been a more important reason than de Marchi's view that the group were put off Hutchison due to perceiving his position as naïvely inductivist or logical positivist.

The tendency to narrow the scope for empirical testing found its expression in adopting a more formal approach to economic theory, and in econometric model building and testing. Archibald (1961) reformulated monopolistic competition theory with the aim of making the implications explicit and thereby testable, while Lipsey (1960) was more concerned with the formal quantification of theory (p 155). As we have learnt in the foregoing chapters of this thesis, Hutchison's response to the difficulty of empirical testing in economics was to widen its scope as much as possible, that is, to test as much as possible. Testing should not be limited to predictions only, but should be extended to assumptions as well. It should also not be limited to regression and other formal econometric model building approaches. This response attempts to take into account the historical and institutional aspects of economics, and the problem of the kind of uncertainty and ignorance, in the Knightian and Keynesian sense, which cannot be reduced to a probability estimate and so incorporated into econometric analysis. It is also one that is keenly aware of the extent to which economic problems are interwoven with wider social and political problems, so that an important part of empirical testing concerns events which are difficult, if not impossible, to model and test in a formal manner.

While Klappholz and Agassi's critical stance towards Hutchison and the fact that the M^2T group did not adopt Hutchison's response to the difficulty of empirical testing in economics represent important reasons for Hutchison's limited influence on the group, the group did not completely reject Hutchison's approach. As mentioned earlier, Klappholz and Agassi acknowledged that they broadly sympathised with, and

indeed respected and applauded, Hutchison's 1938 intervention (1960: 161). This being the case, and given the authoritative methodological position of Agassi, we should expect to find methodological similarities between work that developed out of the M^2T group and that of Hutchison. To the extent that there are such similarities, this would appear to lend support to our view that Hutchison (1938) attempted to introduce a more moderate empiricism into economics than has hitherto been made out to be the case.

This chapter is divided into four separate sections. In the first three sections we develop our argument that Klappholz and Agassi's criticism of Hutchison, and the M^2T group's response to the difficulty of empirical testing in economics, were important factors in limiting the influence of Hutchison's 1938 intervention in economics. In the first section we discuss the 1959-60 exchange between Hutchison and Klappholz and Agassi elaborating on our view that their intervention was overly concerned with philosophical matters leading them to under-emphasise the importance of Hutchison as an ally in a common Popperian-inspired methodological critique of orthodox economics.

In the second and third sections we show how the popularity of the narrow response to the difficulty of empirical testing in economics affected Hutchison's influence. In the second section we discuss Koopman's (1957) essay since this was both the main concern of Archibald (1959), and because Koopmans remarked on the similarities between his and Hutchison's methodological positions. While there appear to be definite similarities, it is Hutchison's response to the difficulty of empirical testing in economics that separates his approach from that of Koopmans who, like the M^2T group, adopts the alternative of deliberately narrowing its scope. In the third section we discuss Archibald's interpretation of Hutchison. This involves both the aspects of Klappholz and Agassi's criticism of Hutchison, and the difficulty of empirical testing in economics. Archibald (1959) shows signs of being influenced by Klappholz and Agassi's interpretation. He fails to give Hutchison a fair reading and lumps him together with the methodologically less sophisticated Oxford full cost theorists. Concerning the difficulty of empirical testing in economics, Archibald (1959, 1961) makes it clear that he favours narrowing, rather than widening, its scope.

In the fourth section we trace the extent to which the approach of the M^2T group was, in fact, in broad sympathy with that of Hutchison. To this end we point to two instances in which there are similarities with that of Hutchison's approach, namely, Klappholz and Mishan (1962) and Lipsey (1963). To the extent that Hutchison's approach is in line with work that developed out of the M^2T group, it lends support to our argument that he has been misrepresented as advocating extreme methodological views (given that the M^2T group's views were not regarded as extreme). Furthermore, it also lends support to our view that Klappholz and Agassi's (1959) criticism was something of a red herring that succeeded primarily in limiting the influence of Hutchison's (1938) intervention, rather than in contributing to progress in understanding how economics could be done in a more empirical manner.

6.1 Hutchison's 1959-1960 exchange with Klappholz and Agassi

In this section we argue that Klappholz and Agassi's (1959) criticism of Hutchison arose from an overly purist interpretation of Popper. More importantly, their criticism was concerned more with a philosophical end (clarifying and promoting their interpretation of Popper) rather than with using philosophical and methodological concepts in order to improve the practice of economics. Their philosophical interest moreover, was one in the philosophy of science rather than social science. Against this, Hutchison (1938) represented a methodology specifically attuned to economics. From the philosophy of science it drew not only on Popper, but also on the British empirical tradition and the writings of the Vienna Circle. In addition to the philosophy of science it drew on a long history of the methodological views of leading economists stretching back to Ricardo and Malthus thereby acknowledging the importance of historical and institutional considerations in economics. Although, as we shall see, Klappholz and Agassi's criticism of Hutchison dealt mainly with a particular philosophical issue, it unfortunately led to the impression that they were generally critical of his approach.

Klappholz and Agassi (1959) review two methodological works: Schoeffler (1955) and Papandreou (1958). They point out that the common theme of both these books is the contention that lack of progress in economics can be solved by adopting some or other methodological rule. Claiming that their view is that of Popper (1959),

Klappholz and Agassi contend that it is neither possible nor desirable to adopt methodological rules, apart from a continuing readiness to be critical of one's work (p 60).[4] They set out to demonstrate this contention by critically examining various methodological rules as put forward by Robbins (1935), Hutchison (1938) and Friedman (1953) before turning to the two books under review. Since our concern is with their criticism of Hutchison, we will omit discussion of Schoeffler (1955) and Papandreou (1958). However, given the intimate relationship of Hutchison's essay to Robbins and Friedman's interventions, it will be helpful to preface Klappholz and Agassi's criticism of Hutchison with a brief examination of their views on Robbins and Friedman.

Klappholz and Agassi criticise two of Robbins's methodological rules. First, Robbins proposed, as a methodological rule, an a priori separation between economics and other disciplines. They reject this since it implies a decision to regard certain factors such as tastes and technology as outside the scope of economics, or as exogenous, and so represents an a priori limitation of the field of economic discussion. Second, Robbins denied a priori the possibility of discovering quantitative laws. For Robbins, scientific laws are 'universal statements known with certainty to be true' (p 61). These laws are derived by deduction from a series of postulates which involve 'indisputable facts of . . . everyday experience' (Robbins, 1935: 8-9). In particular, Robbins explicitly denied they could be derived from history or controlled experiment (1935: 73-4). For Klappholz and Agassi, two points follow from Robbins's view that 'only laws guaranteed by everyday experience will be found' (p 62). First, certain variables, namely those not taken into account by everyday experience, will have to be classified as exogenous (eg Robbins's view on tastes). Second, we cannot guarantee the existence of other laws. In this vein, Robbins claimed that we could not discover quantitative laws.

Klappholz and Agassi point out that Robbins's position can be criticised 'by arguing that empirical statements, however well-grounded in everyday experience, are never indisputable' (p 62). While this might be damaging to Robbins's position, Robbins 'could still claim that, although in principle all of economic theory is disputable, no

[4] All unsupported page references in section 6.1 are to Klappholz and Agassi (1959).

reasonable person would dispute such statements as the law of demand, and that this law, and other similarly trivial statements, can explain much more than meets the eye' (p 62). Klappholz and Agassi respond by pointing out that, although the law of demand might be indisputable, it is in the same category as the law that metals conduct electricity: of little scientific or intellectual interest. The significance of economics, and other sciences, lies in the search for new truths (eg quantum theory in physics) which are controversial. They therefore choose to dissent from, rather than directly criticise, Robbins's position.

From Hutchison's perspective, the central issue concerning Robbins's methodology does not revolve around his insistence upon methodological rules, as Klappholz and Agassi contend. Instead it concerns its a priorist nature. Klappholz and Agassi's purist interpretation of Popper (which led them to focus on arguing against methodological rules) prevented them from responding to the a priorist nature of Robbins's methodology. They thus discussed a side, rather than a central, methodological issue of economics. But then, they were concerned with a philosophical end (clarifying Popper) - using economics as an example - unlike Hutchison whose concern with methodology was to make economics more practically useful.

Klappholz and Agassi interpret Friedman's main thesis as the argument that most methodological criticism of economic theory is misconceived. Yet in establishing this thesis 'Friedman adopts a position which impedes criticism in general' (p 66). They seek to show that Friedman adopts such a position by examining three of his points:

(a) 'A hypothesis can be tested *only* by the conformity of its implications or predictions with observable phenomena' (Friedman, 1953: 40, emphasis added). Friedman's contention that empirical testing is the only valid form of criticism results in his dismissing theories (eg of monopolistic competition and full-cost pricing) for being untestable. Instead they should be subjected to criticism so as to try to make them testable. (b) 'Great confidence is attached to . . . [a hypothesis] if it has survived many opportunities for contradiction' (Friedman, 1953: 9). Klappholz and Agassi protest that we do not accept an hypothesis as true simply because it has survived attempts to refute it. That Friedman does shows he is not in full sympathy with

Popper's critical approach. (c) '[A new or rival theory] must have implications susceptible to empirical contradiction [before it can be regarded as interesting and important]' (Friedman, 1953: 38). One way of criticising an hypothesis is to formulate a rival one. Yet Friedman's view, that existing hypotheses that have survived attempts at refutation must command confidence, militates against attempts to formulate a rival hypothesis.

Klappholz and Agassi's comments on Friedman's essay call forth two points that relate to the interpretation of Hutchison. Firstly, they argue that untestable theories can be criticised and overthrown and that this is borne out by the historical record. They contend that the historical record, for example, shows that, although Say's Law is untestable, it stands rejected today. Their choice of example is unfortunate since the central theme of an entire 'school' of Post Keynesians is that Say's Law is very far from being generally rejected today. For example, this theme would tend to be found in journals such as *The Journal of Post Keynesian Economics*.[5] This failure supports Hutchison's emphasis on the importance of as wide a possible empirical testing of theories (assumptions and predictions). The second point concerns their rejection of Friedman's dismissal of the theories of monopolistic competition and full-cost pricing on the grounds that they are untestable. What needs to be done instead is to make such theories testable. The fact that Archibald (1961) took up this very challenge appears to indicate their influence. This points to their influence on Archibald's (1959) interpretation of Hutchison (1938).

