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FROM POSITIVE TO STRUCTURAL ECONOMICS: SOME UNANTICIPATED CONSEQUENCES OF THE RATIONAL EXPECTATIONS HYPOTHESIS

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From Positive to Structural Economics: Some Unanticipated Consequences of the Rational Expectations Hypothesis

A Thesis in

Economics

by

James Robert Wible

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of the Requirements
for the Degree of

Doctor of Philosophy

November 1980

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ABSTRACT

The thesis defended is that macroeconomic controversy since Keynes is nothing less than a debate over the appropriate world view for economics. In other words, the microfoundation of macroeconomics which is so important for interpretations of Keynes, the Keynesian Revolution, and economic policy is also a debate over the philosophical, psychological, and scientific foundations of economics. This is clearly seen in the most recent episode in macroeconomics, "the rational expectations revolution." The rational expectations hypothesis and the view of economic science on which it is premised, viz, positive economics, are the most consistent manifestation of the Newtonian world view in macroeconomics. Since a new world view is emerging in the latter half of the twentieth century, at the very least, we argue that the rational expectations hypothesis must be reconsidered and its most significant implications rejected.

More specifically, we argue that the question of the appropriate world view for economics can be focused on two issues, mind and rationality. These two issues are mirror images of each other, i.e., they are inherently the same issue. For richer theories of mind and rationality, we look to philosophy of science and modern science itself. A richer, process conception of rationality for complex decision-making (nonjustificational rationality) is now apparent in the work of Karl Popper, Thomas Kuhn, and Imre Lakatos. A richer theory of mind and the universe is now apparent in physics and cognitive psychology.

This is the holographic theory of David Bohm and Karl Pribram. The views of rationality, reality, and knowledge found in the preceding views are taken as components of a "structural philosophy of science." From this structural philosophy of science, we argue for a structural view of economic science.

With respect to recent macroeconomics, we believe that a concern for expectations per se is evidence of the psychological, philosophical, and scientific incompleteness of positive economics and rational expectations. However, even more specifically, we argue that a theory of mind is implicit in rational expectations. The mechanisms of expectation in rational expectations are nothing more than the utilitarian theory of mind, associationism, which most economists have forgotten. This gives added support to our contention that the rational expectations theory is essentially Newtonian in outlook.

Lastly, we attempt to give a simple analytical formulation which encapsulates our view of economic processes. This analytical device is called the Principle of Processing Complexity (PPC). It has the same integrative and interpretative role as Walras' Law in neoclassical economics. The PPC is formulated as a dualism, which simultaneously contrasts the nominal, tautological equivalence of monetary transactions with pervasive disequilibrium. But disequilibrium and indeterminacy are precisely the circumstances for human cognition and creativity to enter processively as determining factors. Thus, real human beings do have an essential and central role in our resulting, wholistic conception of economic processes.

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Chapter 1

INTRODUCTION

Increasingly the economics profession is witnessing a rising crescendo of controversy in two significant domains of inquiry, economic theory and methodology. Concerning economic theory, the search for an adequate microfoundation for macroeconomics increasingly reveals the inadequacy of conventional, neoclassical micro (price) theory. Concerning methodology, the search for an adequate philosophy of science to underpin Friedmanian methodology increasingly reveals how unpositivistic and ad hoc positive economics is. Usually economic theory and methodology are viewed as independent but compatible domains of inquiry. The tasks of the theoretician proceed independently of the inquiries of methodologists. This is not to say that theoreticians are disinterested in methodology. Our best-known economic theorists take time, now and then, to set forth their views on methodology. Milton Friedman and Paul Samuelson are two cases in point.

Although theory and methodology are often viewed as autonomous domains of inquiry, an alternative strategy can be pursued. The question can be raised whether economic theory and methodology (appropriately interpreted) concern similar substantive issues. If theory and methodology ultimately address similar issues, then both domains of inquiry might be enhanced by integrating the two. In what follows, we shall maintain that methodology and economic theory are most fundamentally concerned with the nature of human rationality.[1]

In economics, we believe that the concern for rationality is manifest in two distinct but related ways. Most recently, a concern with rationality is manifest in the rational expectations hypothesis. The rational expectations hypothesis takes maximizing behavior as the _sine qua non_ of rationality. Rational expectations theory is the latest "revolution" in macroeconomics since Keynes.[2] But even more significantly, we believe that a view of human rationality relates to an issue which has chronically plagued theorists before and after Keynes. This issue is how money can be meaningfully incorporated into economic analysis. We hypothesize that monetary controversy since Keynes is basically a consequence of a restrictive, reductionistic conception of rationality which makes the analysis of a monetary economy impossible. Conversely stated, the first step that must be taken to incorporate money into economics is to find a more adequate conception of rationality.

Concerning the rational expectations hypothesis, although it calls attention to the issue of rationality in a monetary context, we believe it is the best example of the tendency toward a reductionistic conception of rationality in economics.[3] Rather than addressing monetary phenomena, we maintain that the rational expectations hypothesis essentially abstracts from the peculiar problems of a dynamic monetary economy.

In recent and contemporary philosophy of science, the debate over rationality is less obscured than in economics. Although they do not always agree, a concern with rationality is manifest in the works of Popper, Kuhn, and Lakatos. What they have in common is the realization that they have a very different conception of the rationality of scientific inquiry than found in logical positivism. (Logical positivism is the empirical philosophy of science that was dominant in the early and mid twentieth century.) What they do not have in common is a consensus concerning psychological aspects of complex decision-making processes. Popper and Lakatos reject but Kuhn accepts psychological aspects concerning the rationality of scientific inquiry.[4]

Rather than merely attempting to integrate aspects of rationality apparent in economic theory and in philosophy of science, we shall argue from philosophy of science to economic theory. This granting of conceptual priority to philosophy of science is done for three reasons. First, we need to develop an alternative conception of rationality to show how restrictive the maximizing notion of economic rationality is. Second, the discussion of rationality in philosophy of science deals with the problem of rationality at a more abstract level; in other words, at a metatheoretical level.[5] A metatheory of rationality is a theory of what criteria are needed to constitute a conception of rationality. Third, philosophy of science has more fully assimilated developments in twentieth-century science. Criteria of rationality in philosophy of science are much freer of Newtonian, mechanical conceptualization than is conventional, neoclassical economics. This means that a coherent, non-Newtonian world view is emerging from our understanding of the basic results of modern science during the last one hundred years. Nowhere is this change of world view more relevant than to the human mind and rationality and to monetary controversy since Keynes. In other words, we believe monetary controversy since Keynes is nothing less than a debate over the appropriate world view for economic science and that this debate ultimately must focus on the issues of human rationality and mind.

POSITIVE ECONOMICS AND RATIONAL EXPECTATIONS

By maintaining that monetary controversy since Keynes concerns the appropriate world view and concept of rationality in economic science, we are challenging one of the most basic aspects of positive economics--that the major differences among mainstream economists are empirical and not philosophical.[6] However, with the latest positivistic episode in macroeconomics, the "rational expectations revolution," we argue that it is no longer possible to ignore how fundamental the differences are among mainstream economists. The rational expectations hypothesis, we believe, makes it obvious that the differences among economists concerning a monetary economy are not just empirical ones. Issues dividing mainstream economists which were and often still are considered to be empirically resolvable are: the slopes or elasticities of the IS-LM functions, the magnitude of the marginal propensity to consume, and the relative speed of adjustment of price or quantity variables. Although these issues are significant as traditionally conceived, the attention and emphasis given to these issues tends to obscure more fundamental matters, like a change in world view and the nature of human rationality.

Concerning the rational expectations hypothesis, we argue that it is the most unified conception of economic <u>science</u>, economic <u>theory</u>, and economic policy that has ever been developed within a static Newtonian world view. The rational expectations view takes each component of its world view (philosophy of science, theory, and policy) to its logical conclusion. Furthermore, the rational expectations hypothesis gives the appearance of mathematical elegance, conceptual simplicity, and relevance to our most difficult of current economic problems--simultaneous inflation and unemployment.

The source of the unity found in rational expectations results from a bold hypothesis. This hypothesis seems to obliterate the distinction between economic science, economic theory, and economic policy. In their theoretical and empirical research, rational expectations theorists essentially hypothesize that rationality is epitomized by maximizers who, in their market transactions, on average achieve the same predictions of the future course of economic activity as do professional econometricians with their models. Sargent and Wallace (1979 [1976]:110) summarize the hypothesis as it concerns rationality:

> The rational expectations hypothesis . . . accords with the economist's usual practice of assuming that people behave in their own best interests. This is not to deny that some people are irrational and neurotic. But we have no reason to believe that those irrationalities cause systematic and predictable deviations from rational behavior that a macroeconomist can model and tell the monetary authority how to compensate for. In this regard, it should be noted that the rational expectations hypothesis does not require that people's expectations equal conditional mathematical expectations, only that they equal conditional mathematical expectations plus what may be a very large random term (random with respect to the conditioning information).

Then, from their view of rational transactors and markets, Sargent and Wallace (1979 [1976]:112) argue for a strong relationship between economic science, theory, and policy:

> The conundrum facing the economist can be put as follows. In order for a model to have normative implications, it must contain some parameters whose values can be chosen by the policymaker. But if these can be chosen, rational agents will not view them as fixed and will make use of schemes for predicting their values. If the economist models the economy taking these schemes into account, then those parameters become endogenous variables and no longer appear in the reduced-form equations for the other endogenous variables. If he models the economy without taking the schemes into account, he is not imposing rationality.

What rational expectations theorists apparently have assumed is that rational transactors use information as efficiently as any scientist.[7] In other words, the rational expectations hypothesis suggests that transactors on average evaluate information as effectively as scientific economists. James Tobin (1972:13), in a conference volume titled, The Econometrics of Price Determination, clearly links the rational expectations theory to a scientific mode of decision-making:

> The structure of the economy, including the rules guiding fiscal and monetary policy must be stable and must be understood by all participants. The participants not only must receive the correct information about the structure but also must use all of the data correctly in estimating prices and in making quantitative decisions. These participants must be better econometricians than any of us at the Conference. If they are, they will always be--except for the inavoidable mistakes due to purely random elements in the time sequence of aggregate money demand--at their utility and profit-maximizing real positions.

What rational expectations theorists seem to be doing is making rational economic man the equivalent of the positive economist. In other words, in rational expectations theory, rational economic man has become positive economic man. Each transactor is the equivalent of a positive, scientific economist.

This assimilation of rational economic man to positive economic man cannot go unchallenged. One way of challenging the concept of positive economic man in rational expectations is by elaborating a much richer, more human picture of man in the scientific process. Thus, the positivistic conception of science may be misleading and need correction. A more realistic picture of scientific activity could provide a more realistic picture of economic activity, insofar as these activities are similar.

PHILOSOPHY OF SCIENCE AND RATIONALITY

From the structure of our argument outlined in the previous sections, it should be apparent that the change of world view, which seems so essential for monetary analysis and for assessing rational expectations, also can be focused within philosophy of science. The change from an equilibrium-Newtonian world view to an evolutionary-process view is paralled by the change from a static-positivistic epistemology to a process-structural approach to knowledge as found in the works of Popper, Kuhn, and Lakatos. As we shall see, this change in world view can also be conceived in terms of rationality.

Behind this change of view in philosophy of science lies an awareness of one of the most fundamental dilemmas of Western thought. The dilemma is that our conceptual systems, relying heavily on classical logic, are inconsistent or infinitely regressive. We can illustrate with empiricism. In a logical sense, consistency requires an empirical justification for empiricism; otherwise empiricism will have no empirical validity and be inconsistent. However, empirically justifying empiricism leads to an infinite regress. The infinite regress appears when the empirical justification of empiricism similarly requires additional empirical justification. This sequence could be repeated *ad infinitum*.

Traditionally, those who encounter the dilemma of inconsistency and infinite regress behind empiricism have become skeptics. David Hume (1955 [1748]) is the classic example of an empiricist who realized that empiricism is not empirically justifiable and therefore became a skeptic. A skeptic is one who believes that knowledge is unattainable because no hypothetical statement of fact can be justified with complete certainty.[8]

Until Popper no one gave an adequate response to the type of skepticism epitomized by Hume. However, Popper took philosophy of science in a new direction. Rather than attempting to resolve logical dilemmas of empiricism, Popper shifted the problem of knowledge from a logical structure of justification to understanding the process of acquiring knowledge. In other words, Popper turned toward the growth of knowledge and deemphasized the logical problem of justification. Consequently, the view of rationality embedded in Popper's work as well as in Kuhn's and Lakatos' works is now called nonjustificational rationality. As a conception of scientific activity, nonjustificational rationality suggests that empirical justification is only one aspect of the processive growth of knowledge and many times one of the less significant aspects.

More systematically, a complete philosophy of science requires more than a theory of rationality. A complete philosophy of science has three basic parts which are highly interrelated and must be mutually compatible (see Figure 1). These parts are: a theory of knowledge (epistemology), a theory of what it is that is (or could be) known when knowledge is obtained (ontology or a theory of reality),

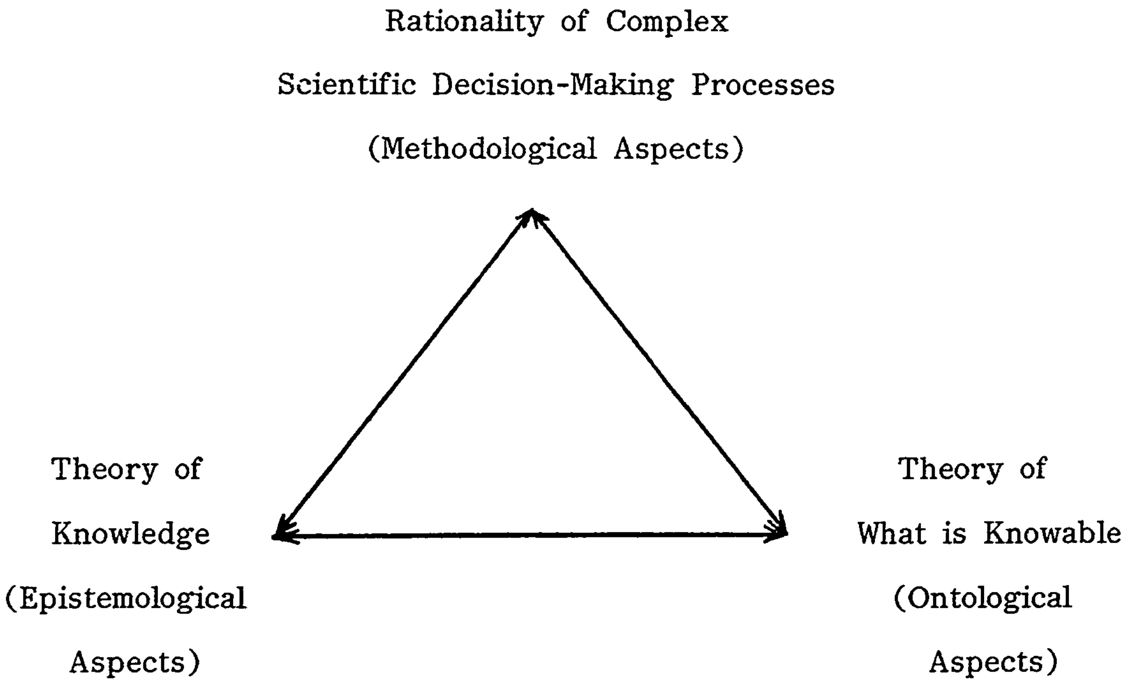

Figure 1 Components of Any Complete Philosophy of Science with the Arrows Indicating the Interrelated Nature of the Components

and a theory of scientific decision-making (methodology or the rationality of science).[9] Although there is no general agreement among philosophers of science about which particular concepts of knowledge, reality, and rationality constitute the best general philosophy of science, we shall choose concepts which seem to be most consistent with each other and which also are evidentially based on the results of modern science. The criterion used in selecting our components of a philosophy of science is structure. Structure, in our judgement, captures the historic concern of science with pattern, process, hierarchy, and order in our world and in our knowledge of our world. Thus, we argue for a structurally oriented epistemology, a structural theory of reality, and a structural theory of rationality as the components of a "structural philosophy of science."

THE DISUNITY OF ECONOMIC THEORY: A DIGRESSION ON TERMINOLOGY

Our thesis that money ultimately concerns rationality and that rationality implies a change in world view is a significant departure from traditional views of macroeconomic controversy since Keynes. Contrary to positive economics, we argue that philosophical differences divide mainstream economists. Besides rationality, another major philosophical difference concerns economic theory--whether economic science is a unified science. Traditionally, positivists take a unified-science perspective. By a unified science, we mean a science which has theoretical and empirical branches that differ methodologically but not substantively. In a unified science, all of its branches supposedly focus on the same subject matter and share common theoretical and empirical languages.

Since we reject positivism as an adequate philosophy of science, we also reject the unified-science conception of economic science. The implications of such a rejection are fundamental and extend to our use of words and terminology. For example, the most widely used terminology to describe economic theory, "micro" and "macro," seems to predispose economists toward a positivistic conception of economics. These terms imply that economics is basically divided into two theoretical branches that differ only by a methodological procedure--aggregation.[10]

But this is not all of the damage. The micro-macro terminology, when taken as the basic theoretical division of economic analysis, also suggests that other types of economic theories do not exist or are inferior types of theories. We believe the micro-macro terminology directs attention away from economic theories concerned with non-equilibrium phenomena. Theories like cycle theory, monetary theory, the theory of economic development, the theory of economic history, and the theory of economic institutions obviously address different types of human economic activity than equilibrium theories. Consequently, the theoretical, empirical, and methodological dimensions of these branches of economics may vary quite significantly from neoclassical micro and macroeconomics.

In the chapters that follow, we occasionally appear to use the terms "micro" and "macro" to refer to economic analysis in the conventional manner as found in many texts. However, it should be apparent that the unified science connotation that is associated with the terms "micro" and "macro" is rejected. We argue that "microeconomics" and "macroeconomics" can be construed as substantively different theories of economic activity. Nomenclature which captures our intention somewhat more accurately is a more recent term--"microfoundation." Apparently the economics of Keynes and neoclassical economics have different "microfoundations."

Because the search for a "microfoundation" is relatively recent, a degree of ambiguity may be associated with this line of inquiry. To remedy such ambiguity we need to develop a theory of what distinguishes one microfoundation from another; or equivalently, what makes economic theories substantively rather than just methodologically different. We maintain that there is only one adequate criterion for a microfoundation or substantive theoretical differences in economics. This criterion is a conception of rationality. In other words, we take a metatheory of rationality as an adequate theory of substantive theoretical differences in economics. A metatheory of rationality may suggest that there are different levels of complexity to human activity. If, in fact, there are different levels of complexity in human activity, then the obvious tact would be to have a different theory, methodology, and concept of rationality for each level of complexity.[11] Levels of complexity are a basic aspect of the real world which positive economists seem to ignore. As a basic aspect of human existence, economic theories keyed to levels of complexity would differ both substantively and methodologically.[12]

ORGANIZATION

The strategy of our undertaking should now be apparent. We shall develop a "structural philosophy of science" in order to develop adequately a structural-process conception of rationality--nonjustificational rationality. Nonjustificational rationality will then serve as a conception of rationality against which the rationality of rational expectations can be juxtaposed. Then the outlines of a structural approach to economics can be suggested, although any view of "structural economics" must remain highly conjectural at this stage.

We begin, as already stated, by suggesting that economic theory and methodology share a common substantive issue, the rationality of complex decision-making processes. As background for developing our structural philosophy of science, we consider positive economics and how it relates to philosophy of science. This comparison is done by assessing postive economics in light of logical positivism and by analyzing the Keynesian Revolution as a non-positivistic episode in the development of economics. With this background, we then proceed to a more general discussion of philosophy of science and rationality.

In Part II, we consider the nature and significance of the rational expectations hypothesis. The significance of rational expectations primarily relates to our basic understanding of so-called macro or monetary economics and processes. As such, we assess some developments from Keynes to rational expectations theorists. All conventional monetary theorists since (and perhaps including) Keynes seem to share an inadequate conception of economic rationality. In rational expectations, this limited conception of rationality is applied universally, without limitation, to all domains of economic phenomena. To support our claim that the rationality of rational expectations is too narrow, we investigate one question in detail. We inquire whether there is a theory of mind in rational expectations. We conclude that a Newtonian, mechanistic theory of mind is implicit in rational and other expectations theories. This mechanistic theory of mind is called associationism. Because mechanistic, associationistic conceptions of mental activity permeate economic theory and research, we conclude that a Newtonian, mechanistic conception of rationality pervades economic theory. Thus, economic rationality may be a one-dimensional analogue of a multi-dimensional (nonjustificational) conception of rationality. Part II ends with an assessment of several unconventional views of rationality held by some economists. These unconventional concepts or theories of rationality permit us to explore the notion of levels of complexity in human activity in some detail. Also, this assessment shows that some economists are beginning to address the issue of rationality, as well as philosophers.

In Part III, we outline the basis of a broader approach to economics than positive economics and its one-dimensional concept of rationality. Since we argue from a structural philosophy of science, this approach to economics is called structural economics. Structural economics would encompass new notions of knowledge, rationality, and reality current in science today. Specifically, in Part III, we develop a richer conception of man in the economic process and explore basic concepts like the validity of various theoretical dichotomies in economics.

Then we end with a beginning, a tentative definition of structural economics.

SUMMARY

Rational expectations theorists aim to develop a theory of a modern monetary economy which is adequate to address simultaneous inflation and unemployment. To achieve this result, the neoclassical microeconomic framework is extended to the analysis of economic phenomena in a rapidly changing and inflationary economic environment. In such transient economic situations, expectations obviously play an important role.

As hinted above, we shall maintain that the genuine assimilation of expectations into economics will change the scientific world view of the economist.[13] The theoretical and conceptual premises of the rational expectations hypothesis found in neoclassical economics systematically preclude expectations in any humanly experienced sense. Simply put, rational expectations theorists do not have an intellectual vehicle which will take them where they want to go. This is why the problem of rationality in the context of modern science and philosophy of science must be considered first.

NOTES

[1]A similar point of view is found in Godelier (1972:9): "The question of economic rationality [of transactors] is thus at the same time an epistemological question, concerning political economy as a science."

Parsons (1968 [1937]:58) states, "Since science is the rational achievement par excellance, the mode of approach here outlined is in terms of the analogy between the scientific investigator and the actor in ordinary practical activities."

Scheutz (1943:134) says, "Considered purely as a human activity, scientific work is distinguished from other human activities merely by the fact that it constitutes the archetype for rational interpretation and rational action."

Weimer (1974b:375-376) represents the same point of view most clearly: "The pattern of reasoning or inference that leads to knowledge is the same for science and for common sense. . . My claim is that a correct characterization of scientific knowledge is also a correct characterization of knowledge simpliciter." Weimer (1975) develops this point of view in great detail.

[2]B. T. McCallum (1980:40) refers to the rational expectations hypothesis as a revolution in macroeconomics.

[3]Brian Kantor (1979:1429) makes the point: "The rational expectations approach meaningfully extends the use of what Coddington describes as the reductionist method as applied in timeless general equilibrium analysis." Kantor's article is an extended review of the rational expectations literature. Other critiques of rational expectations have been offered by Berkman (1979) and by H. A. Simon (1979) in his Nobel lecture.

[4]For a discussion of this controversy, see Kuhn (1970b). Our treatment of rationality is closer to Kuhn's views than those of Popper and Lakatos. We believe the complex psychological process which Keynes discusses in the General Theory are more compatible with a Kuhnian view of complex decision-making processes.

[5]By a metatheory we mean a generative conceptual framework. Metatheory is more completely defined in the glossary which follows chapter ten. For terms which may be unfamiliar to the reader, please refer to the glossary.

[6]For Friedman's statement, see note 3 of chapter two, p. 43. We need to call attention to some uses and misuses of the term positive economics. What we mean by positive economics is the somewhat systematic view of economic science that is espoused by Milton Friedman (1971 [1953]) or one of his interpreters like Lawrence Boland (1979). However, in informal conversation, we often find that many more economists describe themselves as "positive economists" than we would categorize as such. After closer inquiry, we usually discover that the term positive has a broader use than the systematic, Friedmanian approach to economic science. We find that "positive" is often used as a substitute for "objective." Since one can be an objective scientist without being a positivist, we believe this is an unfortunate and mistaken use of "positive." Hopefully, much of what we have to say will be relevant to this large class of objective economists who really are not positivists as the term is correctly used.

[7]Robert E. Lucas as quoted by Guzzardi (1978:74) states, "'We think people are as smart as we are we don't patronize them. They don't have to be brilliant or study the latest figures from the Fed. They do have to know about inflation and the effect of government spending but you'd have to be living underground not to know about that by now. If you assume that people are stupid and can't see opportunities to act in their own interest, you're wrong. We say you must purge your models of such features and build in intelligence.'"

[8]See note 20, chapter four, p. 116.

[9]We emphasize three aspects of philosophy of science because two of them are often ignored, particularly by positivists. Because of the skepticism posed by Hume and the "nonsense" supposedly associated with metaphysical notions of reality, positivists largely have turned their attention away from epistemological and ontological aspects of philosophy of science. What remains is the belief that science is the most rational of human endeavors. Since the success of science cannot be explained on epistemological or ontological grounds, emphasis turns toward technique. Thus, philosophy of science for the positivist gets reduced to a mechanical criterion or methodological technique like predictability or falsification. Thus, the scientist can conduct his professional research "rationally" without ever addressing fundamental issues. While this may be an effective strategy for intellectual specialization in the short run, sooner or later, we maintain, such inattention to basic issues will constrain the advancement of economics. Perhaps economics is already at this point.

[10]The rational expectations hypothesis is the best example of a positivistic, unified-science theory which views "micro" and "macro" as differing only methodologically. For example, Preston Miller (1976: 43-45) outlines a "microeconomic approach to macroeconomic policy." His dissatisfaction is that macroeconomics and monetary theory "have not made enough use of models which contain behavioral relationships grounded explicitly in theories of individual optimization." The implementation of Miller's suggestion would make the concepts, language, methods, and substance of micro and macro almost identical.

[11]It appears that two economists, Nicholas Georgescu-Roegen and H. A. Simon are already headed in this direction. See chapter seven and Figure 8, page 225 for a summary of their views on rationality and economics.

[12]We must point out that a genuine scientific revolution substantively advances a discipline; otherwise there is no scientific revolution, only an exercise in the social psychology of academic interchange. For example, we can hardly imagine Einstein's contributions being seen as purely methodological. Since we premise our interpretation of Keynes on an emerging world view with novel concepts of the universe, man, rationality, time, and process, we feel we are justified in suggesting his theoretical contribution is substantive as well as methodological.

[13]Daniel Fusfeld (1980:42) expresses a similar perspective concerning the philosophical foundations of economics: "A new world view is emerging in the last half of the twentieth century, the outlines of which can dimly be perceived. It has reached the social sciences through controversy about the methodological problems of logical empiricism. In economics, it has caused the breakup of the general equilibrium model from within, the result of pioneering work in pure theory rooted in the ideas of the emerging world view. The need for a new synthesis of world view, scientific method, and causal explanation is apparent."

PART I

PHILOSOPHY OF SCIENCE

Chapter 2

LOGICAL POSITIVISM AND POSITIVE ECONOMICS

Since philosophy of science is unfamiliar to many economists, it is necessary that we begin with concepts, terms, and issues familiar to the economist. As background for a subsequent and fuller consideration of contemporary philosophy of science, we explore the relationship between positive economics and philosophy of science. In particular, positive economics is compared with logical positivism. Logical positivism is the dominating view of science in the early decades of the twentieth century. The obvious question that arises is: How positive is positive economics when compared to the well-known view of science, logical positivism? This question has intrinsic merit--even if we were not concerned with pursuing philosophy of science in any more detail.

This chapter is divided into three parts. We begin by considering the apparent dominance of positive economics as a conception of economic science. Then logical positivism is presented in some detail. Finally, the substance of positive economics is discussed and compared to logical positivism.

THE DOMINANCE OF POSITIVE ECONOMICS

Positive economics, perhaps more than any other view of economic science, apparently dominates economics in the English-speaking world, particularly in the United States. This can be illustrated if one peruses widely used introductory and intermediate theory texts. In these texts, Friedman's (1971 [1953]) well-known essay, "The Methodology of Positive Economics," seems to be the dominating view of economic science.[1] Blaug (1976:149) even states that Friedman's essay is "the one article on methodology that virtually every economist has read at some stage of his career." Curiously, by way of contrast, a large number of texts either refrain from embracing positive economics, or do not embrace any view of economic science at all.[2] Keynesian introductory texts, like Samuelson's Economics (1980), confine their discussion of science to models and theories. Discussion of a comprehensive, unifying view of science and economics seems to be avoided. Additionally, intermediate macroeconomics texts often contain no discussion of economic science or theories and models. Usually these texts use a methodological issue as a point of departure. Macroeconomics is presumed to be aggregated microeconomics with no substantive difference in content. Branson and Litvack (1976:57) epitomize this view:

> Macroeconomics is really just aggregated microeconomics--the trick being to aggregate zillions of micro activities and markets in a way that improves our understanding of how the economy works.

Hollis and Nell (1975:155) criticize this conception of the relationship of micro and macro theory as being positivistic:

> Micro-economics is often introduced to students, as if it were more basic than macro-economics. The economic world is composed of economies, composed of firms and markets, composed of individuals. So it seems natural to start with atoms and work up to the more complex structures. This strategy accords well with Positivist epistemology, since all empirical knowledge is thought of as a hierarchy in which each level is supported from below until a foundation of simple observation statements is reached.

It is often suggested that the major differences among economists are empirical rather than theoretical or methodological.[3] However, an alternative view can be drawn. In the face of a simple-unified conception of economic science, viz, positive economics, some economists like Samuelson, seem to mute their non-positivistic views on the nature of science and economics. Others embrace alternative conceptions of economic science; Ward (1972), Hollis and Nell (1975), Shackle (1972), and Georgescu-Roegen (1971). Yet others attempt to minimize the positivism in Friedmanian positive economics. Although Friedman makes no reference to Popper in his methodology article, Blaug (1976:149) suggests that Friedman is "Popper-with-a-twist applied to economics."[4] It is this last response to positive economics which is most confusing. Reading each new development of philosophy of science into Friedmanian positive economics, seems less than legitimate. Positive economics effectively becomes an intellectual open door to which all economists pay lip service and through which all new ideas on economic science must pass. Needless to say, the simple Friedmanian conception of economic science will dominate obscure reinterpretations of positive economics and obscure, unclear, and eclectic views of economic science. An intellectual vacuum, real or apparent, may be filled just as readily as any physical vacuum.

The inescapable conclusion then is this: For most economists who are not concerned with philosophy of science, Friedmanian positive economics is the dominant conception of economic science. However, if this positive view of economics were shown to be in error, the profession would be in dire straits. Most of the profession would be committed unwittingly to an erroneous conception of economic science while others would compound the confusion by expanding positive economics to encompass each new trend in philosophy of science. But the erroneous nature of positive economics can be demonstrated not only by appeal to more recent philosophy of science, but also from the perspective of logical positivism. Ernest Nagel (1971 [1963]:48-49) has written an article suggesting that Friedman's arguments are wrong while his overall point of view is sound:

> Sound conclusions are sometimes supported by erroneous arguments and the error is compounded when a sound conclusion is declared to be mistaken on the ground that the argument for it must be mistaken. . . I hope to show that despite the inconclusiveness of his [Friedman's] argument his conclusion is sound.

To understand why Nagel regards Friedman's argument concerning positive economics as erroneous, the nature of logical positivism first must be considered. The thrust of Nagel's argument apparently has not been assimilated by economists. Only Nagel's conclusion that Friedman has good conclusions seems to have been heard by economists. This is to be expected since Nagel is known as a former leader of the logical positivistic movement in the United States (Kraft, 1953; Feigl, 1969:8-9; Ayer, 1959:7). Yet, even if Nagel is correct--that Friedman has good positivistic sensitivities despite problems with his arguments--important consequences can follow. If Friedman and other economists follow Friedman's erroneous arguments in carrying out their research, then Nagel's sympathetic conclusion may need to be reversed. Since Nagel is not an economist, he cannot be held responsible for knowing whether the research of economists follows his or Friedman's arguments. We argue in the last section of this chapter, that Friedman recommits in his monetary research, the mistakes Nagel finds in his methodology article. We turn now to a consideration of logical positivism.

EMPIRICISM AND LOGICAL POSITIVISM

The philosophy of science, logical positivism, provides a conceptual bridge to one of the dominant lines of intellectual development since the rise of modern science several centuries ago. This line of intellectual development is known as empiricism. For several centuries empiricism was centered in the English-speaking world. Thus, prior to the twentieth century, British empiricism was the chief rival of continental rationalism. However, in the twentieth century, the center of empiricism briefly shifted to the German-speaking world. During the 1920's and 1930's, Vienna became a leading center of empiricism. Logical positivism as a philosophy of science began in what is now known as the Vienna Circle.

However, it was not long until the English-speaking world was again the center of empiricism. Due to World War II, the Vienna Circle dispersed largely to the English-speaking world. This diaspora of some of Europe's best minds increased the accessibility of the Circle's writings in English. Consequently, many of the original members of the Vienna Circle became well-known in the United States and Britain. Some aspects of the legacy of the Vienna Circle are historical and conceptual accounts of logical positivism. These accounts provide a basis for our discussion of logical positivism. One must be cautious about generalizations, but the nature of logical positivism can be summarized by five major characteristics. They are:

(1) an anti-metaphysical outlook, (2) a verifiability criterion of scientific meaning, (3) an anti-psychological (mentalistic) stance, (4) an emphasis on physicalism, and (5) an emphasis on reductionism and the unity of science. These characteristics will be considered by dividing them into two major groups. Initially, the first two are considered together; then the last three.

Verification and the Anti-Metaphysical Outlook of Positivism

The anti-metaphysical outlook of logical positivism was the most fundamental characteristic which united the movement. For example, Ayer (1946) titled the first chapter of his famous work, Language, Truth and Logic, "The Elimination of Metaphysics." Metaphysical statements, as viewed by positivists, were meant to be knowledge claims about reality that transcended phenomenal experience. Following Hume and Comte, positivists declared that such non-empirically based statements were meaningless (Feigl, 1969:5). To the members of the Vienna School, such meaningless statements of metaphysics abounded. German philosophy of this period (nineteenth century) was dominated by transcendental speculation based on Kant and Hegel (Bergmann, 1954:5; Suppe, 1954:6-7). Talk of absolute or transcendental entities or substances could not be ascertained as true or false. Consequently, such concepts allegedly contributed nothing to human knowledge (Ayer, 1959:10).

Philosophy, if it were to be a genuine source of knowledge for the positivist, had to abandon speculative metaphysics. To remedy the fact that speculative metaphysical statements had no empirical referent, science was advocated as an appropriate model for philosophy. Since scientific propositions were based on sense experience and metaphysical statements were not, positivists suggested that science made "sense" and metaphysics was "non-sense," (Ayer, 1946:41). In its early stages, positivism was characterized as "a philosophy to end all philosophies. . . a decisive turn toward a new form of enlightenment," (Feigl 1969:4). The aim was to separate "factual propositions" from "metaphysical pseudoproblems and pseudosolutions of such problems," (Feigl 1969:5). Science was to have a key role in the development of critical thought. Philosophy had to be scientific.

The analysis of language played a crucial role in the positivists' program to rehabilitate philosophy. Empiricists who preceded the positivists maintained that the most abstract concepts and statements of human thought were empirical generalizations. For example, John Stuart Mill (1973 [1872],I,:193,214-215; 1974 [1872],II,:854) held that all laws, including those of mathematics and logic, were ultimately empirical; only the sense of particularity was lost through time.[5] However, even before Mill, Kant (1943 [1791]:7-9) raised the issue whether there were statements without any reference to empirical fact. This lead to his analytic-synthetic distinction. Analytic statements concerned no matter of fact and were true in virtue of the rules of logic. Synthetic statements were statements of fact which only were true after the fact. Positivists who wished to rehabilitate Mill's crude version of empiricism had to reconstruct empiricism. They had to take into account that genuine analytic statements, independent of experience, were possible and found in mathematics and logic (Kraft, 1953:19, 22-23; von Mises, 1951:5).

In the hands of the positivists and as a result of their concern with language, the analytic-synthetic distinction was transformed into another distinction. The language of science, in their view, was separable into a theoretical language and an observational language. The theoretical language was to be a symbolic, analytical language; its criteria of truth were the laws of symbolic logic. The observational language was considered to be more primitive than the theoretical language (Kraft, 1953:37), containing simple terms which unambiguously referred to entities in the world external to man. To be meaningful, a theoretical statement had to have a corresponding observational referent. Both languages, in principle, were considered to be related by correspondence rules. Correspondence rules defined one-to-one relationships between theoretical propositions and observational terms. Theoretical statements without any observational counterpart were held to be unscientific or nonsense.

Correspondence rules served several functions (Suppe, 1974: 17-28). They specified the admissible experimental and empirical procedures for applying a theory to phenomena; they defined theoretical terms; they guaranteed the cognitive significance of scientific theories (Suppe, 1974: 17).[6] It was this last function (cognitive significance) which led to a consideration of the second characteristic of logical positivism, verification. Since the aim of logical positivism was to distinguish scientifically meaningful statements from "nonsensical" statements, a criterion of meaning was needed to distinguish these two types of statements. A theory was verified if singular statements constituting observational reports were in agreement with the theory (Kraft, 1953:136). If all experimental cases gave a positive result, then the theory was considered to be verified. However, verification presented a logical difficulty. Verification was never achieved with finality unless all possible events or outcomes had been observed (Ayer, 1946:5-16, 135).[7]

Testing was distinguished from confirmation in the following manner. A theoretical statement was considered testable, if a method of experimentation was actually known and technologically feasible; a statement was considered confirmable if potential conditions were known under which the theoretical statement "would be" testable. Confirmation was a conception which did not take account of technological and other types of limitations to scientific inquiry. Thus, theories were envisioned which were confirmable but not testable because of technological, economic, or political constraints which could prevent actual testing.

Confirmation as a criterion of meaning resulted in some changes in logical positivism. The aim of positivists was to reformulate empiricism in light of the Kantian analytic-synthetic distinction. By his own account, Russell noted the influence which his and Whitehead's work on logic and mathematics had on positivists concerning the validity of analytical statements (Russell, 1959 [1924]:33,44). In the hands of the positivists, the analytic-synthetic distinction became the basis of the distinction between a theoretical language and an observational language. Although not all knowledge was empirical, theoretical statements about the external world were to be justified through a modified process of induction, _viz_, verification. Induction, in this rehabilitated form, still led to certain and indubitable knowledge. However, in the shift from verification to confirmation, the outcomes were potential falsifiers; induction led not to certain knowledge, but to near-certain knowledge. Consequently, the quest for an inductive logic in terms of verification was transformed into an exploration of probability. With these subtle changes in emphasis, some individuals preferred to call themselves logical empiricists rather than logical positivists (Feigl, 1949; Ayer, 1946:29, 137).

The "problem of probability" for the positivist was one in which "the alternative 'true-false' of classical logic was replaced by a continuous scale of probability values," (Reichenbach, 1949 [1933]:305). Probability replaced deterministic causal statements requiring true-false predictions with near certain probability statements (R. von Mises, 1951:163). For example, both confirmation (or testing) and verification could be stated in probabilistic terminology. Confirmation required that the evidence confirm the hypothesis only to a certain degree; or, $P(h,e) = p$, where P ranged between 0 and 1, e was the evidence, and h the hypothesis. In contrast, verification required that the degree of probability, p, be zero or one.

To summarize, the rejection of metaphysical statements by logical positivists was tied to their optimism concerning verification. They maintained that verified theoretical statements were the true source of genuine knowledge claims. Later, because of the critique falsification posed, verification was modified to confirmation. In turn, falsification led to a shift in the conception of empirical knowledge. Confirmation led to probability statements which were near-certain rather than certain claims to knowledge.

Reductionism, Physicalism, and Psychology

Having outlined the relationship between the rejection of metaphysics and verification, two of the five important tenets of logical positivism are now presented. To a large extent, the remaining three characteristics are consequences of the anti-metaphysical outlook of positivism and its empirical criterion of meaning. All three issues can be considered simultaneously by considering one specific issue; the problem which mind poses for scientific psychology. More technically, this issue is called the mind-body problem. Mind seems to be something immaterial, while the body quite obviously seems to be something material. The mind-body problem is whether minds do exist; and if they do, how do they relate to the body and other material entities?[8]

Ever since Descartes, the mind-body problem has implied that man has two different aspects, a mental aspect and a physical aspect (see chapter eight). But the positivist has defined away the mental half of the mind-body problem. Scientifically meaningful observation statements were fashioned to confirm theories only about externally observed behavior. The gross responses of individuals revealed through observable actions were precisely the type of empirical evidence which the positivist desired. However, the existence of an immaterial mind, posed major difficulties. Since the human mind was unobservable, no simple observation report possibly could refer directly to mind. Using the criteria of scientific meaning developed above, positivists claimed that mind could be nothing other than a metaphysical question beyond the purview of science.[9] Carnap (1949 [1938]:411) maintained that, "The traditional distinction between bodily (or material) and mental (or psychical) processes had its origin in the old magical and later metaphysical mind-body dualism."[10]

Since mind was considered to be a metaphysical question beyond the purview of scientific inquiry, a program for a scientific psychology was conceived. For psychology to be scientific, it had to deal with physical processes, not with unobservable mental processes. Carnap (1949 [1938]:411) recognized that behaviorism, as found in psychology, was an attempt to make psychology scientific like physics. Behaviorism made consciousness and mentalistic concepts irrelevant to scientific psychology (R. I. Watson, 1971:417-418). Statements about a mentalistic concept like pain could "be retranslated without loss of content into propositions which no longer involve the term 'pain' but only physical concepts," (Hempel, 1949 [1935]:377-378). Kraft (1953:105) summarized the view of the positivists: "The scientific content of statements about the mental can consist in nothing else but statements about bodily states." Perhaps the most extreme and radical view was that psychology, to be scientific, had to be reduced to physics. Carl Hempel (1949 [1935] :378) stated the position unambiguously and succinctly:

> *All psychological statements which are meaningful, that is to say, which are in principle verifiable, are translatable into propositions which do not involve psychological concepts but only the concepts of physics. The propositions of psychology are consequently physicalistic propositions. Psychology is an integral part of physics*." [italics in original]

Hempel's statement was the best illustration of the three tenets of logical positivism under consideration: an anti-psychological bias (mentalistic), reductionism, and physicalism. Making psychology scientific by assimilating it to physics effectively integrated all three tenets into one comprehensive position. But the reduction of psychology to physics was just the beginning. If psychology could be assimilated to physics, so could other disciplines. This program for all the sciences was called the unity of science thesis.

In its strongest form, the unity of science thesis called for the reduction of all sciences to physics.[11] In other words, the language and laws of the biological and social sciences potentially could be reformulated into the language and laws of physics (Feigl, 1953 [1939]: 382-384; 1969:21). Pragmatically, positivists recognized that autonomous theoretical domains for the particular sciences contributed to the success of those sciences. Therefore, the unity of science was to proceed initially by unifying the languages (observational rather than theoretical) of the separate sciences first (Kraft, 1953:162). The unity of scientific laws required the further development of science.

In either of its forms, the unity of science thesis was <u>not</u> to be construed as an ontological thesis (Carnap, 1949 [1938]:413). The unity of science was held to be a logical proposition concerning the various statements. In other words, the theoretical pluralism exhibited by the "laws" of the various sciences was interpreted to be a result of the primitive stage of science rather than a consequence of structural relations in the real world. Theoretical pluralism was not considered to be suggestive of the ultimate nature of reality. This meant that, for the positivist, ideas about reality were not related to ideas about knowledge; ontology and epistemology were completely independent and separable domains of inquiry with only epistemology being scientific.

In summary, one of the most important manifestations of either verification or confirmation as concepts of scientific meaning and the anti-metaphysical stance of logical positivism, concerned psychology as the scientific study of mind. For psychology to be scientific, the issue of mind had to be abandoned. In the extreme, psychology was to be no different than physics. In other words, a positivist who maintained that mental phenomena were scientifically important was guilty of a contradiction in terms.

HOW POSITIVE IS POSITIVE ECONOMICS?

If Milton Friedman's "Methodology of Positive Economics" is taken as the definitive statement of positive economics, then it is apparent that positive economics and logical positivism are, in many respects, quite different. The five characteristics of logical positivism are hardly evident if one peruses Friedman's (1971 [1953]) essay. By way of contrast, in positive economics, the major points of concern are: the normative-positive distinction, the nature of economic theories, and the realism of assumptions. In the following paragraphs, the major points of Friedmanian positive economics are presented; then Nagel's criticisms of Friedman's arguments are discussed; finally, we briefly assess Friedman's research. This last task is pursued to see if Friedman's research manifests those things which Nagel criticizes in Friedman's methodology article.

Friedman conceives of economics as having two major branches, positive economics and normative economics. This distinction has a long history in the development of economic thought (Hutchison, 1964: 23-50). Before Friedman, one of the best statements of this distinction is found in J. N. Keynes' Scope and Method of Political Economy (1917: 34-35):

> As the terms are here used, a positive science may be defined as a body of systematized knowledge concerning what is; a normative or regulative science as a body of systematized knowledge relating to criteria of what ought to be, and concerned therefore with the ideal as distinguished from the actual; an art as a system of rules for the attainment of a given end. The object of a positive science is the establishment of uniformities, of a normative science the determination of ideals, of an art the formulation of precepts.

Following Friedman and Keynes, positive economic science is the domain of objective economic science, while normative economics concerns ethical opinions related to, but not confined to, the conclusions of positive economic science. Policy judgments relate to normative economics. Predictions about the consequences of any change in economic circumstances relate to positive economics. Presumably, it is positive economics which most concerns Friedman. Two topics of significance for positive economics are the nature of theories and the realism of the assumptions on which the theories are based.

In Friedman's view, a major aim of positive economic science is the development of theories. A theory is composed of two elements (Friedman, 1971 [1953]:26). One is a deductive, linguistic component; the other is an inductive, generalizing or abstracting component. When viewed as a language, theories are logical filing devices and must be consistent and coherent. When viewed as inductive generalizations, theories are abstractions based on prior knowledge of the data. Theories and hypotheses must arise out of the data "to assure that a hypothesis explains what it set out to explain," (Friedman, 1971 [1953]: 29). Furthermore, the same data potentially may be used as a basis for an inductive generalization and as a test of this generalization:

"The facts that serve as a test of the implications of a hypothesis might equally well have been among the raw material used to construct it, and conversely," (Friedman, 1971 [1953]:30).[12]

Friedman then proceeds to develop one of his most controversial positions. He maintains that really significant theories are unrealistic because of the view of abstract, inductive generalizations. Such abstract generalizations might even seem descriptively false. Friedman (1971 [1953]:30) maintains that:

> Truly important and significant hypotheses will be found to have 'assumptions' that are wildly inaccurate descriptive representations of reality, and in general, the more significant the theory, the more unrealistic the assumptions. . . To be important, therefore, a hypothesis must be descriptively false in its assumptions.

What Friedman wishes to refute is the widely held notion that the validity of a theory can be determined by the realism of its assumptions. He (1971 [1953]:33) distinguishes between the specification of assumptions as part of the analytical aspect of a theory and the actual empirical determination of the circumstances for which a theory is valid. The experimental circumstances under which a theory may result in valid predictions cannot be analytically specified as part of the theory.

What Friedman is really after with the preceding considerations is a defense of economic rationality as a competitive, maximizing process. Since most transactors do not consciously maximize, one of the most fundamental assumptions of economic theory seems blatantly unrealistic. Friedman is suggesting that economic theory should not be considered unrealistic, just because the assumption of rationality is intuitively unrealistic. He defends this contention by considering (in his view) a similar hypothesis. The alternative hypothesis is that leaves behave "as if" they seek to maximize the amount of sunlight they receive (Friedman, 1971 [1953]:33-34). This hypothesis is acceptable even though we usually think of leaves as being unconscious, having no internal sense of deliberation. Scientists do not reject this maximizing theory in the case of leaves merely because it seems unrealistic. Similarly, prediction or the failure to falsify becomes the only relevant concern for testing the realism of economic theories based on maximization in positive economics.

This concern for not interpreting rationality as an attribution of the internal state of mind of the transactor may be the most positivistic aspect of positive economics. Logical positivism as a philosophy of science is characterized by an aversion to mentalistic concepts; they are pseudo-scientific issues. By maintaining that rationality should not be interpreted realistically and mentalistically, Friedman is very close to the position of the logical positivists. For Friedman, rationality is a counterfactual behavioral assumption which is of great instrumental value. It greatly reduces the concepts needed to predict the behavior of leaves, economic transactors, or even billiard players. Again, in Friedman's (1971 [1953]:34) words:

> It seems not at all unreasonable that excellent predictions would be yielded by the hypothesis that the billiard player made his shots as if he knew the complicated mathematical formulas that would give the optimum directions of travel. . . Our confidence in this hypothesis is not based on the belief that billiard players, even expert ones, can or do go through the process described.

However, it is also clear, from the perspective of logical positivism, that genuine errors appear in Friedman's argument (Nagel, 1971 [1963]:48-49). These errors show a basic misunderstanding of the analytic-synthetic distinction. Failure to understand the fundamental nature of the analytic-synthetic distinction is manifest in Friedman's discussion of theories and realism.

For the logical positivist, a theory is a set of analytical statements which specify relationships between unobservable theoretical terms like "vacuum"; "gene"; and "elasticity of demand at a point," (Nagel, 1971 [1963]:49). Theories are not "empirical generalizations. . . that are simple extrapolations from observed statistical regularities," (Nagel, 1971 [1963]:49).[13] What Nagel is rejecting is the view of earlier empiricists like J. S. Mill, that all laws, even the most abstract laws of mathematics and logic, are inductive generalizations. Following Whitehead and Russell, logical positivists hold that theories are logical statements with no empirical content. However, as presented above, inductive generalization constitutes perhaps the most significant half of Friedman's conception of a theory. A logical positivist rejects this inductive conception of scientific theories. A much sharper separation of theory and fact is maintained. Consequently, from the viewpoint of logical positivism, Friedman's analysis of theories seems antiquated, invalid, and ought to be abandoned.

In the logical positivist's view, a scientific endeavor gains empirical content through a process of correspondence. It is the correspondence rule which effectively maintains the analytic-synthetic distinction in routine scientific research. Correspondence rules coordinate "theoretical terms. . . with observable traits of things;" furthermore, the process of correspondence does not do away with unobservable theoretical terms (Nagel, 1971 [1963]:50). This notion of correspondence helps to resolve the issue of realism. Since Friedman permits theories to be observationally contaminated with inductive generalizations, he has no way to address the concern for realism in theories other than rejecting the concern. An empirical generalization seemingly would have a strong degree of realism, except for those events and circumstances which are lost by abstraction.

For the logical positivist, theories gain empirical content and an element of realism through corresponding observational terms. A theory is somewhat realistic if it leads to successful predictions. But the realism of a theory is derivative and depends on the empirical validity of the theory. In other words, the validity of a theory is transmitted by a correspondence relation from actual observations back to theories. This relationship is construed logically and is modified to account for falsification.[14] Proof can never be transmitted from a verifying observation to the theory. Only falsification can be inferred from disconfirming observations.

Many logical positivists prefer, like Friedman, to confine their concern for realism to prediction and deny that theories have any ontological implications. In particular, Carnap (1949 [1938]:413) derives an ontological interpretation for scientific theories. However, most logical positivists might disagree with Friedman's emphasis on prediction. Friedman's emphasis on prediction is in terms of usefulness. Prediction becomes a criterion of whether a theory works; it is an instrumental notion rather than a concern for realism or genuine knowledge (Friedman, 1971 [1953]:30). Nagel questions Friedman on this point and clearly rejects an instrumental resolution of the issue of realism. Nagel (1971 [1963]:54) asks:

> Is he [Friedman] defending the legitimacy of unrealistic theoretical assumptions because he thinks theories are at best only useful instruments, valuable for predicting observable events but not to be viewed as genuine statements

> whose truth or falsity may be significantly investigated? But if this is the way he conceives theories. . . the distinction between realistic and unrealistic theoretical assumptions is at best irrelevant, and no defense of theories lacking in realism is needed.