Klappholz and Agassi's criticism of Hutchison

While Klappholz and Agassi choose to dissent from, rather than directly criticise Robbins's position, they point out that Hutchison in 1938 in his 'influential book' took the latter option. In his appraisal of Robbins, Hutchison contended that 'the propositions of pure theory' and 'economic laws' described by Robbins were 'tautologies' or 'analytic truths'. His remedy was that, for economics to be a science,

[5] We note that Kaldor (1983: 6) views Keynes's principle of effective demand as a refinement, rather than an overthrow, of Say's Law.

it should limit itself to propositions that are testable. This introduction to economics of Popper's falsifiability criterion was his 'great merit' (p 63).

However, Klappholz and Agassi take issue with Hutchison's appraisal since it is based on a twofold classification of all propositions that have 'scientific sense' into either empirical statements or tautologies. Empirical statements are conceivably empirically falsifiable, that is, they forbid some conceivable occurrence. Klappholz and Agassi argue that here Hutchison makes a mistake: it is quite possible to imagine a factually false statement that is irrefutable. As such it would forbid something, yet would not be empirically falsifiable (p 63). Hutchison (1938, Ch II, section 5) argues that, since it is not possible to falsify statements with unspecified ceteris paribus clauses, such statements are therefore to be regarded as tautological. Such an argument is flawed. For example, a statement might claim that, ceteris paribus, a tax on cigarettes will raise price, while another statement might claim that it lowers price. While these statements with their unspecified ceteris paribus clauses are untestable, Klappholz and Agassi contend that they are not tautologies 'for they are both incompatible with each other' (p 64).

According to Klappholz and Agassi, Hutchison implies that the majority of economic theory is tautologous. Yet Keynes (1936) 'was undoubtedly concerned with empirical issues' (p 64). As Hutchison himself points out (1938: 43-4), we can generally choose to interpret propositions as having empirical content or as being tautologous. 'Why then, did he insist on viewing most of the propositions of pure theory as tautologies?' (p 64). The answer, according to Klappholz and Agassi, is that 'he was driven to do by the adoption of his dichotomy' (p 64). 'He regarded most propositions of "pure theory" as untestable and, given his dichotomy, a statement which appeared to be untestable could be nothing but a tautology' (p 64).

'Hutchison adopted the dichotomy in order to propose the rule, "Do not argue about tautologies, but only about testable statements!"' (p 65) The problem, Klappholz and Agassi contend, is that Hutchison's dichotomy is false. It excludes not only potentially interesting statements (such as statements with unspecified ceteris paribus clauses) since these are clearly untestable, but also statements which do not appear to be testable. 'This would amount to an undesirable restriction on the range of

argument, especially since it is often difficult to know whether an important new idea is testable' (p 65).

The reason Hutchison proposed his rule was 'to ensure adherence to the critical attitude' (p 65). However, such an attitude cannot be ensured by adopting rules. Hutchison seems to believe that enforcing the rule that restricts scientific discussion to testable statements guarantees that these statements would actually be tested, and rejected if falsified. This is because he appeared to have argued (incorrectly) that 'the only way to escape refutation of an empirical statement was to turn it into a tautology' (p 65). Whatever Hutchison's reasons, the point remains that simply insisting that statements must be testable does not ensure a 'critical attitude', that is, statements 'being on the agenda for testing' (p 65). 'Professor Popper has strongly emphasised this point, namely, that the critical attitude demands *severe* and *sincere* attempts to *falsify* our views, and he also stated the arguments against the dichotomy' (p 65).

Before examining Hutchison's response to Klappholz and Agassi's appraisal, a number of points are in order. First, Klappholz and Agassi are against any methodological rule (other than a general injunction to be critical). The rule they ascribe to Hutchison is: 'discuss only testable statements'. Presumably they have in mind that, while Popper's falsifiability criterion says that scientific statements must be falsifiable, Popper does not impose a rule limiting (scientific) discussion to only testable statements. However, from our Chapter Three discussion of Hutchison's (1938) it should be apparent that neither does Hutchison. For example, he outlines the use and significance of propositions of pure theory and accepts the 'invaluable' method of introspection as 'practically indispensable' (1938: 33-6, 142-3). While Hutchison, like Popper, insists that scientific statements be empirically falsifiable, he does not, in fact, propose the rule ascribed to him by Klappholz and Agassi. Their criticism that such a rule does not ensure a 'critical attitude' thus becomes redundant.

Second, Klappholz and Agassi do not accept Hutchison's twofold classification of all scientific statements as being either empirically falsifiable or tautological. Hutchison (1960, 1960a) takes issue with their second point and we accordingly defer comment

on their second point until we examine Hutchison's responses. At this stage we limit ourselves to their first point: that they do not accept Hutchison's classification. They note that Popper also argued 'against the dichotomy', but do not provide a specific reference to his argument. Given this, we can only speculate that their reason for not accepting Hutchison's dichotomy is that it rules out metaphysical statements from scientific discussion in economics (cf Latsis, 1972: 239). Yet, as we have seen above, Hutchison accepts a role for the metaphysical statements regarding electrons in physics and in the method of introspection in economics (1938: 19, n 6 and pp 142-3). This may be among the reasons he emphasises that his testability principle applies only to the 'finished propositions of a science' (1938: 9). Klappholz and Agassi's mistake may stem from the fact that they misquote Hutchison on this point (1938: 26). They refer to his classification of propositions as having 'scientific sense' seemingly not noticing that Hutchison had stressed the term 'scientific' by putting only it in inverted commas (p 63). Like Popper he denied metaphysical statements 'scientific' sense, but did not (as Klappholz and Agassi seem to imply) deny that they had sense, or meaning.

Third, Klappholz and Agassi ask why Hutchison insisted on 'viewing most of the propositions of pure theory as tautologies' (p 64). Hutchison did not view most of the propositions of pure theory as tautologies, he viewed *all* of them as such because he defined pure theory (but not the whole of economic theory) to consist exclusively of analytical or tautological statements. This third point, together with the other two, highlight shortcomings in Klappholz and Agassi's understanding of Hutchison's (1938), shortcomings which in turn limited the influence of Hutchison on members of the M^2T seminar group, in particular, Archibald and Lipsey.

The 1960 exchange between Hutchison, and Klappholz and Agassi

In reply to Klappholz and Agassi (1959), Hutchison (1960: xvi) points out that his 1938 two-fold classification of statements into testable statements and tautologies was put forward 'simply as convenient for the methodological analysis of statements in economics' (1938: 27). He accepts that 'if there was a range of important statements used in economics' which could not be fitted into this classification, then it would

'certainly be misleading and inadequate'. So far, however, there have been no examples of statements 'which do not fit this classification' (Hutchison, 1960: xvi).

Hutchison proceeds to explain why he finds Klappholz and Agassi's (1959) example concerning statements containing unspecified ceteris paribus clauses unsatisfactory. Firstly, they assert that he claimed that *all* such statements in economics are tautological (p 64). Secondly, he interprets Klappholz and Agassi's ceteris paribus example (concerning a tax on cigarettes) as attempting to show the inadequacy of his classification. The example given implies that statements containing unspecified ceteris paribus clauses may be either empirical or tautological and so cannot be definitely classified as one or the other (Hutchison, 1960: xvii).

Hutchison (1960a: 158) responds by pointing out that he did not say that *all* statements containing unspecified ceteris paribus clauses are tautological, but rather that they are *frequently* tautological; that they may be interpreted as either empirical or tautological or, indeed, in other ways; and that they are frequently 'hopelessly ambiguous' (1938: 41, 40, 45, and 162). However, once an attempt is made to specify the empirical content of such clauses, the statement so qualified will be able to be definitely classified as one or the other (Hutchison, 1960: xvii). Hutchison accepts that his classification cannot deal with 'hopelessly ambiguous' statements, but comments that if Klappholz and Agassi wish to add a third category (that of hopelessly ambiguous statements) to his twofold classification, he would not object since this would not imply that his twofold classification was inadequate (ibid).

Klappholz and Agassi (1960: 160) reply by pointing out that their paper was mainly 'a criticism of an attitude of impatience and haste which seems prevalent in methodological writings on economics'. Hutchison's 'view that all statements must be either tautological or testable supports this attitude since it justifies the impatient dismissal of ideas which are not yet testable' (ibid).

They state that they tried to criticise the inadequacy of this dichotomy by pointing out that statements qualified by unspecified ceteris paribus clauses are untestable. This is because, to falsify them, one would need to observe all the variable and constant factors, not because they are tautologies. Hutchison accepts that statements qualified

by unspecified ceteris paribus clauses are untestable and that it follows from this and from his dichotomy that such statements must be tautological. 'Yet he criticises us for attributing this conclusion to him' (Klappholz and Agassi, 1960: 160). They contend that Hutchison 'now implicitly concedes that his dichotomy was inadequate when he says that an ambiguous statement may be neither tautological nor testable; which is all we endeavoured to show in this context' (ibid: 160-1).

While they broadly sympathise with, and respect and applaud, Hutchison's (1938) book, in particular his point that economists were mistaking logic for fact, they regret that they find some of its 'basic tenets unacceptable and believe them to give unjustifiable support to the kind of impatience we sought to criticise' (ibid: 161).

Concluding remarks

The main point we want to emphasise about Klappholz and Agassi's comments is that they are concerned with philosophical issues (clarifying Popper) than with using philosophical concepts to aid the practice of economics. Their philosophical interest in testing the limits of Hutchison's dichotomy leads them to miss the point of Hutchison's concern with the ceteris paribus assumption *for economics*. Given that Hutchison is saying only that ceteris paribus propositions are *frequently* tautological, or 'hopelessly ambiguous', the relevance for economics of Hutchison's criticism is that this favourite assumption 'should be used less often and more cautiously' (1938: 162). Hutchison's point does not occur in an obscure footnote, but in a list of his leading fifteen conclusions in the chapter summary at the end of his book! That Hutchison is more concerned with economics than Klappholz and Agassi is indicated by another example. Hutchison at one point is led to say that Klappholz and Agassi's statement (that one can imagine factually false statements that are not empirically falsifiable) is not only a 'direct contradiction', but of doubtful relevance regarding statements *in economics* (1960: xxi, n 1, emphasis added).