Although Nagel basically agrees with the point of view expressed in Friedman's (1971 [1953]) methodology article, one might question the influence of Friedman's view of positive economic science on his economic research. Nagel essentially agrees with Friedman's positivistic attitude, but not with his arguments for positive economics. If Friedman follows his own methodological arguments as prescriptions for the conduct of economic inquiry, then Nagel's criticism also applies to his research.[15] In Friedman's monetary research, his methodological problems are apparent in at least two important cases: his empirical work is often inconclusive and presented without an explicit theoretical framework; at least one of his most important definitions, money, violates the analytic-synthetic distinction and the logical positivist's notion of correspondence.

For example, in their Monetary Statistics, Friedman and Schwartz (1970) emphasize the inductive conception of scientific theories which also seems to be implicit in the companion study published seven years earlier, A Monetary History of the United States (1963). Friedman and Schwartz (1970:91) state, "Economic theory accepted at any time is in part a systematic summary of the empirical generalizations that have been arrived at by students of economic phenomena." Since, in this view, the distinction between theory and fact does not really exist, we should expect that other economists would criticize the research of Friedman and Schwartz on both theoretical and empirical grounds.

Theoretically, neither of the Friedman and Schwartz monographs has an explicit analytical framework. In an attempt to remedy this deficiency, Friedman (1970:n. 1,193) published another version of the quantity theory of money and noted that "several reviewers. . . criticized us for not making the theoretical framework in that [1963] book explicit." Empirically, Friedman seems to have a research style that includes a long evidential lag. Johnson (1965:388) complains that this style of research, which presents conclusions based on incompletely revealed statistical studies; "imposes a serious handicap on the reader and reviewer." Johnson (1965:396) also criticizes Friedman for adopting a money demand function "that lacks empirical validation."[16]

But the preceding account of the theoretical and empirical deficiencies of research guided by Friedman extends to a more fundamental issue. The confusion of theory and fact is also evident in one of the most significant conceptual issues in monetary analysis--the definition of money. In the Friedman and Schwartz monographs, there seems to be no distinction between the theoretical concept of money and its empirical counterpart. For their purposes, "the most useful definition of money" is "the sum of currency outside banks plus deposits of commercial banks--demand and time--adjusted to exclude interbank deposits," (Friedman and Schwartz, 1970:1-2). This definition of money is an empirical quantification of money. It is not a theoretical conception, nor does it define a correspondence relation between the theoretical conception of money and its empirical counterpart. Friedman and Schwartz (1970:91) even seem to suggest that a "specification of the empirical counterparts" of theoretical terms like money is part of economic theory. What this strategy involves is a retreat to instrumentalism which Nagel rejects. "We conclude that the definition of money is to be sought not on grounds of principle but on grounds of usefulness in organizing our knowledge of economic relationships," (Friedman and Schwartz, 1970:137).

In summary, one should not be confused by Friedman's ambiguous development of positive economics. He can be criticized for incorporating serious errors into his conception of positive economics. These errors are carried into his research, his monetary research in particular. The source of his errors is in not understanding the analytic-synthetic distinction and the implication of this distinction for the realism of scientific theories. Contrary to Nagel's sympathetic treatment of Friedman's views on methodology, Friedman's defective methodology leads to defective scientific theories and defective scientific research.[17]

SUMMARY

The task of this chapter was to begin our discussion of economics and philosophy of science on terrain at least partly familiar to most economists. Positive economics and logical positivism were compared and contrasted. Specifically the following arguments were put forth: (1) Friedmanian positive economics was the dominant conception of economic science, (2) logical positivism was an empirical view of science which was extremely anti-mentalistic and anti-metaphysical; and (3) Friedmanian positive economics, containing serious errors, was not very positivistic in the sense of logical positivism. The one major exception to the last argument concerned rationality. Friedman suggested that economic rationality should not be interpreted to mean economic transactors consciously maximize. Rather, rationality was interpreted as a behavioral assumption that did not concern the conscious state of transactors. This element of realism was taken to be irrelevant to economic theory. Following Nagel, this meant that neoclassical theory abstracted from the internal state of mind of the economic transactor.

Given our concern with rational expectations, one must wonder why contemporary positive economists are concerned with expectations. If internal intent, even maximizing intent, is declared by Friedman to be irrelevant to economic analysis, then expectations are also irrelevant. At least with reference to the methodology of positive economics, the following conclusion can be reached: When economists analyze phenomena supposedly affected by expectations, they violate the methodology of positive economics, if they believe their analysis treats expectations like those experienced by real human beings. This means that neoclassical theory based on the methodology of positive economics is an ideal theory which assumes transactors do not change their minds. We must wonder how many economists are being misled into believing that expectations actually are addressed in contemporary economics, rational expectations in particular.

NOTES

[1]We must be careful how we interpret the role of texts in economics. Following Kuhn (1979a), we believe textbooks are important in disseminating and integrating the ideas, theories, and evidence of any science. However, again like Kuhn, we believe textbooks are misleading. Often they lack a significant historical perspective and obscure the intellectual diversity which exists among active researchers. Nevertheless, positive economics seemingly dominates textbook economics.

Those texts which state a view of economics which can be recognized as positive economics are: Gwartney (1976:10-11), Alchian and Allen (1969:11), Weiss (1975:13), Hirshleifer (1976:11), Becker (1971), and Ferguson and Gould (1975:3).

[2]Those texts which do not explicitly embrace any specific methodology are mostly Keynesian introductory texts or macroeconomics texts like: Samuelson (1980), MacConnell (1969), Spencer (1974), Crouch (1972), Dernberg and McDougall (1976), Shapiro (1974), Branson and Litvack (1976), Miller and Upton (1974), and Smith (1970). The well-known, advanced micro theory text, Henderson and Quandt (1971) contains no discussion of a view of economic science, only theories and the relation of microeconomics to macroeconomics. Two other texts, Fusfeld (1972) and Heilbroner and Thurow (1975) evoke a sense of unresolved tension in their view of economic science by recognizing more than one perspective on economic science.

[3]Friedman (1971 [1953]:25) says, "I venture the judgment, however, that. . . differences about economic policy among disinterested citizens derive predominantly from different predictions about the consequences of taking action--differences that can in principle be eliminated by the progress of positive economics."

[4]Blaug seems to give an instrumental interpretation to both Popper and Friedman. Blaug (1976:149) states, "The idea that unrealistic 'assumptions' are nothing to worry about provided the theory deduced from them culminates in falsifiable predictions carried convinction to economists long inclined by habit and tradition to take a purely instrumentalist view of their subject." Nagel (1971 [1963]:54, quoted below pp. 38-39) rejects the instrumental interpretation of Friedman. A realist, particularly a structural realist, would reject instrumentalism. Instrumentalism does not retain the objectivity of science. Usefulness of a theory is a criterion subject to much variance from one investigator to another.

Popper (1963:111) clearly rejects instrumentalism: "My reply to instrumentalism consists in showing that there are profound differences between 'pure' theories and technological computation rules, and that instrumentalism can give a perfect description of these rules but is quite unable to account for the difference between them and the theories. Thus instrumentalism collapses." Concerning Friedman, Jones (1977:357-358) recognizes that the instrumental interpretation of positive economics "is a controversion of positivist principles."

Recently, in a major article, Lawrence Boland (1979) elaborates a defense of positive economics in terms of instrumentalism. This supports our claim that positive economics is not very positivistic, except with respect to the issue of mind (which is reduced to external observable evidence).

[5]Russell (1959 [1924[:31-32) states, "At the age of eighteen I read Mill's Logic, but was profoundly dissatisfied with his reasons for accepting arithmetic and geometry. I had not read Hume, but it seemed to me that pure empiricism. . . must lead to scepticism rather than to Mill's support of received scientific doctrines."

[6]For example, take the theoretical definition of money as a medium of exchange. The correspondence rule then would be stated as follows: An object (commodity or piece of paper) is money, if and only if it is not consumed, it is involved in most market transactions (directly or indirectly), and it balances budgets over a period of time. Thus, the monetary aggregates (M_1, M_2,...,M_7) may or may not be empirical or observational correspondents of this definition of money.

[7]Falsification is a concept originally developed by Popper (1959: 40-42) and pursued in much greater detail by Lakatos (1970). Falsification is considered in more detail in the next two chapters.

[8]Weimer (1976:7) states, "The prototypic statement of the mind-body problem since the time of Descartes is; 'what are mind and body, and how do they relate to one another?'"

[9]Feigl (1967:3) states, "Tough-minded scientists tend to relegate the mind-body problem to the limbo of speculative metaphysics. . . the puzzle is left to the philosophers to worry about, or is bluntly declared a pseudo problem not worth pondering by anybody."

[10]Schlick (1949 [1935]:393), originally the leading figure of the Vienna Circle, says, "Recent philosophy of science has not been lacking in attempts to free the Cartesian problem of the relation between mind and body from its metaphysical description."

In economics, the issue of mind arises in Little's (1957:54-56) discussion of interpersonal comparisons of welfare. Human welfare like the human mind is an unobservable aspect of human experience. In this context, Little considers whether one needs to recognize that other individuals have minds. This is known as the "other mind issue." "It is clear that if one accepts behavior as evidence for other minds, then one must admit that one can compare other minds on the basis of such evidence," (Little, 1957:55). Behavior is the problem not the solution. The tropism of leaves turning toward sunlight is behavior. But we do not infer leaves have minds or are conscious (Friedman, 1971 [1953]:33). For positivists, other minds is perhaps the most "non-sensical" issue they encountered. The belief in other minds is a postulate which can never be shown to be true (C. I. Lewis, 1949 [1941], 392).

[11]One of the early aims of Otto Neurath (1955 [1938]), a leader of logical positivism, was to embody the unity of science in a multi-volume encyclopedia. The work has never been completed, although two volumes have been published as the Foundations of the Unity of Science. Ironically the second volume included the original publication of Kuhn's (1970) non-positivistic account of the physical sciences. Kuhn's work, more than any other, was responsible for the decline of positivism.

[12]Friedman's reasoning in this case seems to be circular. Notice what implications the footnoted statement in the text has for Friedman's monetary research. His monetary research may be circular in nature. Mason (1976:534) leaves no doubts in this regard: "Thus, Friedman's empirical definition of money appears to be identically the same thing as his restated quantity theory of money--that aggregate of financial assets for which the demand function is relatively stable. This makes his reasoning circular, reducing his definition and theory of money to a tautology."

[13]Nagel (1971 [1963]:54) even restates the position much more strongly: "A theory cannot be viewed, as he [Freidman] repeatedly suggests it can, as a 'simple summary' of some vaguely delimited set of empirical generalizations with distinctly specified ranges of application."

[14]The logical aspects of falsification are discussed in the following chapter, pp. 59-63.

[15]The conundra one encounters in attempting to understand the work of Friedman and his colleagues are explored in great detail by Mason (1976; 1980). No doubt the points raised by Mason (1976) would be accepted by a logical positivist. The sharp separation of theory and fact resulting from their understanding of the importance of the analytic-synthetic distinction may be the most lasting contribution of logical positivism to science. Almost every economist encounters the analytic-synthetic distinction in the hypothetico-deductive model of economic research in which models are carefully developed and then tested. This type of research, which seems to dominate current journals, helps other economists to avoid some of the mistakes Friedman makes in his monetary research in confusing theory and fact.

[16]After reading Friedman and Schwartz's (1963:809) Monetary History, one of the NBER directors, A. J. Hettinger, offered the following comment: "I eagerly await the more pleasant reading afforded by a published volume, where tables appear in context and charts, by their presence, remove that need for faith, defined by St. Paul as 'the substance of things hoped for, the evidence of things not seen.'"

[17]Perhaps Friedman has his own theories (the quantity theory and perfect competition) in mind when he states that: "To be important, a hypothesis must be descriptively false." See page 35 of the text for full quote and reference.

Chapter 3

RECENT PHILOSOPHY OF SCIENCE AND THE KEYNESIAN REVOLUTION

In this chapter, we continue to explore recent philosophy of science as background for a more contemporary and fuller consideration in the next chapter. Having discussed logical positivism in some detail, we now discuss more recent views of science. Recent philosophy of science is dominated by one central concern--the nature and significance of scientific revolutions and their importance for an adequate philosophy of science. Fortunately, much of what follows may be familiar to many economists, because they question whether the most recent revolution in economics, the Keynesian Revolution, is a genuine scientific revolution. Consequently, one finds many articles and books in economics which reference the philosophy of science literature on scientific revolutions.

In this chapter, we aim to do more than summarize the "revolutions literature" in economics and philosophy of science. We argue that most economists do not fully appreciate the importance of scientific revolutions for a view of science. While philosophers use scientific revolutions as a basis for questioning logical positivism, no economist, to our knowledge, takes revolutions in economic science as an argument against positive economics. Rather, economists seem preoccupied with the self-serving task of enhancing the scientific status of economics by declaring the Keynesian Revolution to be a scientific revolution. This failure to direct revolutions in economics against positive economic science may not seem very significant, since positive economics appears not to be very positivistic. However, this failure, we argue, becomes increasingly significant for our understanding of contemporary macroeconomics (rational expectations in particular) and for our understanding of Keynes and the Keynesian Revolution.

KUHN'S SCIENTIFIC REVOLUTIONS AND THE KEYNESIAN REVOLUTION

To make our case, that economists largely seem to have missed the most basic implication of scientific revolutions for a conception of science, we need to follow a very simple stragegy. We simply review the relevant literature in philosophy of science; then we review the impact of this literature on economics. In this section, we consider the work of Thomas Kuhn; in the next section, we consider the work of Karl Popper and Imre Lakatos. Popper, Kuhn, and Lakatos are the principal figures in philosophy of science in the late 1950's, 1960's, and early 1970's. They direct philosophy of science away from a static, logical reconstruction of knowledge, toward a more dynamic concern with the growth of scientific knowledge.

Initially, other than a few economists who had a special interest in philosophy of science, most economists were unaware of the problem shift in philosophy of science from logical reconstruction to the growth of knowledge. It was Kuhn's monograph, The Structure of Scientific Revolutions (1970a [1962, first edition]) which broadly aroused the attention of economists. Ironically, Kuhn's essay first appeared as a monograph in the International Encyclopedia of Unified Science (IUS). The IUS was a project of the Vienna Circle which resulted from the First Congress for Scientific Philosophy held in Paris in 1935 (Popper, 1963: 269). The purpose of the IUS was to demonstrate that all the sciences used common theoretical and observational languages. However, World War II intervened dispersing the Vienna Circle and delaying development of the IUS. Only one unit of the proposed multi-volume encyclopedia has been completed, The Foundations of the Unity of Science (Neurath, Carnap, and Morris, 1969). It contained nineteen essays including Kuhn's.

In his Structure of Scientific Revolutions, Kuhn is concerned with the historical authenticity of logical positivism. When logical positivism is applied to the history of science rather than to a logical reconstruction of the experimental situation, positivism becomes an insufficient methodology to explain the historical development of science. Kuhn constructs a criticism of logical positivism by offering an alternative view of the historical growth of science. Kuhn aims to explain past scientific events which logical positivism cannot explain. In particular, Kuhn focuses on revolutionary periods of the growth of science. To focus on revolutionary science, Kuhn also develops a view of non-revolutionary (normal) science and then compares the two types of scientific activity.

Suggesting two fundamental types of scientific activity, Kuhn proposes a theory of science aimed to capture historical and social-psychological dimensions of the development of science. Science is exemplified by "normal science" and by "revolutionary science," (Kuhn, 1970a:1-9). Normal science is the type of activity which characterizes the bulk of scientific research, while revolutionary science is the type of activity during which the imagination of the scientific community is transformed. Normal science exhibits a commonly held paridigm by the scientific community, while a scientific revolution occurs when a new paradigm replaces its predecessor.

Normal science has several characteristics. One of the most striking features of normal science is puzzle-solving. The task of the scientist is to apply his tools to new phenomena within the accepted scope and limit of the discipline. The scientist is not to begin each new investigation with a search for first principles. Another feature of normal science concerns the specialization of theories, research, and communication. A mature scientific discipline is characterized by a professional literature which is largely inaccessible to the educated layman. Brief reports and articles become the major outlet of communication between scientists. Communication with students and educated laymen occurs mostly through textbooks. Textbooks are ahistorical repositories of theories and crucial examples which illustrate the theories. Summarizing, Kuhn (1970a:42) indicates the nature of normal science:

> The existence of this strong network of commitments--conceptual, theoretical, instrumental, and methodological--is a principal source of the metaphor that relates normal science to puzzle solving. Because it provides rules that tell the practitioner of a mature specialty what both the world and his science are like, he can concentrate with assurance upon the esoteric problems that these rules and existing knowledge define for him. What then personally challenges him is how to bring the residual puzzle to a solution.

Revolutionary science inherently arises from the nature of normal science. Kuhn (1970a:52) maintains that normal science does not aim at novelties of fact or theory. However, unexpected and novel phenomena are uncovered repeatedly in scientific inquiry. Novel phenommena may surprisingly be discovered during routine procedures of investigation. Such phenomena, which continually resist explanation according to the prevailing theoretical framework and testing procedures, evoke a sense of anomaly. Anomalous factual novelties are closely related to the development of novel theories. Anomalies that can be systematically and independently reproduced tend to cumulate. The accumulation of anomalies may evoke a sense of crisis over the accepted paradigm and a new one may begin to appear (Kuhn, 1970a: 84-85). A successful paradigm change is called a scientific revolution (Kuhn, 1970a:92). Scientific revolutions thus represent an advance (sometimes a very rapid advance) in scientific knowldege, both factually and theoretically.

Kuhn's vision of the scientific enterprise often receives a mixed evaluation upon closer scrutiny. Many practicing scientists accept Kuhn's views, while many philosophers question his views. Philosophers direct their criticism to Kuhnian normal science and the concept of a paradigm. The nature of a paradigm is a crucial issue, since it defines the scope and limit of normal science activity and is essential to the concept of a scientific revolution. In Kuhn's work, the concept of a paradigm seems rather ambigious. The source of the difficulty is the multiple uses Kuhn makes of a paradigm. One sympathetic critic, Margaret Masterman (1970), notes at least twenty-one different uses. She recognizes that not all of these different uses are incompatible with one another. Ultimately, she reduces these different uses to three different concepts of the nature of a paradigm; metaphysical, sociological, and artifact paradigms. A metaphysical paradigm refers to the fundamental conceptual framework within which science is conceived; the sociological paradigm refers to commonly accepted modes of scientific practice; and the artifact paradigm refers to the crucial role of texts and classic works in exemplifying the paradigm.[1]

Although philosophers question and criticize Kuhn's theory of the growth of science, professional scientists largely embrace his view. For the practicing scientist, Kuhnian normal science elucidates aspects of the research community which philosophers tend to ignore. This is no doubt due to the concern of philosophers for reconstructing the end products of scientific research. Of even greater significance is the attention Kuhn gives to revolutionary science. The concept of a scientific revolution is not reconcilable with the logical reconstruction of science by the logical positivist. Thus, Kuhn's concept of science provides an alternative outlook on science. This alternative outlook provides alternative criteria for assessing the scientific character of a given discipline. For those unsympathetic to positivism, a reexamination of one's discipline along Kuhnian lines provides an opportunity to assess one's discipline as being "scientific" without adhering to positivism. To restate the view, logical positivists take only the best scientific research as normative examples of science. In their reconstruction of science, scientific revolutions have no place; neither does the everyday experience of a highly specialized scientist. Thus, on two counts, Kuhn appeals to the practitioners of many sciences who are less than enchanted with the logical positivist's view of science.

Likewise, many economists express enthusiasm for Kuhn's theory of science. Since the Keynesian Revolution is the most recent episode of upheaval to affect the majority of economists, it becomes a focal point of inquiry. If the Keynesian Revolution is a genuine scientific revolution, then the positivistic orientation in economics might be challanged. However, to our knowledge no Kuhnian-type account of the Keynesian Revolution by an economist takes the Keynesian Revolution, per se, as evidence that positive economics is an inadequate conception of economics. Even if positive economics is not very positivistic in the sense of logical positivism, it is still mainly an incrementalist, step-by-step conception of science and economics. Obviously, this is an important oversight by economists. To demonstrate our point, we now turn to a review of the "revolutions literature" relating to Kuhn's views of normal and revolutionary science. For the most part, economists do not go beyond their preoccupation with the scientific status of economics in Kuhnian terms, to even considering positive economics. The following paragraphs capture the central thrust of this literature.

Macro theorists find a Kuhnian view of the Keynesian Revolution when they encounter the major theoretical reassessment of Keynes, Axel Leijonhufvud's On Keynesian Economics and the Economics of Keynes (1968). Leijonhufvud and others suggest that Keynes had a revolutionary impact on modern economics, both theoretically and doctrinally. However, the contribution of Keynes seems to be systematically obscured by the theoretical propensities of the profession best illustrated by the work of Patinkin (1965). By associating Keynes with a genuine scientific revolution, Leijonhufvud no doubt enhances his case that some significant aspects of Keynes' thoughts are overlooked; whatever is scientific about the Keynesian Revolution is now lost and must be rediscovered. With respect to Kuhn and the Keynesian Revolution, Leijonhufvud (1968:389, n.1) takes this position:

> The thesis regarding the "Structure of Scientific Revolutions," which Kuhn develops on the basis of historical studies of development in the natural sciences, generally fit the Keynesian Revolution most admirably.

Later, Leijonhufvud altered this assessment.[2]

Other assessments of the Keynesian Revolution follow. A series of articles and a monograph proceed to interpret the Keynesian Revolution in light of Kuhn. A. W. Coats (1969) cautiously suggests that the Keynesian Revolution is an example of a paradigm shift achieved by a new generation of economists. Coats qualifies his view by maintaining that scientific revolutions are much more difficult to ascertain in economics and the social sciences and by suggesting that the Keynesian paradigm does not break completely with preceding theory. Benjamin Ward (1972) makes Kuhn's view of science, the basis for a critique of the entire profession. However, with respect to the Keynesian Revolution, Ward (1972:38) maintains that "Keynesianism was less than a revolution." The short run monetary phenomena with which Keynes is so concerned are still a problem. These phenomena have yet to be intergrated into the main body of economic theory. Ron Stanfield (1974) assesses the Keynesian Revolution more favorably than Coats or Ward. Stanfield (1974:104) maintains, "the Keynesian Revolution was a change in world view" and thus qualifies as a Kuhnian scientific revolution.[3]

These cautious, but sympathetic interpretations of Kuhn, are not unopposed. Other economists, equally opposed to positivism, reject Kuhnian interpretations of the Keynesian Revolution, in particular, and the history of economics, in general. Martin Bronfenbrenner's (1971) major criticism of Kuhn's thesis is that he does not offer an adequate framework to analyze revolutions in the history of economics. Bronfenbrenner (1971:150) sees three revolutions in economic thought; a laissez faire revolution (A. Smith and Hume), a utility revolution (Marshall and Jevons), and a macroeconomic revolution (Keynes). Bronfenbrenner prefers a crude, Hegelian dialectic to describe the ebb and flow of economic theory and thought. Thus, he rejects both Kuhn and positivistic incrementalism as adequate interpretations of the development of economics. Leonard Kunin and F. S. Weaver (1971) also reject the application of Kuhn's concepts to revolutions in economics. They oppose uncritical and mechanical attempts to explain economics using the Kuhnian framework. In their view, not only is the paradigm concept ambigious; but also a concept of science based on a natural science is inappropriate for the social sciences. The social scientist invariably faces a changing domain of phenomena which is inherently more complex than the natural world. Jorg Baumberger's (1977) attitude toward Kuhn is revealed in the title of his article, "No Kuhnian Revolutions in Economics." Baumberger recognizes Kuhn's role in rejecting a positivistic philosophy of science. However, he rejects Kuhn's thesis maintaining the message of Kuhn is much more specific than the rejection of positivism. Thus, it is the specific application of Kuhn to economics, particularly to Marx, which Baumberger rejects.

In summary, our consideration of Kuhnian normal and revolutionary science and its application by economists to the Keynesian Revolution gives us no reason to alter our assessment of the situation;

assessments of the Keynesian Revolution in Kuhnian terms never seem to penetrate to positive economics itself. Yet, Kuhn's account of science and scientific revolutions is a non-positivistic view of science. Scientific revolutions transcend the experimental situation which is so much the concern of logical positivism and Friedmanian positive economics.

LAKATOSIAN RESEARCH PROGRAMS AND THE KEYNESIAN REVOLUTION

Dissatisfaction with Kuhnian paradigms among professional philosophers and interested lay philosophers, like those economists concerned with the Keynesian Revolution, eventually results in another major approach to the history and philosophy of science. The late Imre Lakatos' concept of a research program as a series of interrelated theories is a remedy to Kuhn's theory of science. Lakatos is also known for his close association with Karl Popper at the London School of Economics. Lakatos' theory of scientific research programs is both an extension and a correction of Popper's views on science and the growth of scientific knowledge. Popper is one of the earliest and most effective critics of Vienna-School positivism; positivists themselves recognize this.[4] Both Popper and Lakatos are opposed to positivism. The concept of falsification which Popper originates as a criticism of logical positivism is crucial to understanding Lakatosian scientific research programs. Again, we shall suggest that economists have not taken the ideas of Popper and Lakatos as arugments against positive economics.

Falsification as a criterion of scientific meaning is proposed by Popper (1959) in his monograph, The Logic of Scientific Discovery (LSD). LSD is a translation of the original German work, published in 1935 (see translators note, Popper, 1959:6). Falsification is a critique of the criterion of scientific meaning advanced earlier by the logical positivists--verification. Verification is the positivists' initial rehabilitation of induction. By verification, positivists mean that theoretical statements can be justified empirically through an observation report confirming the theoretical statement. Thus, a verified theoretical statement is a legitimate knowledge claim because it is supported by proof. The difficulty with verification as a criterion of scientific meaning is that it is a logically invalid inference (Popper 1959:40). Universal theoretical statements can never be proven on the basis of a finite series of observations. Only one falsifying instance among future observations may be necessary to question the theory. Thus, induction, even in the more sophisticated form of verification, cannot result in logically jusitified knowledge claims (Popper 1959:40).

Popper (1959:41) points to a logical asymmetry which permits him to suggest a deductive principle of the scientific status of theories. Although theories cannot be logically justified by confirming instances, they can be logically refuted.[5] Popper calls his deductive criterion of scientific status, falsifiability. An empirical theory is falsifiable, if it rules out one type of event. A theory is considered to be falsified, if occurences contradicting the hypothesis can be reproduced (Popper, 1959:86,90).

Although the Popperian reformulation of a criterion of scientific status in terms of falsification rather than verification may seem rather straightforward, it has far reaching consequences. Logical positivism is concerned with establishing the proof of knowledge claims through a process of verification, later modified to confirmation in light of the falsification criterion. Confirmation is specified in terms of probability.[6] This task of confirming knowledge is to be accomplished by logically reconstructing the end products of scientific research. A theoretical statement is considered to be empirically valid, if the process of confirmation can be reconstructed logically. But in the Popperian view of science, the problem of knowledge is the growth of knowledge rather than its logical reconstruction. Knowledge itself is no longer considered to be confirmations based on certain or near-certain empirical justifications. Popper contends, in his <u>Conjectures and Refutations</u> (1963), that human knowledge is purely conjectural. The guarantees of justified or proven knowledge claims are not found in science. Furthermore, scientific knowledge progresses by refutation rather than through verification or confirmation.[7] In the preface, Popper (1963:vii) says:

> The way in which knowledge progresses, and especially our scientific knowledge, is by unjustified (and unjustiable) anticipations, by guesses, by tentative solutions to our problems, by <u>conjectures</u>. These conjectures are controlled by criticism; that is, by attempted <u>refutations</u>, which include severely critical tests. They may survive these tests; but they can never be positively justified: they can neither be established as certainly true nor even as "probable" (in the sense of the probability calculus).

Various interpreters of Popper recognize that Popper's view of science as a series of conjectures and refutations is dominated by his concern with revolutionary science (Kuhn, 1970b:6; Williams, 1970:49; Lakatos, 1970:92).[8] Popper seems to take Einstein and the revolution in physics as a prototypic example of science. Popper (1963:34) even remarks that the first evidence supporting Einstein's theory of gravitation has had a lasting impact on his intellectual development. A colleague of Popper's, John Watkins (1970:27), defends this preoccupation with revolutionary science. Philosophy of science is to be concerned with "science at its best. . . rather than with hack science." Thus, it is not surprising that Popper (1970:52-53) clashes with Kuhn concerning normal science, though both are concerned with the growth of knowledge:

> In my view the 'normal scientist', as Kuhn describes him, is a person one ought to be sorry for. . . The 'normal' scientist, as described by Kuhn, has been badly taught. He has been taught in a dogmatic spirit: he is a victim of indoctrination. He has learned a technique which can be applied without asking for the reason why. . . As a consequence, he has become what may be called an applied scientist in contradiction to what I should call a pure scientist.

To a certain extent, this controversy between Popper and Kuhn concerning normal science elicits attention from both sides of the debate. To the Popperians, Kuhn's emphasis on the social psychology of the scientific community raises the specter of irrationality.[9] To those sympathetic to Kuhn, normal science characterizes a large part of scientific activity and needs to be part of a comprehensive view of science. However, it was one of Popper's colleagues who attempts to remedy both Kuhn and Popper's views on the nature of science. Lakatos fashions a concept of science different from an "irrational" Kuhnian paradigm and different from Popper's revolutions in permanence.[10] Lakatos' (1968-69, 1970) aim is to analyze the growth of science by analyzing "scientific research programs."

Lakatos (1970; 93-133) begins his characterization of science by distinguishing three types of falsification: dogmatic, methodological, and sophisticated falsification. Dogmatic falsification is the most simple-minded way of approaching Popper. The preceding description of Popper's position in LSD is an example. Justifying which theories get thrown away through a process of logical negation is dogmatic falsification. Lakatos maintains that those who interpret Popper dogmatically, misinterpret his views. Dogmatic falsification is a "vulgarized version of Popperian philosophy of science," (Lakatos, 1968-69; 152, n.2).

Methodological falsification is a legitimate representation of Popper's views according to Lakatos. Methodological falsification results when it is realized that dogmatic falsification is too strong. If scientific knowledge is truly conjectural, as Popper maintains, then instances of apparent disproof may abound. This means almost all scientific theories must be rejected, because proof is ever present that theories are false. To remedy this situation, refutation needs to be separated from rejection (Lakatos, 1970:109). Refutation and rejection are separable because both theories and evidence can be in error (Popper, 1959: 86). A theory is not decisively rejected until the quality or robustness of the facts are scrutinized.[11] Popper and Lakatos reject the idea that simple, unproblematic facts are available to test the theory. Rather, one needs to develop a theory or an expectation of what an important fact would be (Lakatos, 1970:98-99). In other words, science is a clash of two types of theories; an explanatory theory explaining the facts and an interpretive theory which determines the facts (Lakatos, 1970:115).[12] A disproving instance of a theory may be directed at the explanatory theory or the interpretative theory. If the problem is not with "the facts" (the interpretive theory), then the explanatory theory must be rejected. Otherwise, the scientist must develop a better empirical basis on which to test his explanatory theory. Thus, a refuting instance of an explanatory theory is not tantamount to its rejection.

For Lakatos, even methodological falsification is yet too strong. Despite the fact that refutation can be separated from rejection, methodological falsification requires a validly refuted theory to be rejected. However, the history of science suggests that often theories are retained despite valid falsifying evidence.[13] Methodological falsification raises the specter of scientific research without scientific theories. But even research guided by false theories may provide a fruitful guide for research since errors can be systematically replicated and haphazard falsifying instances rejected. Lakatos thus argues that a theory should not be rejected unless a better theory is on the horizon which replaces the theory in question. Sophisticated falsification suggests theories are abandoned when better ones are found; such theories have more empirical content because they explain and anticipate more facts (Lakatos 1970: 123). Thus, theories are not appraised in isolation from other explanatory theories. Theories are assessed by comparing them with one another.[14]

Sophisticated falsification is the approach to science which Lakatos develops in detail. He aims to explain scientific growth as the generation of increasingly comprehensive theories in progressive research programs. A <u>research program</u> is a series of interconnected theories. Each successive theory should be more inclusive in terms of empirical content. Lakatos (1970:133-135) further characterizes a research program as having a <u>hard core</u> and a <u>protective belt</u>. The hard core consists of metaphysical assumptions which are irrefutable methodologically (Lakatos, 1968-69:178). Hard-core statements are those presumptions toward which the facts resulting from interpretative theories are never directed. The protective belt consists of touchstore theories, auxiliary hypotheses, initial conditions, and interpretative theories (Lakatos, 1968-69:168-169; 1970:133). When instances of disproof occur, falsification is directed to one of these elements of the protective belt.

Since a research program has a hard core and a protective belt, a research strategy is provided for the scientific community. The hard core forbids the conclusions of empirical research being directed at the hard core (negative heuristic). The attention of researchers is to be directed to those theories which can be refuted according to the currently accepted methodological theory of observation (positive heuristic). If successive theories result in an increase in empirical content, a progressive problem shift occurs. Otherwise, the research program is degenerating (Lakatos, 1970:118; 1978a:112).[15] Thus, falsification, a negative view of assessment, is converted into a positive criterion by confining falsification to progressive theories. A theory for Lakatos is considered to be falsified, if it is replaced by a theory with greater empirical content.

One need not stop with falsification at the level of theories; research programs may be competing. Falsification potentially may be extended to a progressive research program replacing a degenerating program. This is Lakatos' conception of a scientific revolution (1978a [1971]:110-113). However, this causes some problems. A scientific revolution in the broadest sense of Kuhn's paradigm shift is a new set of metaphysical assumptions for science replacing a prior set. However, Lakatos' conception of a scientific revolution requires the metaphysical hard core to remain identical for pre- and post-revolutionary science. Lakatos (1978a:112, n.3) maintains that the rivalry of two competing research programs is a protracted process. Furthermore, he states that the increasing empirical content of a progressive program counts as decreasing empirical content for a degenerating program. Lakatos' (1978a:112, n.3) view is contained in his own words:

> The progress of the programme is a vital factor in the degeneration of its rival. If programme P_1 constantly produces 'novel facts' these, by definition, will be anomalies for the rival program P_2. If P_2 accounts for these novel facts only in an ad hoc way, it is degenerating by definition. Thus the more P_1 progresses, the more difficult it is for P_2 to progress.

The preceding statement implies that two rival programs, P_1 and P_2, have a common hard core of metaphysical assumptions. Otherwise, the increasing empirical content of P_2 is rendered irrelevant to P_1, since the hard core of P_1 may be different than the hard core of P_2. Thus, Lakatos' scientific revolutions are not the same type of revolutions which Kuhn has in mind. The Lakatosian description of scientific revolutions permits more scientific revolutions than what Kuhn permits. Kuhn suggests the big revolutions are scientific revolutions in which a metaphysical paradigm shift occurs. Such shifts could be relatively sudden or take many years.

As they have done with Kuhn's research, economists also direct their attention to the Lakatosian view of science. Primarily this is done by reconsidering the nature of the Keynesian Revolution (rather than positive economics) in light of the Lakatosian conception of scientific research programs. However, the most informative assessments of

Lakatosian research programs are those which simultaneously consider Kuhnian paradigms. Economists and philosophers are aware of the fact that Lakatos' views supposedly rehabilitate Kuhn's views along Popperian lines. The history of economics provides an additional domain of historical detail in which the views of Kuhn and Lakatos can be compared and contrasted. In particular, Mark Blaug (1975, 1976) and Axel Leijonhufvud (1976) reconsider the Keynesian Revolution in order to assess the relative adequacy of Kuhn's account of paradigms with Lakatos' account of research programs.[16]

For Blaug (1975, 1976), the Keynesian Revolution is a genuine revolution on Lakatosian grounds. It replaces a degenerating research program which can not explain the persistence of severe unemployment and it provides a new hard core. Keynes' concern with uncertainty and destabilizing expectations in response to the news contrasts sharply with the classical and neoclassical hard core of rational calculation as maximization (Blaug, 1976:162). Furthermore, the protective belt of the Keynesian approach (touch stone theories, new concepts, auxiliary hypotheses, and initial conditions) contains many new elements. The demand for money <u>per se</u>, the consumption function, the investment function, the multiplier, the various propensities to consume, the marginal efficiency of capital, the emphasis on aggregates, and various institutional rigidities are aspects of the economy which Keynes unites into one, unified analytical construct (Blaug, 1976:162-163).

According to Blaug (1976:164), the concept of a research program also seems to fit events in the profession subsequent to Keynes, like the monetarist counter-revolution and the neo-Keynesian, disequilibrium reinterpretations of Keynes by Clower and Leijonhufvud.[17] Blaug maintains that these reinterpretations discard aspects of the Keynesian metaphysical hard core (uncertainty and destabilizing expectations) such that Keynes is now the special case. Blaug (1976:164-165) concludes: "Keynes' General Theory is now a special case and this is scientific progress in economics, perfectly analogous to the absorption of Newton as a special case in the general theory of relativity."

Three difficulties with Blaug's assessment of Lakatos and the Keynesian Revolution are apparent. First, for Lakatos, a scientific revolution presupposes a common hard core of metaphysical assumptions for pre- and post-revolutionary science. By permitting the metaphysical hard core to change, Blaug seems to misinterpret Lakatos. It is Kuhn rather than Lakatos who takes a scientific revolution as a metaphysical paradigm shift. Second, the abandonment of the hard core of the General Theory, uncertainty and destabilizing expectations, renders the Keynesian Revolution invisible.[18] Blaug interprets monetarists and neo-Keynesian disequilibrium theorists as effectively returning to the rational maximization of the classics and early neoclassics. If this is the case, Keynes becomes less than a special case. As Patinkin (1965:339) implies, his contribution reduces to institutional commentary concerning an "intolerably long period of dynamic adjustment." Contrary to Blaug, this is not analogous to what happened to Newton. Newton becomes a special case after the Einsteinian Revolution. If Keynes is now a special case of a more general neoclassical approach, then it seems his theory would always have been a special case. If Blaug interprets Keynes as being a special case, then there never was a Keynesian Revolution of any substance. The Keynesian Revolution then becomes the first scientific revolution to proceed on the basis of a new (old?) special case.

A third difficulty with Blaug concerns positive economics. This difficulty arises in part from the second one, that recent interpretations of Keynes return to rationality as a competitive maximizing process. Rather than directing the theories of Popper and Lakatos against positive economics, Blaug (1976:149) maintains that positive economics is really a Popperian philosophy of science: "Friedman is simply Popper-with-a-twist applied to economics." Without any documentation for this conclusion, we find this conclusion preposterous. Popper as noted previously would disagree with an instrumentally interpreted positive economics which Blaug prefers (see note 4, chapter four, p. 43). Furthermore, although Popper and Lakatos believe Kuhn is overly concerned with the psychology of decision-making in science, they certainly would not go so far as Friedman and rational expectations theorists go and ignore rationality as a concern with mind. In particular, Popper (1972:153-190; 1977 [Popper and Eccles]) is concerned with the human mind and its importance for a conception of science. Although Kuhn, Popper, and Lakatos disagree on the role of a psychology of decision-making in philosophy of science, each implicitly opposes the reductionistic view of scientific decision-making found in positive economics and the view of common sense decision-making found in economic analysis. Thus, it seems that Blaug is the epitome of a philosophically interested economist who systematically embellishes positive economics to accord with the latest in philosophy of science; even though philosophy of science and positive economics may be fundamentally incompatible. But this is not an inconsistency for Blaug since we have already noted that Blaug sees the Keynesian Revolution as returning to the narrow Friedmanian (Popper with-a-twist?) conception of rationality as competitive maximization.

Like Blaug, Leijonhufvud (1976:83) suggests that the original Keynesian Revolution may be told fairly accurately as a Kuhnian paradigm shift. Again, like Blaug, Leijonhufvud (1976:84) maintains the monetarist counter-revolution is better understood in terms of a Lakatosian research program. For example, empirical research that begins within the Keynesian framework eventually proceeds along post-war neoclassical lines. In particular, the work on consumption and investment is linked to a maximizing theory more neoclassical than Keynesian (Leijonhufvud, 1976:84). However, Leijonhufvud (1976:85) maintains that the Keynesian revolution is unfinished. What Blaug takes as the general case (neoclassical economic theory), does not resolve effectively the central issue. For Leijonhufvud (1976:87), the central controversy of the Keynesian Revolution is the coordination of economic activities. In his view, the microfoundations research on the process of monetary exchange, which appears to be compatible with neoclassical economics, actually constitutes the hard core of the economics of Keynes. This coordination aspect of Keynes' framework is incompatible with the neoclassical framework (Leijonhufvud, 1976:106-107). Leijonhufvud (1976: 107) expresses his view:

> I tend, like Clower, to the belief that the neo-Walrasian [neoclassical] hard core is limiting. . . My suspicions focus (so far) on the maximizing behavior postulate in the particularly rigid form it has come to take in neo-Walrasian economics, i.e. as a 'necessary' condition for the intelligibility of behavior.

Apparently, the last part of Leijonhufvud's statement places him in sharp disagreement with Blaug and with positive economics. According to Blaug, economics, including macroeconomics, is built on a restrictive, maximizing conception of rationality. But if Leijonhufvud believes that such a conception of rationality is incompatible with Keynes, then Keynes is also incompatible with positive economics. In more conventional terms, the debate over the Keynesian Revolution is said to be a question of microfoundations. There are two major roots or microfoundations to contemporary economics; a neo-Walrasian source which is predominant and a neo-Marshallian source.[19] Usually, the microfoundation in Keynes' General Theory is considered to be Marshallian rather than Walrasian. But we can take the argument one-step further. Not only does Keynes General Theory require an alternative microfoundation, but it also seems to require an alternative philosophical foundation. Otherwise Leijonhufvud's observation about the rigidity of maximization only will come to naught. At this point, we can paraphrase Keynes' (1936:383) statement that "Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually the slaves of some defunct economist," to read: "Practical economists, who believe themselves to be quite exempt from any intellectual influences are usually the slaves of some defunct philosopher of science."

If a valid microfoundation for Keynes' General Theory requires something other than positive economics as a foundation for economic science; then, perhaps the Keynesian Revolution will need further assessment. The conclusions which both Blaug and Leijonhufvud reach may need to be modified. If a new philosophy of science is required to adequately understand Keynes, then the metaphysical hard core of economics must be modified beginning with rationality. This may be more a Kuhnian rather than a Lakatosian scientific revolution. In other words, if the central issues of the Keynesian Revolution are not yet settled, then neither is our assessment of the relative merits of Kuhn and Lakatos.

In summary, Imre Lakatos developed Karl Popper's notion of falsification into a general view of science. Science progresses as progressive theories with more empirical content replace previous theories; progressive theories falsify degenerating theories. Science also progresses as research programs are replaced. A scientific revolution occurs when a progressive research program replaces a degenerating one. In economics, Lakatos' conception of science also provides the opportunity to reassess positive economics in terms of the Keynesian Revolution. But, as they do with Kuhn, economists interested in Lakatosian views of the Keynesian Revolution do not direct their views toward positive economics. To the contrary, Blaug misleadingly reinterprets positive economics as a Popperian philosophy of science. We reject this unsubstantiated and questionable interpretation of positive economics. We reject this interpretation of positive economics because it prejudges and restricts our understanding of Keynes and the Keynesian Revolution. Positive economics, as a view of economic science, we suggest, seems intimately tied to a neo-Walrasian microfoundation, and a restrictive maximizing concept of rationality. These conceptual rigidities are sufficient to bias our understanding of the Keynesian Revolution in two ways: (1) in favor of neo-Walrasian, neoclassical economics rather than other interpretations, and (2) in favor of Lakatos rather than Kuhn.

SUMMARY

In the previous chapter, we suggest that positive economics appears to be the only intellectual door through which all acceptable ideas about economic science can pass. In this chapter, we observe an implicit manifestation of this tendency. Economists like Blaug, Leijonhufvud, et. al., appear drawn to the philosophy of science literature on revolutions--not to question positive economics--but rather to enhance the scientific status of the profession and their own research. No economist who considers the theories of Kuhn, Popper, and Lakatos directs his concern with the Keynesian Revolution against positive economics.

Since positive economics is not very positivistic, one might surmise that little is lost by not directing the implications of Popper, Kuhn and Lakatos toward positive economics. But if the Keynesian Revolution and subsequent episodes are really a protracted and heavily obscured dispute over the nature of intelligibility and rational decision-making, then this failure to question positive economics has far reaching consequences. The thought that the Keynesian Revolution and contemporary philosophy of science may be about rationality seems to escape all but a few economists, like Leijonhufvud. Commitment to a view of science, like positive economics, may have stronger implications for one's resulting views on economic theory and policy than many economists believe. Indeed, positive economics, we maintain, is a sufficiently restricted conception of economic science to predetermine our approach to economics (Walrasian versus Marshallian) and to prejudge our interpretations of Keynes and the Keynesian Revolution in favor of the neo-Walrasian scheme. The microfoundations issue is also an issue of the appropriate philosophy of economic science.

NOTES

[1]For his reassessment of the concept of a paradigm see Kuhn (1974).

[2]Leijonhufvud (1976:83, n. 32) says, "Kuhn has been the subject of a bit of a fad in economics (as in other fields). The profession at large has become rather tired of facile employments of Kuhnian terminology (I was one of the earlier sinners), while those few of its members who have been intrigued enough with Kuhn to sample the subsequent philosophical discussion have become rather disenchanted with the evolution of Kuhn's position."

Perhaps what Leijonhufvud has in mind is the psychological turn of Kuhn (1970a) toward "irrational" processes of discovery and intuition. See notes 9 and 10 below.

[3]Recently a full length monograph appeared which analyzes the Keynesian Revolution as a Kuhnian revolution in great detail. This is G. Mehta's (1978) The Structure of the Keynesian Revolution. Mehta concludes, "It is to be hoped that enough evidence has been adduced to prove that the Keynesian Revolution was a revolution in the sense of Kuhn." Like the others, Mehta makes no suggestion that the Keynesian Revolution *per se* is evidence that positive economics needs to be questioned. This further illustrates our point that economists, to a large degree, use philosophy of science for their own purposes rather than learning from it.

[4]Kraft (1953:36,37) states, "Popper, in his Logik der Forschung . . . held against the entire [verifiability] theory of meaning that it amounted to an arbitrary stipulation." Ayer (1959:6) states, "Popper was not in fact a member of the Circle and would at no time have wished to be classed as a positivist. . ." Carnap (1953 [1936 and 1937]:49) noted that Popper pointed out "the impossibility of absolute verification."

[5]Popper (1959:50) qualifies this conclusion: "In point of fact, no conclusive disproof of a theory can ever be produced; for it is always possible to say that experimental results are not reliable. . ."

[6]Carnap (1953 [1936 and 1937]) incorporates Popper's falsification argument when he reformulates verification into confirmation.

[7]Popper's theory that science progresses through conjectures and refutations is the subject of Agassi's (1963:78) monograph. But Agassi's work has not gained the attention that Kuhn's and Lakatos' have. His views of science lie beyond the scope of this study.

[8]Williams (1970:49) states, "If I understand Sir Karl Popper correctly, science is basically and constantly potentially on the verge of revolution."

[9]Lakatos (1970:93) states, "My concern is rather that Kuhn, having recognized the failure of justificationism and falsificationism in providing rational accounts of scientific growth, seems now to fall back on irrationalism." For Kuhn's response see Kuhn (1970b, 1970c).

[10]Lakatos' concern with alleged irrationality in Kuhn's work should not be underestimated. Lakatos (1970:178) states, "In Kuhn's view there can be no logic, but only psychology of discovery. . . In Kuhn's view a scientific revolution is irrational, a matter for mob psychology." Lakatos (1978a [1971]:133) begins to talk of a theory of scientific rationality as being the central concern of history and philosophy of science. This clash between Kuhn versus Popper and Lakatos concerns "our central intellectual values, and has implications not only for theoretical physics but also for the underdeveloped social sciences and even more for moral and political philosophy," (Lakatos, 1970:93). R. F. Baum (1974:99) terms this debate "a crisis of modern intellect."

[11]Popper (1959:86) states, "We have seen that non-reproducible occurrences [of falsification] are of no significance to science." This means that evidence disconfirming a theory must be repeatable to be taken seriously.

[12]Notice that the positivist's distinction between theoretical and observation languages gets reinterpreted. Simple facts no longer abound but are wholly theoretical. Popper (1963:46) states, "Observation is always selective. It needs a chosen object, a definite task, an interest, a point of view, a problem." Also, see Lakatos (1970:161).

[13]Lakatos (1970:119) says, "Contrary to naive falsificationism, no experiment, experimental report, observation statement or well-corroborated low-level falsifying hypothesis alone can lead to falsification. There is no falsification before the emergence of a better theory." For example, Prout's hypothesis was evidently falsified experimentally many times during the nineteenth century. All the chemical elements were not shown to be whole number multiples of hydrogen until isotopes were discovered much later. See Lakatos (1970:138-140).

[14]In Lakatos' (1970:129) words, "We must use a pluralistic model. In the pluralistic model the clash is not 'between theories and facts' but between two high level theories: between an interpretative theory to provide facts and an explanatory theory to explain them."

[15]Blaug (1976:164) maintains that the Keynesian Revolution initially was progressive. However, after World War II, he suggests that static Keynesian macroeconomics degenerated. See our discussion of Blaug, pp. 64-67.

[16]The papers of Blaug and Leijonhufvud were part of a joint colloquium of physicists and economists concerned with Lakatosian scientific research programs. The conference was initiated by Lakatos and held in Nafplion, Greece in September, 1974. This was the first instance of philosophers of science actively widening their direct field of inquiry beyond the natural sciences and physics. Social sciences, although previously considered, were of secondary importance. Leijonhufvud (1976:65) expressed what Lakatos had in mind: "The script that he ordered was to retell my version of the Keynesian Revolution story. . . and discuss whether the story can be told to advantage as one of Kuhnian revolution or as a shift from one Lakatosian research program to another." The economics papers of the conference can be found in Latsis (1976).

[17]Concerning Clower and Leijonhuvud, Blaug (1976:164-165) states, "The efforts of Patinkin, Clower and Leijonhufvud to give a disequilibrium interpretation of Keynesian economics, and thus to integrate Keynesian theory into a more general neoclassical framework with still greater 'excess empirical content', would seem to constitute a 'progressive' research programme, superseding both static pre-Keynesian microeconomics and static Keynesian macroeconomics."

[18]Blaug (1976:164) states, "certain puzzles about the Keynesian Revolution dissolve when it is viewed through Lakatosian spectacles." But if Blaug's interpretation of Keynes is permitted, the whole Keynesian Revolution itself dissolves. There would be no puzzles left.

[19]The distinction between Marshallian and Walrasian microfoundations is developed further in Clower (1975) and Leijonhufvud (1974). Clower (1975:4) implies that the Marshallian view is concerned with process: "Keynes must rather have intended to offer the world an analytically manageable aggregative version of the kind of <u>general process analysis</u> that Marshall himself might have formulated. . ." Concerning a view of economic transactors in Marshallian analysis, Clower (1975:8) states that: "Economic agents are conceived to be not so much rational as reasonable. Individuals fumble and grope rather than optimize."

Concerning the Walrasian view of the economy and individual transactors, Clower (1975:9) again offers these comments: "The rationality of economic agents may be taken for granted, for price information is not only complete but also costless to obtain, and quantity information is irrelevant to anyone but the auctioneer. . . As for the coordination of economic activities, that is not so much a question to be investigated as a proposition to be proved."

Clower's view of rationality is much narrower than ours, since he suggests Marshallian-type transactors are "reasonable" rather than "rational." However, from Clower's comments, we suggest that the nonjustificational view of rationality we adopt is similar to his view of "reasonable" transactors in real economic processes.