A number of further points merit noting. The first has been mentioned before. Klappholz and Agassi (1960: 160) refer to Hutchison's view 'that all statements must be either tautological or testable'. This is simply incorrect. Hutchison admitted a role for other statements (eg the results of introspection and metaphysical ones concerning

electrons). His concern was with a more specific proposition, namely, that the 'finished propositions' of economics be testable (1938: 9). A second point concerns the extent to which Klappholz and Agassi sympathised with, respected, and applauded Hutchison's (1938). When passing to their review of Papandreou (1958) and Schoeffler (1955) they state that these two books 'fall well below the high standards set' by Robbins (1935), Hutchison (1938) and Friedman (1953). Despite this, Hutchison's (1938) failed to directly influence key economists such as Archibald and Lipsey. For this unfortunate outcome, the ultra-Popperian interpretation of Hutchison (1938) by Klappholz and Agassi played an important role. As such, much of the substance of what Hutchison had to say that was relevant to the practice of economics was lost in a cloud of philosophical discussion.

Thirdly, Latsis points out that if Klappholz and Agassi reject Hutchison's proposal that untestable propositions should be dismissed as unscientific, 'what propositions *do* we dismiss as unscientific? Klappholz and Agassi give no reply' (1972: 241). Their conclusion that methodologically all one can do is to adopt a critical attitude appears too idealistic. Popper himself put forward his falsifiability criterion. In this respect Hutchison (1938) appears to have his feet more firmly on Popper's ground.

6.2 Koopmans's 1957 essay and Hutchison

In this section we examine the relation between Koopmans (1957) and Hutchison (1938) for two main reasons. First, in his essay Koopmans explicitly drew attention to the similarities between his position and that of Hutchison (1938). We will show that while similarities certainly existed, Koopmans, like the M^2T group, failed to be attracted to Hutchison's response to the difficulty of empirical testing in economics. This factor, rather than a perception of Hutchison's position as logical positivist or naïvely inductivist, appears to have been the major divide. Second, Koopmans's essay is the main topic of Archibald (1959), another methodological piece to emerge from the M^2T group. Hutchison's 1938 intervention is also the subject of Archibald's appraisal, and this appraisal - when we turn to examine it in the next section - will be better understood within the wider setting of his review of Koopmans's essay.

Our first task is to evaluate Koopmans's claim regarding the similarity of his methodological position to that of Hutchison (1938). Koopmans states that as he was finishing his essay he

> became aware of the similarity of the point of view here adopted with some of the ideas expressed in somewhat stronger terms by T. W. Hutchison (1938). There is some difference in emphasis in that the present essay gives more attention to the detachability of the chains of reasoning from the interpretation of the postulates. In so far as there is duplication or repetition, there seems to be no harm done by it, since the practical consequences of the views expressed have not yet been generally realized or accepted (1957: 132, n 2).

Given that Koopmans is far from being regarded as any sort of ultra-empiricist, his comment is significant since it appears to support our argument in Chapter Five that Machlup (1955, 1956) incorrectly represented Hutchison's position. However, as we will see in our examination of his 1957 essay, while there are similarities, there are also differences, particularly concerning Koopmans's greater attention 'to the detachability of the chains of reasoning from the interpretation of the postulates'.

Koopmans begins his essay by pointing out that the potentialities of recent changes in the tools of both theoretical and empirical research should be accorded methodological recognition (p 130).[6] He then points out that, while progress in empirical sciences such as economics results from the interaction of observation and reasoning, in terms of Marshall's 'diplomatic style of discourse' it is 'extraordinarily difficult' to uncover the parts that observation and reasoning play in building the foundations of economic knowledge (p 131). While it is impossible to say which comes first, observation or reasoning, in this essay he will concentrate on the part of reasoning. He now states that to uncover the parts that observation and reasoning play in building the foundations of economic knowledge, it will be useful to describe its 'logical structure' which he sets out as follows:

Any logically valid chain of reasoning starts from certain premises. Premises which are basic, that is, 'not in themselves conclusions from earlier parts of the reasoning in the same piece of analysis' are called 'postulates' (p 132). The postulates contain

[6] All unsupported page references in section 6.2 are to Koopmans (1957).

terms that are the counterparts of observable phenomena. While for practical purposes the casual meanings of the key words (consumer, commodity, probability) associated with these terms suffice, more formal descriptions or interpretations are needed in order to get down to fundamentals (p 133). Without the interpretations, the postulates are bare statements of relations between terms. Through the interpretations they become statements that specify the range of choices open to the various persons introduced, the effects of these choices, and the rules or principles from which actual choices are derived. 'However, from the point of view of the logic of the reasoning, the interpretations are detachable. Only the logical contents of the postulates matter' (p 133).

At this stage, there are two points to note about the approach outlined by Koopmans in so far as it compares to Hutchison (1938). First, his statement that reasoning starts from basic premises, or postulates, not themselves earlier conclusions, and that contain theoretical terms which are the counterparts of observable phenomena appears to reflect a crude form of inductivism open to the most basic kinds of criticism. Elsewhere Koopmans writes of 'exhibiting the postulational basis, and thereby the ultimate observational evidence, on which our statements rest' (p 144). Even Friedman (1953) recognised that the premises of a piece of reasoning are often the results of an earlier hypothesis. While Hutchison leans towards an inductivist approach, he does not attempt an explicit description such as that of Koopmans. Instead he *implies* the validity of an inductivist approach by referring to the need to test assumptions. In addition, Hutchison accepts that some terms, eg electrons in physical science theory, will not have empirically observable counterparts (1938: 19, n 6).

Second, his emphasis on the detachability of the interpretations appears to reflect his view that the activities of observation and reasoning need to become formally separated as, he claims, they are in the physical sciences. Marshall's diplomatic style needs to be dropped. Here Koopmans takes a decisive step away from Hutchison (and Friedman) who both held Marshall's method in high regard. In urging the introduction of more formal methods of investigation, Koopmans is following views he expressed twenty years earlier. Morgan points out that Haavelmo (1944), in

pioneering the probability approach to econometrics, 'in fact developed the views of Koopmans (1937), who had already argued that economic data could be regarded as a random sample from a hypothetical probability distribution' (1998: 218).

Given that the postulates of economic theory are not completely self-evident, and that the implications of such postulates are not easily testable, Koopmans argues that it will help if the implications can be traced to the supporting postulates. This implies that we view economic theory as a sequence of models that seek to express aspects of a more complex reality. While Koopmans foresees realism preceding rigour in the construction of these models, progress depends upon rigour consolidating the gains in realism. However, 'often we are more preoccupied with arriving at what we deem to be true statements or best predictions, in the light of such knowledge as we have of the phenomena in question, than in exhibiting the postulational basis, and thereby the ultimate observational evidence, on which our statements rest' (p 143). Apart from the difficulties of testing in economics providing reasons for making the postulates explicit, there are psychological reasons. For example, there is a tendency to overestimate the scope of the conclusions of economics. One such conclusion is the belief 'in the efficiency of competitive markets as a means of allocating resources in a world full of uncertainty' (p 146). Yet no model of resource allocation has been developed which deals with uncertainty (p 147). In the remainder of the essay Koopmans states that he will discuss some of the difficulties that arise when factors such as uncertainty and indivisibilities are taken into account (p 149).

Koopmans now considers the directions in which greater realism could be introduced into the system of postulates underlying economic analysis starting with the factor of uncertainty. Here we need greater clarity on whether the class of self-evident postulates outlined by Robbins may perhaps be exhausted. If this is the case, then the previous 'casual' empiricism which in the past proved sufficient for economic analysis needs to be replaced by

> more systematic observation and direct or indirect testing of postulates. The latter view has been strongly expressed by Hutchison (1938, Ch IV) who sees in the recognition of uncertainty the turning point beyond which empirical verification of postulates should become the main preoccupation of economists (p 150).

Likewise, Koopmans points out, although the indivisibility of commodities is a basic fact of technology and even of human existence, and furthermore that such indivisibilities generate increasing returns to scale with direct implications for perfect competition, economic theory has hardly started to take such indivisibilities into account. Koopmans sets out his solution to this problem as follows: 'In a postulational approach, indivisible commodities can be introduced through the suppression of the proportionality postulate for all activities in which one of the commodities in question enters' (p 152).

> Professor Chamberlin's objection that this reduces the 'explanation' of increasing returns to a tautology misses the point. As Hutchison (1938, Ch III) has emphasized, all pure theory, that is, all study of the implications of given postulates is tautological in character. Accordingly, the reproach of tautology has been leveled against many propositions of economic theory (p 152).

The point at issue, however, is that a model that omits the proportionality postulate appears to recognise those aspects of reality responsible for increasing returns to scale (p 152). As such it may prove useful in a first attempt at examining the phenomenon of increasing returns to scale. But so far, 'theoretical analysis still has not yet absorbed and digested the simplest facts establishable by the most casual observation' (p 154). Given that the main obstacle to progress appears to be in the form of mathematical difficulties, 'it may be desirable in initial attempts to select postulates mainly from the point of view of facilitating the analysis, in prudent disregard of the widespread scorn for such a procedure' (p 154).

We are now in a position to comment once again on Koopmans's views in so far as they compare to those of Hutchison's. When Koopmans complains that economists are often over concerned with predictions and neglect examination of assumptions, it appears as if he is putting forward a similar view to that of Hutchison. When he complains that economists overestimate the scope of their conclusions (especially that concerning the efficiency of competitive markets), one can see Hutchison nodding in agreement. Once again his calls for injecting greater realism into the system of postulates beginning with the factors of uncertainty and indivisibility of commodities sound fully in line with an approach which Hutchison would support. Regarding the

uncertainty factor, Koopmans appeals explicitly to Hutchison's call for the greater testing of postulates. But now sharp differences appear.

Regarding the indivisibility factor, Koopmans argues this can be dealt with by 'suppression of the proportionality postulate'. Dropping this assumption will allow increasing returns into the analysis and thereby incorporate the effect of indivisibilities. Koopmans in this case now seems to accept that assumptions should be selected for their theoretical tractability rather than their empirical realism, something that lies at the very heart of what Hutchison has been arguing against. His attempt to justify this move by arguing that is might prove useful in a first attempt is an appeal to the 'optimistic approach' so roundly condemned by Hutchison. Paradoxically Koopmans parts company with Hutchison in his attempt to introduce more 'realistic assumptions' into economic analysis. For Koopmans, this is done by formalising the theory while, for Hutchison, the very opposite is required.

Archibald's review of Koopmans's essay

According to Archibald (1959), Koopmans's essay is subject to two main criticisms. First, where Friedman called for the testing of conclusions, Koopmans takes a step backwards and adopts the 'Hutchison-Oxford position', calling for the testing of assumptions (Archibald, 1959: 64). As we have shown above, although Koopmans calls for the testing of assumptions, there are significant differences between his position and that of Hutchison so that it is not clear what meaning can be attached to the idea that Koopmans adopted the Hutchison-Oxford position. Archibald's term, the 'Hutchison-Oxford position', is also unsatisfactory. Whereas the Oxford economists were concerned with putting forward a particular economic theory rather than with economic methodology, Hutchison's primary concern was with economic methodology. The two parties' engaged with methodological matters at very different levels. For example, Hutchison had adopted much the same response as Archibald to introspection, a matter quite beyond the scope of the Oxford economists' concerns.

Archibald's second main criticism of Koopmans (1957) is that it largely fails to clarify the major extant methodological problems: where Friedman was wrong, Koopmans is of little help (1959: 63). In particular it would have helped if Friedman

(and Koopmans) had clarified their use of the term 'assumptions' since economists use the term in many different senses. In an attempt at clarifying some of these Archibald proceeds to distinguish four separate senses. First there are assumptions that concern motivation; second, empirical assumptions concerning the existence and stability of functional relations; third, assumptions that certain things are constant; and fourth, assumptions 'may also be used to explain the problem to which the hypothesis is meant to refer' (Archibald, 1959: 65). Lipsey (1963), citing Archibald on this matter, was to develop and clarify the classifications offered by Archibald. Hutchison's emphasis on the testing of assumptions appears to have influenced both Archibald and Lipsey in their attempts towards greater clarity in this field.

Finally Archibald points out that, contrary to Koopmans, there is no conflict between realism and rigour. He is misguided in suggesting that testing of a rigorously formulated theory such as von Neumann and Morgenstern (1947) is difficult. 'The implications of a theory can be tested whether its formulation is rigorous (and postulates "unreal") or non-rigorous ("realistic"); if it is rigorously formulated it is, of course, likely to be easier to discover just what it does imply' (Archibald, 1959: 67).

Archibald states he is puzzled by Koopmans's view that there is a conflict between realism and rigour. He cites Koopmans's (pp 155-61) view that von Neumann and Morgenstern (1947) is difficult to test. He maintains a theory can be tested whether or not it is rigorously formulated. Indeed, it is easier to test if it is rigorously formulated. However, as we saw in examining Koopmans's essay, Koopmans parts company with Hutchison in arguing that the way to introduce the effects of uncertainty and indivisibilities into economic analysis is to select postulates for their mathematical tractability. Koopmans is at one with Archibald in moving towards the introduction of more formal approaches in economics. In doing so, they decisively part company with Hutchison.

With this background of Archibald's appraisal of Koopmans's essay, we are now equipped with a useful perspective to view his interpretation of Hutchison (1938).

6.3 Archibald's 1959 interpretation of Hutchison

In this section we are interested in understanding the way in which another member of the M^2T group viewed Hutchison's approach to methodology, namely Archibald. We point out the extent to which his approach is more in line with Hutchison than that of Klappholz and Agassi's purist Popperism. While Archibald's linking of Hutchison with the Oxford 'full cost' economists does appear to support de Marchi's (1988: 146) point that the M^2T group were put off by Hutchison's naïve inductivism, we argue that the more important factor was Hutchison's response to the difficulty of empirical testing in economics. Archibald, as we have just seen in his comments on Koopmans's essay, maintained that a theory was easier to test if formulated rigorously. It is in developing this view, eg Archibald (1961), that his approach diverged from Hutchison's.

Like Klappholz and Agassi, Archibald attempts a general Popperian-inspired critique of economic methodology. Accordingly, he begins with a critical look at Robbins and Friedman's methodological essays before turning to Hutchison's. However, Archibald's less extreme interpretation of Popper leads him to differ from them on a number of points, especially regarding his criticism of Robbins (1935) where, unlike Klappholz and Agassi, he implicitly accepts Hutchison's rule of empirical testing in economics.

Archibald's comment on Robbins (1935) is brief. He notes the following aspects of Robbins's essay. According to Robbins, propositions of economic theory are deductions from postulates that involve indisputable facts of experience (1935: 78-9). Consumers' valuation of goods is explained in subjective and introspective terms (1935: 88). While generalisations (eg concerning electrons) in natural sciences 'are known only inferentially', in economics they are known 'to us by immediate acquaintance' (eg concerning ordering of individual preferences). 'It is true that we deduce much from definitions. But it is not true that the definitions are arbitrary' (1935: 105). Archibald comments on these aspects as follows: given that economic propositions are deductions from assumptions that are indisputable, the testability of such assumptions is irrelevant and unnecessary (1959: 59).

Compared to Klappholz and Agassi's (1959) decision to dissent from, rather than to directly criticise Robbins (1935), Archibald tackles Robbins head on. He gets to the nub of the matter: in terms of Robbins's a priori-leaning approach, the testability and testing of assumptions is simply unnecessary. Archibald thereby supports our interpretation of Robbins's position and appears closer to adhering to Hutchison's emphasis on the importance of empirical testing.

Archibald describes Friedman's (1953) essay as 'revolutionary'. By arguing that assumptions were necessarily descriptively unrealistic, it represented a step towards escaping from this muddle surrounding the issue of the realism of assumptions. Friedman's essay stated some methodological principles well known in the natural sciences. However, applied to economics this raises serious problems concerning the method of, and criteria for, testing. Archibald criticises Friedman firstly for his 'complacency' concerning what has been done in this regard in economics. 'It is extremely doubtful, in my opinion, if the theory of the firm has *ever* been the subject of really serious testing' (p 61).[7] Secondly, he criticises Friedman for omitting to say anything about the criteria for a good test, or about the difficulty of testing in economics. In this regard there are two common problems ignored by Friedman: the problem of how to test a theory that is 'purely static' and so descriptively false; and the problem of the ceteris paribus clause.

Archibald now turns to expand on his second criticism of Friedman: side-stepping the difficulties of testing in economics. In particular, he looks at the problem of the ceteris paribus clause. He contends that, if the 'other things' are not specified in advance, there is an alibi for any refutation. While the list of 'other things' is infinite, we need to specify which of them are taken to be constants in the hypothesis we are trying to test. Moreover, it is vital that such factors are observables. Often factors in economics which need to be held constant are non-observables such as tastes or expectations. If an hypothesis contains such non-observables, it can never be refuted. Archibald explains that this had been pointed out by Hutchison in his discussion of the ceteris paribus clause (Hutchison, 1938: 40-46). However, if the 'other things' are specified in advance, and are observable, 'we at least have a chance of learning from a

[7] From now on unsupported page references in section 6.3 are to Archibald (1959).

refutation that the hypothesis must be widened to include some "other things" as variables' (pp 61-2). Archibald explains that this too had been pointed out by Hutchison (1938: 44).

Here is clear evidence of the direct influence of Hutchison. Archibald recognises that Hutchison had long ago pointed to the problems of testing propositions with poorly specified ceteris paribus clauses. Archibald accepts the significance of the distinction between observable and non-observable factors in order to arrive at a definite test result. This distinction is of course central to logical positivism. The most interesting point, however, is that Archibald approves Hutchison's conclusions about the problem of ceteris paribus clauses. By contrast Klappholz and Agassi (1959) cited Hutchison's analysis of ceteris paribus clauses negatively in their criticism of Hutchison's twofold classification of propositions that have scientific sense.

Archibald's interpretation of Hutchison

Archibald begins by noting Hutchison's insistence 'that the criterion of testability be rigorously applied to existing economic thought' (pp 59-60). In addition, more attention needed to be given to 'looking at the facts in relation to theories' (p 60). While this represented progress compared to Robbins's position, 'Hutchison was unfortunately vague on the question of what we tested' (p 60). It seems as if Hutchison thought it was assumptions rather than predictions that needed testing. Archibald cites various passages from Hutchison concerning the need for testing assumptions (1938: 74, 83, 89, 119-120).

Here we note that Archibald approves of the more empirical approach of Hutchison as compared to Robbins. For Archibald, the problem with Hutchison's empiricism is that he wanted to test assumptions rather than predictions. While Archibald cites passages in which Hutchison spoke about the need to test assumptions, this does not imply that Hutchison is arguing that they should be tested rather than predictions. Instead Hutchison emphasised the importance of a theory generating predictions which are empirically testable (1938: 65-70 and 163-4). Moreover Hutchison's central Principle of Testability refers to the need for the 'finished propositions' to be testable (1938: 9). Here the predictions of a theory are a better candidate for being

'finished propositions' than the assumptions or initial premises of a theory. Unlike Friedman, Hutchison does not restrict testing to concentrating only on predictions. Instead one must seize opportunities for testing whenever they arise, given that such opportunities are so limited. While it is accurate to say of Friedman that he wanted to test predictions rather than assumptions, it is not accurate to say of Hutchison that he wanted to test assumptions rather than predictions. In any case, the main criticism of Hutchison, namely that by Machlup (1955), was that he thought it worthwhile to test assumptions *at all*, let alone as much as, or more than, predictions.

Meanwhile, Archibald notes, certain Oxford economists were attempting to test the realism of assumptions such as the one that businessmen set price and output so as to maximise profits (Hall and Hitch, 1939).

> The Oxford work was, I think, undertaken before the publication of Hutchison's book; but it is convenient to take them together because they are alike in insisting, both that facts and theories be studied in close relation to one another, and, as I interpret them, that the role of the factual study is in checking the descriptive reality of the assumptions of the theory (p 60, n 2).

The method of the Oxford economists was to use questionnaires. Their results were generally considered as indicating that businessmen sought a normal profit determined by a mark-up on fixed costs, rather than a maximum profit. This generated a debate on the realism of the profit maximising assumption.