Chapter 4

TOWARDS A COMPREHENSIVE, STRUCTURAL PHILOSOPHY OF SCIENCE

From the preceding account of recent philosophy of science, we tentatively conclude that an alternative view of science and economic science may be necessary to fully understand Keynes and the Keynesian Revolution. We suspect that the microfoundations issue in monetary theory, which we take up in the next chapter, is also an issue of "what philosophical foundation for economics?" However, before considering our conclusion about an alternative philosophical foundation for economics, we must give a coherent presentation of a structural philosophy of science.

At the very least, our structural philosophy of science must have an adequate theory of decision-making; scientific, economic, or otherwise. This is in sharp contrast to logical positivism and Friedmanian positive economics which maintain an anti-metaphysical and anti-mentalistic view of rationality. Such a positivistic view of rationality implicitly is unacceptable to Popper, Kuhn, and Lakatos; who, since they are concerned with the growth of knowledge, are also deeply concerned with the rationality of decision-making processes in science.[1] A realistic conception of decision-making requires a novel conception of rationality; not only for science, but also for economics.

In this chapter, an attempt is made to bring the various contributions to philosophy of science together into one view of science which also has an alternative conception of rationality. This comprehensive view is a structural philosophy of science which has three aspects. In the first section, epistemological aspects are considered; in the second section, a new theory of rationality, nonjustificational rationality, is discussed; in the last section, ontological implications are considered.

EPISTEMOLOGY

In the preceding chapters, our assessment of various philosophies of science and their relation to positive economics and the Keynesian Revolution may leave one with a chaotic view of philosophy of science. To remedy this impression, we outline a coherent and consistent approach to science--a structural philosophy of science. Since science is largely concerned with knowledge, we begin with an exploration of the epistemological aspects of our structural philosophy of science. If the views of science discussed in previous chapters can be combined into a unified conception of scientific knowledge, then we will have a basic component of our structural philosophy of science. Such a view must combine Kuhn's view of science with Lakatos' and to a lesser amount with Popper's. Only Weimer (1974b) seems to make the attempt to reconcile and synthesize these views.[2] He integrates the epistemological aspects of recent philosophy of science. Weimer's synthesis of Popper, Kuhn, and Lakatos is followed for the most part in the following paragraphs. Rationality and ontological aspects of a structural philosophy of science are considered in subsequent sections.

In the first subsection to follow, Weimer's synthesis of recent philosophy of science is motivated by reconsidering the historical dimension of recent philosophy of science. Then in the other subsection, the integrated view of science is used to give a general interpretation of scientific practice which is semantically independent of historical considerations. In other words, we go from history to methodology and from methodology to scientific practice.

From History to Philosophy of Science

In recent research concerning the nature of science, the distinction between the history and philosophy of science is blurred. Popper, Kuhn, Lakatos, and others effectively maintain that a given philosophy of science can only be "tested" by reconstructing or retrieving the history of science.[3] First, a selective historiography of science is produced which supports an accepted methodology. Then another historiography is produced which "falsifies" one methodology by providing supporting evidence for an alternative methodology. For example, Kuhn's Structure of Scientific Revolutions falsifies the cumulative record view of the history of science implicit in logical positivism by supporting Kuhn's views of science. Likewise, Lakatos' research programs refute Kuhnian and Popperian views on science by supporting Lakatos' views.

However, whether Kuhn and Popper refute positivism and whether Lakatos refutes Popper and Kuhn depends on the nature of scientific activity. If science is a multi-dimensional activity, then the various philosophies of science could be producing special-case evidence which other methodologies ignore. Rather than completely refuting each philosophy of science, an alternative methodology may merely point to the restrictive nature of scientific activity in existing philosophies of science. Following this view leads to the conclusion that recent philosophy of science can be summarized employing two complementary concepts of scientific activity. Science proceeds on different theoretical levels of analytical abstraction and normal and revolutionary science can be distinguished. By tentatively accepting the distinction between normal and revolutionary science and different theoretical levels of analysis within science, a more comprehensive view of science may emerge.

If there are two basic conceptual distinctions implicit in the pattern of philosophical conjecture and historical refutation summarized above, then these conceptual distinctions may be used to provide a consistent and unified metatheory of science (Weimer, 1979:1). A metatheory is a generative conceptual framework which systematically addresses all phenomena in the domain of investigation. Thus, a metatheory of science aims to reinterpret and combine recent contributions (Popper, Kuhn, and Lakatos) to philosophy of science. A more general conception of science is implicit in our structural philosophy of science which entails a fallibilistic view of knowledge and an element of realism.[4] Scientific knowledge results in tentative knowledge claims about structural relations in the real world which are correctable at a later point in time. The corrective process reveals error through various modes and levels of criticism. Evidential considerations are only one part of criticism. Other aspects of criticism are considered once rationality is discussed.

Figure 2 summarizes schematically the epistemological aspects of our structural philosophy of science. There are two types of science and three different levels of analysis in recent philosophy of science. Concerning the types of science; Popper, Kuhn and Lakatos all recognize revolutionary science, while only Kuhn and Lakatos seem to give much attention to normal science. One of Popper's colleagues, Watkins (1970:27), even views normal science as "hack science" not worthy of philosophical attention. Concerning the three levels of analysis in science: within, between and beyond theories; they are apparent in Kuhn's, Popper's and Lakatos' conceptions of science. Lakatosian research programs distinguish science when it is concerned with isolated theories, from when it is concerned with assessing competing theories; while Kuhnian paradigms permit science to be a clash of conceptual frameworks at the metaphysical level. This metaphysical clash is deemed irrational by Popper and Lakatos.[5]

Our consideration of the Keynesian Revolution in the previous chapter would seem to support the conceptual distinctions made above. In an uncritical way, many economists seem to accept the historical events of the Keynesian Revolution as a genuine scientific revolution. Also, events after the Keynesian Revolution, like the monetarist counter-revolution, seem to be accepted as something less than a scientific revolution. Long before Kuhnian scientific revolutions were well-known, the literature shows that economists had talked about the new macroeconomics as the Keynesian Revolution.[6] Also, we know that the monetarist counter-revolution precedes Lakatos' notion of a research program. Thus, there is good reason to suggest that tacitly

Level of Analysis	Nature of Science: Normal science	Nature of Science: Revolutionary science
Within theory	1 Testing of consequences (hypotheses) of particular theories within one research program.	2 Anomaly collection
Between theories	3 Evaluation of particular theories that constitute a research program.	4 Rejection of all theories within the research program (s).
Beyond or behind theories	5 Points of view: absolutely presupposed, never articulated or acknowledged. Commitment to (a) a metatheory, (b) a psychology of 'seeing'.	6 Paradigm clashes--of incommensurable points of view.

Figure 2 Epistemological Aspects of a Structural Philosophy of Science Resulting from Weimer's (1974b:372) Synthesis of Popper, Kuhn, and Lakatos and Giving a More Complete View of the Growth of Scientific Knowledge

some economists do realize extra-theoretical and empirical dimensions to the growth of economic science.

In their more critical assessments of the Keynesian Revolution summarized in the previous chapter, our two conceptual distinctions about science are even more apparent. Concerning the nature of science, the Keynesian Revolution is evidence of scientific activity in economics which is largely non-positivistic.[7] This is true even though many economists do not go one step further and question the most positivistic aspects of positive economics. Concerning levels of analysis in science, the Keynesian Revolution lends support to the views of both Kuhn and Lakatos. The immediate events of the Keynesian Revolution are Kuhnian in nature, while the subsequent interpretations and misinterpretations of Keynes are Lakatosian in nature.[8]

To summarize, two concepts concerning science implicit in recent history of science provide a basis for a more comprehensive, structural view of science. By distinguishing different levels of analysis within science and by distinguishing normal from revolutionary science; the views of Popper, Kuhn, and Lakatos can be combined. With respect to this combined view, any of the particular approaches to science are special cases. This can be illustrated with Figure 2.[9] Popper, emphasizing science as a revolutionary clash of broad, theoretical positions, encompasses boxes two and four. The distinction between levels of analysis in Popper's work is not well developed so boxes two and four are not really separate. For him, all science is revolutionary science. Lakatos, recognizing two levels of scientific activity and both normal and revolutionary science, encompasses boxes one through four. The conceptual distinctions suggested by the appropriate boxes are maintained by Lakatos as well as any other philosopher of science. Finally Kuhn, distinguishing most clearly between normal and revolutionary science, encompasses both columns of Figure 2. This is due to the ambiguity of the paradigm concept in Kuhn's work. Kuhn does distinguish between levels of theoretical abstraction in science, but under emphasizes the mid-level concept of a research program.[10]

From Philosophy and Methodology to Scientific Practice

What is found in Weimer's integration of recent philosophy of science is a more adequate description of scientific activity. It is a more adequate description because it is a more complete account of actual scientific activity and practice. To some extent, each view of science is deficient because some important aspect of scientific practice is ignored by each methodology. Of course, a combination of methodologies into a comprehensive structural philosophy of science, eventually may be shown to be deficient on similar grounds of inadequacy. But the possibility of correcting our views in the future is no reason to eschew our best attempts at conceptual unity for the present.

What follows in subsequent paragraphs is a simple reinterpretation of scientific practice in light of recent history of science and its synthesis into an integrated philosophical point of view. The following description is meant to restate the conceptual distinctions behind Figure 2 and to demonstrate the plausibility of a structural philosophy of science as a conceptually unified approach to science and to current scientific practice. We proceed from the simplest level of analysis and proceed to the most complex, keeping in mind that anomalies may appear at each level of analysis. These anomalies one day <u>may</u> necessitate a rapid shift in the conceptual framework of the scientist. In other words, such anomalies may be the source of scientific revolutions. But at this point, our discussion of different levels of analysis in scientific practice is confined to normal science.

If our metatheoretical description of scientific practice is to have some validity, then it ought to incorporate some positivistic aspects of science as well as the views of Popper, Kuhn and Lakatos. Although logical positivism has been repudiated as an adequate philosophy of science, it is hard to believe that so many intelligent people are universally wrong. A positivistic aspect to science may be just one level of scientific activity which is taken mistakenly as the only level of scientific activity. When individual theories are being assessed one at a time, the logical positivist's view of science may have a degree of validity. At this level, testing the success of a simple theory (hypothesis) by its predictions has some descriptive accuracy. Research proceeds by specifying a model which is representative of a more general theory. Hypotheses are derived deductively from the model. Then, the hypotheses are tested to see if they accurately predict observable outcomes. Observations are those empirical outcomes which are <u>currently</u> available to the scientist. This model of research is found in most research published in journals and is called the hypothetico-deductive or hypothetico-inferential model of scientific research.[11] The hypothetico-deductive model of single theory research is a formulation of the analytic-synthetic destinction which permits the active researcher to accommodate this distinction. This may be the one contribution of logical positivism which stands the test of time.

To restate the view, researching theories one-at-a-time according to a hypothetico-deductive scheme, describes much scientific activity, particularly in economics. Since the scientist is preoccupied with testing the one theory under his consideration, criticisms and competing theories may be ignored. Scientists justifiably are more optimistic if not "positivistic" about their conclusions at this mono-theoretical level of analysis.

Upon reflection, almost any group of scientists realizes the success of their work does not rest on isolated theoretical successes. Success must be succeeded by success within a unified whole. Otherwise, the basis of the scientist's viewpoint presumed in his research is open to question.[12] Additionally, the simple-minded view of facts presumed in testing single theories one-at-a-time is not accurate. Not only are (explanatory) theories needed to explain the facts, but also (observational) theories are needed to ascertain what could constitute a good fact.[13] Thus, the scientific community must at some point assess its own work as the progressive development of better theories, both to explain the facts and to get the facts. As long as a research program is progressing and being enriched in empirical content, the criticism of rival research programs and the force of anomalous findings are recognized but not heeded.[14] Criticism is more apparent at the research program level, but a positive thrust to scientific activity remains.

In economics, one of the best examples of a progressive research program was the consumption function literature. For almost two decades, successively better (greater scope and empirical content) theories of the consumption function were postulated and tested.

Eventually, they resulted in a generally accepted theory of consumption which was one of the key structural relations in models of the United States economy. As long as research on the Keynesian consumption function exhibited success, critics of Keynesianism were held at bay. Once Keynesianism effectively exhausted one of its most fruitful domains of research, then the criticism of opponents began to carry much greater force. As a result, the Keynesian view of monetary dynamics was much less successful than the consumption function research.[15]

Research programs are embedded in a much broader conceptual framework than the scientist usually considers in his professional research. The metaphysical aspects of his viewpoint may rarely be articulated. When attempting to assess the relative adequacy of alternative metaphysical outlooks, criticism is paramount. There is an element of "incommensurability" between perspectives. Criticism advanced on the basis of one point of view is directed at a rival point of view. Often, the criticisms only have force with the originating point of view. The perspective toward which a criticism is advanced might interpret the criticism as misplaced or irrevelant. Thus, at the metaphysical level, the activity of scientists is mostly critical, with neither opposing view penetrating the other.

For example, in economics there are two opposing views on expectations. One is a recent development known as rational expectations. The other is a neo-Austrian position which emphasizes the subjectivity of expectations. Neither rational expectations nor subjective expectations are considered directly in the professional writings of the opposing view.[16] Evidently, there is a clash at the metaphysical level.

Criticisms of each toward the other fall on deaf ears. To the extent that positive economists consider mind to be a metaphysical issue irrelevant to economic science, the issue is metaphysical. To the extent that the subjectivity of the Austrian perspective makes mind a relevant issue to (their) economic science, the issue is scientific. Hopefully, some middle ground will be found to attentuate these differences.

In summary, since we have achieved a tentative integration of the epistemological aspects of recent philosophy of science, a general caricature of scientific practice can be set forth. Practicing scientists and progressive communities of scientists do more than just assess particular theories, one-at-a-time, independently of other lines of research. All levels of scientific activity seem to be necessary for genuine scientific progress to be made. Yet, even multiple levels of abstraction do not guarantee the continuing success of any particular approach to genuine scientific problems. Anomalies may grow which require new metaphysical ways of viewing the world. These rapid shifts of the conceptual framework of the scientist are scientific revolutions.

RATIONALITY AND EMPIRICISM

Lurking behind positive economics, recent philosophy of science, and a view of actual scientific practice is one very fundamental issue, rationality. Rationality, as first encountered in chapter two, takes the form of a basic assumption of the neoclassical research program in economics. For Friedman, rationality takes the form of maximizing behavior. He maintains that conscious maximization is not a realistic requirement for positive economics. Rationality is encountered a second time in chapter three when economists' assessments of the Keynesian Revolution are considered in light of recent philosophy of science. In recent philosophy of science, the issue of irrationalism arises between Kuhn and Lakatos. Lakatos maintains that Kuhn's account of science depends too much on irrational psychological processes of discovery and perception. Finally, rationality is also tacitly at issue in the immediately preceding discussion of epistemology.

The alternative descriptions of scientific practice really deal with rational dimensions of scientific practice. More of scientific practice is "rational" for Popper and Lakatos than for logical positivists. Even more of scientific practice is rational for Kuhn, who has no inhibition about psychological and sociological dimensions of scientific practice. Thus, the lines of Figure 2 also represent boundaries for various concepts of rational scientific activity.

In the following paragraphs, the relation between empiricism and rationality is explored. In the first subsection, an empirical view of rationality is considered and a very fundamental conceptual dilemma to which the empirical view of rationality leads--the limits-of-rationality dilemma. In the second subsection, an alternative conception of rationality is explored, nonjustificational rationality. Nonjustificational rationality may offer a solution to the limits-of-rationality dilemma. In the last subsection, the various dimensions of criticism which relate to nonjustificational rationality are developed. Empirical evidence is just one of several possible dimensions of criticism.

Induction and the Limits of Rationality

The problem of the rationality of scientific activity may be one of the most pressing problems in contemporary philosophy of science. Recent philosophy of science can be viewed as a quest for a theory of rationality. The rationality of science as an issue is concerned with the following question: What is the nature of scientific inference and what makes it a rational source of knowledge? As such, rationality is intertwined with the problem of induction.[17] The problem of induction is how to empirically justify accepted processes of empirical verification found in science and scientific literature.

To empiricists, science is the most rational of all human intellectual endeavors. The locus of rationality lies in the experimental techniques available to the scientist. His hypothetical knowledge claims are justified by appeal to the observed consequences of empirical methods. But in the appeal to observation, the empiricist repeatedly runs into fundamental dilemmas. Logical positivists reformulate induction from inductive generalization to verification in order to account for the analytic-synthetic distinction. Then verification is reformulated into confirmation to account for falsification. But confirmation leads to another dilemma regarding the problem of induction. The problem of induction is to provide an empirical justification of accepted methods of empirical science. If such a justification cannot be found, an infinite regress may result (Popper, 1959: 30; Lakatos, 1978b). If the rationality of science consists in its empirical justification, then science itself may not be entirely rational. The possibility of empirical science not being rational by its own criteria of rationality, raises perplexing but interesting issues. The major issue is the limits-of-rationality.

To illustrate, the problem of induction can be formulated in terms of confirmation. A rational scientist is one who seeks confirmation for all hypothetical statements derived from his theoretical research. Confirmation requires that the evidence confirm the hypothesis to a certain degree r, where r is the relative frequency of supporting observations. Symbolically, the confirmation statement is written $c(h,e) = r$, where h is the hypothesis and e is the relevant evidence. But the question of empiricism goes beyond these simple confirmation statements. A consistent empiricist not only seeks justification for hypotheses like h, but also for confirmation itself. For example, let d be another confirmation statement about the validity of c. Then, $d\ (c,e) = r$, where c is the usual type of confirmation statement, e is evidence relating to the validity of confirmation statements, and r is the relative frequency with which confirmation statements like c have been favorably observed. A statement like d, asks whether the principle of confirmation is itself confirmable with empirical techniques. The process could continue _ad infinitum_. For each confirmation statement, one can in turn formulate a more general confirmation statement at a higher level of abstraction. The more general statement contains the more particular confirmation statement as an hypothesis. Naturally, this process must be stopped. The issue is whether it can be done empirically if it can be done at all; otherwise skepticism prevails.[18]

Skepticism results when empiricism censures itself in the same way empiricism censures other concepts not based in sense experience. If the principle of induction is unobservable in the same manner as causality and mind are unobservable, then the foundation of empiricism is questionable.[19] Without an ultimate standard by which the claims and activities of science can be judged as being scientific and empirical, skepticism becomes irrationalism. This means that knowledge, as such, is unobtainable and attempts to obtain knowledge have no rational foundation.[20]

The best argument which has been suggested to end the infinite regress and refute skepticism and irrationalism is the limits-of-rationality argument (Ayer, 1956:71-77). The strategy is to suggest that a concept of rationality sets the standards of rationality. The concept of rationality, it is maintained, is exempt from the standards which it establishes.[21] Rationality per se is a concern which is limited to theories, hypotheses, and related evidence. The concern for rationality is not extended reflexively to the ultimate standards of rationality. Only if some different standard of rationality is developed, could the standards of rationality be irrational. Ayer (1956: 75) expresses this view:[22]

> The use of scientific method is irrational. . . only if there were a standard of rationality which it failed to meet; whereas in fact it goes to set the standard: arguments are judged to be rational or irrational by reference to it. . . The skeptic makes his point. There is no flaw in his logic: his demand for justification is such that it is necessarily true that it cannot be met. But here again it is a bloodless victory. When it is understood that there logically could be no court of superior jurisdiction it hardly seems troubling that inductive reasoning should be left, as it were, to act as judge in its own case.

Ayer's limits-of-rationality defense of empiricism is the most sophisticated defense of empiricism to date.[23] However, what Ayer assumes to be the case, is the issue at hand. Alternative standards of rationality do exist. Traditionally, rationalists justify knowledge claims with introspective intutition rather than the observational results of experimentation.[24] Both the rationalist's and the empiricist's concepts of rationality may be similar in that standards of rationality are required, but the standards are different. The limits-of-rationality argument works only if one has already decided which standard of rationality is to be followed. The following conclusion is inescapable; If rationality is conceived in the manner described above, then one's concept of rationality is made on the basis of irrational concerns and judgments.[25]

In summary, science is presumed by empiricists to be the most rational of all human intellectual endeavors. The locus of rationality is found objectively in experimental techniques rather than with the scientist himself. His hypothetical knowledge claims can be justified by appeal to the observed consequences of empirical methods. However, when the issue is shifted to a more abstract level of analysis, no empirical justification can be found for the principle of induction. By the principle of induction, we mean that all knowledge consists only of those statements which have an empirical justification for induction; empiricism by its own standards, becomes irrational and leads to an infinite regress.

Nonjustificational Rationality

Irrationality and skepticism are the ghosts which have haunted almost all twentieth century intellectuals. Having accepted unattainable and perhaps irrelevant concepts of rationality and knowledge, many thinkers opt for pragmatic, existential, or sectarian world views.[26] But the possibility of substantively reformulating some of the issues involved may provide the opportunity for some progress. Progress, if it is to be achieved, might be found if better theories of knowledge and rationality are achieved. Already, an attempt has been made to fashion epistemological aspects of a more comprehensive, structural philosophy of science. What is needed at this point is a better conception of rationality than the concept of rationality implicit in empiricism.

In empiricism in general and logical positivism in particular, rationality amounts to delimiting the empirical methods of science as the authoritative justification of genuine knowledge. Confirming observations constitute the rational authority which justifies a scientific hypothesis as a knowledge claim. Since the justification of hypotheses on the basis of evidence assumes supreme importance, the concept of rationality in empiricism is called justificational rationality.[27] This justificationist view of rationality is not limited to empiricism. The traditional rationalist justifies his claims to knowledge, but provides a different source of justification than sense experience or observation. A rationalist justifies his claims to knowledge by introspectively intuiting clear and distinct ideas. With respect to rationality, empiricism and rationalism may not be that different; they may exhibit a common justificational structure to rationality.[28] Thus, our consideration of a justificationist conception of rationality transcends our concern with empiricism; however, the dilemmas of empiricism remain as our point of inquiry.

Justificationist rationality, as portrayed above, is more specifically developed and criticized by one of Popper's students, W. W. Bartley (1962, 1964). Bartley, of course, is opposed to the justificationist conception of rationality; he summarizes the justificationist view as consisting of two requirements. Paraphrasing Bartley's (1962:120; 1964:12) criteria rather closely, to be rational in the justificationist sense means adhering to two requirements:

1. Rationality means accepting <u>any</u> position that is justifiable on the basis of an authoritative criterion.
2. Rationality means accepting <u>only</u> those positions that can be justified by an authoritative criterion.

Now requirements one and two, on the surface, may appear to be somewhat redundant. However, from Bartley's use of the two requirements we get a better idea of what he has in mind. Requirement one is meant to specify what positions or statements may be <u>accepted</u>; while requirement two specifies those statements we are <u>forbidden</u> to accept. Thus, we could restate requirement two as:

2a. Rationality means rejecting <u>any</u> position that cannot be justified by an authoritative criterion.

In terms of requirements 1 and 2a, a traditional justificational empiricist or rationalist, assuming his criterion of justification, accepts any justified position <u>and</u> rejects any unjustified position.

According to the requirements of justificational rationality, a decision is rational if the two preceding criteria are met. The history of western thought and most scientific journals are filled with justificationist arguments. But rather than looking at the justificationist aspects of common thought processes in science and elsewhere, we follow the lead of the limits-of-rationality argument in the preceding section. If the two requirements are a general statement of a generic justificational approach to rationality, then the limits-of-rationality argument ought to be apparent in this general statement of rationality. Bartley (1964:5,12-13) argues that the two requirements lead to a logical difficulty. Both requirements cannot be maintained simultaneously or separately. Taken separately, the requirements lead to an infinite regress; taken together, they are inconsistent (Bartley, 1962:120-122).

Consider the two requirements separately. We must ask the question whether the criterion of rationality in each separate statement consistently applies to each statement itself. For either statement to be accepted as a criterion of rationality, its validity must be intuitively clear or empirically evident. From the empiricist's point of view, there is no observational evidence that justifies the principle of induction. Seeking empirical evidence for either requirement leads to an infinite regress. Similarly, from a rationalist's point of view, it is not apparent that intuition of clear and distinct ideas justifies the pure appeal to reason. One would need a good reason for appealing only to reason or intellectual intuition. Thus, a rationalistic rather than an empirical infinite regress also could result. We can only conclude that each requirement by itself asserts its own untenability.

When the two requirements are considered together, they can be shown to be inconsistent. Of the two requirements, the second is the stronger of the two statements. Since our task is to apply the requirements of rationality to the requirements themselves, we proceed by inquiring whether the first requirement can meet the second (2a). The second requirement (2a) <u>forbids</u> the acceptance of any unjustifiable arguments; we must reject those positions which cannot be justified by appeal to reason or sense experience (Bartley, 1962:121). Because we already know that the first criterion is unacceptable, the second requirement forbids the first. Thus, the second requirement asserts the untenability of the first and the two requirements cannot be simultaneously maintained.[29] We conclude that there is an inconsistency to the logical structure of traditional concepts of rationality.[30]

In a very real sense, the justificationist concept of rationality common to both empiricism and rationalism is overcommitted. To be rational in the sense of the second statement means one must never maintain any unjustified hypotheses or principles. This is extremely restrictive. Pragmatically, at most one could justify only a small portion of the hypotheses, beliefs, and principles on which one acts. The critical assessment of statements beyond one's expertise or professional purview is best left to others.

If the problem with traditional concepts of rationality is due to their justificational structure, then perhaps some other principle of rationality may offer a solution to the logical and pragmatic dilemmas of rationality. An alternative principle implicit in recent history and philosophy of science is criticism (Popper, 1959:16; 1963:256; 1972: 33-35). Criticism encompasses other important dimensions in addition to experimentation and therefore is a generalization of the narrower conception, falsification. Since the other dimensions of criticism significantly alter the nature of rationality, this alternative concept is called <u>nonjustificational rationality</u>. With the work of Popper, Kuhn, and Lakatos, nonjustificational aspects of rationality are paramount. Popper, in focusing on revolutionary science, is perhaps the first to see that a justificationist view of science as growing through a process of empirical verification, cannot account for the activity of revolutionary scientists like Einstein. Bartley (1964:23) states: "The main originality of Popper's position lies in the fact that it is the first nonjustificational philosophy of criticism in the history of philosophy." Then Kuhn wrote a nonjustificational historiography of science which effectively refutes the justificational view of the history of science in logical positivism. Normal science is justificationist only when the more abstract aspects of the conceptual framework of the scientist (see Figure 2, p. 80) are ignored. Revolutionary science occurs with an unjustified (but not unwarranted) shift in the conceptual framework of science. Lakatos further develops the nonjustificational view of science in focusing on research programs. Individual theories often are retained long after they have been refuted since no better one may exist. Also, falsifying evidence in one domain of inquiry in a research program can be held at arms length if progress is being reached elsewhere. At the level of research programs, the evidential consideration of individual hypotheses is rather ambigious.

Nonjustificational rationality relocates the rationality of science from justification to criticism. Falsification as a deductive criterion of scientific merit, in part, is responsible for this shift. However, falsification should not be construed as negative justification. In other words, the impact of falsification is not the provision of more authoritative reasons for throwing a theory away. Falsification opens the door to the proliferation of hypotheses and theories which may not yet be refuted. Without an accepted refutation, the scientist must choose in which research program he will work. In the face of theoretical pluralism, both justificationism and negative justificationism are inadequate.

Nonjustificational criticism makes this view of rationality structurally different from the type of assessment the justificational empiricist or rationalist offers. Criticism is reduced to justification for the consistent rationalist or empiricist, with one major exception--the ultimate standards of rationality are held to be uncriticizable. However, if we make criticism different than justification, the internal inconsistency and untenability of justificational rationality are avoided. Rationality in this critical, nonjustificational view, now amounts to holding every position open to criticism (Bartley, 1962:146). <u>This includes the nonjustificational concept of rationality</u>. If a criticism could be advanced, that the nonjustificational concept of rationality leads to an infinite regress, then a new concept of the structure of rationality would be sought. But this is a major difference between justificational and nonjustificational rationality. Criticism is consistently applicable at all levels of the conceptual framework, including the concept of rationality. Nonjustificational rationality is a concept of rationality which is just as tentative and conjectural as the rest of human knowledge. Indeed for a nonjustificationist, criticism of his concept of rationality is not held back with a limits-of-rationality argument. Rather criticism of the notion of rationality is desirable; it is the "rational" thing to do. Thus, a nonjustificational concept of rationality applies to the concept of nonjustificational rationality as well as to other positions one holds. Avoiding the limits-of-rationality and an infinite regress by reformulating the concept of rationality along nonjustificational lines would seem to be an intellectual achievement of the highest rank.

To summarize, two very different concepts of rationality are presented above, justificational and nonjustificational rationality. The justificational concept is internally inconsistent and confines criticism to empirical or rational criteria of justification with one major exception: the ultimate standards of rationality are unjustifiable and uncriticizable. In contrast, nonjustificational rationality separates criticism from justification. This is a consequence of the conjectural nature of knowledge. Criticism of the concept of rationality is the rational thing to do.

Nonjustificational Criticism

The scientific process is one which is concerned with rationally eliminating errors in the way we think about the real world. To illustrate, for logical positivism, verification is a process of avoiding error since error apparently isolates the rationality of science. However, for the Popperians, falsification effectively replaces verification as a conception of scientific merit and rationality. Thus, falsification requires a reconceptualization of the concept of rationality. Nonjustificational rationality and criticism is the result of generalizing the notion of falsification to the concept of rationality and to the various levels of analysis in science. Nonjustificational rationality is a critical process which requires the scientist to have a command of his science at all levels of analysis. In short, recent philosophy of science takes us from verification to falsification, from falsification to rationality, and from rationality to criticism.

What counts as criticism from a nonjustificational point of view is an important question. Answers to this question give an indication of how error is ascertained by the scientist. Various types of criticism also suggest various types and levels of rational scientific activity for practitioners of science. No notion or assumption, no matter how surely it seems to be true and fundamental, is sheltered from criticism. The potential for error is pervasive for the scientist, even his most fundamental and rarely articulated concepts. Pragmatically, individual researchers accept aspects of their basic point of view in order to test and explore one theoretical approach or one domain of phenomena. However, as a matter principle, all concepts which constitute a view of science are tentative and conjectural. This includes central assumptions like rationality. A rational scientist is one who is primarily committed to an open mind. His research commitment to a particular point of view is only pragmatic and of secondary intellectual importance.

From a nonjustificational point of view, it is rational to be critical and criticism is multidimensional. This is in sharp contrast to the empiricist's concept of rational scientific activity, where rationality is confined to the evidential justification of particular theories. To fully develop a better view of criticism, we need to think back to the levels of science in Figure 2 (p. 80) and to suggest other types of criticism than empirical evidence.[31] Other types of critical arguments permitted from a nonjustificational perspective are logical and real arguments in addition to empirical evidence. When the levels of analysis in science are joined to these three types of criticism, a multidimensional view of criticism results. Nonjustificational modes of rational criticism are found in Figure 3.

	Types of Criticism		
Levels of Analysis	Empirical	Logical	Real
Within Theories	Are there falsifying observations?	Is the explanatory theory internally consistent?	Is there a real world problem that this theory addresses and does not abstract from?
Between Theories	Is the observational theory relevant to its explanatory theory?	Is the explanatory theory consistent with other similar explanatory theories?	Is this a problem that will generate alternative theoretical positions or research programs?
Beyond Theories	Is the problem an empirical one?	Is the theory consistent with its conceptual framework?	Is the problem a genuine real world problem in this conceptual framework?

Figure 3 Nonjustificational Modes of Criticism Delineating Aspects of a Multidimensional, Nonjustificational Concept of Rationality (See Figure 5, p. 132, for a similar conception of economic rationality)

Now the various modes of criticism can be described in some detail, but examples of most types of criticism are found in discussions of recent science or philosophy of science in the preceding chapters. Any view of science (like positivism or positive economics), which confines scientific assessment to prediction, is a one-dimensional view of scientific activity. Criticism for the positivist is directing simple, empirical evidence toward particular theories. However, most economists have had experience with econometrics and thus implicitly with a multidimensional view of assessing theories. Since there is no reason to believe that econometrics is any less theoretical than economic theory, falsifying observations can be directed toward econometrics as well as toward economic theory. In practice, many economists apparently do not realize how fundamentally theoretical econometrics modifies our view of economic science. If observations are simple and self-evident (the assumption of empiricism), then an observational theory is not needed. A sophisticated observational theory, contrary to common sense, undermines the very core of empiricism.[32]

What we are most concerned with, however, is the role of realism as a critical argument in the development of scientific knowledge. In particular, Friedman, suggesting that realism is not an empirical test of basic assumptions, seems to dismiss realism as an important criticism. As suggested earlier, Friedman is defending the maximizing conception of rational behavior. The rationality of behavior in economics evidently is not concerned with the consciousness of transactors. It makes no difference for neoclassical economics whether individuals consciously maximize, as long as neoclassical theory predicts well. In making his argument, Friedman seems to suggest that realism plays no role in economic science. Thus, Friedmanian positive economics does not recognize our third type of argument, arguments from realism.

Realism is a much more difficult notion to understand because scientists and economists explicitly have professional training directed to developing empirical techniques and analytical skills. Realism is a correspondence notion which must be considered first at the most abstract level of analysis. In other words, realism (like falsification) as a check on science gets shifted to higher levels of analysis and then back to individual theories. A theory may be unrealistic to the extent which it abstracts from a problem in order to simplify it. The basis of such abstracting may be that other theories have been successful in making this sort of abstraction. But, eventually, it may take a catastrophic problem-situation like the Great Depression, to make a scientific community realize how unrealistic its most fundamental assumptions and conceptual framework are. For long periods of time, it may be possible for a discipline to maintain an unrealistic conceptual framework if no cases in point are strong enough to force the issue. Then, when the issue is forced, the realism of the whole scientific point of view is in question.

From the perspective of our structural philosophy of science, the fact that positive economists ignore realism is bound to have some unfortunate consequences. From our structural view of science, a restrictive and unrealistic philosophy of science can lead to restrictive and unrealistic theories and research programs.[33] The assumption of rationality as maximization is obviously unrealistic and potentially has severely restrictive implications. Neoclassical positive economics is unrealistically narrow because its conception of rationality is restrictive and naturalistic, particularly in comparison to nonjustificational rationality. Thus, certain economic theories may be unrealistic, if positive economics as a view of economic science is unrealistic. What we need is a problem situation to which neoclassical economics as a theoretical framework and its conception of rationality is demonstrably irrelevant. Our argument will be that the rational expectations approach to simultaneous inflation and unemployment is such a problem situation.

In short, a consideration of the problem of induction in recent history and philosophy of science leads to a consideration of the problem of rationality. For the empiricist, rationality means empirically justifying every principle but empiricism itself. However, without an empirical justification of empiricism, skeptism and irrationalism may result. The rationalist may be no better off since he shares a structural similarity in his conception of rationality with the empiricist. This common structure to rationality is called justificationism. The justificationist concept of rationality is internally inconsistent. Criticism is confined to justification with one major exception. The ultimate standards of rationality are held to be unjustifiable and uncriticizable. In contrast, nonjustificational rationality separates criticism from justification. This is a consequence of the conjectural nature of knowledge; criticism is multidimensional as Figure 3 indicates. Criticism of the concept of rationality is the thing to do. Criticism may take the form of internal coherence, empirical validity, and realism. Knowledge grows as nonjustificational criticisms are addressed to all levels of scientific knowledge.

FROM EMPIRICISM TO REALISM

Having presented two aspects of our structural philosophy of science, epistemology and rationality, we can turn now to the last aspect, ontology. Actually, we consider ontological implications which follow from the other aspects of our structural philosophy of science. This is something less than a full fledged ontology. Our concern for ontological implications can also be construed as a concern for realism.

As presented above, realism is one of the types of criticism which may be advanced in scrutinizing a theory. Realism as a form of scientific criticism is a rather novel innovation in philosophy of science.[34] Friedman, like the logical positivists, maintains the irrelevance of realism to our fundamental views of science. In contrast, the importance of realism for recent philosophy of science is exemplified by our concern for structure as a basic aspect of the real world.[35] Philosophy of science deals primarily with epistemology, the theory of knowledge. Realism is not an epistemological concept, but an ontological concept. Ontology deals with the nature of reality. Our structural philosophy of science is primarily an epistemological notion with ontological implications.[36] This means what can be said about reality depends on what can be known about reality. In this sense, epistemology constrains ontology (Weimer, 1976:7).

In this structural view of science, the epistemological and ontological aspects of science are pluralistic.[37] Epistemological pluralism is a consequence of the shift to falisification from verification (Weimer, (1979). Within scientific research programs, competing theories provide the most effective mode of falsifying criticism: But all theoretical pluralism may not be due to falsification. Theoretical pluralism would result if relatively disjointed domains of real world phenomena existed. If there is an element of separability in the real world, individual theories most likely would be more successfully addressed to only one domain of phenomena. Distinct phases of existence would suggest a plausible reason for divisions among sciences and for major theoretical divisions within sciences.

If there are distinct phases of existence, the unity of science thesis from logical positivism makes no sense. The unity of science thesis is intended to be a logical conception without ontological implications. However, the failure of the various sciences to unify and the failure of positivistic sciences like economics to unify may have the following implication: Theoretical unity is not attainable if the phenomena under study represent more than one distinct phase of existence. After fifty to a hundred years of attempts at theoretical unification, this failure hardly can be due to the provisional and immature status of science. Rather, it may indicate something about the nature of reality.

A pluralistic view of reality results if it is tentatively assumed that the various sciences are better descriptions of reality than sense experience. Then scientific theories are taken to be logical constructs which approximate structural relations in the real world. Structural properties are properties of real world phenomena specifiable with mathematical relations. The mathematical relations of structural properties are descriptive classes of lower level properties called intrinsic properties. Intrinsic properties of external objects may not be directly accessible to the human investigator.[38] Structural properties and intrinsic properties may be related by a transmission mechanism, particularly for the phase of ordinary matter. If it makes sense to consider causal relationships, causality would consist of a transmission mechanism from intrinsic to structural properties within one phase of existence. For example, a causal theory of vision would specify how external objects are related to the physiological process of seeing.[39] But a description in terms of optical physics is vastly different than the description from the point of view of the percipient observer.

Tentatively assuming that the various sciences are better structural descriptions of reality than naive common sense, structural realism is obtained. Figure 4 represents a simple scheme of the different phases of reality. The scheme is highly conjectural. To make comprehensive sense of the meaning of science, something like Figure 4 seems necessary. The various phases of existence are law-like domains of reality. If anything has resulted from modern physics, it is this; the laws which are useful in analyzing ordinary matter are not useful in analyzing quantum phenomena and conversely.[40] Furthermore, few physicists would apply the various levels of laws in physics to living things or to society. To the extent that the biological and social sciences are better descriptions of human existence than common sense, living things, human beings, and societies may compose distinct phases of existence. There may be law-like relations for living things, individual human beings, and society. David Bohm (1957: 31-32) states:

> We may say that with regard to the totality of natural laws we never have enough views and cross sections to give us a complete understanding of this totality. But as science progresses, and new theories are developed, we obtain more

Laws of Society

Laws of Mind-Psychic

Organizational Complexity of Real World Phenomena

Laws of Life-Biotonic

Laws of Matter

Quantum Laws

Figure 4 Ontological Aspects of a Structural Philosophy of Science Resulting from an Inference about Scientific Theories which implies that Reality is divided into Law-like Phases of Existence

> and more views from different sides, views that are more comprehensive, views that are more detailed, etc. Each particular theory or explanation of a given set of phenomena will then have a limited domain of validity and will be adequate only in a limited context and under limited conditions. . . Each different theory or explanation focuses on a certain aspect of the laws of nature that is important under certain conditions.

Often Figure 4 is conceived in a hierarchical fashion. As we move up the figure, each phase of existence seems to presuppose those below. The laws of ordinary matter presuppose the laws of subatomic physics, the laws of living things presuppose those of matter and subatomic physics, and the laws of society presuppose physical, biological, and material laws. The ontological hierarchy is supported with the notion of complexity. The number of factors under consideration, in principle, would seem to increase exponentially from one domain to the next (Hayek, 1964: 334).

A weaker notion than a hierarchy is the concept of nesting.[41] The laws of life and mind seem to be nested within other phenomena. Events at the material and subatomic level may have implications for life and mind. Consciousness and culture seem to be nested within subatomic, material, and biological phenomena and yet, to a degree, independent of these domains. The nesting of one domain within another seems to manifest some invariance (Weimer, 1976: 24). Due to the invariance of the relations between phases, some feedback across phases is possible. It is conscious control over the physical and material environment which suggests feedback may be possible.[42]

An ontological nesting rather than hierarchy is preferred for another reason. As either end of the hierarchy is reached, the tenability of separate phases of existence becomes highly questionable. At the subatomic level, quantum laws are statistical in nature. Bohm (1973: 139) postulates that there is a more fundamental domain of phenomena (matter-energy?) which, if discovered, would account for the probabalistic laws of quantum mechanics. Likewise, economists and other social scientists often maintain that the laws of society can be reduced to the relations of individuals as individuals. Methodological individualism would deny that society, for the most part, is a separate phase of existence independent of individual human beings.

Since there may be a lot of disagreement about a conception of reality to which science as a whole might lead, an alternative strategy is this: Our structural philosophy of science recognizes that all knowledge is conjectural; but conjectural knowledge is attainable. The skepticism which permeates empiricism from Hume to Mill to Russell, might be better focused on ontological questions. Scientific investigation seems to reveal nested structural relations in the world external to man; but, to the levels and direction of nesting, there seems to be no limit.[43] Skepticism is also useful in the study of man and society. If human relations, economic and otherwise, develop and evolve in novel ways, theories about man and his interaction need constant reassessment. Ontological skepticism would suggest such reassessment. By the time theories about human society are set forth and tested, they may be already out of date. The process of postulating, testing, and criticizing theories, particularly economic theories, may affect the phenomena under study. With a sophisticated press and communications network assimilating and interpreting each new theoretical innovation, the independence between the observer and the observed may break down.

In brief, our structural philosophy of science is an epistemological position with ontological implications. Knowledge about the real world is tentative and conjectural. It must be criticized to detect error. Skepticism, which historically is part of epistemology, is relocated from epistemology to ontology. Tentatively, some things can be known about the real world; but there is no reason to suspect that the complexity of the real world is less than infinite. Furthermore, all apparent phases of existence may evolve, some much faster than others. This is another reason for being skeptical.

Realism as a description of science is preferred to empiricism. If scientific theories are more fruitful than the experiences of common sense, reality may exist independently of sense experience. Scientific theories are held to be structural descriptions of objects outside of sense experience. The structure of the external world represented in scientific theories seems to be organized into distinct phases. The phases may be nested within one another. This means that within the phase, objects and events are largely independent of other phases. Since the phases are nested, events in one phase may be unrelated with events in another. Such constant correlation may be due to invariance relations responsible for the nesting. Causality is confined to the relation of objects within one particular phase of existence.

SUMMARY AND CONCLUSIONS TO PART I

History and philosophy of science apparently is evolving from empiricism to realism. Our structural philosophy of science is an epistemological position with ontological implications. Knowledge consists of conjectures about structural relations in the real world. The knowledge of the real world may be partial and incomplete. Theoretical knowledge, since it is disjointed, may be a basis for inferring distinct phases of existence. Concerning the phases and their interrelationships, skepticism may be the best strategy. If reality evolves at all, theoretical knowledge may lag behind such evolution. Again, skepticism may be appropriate.

Consideration of the problem of induction in recent history and philosophy of science raises the question of rationality. If reality is multi-dimensional and evolving, as suggested above, rational justification of all knowledge claims may be impractical and unattainable. Rationality as a nonjustificational process of criticism is both tenable and practical. Nonjustificational rationality means criticism can be directed at all levels of one's conceptual framework. This includes the concept of nonjustificational rationality itself. Types of criticism are internal coherence, realism, and empirical observation. Empirical observation is most effective when single theories are being assessed. Internal coherence and realism are effective criticisms at all levels of science--individual theories, research programs, and conceptual frameworks.

This brings us to the end of Part I, a consideration of recent philosophy of science and economic science. Mind and rationality are the two themes found in each of the previous chapters. Since logical positivists and positive economists consider mind to be a pseudo-problem, it is not surprising that their concepts of rationality do not imply that a decision-maker is conscious. In search of a better conception of rationality, we follow philosophy of science from Kuhn, to Popper, to Lakatos. A more adequate conception of rationality can be found in nonjustificational rationality as developed by Bartley. In nonjustificational rationality, evidence and information have a role to play in decision-making as well as logic and realism. Data is relevant but not decisive. Decision-making is a multi-dimensional, critical process.

Of even more direct importance for economics is the notion raised in chapter three, that an alternative philosophical foundation may be necessary to fully understand Keynes and the Keynesian Revolution. If Leijonhufvud is correct, that what is really at issue concerning Keynes and the Keynesian Revolution is the maximizing conception of rationality in positive economics; then the microfoundations of macroeconomics is also a question of an appropriate philosophical foundation for macroeconomics. Since our structural philosophy of science already incorporates a processive, nonjustificational conception of rationality; it could well serve as a viable philosophical foundation for the more processive and dynamic interpretations of Keynes. Thus, we hope what has been accomplished is more than a review of philosophy of science, and positive economics. We aim to pave the way for substantive progress in monetary and macroeconomics by rejecting the adequacy and relevance of the restrictive conception of economic science, positive economics, and by presenting a viable alternative, a structural view of science. Now we turn to a consideration of these issues in the context of the Keynesian Revolution and recent monetary theory, rational expectations in particular.

NOTES

[1]Popper (1959:52) states, "The view, according to which methodology is an empirical science in its turn--a study of an actual behavior of scientists, or the actual procedure of 'science'--may be described as 'naturalistic'. . . a naturalistic methodology." See also Lakatos (1970: 47).

For Popper and Lakatos, philosophy of science is both a theoretical and empirical study of the history of science. But it may not include psychological or sociological aspects of decision-making. Popper (1970: 58) says, "I regard the idea of turning to sociology or psychology as surprising." Lakatos (1970:179) concerning the same issue says, "The Kuhnian research programme contains a new feature; we have to study not the mind of the individual scientist but the mind of the scientific community. Individual psychology is now replaced by social psychology."

[2]This may be due to the increasingly hostile reception to Kuhn's psychological turn. Evidence of negative assessments of Kuhn are Popper (1970), Lakatos (1970), Blaug (1976), Leijonhufvud (1976), and other assessments by economists summarized in the preceding chapter.

[3]Since positivism has been such an influential view of science, most history of science (or economics or psychology) texts have been written from a positivistic perspective. Due to the fact that positivism is so restrictive, much history has been lost and must be retrieved (Weimer, 1974a).

[4]Concerning realism Maxwell (1970a:11) states, "Realism is true if and only if it is contingently true that the unobservable entities like those referred to by scientific theories exist and false if they do not. If it is false, then perhaps instrumentalism, or operationalism, or some other alternative is true." Also see note 35 below.

[5]Lakatos (1970:55) explicitly opposes the metaphysical level of analysis: "One must never allow a research programme to become a Weltanschaung."

[6]For example, Klein (1966 [1947 first edition]) and Joan Robinson (1962:73-98) speak of a Keynesian Revolution without reference to the leading views on philosophy of science.

[7]See the articles referenced in chapter three, pp. 53-55 and 63-70.

[8]See pp. 63-70 in the preceding chapter.

[9]Logical positivism could be viewed as the most simple, special-case view of science. In logical positivism, science is confined to the umproblematic assessment of individual theories, box no. 1 in Figure 2, p. 80.

[10]Masterman, as summarized previously in chapter three, pp. 51-52, showed that Kuhn's concept of a paradigm was ambiguous. She suggests narrowing the term to these three uses: metaphysical, sociological, and artifact paradigms. This means that the clearest distinction in Kuhn's (1970) work is between normal and revolutionary science, while various levels of abstract analysis are not clearly defined.

[11]For a discussion of the role of the hypothetico-deductive model in a structural view of science, see Maxwell (1970a:6-9 ; 1968:155). Hollis and Nell (1975:8-9) present a much more restrictive account of the hypothetico-deductive method in economics. From a structural point of view, this method is a way of making inferences about structural relations in the real world. For the positivist, the method is confined to the prediction of logical consequences.

[12]Rarely has the history of economic thought been considered a part of economic science. But from Figure 2, p. 80, the history of thought would be the field of economics which could specialize in integrating various isolated contributions to economic science and scrutinizing clashes of perspective at the metaphysical level. Thus, the history of economics must play a necessary role in economic science.

[13]Lakatos (1970:129) states, "In the pluralistic model the clash is not 'between theories and facts' but between two high-level theories: between an <u>interpretative theory</u> to provide the facts and an <u>explanatory theory</u> to explain them; and the interpretative theory may be on quite as high a level as the explanatory theory."

[14]Blaug (1976:164) views the post-war development of Keynesian economics as degenerating: "We gain a new insight into the post-war history of Keynesian economics, a history of steady 'degeneration' as the Keynesian prediction of chronic unemployment begins to lose its plausibility." Blaug sees the work of Friedman, Clower, Leijonhufvud, and others as attempts to remedy this degeneration.

[15]Friedman (1971 [1953]:46) states, "The weakest and least satisfactory part of current economic theory seems to me to be in the field of monetary dymanics." Friedman himself may have contributed to this weakness, since he proposed a widely accepted theory of consumption. Because Friedman's permanent income theory of consumption gives a static Walrasian explanation of the Keynesian consumption function, extension of the permanent income theory to monetary dynamics might be inappropriate. If Clower's remarks (note 18 of previous chapter, p. 74) are correct, a Walrasian approach essentially abstracts from genuine decision-making in a dynamic monetary economy.

[16]For example, compare Shackle (1972) and Lachman (1977) with the works of Lucas, Sargent, and Wallace (see reference list). One can only wonder if these economists all live in the same world. Shackle's views are summarized in chapter seven; those of Lucas, Sargent, and Wallace in chapters five and six.

Recently a review of rational expectations has appeared which claims to draw "on an 'Austrian' theory of expectations," (Kantor, 1979:1423). However, Kantor (1979:1429) goes on to recognize and apparently approve the reductionism inherent in rational expectations. This seems to be extremely positivistic in nature, and thus essentially non-Austrian.

[17]Weimer (1979:2) expresses the view: "What is the nature and source of the rational authority or criterion to which appeal is addressed to justify scientific inference as a source of knowledge?"

[18]Lakatos (1978b:3-4) says, "The controversy between dogmatists --who claim that we can know--and skeptics--who claim that we either cannot know or at least cannot know that we can know and when we can know--is the basic issue in epistemology."

[19]Empiricists have always had difficulty distinguishing causality from constant conjunction. For a realist, causality is a tentative conjecture about the real world. Maxwell (1970a:17) states, "Temporal succession, simultaneity, and causal connection must be counted among these structural properties, for it is by virtue of them that the unobservables interact with one another and observables." One might make a case that a structural view is necessary to talk about causality. Otherwise causality has no meaning.

[20]We have restated the problem of skepticism, which is generally attributed to David Hume. Until Popper, no one had given an adequate reply to Hume. However, Popper focused his attention on the growth of knowledge rather than its justification. Popper (1959:29-30) summarizes, "That inconsistencies may easily arise in connection with the principle of induction should have been clear from the work of Hume; also, that they can be avoided, if at all, only with difficulty. . . My own view is that the various difficulties of inductive logic here sketched are insurmountable. So also, I fear, are those inherent in the doctrine, so widely current today, that inductive inference, although not 'strictly valid,' can attain some degree of 'reliability' or of 'probability'. . . Like every other form of inductive logic, the logic of probable inference, or 'probability logic', leads either to an infinite regress, or to the doctrine of apriorism."

In a later work, Popper (1972:4-5) reviewed the difficulties of induction, and suggested that Hume had to take an irrational view of human knowledge:

"By these results Hume himself--one of the most rational minds ever--was turned into a sceptic and, at the same time, into a believer: a believer in an irrationalist epistemology. His result that repetition has no power whatever as an argument, although it dominates our cognitive life or our 'understanding,' led him to the conclusion that argument or reason plays only a minor role in our understanding. Our 'knowledge' is unmasked as being not only of the nature of belief, but of rationally indefensible belief--of an irrational faith." See Hume (1955[1748]) for his discussion of skepticism.