> The attempt to check the realism of the postulates led only to a methodological schism in economics: on the one hand we had those who paraded their 'realism' - 'this is how businesses actually work' - and were indifferent to the arguments that their theory was indeterminate and therefore irrefutable; on the other hand we had those who stuck to 'rational' theory, and appeared more and more indifferent to reality (as understood by the first group). We still suffer from this sterile and muddle-headed dispute (p 61).

Archibald's linking of Hutchison with the Oxford economists is problematic. While it is true that, in a general way, they both stressed the importance of testing assumptions, in many important respects Hutchison's approach was significantly different from the Oxford economists, as well as being, of course, more sophisticated. The most obvious difference is that Hutchison sought to describe a general methodological approach to tackling economic problems whereas the Oxford

economists were concerned with one particular theory - the theory of the firm. The issue of testing assumptions forms just one part of Hutchison's concerns. Archibald's conflation of Hutchison with the Oxford economists is both inaccurate and misleading. For example, Archibald refers to the 'sterile' debate on the realism of assumptions. On the one hand, he says, were those who 'paraded their realism' and paid no attention to making their theories empirically 'refutable'; and on the other were those who paid no attention to realism of assumptions. From what Archibald has said it would seem as if he would classify Hutchison with those who paraded their realism. Yet, central to Hutchison's whole methodology, is his Principle of Testability, namely, the need for a theory to generate conceivably empirically falsifiable 'finished propositions'. This is far from calling simply for realistic assumptions.

It appears that Archibald, like Machlup before him, misrepresented Hutchison's position. Whereas Machlup made Hutchison out to be an ultra-empiricist, Archibald has presented him as being on a methodological par with the Oxford economists, apparently fixated on testing assumptions. Yet Archibald's (1961) subsequent exchange with Chicago was to show up the limitations of his ultra-Popperian-leaning methodology as opposed to Hutchison's more cautious approach. In the process Archibald discovered that economic theory did not lend itself to Popper's refutability to nearly the extent he had thought. Unlike Hutchison, he had been overly ambitious in attempting to apply Popper to economic theory. He had tried to derive testable predictions from monopolistic competition by applying Samuelson's qualitative comparative static analysis. Contrary to Samuelson (1947: 33) he found that there are almost no unambiguous qualitative predictions, or predictions 'in general', when one is trying to assess the effect of a parameter change, if the change also alters other parameters (de Marchi, 1988: 156-7).

The problem with testing was that it needed so many subsidiary assumptions that it was impossible to pinpoint what had gone wrong in the event of a refutation (ibid). One solution seemed to be to acquire more detailed quantitative information, but it was precisely the problems to which this gave rise that had prompted Archibald's use of comparative static analysis. This led Archibald (and Lipsey) to abandon the notion of Popperian testing which meant getting a proposition into a form in which it strictly

forbids something (a more general statement) (Archibald, 1966; Lipsey, 1966). Consequently they both became convinced that economic hypotheses must be expressed in statistical terms and statistical conventions substituted for Popper's demarcation criterion of strict refutability. Yet Popper (1959: 189-90) was well aware of this difficulty, even acknowledging that physics is based on probability statements (de Marchi, 1988: 158-61). The results of Archibald's attempt to apply Popper too closely to economics appeared to show up the wisdom of Hutchison's more selective drawing upon of Popper.

6.4 Lipsey's 1963 textbook and Hutchison

In this section we trace the extent to which the approach of the M^2T group, in spite of the critical comments of Klappholz and Agassi (1959) and Archibald (1959), was *de facto* in line with Hutchison's methodological views. Given that the M^2T group were not regarded as methodological extremists, to the extent that similarities prevail this supports our argument that Hutchison's approach was not nearly as extreme as made out by Knight (1940) and Machlup (1955). The two examples we choose to examine for such similarities from the work that developed out of the M^2T group are Klappholz and Mishan (1962) and Lipsey (1963).

Klappholz and Mishan (1962) begin by pointing out that a legacy of the saving-investment controversy of the 1930s is the profusion of identities in economic models, coupled with the exhortation to students never to confuse identities with behaviour relations (1962: 117). But if identities were seen to add nothing to our understanding of the working of the economic system, it should be recognised that they were redundant in economic models, and that their presence needlessly gave rise to confusion. In particular, their role as the basis of economic models is untenable (1962: 118).

Klappholz and Mishan first look at the logical status of an identity. A multitude of writers indicate that it is a relationship that is true by definition. Identities are empty statements. They tell us nothing about the universe and are necessarily irrefutable. If an identity is an empty statement, it has nothing to impart to a theory. It can, so to speak, evaporate without a trace (1962: 119).

If this is an obvious point, it has certainly not been allowed to prevail in economics. Instead the literature abounds with tributes to the role of the identity. Some go so far as to regard identities as 'the pith and substance of economics'. Klappholz and Mishan respond to this view by pointing out that, if identities were in fact examples of important propositions in economic theory, they should have to agree with Hutchison (1938) that most propositions of economic theory are indeed 'invincibly true', but therefore empty. They agree, of course, with Hutchison that such statements are of no interest in empirical economics, but they cannot agree with his view that practically all propositions in economic theory fall into this class of statement.

Among those who find them the pith and substance of economics are Robertson (1957) and Boulding who argues that 'it is one of the principle tasks of economic and social analysis to detect and state them' since they express important economic truths which 'serve to limit the social and economic universe to a certain area of possibility' by defining 'the impossible' (1958: 5-6). However, this, it turns out, is only the linguistically impossible. In addition to Boulding, Swedish economists such as Myrdal, Lindahl and Hansen make liberal use of identities (Klappholz and Mishan, 1962: 120). Indeed they are used so generally that Boulding tells his readers that 'the relationships which constitute a model are of two kinds, identities and behaviour equations. All models seem to possess them both' (1941: 47). Klappholz and Mishan find it baffling that so many writers admit that identities are devoid of empirical content and yet believe that nevertheless they are essential (1962: 121).

Klappholz and Mishan proceed to argue, 'first, that to build economic models upon identities is to commit logical absurdities, and second, that the idea of identities serving as a guide in the construction of such models is illusory' (1962: 121). They describe models as 'theories on the basis of which we try to explain or predict economic phenomena' (1962: 121). 'A common practice, to which we have alluded, is the treatment of an equation of identity as though it served as a scaffolding about which to append empirical relationships' (1962: 122-3). 'No economic theorem whatever can be deduced from an equation, no matter how vigorously manipulated, that has been suffered to remain an identity' (1962: 124). Similarly 'no hypotheses whatever about equilibrium conditions can be inferred from identities' (1962: 126).

> What we are condemning . . . is the practice of, and the arguments for, the substitution in economic models of equations which are identities when, in fact, the empirical implications which economists purport to deduce from these models follow only if such equations are not identities but genuinely independent equations - equations that are, in effect, independent empirical statements. This erroneous practice is widespread, particularly the use of identities as a base on which to build simple and familiar models (1962: 126).

The extent to which Klappholz and Mishan repeat the arguments given by Hutchison in 1938 is quite remarkable. They agree with Hutchison that identities are of no interest in empirical economics, but not with his view that most propositions in economics are identities. Yet they proceed to show just how many well-known economists make use of identities and regard them as the 'pith and substance' of economics and acknowledge that they find this state of affairs 'baffling'. Their investigation thus seems to vindicate Hutchison's view about the preponderance of identities in economics. They protest against treating identities as if empirical relationships can be derived from them. This is the same point that Hutchison had raised in 1938: the interpretation of statements which are pure tautologies as yielding propositions which have empirical and practical relevance. The statements quoted from their page 126 in the foregoing paragraphs repeat Hutchison's (1938) arguments to a surprisingly close degree. Indeed, they appear to stress the importance of empirical statements in economics even more than Hutchison!

Lipsey's introduction to positive economics

According to de Marchi (1988: 149), Lipsey (1963) was his answer to Robbins. It stressed methodological awareness from the outset and, in the Popperian spirit, resolutely subjected established theories to criticism. Lipsey's (1963) makes it clear that to judge the correctness of economic theories, one should look in the empirical rather than the a priori direction. If reformulating theory with an eye to testing was Archibald's mission, quantification was Lipsey's (de Marchi, 1988: 144-5). Despite the numerical inconstancy of the givens of economic theory, the question to be addressed in the face of these difficulties is a quantitative one: how much stability is

there? Theory 'can have few applications to the real world without some empirical observations of quantitative magnitudes' (Lipsey, 1963: 161).[8]

There are three major themes in Lipsey's book: (a) explaining economic theory and how to criticise it and so improve it; (b) elaborating the relation between theory and observations; and (c) clarifying the relation between economic theory and policy, and so the significance of distinguishing between positive and normative statements. Regarding (b) it is unfortunate that there is too much of a gap between theory taught as logical analysis only vaguely related to the world, and applied economics which 'becomes description unenlightened by any theoretical framework' (p xii).[9]

After explaining the positive-normative distinction, Lipsey proceeds to explain the term 'science'. He points out that, rather than appealing to authority or introspection, the scientist attempts to relate questions to evidence. If the scientist finds that the issue is framed in such terms that it is impossible to gather evidence, he will try to recast the question so that it becomes possible. There is no formula for such recasting – it is an art (p 6, n 1). While experimental sciences can call up evidence on demand, economics 'must wait for the passage of time to throw up observations which may be used as evidence in testing economic theories' (p 6).

Lipsey is quite clear that the hallmark of a scientific approach is the appeal to fact. While Hutchison (1938: 11) had noted this basic point of empiricism, he failed to give anything like the prominence that Lipsey accorded to it. At the beginning of Lipsey's book, between the contents pages and the preface, Lipsey quotes two pages from Beveridge's farewell address as the director of the LSE (Beveridge, 1937). Apart from Samuelson, Beveridge may well be the only other contender for being awarded a certificate of ultra-empiricism from Machlup. The sections extracted by Lipsey emphasise the inductivist point that science is based on fact. While Hutchison would of course be sympathetic to Lipsey's quotation of Beveridge, it is unlikely that he would have put the matter as directly or simply as Beveridge. In citing Beveridge so fully, Lipsey comes across as someone at least as (if not more) sympathetic to

[8] Torr (1991) has drawn attention to the problems of talking about a 'real world'.
[9] All unsupported page references in section 6.4 are to Lipsey (1963).

inductivism as Hutchison. On this point, Boland (1979: 507) has drawn attention to the inductivist background of the positive-normative distinction.