Popper (1972:1) maintains that he has solved Hume's problem: "I think that I have solved a major philosophical problem: the problem of induction."

[21]The limits-of-rationality argument may give the appearance of being a definitional or semantic issue. We maintain that the issue is semantic because it has to be semantic. Traditional views of rationality like Ayer's depend strongly on logic. Logical is a syntactical system. When a logical system breaks down, we have no choice but to use a semantic system which is open and not static. There seems to be no way of logically closing a logical system that allegedly applies to the real world. Thus, logic is part of the problem behind the limits-of-rationality issue.

[22]Notice the argumentative similarity of Ayer's statement with an attempted defense of Friedmanian positive economics by Lawrence Boland (1979:522): "Friedman's essay is an instrumentalist defense of instrumentalism. That may be interpreted to mean that Friedman's methodology is based on an infinite regress, but if it is then at least it is not internally inconsistent or otherwise illogical. His success is still open to question. The repeated attempts to refute Friedman's methodology have failed, I think, because instrumentalism is its own defense and its only defense." As with Ayer's statement, Boland's view seems to also suggest a limits-of-rationality defense of Friedman. If so, the criticisms of Ayer would also be a criticism of Boland's position.

[23]Ayer's defense of empiricism is the most sophisticated relative to those who preceded him. Simpler views of empiricism can be seen by focusing on the relation between theory and fact. Nineteenth-century empiricists (like J. S. Mill) did not clearly distinguish between theory and fact: they maintained that theories were inductive generalizations. Logical positivists separated theories and facts (see chapter two, p. 27); they maintained that facts can verify a theory. Logical empiricists, in response to Popper, maintained that facts can falsify a theory. Lastly, conventionalists and instrumentalists realized that facts are theoretically and conceptually contaminated; theories and facts are not independent. Contemporary, conventionalists and instrumentalists also seem to be aware of the limits-of-rationality argument. Compare Boland and Ayer's statements as referenced in the previous note.

[24]A recent case in economics may be the best example. Hollis and Nell (1975:3) dispute empiricism: "We shall uphold a theory of knowledge assigning a crucial role to a priori knowledge, which we take to belong to the Rationalist tradition." Hollis and Nell then dispute the conception of rationality in positive economics.

[25]See Popper's comment (note 20 above) that Hume's position on induction was essentially irrational. We do not believe that contemporary empiricists, particularly in economics, have advanced beyond Hume. Such empiricists seem to be trapped in a logical conception of

knowledge that is unnecessarily limited. Specifically, rationality as a logical, maximizing process seems unduly restrictive.

[26]The claim that rationality is subject to a logical limitation like the limits-of-rationality argument has led to skepticism and fideism. Skeptics and fideists are given rational excuses for irrationalism if the limits-of-rationality argument prevails. A skeptic or fideist could claim "that from a rational point of view, the choice between competing beliefs and positions and ways of life, whether scientific, mathematical, moral, religious metaphysical, political, or other, is arbitrary," (Bartley, 1964:5). For Popper's comments on skepticism and belief see note 20 above.

[27]Justificationist and nonjustificationist are terms used by: Lakatos (1970:94; 1978a:121), Weimer (1974b:368; 1977), Bartley (1962:139-146; 1964:23), and Popper (1963:225), to distinguish two metatheoretical approaches to rationality. Since economists have no analogous concepts, this terminology will be used in our present discussion.

[28]Popper (1963:4) says, "I shall try to show. . . that the differences between classical empiricism and rationalism are much smaller than their similarities, and that both are mistaken."

[29]Note that if both requirements are unwittingly maintained, that knowledge as traditionally conceived does not exist. In other words, the set of statements satisfying both requirements is a null or empty set. Once this result is realized, it leads to skepticism. See note 20, above.

Popper (1959:42) earlier had concluded that the dual nature of an empirical criterion of rationality would lead to inconsistency: "The root of this problem [induction] is the apparent contradiction between what may be called 'fundamental thesis of empiricism'--the thesis that experience alone can decide upon the truth or falsity of scientific statements--and Hume's realization of the inadmissibility of inductive arguments. This contradiction arises only if it is assumed that all empirical scientific statements must be 'conclusively decidable,' i.e. that their verification and their falsification must both in principle be possible [italics not in original]."

[30]Rather than following Bartley's interpretation so closely, we suggest another interpretation of his two requirements of rationality. In the first section of this chapter, we emphasized how important levels of analysis have become in philosophy of science. If we take requirements one and two as applying to two different theoretical levels of analysis, then inconsistency again arises within an empirical view of science.

To illustrate, let us take an economic example. Requirement one will be reformulated to address specific empirical hypotheses of Friedmanian monetarism, while requirement two will be reformulated to address monetarism as a theoretical system. Monetarism as an approach to economics will be inconsistent if the following two propositions hold:

1M. All monetarist hypotheses are falsifiable.

2M. Monetarism as a theoretical system is not falsifiable (or refutable).

Although Friedman does not distinguish levels of analysis in economics, he does maintain that economic hypotheses must be testable and that the realism of assumptions is not a test of a theory. We suggest that Friedman's positions on positive economics imply 1M and 2M. Of these two statements, only 2M is potentially controversial. We maintain that 2M is implied in Friedman's rejection of the assumptions of a theory as a test of the theory. Statement 2M is implied because the assumptions he has in mind and discusses, rationality (maximization) and perfect competition, are also basic assumptions of the monetarist theoretical system or research program in the sense of Lakatos. By rejecting realism as a criticism of monetarism, monetarism becomes inconsistent and irrefutable.

[31]Bartley (1962:158-159) suggests four critical ways to eliminate error: "the check of logic,"; "the check of sense observation,"; "the check of scientific theory,"; and "the check of the problem."

[32]The fact that observation is theoretical means that sense experience may also be theoretical. Knight, Bohm, and Hayek view sensation as being abstract:

"Sense perception itself is a capacity built up through communication between minds, without which we could never form the idea of our bodies as objects in a world of objects," (Knight,1925a:254).

"The notion of a thing is. . . an abstraction, in which it is conceptually separated from its infinite background and substructure," (Bohm,1957:146).

"The mind must be capable of performing abstracting operations in order to be able to perceive particulars," (Hayek,1969:311). See also Hayek (1952:52, 142-144). For a recent discussion of the issues which arise from the abstract nature of sensation, see Weimer (1976: 5-29). If sensation is abstract, sensation may have structural properties. See note 35, below.

[33]Bohm (1957:38-39) makes a similar argument for physics: "The philosophy of mechanism eventually came to be a very serious restriction on the further development of science," and for physicists, "their basic philosophical point of view was not really adequate for the understanding of nature as a whole."

[34]Feigl (1967:26) suggests realism is replacing positivism. Popper (1963:117) states, "Theories are our own inventions, our own ideas, they are not forced upon us. . . But some of our theories can clash with reality; and when they do, we know that there is a reality; that there is something to remind us of the fact that our ideas may be mistaken. And this is why the realist is right." Popper discusses realism in much more detail in, "Two Faces of Common Sense: An Argument for Common Sense Realism and Against the Common-sense Theory of Knowledge," and "A Realist View of Logic, Physics, and History." Both are found in Popper (1972:32-107, 285-318).

[35]The various types of realism are: naive, representative, and structural. Naive realism is the view that the real world is directly perceptible through the senses. If modern science is at all correct, naive realism is wrong. The world of science is very different from our perceptions. Representative realism is the view that our sense impressions resemble the real world. This may be correct for phenomena to which our senses are sensitive. But the resemblance of the external world to sensation may be due to the way the senses are structured. Thus, structural realism may be a more general way of considering realism. Structural realism is the view that scientific theories and our senses capture aspects of structure in the real world. See Maxwell (1968:151, 155-156).

[36]Hollis and Nell (1975:21) recognize an interdependence between epistemology and ontology, but they give priority to ontology: "A theory of knowledge is, in our view, a general argument for general claims about what there is."

[37]See note 13, above for Lakatos' view of epistemic pluralism. Observational and explanatory theories are one type of theoretical pluralism. Another type of theoretical pluralism is the proliferation of explanatory theories. Hopefully, such proliferation progressively increases in empirical content (Lakatos, 1970:121).

[38]Russell (1912:46-59) distinguishes knowledge by acquaintance from knowledge by description. Structural or theoretical knowledge is descriptive; common sense knowledge is by acquaintance. Theoretical knowledge starts from common sense knowledge but does not return to common sense knowledge. We are acquainted with the intrinsic properties of our own minds and only indirectly with the intrinsic properties of external, non-mental objects. Knowledge of external objects is indirect through structural properties (Weimer, 1977:140-143).

[39]See Maxwell's (1968) "Scientific Methodology and the Causal Theory of Perception."

[40]By ordinary matter, we mean sensory objects which obey Newton's laws of motion. We use the term quantum phenomena to describe the atomic and subatomic realms. We refer to quantum phenomena because the materiality of the objects around us apparently vanishes in the quantum context. In other words, the impact of quantum theory is to de-materialize matter.

[41]Empiricists are often accused of desiring a certain and indubitable foundation for knowledge based on sense experience. A hierarchy of knowledge fits very well with empiricism. However a realist realizes no absolute foundation for knowledge may exist. Popper (1959:111) states, "Science does not rest upon bedrock. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles." In chapter eight, a different concept than hierarchy or nesting is considered.

[42] Popper (1972:229) says, "What we want to understand is how such non-physical things as purposes, deliberation, plans, decisions, theories, intentions, and values, can play a part in bringing about physical changes in the physical world."

[43] Bohm (1957:137) speaks of "the qualitative infinity of nature." Popper (1963:144) says, "I am first of all an indeterminist, secondly a realist, thirdly a rationalist. . . I gladly admit--with Kant and other critical rationalists--that we cannot possess anything like full knowledge of the real world with its infinite richness and beauty."

PART II

RATIONAL EXPECTATIONS

Chapter 5

RATIONALITY, MONETARY THEORY, AND THE KEYNESIAN REVOLUTION

To this point, our major conclusion is that recent philosophy of science is really a debate over rationality and the scientific importance of mind. Our structural philosophy of science makes mind and rationality a central concern rather than a "pseudoproblem" as did the positivists. In addition, our exploration of philosophy of science in relation to the Keynesian Revolution raises another issue; positive economics and its maximizing concept of rationality may be too narrow for dynamic monetary phenomena. In effect, we believe conceptions of rationality, mind, and science are the central issues in economics since Keynes.

In part two of the thesis, the issues of mind and rationality are considered in the context of recent monetary theory. Monetary theory since Keynes seems to be in a state of continual upheaval suggesting unresolved issues remain at a fundamental level. The point of view taken in the following chapters is that mind and rationality are the unresolved issues which generate new approaches (microfoundations) to monetary problems. Monetary controversy is really a more specialized forum for the same types of intellectual dilemmas faced in philosophy of science.[1] The Keynesian Revolution may be largely unfinished because economists effectively ignore the nature of the issues. By restricting their attention to economic aspects of the Keynesian Revolution, economists, for the most part, really deal with the issues only at a surface level.

From the preceding comments, it should be apparent that a new way of relating philosophy of science to economics and the Keynesian Revolution is being advocated. Economists' discussions of the Keynesian Revolution and philosophy of science, for the most part, focus on economists and points of view in economics, like Keynesian and monetarist. This follows the debate in philosophy and physics on scientific revolutions. The question asked is whether the activities of economists during and after the Keynesian Revolution look at all like those of physicists during and after scientific revolutions. However, our approach penetrates to a deeper level. The view is this: Some transactors in a modern monetary economy may face the same degree of decision-making complexity as do practicing scientists.[2] Where decision-making complexity is found in the economy which rivals that of science, our conception of the capabilities of economic science must be modified. If economic science is altered to conform to our structural view of science, then the point of view of economic science needs to be changed. No longer can economic science be described as positive economics. Perhaps a more comprehensive view of economics, structural economics can be developed. This is the subject of part three.

In this chapter, we develop the view that the economic problem facing monetary theorists since Keynes is the problem of mind and rationality. Attention is directed away from the activity of practicing economists who participated in the Keynesian Revolution. This is not meant to deny that the concepts of rationality and mind in our structural view of science would be very helpful in interpreting the activities and points of view of professional economists. Rather, we focus on monetary theory in relation to the problem of decision-making in a complex monetary economy. It is suggested that a more complex view of decision-making, however, is in conflict with the accepted conception of rationality. Having to choose between Keynes' view of decision-making and rationality, economists in effect choose rationality. In light of our new conception of nonjustificational rationality, this restrictive and positivistic choice may not be necessary.

In the first section of this chapter, the behavioral conception of economic rationality is reviewed, followed by a formulation of a nonjustificational conception of rationality for economics. In the second section, the complexity and psychological disparity of human economic activity in the General Theory is assessed. In the third section, the attempts by economists to resolve the issues of the Keynesian Revolution purely with economic considerations are presented--the microfoundations debate. Then, in the last section, the most recent episode of economists' eschewing complex decision-making processes as a result of their restrictive conception of rationality is reviewed--the rational expectations hypothesis.

RATIONALITY AND THE DE-PSYCHOLOGIZING OF ECONOMIC ANALYSIS

Our argument that the most fundamental problems behind the Keynesian Revolution are mind and rationality may not be seen as all that innovative by some. Recent theoretical trends in economics, at first thought, might suggest that the profession is giving a great deal of attention to the consciousness of transactors. Macro-models are proliferating which recognize the importance of "expectations." Expectations are viewed by many economists as being crucially relevant to the major economic problems now facing the Western economic community--simultaneous inflation and unemployment. Rational expectations models supposedly more readily capture the dynamic aspects of a rapidly adapting economy. However, we must ask what it means to talk about rational expectations, both in terms of rationality and in terms of actual expectations.

In the following section, rationality and expectations are considered. The view developed is that economic rationality is both too narrow and too broad. The implicit conceptions of mind and rationality in the economist's conception of rationality are too narrow, while this narrow conceptual scope of rational economic activity is often applied too broadly. The concept of rationality is too narrow because it frees economics from any dependence on psychology, either a psychology of behavior or a psychology of decision-making and expectations.[3] The concept of rationality is applied too broadly, because all rational human activity would seem to be economic. The positive economist borders on making economics the only scientifically viable approach to human activity.

In contrast to recent concern for the economic significance of expectations and decision-making, turn-of-the-century theorists and some who followed were concerned with making economic science independent of any explicit psychological content. Hedonism as a theory of economic behavior was the source of difficulty for them. Previously, several of the originators of neoclassical micro theory had based their theory of value on hedonism. Economics was formulated, in their view, to analyze economic behavior as maximizing pleasure while minimizing pain. This explicit hedonism of Jevons (1957:23), Edgeworth (1881: 13-15), and others created a sense of uneasiness with later generations of economists.[4] Fisher and Pareto and then Hicks and Samuelson rejected the hedonistic interpretations of neoclassical value theory.[5] In its place was substituted a logical theory of choice.[6] Attention was directed increasingly to the logical properties of a well-ordered bundle of consumer choices: strong and weak ordering, transitivity, integrability, and consistency. Hedonistic utility maximization was reinterpreted as a consistent ranking of consumer preferences, rather than a description of a psychological process which is measurable and interpersonally comparable. In short, "the utility theory of value has a much better claim to being called a logic than a psychology of values," (Schumpeter, 1954:1058).

However, when it comes to rationality, many economists seem to exhibit some confusion concerning the interpretation of the logical theory of consumer behavior. The logical theory of the consumer may be interpreted as a theory of rationality for externally observed transactor behavior or it may be interpreted as an inferred description of the transactor's internal state of mind concerning expectations and choice. To add to the confusion, a hypothetical view of the individual transactor is developed--economic man. This hypothetical transactor both behaves and thinks rationally. Rational economic man is free of emotions and actual, individual circumstances which might make him respond (choose?) in "irrational" ways.[7] Rational economic man is assumed to have stable preferences and to be an optimizing calculator of subjective utility or profits. Economic man is always rational because his behavior always conforms to the logic of his economic situation as perceived by an external observer. For economic man, rational choice and rational behavior are identical concepts. One of these conceptions, rational behavior or rational choice, seems to be redundant and needs to be eliminated.

The conflation of rational behavior with rational choice is manifest in the portrayals of rationality by economists. For example, Oscar Lange (1945:46-30) seems to make no distinction, methodologically or philosophically between rational intent and rational behavior:

> A unit of economic decision is said to act rationally when its objective is the maximization of a magnitude. . . The postulate of rationality is the assumption that all units of economic decision act rationally. This assumption provides us with a most powerful tool for simplification of theoretical analysis. For if a unit of decision acts rationally, its decisions in any given situation can be predicted by mere application of the rules of logic (and of mathematics).

Many years later, H. A. J. Green's (1971:22-24) view is not all that different:

> An assumption which pervades the theories of consumer behavior. . . is that consumers behave rationally. . . To the economist rational behavior is behavior in accordance with a systematic set of preferences.
>
> . . . .
>
> Rational behavior is easiest to think of. . . negatively. It would not be rational to choose x from a set of alternatives if there were another alternative x' in the set which is preferred to x.

That Lange and Green both interpret rationality as a logic of both choice and behavior illustrates how difficult it is to remain within the boundaries of positive economics. Lange gives his interpretation an element of non-positivistic realism by adding a dimension of purpose or intention. A purposeful objective requires conscious awareness of maximizing on the part of the transactor. Similarly, Green initially interprets rationality as systematic behavior; then he switches to a discussion of rationality in terms of choice.

But interpreting rationality as applying both to choice and to behavior conflicts with Friedmanian positive economics. Mental intent, in his view, is redundant with the empirical evidence relating to maximizing behavior.[8] For Friedman, the element of realism which is irrelevant to the postulate of rationality is the question of mental intent or self-conscious maximization on the part of "rational" transactors. Apparently, in Friedman's view, the postulate of rationality need not imply anything about the conscious state of mind for economic transactors. The de-emphasis of the conscious state of mind fits very well with logical positivism. As mentioned previously, the anti-psychological posture of positive economics may be the most consistently positive aspect of positive economics. The descriptions of rationality in terms of objectives and choice are pseudo-realistic and tangental to the positive economist's task. Thus, the postulate of rationality, having evolved out of economists' attempts to de-psychologize economic behavior by replacing hedonism with logic, also frees economics from a realistic psychology of decision-making and expectations.[9]

Obviously the preceding interpretation of positive economics is extremely restrictive. A de-psychologized economic science as the study of rational behavior is too narrow, when as a study of behavior, it essentially abstracts from decision-making processes and expectations. If the relationship between behavior, decision-making, and expectations is not uniform or one-to-one, then the behavioral approach may break down. Transactors are quite capable under certain circumstances of continually changing their minds. A rapidly changing economic environment may require constant reassessment of economic information and theories in order to minimize potential losses. From a behavioral perspective, the processes of decision-making and reformulating expectations are apparently irrational. Only if transactors stop changing their expectations about the economic situation and turn their attention to other activities will economic affairs stabilize enough to be again amenable to a behavioral approach.[10] Thus, a behavioral approach is too narrow because complex decision-making processes and expectations are excluded from positive economic science.

A de-psychologized positive economic science, despite being too narrow, may simultaneously be too broad. If economics is coextensive with all logically rational human behavior, then the scope of economics is largely unrestricted. Frank Knight (1930:22) expresses the view: "The general theory of economics is therefore the rationale of life--in so far as it has any rationale!"[11] What this means is that economics may be the only legitimate social science. The only domain of human behavior left to other social sciences is, by definition, irrational human behavior. Other social scientists, like economists, are extremely reluctant to call any activity, which studies irrational behavior, a science. Therefore, with this view of rationality, economics becomes the only possible conception of a social science. That economics has no limits to its subject matter is expressed by Robbins (1935:16-17):

> The conception we have rejected, the conception of Economics as the study of the cause of material welfare, was what may be called a classificatory conception. . . The conception we have adopted may be described as analytical. It does not attempt to pick out certain kinds of behavior, but focuses attention on a particular aspect of behavior, the form imposed by the influence of scarcity. It follows from this, therefore, that in so far as it presents this aspect, any kind of human behavior falls within the scope of economic generalizations. 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. There are no limitations on the subject-matter of Economic Science save this.

To avoid the restrictiveness of positive economics, what is needed at this point is a more adequate conception of rationality than economic rationality. A better conception of rationality would avoid applying a narrow conception of rationality too broadly. In the previous chapter, a nonjustificational view of rationality as a multi-dimensional critical process is developed from our discussion of decision-making in science. If some economic transactors are as sophisticated in their decision-making as practicing scientists, then nonjustificational rationality may serve as a general conception of rationality. There is no reason to believe that some economic transactors would not go through a process of decision-making similar to the nonjustificational modes of rational criticism. A nonjustificational conception of economic rationality is found in Figure 5.[12] The business world is certainly concerned with realism since its problem orientation may be one of its strongest characteristics. Also, due to increasing professionalization of management, modern managers in their education are taught how to use information and various modes of formal analysis in their decision-making. Business education is usually interdisciplinary which potentially makes it

	Type of Decision		
Level of Analysis	Informational	Analytical	Problematical
Simple Hypotheses	Is information available which implies a rejection of this project?	Are profits and present value being maximized for this project?	Is this project a solution to a genuine real world problem, economic or otherwise?
Theoretical Systems	Are the informational concepts relevant to the analytical concepts which are used to analyze the project?	Is there more than one mode of analysis in economics or otherwise which is relevant to this project?	Is the problem behind this project likely to generate alternative theoretical positions in economics and outside of economics?
Conceptual Framework	Is information, in principle, available given the role of uncertainty in my conceptual framework?	Are there limits to maximization and other modes of analysis, given my conceptual framework?	Is this problem a genuine problem in the analysis and in my conceptual framework?

Figure 5 A Nonjustificational Conception of Economic Rationality Resulting from a Nonjustificational Conception of Rationality in Philosophy of Science and Indicating Maximization as a Special Case

theoretically pluralistic. Thus, contemporary business education would seem to be theoretically pluralistic and incorporate all the arguments of nonjustificational rationality: real and logical as well as empirical evidence and information.[13]

To restate, economic rationality as maximization is intertwined with the psychological and philosophical foundations of economic science. Economic rationality can be criticized for being psychologically unrealistic and for being philosophically and methodologically too narrow and too broad. Psychologically, economic rationality is doubly reductionistic; it frees economics from both behavioral and cognitive psychology. Philosophically and methodologically, economic rationality makes this narrow conception of economics the only scientifically viable social science. For the positive economist, who emphasizes prediction rather than realism and the simplicity, elegance, and breadth of neoclassical economics, our criticisms may not have much force. But there are costs associated with this extremely positivistic point of view. Rationality becomes a specialized scientific term which describes externally observable change in the natural world.[14] Rationality becomes nothing more than other descriptions of naturalistic properties like: mass, hardness, solubility, durability, pliability, measurability. From a human point of view, such a concept of rationality is largely sterile and relevant only to the most repetitive economic situations. But our more realistic and general conception of rationality, like a nonjustificational conception of economic rationality, goes a long way toward resolving the sterility and narrowness of an economic science based on a maximizing concept of rationality. Thus, a nonjustificational view of economic rationality, as found in Figure 5, becomes, for us, a benchmark conception of rationality which is a superior alternative to rationality as maximization.

THE DUALISTIC NATURE OF KEYNES' GENERAL THEORY

From the philosophical and psychological restrictiveness of positive economics and economic rationality comes a significant implication for the development of economic theory. To the extent that dynamic, non-repetitive economic situations requiring a high level of intelligent decision-making capacity influence the timing and magnitude of economic events, such dynamic events lie beyond the purview of positive, logico-behavioral economics. Thus, by definition, monetary phenomena may be outside the domain of positive economics. Nowhere is this more apparent than in economists' assessments of Keynes and their attempts to render the General Theory scientific in the positivist sense.

In the following paragraphs, the psychological foundation of the General Theory is considered. Keynes seems to be tacitly aware of restrictive notions of psychology and rationality in economics. Yet, he did not let a restrictive conception of economic science with respect to rationality and psychology prevent him from developing his theory. Consequently, Keynes' theory exhibits a high degree of psychological realism. But this greater psychological realism is achieved by making his point of view dualistic. His arguments are remarkably complex in their treatment of economic events; while his theory is divided into an equilibrium model and factors which alter equilibrium. Our task at this point is to more fully understand the dualistic conceptual structure of the General Theory as a strategy accommodating a more complex psychology of decision-making.

In contrast to Fisher, Hicks, Samuelson and others, Keynes appears to have no inhibitions about attributing psychological characteristics to individuals, nor addressing human nature in general.[15] Although the theorists mentioned above are concerned with developing a logical theory to free economics of any psychological reference; Keynes makes many references to psychological arguments, both behavioral and cognitive. Behaviorally, the best known reference to psychology by Keynes (1936:114) is the marginal propensity to consume as "our normal psychological law." "The psychology of the community is such that when aggregate real income is increased aggregate consumption is increased, but not by so much as income," (Keynes, 1936:27). However, the extent of Keynes' reliance on psychological propensities is not limited to consumption, nor to behavior extending to financial investment decisions. The title of chapter 15, "The Psychological and Business Incentives to Liquidity," indicates how extensively Keynes uses psychological arguments.

Psychological arguments are also evident in Keynes' discussion of money. Keynes examines three motives for holding money: transactions, precautionary, and speculative motives. The first two motives are largely behavioral, while the last requires a large element of cognition. The transactions and precautionary motives are related to the level of economic activity. The speculative motive is significant because it is the avenue through which the central bank may aim to control the economy (Keynes, 1936:196). Although a change in the level of interest rates may be obtained through open market operations, speculative liquidity preference is highly volatile due to changes in expectations. Expectations can change rapidly as a result of a change in the 'news.' Expectations also influence the marginal efficiency of capital which implies "somewhat violent fluctuations of the Trade Cycle," (Keynes, 1936:144).

Towards the end of the General Theory (1936:245-254), Keynes briefly restates his theory. He separates his theory into independent and dependent variables. Dependent variables are employment and national income; independent variables are the marginal propensity to consume, the marginal efficiency of capital, and the rate of interest. However, there are "ultimate independent variables" (Keynes, 1936:246) which go beyond independent variables of "the first instance," (Keynes, 1936:245). One category of ultimate independent variables are psychological factors: They are the propensity to consume, liquidity attitudes, and capital asset expectations concerning the future. The two remaining categories are the wage bargain and central bank action in relation to the quantity of money. These ultimate independent variables are of most importance for they are the ultimate source of dynamism in the economy.

Since forty years have passes since the General Theory was published, macroeconomic theory is now more sophisticated and complex. Figure 6 outlines in a non-formal way the essential variables of a simple macroeconomic model similar to Keynes'. Dependent variables are national income, the rate of interest, employment, wages and prices. Independent variables are consumption, investment, and government spending, money supply and demand, labor supply and demand, and

Determinants of GNP

	Aspects of Model	Determinants of Aggregate Demand[a]					Determinants of Aggregate Supply[a]		
UPPER	Structural Relation	C Function	I Function		Transmission Mechanism		Production Function	Labor Supply	Labor Demand
UPPER	Dependent Variables	y,r			M,r,P		y,N,W, P		
UPPER	Independent Variables	C	I	G	Md	Ms	f(K,N)	Ns	Nd
LOWER	Ultimate Independent Variables (UIV)	MPC	MEC	EXOG	Liq. Pref.	EXOG	EXOG	Wage Bargain	
LOWER	Type of Explanation Behind UIV's	Psy	Psy	Inst	Psy	Inst	Psy	Psy and Inst	
LOWER	Expectations Affecting UIV's	Y	K	Pol. Endog	r,P	Pol. Endog	Inventive Activity	Contracts and Job Search	

Figure 6 A Simple Keynesian Macro Model Constructed following Chapter Eighteen of Keynes' <u>General Theory</u>, "The General Theory of Employment Re-stated"

[a]For a list of variables and abbreviations, see next page

Variables:

C	Consumption
I	Investment
G	Government Spending
y	Income
r	Interest
M	Money, Supply and Demand
P	Prices
f	Production Function
K	Capital
N	Employment
W	Wages

Abbreviations:

MEC	Marginal Efficiency of Capital
MPC	Marginal Propensity to Consume
PSY	Psychological Factors
POL	Political Factors
INST	Institutional Factors
EXOG	Exogenous to the Equilibrium Model
ENDOG	Endogenous to the Equilibrium Model

Figure 6, continued

technology. Following Keynes' restatement of his theory, ultimate independent factors can be distinguished which provide the dynamism for the independent variables. These ultimate factors can be categorized as institutional or psychological. The institutional variables, for the most part, are politically endogenous or contractually determined. The psychological factors (except for the MPC) normally are denoted as expectations.[16] Although most economists make them analytically exogenous, expectations are endogenous to real world transactors.

The purpose of presenting a simple macro schema is to suggest that Keynes' macroeconomics makes no sense unless institutional and complex psychological factors about the real world are considered. Using a dualistic conceptual scheme, Keynes appears to make those factors most significant, which professional economists ignore or take as given.[17] The structure of the General Theory appears to have been formulated with complex psychological and institutional factors in mind. The usual strategy of a model builder is to isolate those patterns of behavior, individually or in the aggregate, which are most repetitive and stable. Keynes' theory isolates and magnifies nonrepetitive, instutional and psychological sources of potential economic instability. The greater generality of premises which Keynes (1936:v) desires must be these ultimate sources of explanation which earlier theorists did not address. Otherwise, Keynes is at the mercy of the logical consistency of positive, neoclassical theorists, meaning that Keynes' concern with sources of dynamic change repeatedly is abandoned. To restate the view, Keynes' preoccupation and point of departure seems to be dynamic change, not static equilibrium. His choice of variables may result from his desire to structure a model which peculiarly emphasizes and is sensitive to cognitive and institutional sources of dynamism in the real world. Without a consideration of these sources of dynamics, the model loses its character.

In the hands of his interpreters and systemizers, the theoretical scheme presented by Keynes is modified. Attention is directed away from the ultimate independent variables to a comparative static model of economic equilibrium. Concern with psychological and institutional sources of instability is replaced with concern for theoretical detail. Theorists turn their attention to the multiplier, the consumption function, and then to the details of a Keynesian theory of monetary and fiscal policy. Increasingly, economic mechanisms as relative-price-adjusting-processes preoccupy theorists. The transmission mechanism of monetary policy is a case in point (Park, 1972). In short, the view of a monetary economy found in the General Theory is converted into national income analysis. The central idea of Keynesian income analysis is managing aggregate demand to attain full-employment. The indeterminacy suggested by Keynes' awareness of ultimate independent variables is not a significant component of Keynesian economics. Hicks (1976 [1937]), Hansen (1953), and Samuelson are largely responsible for the development of Keynesian income analysis as a comparative static theory of macroeconomic equilibrium. In his recent monograph, Money and the Real World, Davidson (1972:1) expresses this same view:

> Despite this apparent victory of Keynes's ideas and philosophy, a small but growing group of economists have continued to warn that what passes for 'Keynesian' economics is nothing but pre-keynesian simplicities camouflaged with some Keynesian cosmetic terminology. For those who were unaware of this small but important literature, the appear-

> ance of Leijonhufvud's book [On Keynesian Economics and the Economics of Keynes] emphasising the dichotomy between the economics of J. M. Keynes and the accepted Keynesian economics must have been a shocking experience.

In summary, the theory of a monetary economy found in the General Theory is inherently dualistic. In contrast to the logical theory of micro behavior, Keynes presents a "macro" theory which has independent variables sensitive to more ultimate independent variables--complex psychological and institutional processes. This strategy constitutes a dualistic conceptual framework which separates static and dynamic concerns. The model appears static and analytical, an equilibrium model; while the explanations seem dynamic, though somewhat casual. From a more recent perspective, Keynes' explanations appear to be ad hoc and unsystematic; but they permit a more realistic consideration of cognitive complexity facing transactors in a modern monetary economy. But such realism has one drawback. Although the dualistic point of view in Keynes' work permits greater psychological complexity and realism, in monetary economics it provides the opportunity for such psychological and institutional arguments to be abandoned. Psychological and institutional factors are central to the argument but not to the static national income model. As a consequence, a profession in the process of getting rid of hedonistic foundations of value theory, can hardly be expected to develop the less formal, dynamic aspects of the General Theory, which conflict with the accepted conception of rationality. Thus, Keynesian economics evolved into a static, equilibrium model of income, output, and employment.

THE MICROFOUNDATIONS DEBATE

If one puruses much of the theoretical literature in economics since Keynes, the major issues are hardly mind and rationality. Keynesians are preoccupied with many of the theoretical details of the equilibrium macro model, while monetarists and neo-Keynesians pursue alternative interpretations of Keynes. Most importantly, the controversies among economists as economists are largely economic questions rather than philosophical, psychological, or methodological. Broadly construed, the economic questions concern the microfoundation compatible with Keynes' or a Keynesian conception of the economy.[18] Monetarists emphasize a microfoundation based almost exclusively on the asset function of money; this microfoundation operates via the rather mechanical real balance effect. Neo-Keynesians give more emphasis to a microfoundation based on the medium of exchange function of money; this microfoundation emphasizes the complexity of a monetary transactions structure.[19] In the following section, the microfoundations issue is explored in more detail. Our purpose is to demonstrate what great lengths economists have gone to in order to avoid a consideration of the issues of mind and rationality. Additionally, consideration of the microfoundations question provides needed background for Part III. We begin first with the monetarist interpretation of Keynes, Patinkin's Money, Interest, and Prices. Then the neo-Keynesian work of Clower and Leijonhufvud is considered.

The Monetarist Counter-revolution

Economists concerned with the General Theory apparently ignore the dilemmas posed by Keynes concerning psychology and rationality for a very good reason. The General Theory is a difficult book, even when the issues are confined purely to economics. Keynes tries to do many things. Theoretically, Keynes attempts to formulate a more general theory of aggregate output, employment, and prices than classical or early neoclassical theory.[20] Doctrinally, Keynes (1936:18-21) wants to demonstrate the inapplicability of Say's Law to a monetary economy; that a market economy is not inherently self-adjusting toward full employment.[21] Furthermore, Keynes wants to transcend the theoretical dichotomy between monetary and value theory by formulating a macromodel which is sensitive to the dynamics of a monetary economy.[22] With so many innovative economic contributions packed into one volume, one begins to understand the difficulties facing economists in assimilating even the economic aspects of Keynes' work.

The Keynesian interpretation of Keynes largely ignores the questions of rationality, psychology, and institutions found in the General Theory. Focusing on economic concerns, Keynesians develop the simple, comparative static national income model which emphasizes the market concepts of aggregate supply and demand. Theoretically, the major point of the simple Keynesian model is that macro equilibrium is determined at the intersection of aggregate demand and supply. Only one set of real variables (income, output, wages, real money balances, and interest rates) are compatible with full-employment-equilibrium. Full-employment-equilibrium is just one of many possible equilibrium situations. Theoretically, it is a special case of an infinite number of potential equilibrium positions. Furthermore, at an equilibrium position below full employment, there may be no tendency for the economy to move towards full employment. Thus, Say's Law as a tendency toward full-employment-equilibrium, is invalid. Keynes' views are the more general theory.

However, it was not long before the Keynesian interpretation of Keynes was criticized. The Keynesian interpretation was challenged on economic grounds and not with respect to psychological complexity, rationality, or a view of institutions. Theoretically, the major challenge came from Patinkin's attempted integration of monetary and value theory. An adjusting mechanism for the economy which would keep the economy moving toward full employment was perhaps Patinkin's major contribution. Theoretically, if this adjusting mechanism were correct, then Keynes' General Theory would not be very general. Keynes' theory in this view, was a special-case theory addressed to a unique historical situation, the Great Depression. Historically, the uniqueness of the Great Depression was the extraordinary amount of time the economy required to automatically adjust toward full employment.

To demonstrate Patinkin's claim that the Keynesian interpretation of Keynes is theoretically invalid, takes us deeper into economic issues and further away from the issues of rationality and psychological complexity. But this is precisely the point we want to make. The economic issues escalate in complexity apparently without coming much closer to a resolution of the issues. Patinkin, like Hicks and Samuelson, are part of a different analytical tradition than Keynes. Patinkin's conceptual and analytical roots are Walrasian rather than Marshallian.[23]

In the late nineteenth century, Alfred Marshall was the dominant economist at Cambridge, significantly influencing both Keynes and his mentors. Marshall's French contemporary was Leon Walras, who spent most of his professional life in Switzerland. Walras was a general equilibrium theorist rather than a partial equilibrium theorist like Marshall. It was Walras (1954 [1926]:315-337) who first attempted to systematically account for money in a general equilibrium framework.

A general equilibrium model is a model in which all markets are represented; not just isolated markets or certain sectors of the economy. A simple general equilibrium model with m-1 commodities and money might be represented as follows:

$$D_1 = d_1\,(1/(P_1/P_n)) \qquad S_1 = s_1\,(P_1/P_n)$$

$$\vdots \qquad\qquad \vdots$$

$$D_{m-1} = d_{m-1}\,(1/(P_{m-1}/P_n)) \qquad S_{m-1} = s_{m-1}\,(P_{m-1}/P_n)$$

$$D_m = d_m\,(1/(P_m/P_n)) \qquad S_m = s_m\,(P_m/P_n)$$

where n denotes a numeraire and m is fiat money. A numeraire is a commodity in which all other goods may be valued rather than money. Any of the m-1 commodities could serve as the numeraire.

To consider money in his general equilibrium framework, Walras (somewhat like Keynes) divides his conceptual point of view into two stages. This two-stage sequence is a hypothetical double tatonnement process.[24] As with Keynes, the first stage, a static equilibrium model, is most consistent with the general development of static economics; while the second stage is an attempt to make the point of view dynamic.

The first tatonnement is essentially barter exchange and is confined to the m-1 commodities. They are all valued in terms of the numeraire. With the help of an auctioneer, relative prices (barter ratios) are determined which clear all commodity markets. Trade is not permitted to take place until such relative prices clearing all commodity markets are obtained. Apparently the auctioneer has a degree of omniscience not possessed by human transactors.[25] Fiat money is included in Walras' general equilibrium model in the second tatonnement. Fiat money is not a commodity but a paper claim to commodities. In the second process of establishing prices, prices are altered from the first round. Thus money, if it is held, influences various markets by affecting the prices in each market. Money prices appear to be whole number multiples of numeraire or barter prices resulting from the first tatonnement.

However, one must consider whether money prices are merely whole-number multiples of barter prices. If relative prices do not change, then both stages of adjustment would be static with money being redundant with the already-attained equilibrium. But Walras (1954 [1926]:327) maintains that in the real world, money transcends this minimal role. Money has an impact on real commodity markets. Money may actually distort the relative price ratios determined in the first tatonnement. The process of adjusting to money requires "a general process of adjustment by groping in order to be sure of reaching equilibrium," (Walras, 1954 [1926]:327).[26] Partial and general disequilibrium may be a possibility. To preclude general disequilibrium in this context, an equilibrium condition is needed.[27] A Walrasian economy with money would be in equilibrium if each commodity excess demand equals zero and money excess demand equals zero.[28] This equilibrium condition is denoted by later economists as Walras' Law. Mathematically, Walras' Law is an equilibrium condition, which if fulfilled would imply a stable, equilibrium economy. Mathematically, it is expressed as follows:

$$\sum_{i=1}^{m} P_i S_i = \sum_{i=1}^{m} P_i D_i$$

where D_i and S_i are the quantities demanded and supplied.[29]

Patinkin's model is a small, Walrasian general equilibrium model. His aim is to integrate monetary and value theory with the real balance effect and thereby demonstrate the special-case nature of less than full employment equilibrium which is so important in the General Theory. Real balances are price deflated money balances. A real balance effect operates in each of the aggregated macro markets: commodities, bonds, money, and labor. When one of the markets is disturbed, it is the real balance effect that connects all markets and leads to adjustment back to full employment equilibrium. Thus, the real balance effect is a self-correcting market mechanism which gives economic sense to Say's or Walras' Law.[30] In his interpretation of Keynes, Patinkin first concentrates on aggregate demand by assuming full employment; then he concentrates on a model with unemployment. In both models, a real balance effect exists which returns aggregate demand to the level needed for full employment equilibrium. The unemployment model will be the subject of our subsequent attention.

Patinkin's unemployment model is a general equilibrium model, usually written as a series of equilibrium conditions:[31]

$D_G = S_G$ (Goods)

$D_B = S_B$ (Bonds)

$D_M = S_M$ (Money)

$D_N = S_N$ (Labor)

The goods market includes both consumption and investment commodities; while the bond and money markets are separated due to the attention Keynes gives to bond-money transactions. The hypothetical, conceptual test which Patinkin develops to assess Keynes' work is a decline in aggregate demand. If, in the model, a decline in aggregate demand is not self-adjusting, then Keynes was correct; otherwise his General Theory is the special case of a Patinkin-type economy.

In a Patinkin-type economy with the real balance effect, an autonomous or exogenous decline in aggregate demand is only temporary. Over some period of time the economy adjusts back toward full employment.[32] This means unemployment occurs during the transition period--the time that it takes for full employment to be restored. The initial decline in aggregate demand results from reduced consumption or investment spending. Reduced consumer or investment spending affects the labor market as production is reduced and unemployment rises. Thus, an excess supply of goods appears in the goods market and an excess supply of labor appears in the labor market. This is definitely a Keynes-type situation where inadequate aggregate demand is pervasive. If wages and prices are flexible, this situation of disequilibrium cannot last. Wage and price adjustments tend to eliminate market disequilibrium. The nominal wage rate begins to fall. Falling commodity prices raise the purchasing power of nominal money balances and tend to reduce the interest rate. These effects stimulate aggregate demand. Falling nominal wages adjust the labor market by increasing the quantity of labor demanded. Labor market adjustment ceases when falling wages and prices bring the labor market back to full-employment-equilibrium at the original real wage. Contrary to Keynes, Patinkin alleges that in a macro model with the real balance effect and flexible wages and prices, involuntary unemployment is subject to automatic forces which eliminate such unemployment (Patinkin, 1965:324-325).

However, an approximation to a prolonged, Keynes-type period of involuntary unemployment (like the Depression) can be set forth. If either wages or prices are downwardly inflexible, then the Patinkin-type economy reaches a state of rest below full employment. But rather than being a less-than-full-employment-equilibrium, the situation is one of disequilibrium. Nominal price rigidity implies goods-market disequilibrium (Patinkin, 1965:327-328). But Patinkin denies that nominal wage and price inflexibility is the essence of Keynes' position. Prolonged involuntary employment may result if the stimulating effects of the real balance effect are too little and too late. The dynamic adjustment processes due to flexible wages and prices take "an intolerably long period" of time to work (Patinkin, 1965:339). The economics of Keynes is the economics of unemployment disequilibrium which results from a different type of rigidity than wage-price inflexibility. Autonomous investors and consumers may slow down actual market adjustment. Patinkin (1965:343) summarizes:[33]

> Nevertheless our theory does depend on rigidities. For, by definition, any system which fails to respond quickly and smoothly to equilibrating market forces is suffering from rigidities. But the offending rigidities are not those of extraneous monopolistic elements interfering with the otherwise smooth functioning of a capitalist economy, but those

> inherent in the very fact that the level of aggregate commodity demand in such an economy is the resultant of individual decisions to consume and to invest, and that these decisions respond only "stickily" to market changes in interest and prices. They are the rigidities of sovereign consumers and investors unwilling to modify their expenditure habits on short notice.

Summarizing, Keynesians and monetarists increasingly give less and less attention to the dualistic conceptual structure of the General Theory. While Keynesians develop the static national income model, Patinkin develops an alternative perspective from which Keynes may be viewed as the special case: Theoretically, the General Theory is concerned with the special case of the real balance effect taking too much time to take the economy towards full employment. Less-than-full-employment-equilibrium, at best, is a theoretical misstatement of a prolonged period of dynamic mal-adjustment. Our only criticism of Keynesians and Patinkin is that only half of Keynes' point of view has been considered. Keynes' more ultimate independent variables, which defy accepted notions of rationality and the institutional and psychological foundations of economic science, are not considered. This may in part be due to the restrictiveness of positive economic science. Failure to confront the whole conceptual structure of the General Theory makes points of view like Patinkin's less than relevant to what Keynes had to say.

The Neo-Keynesian Counter-revolution

Although one suspects that neo-Keynesians might return to the dualistic conceptual structure of the General-Theory, this is not wholly the case. The emphasis of Keynes on psychological complexity and expectations that defies the maximizing conception of rationality are not emphasized by neo-Keynesians any more than by monetarists.[34] Rather, Patinkin's work provokes a reappraisal of Keynesian economics. Thus, Patinkin as much as Keynes is an intellectual point of departure for neo-Keynesian economics. Neo-Keynesian economists contend that a Patinkin-type economy is an inappropriate theoretical framework in which to assess the importance of Keynes' theory. In contrast to Patinkin, who builds his theory on the real balance effect, neo-Keynesians emphasize another function of money as a microfoundation for macroeconomics.[35]

Neo-Keynesians, particularly Clower and to a lesser degree Leijonhufvud, develop a microfoundation emphasizing the medium of exchange function of money.[36] This microfoundation can be seen as a continuing concern for issues mostly economic in nature. In a very real sense, some neo-Keynesians may not be much closer to Keynes than Patinkin and other monetarists.[37] In other words, the neo-Keynesian point of view comes very close to confining the content of the General Theory to the static equilibrium issues of the upper part of Figure 6.

In stressing the medium of exchange function of money, neo-Keynesian theorists increasingly analyze the transactions structure of the economy.[38] Concern with the transactions structure of the economy seems inherently more institutional in orientation than Patinkin's approach. Thus, with respect to institutional detail, neo-Keynesians may exhibit their most Keynesian characteristic. A monetary economy requires money to make transactions effective (Leijonhufvud, 1969: 44-45). If demands cannot be placed with money or monetized, then planned (notional) demands may differ from effective demands. Notional but ineffective demand may be the source of the Keynesian features of a monetary economy like pervasive unemployment.

The distinction between effective and notional demands is developed by Clower (1965) as a critique of a Patinkin-type economy. Economic models like Patinkin's essentially assume that notional demands are the relevant demands. There is no reason to suspect that anticipated income for the current period is realized. If realized incomes are less than expected incomes and borrowing opportunities are exhausted, then realized demands are less than expected demands. If notional demands cannot be effected, excess supply may appear in various markets. Ineffective aggregate demand may result; this is a typical Keynesian situation.

The process of sequential exchange in a monetary economy is responsible for the Keynesian features of an economy; ineffective aggregate demand and sluggish wage-price adjustment. In a monetary economy, the offer to work must be understood as a demand for money. The reservation wage of the worker is related to his notional demands. His reservation wage must cover his notional demands, otherwise he will remain unemployed as long as he can finance his purchases (Leijonhufvud, 1969:43-44). Furthermore, the individual laborer's demand for a job and money in no way guarantees an increase in final product demand to the individual employer. A monetary exchange system permits the worker to spend his income on goods not produced by his own employment. Thus, the demand for money by workers seeking jobs does not constitute a demand for products produced by potential employers. In short, the supply of labor does not create its own demand in a system of monetary exchange.

The distinction between realized and notional demands is extended to a Patinkin-type general equilibrium model by Axel Leijonhufvud (1968:86-91). Leijonhufvud uses a four market model (goods, bonds, money, and labor) which is quite similar to Patinkin's. And like Keynes and Patinkin, Leijonhufvud analyzes the impacts of a downward revision of entrepreneurial expectations. Initially, equilibrium is assumed; this means all market excess demands sum to zero. Then the decline in demand is used by Leijonhufvud to focus on the nature of the dynamic adjustment process in the economy. This adjustment process is divided into a sequence of three stages. In stage one, the initial impact of the downward revision of expectations is analyzed. Since investment is likely falling due to pessimistic expectations, firms decrease their issue of bonds to finance investment. These pessimistic expectations may be due to rising inventories as the level of economic activity declines. Declining bond issues and rising inventories mean two things--excess demand for bonds may appear and excess commodity supplies exist. Thus $E_g < 0$, $E_b > 0$, and $-E_g = E_b$, where E_g is the excess supply of goods and E_b is the excess demand for bonds.

In stage two of the sequence, a portfolio adjustment process occurs. Interest rates fall and bond prices rise. This transfers excess bond demand, $E_b > 0$, into the money market. Consequently, the excess demand for money just offsets the excess supply of goods; $E_g < 0$, $E_m > 0$, and $-E_g = E_m$. Ideally, this market disequilibrium would be adjusted by a decline in the price level, similar to Patinkin's real balance effect. A falling price level would: (1) raise the purchasing power of money balances, eliminating excess money demand, and (2) lower the price of final goods, eliminating excess supply. This type of adjustment process may be precluded in a system of sequential monetary exchange. A sequential exchange process is susceptible to a degree of sluggish wage and price adjustment. The sluggish wage adjustment is due to the worker's reservation wage, while sluggish price adjustment is due to the employer's contractual obligation for quantity adjustments (lay-offs) which prevent cost and price reductions.

A third stage can be constructed when consideration is given to the sequence of monetary transactions. Excess supply in the goods market means that employers reduce the number of employees. Excess supply of goods is thus converted into an excess supply of labor as employers adjust, or $E_g < 0$ becomes $E_n < 0$. The excess demand for money found in stage two also remains. Without a price reduction there is no automatic way for the purchasing power of money balances to increase. Furthermore, the workers' search for employment as a demand for money balances is ineffective. His offer of labor services in no way constitutes effective demand for the products he would produce. Traditionally, an excess demand is counted as such, if there is some apparent manifestation of the excess demand. An excess supply of labor is also a manifestation of excess demand for money. But in a monetary economy there is no economically effective way to express this demand for money. A monetary transaction structure cuts the adjusting link between notional excess money demand and excess labor supply. Realized excess money demand is the relevant concept. Thus, realized excess money demand is zero, $E_m^r = 0$, and there is an excess supply of labor, $E_n < 0$. In the sense of Walras' Law, a system of sequential monetary exchange may be in pervasive disequilibrium.

All excess demands are at most equal to zero (Clower, 1965:122).

Recapitulating, neo-Keynesians, like previous economists, do not pay all that much attention to the dynamic aspects of the General Theory. Keynes' concern with greater psychological realism and the rationality of complex decision-making processes is ignored in favor of economic concerns. However, neo-Keynesians are concerned with a more realistic, monetary transaction structure. A sequential monetary exchange system permits a different assessment of Keynes than Patinkin's; who maintains that Keynes is more concerned with the process of adjustment than economic theory. By grounding the process of adjustment in a theory of sequential monetary transactions, much of what Keynes had to say is given a viable rather than an ad hoc economic theoretical foundation. The existence of money as a medium of exchange makes the process of macro adjustment more indeterminant. But this brings us back to Keynes. What characteristics of cognition and rationality does a transactor need to cope with the complexity of such economic indeterminacy? Thus, the questions of psychology and rationality are raised again.

THE RATIONAL EXPECTATIONS HYPOTHESIS

To this point, our claim is that mind and rationality are the fundamental issues in recent philosophy of science and in recent monetary theory. In philosophy of science, the consideration of these issues has a much longer history than in monetary theory; only quite recently do monetary economists raise even implicitly the issues of mind and rationality. Rational expectations theorists raise these issues from the perspective of positive economics. The older question of microfoundations effectively is being transformed into the issue of mind and rationality: What psychological perspective, if any, and what conception of rationality is necessary to make micro and macroeconomics compatible?

In positive economics, the perspective on mind and rationality leads us to suspect that much of the dynamic nature of Keynes' point of view is abandoned. Since rational expectations theorists claim to be more monetarist than previous monetarists (Sargent, 1976a:207), we suspect that they also may be more positivistic than other positive economists. Logical positivists and allegedly positive economists like Friedman and rational expectations theorists, we argue, share a common perspective on mind and rationality. Economic rationality as a maximizing postulate is devoid of any psychological content, either cognitive or behavioral. Cognitively, conscious maximization on the part of individual transactors is simply an irrelevant concern with too much realism; the logic of prediction is the only relevant concern. Behaviorally, a logic of behavior replaces a psychology of behavior; externally observed consistency is the positive economist's only concern.