The question of a social science now arises. A science of human behaviour is feasible, Lipsey argues, since 'predictions about the behaviour of large groups are made possible by the so-called "law" of large numbers' (p 8). This is derived from a behavioural constant, the normal curve of error. While different people will make different measurements of a room, we can predict with perfect accuracy how a group will make its errors. If group behaviour were in fact random there could be no life assurance. Most people accept that there are relatively stable relations in human behaviour. The significant question is now a quantitative one: 'How much stability is shown by human behaviour and how much of behaviour appears to be random?' (p 10). This is an empirical question. It cannot be settled by a priori arguments; it can only be settled by observation.

Lipsey here implicitly follows the thesis of naturalism, or the logical positivist unity of science thesis. He is not as direct or extreme as Friedman on this point. Friedman (1953: 4) had claimed that economics can be an 'objective' science in exactly the same sense as the natural sciences. We have remarked that, while Hutchison adopted a version of the naturalistic thesis in 1938, he came increasingly to recognise the significance of the differences between the natural and social sciences. Lipsey's approach seems to be in line with Hutchison's more circumspect attachment to this thesis.

Lipsey now turns to discuss the nature of scientific theories. He claims we all know that the natural sciences progress through the development of theories. These account for observed phenomena and suggest new phenomena which can then be investigated. He proceeds by asking three questions:

(1) What is the use, or purpose of a theory? Theories grow up in answer to the question 'why?' Some sequence of events is observed in the world and someone asks why this should be so. A theory attempts to explain why. Whether or not it takes us any nearer to an understanding of 'ultimate reality' is a very difficult philosophical question. Whatever may be the answer to this question, one of the main practical

consequences of a theory is that it enables us to predict as yet unobserved events. Thus, for example, national income theory predicts that a government budget deficit will reduce the volume of unemployment (p 11).

Unlike Friedman, Lipsey recognises that theories aim not only at the prediction, but also at the explanation, of events. Hutchison (1938: 70-72) had discussed explanation in so far as it relates to the question of causality. In 1938 he displayed a keen awareness of the problems of talking about ultimate causes, so reflecting not only the general empiricist scepticism on the topic following Hume, but also the more logical positivist interventions.

(2) What exactly is a theory and how does one test theories? A theory consists of a set of definitions and a set of assumptions about the way in which the world behaves. These can both be expressed as mathematical equations. The next step is to use a process of logical deduction to discover various implications of these assumptions. This may be carried out in words, geometry or mathematics. 'The implications which are deduced from the assumptions can be tested against actual empirical observations, and we would then conclude either that the theory is refuted by the facts, or that it is consistent with the facts' (pp 11-12).

Assumptions may often seem totally unrealistic, eg 'assume that there is no government'. But this assumption may merely be the economist's way of saying that, whatever the government does, even whether or not it exists, is irrelevant for the purpose of this particular theory. Now, put this way, the statement becomes an empirical assertion, and the only way to test it is to see if the predictions which follow from the theory do or do not fit the facts which the theory is trying to explain. If they do then the theorist was correct in his assumption that the government could be ignored: the criticism that the theory is unrealistic because we know that there really is a government is completely beside the point (p 12).

Assumptions, however, are used in economics for other purposes, particularly to outline the set of conditions under which a theory is meant to hold. Consider a theory which starts. 'Assume that the government has a balanced budget'. This may mean that the theorist intends his theory to apply only when there is an approximately

balanced budget; it may not mean that the size of the government's budget surplus or deficit is irrelevant to the theory. The fact that an assumption may mean different things in economics has often been a cause of great confusion to professional economists (Archibald, 1959; Friedman, 1953). Two points should be borne in mind:

(i) what information the assumption is intended to convey. For example, that the world actually behaves, or is, as assumed; that the factor under consideration is irrelevant to the theory; that a convenient fiction is being introduced to formalise some quite complex piece of human behaviour.

(ii) that it is not always appropriate to criticise the simplifying assumptions of a theory on the grounds that they are unrealistic. It is important to remember that all theory is an abstraction from reality. A good theory abstracts in a useful and significant way, a bad theory does not. If a student believes the theorist has assumed away something that is important for the problem at hand, then he must try to show that the conclusions of the theory are contradicted by the facts (p 12).

In answering this second question, Lipsey appears to allow his quoted passage from Beveridge, which stresses the importance of induction, to pass into the background. Theories begin with assumptions, but it is not clear how these assumptions are arrived at. Instead Lipsey concentrates on explaining the different kinds of assumptions, so following Archibald (1959) and anticipating Musgrave (1981). Lipsey explains that some assumptions may appear unrealistic, but are not in fact so, once they are properly understood. He distinguishes three kinds of assumptions. To explain the first he gives the example of the assumption of no government. This may mean, when properly understood, that whatever the government does (eg whether or not the government's budget is balanced) is irrelevant to the theory. Musgrave (1981) later terms this a negligibility assumption. Then again the assumption that the government had a balanced budget may mean that the theory only applies when the budget is balanced. Here a budget surplus or deficit is not irrelevant to the theory. Musgrave (1981) later terms this a domain assumption. While a negligibility assumption says that the factor is irrelevant, a domain assumption says that it is relevant, to the theory. Thirdly, Lipsey also refers to what Musgrave (1981) later terms a heuristic

assumption. Such an assumption, Lipsey explains, is a convenient fiction introduced to aid the analysis of some complex behaviour.

Lipsey's purpose in distinguishing these different kinds of assumptions is to show that it is not always appropriate to criticise assumptions for being unrealistic. While it may be appropriate to criticise negligibility or domain assumptions, it is inappropriate to criticise heuristic assumptions for being unrealistic. This represents an advance on Friedman's generalisation that the realism of (all kinds of) assumptions is irrelevant. It probably reflects Lipsey's Popperian background as opposed to Friedman's instrumentalism. It also represents an advance on Hutchison, for had Hutchison distinguished in a similar way between different kinds of assumptions, he would have been able to have shown Machlup (1955) that only heuristic assumptions may be properly thought of as convenient fictions.

In continuing to answer the second question, Lipsey now turns to explain the nature of predictions:

> A theory enables us to predict as yet unobserved events. What is the nature of a scientific prediction, and is it the same thing as being able to prophesy the future course of events? The critical thing to notice about a scientific prediction is that it is a conditional statement of the form: ' if you do this then such and such will follow'. If you mix hydrogen and oxygen under specified conditions, then water will be the result. If the government has a large budget deficit, then the volume of employment will be increased (p 13).

Hutchison (1977: 18) cites this passage and comments: (1) what makes predictions scientific is not their being conditional, but rather the kind of generalisations on which they are based - these need to be well tested; (2) the juxtaposition of the chemical with the economic prediction is unjustified because, unlike the economic prediction 'the chemical prediction is relatively precise, quantitatively and temporally, and it is deduced from a precise and repeatedly well-tested generalisation or law, and from easily testable specific initial conditions' (p 18). Lipsey later continues the natural science analogy. He urges students to think of demands and supplies flowing around a loop [a closed loop control system] much like water in a series of pipes and tanks, or like electricity in an electrical circuit (p 125).

Finally, Lipsey turns to consider what we learn by testing our theories (p 13). A theory can never be proved to be true because we can only make a limited number of observations, while the theory states that a result will always hold. Even if we have made a thousand observations which agree with the prediction, it is always possible that tomorrow someone will make an observation which refutes the theory. Thus, we may conclude that a theory is refuted or that it is consistent with the observations, but it is not possible to conclude it has been proved correct (Popper, 1959). When Beveridge (1937) speaks of the verification of scientific theories, he would appear to believe that it is possible to prove a theory to be true. Unfortunately this is not possible. Likewise we should not speak of laws which are proved to be true, but, rather, of theories which have not yet been refuted (p 14, n 2).

We mentioned earlier that Hutchison over time has increasingly recognised the significance of differences between the natural and social sciences. Here in 1977 he criticises Lipsey for adopting a too natural science view of economics. This arises in Lipsey through his attempting to apply Popper too directly to the concerns of a specific social science, economics. Hutchison also alludes to our point that Lipsey discusses the different kinds of assumptions, but not the appropriate grounds on which to base them. Finally, following Popper, he explains that theories can be refuted, but never verified as true, as the inductivist Beveridge implies. Similarly Hutchison, also influenced by Popper, explains that there is no sense 'in talking of some kind of "absolute" test which will "finally" decide whether a proposition is "absolutely" true or false' (1938: 9).

(3) How are theories developed? Theories usually grow up to account for a set of empirical observations. They are often the result of creative genius of an almost inspired nature (Koestler, 1959). A possible theory is one which is consistent with (ie predicts) the already observed phenomena. However, almost any interesting theory that has developed to explain some set of observations will imply relations in the world other than the ones which the theory was specifically designed to account for. The theory is tested by seeing if these other implications are consistent with the facts. If they are not, then the theory is refuted. But in refuting the theory we will have learned some new facts, and the theory will have to be amended (or a totally new theory propounded) so that it is consistent with these new facts as well as with what

was previously known. Thus knowledge can progress through successive refutation and consequent reformulation of theories (p 14).

In economics there are many observations, for example the distribution of the national product, for which there is at the moment no satisfactory theoretical explanation. On the other hand, there are many predictions (free trade equalises factor earnings) which no one has yet satisfactorily tested. Generally the student must expect to find not answers, but a set of problems which provide the agenda for further theoretical and empirical research. Even when he does find answers to problems, he should accept these answers as tentative and ask, even of the most time-honoured theory: what observations will refute this theory? Economics is still a very young science (p 16).

In answering this third question, Lipsey's response draws once again on Popper. His reference to Koestler probably reflects Popper's view that theories begin as conjectures rather than from facts as Beveridge implies. Here we note an inconsistency in Lipsey's position. Hutchison never attempted to be as explicit as Lipsey and acknowledged a role both for an inductivist, and a hypothetico-deductive, approach. Lipsey also reflects Popper's view that knowledge grows through the successive refutation of theories. Here again, Lipsey's view is in line with Hutchison's critical stance and opposed to Friedman's apologetic defence of neoclassical theory. Yet, for the various reasons discussed above, Lipsey (and Archibald) did not accept Hutchison as a methodological ally against Friedman.