Not surprisingly then, rational expectations theorists claim that macroeconomics and monetary theory do not make enough use of relationships based on a theory of rationality as a logic of individual optimization (Miller, 1976:43). Essentially, rational expectations theorists suggest that the neoclassical-positivistic concepts of rationality and individual optimization provide the components of an appropriate microfoundation for macroeconomics.[39] However, at the macro level a problem arises. Individual optimization may not be directly observable;

rational behavior may not be apparent because of the complexity of the situation. Individuals may have insufficient or incorrect information; while institutional or disequilibrating factors may effectively alter individual optimization.[40] But, on average, one might suspect that individuals are rational in the restrictive, positivist sense (Sargent, 1972:74). Consequently, at the macro level, individual optimization needs to be represented in stochastic models which permit some random indeterminacy. The statistical means or expectations generated from all available information might well substitute for a conception of the public's psychological anticipation.[41]

The rational expectations point of view has strong implications for the conceptual structure of the General Theory. Like Keynesians, monetarists, and to a lesser extent neo-Keynesians, rational expectations theorists ignore the more dynamic half of Keynes' point of view--the lower part of Figure 6. But they do this for a reason; they suggest that Keynes permits an element of irrationality into his model (Poole, 1976:463). Since irrational factors are by definition unpatterned or random, rational expectations theorists suggest replacing Keynes' psychological expectations and expectations about institutions like the Fed, with statistical expectations. Furthermore, randomness may be so pervasive that structural macroeconomic relations are to be avoided. Only the most simple, reduced-form equations are to be used in place of structural relations. Thus, the concern for rationality is not only used to question Keynes' dualistic point of view, but also to undermine much of contemporary macro theory.

The implications of the rational expectations point of view for macroeconomics can be illustrated schematically in Figure 7. Figure 7

		Determinants of GNP			
	Aspects of Model	Determinants of Aggregate Demand		Determinants of Aggregate Supply	
UPPER	Reduced Form Relation	Long-Term Interest Rate Martingale (1)[a]	Portfolio Balance Equation (2)[a]	Prod Funct. (3)[a]	Labor Force Participation Equation (4)[a] Phillips Curve (5)[a]
UPPER	Dependent Variables	Y_t R_t P_t		U_{nt}, N_{ft}, P_t	
UPPER	Independent Variables	Z_t	M_t	Technology	P_{Qt}
LOWER	Ultimate Independent Variables (UIV)	Random Shocks	Random Shocks	-----	Random Shocks
LOWER	Type of Explanation Behind UIV's	Statistical	Statistical	-----	Statistical
LOWER	Expectations Affecting UIV's	Rational (Autoregressive) (6)[a]	Rational (Autoregressive) (7)[a]		Rational (Autogressive) (8)[a]

Figure 7 Sargent's Prototypic Rational Expectations Macro Model in a Form Comparable to A Keynesian Macro Model as found in Figure 6

[a] For variables and equations, see the next page. See the glossary for a definition of martingale.

Reduced Form Relations:

$$R_t = R_{t-1}+a(Z_t-E_{t-1}Z_t) + U_{1t} \qquad (1)$$

(Long-Term Interest Rate Martingale)

$$M_t-P_t = b_1R_t+b_2Y_t+b_3(M_{t-1}-P_{t-1})+ U_{2t} \qquad (2)$$

(Portfolio Balance Equation)

$$Y_t = C_0t + C_2(Nf_t-Un_t+Pop_t) + U_{3t} \qquad (3)$$

(Production Function)

$$Nf_t = d_0(P_t-E_{t-1})+d_1\ Un_t + \sum_{i=1}^{n_1} g_i\ Nf_{t-1} + U_{4t} \qquad (4)$$

(Labor Force Participation Equation)

$$Un_t = h(P_t-E_{t-1}P_t)+ \sum_{i=1}^{n_2} l_iUn_{t-i}+ U_{5t} \qquad (5)$$

(Phillips Curve)

Figure 7 continued

Autoregressive Expectations

$$Z_t = \sum_{i=1}^{n_4} s_i\ Z_{t-1} + U_{6t} \qquad (6)$$

$$M_t = \sum_{i=1}^{n_3} r_i\ M_{t-1} + U_{7t} \qquad (7)$$

$$Pop_t = \sum_{i=1}^{n_5} W_i\ Pop_{t-1} + U_{8t} \qquad (8)$$

Variables

Un	Unemployment Rate
P	Log of GNP Deflator
Nf	Log of Labor Force Participation Rate
Y	Log of Real GNP
R	Long-Term Interest Rate
M	Log of Money Supply
Z	Vector of Exogenous IS Variables
Pop	Log of Population
U	Random Error Terms
E_{t-1}	Expectation of the Value of a Current Variable Based on Information in Previous Period

Constants

a,b,c,d,g,h,l,r,s,w

is similar in design to Figure 6. What is summarized in Figure 7 is a prototypic rational expectations model of one of the leading theorists, Thomas Sargent (1976a:209-213). The basic model consists of five endogenous variables: income, interest rates, prices, unemployment, and the labor force participation rate; and three exogenous variables: the money supply, population, and a vector of IS variables (consumption, investment, and government spending). The IS curve describes the goods market; the portfolio balance curve describes the money market; the Phillips curve and the labor force participation equations describe the labor market; and the production function completes the model. The first two equations generate a negatively sloped aggregate demand curve, while the last three equations generate a positively sloped aggregate supply curve. The three exogenous variables are determined by statistical equations which relate the current value of the independent variables to their past values.

Rationality as maximization enters the model in the form of rational expectations. An expectation of this period's price-level is rational if it is based on all available information in the previous period. Since mathematical expectations are assumed to be good stochastic approximations of the public's psychological anticipation, an expectation is rational if it equals $E_{t-1}(P_t)$. $E_{t-1}(P_t)$ is the last period's expectation of this period's rate of price increase, conditioned by information available only in the earlier period. The Phillips curve embodies rational expectations.[42] The inverse relationship between inflation and unemployment depends only on the unanticipated component of inflation, $(P_t - E_{t-1}(P_t))$. Anticipated inflation has no effect in the current time period if transactors are rational optimizers.

Rationality is also important in closing the model. Exogenous variables are assumed to be autoregressively related to the past values of the exogenous variable. This means that the significance of a past value of an exogenous variable declines the more distant it is in the past. Rationality amounts to taking the expectations of the exogenous variables or $E_{t-1}(M_t)$, $E_{t-1}(Z_t)$, and $E_{t-1}(P_t)$. This again means that more distant information in time is given less weight in determining the last period's exogenous variable. But the important point is that all available information is used to form the expectation of the value of the variable for the next period. Furthermore, there is no partial equilibrium structure to impose artificial constraints on the decisions of transactors. Such stochastic equations allegedly permit the economist to get as close as possible to the aggregate effects of individual optimization. Rationality thus is used to hold almost all theoretical macrostructure in abeyance.

In conception, the macro model incorporating rational expectations is similar to Keynes'. Figures 6 and 7 are remarkably similar. The major differences are: (1) the use of reduced-form equations for each market rather than structural relationships; (2) the ultimate independent variables; and (3) the type of explanation and expectations behind the ultimate independent variables.[43] In the rational expectations model, everything which is fully anticipated has no economic impact in the current period. Monetary and fiscal policy, as usually conducted, can be anticipated and fully offset by private transactors. Only unanticipated changes or random shocks can have any systematic impact in the current period. Even the business cycle is due to unanticipated exogenous shocks. Keynes' model, in the rational expectations view, is unacceptable because he implicitly permits too much irrationality into his model. Psychological expectations are unpredictable, thus partially irrational. Keynes (1936:162) himself suggests the state of long-term expectation may be irrational. Thus, the issue concerning the theoretical generality of the General Theory can be restated: The economics of Keynes is not good economic theory. Most of Keynes' insights concern the random shocks which effect the economy in a process of dynamic growth. Random shocks are not much of a theoretical contribution.

In brief, rational expectations theorists may be the first group of economists to hold that the development of economic theory is constrained by views of rationality and psychology. They take a restrictive conception of economic man, both rationally and psychologically, and use it to question all previous macro theory. The best way to study the aggregate activity of an economy with "intelligent" decision-makers is to use economically non-theoretical statistical techniques like autoregressive mechanisms and reduced-form models. Individuals on average are rational if their anticipations are equivalent to the expectations of statistical models incorporating all available information. Of course, we must question whether a restrictive, de-psychologized concept of rationality actually permits assessment of decision-making. Rational expectation theorists obviously remove the most dynamic aspects of the conceptual structure of the General Theory. But, they no doubt believe that they have good reasons for repudiating these aspects of Keynes' view. Having to choose between relevance and rationality, rational expectations theorists obviously opt for the latter.

Frank Knight (1925a:251) was aware of such an attitude more than a half century ago:[44]

> The practical and theoretical interests combine in pushing us toward the repudiation of everything that cannot be shown to be true for all persons at all times, as outside the realm of knowledge, unreal, or merely subjective. The next step is the repudiation of all sources of knowledge which do not yield demonstrably uniform and universal results. Thus we reach the ideal of "science," of mechanistic monism [behaviorism] as a world view. Its criteria for distinguishing reality from appearance or illusion leave only configuration and motion in space as "real."

SUMMARY

Recent monetary controversy, like recent philosophy of science, is really a debate over rationality and the scientific importance of mind. With the advent of the rational expectations approach, economists no longer can ignore the fundamental nature of the issues and concentrate only on economic questions. The Keynesian Revolution may exhibit so many revolutions and counter revolutions because the nature of the issues are ignored by economists. If such complex and fundamental issues continually are ignored, new approaches to macroeconomics will be found for some time to come. Each new microfoundation may emphasize a different aspect of economic activity than previous ones. Even if solutions to the dilemmas of mind and rationality appear difficult, after four decades it is time economists face the issues.

Keynes is partly to blame for this continuing saga of monetary controversy. His point of view is very dualistic; he sharply separates static from dynamic factors. His connection between theoretical, psychological, and institutional factors is largely verbal. This makes it quite easy for Keynes' interpreters to passively or actively abandon the more dynamic aspect of his point of view. Consequently, neither Keynes nor his interpreters provide a unified conceptual framework for all of the factors Keynes has in mind. Only the rational expectations approach achieves a degree of conceptual unity. But the unity of the rational expectations approach is achieved by abandoning a scientific concern for the real world. This may be unnecessary given our structural view of science and the nonjustificational view of economic rationality.

NOTES

[1]See note 1, chapter one, p.16.

[2]See the quote of James Tobin, chapter one, p. 6.

[3]Behavioral psychology deals with the lower mental processes: reaction time, habit, reflex action, and their physiological concomitants. Cognitive psychology deals with the higher mental processes: learning, concept formation, inferences, expectations, problem solving, memory, and reasoning. In the context of monetary theory, decision-making, expectation, and inference are used almost interchangeably. We must be quite careful to separate cognition and behavior and not to give a cognitive interpretation to behavior. Unless one has a philosophy of science which encompasses both cognition and behavior, the two perspectives become redundant. Friedman's view of positive economics makes this quite clear.

[4]W. C. Mitchell (1924:15) says, "This foundation began to create uneasiness about the time utility theory came into favor among economists."

[5]Fisher (1965 [1892]:5) says, "The foisting of Psychology on Economics seems to me inappropriate and vicious. Others besides Professor Edgeworth have done it. Gossen and Jevons appeared to regard the 'calculus of Pleasure and Pain' as part of the profoundity of their theory." Pareto (1971 [1927]:29) suggests that "indices of ophelimity [desire]" be used rather than measurable hedonistic concepts. Hicks (1946:22-23) aims to provide a "surer foundation" for economics with a "pure logical analysis of capitalism." Samuelson (1947:93-94) notes that only relational comparisons of preferences are necessary for a theory of consumer behavior. At one point, Samuelson (1966 [1938]:4) aims to start anew and formulate the theory of consumer behavior "dropping off the last vestiges of utility analysis."

[6]For an axiomatic development of consumer choice see H. A. J. Green (1971:22-44).

[7]Scheutz (1943:144) comes very close to stating a similar position: "This fictitious consciousness [like economic man] is constructed in such a way that the fictitious actor. . . would have the same stream of consciousness as a living man acting in the same manner, but with the important modification, that the artificial consciousness is not subjected to the ontological conditions of human existence. . . In

short, the ideal type is but a model of a conscious mind without the faculty of spontaneity, and without a will of its own." Our criticism is, that for such an ideal type, it does not seem to make sense to talk about mind at all.

[8]Knight (1925a:248) disagrees with Friedman's point of view: "We cannot treat human beings as unconscious organisms or mechanisms. . . man is more than an observed object."

[9]Simon (1976:131)says, "The assumptions of utility or profit maximization. . . and the assumption of substantive rationality. . . freed economics of any dependence on psychology."

[10]Simon (1978:13) suggests that attention be viewed as a scarce resource: "In a world where attention is a major scarce resource, information may be an expensive luxury, for we may turn our attention from what is important to what is unimportant." Knight (1930:233) echoes a similar view: "Instead of progressing toward the condition of unconscious automata we are called upon constantly for more thinking."

[11]To be fair to Knight, he has a restrictive conception of science which he was willing to go beyond. Concerning the limited scope of economic science, Knight (1930:254) concludes, "We seem to be forced to the conclusion, not that prediction and control are impossible in the field of human phenomena, but that the formal methods of science are of very limited application." In an earlier paper, Knight (1925b:386) states, "The economist must be more than an economist, and. . . he must know when he is a scientist and when he is something else."

[12]For those who suspect that our nonjustificational conception of economic rationality is too informal, we reply that we believe that it can be given a mathematical formulation. Such a formulation might require mathematics like set theory and topology. Since most economists are not trained with these tools of mathematics, our concept of rationality may imply a mathematical retooling for the profession. However, the lack of such mathematical facilty should not stand in the way of our theory. David Bohm (Pryce and Bohm, 1962:73) makes a similar claim for quantum physics:

"In the sixteenth century people didn't know differential equations, and this was one of the main difficulties in the way of the development of classical physics. In fact, it was a gigantic achievement for Galileo to be able to describe accelerations algebraically. . . But I do not believe topology is as difficult as people generally think it to be. What is more, I see very strong analogies between the way in which topological relations would be expressed and the way in which the laws of quantum mechanics are now expressed in terms of operators and matrices."

See chapter nine for a more detailed discussion of economics, rationality, and quantum mechanics.

[13]This may be an overly optimistic point of view. But, if it were the case, then nonjustificational rationality takes us back to Marshall's (1964 [1920]:1) position: "Economics is the study of mankind in the ordinary business of life." Also see note 1, chapter one, p. 16.

[14]Hollis and Nell (1975:59) state a similar view: "It is tempting to argue that human behavior is amenable to science only insofar as it is observable. . . A man is a complex observable servo-mechanism. . . Rational is simply a predicate of certain types of behavior."

[15]Keynes (1936:15) states, "It is therefore useful to consider what hypothetical psychological propensities would lead to a stable system; and then, whether these propensities can be plausibly ascribed, on our general knowledge of contemporary human nature, to the world in which we live."

[16]What we are emphasizing is that Keynes seems to view consumption as more stable than the other components of aggregate demand. In psychological terms (see note 3 above), we believe that Keynes took consumption as a behavioral variable in contrast to other variables which he developed implicitly as cognitive variables. Speculative money demand and investment demand are two examples of cognitive variables which significantly depend on expectations. Cognitive variables ought to exhibit more volatility than behavioral ones.

More recently as inflation has accelerated, the stability of consumption may have decreased. Economists have introduced expectations into the study of consumption. Thus, the emphasis on psychological factors exhibited in the <u>General Theory</u> perhaps needs to be expanded rather than narrowed, as rational expectations theorists would have it.

[17]Other economists have recognized the need for a dualistic point of view. Both Taylor and Knight make their remarks in the context of a naturalistic, unified, behavioral approach to economics. Knight (1925a:165) states, "We come back to dualism; we cannot talk about human beings in monistic [behavioristic] terms and talk sense." Taylor (1929:26) says, "Human beings who have to do the manipulating are, so to speak, themselves part of the [economic] mechanism. The conception seems to involve a dualism which leaves the 'economic man' a cog in the mechanism, but regards the same man in his capacity as a 'political man,' a citizen, a refiner, legislator, or public administrator, as 'free' to act. . . to promote the general welfare."

[18]As stated in chapter one, we argue that alternative microfoundations implicitly can be interpreted as encompassing alternative conceptions of rationality. The reductionistic, maximizing conception of rationality seems implicit in the Walrasian microfoundation; while we suggest nonjustificational rationality as a way of formulating the Marshallian "reasonableness" mentioned by Clower as quoted in chapter three, note 18, p. 74. Since the issue of nonjustificational view of rationality concerns the appropriate world view for philisophy of science, our argument implies that the issue of microfoundations is also a question of world view for economics.

[19]On the surface, monetarists and neo-Keynesians may seem to have quite similar conceptions of monetary adjustment as a portfolio adjustment mechanism. However, a neo-Keynesian like James Tobin, who (1958) suggested a maximizing microfoundation for money and bond demand, goes beyond the mechanistic implications of such a narrow theory. Tobin (1978) emphasizes slippages in the sequence of asset transactions and the effect such slippages can have on real capital formation. The sequence of asset transactions in a highly developed financial system seem to be bypassed by quantity theorists like Friedman and other monetarists like rational expectations theorists.

[20]Keynes (1936:3) denies the theoretical generality of classical theory and its relevance to the Depression: "I shall argue that the postulates of the classical theory are applicable to a special case only and not to the general case, the situation which it assumes being a limiting point of the positions of equilibrium." Also, this one paragraph chapter is titled, "The General Theory."

[21]Keynes (1936:18) defines Say's Law in the following manner: "From the time of Say and Ricardo the classical economists have taught that supply creates its own demand;--meaning by this in some significant, but not clearly defined, sense that the whole of the costs of production must necessarily be spent in the aggregate, directly or indirectly, on purchasing the product." In the text, we have given a substantive interpretation of Say's Law as an equilibrium proposition about a barter economy.

[22]Keynes (1936:293) says, "The division of Economics between the Theory of Value and Distribution on the one hand and the Theory of Money on the other hand is, I think, a false division. The right dichotomy is, I suggest, between the Theory of Individual Industry or Firm and of the rewards and the distribution between different uses of a <u>given</u> quantity of resources on the one hand, and the Theory of Output and Employment <u>as a whole</u> on the other hand. . . But as soon as we pass to the problem of what determines output and employment as a whole, we require the complete theory of a Monetary Economy."

[23]The distinction between Marshallian and Walrasian approaches to economic analysis is developed by Leijonhufvud (1974) and Clower (1975). Patinkin (1965:3) places himself in the Walrasian tradition. See note 18, chapter three p. 74.

[24]Patinkin (1965:531-572) discusses Walras' monetary theory and the tatonnement process in greater detail.

[25]This is a description of a recontracting process perhaps more compatible with Edgeworth (1881:17,35) than Walras. But Patinkin (1965:533) and Schumpeter (1954:1002) suggest the recontracting process is compatible with Walras' theory.

[26]Mason (1975:vi-25, n. 22) states, "The Walrasian text [Walras, 1954 [1926]:327]. . . clearly implies that a real sector equilibrium may be distorted more or less by the introduction of money, thus requiring, to some extent at least, a secondary (as well as primary) real *tatonnement* as an adjustment to the completed monetary *tatonnement*."

[27]Walras (1954 [1926]:327) recognizes that the treatment of money comes very close "to falling outside the system of equations of [general] economic equilibrium."

[28]Our interpretation of Walras' Law differs slightly from the usual interpretation in the general equilibrium literature. This literature tends to suggest that every excess demand is zero rather than the sum of excess commodity demands. For Ferguson and Gould (1975:434) Walras' Law states, "that in an economy with n markets, equilibrium in n-1 of those markets assures that equilibrium must hold in the nth market also." Similarly Henderson and Quandt (1971:159-160) view Walras' Law as an aggregate budget identity: "This identity is called *Walras' Law*. The equilibrium conditions require that every aggregate excess demand equal zero." Our objection to this interpretation is that general equilibrium is achieved by imposing a series of partial equilibriums in each market. Like Douglas Fisher (1978:49), we believe this position is unnecessarily restrictive "leaving us no room whatsoever for an interesting monetary theory."

[29]If the economy were in equilibrium, then money excess demand would be implied by the m-1 commodity excess demands. In equilibrium, the indicator, i, could just as well be stopped at m-1. Since equilibrium rarely prevails in actual states of the world, this suggests that money implies disequilibrium. Thus our mathematical statement of Walras' Law, carefully reinterpreted for a monetary economy, might also encompass disequilibrium. See our discussion of Clower and Leijonhufvud in the following paragraphs.

[30]Walras' Law was first so denoted by Oscar Lange (1942), who distinguished between Say's and Walras' Laws on the basis of barter versus monetary analysis. Say's Law as an identity applies to a barter economy; Walras' Law as an identity allegedly applies to a monetary economy. This use of Say's and Walras' Laws set off a controversy known as the Patinkin controversy (Mauer, 1966). Patinkin's contributions can be found in Patinkin (1951, 1965). Other perspectives are those of Archibald and Lipsey (1958), Becker and Baumol (1952), Encarnacion (1958), Valavanis (1955), and Mason (1975: Chapter 5).

[31]See Patinkin (1965:229) for a complete specification of the equations.

[32]Patinkin (1969 [1951]:394) with reservations states, "There always exists a sufficiently low price level such that, if expected to continue indefinitely, it will generate full employment." A disastrous increase in uncertainty caused by deflation is one of Patinkin's reservations (Patinkin, 1969 [1951]:396).

[33]Notice how Patinkin differs from Friedman (1971 [1953]:43-44), who in his methodology article seems committed to maintaining the relevance of competition. Friedman's concern is the theory of monopolistic competition of Chamberlain and Robinson. Patinkin's (1965:343) concern is not with "extraneous monopolistic elements," but with rigidities following from consumer sovereignty. See quote from Patinkin, pp. 149-150, above.

[34]Blaug (1976:162) represents this point of view: The "hard core" of classical and neoclassical economics was "rational economic calculation." Allegedly, Patinkin, Clower, and Leijonhufvud attempt to integrate Keynes with the neoclassical-maximizing point of view in which uncertainty is insignificant (Blaug, 1976:164).

[35]See notes 18 and 19 above.

[36]Leijonhufvud (1968:41) presents a difficulty in that he defines money as any short-term financial asset; yet he (1968:90) maintains that the "<u>dynamic properties of an economic system depend upon</u> . . . <u>its 'transactions structure</u>.'" We would prefer that Leijonhufvud maintain the narrower conception of money as a medium of exchange and then emphasize the significance of sequential financial asset transactions in a highly developed monetary economy. In this regard, Leijonhufvud is like Tobin (see note 19 above) who might be incorrectly construed as overly emphasizing the asset function of money. Apparently, Leijonhufvud defines money as any short-term asset to support his argument concerning the aggregative structure of Keynes' theory. We do not think money has to be so defined for his argument to retain its validity.

[37]Davidson (1972:xiii) castigates Leijonhufvud for being too concerned with the American neoclassical synthesis as the only analytical device for comparing his interpretation of Keynes. The neoclassical synthesis in Davidson's view cannot be concerned with actual monetary economics.

[38]See Clower (1967, 1977), Ostroy (1973), and Starr (1972). Leijonhufvud (1969:30-31) suggests that the multiplier is a monetary phenomenon. In a monetized transaction structure, errors in trading may be multiplied rather than reduced.

[39]Note the difference in point of view between neo-Keynesians and rational expectations theorists. Neo-Keynesians begin with an interpretation of Keynes and search for a consistent microfoundation. Rational expectations theorists begin with the logical theory of transactor behavior and search for a consistent theory of the economy. The two approaches to economic analysis may never meet following this strategy. This incommensurability indicates how important assumptions are in economic analysis.

[40]Sargent (1972:75) states, "The requirement that available information be used efficiently is much weaker than the requirement that expectations be very accurate."

[41]The mechanisms of forming expectations are explained in more detail in the following chapter. The conceptual structure of the rational expectations macro model is our concern in this chapter.

[42]Lucas (1972:57) maintains that the Phillips curve is not a description of transactor behavior alone, but is also due to government policy. In the short run, the Phillips curve may not be vertical.

[43]Another major difference could be the appropriate conception of rationality to use in interpreting Keynes. Our multi-dimensional, non-justificational concept of rationality is quite different from the logical conception of rationality as consistency of observed behavior.

[44]Knight (1925a:265) clearly equates behaviorism with a monistic point of view in science.

Chapter 6

RATIONAL EXPECTATIONS, UTILITARIANISM, AND A THEORY OF MIND

In the previous chapter, we argue that fundamental issues lie behind the Keynesian Revolution. These issues are the nature of rationality and the scientific importance of mind. They receive very little attention from monetary theorists. The most recent group of monetary theorists, rational expectations theorists, apparently would get rid of any remaining vestiges of psychological aspects to rationality and expectation. Thus, the rational expectations theory seriously undermines the search for a structural, theoretical approach to macroeconomic activity because of narrow views of rationality and mental phenomena like expectations.

No doubt some economists may differ with our analysis, believing that the rational expectations approach does address real expectations relating to inflation and unemployment. Although we maintain that such an interpretation is inconsistent with Friedman's original views on positive economics, such a realistic interpretation of rational expectations needs to be considered.[1] Few positive economists, including Friedman, are likely to interpret positive economics as consistently as we have.[2] However, we argue that the appeal to a more realistic interpretation of rational expectations may have an unanticipated consequence. Demonstrating such a consequence requires that we treat rational expectations as an implicit theory of mind. When expectations mechanisms (both rational and adaptive) are considered to be real psychological processes of anticipation, we argue that they essentially constitute an empirical theory of mind. The theory of mind one finds implicitly in recent monetary research on expectations is quite similar to the empirical theory of mind in nineteenth-century utilitarianism. This theory of mind is known as associationism.

Both our argument and our strategy are quite simple. Our argument is that the attempt to add realism to the current debate about expectations by interpreting expectations mechanisms realistically (relating to psychological anticipation), takes monetary theory back to the somewhat antiquated, utilitarian theory of mind--associationism. Our strategy involves a simple comparison between rational and adaptive expectations mechanisms and the mechanisms of mind in utilitarian associationism. In the first section, we explore the utilitarian theory of mind, associationism. In the second section, for the sake of completeness, we explore both adaptive and rational expectations mechanisms. In the third section, we compare the mechanisms of mind with rational and adaptive expectations mechanisms in recent monetary literature.

UTILITARIAN ASSOCIATIONISM

Utilitarianism was both a theory of ethical behavior and a theory of mind (Russell, 1945:773-775). For the past century, economists gave most of their attention to behavioral aspects of utilitarianism. Utilitarians based the motivation of economic behavior in pleasure and pain. The development of microeconomics as a logic of choice resulted from economist's attempts to get rid of hedonism as a fundamental concept in economics.[3] Recently the logical concept of rational choice, supposedly amended for learning and intelligence, was extended from microeconomics to macroeconomics in the form of rational expectations.[4]

Presently, mechanisms of how expectations are formed are being postulated, incorporated into models, and the models are being tested.[5] However, these mechanisms of forming expectations raise a fundamental issue. In postulating and interpreting expectations mechanisms which are essentially unobservable, economists are postulating that mental activity can be known inferentially through empirical techniques. This is similar to the empirical psychology of the British empiricists--associationism, which makes mental concepts ultimately derivative from sensation. Economists may be rediscovering the associationistic mechanisms of mind in their research on adaptive and rational expectations.

Associationism is an empirical theory of mind because abstract ideas or concepts are traced ultimately to sensation. Historically, associationism can be traced from John Locke, George Berkeley, David Hume, and David Hartley in the eighteenth century; and James Mill, John Stuart Mill, Alexander Bain, and Wilhelm Wundt in the nineteenth century; to behaviorists J. B. Watson, E. C. Tolman, C. L. Hull, and B. F. Skinner; and to gestalt psychologists M. Wertheimer, W. Kohler, and K. Koffka in the twentieth century. Behaviorism is essentially the theory that elemental stimuli are associated to form elemental responses, while gestalt psychology is the theory that whole perceptual configurations are associated to form total-functional response patterns (Weimer, 1974a:251).[6]

The late historian of psychology, E. G. Boring (1950: 193-199), gives David Hartley the credit for the first thorough formulation of associationism. Hartley's statement of associationism, in his Observations on Man (1749:ii), conveys the essence of associationism as a psychological theory of human thought:

> Sensations are those internal Feelings of the Mind, which arise from the Impressions made by external Objects upon the several Parts of our Bodies. . .
>
> The Ideas which resemble sensations, are called Ideas of Sensation: All the rest may be therefore called Intellectual Ideas.
>
> It will appear in the Course of these Observations that the Ideas of Sensations are the Elements of which all the rest are compounded. Hence Ideas of Sensation may be termed simple, intellectual ones complex.

The strategy of associationism is to make simple ideas directly derivative from external stimuli and to make complex ideas compounds of simple ideas. Thus, even the most sophisticated concepts are entirely and ultimately derived from factors external to the human mind. However, Hume (1955 [1748]:29-30) considers the possibility that an abstract idea could arise apart from sensation or external factors. Abstract terms which cannot be traced to sensation are under "suspicion" and "without any meaning."

Prior to Hume and Hartley, associationsim is forshadowed by John Locke and George Berkeley. They are concerned with the connections of sensations that individuals make in everyday experience. Locke (1955 [1700]:394) originates the phrase "association of ideas" (Boring, 1950:176), while Berkeley (1967 [1709]:302-303) implies that ideas like magnitude and distance result from the simultaneous functioning of the five senses:

> Sitting in my study I hear a coach drive along the street; I look through the casement and see it; I walk out

> and enter into it. Thus common speech would incline one to think I heard, saw, and touched the same thing, to wit, the coach. It is nevertheless certain the ideas intromitted by each sense are widely different, and distinct from each other; but having been observed constantly to go together, they are spoken of as one and the same thing.

After Hartley, associationism is further developed by James and John Stuart Mill. James Mill, in his Analysis of the Phenomena of the Human Mind (1869 [1829], I:70-71), makes everything in the conscious mind the result of associationism; while John Stuart Mill, in his System of Logic (1973 [1843]:853), uses a chemical analogy to describe associationism:

> The laws of the phenomena of mind are sometimes analogous to mechanical, but sometimes also to chemical laws. When many impressions or ideas are operating in the mind together, there sometimes takes place a process of a similar kind to chemical combination.

Mill uses the analogy of mental chemistry to call attention to the complexity of human thought. Human thought has attributes which cannot be predicted from the simple phenomena being associated. Similarly, chemical compounds have properties which cannot be predicted from the simpler properties of the constituent elements.

Mechanisms of association are the key to understanding our contention that economists unknowingly have retrieved this forgotten theory of abstract ideas and concepts from the utilitarians. In his System of Logic (1974 [1843]:852), J. S. Mill gives a simple description of three laws (mechanisms) of association:

> Ideas, or secondary mental states, are excited by our impressions, or by other ideas, according to certain laws which are called Laws of Association. Of these laws the first is, that similar ideas tend to excite one another. The second is, that when two impressions have been frequently experienced (or even thought of) either simultaneously or

> in immediate succession, then whenever one of these impressions, or the idea of it, recurs, it tends to excite the idea of the other. The third law is, that greater intensity in either or both of the impressions, is equivalent, in rendering them excitable by one another, to a greater frequency of conjunction. These are the laws of ideas.

Boring (1950:229) calls the three laws similarity, contiguity, and intensity. In a later work, the laws of association appear to be modified by Mill (1865, I:307-308) to give four principles of association: similarity, continguity, frequency, and inseparability. These mechanisms are processes by which complex, abstract ideas can be constructed from simpler elements. Effectively these mechanisms make the content of the mind refer ultimately to sensation.

To summarize, associationism is the utilitarian, empirical theory of mind. Associationism means that conscious, abstract ideas and thoughts are compounded from sensational stimuli according to the laws of association. The laws of association (following Boring) are similarity, continguity and intensity. These laws make all meaningful concepts and ideas derivable entirely from sensation.

RATIONAL EXPECTATIONS AND MONETARY THEORY

Having reviewed in some detail the utilitarian empirical theory of mind, associationism, we now turn to expectations mechanisms in recent monetary theory. Due to the length and technical nature of the expectations literature, the entire section which follows is given to reviewing recent expectations mechanisms in economics. Our claim that expectations mechanisms bear a remarkable resemblance to the laws of associationism must wait until the next section. One should not lose sight of our ultimate purpose while the details of the expectations mechanisms are being discussed. Adaptive expectations are considered first, then rational expectations.

Adaptive Expectations and Term Structure

An adaptive hypothesis concerning expectations is first developed in the micro-oriented consumption and finance literature. Stimulated by Milton Friedman's (1957) seminal work on consumption, adaptive expectational variables usually are approximated with geometrically distributed lags. Friedman (1957:184-186) suggests that "permanent income" (expected income) for a two year period be empirically approximated as "a geometric average of the contributions in the two years." Many econometricians following Friedman, then estimate permanent (expected) income in the present period as a function of incomes from previous periods weighted by a geometric series of declining weights (Zellner, Huang, and Chau, 1965: 571-572; Zellner and Giesel, 1970: 865). Although Friedman (1957:15 and 21) is careful to imply that "'expected' is used in the sense of 'mean value' rather than of 'anticipated,'" it is clear that permanent (expected) income is a concept connected with maximizing economic man. Friedman, following Irving Fisher, develops a two period consumption model in which wealth is maximized for the two periods. Permanent income is that portion of income in the current period which is due to human and non-human wealth under the control or ownership of an economic transactor.

The notion that adaptive expectations are consistent with both economic rationality and maximizing behavior, is extended to research on the term structure of interest rates. An expectations hypothesis is advanced to explain the shape of the yield curve. The yield curve illustrates how the interest rate on bonds varies with term to maturity for bonds which are identical except for term to maturity. Yield curves can have varying shapes--ascending, decending, or humped--depending on investors' expectations as observed in the market place.[7] If investors' expectations determine the yield curve, then anticipated rates of interest ought to correspond to rates which are observed at a later date. For example, long term rates of interest can be expressed as a geometric average of existing and expected short-term rates. A two year example will illustrate. Let $R_{II\ 1980}$ be the current yield on a two-year bond, let $R_{I\ 1980}$ be the current yield on the same type of one-year bond, and let ${}_{1980}\,r_{I\ 1981}$ be the expected yield on a one year bond for next year, based on this year's data. Then we have:

$$(1 + R_{II\ 1980})(1 + R_{II\ 1980}) = (1 + R_{I\ 1980})(1 + {}_{1980}\,r_{I\ 1981}) \qquad (1)$$

Equation (1) means that the annual successive yields on a current two year bond ought to be the same as successive yields on an existing bond ($R_{I\ 1980}$) and an anticipated yield on a one year bond (${}_{1980}\,r_{I\ 1981}$). Equation (1) can be rewritten to make next year's anticipated one year rate a function of this year's long and short-term rates:

$${}_{1980}\,r_{I\ 1981} = \frac{(1 + R_{II\ 1980})^2}{(1 + R_{I\ 1980})} - 1 \qquad (2)$$

The simplest expectations hypothesis is that expected rates for future periods ought to be good predictions of the rate on yields actually observed for the period in question. Early studies tested forecasting equations like the following:

$$ {}_{1980}r_{I\ 1981} = R_{I\ 1981} + u_{1980} \tag{3} $$

Equation (3) means that the one-year rate implicit in this year's yield curve ought to be a good predictor of next year's observed one-year rate. However, tests of the preceding relationship did not give good results (Macaulay, 1938; Culbertson, 1957; Malkiel, 1970).

A more sophisticated theory of the term structure is set forth by David Meiselman (1962: 18-21 and 38-42). Meiselman criticizes tests of forecasting equations like (3) on the grounds that forward (expected) rates such as ${}_{1980}r_{I\ 1981}$, inferred from observed yields, could be wrong. In its place, Meiselman postulates an error-learning model. Every period, a new set of interest rates is observed for bonds of all maturities. From such observed yield curves, previous predictions about future yields can be revised. Meiselman aims to show that changes in anticipated one-year rates are very highly correlated with errors made in forecasting. His error learning model is exemplified by equation (4) which is more simply rewritten as equation (5):

$$ {}_{1981}r_{I\ 1985} - {}_{1980}r_{I\ 1985} = f(R_{I\ 1981} - {}_{1980}r_{I\ 1981}) \tag{4} $$

$$ \Delta\, r_{I\ 1985} = f(R_{I\ 1981} - {}_{1980}r_{1981}) \tag{5} $$

where $(R_{I\ 1981} - {}_{1980}r_{I\ 1981})$ is the most recent forecasting error. Equation (5) means that the forecast for the yield on one-year bonds in 1985 is revised from the forecasting error of the yields on one-year bonds in the current year. Empirically, Meiselman's hypothesis yields better results for the immediate future than for distant time periods. Malkiel (1970:16), when interpreting Meiselman's results, conjectures that investors are more seriously concerned with forecasting short rates for the immediate than the distant future.

Another supportive test of the expectations hypothesis is the test conducted by Modigliani and Sutch (1966). Their assumption about expectations formation is different from Meiselman's. They assume expectations are formed on the basis of historical experience. This assumption has two parts, a regressive and an extrapolative mechanism. Expectations are formed by a combination of these two mechanisms. The regressive mechanism implies investors have in mind a "normal level" of long-term interest rates. Investors anticipate that future interest rates will regress to this "normal level." The extrapolative mechanism assumes that recent trends in interest rates tend to continue. If yields are falling, then investors anticipate they will continue to fall. In their empirical work, Modigliani and Sutch (1966:189-196) test for the spread between long and short rates. The findings are that: "The expectation model can account remarkably well for the relation between short and long-term rates of interest in the United States."

In brief, expectational variables are used first in consumption, investment, and term structure research. Friedman's permanent income theory, Meiselman's error-learning model, and Modigliani and Sutch's term structure model are the best examples of adaptive expectations. Variables which rely on adaptive expectations consider only present and past factors. Adaptive expectations depend only on observable data (Poole, 1976:463).

Rational Expectations and Macroeconomics

Adaptive expectations present a difficulty for macro theorists. If prices and interest rates change as described by the adaptive expectations hypothesis, then rational decision-makers should take advantage of such information about time trends. This raises the issue of rationality. Although the work of Friedman, Meiselman, and Modigliani seems consistent with economic rationality, Thomas Sargent implies that adaptive expectations are irrational. Critizing Meiselman's error-learning model, Sargent (1972:75) states, "it nevertheless seems unwise to construct models that build in 'irrational' expectations."

In a benchmark article, John F. Muth (1961) offers an alternative theory of expectations. He effectively replaces psychological and adaptive concepts of expectation with mathematical-statistical concepts of expectation (rational expectations): "I should like to suggest that expectations, since they are informed predictions of future economic events, are essentially the same as the predictions of the relevant economy theory," (Muth, 1961:316). Muth (1961:315) believes that "dynamic economic models do not assume enough rationality."

Subsequent theorists recognize that psychological anticipations are being replaced or "approximated" with the statistical (rational) expectations of economic models:[8]

> The expectations of investors are rational in the sense of John F. Muth. By this we mean that investors' expectations are equivalent with the optimal forecasts of statistical theory for a certain specified class of statistical models, (Sargent, 1972:74).

> The rational-expectations hypothesis is that the market's psychological anticipation, ${}_tP_{t+1}$, equals the true model's expectation, $E(P_{t+1})$, (Poole, 1976:468).

> The public's psychological expectation about the price level is supposed to be 'rational', meaning that it equals $E_{t-1}P_t$. The equation embodies the natural rate hypothesis, since it asserts that unemployment does not depend on the anticipated rate of inflation, (Sargent, 1976a:210).

> Rationality amounts to requiring that the public's expectation of the exogenous variables [money supply, tax rates, government purchases, and population] equal the mathematical expectations computed from the appropriate objective probability distributions. . . (Sargent, 1976a:211).

For our purposes, the macroeconomic application of rational expectations is the most significant use of this hypothesis. The extension of the rational expectations theory from financial markets to macro models is initiated by Lucas (1972, 1975), Sargent (1973, 1976a, 1976b) and Sargent and Wallace (1975). In turn, their research is stimulating a series of studies which modify their conclusions for various real world economic phenomena such as: widespread use of contracts, business cycles, and the existence of transitional periods for economic policy.[9] A rational expectations hypothesis is particularly appealing to some economists because it extends rationality directly to the process of forming expectations and to macroeconomics. Furthermore, the empirical results from the literature on efficient financial markets tend to support with evidence the assumption of rational expectations which Muth, Poole, and Sargent assume. Poole (1976:465) summarizes, "In the opinion of most economists familiar with this literature, the argument that speculation is highly irrational and destabilizing has been demolished."

The purpose of the rational expectations research of Lucas, Sargent, Wallace, et al., is to formalize the leading verbal accounts of macro theory that make significant use of expectations--those of Milton Friedman (1974 [1968]) and E. S. Phelps (1970). In his presidential address to the American Economic Association, Friedman (1974 [1968]: 414) is concerned that "we are in danger of assigning to monetary policy a larger role than it can perform." Friedman denies that the rate of unemployment can be pegged with monetary policy, except for very limited periods; the monetary authority should not adopt an unemployment target and have a tight policy in one employment situation and an easy monetary policy in another: "The reason it cannot is precisely the same as for interest rates--the difference between the immediate and the delayed consequences of such a policy," (Friedman, 1974 [1968]:416). Following Wicksell, Friedman aims to separate real and monetary forces relating to unemployment; Wicksell distinguishes the natural and market rates of interest. The market rate of interest changes as a result of monetary forces; the natural rate of interest relates only to real factors. Similarly, the natural rate of unemployment relates to real forces impinging on the labor market, while inflation results from monetary forces. Friedman maintains that only temporary departures from the natural rate of unemployment can be obtained as a result of inflationary monetary or fiscal policy: "The temporary trade-off comes not from inflation per se, but from unanticipated inflation. . ," (Friedman, 1974 [1968]:418). The long-run Phillips Curve is vertical at the natural rate of unemployment.

Similarly, Edmund Phelps is concerned with the assumptions of neoclassical equilibrium models which are violated in real world situations. More realistic assumptions might imply disequilibrium, perhaps even general disequilibrium (Phelps, 1970:5). The assumption of perfect information can be abandoned; firms may be wage-makers rather than wage-takers; firms may ration jobs; individuals may search and wait for new jobs; firms may continuously experience and exercise transitory market power due to market imperfections. Phelps (1970:16) effectively maintains that momentary Phillips curves emerge from macroeconomic models when the preceding real-world factors are considered. However, Phelps (1970:23) denies the existence of a lasting trade-off between inflation and unemployment: "equilibrium output and employment are approximately independent of wage and price increases." Hence, if these expectations adapt to actual wage and price increases, little or no permanent increase of output and employment would be obtainable through rising aggregate demand.

Summarizing, rational expectations are first employed as an hypothesis to explain speculative and financial market activity. Then Milton Friedman (1974 [1968]:417-418) and Edmund S. Phelps (1970: 3, 8-12) argue that macroeconomic phenomena, particularly simultaneous inflation and unemployment can be explained within a neoclassical macro model incorporating expectations. Initial econometric results show a negative correlation exists between inflation and unemployment (Phillips, 1958; Frisch, 1977; 1290-1291). Friedman and Phelps crystallize a debate between those economists who maintain there is an exploitable trade-off between inflation and unemployment--the Phillips curve--and those who argue there is no long-run trade-off. Some critics of a viable, exploitable Phillips curve justify their criticism on the basis of rational expectations. Subsequently, rational expectations are incorporated into macroeconomic theory. Macroeconomic models with rational expectations exhibit no long-run trade-off between inflation and output or unemployment, effectively ignoring dynamics and disequilibrium. In their review of the Phillips curve literature, Santomero and Seater (1978:533) conclude, "prevailing models of rational expectations share with neoclassical models the undesirable feature of suppressing all dynamic and short-run disequilibrium phenomena."

ASSOCIATIONISM AND ECONOMIC EXPECTATIONS

Our initial point of inquiry in this chapter is an investigation of what implications may follow from realistically interpreting expectations mechanisms found in recent monetary literature. In Friedman's methodology article, nothing is clearer than his rejection of a self-conscious, maximizing decision-making process as an appropriate interpretation of economic rationality. Although the appeal to realism, even an informal one, violates Friedmanian positive economics, we believe many economists in fact go beyond positive economics.[10] In this section, we set forth the most important implication of interpreting expectations mechanisms realistically. We argue that adaptive and rational expectations mechanisms, when construed realistically, constitute a return to the empirical mechanisms of mind in utilitarianism.

More specifically, we ask: "Do geometric lags, expected yields, error learning models, regressive and extrapolative expectations, and rational expectations correspond to the mechanisms of mind found in John Stuart Mill's Laws of Association?" Since most monetary research, except the rational expectations literature, assumes adaptive expectations, we begin with adaptive expectations.

Adaptive expectations apparently exemplify the first two laws of association, similarity and contiguity. There are two types of contiguity, successive and simultaneous. Events are related in the mind by the principle of <u>successive contiguity</u>, if they immediately follow one another in time. Decaying geometric lags used to approximate permanent or expected income appear to be a more sophisticated version of successive contiguity. With geometric lags, future expectations are dependent on contiguously-successive, past and present values of income. The values of income are weighted with a declining geometric lag according to the amount of time elapsed from the current time period.

Events are related in the mind by <u>simultaneous contiguity</u>, if they happen concurrently. The attempt to base a theory of the term structure of interest rates on expectations is an example of simultaneous contiguity. Although the expectations theory states long rates are a geometric average of current and anticipated short rates, empirical investigations of the theory test for simultaneous contiguity. Equations like (3) derived from a relation like equation (2), can be tested to see if expected short-term rates are good predictors of short-term rates observed at a later time. The expected short-term rate in equation (2) is solely a function of simultaneously existing yields on bonds of different maturity. If an expected, future interest rate can be derived from an array of currently existing rates, then an abstract concept (expected interest rate) is derived from an array of observable, external information. Estimates of future rates are abstract concepts, because the future, by definition, is not yet actualized.

Events also may exhibit both types of contiguity, successive and simultaneous. Meiselman's error-learning model is an ingenious way of combining both types of contiguity in one model. The information upon which revisions of expectations are made, depends on yield curve information, or simultaneous contiguity. However, expectations of future interest rates are revised adaptively from one period to another as new information becomes available. Testing for changes in expected interest rates, Δr_i, implies that such expectations are temporally related. This is a successive contiguity.

The first principle of association, the law of similarity, also is exemplified in adaptive expectations. If successive time periods exhibit a degree of similarity, then adaptive expectations processes eventually will capture this affinity. The types of expectations processes postulated by Modigliani and Sutch (1966) exemplify both types of contiguity since they build on previous work. In addition, their regressive and extrapolative expectations processes suggest investors expect adjacent time periods to have a degree of sameness. Regressive expectations imply that interest rates change in the direction of normal levels. This suggests a degree of temporal regularity to interest rate determination. Extrapolative expectations imply that current trends in changing interest rates continue into the next period. Again, a degree of temporal interdependence is suggested by extrapolative expectations.

Rational expectations are even simpler to analyze in terms of the laws of association than adaptive expectations. The assumption that expectations are rational means that investors fully incorporate information about temporal trends into their current anticipations of the future. If financial markets are efficient, then rational maximizers ought to incorporate information about future prices within this period's predictions about future prices. The predictions or expectations about future movements in economic variables ought to be uncorrelated if expectations are rational. The rational expectations hypothesis effectively removes similarity and successive contiguity as aspects of the expectations-forming process. In this view, serial dependence in expectations implies that investors are irrational. Adaptive expectations are irrational because they mean that investors do not take advantage of temporal trends in expectations. Rational expectations and efficient markets imply that "changes in stock prices are. . . serially uncorrelated because investors react rationally when responding to unpredictable causal events," (Poole, 1976:475).[11]

If the rational expectations hypothesis removes serial similarity and successive contiguity, this reduces the mechanisms of association available to explain rational expectations. Of the mechanisms discussed in the immediately preceding paragraphs, this leaves only simultaneous contiguity. A recent article by Phillips and Pippenger (1976) presents a model of financial markets which embodies simultaneous contiguity.[12] They make short and long-term rates depend on three types of current information: new information relevant to short rates, to long rates, and to both. Separate linear equations are postulated for the long-term and short-term rates. Since the equations are linear, they can be combined to form a linear equation to express a relation between long and short-term rates: "If capital markets are efficient and interest rates essentially perform a random walk, then market expectations contain neither regressive nor extrapolative elements," (Phillips and Pippenger, 1976:12). To repeat, a theory of expectations which makes expectations dependent solely on currently observable information is a theory which implicitly exemplifies an associationistic mechanism of mind, viz., simultaneous contiguity.

In short, some monetary economists postulate that information about expectations forming processes can be inferred from observable information about economic activity. First, theorists concerned with financial and monetary phenomena develop an adaptive expectations hypothesis. It is followed by a rational expectations hypothesis. These expectations hypotheses constitute an empirical theory of mind. They are associationistic in the sense of utilitarian empirical psychology, namely, associationism. Thus, contemporary economists, like their utilitarian predecessors, make abstract processes like mind and expectations derivative and solely dependent upon empirical factors external to man.

SUMMARY

In this chapter, further evidence is produced that rationality and the scientific importance of mind are the fundamental issues behind recent monetary controversy and the Keynesian Revolution. Despite the fact that theorists attempt to free economics of any psychological foundations, we argue that expectations mechanisms in recent monetary and financial theory when interpreted realistically, constitute an empirical theory of mind. This implicit theory of mind is similar, if not identical, to the empirical theory of mind in nineteenth century utilitarianism--associationism. As we shall see in the next chapter, associationism as a theory of mind may be very restrictive. But having transformed our concern with expectations and rationality into a concern for a theory of mind, perhaps the limitations of both expectations mechanisms in economics and associative mechanisms of mind in psychology can be transcended. Going beyond associative mechanisms of mind and expectation will eventually take us to some of the most basic concerns of contemporary science. Knowing how the problem of mind is reconceived in light of contemporary science, might permit less restrictive approaches to rationality and expectations in contemporary monetary theory and policy.

NOTES

[1]We have always wondered why positive economists reject realism so strongly. If a theory predicts well, then more realistic interpretations are not harmful. Unless realism conflicts with some normative assumption, we see no reason why more realistic interpretations cannot be invoked. Of course, since the clash over realism is really a disagreement over the relevance of perfect competition (at least for Friedman), this debate is loaded with normative concerns. In other words, the rejection of realism by Friedman, appears to be an attempt to deflect criticism of his own normative commitment to a free market economy. Mason (1980) makes a similar argument.

[2]By now, the reader must be wondering how one would categorize Friedman's positive economics relative to philosophy of science. The answer is we don't; Friedman is really too inconsistent to categorize according to any particular philosophy of science. Consider the alternatives; based on Friedman's article, we believe he can be interpreted in at least four different ways: (1) as a nineteenth-century empiricist or positivist like J. S. Mill; (2) as a logical positivist like those in the Vienna Circle; (3) as an instrumentalist or conventionalist, which is a post-positivistic view of science; and (4) as a Popperian.

Friedman is like a nineteenth-century empiricist when he claims theories are inductive generalizations. Friedman is like a logical positivist when he claims theories are an empirically vacuous, analytical language and when he maintains that rationality makes no reference to the internal state of mind. Friedman is like an instrumentalist when he claims prediction is the hallmark of science. Friedman is like a Popperian when he emphasizes falsification.

Since Popper disavows the first three approaches; since instrumentalists disavow the first two approaches; and since logical positivists disavow the first; it is difficult to see how all of Friedman's positions can be identified with any particular philosophy of science. Thus, we conclude that Friedman's positivism is really a collage of various approaches to science. Perhaps <u>collage empiricism</u> is the best way to describe his views. Also, any attempt to render Friedman's view consistent with any particular philosophy of science, inconsistently alters Friedman's own inconsistencies. See Boland (1979) for an attempt to consistently and coherently interpret Friedman.

[3]See previous chapter for a discussion of hedonism, pp. 126-127.

[4]Miller (1976:43) states, "Unlike other fields in economics, these two branches [macroeconomics and monetary theory] traditionally have not made use of models which contain behavioral relationships grounded explicitly in theories of individual optimization. This situation cannot be regarded as desirable."

[5]See Sargent (1976b:65-85), "Testing for Neutrality and Rationality"; "The empirical work done to date does not support out-of-hand rejection of the natural rate-rational expectations hypothesis," (Sargent, 1976b:83).

[6]The relation between behaviorism and gestalt psychology is that of the part to the whole. Behaviorism is a psychology of the elemental response; while geltalt psychology is the theory of a response to a whole configuration of elements. Since the whole may be more than the sum of its parts, behaviorism and gestalt psychology may be quite different. But the problem does not end here. The wholistic configurations of the gestalt view may be elemental relative to some more encompassing theory of mind. A more 'holistic' theory of mind is discussed in Part III--Pribram's holographic theory of brain function and its possible implications for economic rationality.

[7]Since long-term rates are a geometric average of current and expected short-term rates, expected interest rates can be used to explain the shape of any yield curve. If short-term rates are expected to rise, the yield curve would have a positive slope; the converse is also true. If investors expect short-term rates to rise and then fall, a humped shaped curve will result. See Malkiel (1970:7-12).

[8]Poole (1976:7-12) recognizes the limitations inherent in this extremely restrictive assumption: "But they were thought to be serviceable approximations, especially when they yielded good fits in estimated models."

[9]The research of Lucas and Sargent constitutes the foundation for the extension of macroeconomic models with rational expectations to various economic issues such as inflation, unemployment, the business cycle, labor contracts, transitional periods, and optimal control of the money supply. Sargent develops a classical macroeconomic model which incorporates rational expectations, see Sargent (1973, 1976a). He and Wallace (1975) develop the theory in relation to the optimal money supply issue. Sargent (1976b:65-83) also tests his model for rationality and neutrality. Systematic monetary policies that can be fully anticipated may have no effect on real variables such as output and employment.

In a 1972 article, Lucas suggests that the trade-off between inflation and unemployment, if it exists, is a property of the entire economic system. The natural rate or rational expectations hypothesis is too restrictive. Policy parameters are part of the economic system in addition to behavioral relationships: "The natural rate hypothesis restricts the relationship of policy parameters to behavioral parameters. It cannot be tested on a behavioral relationship (Phillips curve, supply function, and so on) alone," (Lucas, 1972:57). In a more recent article, Lucas (1975) supposedly shows that a business cycle can be generated in an equilibrium model where the economic system has imperfect information and is subjected to stochastic monetary shocks.

John Taylor (1975:1013) recognizes that rational expectations assumes, "that people have already learned from their mistaken predictions of the past." He concludes (1975:1017) that monetary policy can have effects during transitional periods when expectations are being formed. Echoing a similar theme, Cyert and Degroot (1974:572) conclude that, "the concept of rational expectations is essentially a black box." Their aim is to describe the learning process as a feedback process from the market. Market feedback continually modifies the prior distribution, where the prior refers to a Bayesian description of learning. Bayesian learning is significant in the transition to equilibrium when transactors cannot move immediately to equilibrium.

Alternatively, Phelps and Taylor (1977) and Fischer (1977) restore the effectiveness of monetary policy in economic systems where wages and prices are predetermined. The existence of labor contracts and relatively stable price lists for periods of three months or longer are aspects of their model. When these features are integrated into a macro model similar to Sargent's, monetary policy can make a difference even when policies are fully, systematically, and correctly anticipated.

[10]This argument is developed more fully in chapter seven, pp. 197-199 and in note 1 above.

[11]One of the implications of rational expectations is that the term structure equation can no longer be considered a fundamental relationship in the transmission of monetary policy to the real sector: "The interpretation of the term-structure equation as a structural relationship is inconsistent with the efficient markets theory," (Poole, 1976: 475). Phillips and Pippenger (1976:13) state the view more fully: "A large amount of empirical evidence indicates that there is essentially no exploitable regularity in the movement of interest rates. If that is correct, and capital markets are efficient, then current interest rates fully reflect all available information, and there should be no systematic relation between current long-term rates and lagged short-term rates."

Rather than doing away with adaptive expectations, perhaps they could be considered in the same way Patinkin suggested Keynes' theory be viewed, as a description of the dynamics of the economy. If this suggestion makes sense, then this is one more reason to consider Keynes' work to be extremely sensitive and dependent on cognitive processes of human choice.

[12]The basic equations of Phillips and Pippenger's (1976:13) model are:

$$L(t)=L(t-1) + z(t) + y(t) \tag{1}$$

$$S(t)=S(t-1) + z(t) + x(t) \tag{2}$$

where $L(t)$ and $S(t)$ are short and long-term rates for period t; $x(t)$ is new information relevant only to short rates, $y(t)$ to long rates, and $z(t)$ to both. An equation can be derived which looks very much like a traditional term structure equation, except that the long rate is not dependent on lagged short rates:

$$L(t)=L(t-1) + S(t) + u(t) \tag{3}$$

One gets equation (3) by solving (2) for z(t) and then substituting into (1). The error term, u(t), is serially independent. Other than the lagged long-term interest rate, equation (3) depends only on information in the current time period.

Chapter 7

ASSOCIATIONISM, RATIONALITY, AND RECENT CRITIQUES OF ECONOMIC SCIENCE

To this point, our argument is that mind and rationality are issues facing monetary theory and philosophy of science. With respect to monetary theory, we support the argument in two ways: (1) We argue, in chapter five, that Keynes is more concerned with psychological realism than many of his subsequent proponents or opponents; and (2) we show, in chapter six, that expectations mechanisms, which increasingly have become a source of debate in recent monetary controversy, constitute an empirical theory of mind. This empirical theory of mind found in expectations mechanisms is quite similar to the associationism of the utilitarians. Our purpose in introducing a theory of mind like associationism is to focus on broader intellectual issues which apparently are manifest in monetary economics.

In this chapter, among other things, we attempt to show why an associationistic, empirical theory of mind is so restrictive. In addition, this restrictiveness is then juxtaposed against the approaches of some economists who have different approaches to the problems of mind (expectations) and rationality. We shall show that those economists who oppose notions of expectations and rationality like those found in rational expectations have one thing in common: <u>They all implicitly reject associationistic approaches to mind and rationality</u>. The economists we have in mind are G. L. S. Shackle, H. A. Simon, and Nicholas Georgescu-Roegen. Furthermore, we shall show how much the approaches of these economists have in common with our structural philosophy of science. Since the concept of rationality in our structural philosophy of science is non-associationistic, we expect the views of these economists to have something in common with our structural philosophy of science presented in Part I. The restrictiveness of associationism is considered first; the non-associationistic views of Shackle, Simon, and Georgescu-Roegen are considered second; finally, the views of these economists are compared with our structural view of science.

ASSOCIATIONISM, POSITIVISM, AND RATIONAL EXPECTATIONS

In the preceding two chapters, we maintain that mind and rationality are the most fundamental issues underlying recent monetary controversy and the Keynesian Revolution. Mind and rationality can be viewed as: (1) relating to the logic of externally observed behavior or, (2) relating to the conscious formation of abstract ideas. Concerning the first position, within a positivistic conception of science and economics, the issue of mind usually is dropped as being unscientific; rationality becomes a behavioral consistency concept. This leads to a caricature of rational expectations as the most positivistic approach to contemporary monetary theory. All concern for mind and economic expectations is replaced with behavioral relations; which for the rational expectations theorist, can only be approximated statistically in the aggregate. Concerning the second position, if expectations are viewed as empirical mechanisms which give rise to abstract ideas, then expectations mechanisms may constitute a theory of mind. Rationality, if interpreted in the context of a theory of mind, then validly may be construed as an internal thought process which is relevant to actual human activity.

At this point a question arises as to which point of view is correct; or alternatively, can both views be maintained simultaneously? In the following paragraphs, we shall hold that only the first point of view can be maintained within a positivistic conception of science. Furthermore, the second view, if the history of psychology is a valid guide, leads back to the first. In other words, historically, an associationistic, empirical theory of mind leads eventually to behaviorism--the abandonment of mind as a scientific issue. In this regard, the rational expectations approach may be pre-behavioristic.

There is some evidence to suggest that rational expectations theorists maintain a conceptual dualism in some respects similar to that of Keynes and Walras. As part of their approach to monetary theory, Keynes and Walras make their theory dualistic. Keynes distinguishes independent from ultimate independent variables, while Walras distinguishes barter and monetary tatonnements. Rational expectations theorists similarly make a distinction between non-random, static factors and random, dynamic factors. Non-random factors can be approached scientifically with a deterministic, behavioral point of view; while random, unpredictable factors need to be approximated statistically. But there is an important difference. In contrast to the dualistic views of Walras and Keynes, the random-non-random dualism of rational expectations not only makes dynamic factors exogenous to an equilibrium model, but also external to economic science. By maintaining that a theory of expectations for dynamic monetary phenomena must be a statistical one, the theory of expectations thus lies outside the domain of science and economic science.

But in practice the dualistic point of view in rational expectations effectively retains a dualism similar to the views of Keynes and Walras.[1] While rational expectations theorists seem to place dynamic factors like mind and expectations beyond the purview of science and economics, the descriptions of such dynamic processes seem central to their understanding of economic processes. In other words, some important interpretations of rational expectations are in terms of anticipation, thinking, decision-making etc. This suggests that dynamic mental phenomena have worked their way into the considerations of rational expectations theorists. For example, an official of one of the Federal Reserve Banks which has supported a significant part of the research on rational expectations, clearly gives a mentalistic interpretation to rational expectations. Mark Willes (1978:2), President of the Federal Reserve Bank of Minnesota, puts it this way:

> This appraisal [of the current economic situation] is more optimistic than many because it is based on some insights that are surprisingly new to macroeconomists, although they have been self evident to the average mortal. One of these revolutionary 'new' insights is that what people think meaningfully affects what they do. Thus, an anticipated change of policy can have results quite different from an unanticipated one.

Then Willes, in the same paragraph, turns his optimism concerning rational expectations into a criticism of more traditional approaches to macroeconomics:

> Most economists, if pressed, would concede that people do exist. Some would even admit that people have wills of their own, however inconvenient. But these humanistic values find no home in mathematical [Keynesian structural] models of the economy. Most forecasts of the impact of a

> new economic policy are based on the implicit assumption that people can be repeatedly fooled and don't act in their own best interests.

Walter Guzzardi gets the same impression from rational expectations theorists. The rational expectations approach to macroeconomics is supposed to be a highly sophisticated way to study complex human activity. Guzzardi (1978:4) states:[2]

> His colleagues at the University of Chicago describe Robert E. Lucas. . . as a 'superb technical economist.' At the core of Lucas' rational-expectations philosophy, though, lie human rather than mathematical factors. He tries to understand and explain how people react in real life when faced with racing inflation.

If the mentalistic interpretations are as significant as the preceding quotes suggest, then the nature of such talk concerning mind should be formalized into a theory of mind. This would systemize what, at this point, amounts to ad hoc addenda to a rather restrictive approach to science and economics. But if our analysis in the previous chapter is correct, mechanisms of expectation in economics which are realistically interpreted as a theory of mind take us back to associationism--the empirical theory of mind of the utilitarians. To associationism, rational expectations theorists add one important dimension. They generalize the mechanisms of associationism from individuals to society. In other words, expectations mechanisms in economics constitute an associative psychology of individual minds, while the social psychology of such minds working together is largely indeterminate and captured in rational expectations models.[3]

Treating expectations mechanisms as a theory of mind offers some advantages. Not only does it formalize the ad hoc mentalistic interpretations of expectations by proponents of rational expectations, but it also may offer some insights into where rational expectations might lead. If expectations mechanisms in economics follow the same course as associative mechanisms of mind, then all concern with mind as a scientific issue eventually might be abandoned in economics. In other words, if we take the conceptual dualism of the rational expectations approach as seriously as possible, this leads us to formulate expectations mechanisms as a theory of mind. But the implicit mechanisms of mind in economic expectations, being so similar to those of associationism, might collapse along with associationism. Thus, even the dualistic point of view that is prevalently used to interpret the rational expectations view may collapse for theoretical and historical reasons.

Nowhere can the collapse of a scientific, empirical theory of mind be seen better than in the rise of scientific psychology. Scientific psychology developed contemporaneously with the rise of economic science. Around the turn of the century, psychology was transformed from the study of the conscious mind into the study of externally observed behavior. In other words, associationism was transformed into behaviorism. Associationism ceased being a theory of the elemental interaction of the ideational contents of consciousness and became a theory of the elements of behavior--stimulus and response. Boring (1950:620,621) states:

> There have been many. . . [psychologists] who have argued that is is unprofitable to study consciousness directly and that better data for the same problems are obtained by limiting research to the study of behavior.
>
> . . . .
>
> Any experimenter who knows fully what went on in his introspective experiment can transform the data of consciousness into the data of behavior, a practice that has been called operational reduction.

Behaviorists thus maintain that the study of consciousness is unprofitable. Rather, consciousness and behavior are presumed to be systematically related. The study of behavior is both elemental and associationistic (Boring, 1950:643). The elements of behaviorism are the stimulus and the response. The stimuli in the individual's environment are associated in a uniform way to give a predictable response. Boring (1950:644) even suggests "the conditioned reflex of Pavlov as the behaviorist's substitute for association."

As discussed in chapter two, behaviorism can be described using all of the attributes of the positivistic approach to science. Not only is behaviorism anti-metaphysical, anti-mentalistic, reductionistic, and physicalistic; but it is also associationistic. This means it has a black box theory of mind, which means that mind is an irrelevant issue.[4] Concerning mind and consciousness, scientific-positivistic psychology must remain agnostic. Psychology, if it is to be like the natural sciences, must study only behavior. In short, behaviorism becomes the psychology consistent with logical positivism. Boring (1950:655) summarizes:

> There began a movement which Feigl later named <u>logical positivism</u>, which in science (including psychology) became <u>physicialism</u>, because it reduced all scientific language to the communal language of physics, and which in psychology became <u>behavioristics</u> because the psychological operations are all observation of behavior. Even the mentalistic entities, when they are reduced to the physical operations by which they are observed, reduce to behavior. This is the <u>reductio ad actionem</u> of behavioristics.

Returning to economics, we must assess the importance of the behavioral turn in psychology for rational expectations. If the mathematical expecations and autoregressive processes in rational expectations are construed in light of logical positivism rather than utilitarianism, then economic expectations mechanisms do not necessarily relate to human cognitive learning in an inflationary context. Positivistically interpreted, what rational expectations theorists are doing is generalizing the conditioned reflex from individual to aggregate behavior. If any learning occurs at all in the rational expectations view, it is passive in true Pavlovian fashion. In the aggregate, consumers, producers, and workers adapt their economic behavior in a rapidly changing inflationary economy because they painfully are made aware of the consequences if they do not. Maintaining past economic behavior from pre-inflationary periods into inflationary ones would not be done even by an unthinking automaton.

The policy implications which follow from this narrow view of human learning in economic situations are rather significant. Once an aggregate reflex of inflation is conditioned by several years of experience with inflation, somehow transactors generalize the response to all potential inflationary sources including monetary and fiscal policy. Perhaps monetary or fiscal expansion is one of the concomitants of the original transition to a significant, positive rate of continuing inflation. What this means, paraphrasing the rational expectations view, is that a higher rate of monetary expansion stimulates a counterveiling response on the part of all transactors on average. Figuratively speaking, when the bells of monetary and fiscal expansion are rung, actively or in accommodation by the Fed and/or the administration, transactors merely salivate in anticipation of another round of higher nominal wages and prices.

Our message should be quite clear at this point. Nowhere is it more apparent that positivists have abandoned any claim to realism than concerning rationality, mind, and expectations. Logical positivists and Milton Friedman seem quite clear on this point. Furthermore, the history of associationism illustrates the dilemmas an empiricist faces in studying mind through observable behavior. Mind gets rejected in favor of behavior. But rational expectations theorists somehow seem to believe that they can both maintain the view of positive economics and interpret expectations realistically. How this can be done and still maintain a degree of intellectual integrity within positive economics remains unexplained. Evidently previous considerations of mental phenomena by positivists has escaped the attention of rational expectations theorists.

At this point, one can begin to understand why, in chapter five, we interpreted rational expectations so restrictively. Positive economists have conceptually, scientifically, and theoretically abandoned the real world. However, rational expectations theorists seem to believe that somehow they can get back to the real world in which we live. But we cannot let them maintain any relevance to the real world without making their case. Rational expectations theorists need to show why the reductionistic history of positive economics, associationism, and logical positivism should not be taken seriously. Our criticism of rational expectations as being extremely restrictive rests on a broader sense of the history of science, psychology, and economics than seems apparent in the rational expectations point of view.

To summarize, if economic expectations mechanisms are associationistic, then all attempts by rational expectations theorists to interpret their model dualistically by giving a dynamic interpretation to expectations mechanisms in a static model fails. Furthermore, our awareness of the utilitarian empirical theory of mind, i.e., associationism, suggests that no empiricist can consistently and simultaneously talk of both mind and behavior. Minds are intangible entities which pose significant problems for the empiricist who requires direct or instrumentally aided observation. In psychology, behaviorists resolve this problem by making psychology deal only with observable behavior. In economics, rational expectations theorists unwittingly follow the same strategy. Behaviorism is a scientific psychology without a theory of mind, while rational expectations is a "scientific" approach to economic expectations without a theory of cognitive learning and anticipation.

NON-ASSOCIATIONISTIC CRITIQUES OF ECONOMIC SCIENCE AND RATIONALITY

Despite having gone to great lengths to demonstrate the dilemmas one encounters when considering expectations realistically within a narrow, empirical conception of science, the restrictiveness of our interpretation of rational expectations might simply be rejected by those who, by force of habit, interpret rational expectations less restrictively. To give added force to our argument that economic expectations mechanisms are associationistic in the behavioral but not the cognitive sense, the views of some other economists on rationality and/or expectations will be considered. Philosophically, methodologically, and economically these economists write with greater intellectual and historical depth than rational expectations theorists. The economists whose work we shall review are G. L. S. Shackle, H. A. Simon, and Nicholas Georgescu-Roegen. It is quite apparent that in their criticisms of contemporary economic science, each rejects the validity of purely associationistic approaches to rationality and expectations. Additionally, an element of dualistic or even pluralistic tension remains in the views of each. Since they all reject associationistic approaches to mind and expectations, the tension of a dualistic or pluralistic view cannot be rejected by a rejection of associationism as a theory of mind. The lack of conceptual unity on the part of a dualistic or pluralistic conception of economic science will be explicitly considered in part three of the next chapter.

Shackle: The Incompatibility of Expectation and Economic Rationality

Although rational expectations theorists espouse restrictive conceptions of economic science and rationality and then attempt to make them fit monetary phenomena, G. L. S. Shackle recognizes the limitations of economic science and rationality and takes such limitations seriously. Because Shackle has such a unique way of stating his case, we quote him frequently in the following section. We begin by noting Shackle's views on economic science, rationality, and expectations. Out of these considerations come his assessments of equilibrium theorizing and Keynes' General Theory. Finally, we demonstrate that Shackle, quite unlike rational expectations theorists, is really concerned with nonassociationistic decision-making processes for economics and monetary theory. Thus, the dualisitc point of view which collapses within the rational expectations approach remains with Shackle as it does with Keynes.

Perhaps one of the most caustic critiques of contemporary economic analysis is Shackle's (1972) Epistemics and Economics. From this work, one gets the impression that much of what passes as bonafied economic research is quite irrelevant to the economic problems faced by market economies of the Western world. Such a point of view leads Shackle to reconsider the fundamentals of economic science. In Shackle's view, economics is a science which inappropriately applies the methods of mechanics to all economic phenomena. Economists are not really concerned with the problem of knowledge facing individual transactors as such, but rather with external circumstances that are perfectly known by an external observer. What most economists with a mechanistic approach ignore are cognitive dimensions of individual decision-making. Shackle's term for cognition is epistemic. Epistemic problems ignored by theorists are "ignorance, uncertainty, risk, deception, delusion, perception, conjecture, adaptation, and learning," (Coddington, 1975: 151). Shackle's thesis is that the problems of knowledge require time for resolution. Reason as developed from its classical roots has been "sure, safe, even in a sense simple," (Shackle, 1972: xii). For Shackle (1972; xi and 254) time is alien to reason. The usual approach taken by theorists is to consider information and epistemic issues as complications which merely modify static theory. However, Shackle sees a basic clash (1972; xvi):

> If there is a fundamental conflict between the appeal to rationality and the consideration of consequence of time as it imprisons us in actuality, the theoretician is confronted with a stark choice. He can reject rationality or time.

and again in Shackle (1972:3):

> Economic theory took on a character belonging to the manipulable, calculable, external world of things, not the world of the conscious mind whose being consists precisely

> in the endless gaining of knowledge. Knowledge and novelty, the essential counter-point of conscious being, was given only a casual and subsidiary role.

Economic rationality, in Shackle's view, cannot apply to real-world individuals and situations because complete knowledge of circumstances is impossible. Economic transactors are faced with the task of framing economic strategy on the basis of incomplete knowledge and information. Expectation and imagination are required in actual affairs, in addition to reason. Imagination and expectation point to an existential sense of duration which is largely irrelevant to equilibrium models.[5] Most modern economic theorists abstract from actually experienced time by making time just another spatial dimension. Expectations resulting from the use of imagination to consider the future are the origin of the indeterminancy economists observe in economic phenomena:[6] "Expectation undermines the view of conduct as purely rational," and "Expectation is not rational," (Shackle, 1972: xvii and xv). "The meaning is that rationally, fully informed equilibrium is excluded by the denial to us of anything but fragmentary suggestion of what will be the sequel of today's efforts and plans," (Shackle, 1972: xv).

For Shackle, economic theory as a rational system (in the conventional sense) is falling apart. This dissolution of rational economic analysis results when money is first considered by economists. Money implicitly has a time orientation. Shackle points to Knut Wicksell as the first to show that "money was different," (Shackle, 1972:xii). But the dissolution of the restrictive system of rational economic theory is also apparent in Keynes' General Theory. It is a time orientation to which Keynes is so sensitive which creates such difficulty for contemporary economists like rational expectations theorists. Realizing that the restrictiveness of the concept of rationality in economics has consequences for monetary theory, the term irrationality does not intimidate Shackle. Irrationality might be descriptive of significant aspects of economic activity, if the referenced concept of rationality is unduly narrow. Thus, Shackle (1972:160) suggests Keynes is outside the domain of economic rationality:

> The word liquidity was made common coin of discussion among economists. . . by John Maynard Keynes, for whom it was an indispensible and central stand in an argument which ended by rejecting the claim of economic conduct to be capable of rationality.

Shackle's realization that knowledge and rationality in economics are extremely restrictive concepts leads him to interpret Keynes dualistically. His dualistic interpretation of Keynes is not at all unlike the one developed previously in chapter five. This conceptual dualism which Shackle sees in Keynes' work is due to Keynes' concern with the processes of decision-making in humanly experienced time. In Shackle's (1972:208) own words:

> The General Theory is, throughout, in two minds. It turns instinctively towards stable functions, uninterrupted movements along curves, underemployment equilibrium, secular stagnation, step by step declension. . . Yet the message spelled out by all this creaking semaphore is that intended (designed *ex ante*) investment is a law to itself, dependent (if at all) on too elusive and involved a skein of subtle influences, too eagerly clutching at the straws of suggestion whirled along by 'the news,' to be ever captured in any intelligible, let alone determinable, equation.

For the traditionally trained, empirical economist, Shackle's overt criticism of monetary theory in general and his implicit criticism of rational expectations in particular, may be difficult to comprehend. An economist who maintains that irrational human activity is economically significant, effectively has left the domain of economic science.

But having reviewed associationism as a theory of mind, we have another theoretical domain in which to fit Shackle's criticism. Shackle effectively rejects the mechanical, associationistic theory of mind implicit in expectations mechanisms in economics. Shackle's rejection of associationistic mechanisms of mind is seen in his insistence on the incompleteness of knowledge, in his insistence on the limitation of economic rationality, in his emphasis on imaginative thought processes, and in his dualistic interpretation of Keynes.

More generally, if there were any doubt about Shackle's rejection of associationistic approaches to mind and decision-making, such doubts are resolved with his consideration of probability. The relative-frequency interpretation of probability exemplifies one aspect of the third law of association (Mill, 1865, I:307-308, 326). Shackle (1972:16) explicitly rejects probability as an aid to reduce uncertainty: "Probability has been seized as an incantation to perform what reason declares impossible, the prescription of rational conduct in face of ignorance concerning the outcome of rival courses of conduct." Certainly this perspective rules out the use of autoregressive mechanisms and other statistical procedures as proxies to cognitive expectations like those found in the rational expectations approach. For Shackle, probabilistic approaches to expectations direct attention to the wrong type of questions. Concerning future investment alternatives, there is no sample of investment projects which can be used to form relative frequencies. Presumably, similar considerations apply to unanticipated inflation and to unanticipated changes in the rate of inflation. In either case, Shackle (1972:407) maintains that future outcomes of investment alternatives or rates of inflation are highly conjectural:

> Probability does not supply knowledge out of unknowledge. . . We rely on it because there is no substitute. When we are finally confronted with irremediable uncertainty, with unknowledge from which there is no ex ante escape, the only expedient of the human mind is to turn it to advantage by seeing it as freedom, a freedom for the imagination.

Shackle's rejection of economic rationality and traditional methods of scientific inquiry is only partial. He effectively rejects techniques of associationistic behavioral economics where creative human thought processes are most significant--in financial and investment decisions. Money and capital are time oriented, they look forward. Shackle (1972:446-447) believes this future orientation of real world economic processes requires imagination and expectation in addition to the processes of reason and rationality with which economists traditionally have been concerned:[7]

> A theory which denied men reason would be an abdication from our claims of humanity; general exchange is the operation which gives unity and definition to our field; the market is the source of that knowledge which makes rational conduct possible; equilibrium names the result of the market's prereconciliation of endlessly diverse tastes confronted with an infinite diversity of circumstances. . . What the theory neglects is the epistemic problem, the problem of how necessary knowledge on which reason can base itself is to be gained, the problem of what to suppose that men will do when time's sudden mockery reveals their supposed knowledge to be hollow.

In summary, in his critique of contemporary economics, Shackle rejects the adequacy of accepted notions of economic science, rationality, expectation, and the relevance of equilibrium theorizing. Economics depends too much on a mechanical conception of science which relies on external circumstances. Internal or cognitive dimensions of economic processes are systematically ignored. But something more fundamental separates Shackle's views on rationality, expectation, and economics from those of rational expectations theorists. Shackle effectively maintains the relevance of non-associative decision-making for economic processes. Thus, Shackle and rational expectations theorists implicitly differ concerning the nature of a theory of mind and its relevance for economics.

Simon: From Substantive to Procedural Rationality

Shackle's views on rationality and expectations leave one with the same sense of tension between incompatibles as Keynes' General Theory. Whereas Keynes attempts to put supposedly "irrational" psychological propensities in a theory of macroeconomic equilibrium, Shackle effectively separates non-rational from rational psychological processes of decision-making. In his view, both types of processes are necessary, particularly to explain monetary phenomena. A portion of the internal tension in the works of Keynes and Shackle can be resolved by looking at the research of other economists who attempt to formulate criteria of rationality for the cognitive psychological processes of human decision-making. H. A. Simon and Nicholas Georgescu-Roegen develop views of rationality which apply to the Keynes-Shackle-type monetary phenomena. Their views on rationality exhibit an awareness of the limitations of associationistic approaches to human activity. However, Simon's approach is somewhat enigmatic. He recognizes that behavioral theories cannot cope with cognitive processes such as learning, problem solving, and concept attainment (Shackle's epistemic processes of imagination and expectation could be added to this list). Yet, he proposes to study these cognitive processes by theories of games such as chess and computer algorithms which simulate sequential processes of thought.

This reliance on artificial intelligence devices, particularly on computer simulation, harks back to associationism. Algorithms are pre-structured responses to the data of the external environment. Computers merely enlarge the associative capacity of pre-determined or pre-structured responses.

Simon's ideas on rationality and cognitive psychology are found in a recent article (1976) and were presented by Simon (1978) to the economics profession in the 1977 Ely Lecture. Simon distinguishes between substantive rationality and procedural rationality. Substantive rationality implies goal maximization on the part of a substantively rational economic actor. "Behavior is substantively rational when it is appropriate to the achievement of given goals within the limits imposed by given constraints," (Simon, 1976:130). Substantive rationality frees economics from any dependence on psychology (Simon, 1976:131).

Alternatively, procedural rationality focuses on processes of reasoning or thinking. "Behavior is procedurally rational when it is the outcome of appropriate deliberation," (Simon, 1976:131). Procedural rationality explicitly entails recognition of internal thought processes, processes which substantive rationality ignores. The time domain of decision-making is taken into account in procedural rationality. "Man's equipment for thinking is basically serial in organization. That is to say, one step in thought follows another, and solving a problem requires the execution of a large number of steps in sequence," (Simon, 1976:135).

Simon's aim is to shift the focus of theory formulation in economics from concern with optimization to "good solutions" applicable to serially ordered decision processes (Simon, 1976:133). His position is that economics implicitly has already shifted its concept of rationality from substantive to procedural rationality. Evidence of greater concern for processes of decision-making is found by Simon (1976:141) in the literature on oligopoly and imperfect competition, in the operations research literature, in the expectations literature, and in the literature on artificial intelligence (chess and computer algorithms).[8]

Complications arise from the schema which Simon presents. Many of the very complex, serially ordered algorithms which have developed from the areas of study mentioned above, require a great deal of stored information. Thus, Simon's procedural rationality has a Shackle-type epistemic dilemma; how can such information be obtained or adequately approximated before it is available? A second difficulty is that not all human thought processes can be approximated by artificial intelligence algorithms and procedures: "There are still many areas of decision--particularly those that are ill structured--where human cognitive processes are more effective than the best available optimization techniques or artificial intelligence methods," (Simon, 1976:144). This statement shows Simon's awareness of the limitations of associative mechanisms in the study of human intelligence. The use of computers drastically reduces the time that a human mind would need to work through a predetermined algorithm. Computers accelerate the processes of successive contiguity. Examples of ill-structured situations are the initial creation of computer algorithms or the formulation of new investment and financial criteria.

There are three consequences which result from Simon's analysis of human cognition and economics. One consequence is that substantive rationality is applicable only in the most simple situations. "We can expect substantive rationality only in situations that are sufficiently simple as to be transparent to this mind," a mind dominated by "skills, behavior patterns, problem solving repertoires, and perceptual habits," (Simon, 1976:144). Second, where there is genuine uncertainty, a whole range of actions are available to reduce susceptibility to uncertainty. Hopefully, forecasts can be improved, buffer mechanisms can be established, market power can be used to segment markets, and the range of alternatives in high risk situations should be enlarged as much as possible (Simon, 1976:143-144). Third, procedurally rational activities (to which serial decision-algorithms are inapplicable) are inherently unpredictable.[9] Prediction of procedurally rational activity requires an observer to know many things: a transactor's complete situation, the information available to him, how it is represented, computational algorithms available, and the transactor's ability to synthesize novel, alternative algorithms. Concerning prediction and human minds, Simon (1976:146) maintains that:

> Decision processes like all other aspects of economic institutions, exist inside human heads. They are subject to change with every change in what human beings know, and with every change in their means of calculation. For this reason the attempt to predict and prescribe human economic behavior by deductive inference from a small set of unchallengeable premises must fail and has failed. . . Economics will progress as we deepen our understanding of human thought processes.

In short, Simon, like Shackle, recognizes the restrictiveness of associationistic approaches to decision-making in economics. But Simon is much more optimistic that economists are giving attention to the restrictiveness of associative concepts of decision-making and economic rationality. Simon goes even one step further and formulates a second concept of rationality for complex, temporal decision-making processes.

This concept of rationality Simon calls procedural rationality. Some procedurally rational activities can be simulated with computer algorithms, while others cannot. Those activities that cannot be so simulated are unpredictable. Such procedurally rational and unpredictable aspects of decision-making seem to be what Shackle has in mind. Thus, Simon's concept of procedural rationality helps us to further understand what Shackle attends to and what rational expectations theorists ignore.

Georgescu-Roegen: Third Order Rationality

In the preceding paragraphs, the views of Simon and Shackle on rationality are related to two concepts. They both recognize limitations to associationistic-behavioral economics and they recognize a distinction concerning the degree of complexity separating behavioral from non-behavioral theories. That is, non-associationistic, non-behavioral theories address phenomena which exhibit a greater degree of complexity than do behavioral theories. In a major reconsideration of the philosophic and scientific foundations of economics, Nicholas Georgescu-Roegen essentially uses the two concepts of associationism and complexity as criteria for a theory of rationality. For Georgescu-Roegen differences in the complexity of the phenomena imply different concepts of rationality. Associationistic approaches to mind are not capable of addressing the complexity of human decision making. This calls for a new conception of rationality.

Nicholas Georgescu-Roegen, in his The Entropy Law and the Economic Process (1971), develops a view of rationality that is in many respects similar to Simon's and compatible with the views of Shackle.

Like Shackle, Georgescu-Roegen is concerned with the influence of classical mechanics on economics. He (1971:1) tells us, "the fiction of homo oeconomicus. . . is tantamount to saying that in his economic life man acts mechanically." Mechanics is concerned simplistically with matter, not with emergent properties of matter (chemical, thermodynamic), nor with the emergent properties of life, mind, and society (biology and the social sciences), (Georgescu-Roegen, 1971:114-117).[10] The nonmechanical properties of matter--life, and mind--give evidence of novelty. These properites cannot be deduced from the simple constituent components of each.

Georgescu-Roegen uses a consideration of novelty to postulate three types of rationality: first, second, and third order rationality. Each type of rationality varies according to the degree of novelty the phenomena under consideration display.[11] Phenomena of first order rationality "can be discovered by the tip of a pencil doing some algebra or logistic calculus on paper as in the case of mechanics. Those phenomena that can be so deduced. . . can be referred to as rational phenomena of the first order," (Georgescu-Roegen, 1971:117). It is evident from Georgescu-Roegen's view of economic man quoted above that economic rationality in the sense of ends and means is equivalent to first order rationality.

Second order rationality refers to phenomena which are not deducible from other phenomena. Chemical compounds display a degree of novelty; properties of compounds are not derivable from the properties of constituent elements. However, the novelty of compounds is uniform; once such properties are discovered, they have an almost absolute degree of permanence. The second time a chemical compound is synthesized, the chemist no longer is confronted with further novelty. The mental chemistry in John Stuart Mill's Laws of Association, discussed previously, is an example of second order rationality. If the associative mechanisms of expectation in rational expectations are interpreted cognitively, then the concept of rationality in rational expectations would best correspond to second order rationality. The point is that some types of economic behavior may give evidence of changing more rapidly than others. A distinction of first and second order rationality might capture this difference.

The use of creative human intelligence in unstructured situations is an example of third order rationality. Third order rationality refers to phenomena which manifest novelty in greater degree than novelty exhibited in chemical compounds. A new dimension of novelty is apparent in the "organic" and "superorganic" domains since the permanence of the physico-chemico level is absent (Georgescu-Roegen, 1971:117).

Georgescu-Roegen clearly rejects any type of associative mechanism to explain the formation of human thought, or third order rationality. He (1971:83-94) recognizes that human processes, even thinking, have been conceived in terms of machines from Rene Descartes to John von Neuman and H. A. Simon (Georgescu-Roegen, 1971: 87, n. 102). The most recent example of using machines to study human thinking is the computer.[12] Computers are better than the human mind in some tasks, but not in others:

> When everything is said and done, it is seen that all proofs of the 'computer performance = the human thought' involve the eternal verbal swindle. 'Thinking' is only what computers do (or may do on paper), not what the primitive computer, the human brain, does in fact.
>
> . . . .

> The computer does transcend some of the intellectual limitations of its designer, but not his intelligence in the relevant meaning of the term.
>
> . . . .
>
> They [computers] are morons because they cannot Think (Georgescu-Roegen, 1971:86, 88, and 92).

Two suggestions for a more comprehensive view of the nature of economic science emerge from Georgescu-Roegen's work. One suggestion is that economics should recognize a greater interconnection with biology. Georgescu-Roegen points to Marx, Malthus, Marshall, and others who have rejected the isolation implied by Robbins' ends-means definition of economics (Georgescu-Roegen, 1971:318). Economists should focus on the evolutionary pace of economic change: "The question is why a science interested in economic means, ends, and distribution should dogmatically refuse to study also the processes by which new <u>economic</u> ends, and new <u>economic</u> relations are created," (Georgescu-Roegen, 1971:320). A second implication is that sciences concerned with phenomena which exhibit third order rationality "would be continuously 'under construction,'" (Georgescu-Roegen, 1971:322). Continuous construction of the logical structure of a science limits the applicability of theoretical formulations of the sciences; third order rationality implies a degree of indeterminancy which scientific methods cannot reduce (Georgescu-Roegen, 1971:170-172, 324-328).

Briefly, in contrast to macrotheorists who reduce expectations and rationality to an associative, behavioral theory of maximization; Shackle, Simon, and Georgescu-Roegen use rationality and expectations as a way to reconsider the nature of economic science. They recognize the limitations of mechanistically behavioral or associative theories (like rational expectations) for addressing the economic consequences of human intelligence in highly novel situations. None of them consider the psychological aspects of "ill-structured" (Simon), non "pre-reconciled" (Shackle), or "novel" situations (Georgescu-Roegen) to be irrational. For Georgescu-Roegen, such situations exhibit third order rationality; for Shackle they, seem to be non-rational; for Simon, they are procedurally rational. Ultimately, their consideration of rationality and expectation alters their conception of economic science.

CONCEPTS OF RATIONALITY AND CRITIQUES OF ECONOMIC SCIENCE

Our argument to this point is that mind and rationality are the fundamental issues generating continuous monetary controversy since Keynes. In this sense, the Keynesian Revolution is yet unfinished. Apparently, Keynes maintained greater psychological realism than many of his subsequent interpreters. This includes some neo-Keynesian proponents as well as obvious opponents to Keynes' view of the economy. Of contemporary opponents to the legacy of Keynes, perhaps the rational expectations theorists raise the strongest challange. But our assessment of the associationistic nature of expectations mechanisms suggests that the rational expectations view is simply irrelevant to the decision-processes Keynes had in mind. We find reinforcement for our argument of the irrelevance of rational expectations to Keynes in the approaches of others to the issues of mind, expectation, and rationality. Shackle, Simon, and Georgescu-Roegen definitely embrace non-associationistic concepts of rationality and mind, in marked contrast to the rational expectations view. This means that a source of disagreement in economic research among economists is really a disagreement over an appropriate theory of mind and rationality.

Before considering the problem of mind in more detail in the next chapter, it would be helpful to pull the various concepts of rationality together. This can be achieved by returning to the concept of science developed in part one, a structural philosophy of science. This structural view of science not only offers a conception of nonjustificational rationality; it also offers a consideration of the complexity of phenomena which is so apparent in the views of Shackle, Simon, and Georgescu-Roegen. If these approaches can be interpreted in light of a structural view of science and nonjustificational rationality, then a significant result would be achieved. Our structural philosophy of science would not only be a more comprehensive philosophy of science from which to consider positive economics, but also it would provide a conceptual framework for unifying the criticisms of the dissenters. Thus, a structural concept of science would provide a way of achieving a tremendous intellectual economy and conceptual unity.

Our structural philosophy of science is an epistemological position with ontological implications. Knowledge is tentative and conjectural; yet scientific theories seem to be the best approximations to structural relations in the real world. Taking theoretical disunity and disciplinary specialization seriously; the scientific endeavor as a whole seems to suggest distinct phases of existence. Figure 4 (p. 107) illustrates the phases of existence which can tentatively be postulated based on what is known about science, and indirectly through science what is known about the real world. Rather than conceiving of Figure 4 as a hierarchy, the concept of nestedness might be preferred. Each phase of existence seems to be related through invariance relations which define the nesting relations.

The scheme of things presented in Figure 4 provides the type of criteria and makes the kinds of distinctions Shackle, Simon, and Georgescu-Roegen suggest in their critiques of economic analysis. With mind as a separate but nested phase of existence, the operations of mind are separated from those of inert matter and from those of simpler-than-human life-forms. Associationistic approaches to the study of economic phenomena implicitly abstract from these approaches, if the abstractive reduction is explicitly recognized. But man studied as a simple object in motion, or as a simple life-form without a mind, remains less than man.[13] These strategies may be successful in explaining a vast amount of repetitive human economic activity. However, the question of the economic consequences of manifestations of human intelligence has never been answered. Only a few have dared to ask.

From another perspective, a structural philosophy of science represents a desirable intellectual focal point. A structural view of science, as developed in chapter four, incorporates a richer conception of rationality. Nonjustificational rationality is a critical conception of rationality. Data and information are relevant but not decisive in explaining the decision-making processes facing the scientist. In addition to the empirical data, the scientist is concerned with the realism and coherence of his theories and the adequacy of his empirical framework (facts). Also, an assessment of the results of an experiment proceeds at different levels of abstraction--within, between, and beyond theories. This means decision-making is a multi-dimensional activity for the researching scientist. Any attempt to explain the growth of science solely in terms of one of these dimensions, say the experimental situation, obviously would lead to problems of inadequacy. In short, nonjustificational rationality is a non-associationistic concept of rationality because of the multi-dimensional nature of criticism and because of the many levels of abstraction important in understanding the growth of science.

There is no reason to suspect that nonjustificational rationality is limited to highly creative and intelligent scientists. Transactors in a monetary economy, as indicated by a nonjustificational conception of economic rationality (Figure 5, p. 132), may also exhibit a highly complex, multi-dimensional process of making decisions. Indeed, from the perspective of our nonjustificational view of economic rationality, rational expectations theorists do not assume enough rationality.[14] Confining decision-making to data available to an external observer is hardly the "rational" thing to do. Since Shackle, Simon, and Georgescu-Roegen reject the adequacy of justificational, associationistic concepts of decision-making and rationality in economics, their views exhibit a great deal of similarity to our nonjustificational view of economic rationality. Nonjustificational rationality, as developed in chapter four and reinterpreted for economic situations in chapter five, may be a more precise and broader notion of rationality. Nonjustificational rationality is an intellectual focal point from which recent critiques can be synthesized and from which other reductionistic, associationistic conceptions like the rationality of rational expectations can be critiziced.

The various concepts of rationality which have been discussed are compared in Figure 8. Each concept of rationality is related to the phase of existence to which it ostensively applies, according to the degree of complexity permitted by the concept of rationality. Conceptually, nonjustificational rationality is the most fundamental and best worked out version of rationality applying uniquely to the domains of mind and society.[15] Whether mind and society are separate phases of existence is a question which will not be considered at this time. Georgescu-Roegen's third order rationality and Shackle's non-rational thought processes seemingly correspond to a nonjustificational conception of rationality; Simon's conception of procedural rationality does not rule out simple sequential learning processes observed in non-humans. Thus, it is somewhat more ambiguous than nonjustificational rationality. The concept of economic rationality in rational expectations as a predictable maximizing process corresponds to the domains of matter and non-human organisms. The realism of the mind and society is unpredictable and thus irrational for the rational expectations approach.

There may be a sense in which the rational expectations approach actually enlarges the domain of economic rationality. If Georgescu-Roegen is correct in restricting economic rationality to first order rationality, then rational expectations theorists moderately enlarge the domain of economic rationality. Georgescu-Reogen maintains that economics is modeled on static physics. If this is the case, then rationality is no more complex than a notion embracing the patterns of movement for inert objects. Then economics would be social mechanics

<table>
<tr><th rowspan="2">Concepts of Rationality by Phase of Existence</th><th colspan="5">Concepts of Rationality by Point of View</th></tr>
<tr><th>Structural Philosophy of Science</th><th>Georgescu-Roegen</th><th>Simon</th><th>Shackle</th><th>Rational Expectations</th></tr>
<tr><td>Society</td><td rowspan="2">Non-Justificational</td><td rowspan="2">Third Order</td><td rowspan="3">Procedural</td><td rowspan="2">Non-Rational</td><td rowspan="2">Irrational</td></tr>
<tr><td>Mind-Psychic</td></tr>
<tr><td>Life-Biotonic</td><td rowspan="2">Justificational</td><td>Second Order</td><td rowspan="2">Rational</td><td rowspan="2">Rational Maximization</td></tr>
<tr><td>Matter</td><td>First Order</td><td>Substantive</td></tr>
<tr><td>Quantum</td><td>Non-Justificational[a]</td><td>—</td><td>—</td><td>—</td><td>—</td></tr>
</table>

Figure 8 Concepts of Rationality as Categorized by Point of View and by the Level of Ontological Complexity (Phases of Existence) Presumed in each Concept

[a] See chapter 9 for a discussion of rationality and quantum mechanics as they relate to monetary theory

with a mechanical concept of rationality. If simple learning processes are important both for man and other life forms, then such processes are inherently different than the movement of ordinary matter. Some learning processes, even if not distinctly human, might be useful in economic analysis, keeping in mind how restrictively static economic theory would otherwise be. The dilemma rational expectations theorists face is that processes of second order rationality are hardly complementary. The libertarian conception of man at the very least embraces third order or nonjustificational rationality.

In summary, the various conceptions of rationality presented in this and earlier chapters can be categorized according to the ontological complexity apparent in each. For a consideration of complexity in the real world, we return to the structural philosophy of science developed in Part I. The phases of existence that are inferred tentatively provide a basis for comparing the scope of each conception of rationality. Nonjustificational rationality seems to be the best conception of rationality for the processes of life and mind. Economic rationality, as it is manifest in the rational expectations literature, seems to be limited to mechanical processes and simple, adaptive learning processes evident in most life-forms. Thus, we can understand why economists, who have different views of rationality and expectation, really differ over a theory of mind.

SUMMARY AND CONCLUSIONS TO PART II

Having criticized the rational expectations approach for providing an antiquated, associationistic theory of rationality and expectations, it is imperative to go beyond associationism to the critiques of those economists who have a non-associationistic conception of economics. A non-associationistic conception of decision-making causes Shackle, Simon and Georgescu-Roegen to question the nature of economic rationality, expectations, and economic science. Their critiques exhibit a remarkable similarity. Since each gives some attention to differences in complexity in economic phenomena, an ontological dimension is apparent in each. In other words, a tentative aspect of the real world finds a more fundamental place in their positions than merely prediction. Having already established that a structural philosophy of science has ontological implications, the possibility arises of unifying these critiques from one fundamental conceptual framework. A structural view of science with its companion conception of nonjustificational rationality, offers an integrative framework for the types of critiques reviewed above.

This brings us to the end of Part II, a consideration of recent monetary theory and the Keynesian Revolution. As in Part I, mind and rationality have been the two themes which pervade our discussion. In contrast to his subsequent followers, Keynes gave his theory of a monetary economy a great deal of psychological realism. This emphasis imbued his theory with a conceptual dualism that was largely lost by subsequent theorists. Such conceptual dualism can be maintained only within an acceptable theory of mind. Rational expectations theorists seem to vacillate between abandoning or embracing a dualistic point of view somewhat similar to Keynes. If we interpret the rational expectations view realistically, the mechanisms of expectation in economics can be formulated as a theory of mind, viz, utilitarian associationism. But historically, associationism paves the way for mind to be abandoned as a scientific question. Thus, even a strategy emphasizing the realism of rational expectations leads back to our positivistic interpretation that rational expectations theorists abandon the dualistic point of view of Keynes and hence are irrelevant to Keynes. Only a handful of economists like Shackle, Simon, and Georgescu-Roegen maintain a conception of economic science adequate to the conceptual complexity apparent in the General Theory.

Having shown that mind and rationality are the most fundamental issues behind recent philosophy of science and the Keynesian Revolution, we now turn in Part III to a more complete consideration of the issue of mind and its implications for economics. With a perspective on science and a perspective on recent monetary controversy, perhaps some basic points can be made concerning the nature of economic science. There may be even sufficient arguments to suggest the outlines of a new conception of economic science, i.e, structural economics.

NOTES

[1]William Poole (1976:505) captures the implicit dualism in rational expectations: "Because all 'new' information is, after all, unpredictable in the rational-expectations theory, 'miscellaneous spirits' determine the paths of exogeneous variables, requiring only slight amendments to the role assigned to 'animal spirits' in the General Theory."

[2]In a recent Newsweek article by Shiels and Thomas (1978:60), Sargent is quoted as also describing economic behavior in cognitive terms: "It [monetary and fiscal policy] would work only if people were stupid. . . investors make their plans based on what they think the government is up to." [italics added]

[3]See p. 203.

[4]Rational expectations also has been described as a black box by Cyert and Degroot; see note 9 to chapter six, pp. 193-194.

[5]Austrian scholar Ludwig Lachman (1976:57) suggests there is a "Bergsonian affiliation" to Shackle's thought. Henri Bergson is the French process philosopher who denied the adequacy of mechanical conceptions of time. Compare similar passages from Bergson and Shackle. Bergson (1960 [1910]:91) says, "We are compelled to borrow from space the images by which we describe what the reflective consciousness feels about time and even succession; it follows that pure duration must be something different." Then Shackle (1972:279) gives a similar statement: "Our mode of thought requires us to assimilate time, in one of its senses, with a space with succession, in order that we may arrange the record appropriately in our thoughts."

[6]Shackle rejects the fruitfulness of formal mathematical techniques in the domain of imagination. Imagination and expectation result in an "epistemic" business cycle (Shackle, 1972:341). This notion of the business cycle cannot be strengthened by formalization: "Would its suggestions be rendered more acceptable or more likely to be fruitful by its replacement by a formal mathematical expression of such of its character as could be given any precise form? We think they would not," (Shackle, 1972:324). This theme is also seen in Shackle's discussion of probability, discussed in later paragraphs.

[7]Following Simon (1978:13), we could add a symmetric problem, one of too much information. When there is too much information, the question arises as to what information attention is directed. Attention is selective; thus attention is a cognitive or "epistemic" problem. See note 10, chapter five, p. 166, and note 17, chapter eight, p. 262.

[8]If Simon is correct that, in practice, many economists violate the conception of rationality in positive economics, this is all the more reason to abandon a restrictive conception of rationality as found in texts and formal literature.

[9]Perhaps a fourth consequence of Simon's work is this. He explicitly rejects the concepts of rationality and expectation in rational expectations. Simon (1978:10) states, "The so-called 'rational expectations' models. . . pass over these problems rather than solving them. . . . They [rational expectations] do not correspond to any classical criterion of rationality, and labeling them with that term, rather than the more neutral 'consistent expectations,' provides them with a rather unwarranted legitimation."

Since the preceding comments were written, Simon has been awarded the Nobel prize in economics for his research on organizational behavior. In his Nobel lecture, delivered in 1978, Simon (1979:505) continues to call rational expectations the "consistent expectations theory." He (1979:505) concludes, ". . . the policy implications of the rational expectations rule are quite different under conditions where new information continually becomes available to the system, structural changes occur, and the decision-maker learns, than they are under steady-state conditions."

[10]The concept of emergence is discussed by Nagel (1961:366-367), who states, "The doctrine of emergence is sometimes formulated as a thesis about the hierarchical organization of things and processes, and the consequent occurrence of properties at 'higher' levels of organization which are not predictable from properties found at 'lower' levels."

[11]Georgescu-Roegen's treatment of rationality implies that rationality is an ordering concept. Different hierarchical levels of observed phenomena vary in the complexity of order they manifest. Ordering suggests rationality may be related to causality. Hierarchical domains of law-like explanation may be seen in economics in the micro-macro dichotomy. Different concepts of rationality and causality may be required for individual, market, and macroeconomic activity. Gary Becker (1962:8) recognizes that household and market rationality (ordering) may not be identical. Indeed one does not imply the other: "Households may be irrational and yet markets quite rational." Georgescu-Roegen's hierarchical treatment of rationality seems to be compatible with Hayek's (1964:335-337), "Theory of Complex Phenomena."