Conclusion

Our major organising distinction in Chapters Four, Five and in this chapter, has been Latsis's (1972) contrast between those concerned with an empiricist or Popper-inspired methodological criticism of the neoclassical programme and those concerned with its apologetic defence. In 1938 Hutchison was alone and ranged against him were Robbins, Knight (Chapter Four), and Friedman and Machlup (Chapter Five). In Chapter Six we saw that Hutchison, for the first time, was joined by the Popper-inspired methodological critics of the M^2T group. Yet, instead of teaming up with their fellow Popperian, some of these new critics engaged with Hutchison in a family quarrel that was quite as fierce as family quarrels can be. In this past chapter we have

put forward two factors that, we have argued, account for Hutchison's relative neglect by the M^2T group: Klappholz and Agassi's purist Popperism, and the fact that the group failed to be attracted to Hutchison's response to the difficulty of empirical testing in economics. Indeed, the group often appeared to be more caught up with the problem of empirical testing in economics than with joining Hutchison in his methodological criticism of orthodox economics. Nevertheless, Archibald (1961) mounted a powerful criticism of the Chicago economists attempts to defend orthodox price theory by criticising Chamberlin (1933).

In section 6.1 we examined the 1959-60 exchange between Hutchison and Klappholz and Agassi. We dealt with this exchange first and separately from the remainder of the M^2T seminar group for two reasons. First, their approach was concerned more with philosophical than economic issues and represented purist or ultra-Popperianism. Secondly, their contribution needs to be understood ahead of dealing with the M^2T seminar group because it was through them, and Agassi in particular, that the rest of the group came to acquire instruction in Popperian thought (de Marchi, 1988: 141, 148; Lipsey, 2001a).

We argued that Klappholz and Agassi's purist Popperian stance led them to a debate with Hutchison which was aimed, not so much at clarifying methodological issues so as to aid the practice of economics, but rather at promoting a philosophical end: the clarification of Popper's philosophy of science. They tried to show that Hutchison's twofold classification of statements with scientific sense was false. According to Latsis (1972) they wanted a third group to be recognised: metaphysical statements. According to Blaug (1980: 96) the statements they wanted recognised as a third group consisted of empirical propositions which were in principle untestable. We showed that their chief argument against his dichotomy - that propositions with unspecified clauses were ambiguous and could not be definitely fitted into either of Hutchison's categories - was not only resoundingly rebutted by Hutchison, but, more importantly, did not acknowledge that Hutchison was not concerned with water tight philosophical arguments. Rather than being concerned with a philosophical issue, Hutchison's main argument about ceteris paribus clauses was that they should be used 'less often and more cautiously' *in economics* (1938: 162).

In section 6.2 we examined Koopmans's 1957 claim that his approach and that of Hutchison (1938) were essentially similar. Examination of his 1957 essay indicated certain areas of similarity, but also those in which there were significant differences. The chief of these differences was Koopmans's decision to opt for mathematically tractable, rather than 'realistic', assumptions as part of his response to the difficulty of empirical testing in economics. Hutchison's 'wide' response to the difficulty of empirical testing in economics failed to influence Koopmans. Yet, to the extent that we found similarities between Koopmans and Hutchison, this, we argued, constituted evidence towards our view that Machlup (1955) had wrongly classified Hutchison as an ultra-empiricist. For Koopmans (1947) had famously argued against Burns and Mitchell's (1946) as 'unbendingly empiricist'. Now, only two years after Machlup's classification, he was happy to link himself explicitly to Hutchison's approach. The motivation for Machlup's criticism of Hutchison was not only his anti-empiricism. It was also his attempt to defend orthodoxy by dismissing one of its more independent and 'respectable' (ie not institutionalist or socialist) critics. We saw that Archibald (1959) adopted a critical stance towards Koopmans and accused him of adopting the 'Hutchison-Oxford' position in calling for the testing of assumptions. Apart from his error in linking Hutchison and Oxford, Archibald need not have worried about Koopmans's apparent concern with assumptions - it was strictly secondary to his advocating a more formal approach in economics.

In section 6.3 we showed how Klappholz and Agassi's (1959) criticism of Hutchison, and the fact that Archibald responded to the difficulty of empirical testing by seeking to deliberately narrow its scope, limited Hutchison's influence on Archibald. Nevertheless, we showed that Archibald's more moderate interpretation of Popper resulted in his advocating, albeit implicitly, Hutchison's view that empirical testing in economics had a central place and was not something to be swept aside à la Robbins as simply unnecessary. With regard to his appraisal of Friedman, two points should be noted. First, his criticism of Friedman's 'complacency' reflects his Popperian outlook. Second, his general acclaim for Friedman's 'revolutionary' essay can be read as a reflection of a broad approval of Hutchison, for, as we saw in Chapter Five, Friedman's essay is much more in tune with Hutchison's approach than Machlup (1955, 1956) made out to be the case.

In section 6.4 we attempted to show, by examining two examples of work that arose from the M^2T group, the extent to which this work followed in the same broad empirical tradition as that of Hutchison (1938). This helps to substantiate our Chapter Five conclusion that Machlup (1955) misrepresented Hutchison as a methodological extremist. We discovered that Klappholz and Mishan (1962) took up Hutchison's arguments to a surprising degree. Indeed their conclusion that 'the only equations required in economic models are empiric in nature' appears to go beyond Hutchison himself (Klappholz and Mishan, 1962: 126).

Apart from Archibald's reasons for neglecting Hutchison, we have seen how Lipsey had been influenced against Hutchison by Klappholz and Agassi (Lipsey, 2001a). Lipsey has spoken about how Robbins's methodology laid great stress on the intuitive plausibility of assumptions as *the* way to criticise a theory (Lipsey, 2001: 170, original emphasis). This may have been what Johnson (1978: 158) referred to as the emphasis on 'realistic' assumptions from the 1930s to the 1950s in British economics. Lipsey (1997: 213, 217) refers to the sense of a breakthrough in realising that one did not have to argue about he plausibility of assumptions. Instead one could look to predictions. These remarks indicate that Lipsey may also have been put off looking at Hutchison in more detail by Hutchison's talk of realistic assumptions, perhaps thinking it (also) involved reasonable or plausible assumptions. Nevertheless, it appears that it was for some such reason, rather than seriously substantive issues, that Lipsey side-stepped Hutchison. For, as we have shown in our examination of the introductory chapter of his famous textbook, Lipsey's methodological views are in fact similar in many ways to those of Hutchison (1938). Indeed, Lipsey (1993) favourably reviews Hutchison (1992) in which he continues to uphold his 1938 views in all important respects. And in a retrospective survey, he significantly notes 'Hutchison's earlier exposition of a similar position' to that espoused in his famous textbook (Lipsey, 2001: 171).

CONCLUSION

We begin our conclusion by pointing to two aspects of Hutchison's 1938 essay, aspects which appeared to attract the attention of most of Hutchison's critics. The first is Hutchison's principle of testability, or criterion of falsifiability (1960: vii, x). It has to do, he argues, with the same problem for which Popper (1957: 162) formulated his demarcation criterion for distinguishing between scientific and non-scientific statements. According to Coats (1983: 8), while something close to this concept was implied at various points in Hutchison's 1938 essay, the 'first published reference' to Popper's demarcation criterion occurred only in Hutchison (1960). As we have argued in Chapter Three, Coats's comment is somewhat misleading. For Hutchison had, to all intents and purposes, introduced Popper's criterion in 1938 rather than something only closely related to it. Indeed he expressed his preference for distinguishing between science and non-science rather than between sense and nonsense so following Popper rather than the logical positivists, although not acknowledging Popper explicitly on this particular point (1938: 19, n 8).

Coats's comment is only somewhat misleading because it does correctly draw attention to the nature of the relationship between Hutchison's ideas and those of Popper. While the criterion introduced by Hutchison was in substance no different from that of Popper, the implications for Hutchison of this criterion were different from those emphasised by Popper. For Hutchison, the criterion indicates whether or not we are dealing with an empirical inductive statement (ie the kind relevant for science), whereas for Popper it was valuable as a means of indicating whether or not we were dealing with a scientific statement while avoiding the problem of induction. This may have been what Coats had in mind in describing Hutchison's criterion as closely related to Popper's, for a few pages later on he raises the matter of 'significant differences' between Hutchison's empiricism and Popper's 'rejection of inductivism' (1983: 10-11).

Hutchison acknowledges that his criterion, in implying the significant distinction is that between science and non-science rather than the natural and social sciences, is

'naturalistic' (1960: xi). He contends that the main criticism of his criterion has stemmed from this particular implication, since it is held, notably by Knight (1940), that economics is concerned with purposive behaviour that cannot be empirically tested. He points out that on this issue his views have become 'considerably less "naturalist"' than in 1938. He now recognises many important differences, although these remain ultimately differences of degree rather than of kind (1960: xi-xii). Yet, we wish to point out that even in 1938 Hutchison appeared cautious about the applicability of the naturalistic thesis. At the end of his chapter one he explains that, while logical analysis may be used to remedy 'the unsatisfactory state of the foundations' of economic science, 'other deficiencies lie rather in the nature of the subject matter as compared with that of natural sciences and may never be thoroughly overcome in the same way' (1938: 18). On this matter it is noteworthy that he refers to Weber (1922) rather than a logical positivist.

Concerning this first key aspect, Hutchison's actual criterion or rule is in practice the same as Popper's and certainly removed from the logical positivist criterion of verifiability. Regarding his conception of the nature of science to which this criterion applies, we have argued that Hutchison's conception differs to a significant extent from either that of Popper or, for that matter, from the hypothetico-deductive version of logical positivism and its later developments. Instead, Hutchison emphasises the inductivist aspects of logical positivism - those aspects which so troubled Popper. In terms of the strong version of our thesis, Hutchison espouses inductivism-SM more generally in its role as part of the inductivist-empirical-historical side of the long-standing *methodenstreit* in economics. His essay is centrally concerned with support for this inductivist side of the *methodenstreit*, rather than for advancing the claims of logical positivism (or any other philosophy of science).