[12]Verbal descriptions of mechanistic processes of mind in prescientific associationism have been replaced with experimentation on actual machine processes. For example, Von Neumann's (1958) little monograph, The Computer and the Brain, compares the central nervous system with the functioning of a digital computer. In an earlier paper, Von Neumann (1951:29) states, "I shall consider the living organisms as if they were purely digital automata." A. M. Turing, who was also influential in developing the computer, addressed the issue, "Can machines think?" In his famous article, "Computing Machinery and Intelligence," Turing (1950) denies that the question, "Can machines think?" has meaning. He replaces this question with an imitation

game. The issue thus shifts to this; can a machine imitate the human mind? Turing (1950:450) does recognize that computers cannot originate anything even though they simulate conditioned reflexes and responses.

In an article which precedes some of Turing's research by more than a decade, Frank Knight (1925b:385, n. 5) recognized that rational economic man was really an automaton:

"The hedonistic man, the selfish man, and the 'rational' man are closely related conceptions, all designed for the same function. All reduce, if consistently applied, to the thesis developed above, that the scientific man is one who does what he wants to do and whose wants are consistently related to the situation in which the man is placed. Followed out, this really means, as we have shown, simply the mechanistic view of man as an automaton, one whose conduct is in accordance with law in the scientific sense--that is, completely describable in terms of uniform relations to his situation. He may be conscious, but only in an 'epi-phenomenal' sense, and consiousness is to be left out of the scientific description of behavior."

Ultimately behavioral approaches in macro and monetary economics, like rational expectations, come down to this: Could a highly sophisticated computer be programmed to successfully imitate financial and investment activity? To the extent that monetary and capital decisions are highly original and ill-defined, automata could not imitate these decision processes.

[13]Knight's comments in the preceding note are a case in point.

[14]Compare with Muth's (1961:316) statement: "Dynamic economic models do not assume enough rationality."

[15]The concept of rationality plays an important role in the most influential work in modern political philosophy, Rawls' (1971) A Theory of Justice. Nonjustificational rationality has implications for the type of society for which we might argue. Perhaps Rawls has worked out some "rational" strategies for problems like inflation and unemployment, perhaps not. Explanation of these issues is beyond the scope of this study. For further remarks on Rawls, see chapter ten.

PART III

STRUCTURAL ECONOMICS

Chapter 8

MIND, STRUCTURAL AMBIGUITY, AND THE THEORETICAL DISUNITY OF ECONOMIC SCIENCE

There can be no doubt, at this point, that we are in sharp disagreement with the most widely accepted foundations of economic science. By calling in question the maximizing conception of rationality and demonstrating its restrictive macroeconomic manifestation in rational expectations, we are repudiating the adequacy of the most fundamental aspect of the positivistic conception of economics. No doubt this is tantamount to a rejection of the adequacy of the positive economist's basic vision of economic processes.

But having largely accomplished our task of calling attention to broader notions of rationality and mind in philosophy of science and in economics (Shackle, Georgescu-Roegen, and Simon) and having compared maximizing rationality to these broader conceptions of rationality, we now can turn to a different task. Implicit in our criticism of positive economics and rational expectations, one can find the basic elements of an alternative vision of economic science and economic processes. However, any new conception of economics cannot immediately achieve the specificity and sophistication of received doctrine which has had more than a century of continuous intellectual and scholarly development. Consequently, in the chapters which follow, we cannot hope to attain the coherence and elegance of neoclassical economics. Rather, we are limited to broadly outlining the basic aspects of our alternative view of science and economics.

In the following chapters, we pursue an elaboration of basic aspects of a more encompassing vision of economic processes and economic science. We already have a more encompassing theory of rationality--nonjustificational rationality--and a more encompassing, structural philosophy of science. At this point, what we need to do is this: We need to explore some of the most recent conceptions of man and his universe which are being developed in science and philosophy of science. Primarily this exploration takes us into physics and psychology. In these two domains of inquiry, similar issues and concerns are arising. Increasingly, developments in physics have implications for psychological theories of perception and cognition. Conversely, complex processes of scientific observation and measurement increasingly depend on our categorizing and conceptualizing minds. Such developments concerning the physics of cognition and the psychology of observation ought to have fundamental implications for economics. After all, a theory of economic processes ought to be consistent with physical and psychological theories of cognition and information processing. If such theories have radically different implications for our understanding of human decision-making in general, then our conception of economic decision-making processes also needs to be reconsidered.

In the following chapters, we explore some fundamentally new ideas relating to man, his universe, and his mind and some possible implications for a broader conception of economics. We follow the issues of mind and rationality beyond positivism and beyond the associationism of the utilitarians. In this century, we are seeing some remarkable advances in how the problem of mind is conceived. These advances are based on some of the most fundamental discoveries and interpretations of modern science. Quite similar structural interpretations tentatively are being given to theoretical dualisms in physics and psychology--the wave-particle theories of light and the mind-body problem. These structural interpretations are based on the characteristics of a hologram. Holography is an imaging technique with remarkable information-storing capacity and other properties. In other words, a hologram seems to offer evidence that man and nature are unified wholes despite being divided analytically into many autonomous parts.[1] With respect to the issue of mind, the holographic theory seems to offer a non-associationistic, holistic way of conceiving the cognitive functions of mind. This holographic theory of mind and its broadest implications for economics are introduced in this chapter. In chapter nine, we shall seek a more detailed and formal specification of holography by comparing economics with physics. In particular, we pay attention to the rational expectations view of economics and the instrumental interpretation of physics. They both lead to remarkably similar dualistic conceptions of physics and economics. But our exploration into holography permits us to assess both dualisms and go beyond them. Part III concludes with a suggestion for a new conception of economics, structural economics, in chapter ten.

THE DUALISTIC FOUNDATIONS OF MODERN SCIENCE

One of the major themes developed in Part II was a dualistic distinction between mind and behavior. Also, in recent monetary economics, we found dualistic distinctions prevalent in the thought of Walras, Keynes, and more contemporary theorists. Most often economists made the distinction in terms of statics and dynamics rather than mind and behavior. At least for Keynes and perhaps for Walras, the distinction between static arguments and dynamic arguments largely followed the mind-behavior distinction. Repetitive behavior was static and captured in an equilibrium model, while decision-making was more complex and dynamic, but of equal importance to economic activity.

A similar dualism was found in utilitarian psychology. Utilitarianism was both a theory of behavior--hedonism, and a theory of mind--associationism. Positivists, preferring to follow the monistic lines of argument in the natural sciences, essentially got rid of the mind-behavior dualism by reducing the theory of mind, associationism, to the theory of behavior and by declaring mind to be a pseudo-scientific issue. No doubt positivism has failed as a comprehensive philosophy of science perhaps because of its restrictive view of the problem of mind.

In philosophical terminology, the mind-behavior dualism is known as the mind-body problem. Whether this dualism can be transcended or reconceived is the problem at hand. A non-associationistic (holistic) approach to mind must have something to say about the mind-body dualism if it is to be relevant to monetary economics. The mind-body dualism is found at the very roots of modern science. Let us consider how fundamental this issue is.

A dualism which pervades the most basic conceptions of science is the distinction between the mental and the physical.[2] Historically, the relation of these domains of phenomena is known as the mind-body problem. At issue is how internal states of mind like expectations can make a difference in the observed behavior of human beings, both individually and collectively.[3] The mind-body problem raises concern whether causal connections exist between the existential, psychic stream of consciousness and externally observed human behavior.[4] Common sense tells us that a headache affects observed performance or that serious injury implies pain which is actually felt by some particular individual. Similarly, educated guesses and libertarian sensitivities suggest that expectations are relevant to the economic difficulties now facing Western democracies (simultaneous inflation and unemployment). Yet, pain, headaches, and expectations are subjective and abstractive phenomena. They are subjective because they are experienced individually and cannot be replicated by a neutral, objective observer. They are abstractive and cognitive because headaches, pain, and expectations, when they refer to more than one individual, implicitly infer that all human beings have analogous physical and mental structures.[5]

Historically, the mind-body problem was formulated by the French philosopher and mathematician, Rene Descartes. Descartes divided man into two things, body and mind.[6] The body of man was to be studied as though it were a machine or an automaton; the intellectual and abstract aspects of man, thinking and consciousness, were separated from the mechanical operations of the body. The mental operations or innate ideas were to be known through introspection, while the operations of the body were to be known through the laws of motion. Man was a unity of opposites; he was both a thinking thing and a mechanical thing.

Although Descartes suggested that mind and body interact through the pineal gland at the base of the brain, the scholastic nature of his view of man as a dialectical unity of opposites was apparent to others (Brett, 1962:370-372). Consequently, the Cartesian view of man as interacting mind and body was abandoned for various forms of philosophical mechanism. Philosophical mechanism is the idea that the laws of motion have a universal validity: Human activity and thought completely is determined by the laws of motion (Bohm, 1957:36).

One form of mechanism which retains the dualistic aspect of the Cartesian view is occasionalism. Occasionalism makes mind and body two parallel clockwork mechanisms (Watson, 1963:165). Mind and body as two clockwork mechanisms are perfectly synchronized with no interaction. This means that consciousness is a secondary attribute of the physical or that aspects of mind such as intelligence, interest, will, expectation, etc., are completely reducible to verifiable physical processes. An extension of this view is epiphenomenalism.[7] Epiphenomenalism means that mental processes are completely derivative from physiological brain processes and caused by them. Thus, man could be studied exhaustively by centering attention on physical laws of motion.

A stronger form of mechanism as a philosophical outlook is Laplacean determinism (Bohm, 1957:36-38), which replaces the dualism found in occasionalism with mechanistic monism (Knight, 1925a:251). Essentially, this form of determinism assumes that everything which human beings experience is subject to unified, quantitative laws which determine the behavior of several basic entities or variables. These quantitative laws apply to a large diversity of things; commonsense things experienced daily as well as scientific entities. Indeed, Laplacean determinism entails a world with its future completely determined by the laws of motion. If there were a super-being who could know all velocities and positions of all things in motion and who could also calculate the mathematical laws of everything which moves, then that being could know everything about the future that could be known with complete precision.[8]

In short, when the Cartesian foundations of modern science are reconsidered, one discovers an idea which seems to have little currency in positive economics. Rene Descartes suggests that man cannot be adequately studied by assuming that associationistic, machine-like processes are good approximations for the processes and effects of human cognition.[9] Descartes' strategy requires different analytical concepts for mental processes than for physical and behavioral processes. With the subsequent development of science after Descartes, mechanical explanation of natural phenomena is most successful. This success leads to the gradual extrapolation of mechanical explanation from physical to mental phenomena--occasionalism and Laplacean determinism are cases in point. They are naturalistic world views based on the success of Newtonian physics.

For the economist, the Cartesian mind-body dualism may still be a problem. The problem essentially concerns the adequacy of neoclassical economics for the study of real human economic activity. Since physics is the ideal science which stimulated the development of logical positivism, the issue raised is whether positive economics is one variant of philosophical mechanism. Alternatively, one might ask contemporary theorists whether they answer Descartes' implicit criticism; that explanation of human activity patterned after mechanical, natural-science phenomena is grossly incomplete.

MIND AND CONTEMPORARY SCIENCE

If there were no scientific progress in this century, new strategies for addressing the issues raised by the mind-body problem might not yet be apparent. However, there is new evidence on which to base a structural interpretation of the problem of mind.[10] In part, we have adopted a structural philosophy of science, because a structural view of mental and physical reality would seem to imply a structural view of social reality. Our structural view of reality is quite different from the view prevalent in the methodological writings of positive economics. In positive economics, any structure to reality (realism) is irrelevant to positive economics; except what realism is captured in prediction. The following paragraphs set forth some considerations relating to a structural view of the mental and physical domains of existence. These considerations are based on an interpretation of the importance of some recent discoveries in physics in the field of holography. Eventually, we turn our attention back to the study of man in order to assess potential implications for a social science like economics. We begin with a review of our structural philosophy of science; second, we briefly suggest a structural interpretation to the wave-particle theories of light in physics with holography; third, an application to the problem of mind is explored, followed by some generalizations about a conception of man.

When a structural philosophy of science was presented previously, it was presented with epistemological and ontological aspects being highly interdependent. Theories were considered to be conjectural knowledge claims about the real world; theories expressed as mathematical relations imperfectly approximated structural relations in the real world.[11] Furthermore, repeated failure to unify science or to remove theoretical dichotomies within sciences like economics, were taken tentatively as evidence that the real world structurally is divided into autonomous phases of existence. Structural relations found in theories at best have limited domains of applicability in the real world.

A structural philosophy of science is preferred to logical positivism because it explicitly recognizes the problem of mind. Perhaps the most important ontological distinction for the social sciences is the distinction between mind and other phases of existence. Positive economists repeatedly ignore or dismiss the distinctions as being irrelevant to economic analysis as long as theories predict well. If our structural view of science is correct, the mind-body distinction may be the most important element of realism that economists could possibly consider. Without an awareness of this significant aspect of the real world, economists are bound to make fundamental mistakes.

The distinction between mind and the other phases of existence is captured in Figure 4, p. 107. As such, mind is an autonomously nested domain of existence with its own distinct structural characteristics; hence theories relating only to the mind are possible. This is the area known as cognitive psychology. Cognitive psychology addresses topics like memory, problem solving, learning, rationality, and concept attainment rather than behavior construed as physical movement.

The difficulty with multiple domains of existence is that they conflict with our common sense intuition that the world is an organically unified whole. Cutting the world into distinct phases of reality may help solve fundamental dilemmas which both natural and social scientists face. But the apparent pluralism in science may be a bit overdone. If it were possible to find a process which permitted theoretical and ontological pluralism within a more general framework of unity, then genuine conceptual, methodological, and scientific progress might be possible. Evidence for such a process is developing in physics as a result of studies of holographic optical information processing.[12]

For example, suppose certain scientific processes were known which might result in inferred indeterminancy. Without a direct awareness of this process, the best methods to study this process might be statistical. Before knowledge of this process were known, scientists might be willing to conclude that the process is inherently indeterminate. In other words, unobservable structural properties of an unknown process could "generate" apparent indeterminancy when it may not be a warranted conclusion.[13]

Furthermore, suppose that evidence were produced that the brain operates according to properties structurally similar to the process mentioned above. If this were the case, mind and the natural world would exhibit a remarkable structural unity. Similar structural properties behind both mind and matter would make the world a unified, organic whole. Discovery of a common structural unity behind mind and nature, which also permitted mind and nature to be distinct, would permit a reinterpretation of our structural view of science. Multiple phases of existence would result because we may not have penetrated deeply enough into the nature of the perceived universe. The domains of existence that we experience may be surface level manifestations of a deeper, structural continuity to reality.[14] If this were the case, then reality is a unified but structurally ambiguous whole; surface structures would be alternative manifestations of an implied deep structure. Furthermore, compatible surface structures could exist simultaneously. Surface structures of differing orders of complexity, which could exist simultaneously, would lead us back to multiple phases of existence as developed previously.

The process common to both the brain and the external world which suggests a basic deep-structural unity to the universe is due to the interference of intersecting wave patterns.[15] Wave patterns are found within the brain and within nature. Specifically, the optical interference patterns captured in a hologram are known to contain a vast amount of information. For example, a laser beam can be directed at a special mirror which deflects part of the beam and lets part go through the mirror. The deflected part of the beam can be bounced off an object. This reflected light beam then intersects with the unreflected part of the beam. The intersection of the reflected and unreflected parts of the beam creates a unique wave pattern which can be recorded on film. At a later time, when the film with the interference pattern is exposed to another laser beam, the structure of the original object can be seen in three-dimensional space. Also, the object can be seen from a whole range of perspectives. Even if only a small part of the film is illuminated a second time, the whole object can still be seen in three dimensional space. Only the range of perspectives and the sharpness of detail are reduced (Bohm, 1973:143-146).

What is important about a hologram is that the whole structure of an object is recorded anywhere in the hologram. Exposing a small portion of the hologram still restructures the object. Thus, there is a unity to holography which helps to explain perception.[16] Any particular individual sees only a small part of the reflected interference pattern coming from any object in space and time. For cosmic objects in distant parts of the universe, only an extremely small part of a reflected interference pattern ever reaches Earth let alone the eye of the astronomer. Yet, this small portion of the interference pattern is sufficient to permit visualization of the object in three dimensional space.

Concerning the brain, Pribram (1978:84) and Gabor (1972:304) note that information processing or memory capacity is distributed throughout the brain. Memory is not localized in any particular part of the brain. Furthermore, wave patterns are created by electrical currents in the brain. Such wave patterns can intersect, forming interference patterns. Although electrical waves are not light waves, the principle of intersecting electrical wave patterns storing vast amounts of information may hold true.[17] For an event to be recalled, only a small portion of the interference pattern needs to be referenced. Like the optical interference pattern of the hologram, interference patterns in the brain may capture an entire event everywhere in the interference pattern. The structure of a whole event is recorded everywhere in the interference pattern of intersecting electrical waves in the brain.

What recent developments in physics and brain physiology ultimately show are that the brain and the physical world are part of the same natural universe. Intersecting wave patterns appear to be a key to the "ultimate" nature of the universe (Pribram 1978:96). Holonomic properties of brain and physical processes are indirect manifestations of the natural order. (Holonomic refers to the enfolding of information in all types of wave interference patterns, whereas holographic refers only to optical interference patterns). Whatever it is that forms the basis of existence (particles, waves?) may give rise to the holonomic properties of light and the brain. Physicist David Bohm (1973) suggests there is an implicit (holonomic) structure, which he calls the implicate order from which explicit observable events are derived.[18] The explicate order is the phenomena and events with which the theoretical sciences are usually concerned (Bohm 1973:148). Observable events can be analyzed and broken down into components (Bohm 1973:146). Processes actually observable with the senses or with instruments seem to be divisible. Once such processes are divided, they can be reconstructed. Thus, analysis and synthesis are two of the basic methods of modern scientific practice.

However, the importance of the hologram and other processes with holonomic properties is that the world seems to "hang together" in a unified way. Undivided wholeness may be a more fundamental characteristic of the universe than separability or divisibility. Holonomic processes are indications of unobserved (implicate) structural unity to natural and psychic processes. The holograph is the first optical technique which suggests that observer and observed can't be separated, for some processes; that theories about optical phenomena have been unduly restricted by the available technology; and that there is a close relationship between theory and instrumentation.[19] In other words, there may be fundamental structural properties of the universe which might account for the apparent indetermancy of physical phenomena.[20] Indeterminacy may not be as fundamental an aspect of the real world as originally thought. Indeterminacy may be a straight forward consequence of theories and instruments developed with respect to the special case of ordinary matter. The undivided wholeness found in holonomic processes may be evidence of a new level of structural order to the natural world (Bohm 1971:377ff). This new level or order defies analysis and synthesis. The perceiver and the perceived may share a remarkable deep structural or implicate order continuity, despite disparate surface structural properites.

In our previous discussion of a structural approach to science, it was suggested that the skepticism of the empiricist concerning the possibility of knowledge needs relocation. Skepticism might better focus on the ontological implications of scientific research. The preceding discussion of the holonomic aspects of mind and nature reinforces the relocation of such skepticism. Claims about the real world must be made, but they always must remain conjectural and tentative. Perhaps the best conclusion about the real world, based on systematic consideration of recent physiology, psychology and physics, is this: From a scientific point of view, the real world is structurally ambiguous. At the surface level, autonomous phases of existence seem readily apparent; yet at a deeper level, there are indications of a fundamental structural unity to the various phases of existence. Structural ambiguity results from the multiplicity of the phases of existence, and from the surface-deep structure distinction.

A structural view of science is important for what it implies about man. Our structural theories, (physical, natural, and social), divide man into component surface structures or processes. This may be a consequence of the close relationship between available instrumentation and theory formulation. Also, there may be no obvious natural processes indicating the undivided wholeness of the real world. To the degree that almost all social sciences imitate nineteenth-century physics; they divide man into analytical parts and do not put him together into a unified whole. Man is more than a biological organism with living matter and an organ called a brain. Man has a mind which is symptomatic of the undivided wholeness indirectly evidenced by the holonomic properties of intelligence.[21] Man is structurally ambiguous; he can be analyzed according to the laws within one of the phases of existence, but man as man is a unified whole. It is the totality of being human that remains unexplained. A person is simultaneously material and immaterial, divisible and indivisible, and mind and body. Mind and body are simultaneously and structurally divisible and unified, but at different levels of analysis or orders of complexity.

In summary, the mind-body dichotomy has existed since the very foundations of modern science. The mental half of this dichotomy has been ignored by many scientists, positivists in particular. But recent developments in physics have brought us back to the problems of mind and intelligence. Mind and the natural world both are characterized by properties which imply that there is an underlying fundamental structural unity to mind and the perceived universe. In other words, positivists misplace the unity of science in physical surface structures rather than in holonomic properties.

But mind and body (behavior), despite sharing a unity in holonomic properties, may still be distinct.[22] The differing orders of complexity between the mental and the physical may give rise to relatively autonomous structures or domains of law-like existence. What this means is that man is a structurally ambiguous entity, because he is a unified whole simultaneously consisting of mind and body. To ignore the structural ambiguity of man is to ignore some of the most basic advances of Twentieth Century science. Perhaps what economists need more than anything else is a realization of the structural complexity to human reality.

STRUCTURAL AMBIGUITY AND ECONOMIC MAN

If structural ambiguity is a dominant aspect of the world in which we live, then one would expect such ambiguity to be manifest in the social sciences. In particular, structural ambiguity should be apparent in the events and processes which economists investigate. Moreover, economic theory and concepts may evidence this type of structural ambiguity. The attempts of economists to fashion fundamental explanations of human economic activity may already embody distinctions which correspond to the separate phases of existence. Indeed, if a structural view of science is a more adequate view of science than positivism, then it ought to be a more adequate philosophy for economic science. In particular, if a structural view of science is correct, then positive economics is untenable. The theoretical and conceptual unity to economic science, which positivists desire, cannot be obtained from positivistic tenets. Positive economics is untenable because of the various orders of structural relations to real world economic phenomena which are excluded by the positivist's rejection of realism.

Although our demonstration of the untenable nature of positive economics and the rational expectations hypothesis is mostly negative in orientation, there is a possibility that the first tentative steps can be taken toward a *different conception* of economic science. The novelty in the following argument is that it follows from a structural philosophy of science which entails greater realism. Yet realism has important empirical implications. Structural ambiguity as an aspect of realism means that empirical methods and data are relevant but not decisive in resolving theoretical dilemmas. Also, empirical research techniques themselves are part of the structural ambiguity. Such ambiguity is understood or reduced through a critical process of conjecture and refutation.

If one surveys from the perspective of a structural philosophy of science, the empirical, theoretical, and doctrinal terrain which makes up the field of economics, one can begin to look for manifestations of structural ambiguity. Structural ambiguity is a potential source for fundamental controversies in economics. To the extent that the various phases of existence make man structurally ambiguous, economic man is structurally ambiguous. To the extent that economists insist on methodological individualism, surface structure pluralism is sacrificed mistakenly for surface structure monism. Deep structural unity to economic man, as evidenced by holonomic properties of mind and nature, is a more adequate locus of methodological individualism in economics. Many of the controversies economists have faced in the recent past may result from their failure to recognize the structural complexity of man the perceiver and his perceived universe.

Structural ambiguity results when one concept or object or thing possesses several meanings and functions (Weimer, 1980). Since an active cognitive capacity is necessary to remove or reduce (through plans and choices) the apparent ambiguity of economic processes, meaning and functioning are necessarily purposive or intentional concepts. In other words, if genuine structural ambiguity pervades real world economics, then transactors with a high level of intelligence and purpose are a must. In contemporary economics, structural ambiguity seems to be a source of several fundamental controversies in macro and monetary economics. All of the economic controversies directly or indirectly considered in previous chapters, appear to center in structural ambiguity. The definition of money, the appropriate concept of rationality, and the microfoundations issue, all may be indicative of the structural complexity of economic man and the economy. Consider now some of the issues relating to the Keynesian Revolution and subsequent controversies.

Keynes suggested in the General Theory that the distinction between monetary and value theory be reconceived.[23] A theory of the firm and distribution would deal with individual transactors, their interaction on markets, and the distribution of income and output; a theory of a monetary economy would deal with output and employment for the whole economy. However, in the profession at large, the theoretical distinction Keynes suggested became distorted as a micro-macro distinction. But, over several decades, awareness of such distortion grew. Subsequent theorists became concerned whether neoclassical micro theory and Keynes' theory are consistent. Their concern is whether Keynes' theory may be conceptually and theoretically incompatible with neoclassical micro theory.[24] Keynes may have different, but alternative views of economic man (Marshallian) from the view of economic man found in neoclassical price theory (Walrasian). This is the question posed by the microfoundations issue.

If the microfoundations debate is a substantive rather than methodological debate--dealing with significant issues other than aggregation --then the debate may be due to structural ambiguity which may pervade human existence. Alternative microfoundations might be complementary rather than mutually exclusive. Each microfoundation could refer to different orders of complexity. Alternative microfoundations could emphasize one of the various orders of structural complexity apparent in human economic activity by abstracting from the others. The structures of human interdependence (language, institutions, communication) may be quite different and of a higher order of complexity than the structures of relatively autonomous and independent human behavior (consumption, exchange, responses to relative income and prices). Structures of dependence and interdependence of differing orders of complexity may exist simultaneously and may be ontologically quite compatible with one another. However, the compatibility of these structures may be subject to breakdown. Transactors may unwittingly or purposefully confuse the two types of structures. In this case, individual choice may not be in harmony with the individual and collective decisions of other transactors.

Consider also the functions of money; money is defined by what it does (functions) rather than by its material composition. Of the various functions of money, perhaps the two most important are the store of value and the means of payment functions. The means of payment function implies an informational and transactional interdependence between transactors engaging in monetary exchange. The transactional structure of the economy enhances the specialization and efficiency of exchange, production, and consumption. Furthermore, the success of a system of monetary exchange depends upon the psychological attitudes of the participants. Without confidence in the exchange process, exchange would revert to the less efficient system of exchange known as barter.

In contrast, the store of value function of money, we maintain, implies that individuals are isolated, independent economic entities. In other words, money is to be treated just like any other real or financial asset that enters into individualistic maximizing behavior.[25] No less an economist than Lionel Robbins (1935:12) has stated that an "isolated man" like Robinson Crusoe "is typical of the whole field of economic studies." If economic man is nothing more than a Crusoe-type individual, then money may be largely irrelevant to such a view of the transactor and economic activity. In other words, when economic man is not dependent in any substantive way on other transactors, the structure of this situation of independence effectively abstracts from money. Again, if the functions of money are a substantive rather than a methodological distinction, then the various functions of money may be evidence of structural ambiguity pervading the monetary system and monetarized exchange.

Lastly, consider the discussion of rationality from the previous chapter. The relevance of structural ambiguity should be apparent from Figure 8, p. 225. Figure 8 is derived from the concepts of rationality, mind, and reality present in recent philosophy of science and in the views of Shackle, Simon and Georgescu-Roegen. The point to the consideration of rationality in chapter seven is this: Rationality describes the various orderings or patterns of phenomena in the real world. If there are different domains of existence, then concepts of rationality are descriptions of the order obtaining within a particular phase of existence.

Typically, the economics profession abstracts from the structures of the psyche. For the investigating economist, the rationality of economic man is of the same order of complexity as matter in motion or simple biological species.[26] Alternatively, nonjustificational rationality is a concept of rationality which is of the same order of complexity as complex decision-making situations in the economy. A major concern of nonjustificational rationality is why an intelligent being questions and tests the information and knowledge available in the market (or through experimentation). Both economic rationality (maximization) and nonjustificational rationality may be relevant to economic activity. To the extent that economic activity is highly repetitive, economic rationality as maximization may be an important concept for economic science. Indeed, most economic activity may be highly repetitive and amenable to research based on very simple assumptions like maximization. Within its domain of abstraction, economic rationality is not an unrealistic assumption.

However, to the extent that economic activity is not repetitive, nonjustificational rationality (Figure 5, p. 132) is the relevant concept of rationality. Ill-structured and uncertain situations require the decision-maker to consider, not only the evidence, but also the accuracy of the evidence and the relevance of his theoretical and conceptual framework. One can hardly expect such situations to be as predictable as situations which are economically rational. To the extent that multi-dimensional, nonjustificational rationality partially is descriptive of economic activity, such economic activity is inherently indeterminate and unpredictable. In a very real sense, the transactor has to deal with the ambiguity and interdeterminacy of his market situation.

For the economist, the entrepreneur performs this role in situations where markets have the potential to work efficiently.[27] The entrepreneur is the one who "determines" the market process, by demonstrating to other transactors that pure economic profits actually can be earned. It is the actualization of pure economic profit by the entrepreneur that induces imitation on the part of others. But the entrepreneurial function presupposes that markets work efficiently. When markets fail or are in disequilibrium, this would seem to reduce the scope of potential entrepreneurial performance. No doubt the entrepreneurial function is nonjustificational in what it requires from the entrepreneur. An entrepreneur is one who must be able to deal with an ambiguous, multi-dimensional, uncertain situation. But if, when markets fail, the entrepreneurial role also fails, then an alternative approach must be found. Entrepreneurs are essentially specialists in structuring ill-structured economic situations. There role is limited by the extent of the market.

Where the entrepreneurial function fails, the economic situation remains unstructured, indeterminant, and perhaps chaotic. Non-market attempts to reduce chaos and indeterminacy imply that government and other institutions may evolve. Nonjustificationally rational decision-makers recognize the desirability of increased order to their collective situation. As individuals they conceive strategies aimed at avoiding chaos and indeterminancy. As individuals they are not wholly independent. Indeed, they may not be primarily independent. As individuals they may recognize their interdependence with other individuals. The biological and physical autonomy of individual human beings may be grossly misleading in situations of disequilibrium and market failure. The existence of highly developed networks of interpretive interchange (as opposed to informational exchange) like the news, specialized news magazines, and journals is evidence of individual and collective indeterminacy and interdependence, even in the face of an abundance of information.

There are no guarantees that attempts to structure economic processes subject to market failure will succeed. Entrepreneurial or institutional processes may break down.[28] Or, if initial attempts at establishing economic institutions like monetary exchange, government, and welfare do succeed, they may ultimately run into difficulty. To the extent that a market system emphasizes the autonomy of the individual and permits him to ignore the degree of interdependence and specialization in the economy, market processes may undermine attempts to redress market failure. It is not at all apparent that market attitudes enhance the functioning of non-market processes. After all, the mercantile mentality in the sphere of politics and institutions is known as corruption. Bureaucrats and politicians, who cannot understand the differences between the market and other institutional structures with opposing goals and motives, are usually removed from office and/or put in jail. This apparent contradiction of intentions between market and non-market processes does not mean social reality is inconsistent. It only means that both man and his environment are structurally ambiguous.

Recapitulating our argument, structural ambiguity seems to pervade economic analysis particularly monetary economics. The so called micro-macro dichotomy, the various functions of money, alternative concepts of rationality, and the varieties of microfoundations reflect apparent differences in the order of complexity to human economic activity. Moreover, such differences in order of complexity are manifest in individuals as individuals. An individual is a structurally ambiguous entity who, in some aspects of his activities, is independent of other transactors while in others he is very interdependent. The ambiguity of economic man's economic relations can be reduced processively. In those situations in which markets are potentially feasible, entrepreneurial processes might result in a more determinate economic situation. In those situations in which markets are prone to failure, institutional processes may be needed to reduce a degree of the ambiguity. But entrepreneurial and institutional processes are no guarantee that the task of reducing indeterminacy is being accomplished. Ultimately, an appeal to some type of political process is needed. But political processes possess no guarantees either.

SUMMARY

In this chapter, we have sketched the bare outlines of a theory of mind. Initially, we explored the Cartesian view of mind which preceded the utilitarian theory of mind, associationism. Then we elaborated one of the newest approaches to a theory of mind resulting from both comtemporary cognitive psychology and physics. This was a holonomic theory of mind. It implied that both physical and psychological processes have remarkable deep-structural similarities as exhibited by the informational properties of mind and the hologram. In Bohm's terminology, the same point of view was stated as an implicate-explicate order distinction; mind and the natural world share a common holonomic implicate order.

We concluded this chapter by tentatively suggesting some applications to economics. Since positive economists have ignored the human mind and its consequences for economic theory and science, applications to economics were readily apparent. The implicate-explicate order was proposed as a framework for understanding the various dichotomies and dualisms in economics and the various concepts of rationality reviewed in the previous chapters. However, of even greater importance was the major inference drawn from our understanding of man and his universe based on the implicate-explicate order distinction--the pervasiveness of structural ambiguity. Something as fundamental as implicate and explicate order means that structural ambiguity permeates the economic affairs of highly intelligent transactors in a modern monetary economy. Apparently, such ambiguity has generated many of the dilemmas and controversies in contemporary economics.

NOTES

[1]Arthur Koestler (1978) takes a similar view of man and his universe and calls his view a "holarchy."

[2]The mind-body problem is a cluster of issues which have been ignored or evaded by behaviorists. They are primarily sentience, sapience, and selfhood; see Feigl (1967:136-137) and Weimer (1976:6-7). Sentience deals with the phenomenal qualities of sensation. Sentient qualities such as color disappear in scientific investigation; they are subjective. Sapience refers to intelligence. Properties of intelligence are not obviously reducible to those of matter, or the non-mental realm. Selfhood refers to the unique identity of each human being. A person is a compound of both the mental and the physical. Behavioral approaches do not address these issues, nor does positive economics suggest what the economic significance of these human realities might be. Our concern with expectations suggests sapience is the mind-body issue of most direct interest.

[3]This is what Popper (1972:231-232) calls Descartes' problem. Briefly stated, how is it that abstract entities influence human behavior? "How can it be that such things as states of mind--volitions, feelings, expectations--influence or control the physical movements of our limbs? And. . . how can it be that the physical states of an organism may influence its mental states?"

[4]"The Stream of Consciousness" or "Stream of Thought" is a notion developed by William James (1962 [1892]:166-188; 1967 [1890]: 21-74). Behavioral approaches to mind make mind a compound or association of simple elements called sensations. James asserts the wholistic continuity of consciousness in a state of constant change.

[5]Bohm, Knight, and Hayek recognize an abstractive element in sensation. See note 32 in chapter four, p. 119.

[6]Descartes (1960 [1641]:80 and 81) states; "The human body may be considered as a machine, so built and composed of bones, nerves, muscles, veins, blood, and skin that even if there were no mind in it, it would not cease to move in the ways that it does at present. . . . There is a great difference between the mind and the body, in that the body, from its nature, is always divisible, and the mind is completely indivisible." For a discussion of the literature on automata, see Georgescu-Roegen (1971:83-94).

[7]For a discussion of epiphenomenalism, see Feigl (1967:4, 50, and 60-61) and Campbell (1970:55-58 and 110-125). Knight (1925b:385 n. 5) suggests that economic man necessary for a scientific treatment of human behavior is "conscious, but only in an 'epiphenomenal' sense,

and consciousness is to be left out of the scientific description of behavior." One can see why empiricists like logical positivists would essentially argue that one of the perspectives was redundant. See chapter two, pp. 30-33.

[8]The omniscient auctioneer which has played such an important role in static monetary theory is the economic equivalent of a Laplacean super-being often called Laplace's demon. Lionel Robbins (1935: 131-132) recognizes a Laplacean dimension in economic analysis: "If we were able to ascertain once and for all the elasticities of demand for all possible commodities and the elasticities of supply of all factors, and if we could assume that these coefficients were constant, then we might indeed conceive of a grand calculation which would enable an economic Laplace to foretell the economic appearance of our universe at any moment in the future. But, as we have seen, useful as such calculations are for judging the immediate potentialities of particular situations, there is no reason for attributing to them permanent validity. Our economic Laplace must fail in that there are no constants of this sort in his system. We have, as it were, to rediscover our various laws of gravitation from moment to moment." Our only disagreement with Robbins is that laws of higher order of complexity need to be discovered which apply to individual and collective choice. These laws are not the same type of laws as the laws of gravitation.

[9]Some of the reasons why Descartes made the distinction between mind and body are as follows: First this distinction was found in ancient Greek philosophy; Descartes evidently was not as independent of his past as he would have liked. Second, for Descartes the loss of an arm or a leg did not mean anything had been lost to the mind. Third, Descartes thought an awareness of an immaterial self was the most indubitable thing that could be known. His cogito, "I think therefore I am," was the basis of his theory of knowledge. This starting point is both subjective and mentalistic. Mind was more certain than matter for Descartes. An elaboration of these themes lies beyond the scope of our inquiry. See R. I. Watson (1971:146, 152-155) for a more complete treatment.

[10]To this point we have not made specific what we mean by structure. Concepts which are part of our notion of structure are hierarchy, pattern, extension, order, measure, limit, and boundary (Bohm, 1971:364-366). Bohm (1971:365-366) defined structure in a way compatible with our interpretation of structural realism and economics: "The consideration of the working together of order and measure in ever broader and more complex contexts leads to the notion of <u>structure</u>. As the Latin root 'struere' indicates, the essential meaning of <u>the</u> notion of structure is to build, to grow, to evolve. This word is now treated as a noun, but the Latin suffix 'ura' originally meant 'the action of doing something.'" Later in this chapter, this activity of doing something will be located in the entrepreneurial role. In Keynes' <u>General Theory</u>, spontaneous action was called <u>animal spirits</u>. Perhaps animal spirits should be viewed as the activity of continuously structuring and restructuring one's economic situation.

[11]See Figure 1, p. 9 and discussion in chapter four, p. 104 ff.

[12]Accounts of holography accessible to the scientific community in general are found in Gabor's (1972) Nobel lecture published in Science and in Bohm's (1973) article. Also, holography is interpreted as a theory of mind by Pribram (1976,1978). An interview with Pribram titled "Holographic Memory" recently was published in Psychology Today (Goleman, 1979). For additional references, one should refer to the preceding works.

[13]For a discussion of indeterminacy and Heisenberg's uncertainty principle, see Nagel (1961:294-316) and Bohm (1957:91-103). In the quantum context, indeterminancy means the position and momentum of the electron cannot be simultaneously determined. Rather one calculates the statistical probability for momentum, given the position of the electron and conversly.

In recent time series analysis (Box-Jenkins), the criterion for a process having no deterministic order is white noise. White noise is the time series equivalent of pure randomness. However, a white-noise hologram apparently may contain a great deal of information. By exposing a hologram many times to different objects while varying the angle between intersecting light beams, multiple exposures can be stored on one hologram (Gabor, 1972:304). The magnified image of such a hologram is a noise-like code. Thus, reducing a time series to white noise process does not mean that all determining structure has vanished. On the contrary, a noise-like pattern may be the result of structure rather than a symptom of its absence. Also, see Pribram (1978:94-96) for comments on structure, probability and holography).

[14]For a discussion of the surface-deep structure distinction, see Chomsky (1965:198-199). For an application of Chomsky's ideas to the problem of mind, see Weimer (1976:19-23). Popper (1972:37) seems to realize such a distinction in his discussion of realism: "Common sense also realizes that appearances (say, a reflection in a looking glass) have a sort of reality; or in other words, that there can be a surface reality--that is in appearance--and a depth reality."

[15]Physicist Gabor (1972:304) recognizes holographic aspects to human memory: "A diffused hologram is therefore a distributed memory, and this has evoked much speculation with respect to whether human memory is not perhaps, as it were, holographic, because it is well known that a good part of the brain can be destroyed without wiping out every trace of a memory."

[16]In the appendix to his Special Theory of Relativity, Bohm (1965) discusses the relationship between modern physics and perception. Bohm considers the research of psychologists Piaget and Gibson. Bohm is concerned with how the concept of structure is learned. The process by which the scientist perceives structure, in his view, is not all that different from usual processes of perception in individual human beings. This is consistent with the views of Parsons, Scheutz, and Weimer as noted in chapter one, note 1, p. 16.

[17]In chapter seven, we saw that too much information may cause just as many problems for a transactor as too little information. Too much information raises the problem of attention. H. A. Simon (1978: 13) has already raised this issue with respect to economics. The holographic interpretation of memory would add further support to Simon's view, that attention is a scarce resource.

[18]Note the similarity between Bohm's implicate-explicate order distinction and Chomsky's surface-deep structure distinction. The phases of existence as shown in Figure 4, p. 107, are explicate order or surface structure manifestations.

[19]Bohm (1973:143) states: "The individual wholeness of modes of observation, instrumentation, and theoretical understanding. . . implies the need to consider a new order of fact, i.e., the fact about the way in which modes of theoretical understanding and of observation and instrumentation are related to each other."

[20]Pribram (1978:98) summarizes his view of holography and indeterminacy: "The consequence for this view is a reevaluation of what we mean by probabilistic. Until now, the image, the model of statistics, has been indeterminacy. If the above line of reasoning is correct, an alternative view would hold that a random distribution is based on holographic principles and is therefore determined. The uncertainty of occurrence of events is only superficial and is the result of holographic 'blurring' which reflects underlying symmetries (much as does the Gaussian distribution in our earlier example) and not just haphazard occurrences."

[21]As noted in note 3 above, William James' conception of the stream of consciousness' expresses the wholistic continuity of human mental experience.

[22]Mind and body or mind and behavior are the same distinction. Mind and body is the distinction more relevant to philosophy; mind and behavior is the distinction more relevant to the social sciences. A behavioral theory is one which focuses on a sequence of observable bodily movements.

[23]See note 22 to chapter five, p. 168.

[24]See note 39, in chapter five, p. 170.

[25]We believe that portfolio choice as it is usually presented extends the conception of isolated economic man into monetary economics where it does not belong. Portfolio theory treats money as a riskless financial asset which is held for diversification purposes. We also maintain that neo-Keynesian theorists who adopt portfolio choice as the mechanism behind monetary policy go beyond portfolio choice. They tend to emphasize the uncertainty endemic in a monetary economy which affects the sequences of both real and financial transactions. See chapter five, note 19, p. 168 and note 36, p. 170.

[26]For example, Schumpeter (1954:447 and n. 4) maintains that the associationistic theory of mind of the British utilitarian economists had nothing to do with their economics. Thus, Schumpeter recognizes that economists largely have abstracted from mind as an issue for economic science.

[27]The emphasis given to the role of the entrepreneur is somewhat different than the usual emphasis. Kirzner (1973:14) defines entrepreneurs as those "who are themselves neither would-be sellers nor would-be buyers, but who are able to perceive opportunities for entrepreneurial profits; that is they are able to see where a good can be sold at a price higher than that for which it can be bought." Kirzner has an element of cognitive activity in his conception of the entrepreneur in the form of perception. But, our conception of the entrepreneur makes him fully cognitive in the sense of nonjustificational rationality and modes of argument.

[28]The rapidly expanding literature on public choice and political processes in economics offers good reasons why political and institutional processes may break-down for individualistic reasons. This public choice literature seems to have been developed by economists interested in public finance rather than monetary policy and theory. Perhaps monetary policy as well should be conceived in terms of a public choice problem. A criterion of monetary policy, at the very least, would need to provide the political basis and validity of the conduct of monetary policy. See Mueller (1976) and Gwartney (1976:511-545) for discussions of public choice and Mason (1963) for a definition of the monetary standard as the criterion of monetary policy.

Chapter 9

DUALISM AND THEORETICAL DISUNITY IN PHYSICS AND ECONOMICS

Our consideration of the problem of mind in modern science and in economics has led us to emphasize a phenomenon which fundamentally alters our vision of economic processes--structural ambiguity. Structural ambiguity is pervasive in a sophisticated monetary economy in which highly intelligent transactors are present. If structural ambiguity pervades monetary phenomena, then structural ambiguity is a likely source of monetary controversy manifest in revolutions and counter-revolutions since Keynes. Indeed, structural ambiguity, when genuinely encountered by the monetary theorist, seems to lead to some sort of theoretical or pardigmatic dualism. Theorists like Keynes and Walras make dualistic notions central to their conception of economic processes; while rational expectations theorists make structural ambiguity and its consequences external to economic processes and economic science. This dilemma of dualism--whether structural ambiguity should be internal or external to a conception of economic science--may be the most pressing problem facing the economics profession. It must be resolved before economists can meaningfully communicate with one another. Because we believe rational expectations theorists have taken a troublesome and unwarranted turn back to nineteenth-century utilitarian psychology, we obviously opt to make structural ambiguity internal to a conception of economic processes and economic science.

Since phenomenal structural ambiguity seems to give rise to theoretical dualism in economics, we must address this propensity for theoretical dualism in greater detail and generality.

In the first part of this chapter, we begin with the problem of phenomenal and theoretical dualism in physics and the instrumental interpretation of such dualism. At this point, we also consider the problem of dualism in the rational expectations conception of economics and the instrumental interpretation of this duality in economics. We show that rational expectations largely follows the instrumental conception of science so widely accepted in physics. In the second part of this chapter, we attempt to give a more analytical formulation to the preceding issues. We hope to show that the duality problem in physics can be respecified in economics as a duality concerning alternative states of rationality (microfoundations). Finally in the last section, we hope to capture all of these notions in a succinct, encapsulating vision of economic processes. This notion has the same integrating and interpretive role in our conceptual scheme that Say's or Walras' Law has for an understanding (or misunderstanding) of classical or neo-classical economics. Since our notion is so different from Say's or Walras' Laws, we prefer to give it a name which emphasizes how different it is. Our notion of economic processes is called a Principle of Processing Complexity. It has a dualistic analytical formulation, but is developed within the context of a unified conceptual and philosophical framework. Thus, we provide a unified point of view which is not reductionistic. We turn now to dualisms in physics and the rational expectations conception of economics.

RATIONAL EXPECTATIONS AND PARALLELS BETWEEN ECONOMICS AND PHYSICS

We should keep in mind that the rational expectations approach to economics is our point of focus. When we attempt to contrast physics and economics, it is the view of economic science implicit in rational expectations that is used as a basis for comparison. By the rational expectations view of economics we mean the following. First, we assume that positive economics as developed by Friedman and instrumentally elaborated by Boland (1979) and Blaug (1976) is the conception of economic science behind rational expectations.[1] Second, we assume that maximization is the concept of rationality in rational expectations. Third, we assume that the ends-means conception of economics is the view of the subject matter of economics in rational expectations. Even more strongly, we could reverse our train of thought and say that taken together positive economics, maximizing rationality, and the ends-means conception of economics lead logically and inexorably to the rational expectations hypothesis.

How then does the view of economics implicit in rational expectations compare to physics? To make this comparison we must first give a reasonably accurate picture of physics and then consider rational expectations.

A Characterization of Recent Physics

During the Twentieth Century the concepts and theories of physics have undergone rapid transformation.[2] This transformation is due to the discovery of new entities which compose atoms and to the discovery of the relativistic nature of the cosmos. Together, quantum theory and relativity theory lead to a drastic revision of our ways of thinking about man and his universe. Revisionist thinking about the universe still continues. In physics as well as in economics, scientific revolutions appear to need years if not decades to be completed coherently.

Before the discovery of quantum and relativistic phenomena, the dominant conception of science was Newtonian physics. In the Newtonian universe, absolute space and time characterized the world in which we live. Materially, this world was thought to be constituted of isolated and discrete entities or particles which were incessantly in motion. These isolated and discrete particles were thought to be the ultimate building blocks of life and matter. The phenomenal divisibility of such discrete building blocks also meant that observation and operations of mensuration had no apparent effects on the objects in motion. In other words, the apparent independence of physical phenomena from the human intellect greatly facilitated experimentation and the rise of an empirical point of view. But a significant dilemma arose from the apparent autonomy of physical phemomena. In the extreme, such a view lead to determinism; not only did man not have a causal role in the experimental process, he also did not have any significant causal role anywhere in the universe (Guillemin, 1968:264).

Despite such difficulties, the Newtonian view was widely accepted. No doubt this acceptance was due to its successes as well as to its intuitive appeal. Most of the aspects of the Newtonian framework lent themselves to geometric interpretation in such a way as to enhance their appeal to human intuition. Indeed, their appeal was so great, that it has been difficult to supercede such mechanistic intuition even in the study of human affairs.

However, around the turn of the century new phenomena were discovered which undermined the aura of success associated with the Newtonian view (Dampier, 1971 [1948]:369-374). The discovery of radioactivity effectively reopened the perennial scientific question concerning the ultimate building blocks of the universe. In the nineteenth century, atoms had been considered the ultimate building blocks of matter. But with the discovery of radioactivity, it began to be realized that even atoms are composed of smaller constituents; for a while, it seemed as though the constituents of atoms were protons, electrons, and neutrons. The view of the atom appeared to be that of a miniature solar system composed of densely packed protons and neutrons surrounded by electrons moving in fixed orbits (Dampier, 1971 [1948]:390).

But problems began to appear with this miniature, solar-system-view of the atom. It was found that at very high velocities approaching the speed of light, the atom could be split into other entities. For the most part, these entities were not protons, neutrons, or electrons. They exhibited different charges and weights than protons, neutrons, and electrons. Furthermore, such entities were transformable from one type of entity into another. Indeed, it appeared as though there may be a continuous process of creation and destruction behind the natural world of human perception.[3] However, even more perplexing was that these newly discovered entities exhibited properties both of energy fields and of matter; that is, these entities were characterized as having both wave and particle properties. This wave-particle duality has been one of the most perplexing aspects of recent physics. For scientists trained to search for theoretical unity, such dualism has been difficult to handle.

By now it is well known that a problem of observation occurs in quantum physics. The state of a "wavicle" cannot be fully determined like the state of a discrete material object in mechanics.[4] The state of an ordinary object in motion consists of a time dependent description of its position and momentum. For a quantum particle, position and momentum cannot be determined simultaneously. To ascertain the position of a quantum particle, it must be confined to a very small space. Such confinement increases the amplitude of the wave of motion, therefore increasing the velocity of the particle (Levich, Myamlin, and Vdovin, 1973:19-23). Thus, the attempt to measure one aspect of the state of the particle affects the other aspect and conversely.

Recently, one interpretation maintains that quantum phenomena should not be given an intuitively realistic interpretation like Newtonian mechanics. Rather, the mathematics of quantum phenomena should be given only a formalistic interpretation. Since quantum phenomena are best described by a probability wave function, mathematical statistics becomes the basis of a formal interpretation (Nagel, 1961:308). Although physicists maintain that only a formal interpretation should be given to statistical descriptions of quantum phenomena, the question does arise whether reality is indeterminate. In other words, the ultimate building blocks of the universe may be describable only in indeterministic modes of analysis and therefore the fundamental constituents of the universe may be indeterminate (Nagel, 1961:312).

Ultimately, a dualistic view of the material world results from the considerations mentioned above. If one goes beyond the formalism of the statistical models, then our world seems deterministic in its macro properties (ordinary matter), but indeterministic in its micro (quantum) properties. Such a dualism is a source of disenchantment for those scientists accustomed to theoretical and phenomenal monism.

Another (non-instrumental, structural) way of resolving this dilemma is suggested in the previous chapter. Interpretations of holographic phenomena seem to suggest a new way of conceiving our universe. The dual property of quantum entities may be a consequence of unperceived limitations relating to theory formulation and ordinary observation. A basic implicate unity may be behind quantum phenomena and other phenomena like the mind (following Pribram).

In summary, recent discoveries in physics have lead to a significant transformation in the various modes of conceptualizing the universe. The characteristics of the Newtonian world view have been seriously questioned. Absolute space and time, phenomenal separability, mechanical causality, and determinacy have been undermined as universally valid scientific concepts. We even raised the issue regarding the ultimate building blocks of the universe. Material substantial constituents of the universe have been replaced with a processive awareness of the continuous anihilation and creation of matter. In the face of these difficulties, many physicists have opted for epistemological and ontological agnosticism better known as instrumentalism. The conundra resulting from attempts to realistically interpret quantum mechanics have led physicists to emphasize statistical formalism, computational facility, and predictability. Apparently, in this instrumental view, physics has reached the limits of human knowledge.[5]

Rational Expectations

In previous chapters, we have argued that dualisms have pervaded economic analysis. A dualistic point of view was evident in the works of Walras and Keynes and also in our major theoretical distinctions--the alleged monetary and value and micro-macro dichotomies. Dualism as an issue was also raised with respect to the rational expectations hypothesis. We found that those who accepted the rational expectations point of view exhibited some inconsistencies regarding dualism. Some interpreters suggested that the rational expectations point of view related to humanly experienced anticipations of the future; while other interpreters (primarily theorists) maintained that rational expectations replaced psychological anticipations. Those who preferred the more realistic interpretation of rational expectations obviously had a dualistic interpretation, since such realism transcended the domain of positive economics. However, even the allegedly monistic interpretation involved an element of dualism.