The second aspect of his 1938 book, which drew even more criticism, is his two-fold classification of empirical synthetic and a priori analytic statements. Hutchison (1960) does not mention that his classification is supposed to be exhaustive of all statements that have scientific sense. In 1960 he steers clear of this general epistemological proposition, acknowledging instead that it may well be inadequate for the philosopher, or in disciplines besides economics, and that it was a reaction to 'the dogmatic and extreme a priorism of Professor Mises' (p xxi). Nevertheless, he

upholds the relevance of his dichotomy to economics and replies to criticisms of its inadequacy by calling for examples of statements in economics that fall outside this classification.[1] He dismisses as unsatisfactory attempts by Klappholz and Agassi (1959), and Machlup (1955, 1956), to provide such examples.

This aspect of Hutchison's essay represents the section that is most clearly influenced by logical positivist thinking. Indeed, in Chapter Three we pointed out how Hutchison's view - that empirical investigation (of empirical synthetic statements) and logical analysis (of a priori analytic statements) is central to the business of science - is also central to logical positivism. We further pointed out that he regarded the purpose of his book as the logical analysis of the language of the science of economics (1938: 9). Clearly these central features of logical positivism play an obvious and basic role in Hutchison's methodology. Yet, we have questioned in this thesis just how basic much of logical positivism is to Hutchison's essay.

We detailed in Chapter One how the logical positivist view of science is related to their verifiability principle of meaning and, in turn, to the protocol-sentence debate. Yet, while Hutchison drew upon the literature surrounding in this debate, it appears that he did so with a limited purpose in mind, namely, that of supporting his methodology of economics. For instance in his chapter five (1938: 144) he draws on both Schlick (1936) and Carnap (1934, 1935) in sentences that follow on from each other, and yet does not refer to Schlick's basic disagreement in the course of the protocol-sentence debate with Carnap's interpretation of empiricism as 'itself framed by conventional and hence non-empirical choices' (Friedman, 1998: 793). This appears to provide some limited evidence for our (strong thesis) view that Hutchison was concerned first and foremost with issues related to the *methodenstreit* in economics rather than the philosophy of science. Even within the methodology of economics, his concern is with particular problems rather than with general issues, leaving such issues to 'specialist works on . . . scientific method' (1938: 12).

[1] 'My one request is for a number of clear examples. After all, I think it would be agreed that one had only to take up a book of principles, or a textbook, and one could, without any shadow of ambiguity or disagreement, point immediately to numerous examples of empirical statements and of tautologies' (1960: xxi).

It is the above two (logical positivist) aspects that have received practically all of the critical attention devoted to Hutchison's 1938 essay. Yet it would be a mistake to suppose that this was what Hutchison's intervention was all about. In terms of sheer physical space, these two aspects constitute only part of the first two of Hutchison's six chapters of his 1938 book. In the weak version of our thesis we have drawn attention to the extent to which the logical positivist aspect of his intervention has been overemphasised and the inductivist-empirical-historical aspect overlooked. In stark contrast to the attention lavished upon the logical positivist aspects of Hutchison's essay, Hutchison's concern with over-reliance on deductive reasoning in economic analysis has met with little or no comment in the economics literature. This is despite the fact that Hutchison repeatedly details his concern throughout his essay devoting whole sections of the leading chapters of his book to the deficiencies, of what he variously terms, the hypothetical method (in his chapter two), the 'optimistic' approach (in his chapter three) and the 'psychological method' (in his chapter five). His most important chapter, his chapter four, is all about exposing the extent to which the deductive approach in economics relies on the implicit, but vital, assumption that individuals entertain expectations that turn out to be perfectly correct. Hutchison has recently stressed that, although Keynes (1936) and Shackle (1967) criticised this assumption and drew attention to the importance of uncertainty in economics, they failed to point out its methodological consequences - 'that there is not this one postulate from which so much can be deduced' - as he had done in the conclusion of his chapter four in 1938, the chapter he regarded as his 'least uninteresting' (Hart, 2002).

Hutchison's exposure of how the traditional procedure in economics depends on the assumption of perfect expectations not only shows up the limitations of deductivism in economics, but provides us with a leading example of how the institutional and historical aspects the subject matter of economics (such as uncertainty) are neglected or side-lined in terms of the deductive method. While Hutchison has come to stress the importance of these aspects in economics more explicitly in his writings since 1938 (eg Hutchison, 1977, 1992, 2000), the importance with which he regards them is clearly evident in his 1938 work. Apart from his criticism that the 'closed deduction' of the 'optimistic approach' did not allow the possibility of (historical) change and hence of any dynamic analysis (1938: 73-6), he drew on one of the leaders of the

English historical school, Cliffe Leslie (1879, 1879a), in emphasising the importance of uncertainty in economics. Although he cites Schmoller (1883: 979) only once - with regard to Schmoller's contention that any worker in a chemical laboratory proclaiming Menger's conception of the exactness of scientific laws would be summarily ejected (1938: 59), this is most likely due to the fact that the circumstances of Germany in the late 1930s exacerbated the opprobrium surrounding Schmoller in Britain at the time (cf Hutchison, 1988). This may be why Hutchison (1938: 159) cited Clark (1931) instead in support of including the historical aspect into economics:

> Economic dynamics will, in its entirety, incorporate into itself historical economics. The changes that are going on in the world will in future be studied inductively, as well as deductively; and it is the inductive part of the work that falls to the historical economist. In the long run it is this part that will need to absorb the most scientific labour. The static laws of economics ought, consequently, to be known at an early date.

A final example of the importance Hutchison attached to the institutional and historical aspects of economics is his contention that any fruitful economic advice to a policy problem cannot be purely economic, but must be fully integrated with wider political and sociological investigations. It was to facilitate such investigations that he pointed to the need to 'begin more or less from the beginning with extensive empirical investigation' (1938: 166) - a remark, it may be remembered from Chapter Five, that Machlup seized upon as evidence of Hutchison's ultra-empiricism. While the motto with which he pre-empted his entire book was a quotation from Pareto (1935) regarding the importance of mathematical economics, the penultimate motto prefacing his concluding chapter is again drawn from Pareto (1935), but this time with regard to the importance of taking account of the 'inextricably interconnected[ness] of social phenomena' by means of extensive empirical investigation:

> Until economic science is much further advanced, 'economic principles' are less important to the economists than the reciprocal bearings of economics and the results of the other social sciences. Many economists are paying no attention to such interrelations, for mastering them is a long and fatiguing task requiring an extensive knowledge of facts; whereas anyone with a little imagination, a pen, and a few reams of paper can relieve himself of a chat on 'principles'.

Apart from the nature of Hutchison's 1938 intervention in economics, we have also been concerned with examining its influence as revealed in the journal debates

between 1938 and 1963, especially those that arose with Knight, Machlup and Klappholz and Agassi. Here, Weintraub's (1989: 478) distinction between two senses of methodology, 'Methodology' and 'methodology' appears pertinent. Methodology with a capital M has the same relationship to economics as the philosophy of science has to science. Methodologists' perspectives are developed from 'outside' economics and for that reason the work of Methodologists cannot have any consequences for the practice of economics. By contrast, methodological views aired by practising economists (methodology with a small m) as well as work in the history of economic thought can have significant consequences 'for our understanding of practice' (1989: 477-81).

While some may regard Hutchison (1938) as a clear example of a Methodologist, we have argued throughout this thesis against such an interpretation. Hutchison did not so much attempt to apply logical positivism to economics as use it to support an empirical-inductivist-historical approach to economics. Instead Knight, Machlup and Klappholz and Agassi appear much better candidates for being awarded Weintraub's capital M. Weintraub's (1989: 481) metaphor of economists using Methodology as a club with which to batter others may well be applied to their criticisms of Hutchison (1938) as positivist, ultra-empiricist and for not adopting a purist Popperism. This seems especially the case with Klappholz and Agassi who were more intent on clarifying Popper than with methodology as a way to make economics more practically useful.

While Weintraub argues that the work of Methodologists cannot have significant consequences for understanding the practice of economics, it appears from Chapters Four, Five and Six that they can have significant consequences in misleading economists about the work of other economic methodologists with a small m - in our case Hutchison. For example, this is how Ferguson (1969: 6) describes Hutchison's position as leader of the group of 'ultraempiricists':

> Instead of beginning with a system of axioms, the ultraempiricists presumably prefer to start with a body of what they call facts. Starting with facts of course entails sacrificing the very simplicity that is sought. One's approach immediately involves all of the complexities of the real world; and he (sic) is deprived of the use of the single tool - model analysis - that enables him to

> escape the morass of meaningless facts and to reach conclusions of some generality.

Ferguson is clearly influenced by Machlup's (1955) misrepresentation of Hutchison's position. Among the many errors in the above description, we attempt to correct but one: in his chapter two Hutchison explained the vital use and significance of pure theory (1938: 21 ff).

We argued in Chapter Five that Friedman (1953), except for his irrelevance-of-assumptions thesis, shared very much the same methodological position as Hutchison (1938). Weintraub cites Friedman (1953) as an example of a small m methodologist. From our perspective, he might well have also cited Hutchsion (1938). As noted earlier in this Conclusion, Hutchison was concerned with particular problems of economics and shunned general issues which he left to 'specialist works on . . . scientific method' (1938: 12). His detailed analysis in his chapter four of the particular way in which the traditional deductivist procedures in economics (the hypothetical method, the optimistic procedure and the psychological method) depend on the assumption of perfect expectation appears to be a good example of an economist practising methodology with a small m. In 1960, in replying to criticism of two-fold classification of scientific statements, he accepts that it may not be satisfactory for the philosopher, but makes it plain that he is concerned with its relevance to economics. In this respect he notes that examples of its inadequacy *in economics* have so far not been forthcoming (Hutchison: 1960: xxi). Finally, more recently Hutchison has addressed the issue pertaining to Weintraub's distinction as follows:

> I maintain this about the relations between economics and philosophy: the economists have got to make sense of what they're saying. If philosophers try to tell economists what they really mean they may not be right. I want to derive the foundations of the subject from inside the subject (Hutchison, in Hart 2002).

Here Hutchison's statement that he wants to work inside the subject corresponds nicely to Weintraub's description of this as true of methodologists with a small m as opposed to Methodologists with a capital M whose perspectives are developed from outside economics. Weintraub (1989: 492) concludes that progress in understanding the practice of economics requires 'not a philosopher-economist, nor a Rhetorician-

economist, but rather an historian-economist sensitized to the importance of ideas, and their context'. In terms of our thesis, Hutchison stands out as an exemplar of the historian-economist.

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