If we consider the more restrictive interpretation of rational expectations, then some interesting parallels with recent physics can be obtained. By the more restrictive interpretation, we mean the consistently positivistic position that rational expectations have nothing to do with human anticipation; rather rational expectations theory is an analytical formalization which is devoid of realism and justified scientifically by its predictive accuracy. To illustrate, rational expectations theory as embodied in Sargent's prototypic model is a relatively simple reduced-form model that depends on statistical time series to close the model.[6] Rationality enters the model as the calculated statistical expectation of all present and past information relating to next year's price level. The assumption is that such a statistical expectation is equivalent, on average, to the decisions of an intelligent public. Thus, this more restrictive interpretation of rational expectations reduces psychological anticipation to the formalism of a statistical mean.

Notice that the implicit reduction of psychological to statistical expectations also involves a dualistic point of view. However, the dualism takes a different twist. Rational expectations involves a distinction between non-random, static factors and random, dynamic factors. Non-random, static aspects of economic activity can be approached deterministically with a behavioral theory of maximization. Random, dynamic factors are approximated statistically and cannot be explained in any more detail with a more elaborate theory. Thus, in contrast to Walras and Keynes who make dualistic conceptions central to their theory (but not to their models); rational expectations theorists have a unified model and a unified theory. However, their view of the economic phenomena remains dualistic. They seem to make maximizing behavior the exclusive domain of scientific economics; while all other human activity (particularly in aggregate) lies beyond scientific explanation. What lies beyond the domain of scientific explanation apparently is ambiguous, uncertain, unintelligible, indeterminate and constitutes the domain of the non-rational and the unpredictable.

It is apparent that economists are imitating physics. Daniel Fusfeld (1980:11) suggests that economics has adopted, in altered form, some of the most basic conceptions of Newtonian physics:

> The analogy with Newtonian celestial mechanics has often been made: Consumer units and business firms replace the planets and the sun; self-interest replaces gravity; and the general equilibrium of prices, quantities, consumers, and producing units is the analog of the movements and positions of the units of the solar system.

However, we believe the analogy goes even further than Fusfeld implies. One of the most widely accepted interpretations of physics views Newtonian physics as a macro theory of matter and quantum theory as a micro theory of matter. Macroscopic entities and their movement is deterministic, while the movement of microscopic, quantum entities is indeterministic. Furthermore, the Newtonian, macro theory of matter readily lends itself to an intuitively realistic interpretation and the quantum, micro theory apparently has no such realistic interpretation. Thus, the quantum theory remains merely a formalism which provides the best mathematical model of the observable phenomena. Since quantum entities cannot be construed realistically as being both a wave and a particle, some physicists are reluctant to go beyond the formalism of their models.

Rational expectations theorists apparently achieve a similar result in economics, but for one major innovation. Rational expectations theorists invert the deterministic, indeterministic relationship. Whereas micro quantum phenomena are indeterminate and macro mechanical phenomena are determinate in (instrumentally interpreted) physics, in the rational expectations view we argue, it is microeconomic behavior that is determinate and macroeconomic activity that is indeterminate.

Other than this inversion of micro-macro indeterminacy, rational expectations theorists exhibit other similarities with recent physics. Prediction and formalism are emphasized rather than realism. Why rational behavior is determinate for individuals and indeterminate for groups of individuals apparently has no intuitive explanation. But the realism of what is actually happening during a period of changing rates of inflation is irrelevant to economic science (positivistically interpreted) as long as such models predict well.[7] Thus, rational expectations models are mere formalisms which serve as the instruments for predicting the future time path of various macro variables. These statistically oriented macro models are the models which least constrain the potential responses of a rationally maximizing public. The responses of an intelligent, rational public to the uncertain and unanticipated surprises of the future is incorporated into rational expectations macro models as indeterminacy.

In summary, the rational expectations view of economics, like the instrumental view of physics, represents an accomodation to indeterminacy. It resorts to formalism, and substitutes prediction in the form of statistical probabilities as hallmarks of the scientific point of view. However, there is one exception; in rational expectations, the determinancy-indeterminacy relation between micro and macro phenomena is inverted from what it is in physics. Rational expectations theory supposedly addresses the indeterministic aggregate macroeconomic analogue of deterministic microeconomic behavior. Such an inversion, we believe, supports rather than detracts from our comparison of rational expectations and physics.

RATIONALITY AND DUALITY IN ECONOMICS AND PHYSICS

The path we have taken in comparing economics and physics may be troubling to many thoughtful economists. Their objection could be that human beings are very different from the objects which are studied by physicists. Therefore, economics as a science will run into serious difficulty if economists attempt to be "social physicists." We can do no more than lend full support to this notion; but we take this injunction to mean that it is Newtonian physics that economics ought not to emulate. However, our discussion of holography in the previous chapter, we feel, lies outside this injunction forbidding economics to emulate physics. Our discussion of holography permits us to raise important issues and to become aware of recent dilemmas in physics. What may be surprising to the economist is that recent dilemmas in physics take a psychological turn. Thus, our discussion of contemporary physics may have the opposite impact on our conception of economics than nineteenth-century physics has had on neoclassical economics, leading us back to, rather than away from, psychology.

We have introduced the wave-particle duality so important in modern physics precisely because it takes a psychological turn. Many thoughtful physicists are aware that the problem of observation which they encounter in quantum mechanics inherently has psychological dimensions and raises the problem of human consciousness. In this section, we aim to extend the notion and problems of duality in physics to economics. We hope to accomplish this by fashioning a dualistic conception of rationality. Since the wave-particle dualism in physics is inherently psychological, a duality of rationality modeled on wave-particle duality will also be psychological in nature. This is how we would like it because economics has limped along for so long without a psychologically robust conception of rationality. We begin by taking up the wave-particle duality as a psychological question and then reformulate this duality in terms of rationality for economics.

Quantum Mechanics and Human Consciousness

The psychological problem which arises from a consideration of the wave-particle duality involves more than indeterminacy and measurement, it involves human consciousness. What is needed to reduce the ambiguity inherent in quantum theory is a conscious, intelligent observer. Weimer (1980:3) puts it this way:

> The problem, grossly oversimplified, is that a conscious, percipient observer (rather than an inanimate device) is necessary to determine which of several possible state configurations a quantum system is in. It is not the traditional problem of refining measurement, but rather that a cognizing subject must determine what a given measurement (or state description) _means_ in describing a possible state of the real world.

The information about a quantum state is found in a special equation called a state vector. This state vector is known as the Schroedinger equation of motion (also called the "psi" equation).[8] Formally, the Schroedinger equation has the same functional form as any wave equation. The Schroedinger equation, however, is a "probability wave" rather than a wave extended in some material medium in time and space. That is, the components of the Schroedinger equation do not represent a real wave as such; rather the components are square roots of the probability of finding a subatomic particle at a given location at a given time.

Since subatomic particles cannot exist simultaneously in all possible states (locations at time, t), it is very difficult to interpret the Schroedinger equation realistically. The probabilities relating to the various quantum states express the indeterminacy that seems apparent in quantum mechanics. To illustrate, let us consider a quantum situation in terms of the Schroedinger Psi equation, represented in functional notation by S:

$$S_i = S(x,y,z,t) \tag{1}$$

Equation (1) represents the amplitude of a wave field with spatial coordinates x,y, and z at an instant of time, t. For an electron, the probability, P, that it can be found in a specific neighborhood of x,y, and z is the square of the amplitude of S or:

$$P = [S(x,y,z,t)]^2 \tag{2}$$

Rather than going any deeper into the mathematics of quantum mechanics, we can simplify the situation a great deal and still show the things we have in mind. For example, rather than permitting S to take on many possible states, we could limit S to two states, $S_1(x_1, y_1, z_1, t_0)$ and $S_2(x_2, y_2, z_2, t_0)$. In other words, at time t_0, the wave equation has two possibilities, S_1 or S_2. To ascertain whether the state of the system at t_0 is S_1 or S_2, we need an observation device. An atom which is excitable by a passing electron could indicate whether the state of the electron is S_1 or S_2. Let the state of the electron before observation be represented by the linear combination of the two states:[9]

$$S_i = aS_1 + bS_2 \qquad (3)$$

where a^2 and b^2 are the probabilities for states S_1 and S_2 respectively. Let the state of the atom after observation be K_1 and K_2, where K_1 is the measurment that S_1 is the observed state and K_2 that S_2 is the observed state. After the experiment is completed, a wave function which describes the whole system (observed plus observer) can be formulated as:[10]

$$S_i = a(S_1 \text{ X } K_1) + b(S_2 \text{ x } K_2) \qquad (4)$$

This wave equation for the joint system, the original state of the particle plus the state of the observational device, is a linear combination of the two separate systems. Again, the probability that the state of the particle is S_1 and observed as K_1 is a^2; likewise, the probability for S_2 being observed as K_2 is b^2. Thus, equation (4) is a wave function which completely characterizes the joint state of the particle and its observational apparatus.

The state of the joint system is characterized much differently if a human observer is substituted for the atom as an observational instrument. Let us suppose that the observer sees a flash when the initial state of the particle is S_1 and no flash when the initial state of the particle is S_2. Now the observer answers yes, K_1, if he sees the flash, S_1; and K_2 if he does not see the flash, S_2. Again, the initial state of the system can be represented by equation (3). However, once this latter experiment takes place, we need a new equation to describe the state of the joint system, object plus observer. Equation (4) is somewhat misleading if the observer is a human being. A conscious observer will report either that he has seen (S_1 X K_1) or has not seen (S_2 X K_2) the flash. For equation (4) to be the response of a human observer, such an observer essentially would have to be in a suspended, quiescent state of suspended animation (Wigner, 1967:180). Thus, either (5a) or (5b) represents the state of the joint system (S_i) after the experiment with a human being as an observer:

$$S_i = (S_1 \text{ X } K_1) \tag{5a}$$

$$S_i = (S_2 \text{ X } K_2) \tag{5b}$$

For either (5a) or (5b) to obtain, the linearity of the equation (4) must be in error.[11]

The points made in the previous two experiments, one with an atom as an indicator and the other with a person as an observer, can be combined into one example. This example is known in the literature as "Schroedinger's cat."[12] Suppose that a photon is directed toward a half silvered mirror. The photon will be reflected or pass through the mirror. If it passes through the mirror, the photon activates a triggering device. This triggering device fires a gun which kills a cat that is placed in a box. After the experiment has taken place, the state of the system can be characterized by equation (4). The cat is either alive with probability a^2 or dead with probability b^2. An equation like (4) represents all of the information that quantum mechanics can specify about the state of the cat. Indeed, before any individual looks in the box where the cat is placed, it is impossible to say whether the cat is dead or alive. But when someone goes and looks inside the box, he sees that the cat is either dead or alive. Consequently, the state vector collapses from (4) to (5a) or (5b).

In brief, our understanding of what modern physics is all about is being altered. The distinction between determinacy (Newtonian mechanics) and indeterminacy (quantum mechanics) may be somewhat beside the point. If one takes a closer look, the intolerable ambiguity suggested by indeterminacy at the quantum level may require the cognitive intervention of a conscious human being. In this context, human consciousness becomes the determining factor in the process of observation. Thus physics, supposedly science par excellance, essentially returns to psychology. Economics may be several decades behind physics in recognizing the importance of the apparent indeterminacy of economic phenomena. But at least economists have the opportunity to recognize that economics may also return to psychology. How economists interpret and treat expectations is of vital importance to the progress of the discipline.

Rationality and Duality in Economics

In our initial comparison of economics with physics in this chapter, we argued that, rational expectations theorists invert the determinacy-indeterminacy relationship found in instrumentally interpreted physics. In the rational expectations view, macroeconomic activity was the indeterminate, aggregate analogue of determinate, individualistic microeconomic behavior based on rational maximization. By comparison, in physics, the micro-macro relationships relative to determinacy were the opposite. However, the determinacy issue was directed toward a different issue in the previous section. The problem of measurement and observation at the quantum level took a turn toward psychology. Indeed, the indeterminacy implied by the quantum situation seemingly required the participation of a conscious, intelligent human observer as a co-determiner in the process of observation.

The preceding comments taken together with an understanding of the Schroedinger Psi equation have implications for economic analysis. Our problem to this point concerns the restrictiveness of rationality if construed solely as maximization. Maximization, according to Friedman's essay, is not necessarily related to self-conscious maximizing behavior. Taking this aspect of positive economics seriously and consistently, positivistic interpretations of rational expectations, seemingly would follow Friedman and deny any claim about psychological realism. This means that rational expectations are really statistical expectations and ought not to have any psychological reference. Such psychological reference is an appeal to realism which is unwarranted by the positive economist's own point of view.

What implications would a more psychologically robust conception of rationality have for economics? This question is left for the next section. However, we first need to sketch the outline of a more general theory of rationality. Since the problem and role of consiousness arises in the quantum context, one possibility could be to reformulate our conception of rationality along the lines suggested by quantum theory. We propose, at this point, to formulate a theory of rationality, which is quite similar in its broadest features to quantum mechanics.

We shall argue for a theory of rationality which follows, to some extent, the Schroedinger Psi equation. Rather than having a probability wave function, we are suggesting a "rationality wave" function.

For example, in chapter seven, we suggest that human behavior be understood as a hierarchy of rationalities. This results from the notion that human beings are capable of manifesting varying degrees of complexity in human action. To simplify our theory of rationality, let us assume that, at the very least, there are two modes of complexity manifest in human economic activity; one is very repetitive and habitual, while the other is non-repetitive, processive, and manifest in novel situations. For purposes of discussion, let the repetitive and habitual modes of economic activity be denoted as behavioral rationality and let non-repetitive and processive manifestations of human economic activity be denoted as process rationality.[13] By behavioral rationality in economics we mean maximization, while by complex rationality we have a concept of rationality like nonjustificational rationality in mind (see Figure 5, p. 132).

Following the Schroedinger wave equation, we can give a similar formulation to the problem of rationality. Let S_B be a behavioral conception or state of rationality and let S_P be a complex conception or state of rationality. If we assume that information sets (I) and decision processes (variables, V) characterize a state of rationality, then our rationality function takes the following form:

$$S_i(I_i, V_i) = P_B S_B(I_B, V_B) + P_P S_P(I_P, V_P) \tag{6}$$

where i = B or C and P_B and P_P are the probabilities that the state of rationality is habitual (behavioral) or more complex (processive).

Interpreting equation (6) as we interpreted the problem of observation at the quantum level has some interesting implications. If we call equation (6) a rationality function, then it implies that human economic phenomena manifest differing degrees of complexity. The varying degrees of complexity to human economic phenomena might be thought of as being nested within one another. Which particular mode of rationality is manifest at any point in time depends on the individual, his information sets, and the number of decision variables or processes under consideration. For example, it seems to be the function of attention to determine which mode of rationality that a person is in.[14] Human processes where attention is focused are more indeterminate than human phenomena which are largely habitual and exhibit the absence of attention. Thus, we conclude from the preceding, that maximization as a theory of rationality confines economic phenomena to relatively simple modes of human economic activity.

We can focus on the differences between behavioral and complex modes of rationality, by elaborating some aspects of each. Behavioral rationality we take to be a one-dimensional theory of rationality. For economics, this unitary dimension is maximization. Thus, V_B represents only one mode of economic activity. Maximization requires information for the decision-making process to take place. I_B represents all of the information that is relevant to this monistic decision variable, V_B. The limits imposed on I_B relate to human memory and computational capacities. Of course, a one-dimensional maximizing process may be augmented by artificial intelligence devices, like computers. Once an algorithm is developed and is operational, computers obviously augment a one-dimensional maximizing process due to the superior memory and computational facility computers afford the individual transactor.[15]

By way of contrast, complex rationality is multidimensional. Differing modes of argument and different levels of abstraction are considered simultaneously. Thus, there are many V_C or complex decision processes which may be followed. Indeed some V_C may be nested within others. To illustrate, in our conception of nonjustificational rationality, we argue for nine different modes of decision-making. There is evidence to suggest that the simultaneous juxtaposition of alternative modes of conceptualization is essential to creative thought and thus intrinsic to complex modes of thought or rationality.[16]

Our conception of rationality has another significant difference with maximization as extended to "expectations" in the rational expectations hypothesis. Economists often assume that the search for information is itself a maximizing process limited by a decision which equates costs of search to the extra benefits. The search for information to operationalize V_B (maximization) ends when the marginal cost of the search apparently equals marginal benefits.[17] One problem with this type of calculation is as follows: One's decision of when to search for information and when to cease must also be based on information. In this context, the problem arises of how one can search for information about searching for information. Sooner or later the explanation of search as a maximizing, calculating decision will break down. When the past is not an adequate guide to the future or one has a limited repertoire of experience, then one must rely on trial and error or some other decision-process. From the preceding arguments, we conclude that the search for information is one of the indicators of complex rather than behavioral rationality. When search is reduced to maximization, all that is left is a mechanical computation rather than search. Thus, searching for information we take as evidence concerning the irrelevance of behavioral rationality as a maximizing process.

Our theory of rationality as developed to this point has an important implication for the investigating economist. The economist, like the physicist, faces a problem of observation which raises psychological issues. In the previous section of this chapter, we argued that human consciousness is necessary to reduce the ambiguity which results in quantum observation. A conscious observer has a role to play in helping to determine what quantum state is encountered in an experimental situation. Likewise, we postulate that an economist must make a decision as to what state of rationality is encountered in human economic phenomena. To this point, virtually all economists have presumed that maximization is an adequate conception of rationality. Our rationality function (equation 6) suggests that this a priori assumption on the part of many theorists is invalid. For example, consider what our rationality function implies. From a neutral objective point of view, the rationality function implies that the state of rationality found in economic phenomena is ambiguous. Human beings are capable, within limits, of structuring their state of conscious attention. Thus, human transactors are quite capable of altering their own subjective state of rationality from S_B to S_C and back again. The economist may not know on apriori grounds what state of rationality is characteristic of certain economic phenomena. Indeed, both states of rationality may be relevant.

What is needed is agreement among economists concerning what circumstances warrant the use of a behavioral conception of rationality and what circumstances warrant a complex conception of rationality. Some may suggest that perhaps the economist ought to become a participant in the economic process which he is observing and use his judgement gained experientially to ascertain the appropriate mode of rationality. One difficulty is that the presence of the economist in the process may alter the process itself, making data from such a process unreliable.[18] For example, the very presence of an investigator attempting to gather data from human subjects alters the state of attention and hence the state of rationality of the subject.

One way of restating the issues presented above is this: Our considerations of rationality in economics in relation to the psychological aspects of quantum theory has led us to reformulate the conception of rationality in economics. Rather than confining economics to a simple theory of rationality like maximization, the theory of rationality can be reformulated. But going beyond the use of the analogy from physics, rationality can be seen as a metatheory of the complexity of human economic phenomena. It is from a metatheory of rationality that criteria for the relevance of certain types of scientific analysis will come. Equilibrium analysis is relevant to phenomena characterized by maximizing behavioral rationality. Disequilibrium-process analysis is relevant to phenomena characterized by creative intelligence and cognitive interchange, where the active attention of human transactors obviously has significant economic implications. Thus, a metatheory of rationality implicitly defines the scope and limit of the various modes of scientific endeavor in economics.

RATIONALITY, STRUCTURAL AMBIGUITY, AND DUALITY IN ECONOMICS

The themes pursued in this chapter--duality, ambiguity, indeterminacy, and rationality--are such basic concepts that they have significant implications for our vision of economic processes. What is needed at this point is a simple analytical expression which captures the implicit vision of economic processes set forth to this point. Because we have attempted to give our theory of rationality an analytical formulation, it will be easier to develop an analytical formulation of our conception of economic processes. This concept will be called the Principle of Processing Complexity (PPC). Like our rationality function, equation (6), we will give the PPC a dual formulation. This dual formulation is a consequence of: (1) our understanding that monetary controversy since Keynes is concerned with mind, rationality, and structural ambiguity; (2) that such issues, at the very least, lead to a form of (explicate order) dualism; and (3) that such issues require the presence of human consciousness as a determining factor to reduce what would otherwise be intolerable uncertainty, and ambiguity. Uncertainty and ambiguity pose no inherent dilemmas for transactors who have none of their mental abilities assumed away.

In recent monetary literature, the analytical formulation which suggests a vision of economic processes is Walras' Law.[19] Walras' Law explains the tendency of a whole economy toward equilibrium where aggregate demand equals aggregate supply at the full employment level of income and output via the real balance effect. However, the lag of the real balance effect as an adjustment mechanism may be too lengthy to be politically feasible--particularly for protracted recessions like the Great Depression (Patinkin, 1965:339). With respect to the speed of adjustment, rational expectations represents a much faster and more powerful endogenous adjustment mechanism than the real balance effect. The case against activist intervention in the economy by monetary and fiscal authorities requires a rapid adjustment mechanism, both in theory and in fact. Thus, the rational expectations hypothesis is a rather potent reincarnation of Adam Smith's invisible hand which also restores the relevance of Walras' Law.

Mathematically, for a simple four market macro model as found in chapter five, Walras' Law can be formulated as follows:[20]

$$\sum_{i=1}^{3} P_i S_i = \sum_{i=1}^{3} P_i D_i \tag{7}$$

where D_i and S_i are the quantities bought and sold in the market place. If equation (7) holds, then this economic regime is in a state of general equilibrium. This means that in aggregate, each excess demand equals zero; in such a situation, this economic regime would be in full-employment-equilibrium with price stability. Walras' Law as represented above encompasses only three markets (commodities, bonds, and labor) because, in equilibrium, money is redundant and has no significant role in the economy. The insignificance of money in general equilibrium is based on the mathematical notion that n-1 equations are sufficient to determine an equilibrium system of n equations.

In such a system, the demand and supply equations of money are implied by the equations of the other three markets in the model.

However, Walras' Law as outlined above, hardly approaches our vision of economic processes found in the previous sections of this chapter. The ambiguity and indeterminacy apparent in economic phenomena may be a consequence of the complexity of human rationality and decision-making relating to economic questions. Indeterminacy and ambiguity in both physics and economics apparently is an indicator of human consciousness and cognition. Where consciousness and cognition are evident, disequilibrium and theoretical dualism also seem to be manifest. But dualism and disequilibrium also indicate that a view of phenomena as a process may be more informative. What we need is an analytical device which captures the basic nature of economic activity as a process and which indicates the economic consequences of transactors who engage in complex processes of (nonjustificationally) rational decision-making.

Now the view of the transactor we have in mind is that of a transactor who rivals the decision-making processes of the scientist in complexity, criticism, and in the search for and the use of information and evidence.[21] Our transactor is committed to a process _per se_, because his experience teaches him that ultimate success or failure lies as much in the process as in external circumstances. Novel ideas and opportunities appear as the process of decision-making unfolds; without initiating the process, novel circumstances cannot be forseen or perhaps even imagined. Additionally, at each point in the process, alternatives arise which need to be explored. Because such alternatives are not completely forseeable, the transactor lacks sufficient information to make well-informed choices. In situations of this type, maximization may be an impossibility; the transactor must rely on other modes of decision-making, perhaps some like we suggest in nonjustificational rationality. Nonjustificational rationality is an apt description of this type of economic decision-process, because the transactor does not have sufficient information to justify his choices. In such cases, he must rely on his judgement of the situation, which most likely is based on several modes of assessment. In brief, a decision-process like we have in mind must be multidimensional, both in theory and in practice.

If our view of creative decision-making processes is descriptive of even a minority of transactors, then such entrepreneurial activity seemingly implies disequilibrium. Such entrepreneurial types continually plan and revise their plans. This means that market supply and demand are constantly in flux.[22] However, there are factors which constrain the scope of such decision-processes. The entrepreneur must be willing and able to finance both his plans and the financial consequences of past activities. In this regard, money is a societal memory-keeping structure.[23] The tremendous economy of information which money affords the transactor no doubt facilitates decision-making and specialization in production (as well as diversity in consumption); but the constraints ultimately confronted are financial in nature. Ultimate success or failure is defined in financial terms.

Based on the preceding argument, we maintain that there are two features of an economic regime with genuine monetary processes of exchange, production, consumption, and distribution: (1) In nominal terms, individual transactors and budgets are always in balance;[24] and (2) in real terms, supply and demand are perpetually in flux. Our Principle of Processing Complexity is designed to capture these two features. We recognize that monetary exchange may give the appearance of market clearing; since, in nominal terms, transactions and budgets are always in balance. We also recognize that even the nominal appearance of market clearing does not preclude real disequilibrium. Therefore, we state the PPC in dual fashion as two relations which simultaneously apply to monetized exchange, production, consumption, and distribution:

$$\sum_{i=1}^{4} P_iQ_b = \sum_{i=1}^{4} P_iQ_s \tag{8a}$$

and

$$\sum_{i=1}^{4} P_iD_i^r \leqq \sum_{i=1}^{4} P_iS_i \tag{8b}$$

where Q_b and Q_s are the units bought and sold and D_i^r and S_i are the realized demand and notional market supply curves. The PPC means that (1) nominal purchases (P_iQ_b) and nominal sales (P_iQ_s) are identically equal due to the tautology behind the accounting function of money; and (2) that inadequate or excessive aggregate demand may be pervasive. The PPC expresses the nature of economic activity that exhibits varying levels of complexity and rationality.

The PPC is also a theory of how a market economy adjusts. In contrast to the adjustment mechanisms implied by the real balance effect and by the rational expectations hypothesis, the PPC suggests that adjustment is processive rather than mechanical. The more novel the situation, the higher the degree of rationality needed to meet changing circumstances. The more complex the degree of rationality and decision-making, the less predictable and determinate is the adjustment. Human transactors in novel circumstances need time to try out various strategies relevant to their economic situation. Thus, the adjustment of the economy to changing circumstances takes time; the more novel the circumstances, the greater the amount of time transactors need to explore the parameters defining their immediate situation.

If expectations truly affect our view of the self-correcting nature of the economy, then the PPC reflects a view of macroeconomic processes more like those of Keynes than rational expectations theorists. The more sophisticated the processes of thought, decision, and expectation relevant to economic circumstances, the more protracted may be the process of adjustment. Even if such adjustment is descriptive only of a small minority of transactors, this minority may be quite significant in terms of their impact on the economy. Significantly situated decision-makers may make key decisions in our economy.[25] The impact of such well-placed decision-makers potentially has large multiplier effects on the economy. In other words, our conception of economic rationality as a nonjustificational argumentative process, does not have to be universally descriptive of all economic transactors in their economic activities. For the PPC to be an adequate description for macroeconomic adjustment in a monetary economy, we need only a small minority of well-placed transactors who bring to their economic activities the sophistication, skills, and attitudes rivaling those of the scientist. In this regard, the rational expectations position abstracts from the process in which expectations are formed and presupposes the most favorable external circumstances for economic choice.

To recaptiulate, in broadest terms, our view of economic processes brings us to conclusions not unlike those we found for physics. How an economy adjusts may not be as independent of human consciousness as most economic theories implicitly assume. Moreover, the economy may not be fully independent of conscious attempts to measure or manipulate the economy. If the efforts of economists and authorities to assess or alter the economy affect the state of rationality of transactors, then such "exogenous" effects become internalized within the economic system. Transactors may be capable of being provoked into using their most creative efforts to hinder or enhance the policies of those authorities influencing the economy. The indeterminacy and ambiguity implied by such a view of economic affairs is captured in the Principle of Processing Complexity. The dual form of the PPC is meant to convey the same sense of ambiguity present elsewhere in science and manifest in other dualisms; wave and particle theories of light and the mind-body dualism. One aspect of the PPC captures the tautological repetition and identity of nominal transactors; while the other aspect captures the unobserved and hierarchical processes of economic rationality--expectation, planning, and decision-making.

SUMMARY

Our argument is that the problems of mind and rationality lie behind recent monetary controversy. Mind and rationality are two issues which most scientists wittingly or unwittingly choose to ignore. However, mind and rationality also reappear in contemporary philosophy of science as well as physics. If we interpret rational expectations as realistically as possible (ignoring the positive economist's injunction against realism), then the problems of mind and rationality also reappear in contemporary economics. We attempt to demonstrate how a theory of rationality relevant to the psychological processes in monetary phenomena can be reformulated in light of some dilemmas in quantum physics. The dilemmas in quantum physics can be reformulated as a thrust toward psychology, since human consciousness is needed to resolve ambiguity inherent in the experimental situation. When these psychological considerations in quantum physics are transferred to economics, we have the outlines of a more general theory of rationality. Rationality is really a hierarchy of rationalities which are, to a certain degree, under the control of individual transactors. In Bohm's terms, each type of rationality is always a part of the implicate mental order, but only one degree of rationality is explicated at any one time.

All of the preceding considerations imply nothing less than an alternative vision of economic processes. In our view of the economy, individual decision-makers make complex decisions requiring the full use of one's attention and cognitive capacities.[26] Such a perspective emphasizes the interdependence among individuals who continually structure (plan) and restructure their economic situation. Our Principle of Processing Complexity is developed to capture this dynamic, processive view of economic activity in a simple analytical formulation. The PPC is formulated dualistically to show that the attempt to capture the effects of human consciousness in economics, at the very least, leads to theoretical dualism. Theoretical dualism implies ambiguity, but not complete indeterminancy in one's economic situation. Indeterminacy can be reduced by the creative and processive use of one's intelligence.

But there are no guarantees for the economy as a whole. Decentralized processes of adjustment may fail to be stabilizing in certain historical situations. Our PPC encompasses this possibility as well as the synchronous meshing of plans and decisions. In this regard, both Keynes' and rational expectations theories may represent special cases.

NOTES

[1]For comments on instrumentalism, see chapter two, note 4, p. 43 and chapter six, note 2, p. 192.

[2]See Dampier (1971 [1948]), Jammer (1974), Nagel (1961), and Bohm (1978).

[3]The transformation of one particle into another is discussed by Capra (1975:211-248). Bohm (1978:101) suggests that matter might be present even in a vacuum: "The laws that relate matter to the vacuum might be of an entirely different character than the common laws of matter itself, relating one part of matter to another. Matter is somehow enfolded in the vacuum."

[4]"Wavicle" is a term used by Pribram (1978:96). No doubt others have used this term to refer to the wave-particle nature of quantum phenomena.

[5]The position is epitomized by the views of Werner Heisenberg (1953). Heisenberg's views are criticized by Bohm (1978:84): "Heisenberg was not a completely consistent positivist. He said that the electron has in some sense a position, which is disturbed. Thus he used a highly nonpositivist argument to justify a positivist conclusion, which is perfect confusion, you see. It is nonpositivist to say that the electron is disturbed in an unknown way, but he concluded from this that there is an ultimate limitation on our knowledge of precisely where the electron is, which is very positivistic. From the unknowable, Heisenberg thus concluded something about the limits of the knowable."

[6]See chapters five and six for a more complete treatment of the rational expectations hypothesis.

[7]Sargent (1976b:66) maintains that rational expectations models may not predict well, but that such models predict as well as any other model: "The econometric evidence is not spectacular in ruling against either the natural rate hypothesis or its potential competitors."

[8]For development and discussion of the Schroedinger equation see Levich, Myamlin, and Vdovin (1973:3-55), Nagel (1961:293-297), and Jammer (1974:24-33).

[9]Our presentation at this point follows Wigner's (1967:171-184) discussion.

[10]Levich, Myamlin, and Vdovin (1973:18) show that a linear combination of wave functions is also a wave function describing one of the possible states of a quantum system.

[11]Wigner (1967:183) suggests that the non-linearity of the equations is an indication of life and consciousness. Similarly, for economics we must wonder if linearity in formal models like rational expectations models effectively reduces economic activity to mechanical interaction.

[12]See Weimer (1980) and Bub (1979) for a discussion of this topic.

[13]Our distinction between behavioral and process rationality is quite similar to Simon's (1976) distinction between substantive and procedural rationality. However, we go beyond Simon's conception of procedural rationality to nonjustificational rationality.

[14]As noted previously (chapter eight, note 17, p. 262) H. A. Simon has suggested that attention may have economic implications as a scarce resource.

[15]We previously discussed the role of computers in decision-making in chapter seven, pp. 212-216 and 218-219.

[16]Psychiatrist Albert Rothenberg has studied processes of creative thinking and has found an element of contradiction is evident in truly creative thinking. In other words, a creative act often synthesizes what were previously thought to be contradictory opposites. Rothenberg (1971:197) calls this type of thinking "Janusian thinking," because Janus was the Roman god with two faces looking simultaneously in opposite directions.

[17]Feige and Pearce (1976) modify rational expectations to account for the costs of searching for information. Their concept, "economically rational expectations," is a half-way house between adaptive and rational expectations as discussed in chapter six.

[18]Friedman, has noticed this problem, but did not carry through and make this problem central to his conception of economic science as we do in our theory of rationality. Friedman (1971 [1953]: 24, note 4) states, "The interaction between the observer and the process observed that is so prominent a feature of the social sciences, besides its more obvious parallel in the physical sciences, has a more subtle counterpart in the indeterminacy principle arising out of the interaction between the process of measurement and the phenomena being measured. . . ."

[19]An interpretation of Walras' Law has been a source of much controversy. See Becker and Baumol (1952), Encarnacion (1958), Lange (1942), Patinkin (1965:645-650), Clower (1965), and Leijonhufvud (1968: 81-102).

[20]Our interpretation of Walras' Law varies from the usual interpretation in the general equilibrium literature. See note 28, chapter five, p. 169.

[21]We realize that our view is not a general view of the transactor; rather it is a view of the transactor relating to his most creative economic endeavors. Many transactors direct their most creative efforts to non-economic activities or are situationally constrained from participating in market processes.

[22]Fusfeld (1980:22) argues that, "randomness in the demand curve has unanticipated and probably ungeneralizable consequences." Later in the same paragraph, Fusfeld draws the same conclusion for factor supplies.

[23]Georgescu-Roegen (1971:307) views economic processes as extensions of biological processes. Economic processes like biological processes need memory systems. The monetary system performs such a function, but less deterministically than economists imagine. Karl Pribram (1980:22) explicitly captures our point of view: "When listening to Hayek, for instance, I was struck with the importance he placed on the distribution of information in the marketplace which allows each individual to act with respect to the whole. Are we economic monads perhaps? Is the supply of money the hologram of the marketplace and is the neural hologram the marketplace of the brain?"

[24]Our development of the PPC is based on the work of Ostroy (1973), Starr (1972), and Clower (1965). The dual formulation of the PPC, we view as an extention or generalization of Clower's (1965) dual-decision hypothesis.

[25]See note 5, chapter ten, p. 312.

[26]Our view of the individual decision-maker is different from the neoclassical view of individual behavior. Rather than viewing the individual as isolated from other transactors in Robinson Crusoe fashion (Robbins, 1935:19-12), nonjustificationally rational transactors will be linked economically and argumentatively to those around them.

Nonjustificational rationality could also serve as a conception of institutional rationality. It may be quite rare for one individual to be in command of all the various types of arguments and levels of analysis which characterize nonjustificational rationality. Perhaps some argumentative specialization is necessary. Thus, in our view, institutions like the Fed, labor unions, and large corporations would qualify as loci of decision-making.

Chapter 10

SUMMARY AND CONCLUSIONS

We have nearly completed the strategy proposed in chapter one of analyzing recent macroeconomic theory and thought in light of recent philosophy of science. What remains is to summarize and review the arguments and to draw the major conclusions from those arguments. However, implicit in the arguments and conclusions are suggestions for further research. In the following paragraphs, we first review the arguments developed to this point and draw some general conclusions; second, we explore the implications of our arguments and conclusions for an alternative conception of economic science; third, we outline potential topics for further research.

THE ARGUMENT AND ITS CONSEQUENCES

We began our inquiry in chapter one by proposing a novel strategy for relating philosophy of science to monetary theory and controversy. We noted that philosophy of science and economics were both concerned with human rationality. Since in philosophy of science a theory of rationality had developed beyond a simple logico-behavioral conception to a process theory of rationality, we gave priority to the concept of rationality in philosophy of science. This concept of rationality, nonjustificational rationality, was then used to assess the maximizing conception of rationality as found in the rational expectations hypothesis. Our conclusions were as follows:

1) In Part I, we found that positive economics was not very positivistic in the sense of logical positivism, except with respect to mind and rationality.

2) In Part II, we argued that the microfoundations of macroeconomics really involved the issues of mind and rationality; nowhere was this more apparent than in the more realistic but inconsistent interpretations of rational expectations as dealing with psychological anticipation.

3) In Part III, we followed the problem of mind and rationality to the very foundations of modern science--quantum mechanics and relativity theory--and found the outlines of an emerging world view. This world view implied that man and his perceived universe are an organic whole. The holistic point of view was used to formulate a theory of rationality and an analytical device which expressed our vision of economic processes. The Principle of Processing Complexity captured pervasive disequilibrium which was reducible with the creative use of the cognitive abilities most human beings possess.

Taken together, the preceding arguments lead to one grand conclusion with two corollaries: The question of microfoundations so crucial for our understanding of Keynes, the Keynesian Revolution, and the economy itself, is also a question of philosophical foundations. Since a complete philosophy of science requires a conception of human rationality, a restrictive view of science may restrict economics to reductionistic, behavioral conceptions of rationality. At the very least, what economics needs is a philosophy of science, like our structural view of science, which adopts a process conception of rationality.

Otherwise, a process conception of economic affairs like those found in the views of Keynes, Hayek, Clower, Leijonhufvud, Shackle, Georgescu-Rogen, Simon, and others will wither away.

The two corollaries to our conclusion that the microfoundations question is really a question of philosophical foundations are as follows:

1) The rational expectations hypothesis, we believe, is the most consistent macroeconomic manifestation of positive economics and the Newtonian scientific world view in economics. We believe this world view is a dying world view and that rational expectations as a manifestation of this dying world view is its ultimate reductio ad absurdum.[1]

2) If there really is a new scientific world view now emerging and if the microfoundations debate is really a debate over a scientific and philosophical world view for economics, then we suspect a new conception of economic science will also emerge. Since we have relied on a structural philosophy of science with structural, processive components (knowledge, reality, and rationality), we would call such a view of economics, structural economics. Structural economics would be a study of the structures of human welfare, wealth, and income, the ambiguity of such structures, and the evolutionary processes of structural change. A more detailed elaboration of structural economics follows in the next section.

TOWARDS A STRUCTURAL CONCEPTION OF ECONOMIC SCIENCE

By now, it should be apparent that the dilemmas and difficulties we encounter with the rational expectations hypothesis ultimately take us to issues and notions which lead us to question the positivistic philosophical foundation of rational expectations. Since we have reason to question positive economics, we also have reason to question the rational expectations approach to monetary and macroeconomic problems. However, to complete our case against positive economics and the rational expectations hypothesis, we need to suggest the bare outlines of a more encompassing approach to economics. As already stated, we call this approach structural economics. Our conception of structural economics is designed to remedy the deficiencies which we have found with positive economics and rational expectations.[2] We define structural economics as the science which studies the real, dynamic rationality of human welfare, wealth, and income. Rationality refers to the orders, hierarchies, limits, patterns, processes, and ambiguities of human welfare as well as to the nonjustificational argumentative determination of human welfare through scientific, political, and social debate. We prefer even more active notions like ordering, limiting, patterning, processing, and forming hierarchical relationships to emphasize the dynamism of our view of economics.

We have much more in mind than our simple definition of structural economics might suggest. A structural conception of economics would incorporate the three components of our structural philosophy of science; a structural epistemology, a structural conception of rationality, and a structural conception of what it is that is really knowable. In Lakatosian terms, these basic assumptions constitute the metaphysical hard core of our conception of structural economics. In addition, structural economics must be compatible with and of the same order of complexity as political theories of complex decision-making. What we envision is a philosophy of science, a conception of economics, and a political philosophy which share a common conception of rationality, nonjustificational rationality.

Consider the implications of our structural, assumptive hard core for a conception of economic theory and economic policy. Economic theories are seen as tentative approximations to structural relations in actual human economic activity. But real economic activity is in a constant state of flux. From a statistical point of view, such flux may be manifest as randomness. However, human transactors have the capacity to structure and continually restructure their economic circumstances. Such capacity is obviously limited both by economic constraints and cognitive constraints of human intelligence. Human transactors only reach the appearance of queiscent, static equilibrium when they turn then attention away from economic questions and succeed in keeping their attention on other matters.

Obviously, the more uncertain are external economic circumstances, the more conscious attention must be devoted to economic concerns and the more indeterminate economic phenomena will appear to be. But indeterminacy presents the occasion for the exercise of intelligence, will, and emotion. In other words, indeterminacy presents the transactor with the occasion to processively determine his own economic situation. Since many individuals specialize in determining the outcome of economic processes, indeterminacy per se, becomes a concern when it results in a shift of "cognitive resources" from non-economic to economic questions. In a very real sense, attention is a scarce resource, which can and will be wasted if the economy functions poorly.

The processive character of economic phenomena sketched above, has strong implications for what economic theory will look like. The economic theories which result from our consideration of the exercise of conscious intelligence in economic affairs will be more tentative and more heuristic than neoclassical economic theory.[3] Time will enter the theory as a concern for process rather than as a mechanical, fourth dimension. In other words, notions of time may vary depending on the type of rationality apparent in economic phenomena. Mechanical, cardinal concepts of time become less appropriate, the more complex the conception of rationality.[4] Time as a concern for dynamic economic processes becomes part of our structural, processive hard core, rather than an attempted modification of an inherently static conception of economics.

From our processive portrayal of economic phenomena and its implications for economic theories, we can draw conclusions for economics as a whole. The dynamic, processive nature of economic activity, we believe, is sufficient to generate theoretical dualism if not pluralism. The structural complexity of human action apparent in our hierarchial conception of rationality means that attempts at a unified theory of economic activity are likely to fail. This means our basic theoretical distinctions are manifestations of varying orders of complexity in human economic phenomena which we tentatively infer as existing in the real world. More specifically, our argument implies that theoretical distinctions in economics, despite the fact that positive economists construe them methodologically, are ultimately manifestations of the varying orders of substantive complexity in human activity. In implicate-explicate order terms, theoretical distinctions in economics are alternative explicate order manifestations of a wholistic implicate order underlying economic man and the universe. Intellectual unity then most likely will not be found at the theoretical level but at the meta-theoretical level. In other words, unity in economics seems to be attainable only conceptually in the philosophy of economic science and not within economic theory itself.

Our Principle of Processing Complexity developed in the previous chapter captures the thrust of our preceding remarks. As a first approximation, the PPC is formulated as an analytical dualism which captures, in processive terms, what we believe is usually meant by the term disequilibrium. In terms of notional demand and supply, disequilibrium seems pervasive. As a response to such disequilibrium, some transactors can continually restructure their economic circumstances by becoming trading specialists.[5] Alternatively other transactors can structure their economic circumstances based on rules-of-thumb fashioned in evolutionary fashion through trial and error processes. For these transactors, economic activity becomes structured by habit and has the external appearance of individual equilibrium.

At this point, we have elaborated most of the aspects of structural economics but one. We suggested that structural economics must be compatible with theories of political decision-making processes. This qualification is necessary because of the way neoclassical economics has developed. It is a well-known fact that economics is a theory of efficiency and has little to say about equality. By this we mean the oft-repeated statement of price theory that any distribution of income and wealth is consistent with efficiency. Questions of equity are then left to those outside economics to answer; in other words, economics appears incomplete. This leaves monetary economists without much to say in a period of rampant inflation and high unemployment. Any policy that the economist advocates to resolve our present dilemmas has significant distributional consequences. If the economist advocates anything at all, he implicitly transcends the limitations of positive economic science.

In a monetary economy with sophisticated public and private monetary institutions, policy questions are bound to dominate. But at this point the economist needs an ethical criterion to avoid being accused of arbitrary policy-making decisions. Such an ethical criterion must come from political theory and philosophy. Since no criterion is likely to be self-evident, a processive political structure will need to be established to obtain such a criterion. At this point, individuals in the economy must be presumed to have an intellectual capacity just as complex as any professional economist or policy maker. In other words, distributional issues put transactors and economists on identical intellectual footing. At this point, the positive economist runs into a problem. A maximizing view of rationality assumes that transactors always react uniformly, at least on average, to changes in their economic environment. This means transactors are presumed to have no active role in changing their economic circumstances, or if they do, the process must be outside of economics.[6]

But notice what we have achieved with structural economics and nonjustificational rationality. We already have a theory which suggests that an individual can decide either to respond or to alter his economic environment. In other words, a nonjustificational political theory would be consistent with our structural philosophy of science and economics. In other words, we have conceptions of political, scientific, and economic decision-making which are mutually compatible and consistent.[7] We can not say the same for positive economics and rational expectations.

Actually, the beginnings of a nonjustificational political philosophy may already be evident. The Harvard philosopher, John Rawls (1971), has set forth an elaborate philosophical scheme which will yield ethical criteria for economic decisions in the political sphere. His theory of justice as fairness effectively elaborates the structure of a hypothetical "original situation" from which his maximin criterion follows. The maximin criterion is that the least advantaged in society should benefit most from policy-decisions which affect the economy as a whole. Although public finance specialists have discussed maximin in relation to fiscal redistribution schemes, to our knowledge Rawls' maximin criterion has not yet been brought into the discussion of monetary policy. Since redistributional consequences are compounded as the rate of inflation accelerates, this omission on the part of monetary economists ranks as a major oversight. Even for those who disagree with Rawls, the question of an ethical criterion or standard for monetary policy is of paramount importance.

In summary, we have argued for a structural conception of economics. Structural economics is derived from our structural philosophy of science. In this view, economic events as well as economic science are continually being constructed and restructured. We emphasize the dynamic aspects of economic events and processes by making dynamic considerations part of our assumptive hard core. This contrasts with positive economics where dynamics are seen as a modification of a static point of view. Additionally, we have fashioned our structural conception of economics to include a conception of rationality that is also adequate for scientific and political decision-making processes as well. Thus, structural economics exhibits an autonomy from other fields without, at the same time, becoming incompatible with them. After all, scientific, political, and economic decision-making may be more similar than different. Each of these processes deals with choice relative to information; the content of each decision may vary, but we have no reason to expect that the structure of rational decision will vary for the individual just because the topic changes from science, to economics, to politics.

TOPICS FOR FURTHER RESEARCH

It should be apparent that the way we have related economics to other disciplines has implications for other social sciences. The proposals suggested in the preceding paragraphs amount to nothing less than a new, structural conception of the social sciences and their interrelationships. However, we do not wish to further develop these notions. Rather we wish to outline topics for further research as they pertain to economics and follow from this thesis. The tasks left unfinished which need to be explored in the future include the following:

1. A reassessment of Keynes and Hayek and the relationship of their analyses.
2. A consideration of the instrumental interpretation of positive economics.
3. An exploration of an ethical criterion for monetary policy.
4. A further development of a theory of rationality.
5. Additional exploration of parallels between economics and physics.
6. A consideration of the type of economic system to which our notions might lead.

We may need to reassess the debate between Keynes and Hayek because our conception of rationality might attenuate their differences. Our nonjustificational conception of rationality, which we have used to interpret Keynes' General Theory and the Keynesian Revolution, relies heavily on the work of Popper and the Popperians. When this writer queried Hayek about Popper, Hayek in effect stated he has no disagreements with Popper.[8] If we take Hayek's close relationship to Popper to mean that Hayek holds a similar conception of rationality, then Keynes and Hayek may have something fundamental in common.[9] At the very least, we suspect that Keynes and Hayek would agree to oppose the excessively aggregative and mechanistic approach of rational expectations. At last they might be on the same side.

Since we have largely completed our analysis, it has become clear that positive economics is being significantly reinterpreted. In an important article, Lawrence Boland (1979) construes positive economics as instrumentalist philosophy of economic science. This development is interesting because it appears that positive economics is following a pattern established in recent monetary theory. Rather than Keynes and Keynesians, we appear to be on the verge of a Friedman and the Friedmanian's episode. The Freidmanians seemingly ask the question; "What did Friedman really have in mind in his methodology article?" Then they proceed to argue that Friedman is an instrumentalist, Popperian, or something else. We hope that those economists with a strong interest in philosophy of science can learn lessons from the earlier episode which don't have to be repeated in discussions of economic science.

Since we discussed an ethical criterion as a standard for monetary policy in the previous section, we have nothing to add at this point and move to the next issue--a theory of rationality. Our theory of rationality is strongly oriented to the real constraints and processes that affect actual decision-making processes. In this regard, our theory of rationality has something in common with H. A. Simon's recent work on rationality. Simon maintains that all of the most important results of standard economic analysis are consistent with his concepts of bounded or procedural rationality. If this is true, then we suspect that most of the usual results of conventional economic analysis are also consistent with our nonjustificational theory of rationality. Then our theory, like Simon's, has more to offer than the more traditional maximizing conception of rationality. Indeed, there may be an embarrassing richness of alternatives to positive economics and maximizing rationality.

With respect to physics, we suspect that we have not exhausted the potential for mutual benefit. As long as we avoid reductionistic interpretations of physics, we are not likely to carry alien and harmful concepts into economics. Since human consciousness now seems to be an integral part of scientific results in physics, what physicists learn may be of some use to economists. In particular, new notions of causality are being developed which might be appropriate for causality in a monetary economy. A conception of causality is radically different in a wholistic process than in a divisible mechanical system. A genuine monetary economy has properties closer to a wholistic process than a divisible mechanical system.

Lastly, if there is a new scientific and philosophical world view which is now emerging, then we suspect that the old debate between captialism and socialism must be rethought. Perhaps a new view of an economic system will emerge which is different from both capitalism and socialism. We hesitate to speculate what such a new economic order might look like; but the importance of the concepts and notions developed heretofore ought to have some concrete impact on the conduct of economic affairs.

NOTES

[1]Fusfeld (1980:42) expresses a similar point of view about Friedman: "A famous economist tells us, in effect, that it does not matter if a theory is unrealistic, as long as it provides useful predictions. These are signs of a dying theory, one that is no longer congruent with new ways of thinking about the world and human behavior."

[2]See note 10, chapter eight, p. 260, for a more elaborate notion of structure.

[3]This also means that predictability will have a much less significant role in structural economics.

[4]The distinction between cardinal and ordinal time is found in Georgescu-Roegen (1971:135).

[5]The notion of trading specialists was suggested by Clower (1977: 206-207). Also see our remarks concerning the entrepreneur in chapter eight, pp. 254-255.

[6]See note 17, chapter five, p. 167 for remarks relating to this implicit dualism.

[7]Note that our nonjustificational concept of rationality can describe institutions or individuals. Because of the requirements of a multi-dimensional concept of rationality are so demanding, communities of scholars and citizens may be necessary to explore each mode of argument. Thus individuals, for the most part, will specialize in developing certain types and levels of argument. However, under extraordinary circumstances, particular individuals may effectively command all of the types of argument at all levels of analysis. In science, no doubt, such extraordinary individuals effect scientific revolutions. See note 25, chapter nine, p. 298.

[8]Hayek was one of the principle participants of the Second Penn State Conference on Cognition and the Symbolic Processes held at University Park, Pennsylvania during May, 1977.

[9]This is best illustrated by Shackle and Leijonhufvud. Shackle's lifetime task has been to relate the monetary theories of Keynes and Hayek. A statement to this effect is found in the acknowledgements section of one of his major monograph's (Shackle,1971). Leijonhufvud's interest in the coordination problem of monetarized exchange appears to be heading in an Austrian direction.

GLOSSARY

Algorithm: An algorithm is a step by step procedure for solving a problem. For example a computer program is an algorithm.

Associationism: This is the theory of mind in utilitarianism. It is an empirical and mechanical theory of mind. The ideational contents of consciousness are totally derivative from the senses and the mechanical interaction of the senses. See chapter six.

Confirmation: This is the most general method of scientific procedure developed by logical positivists after their previous method, verification, was severely criticized. Confirmation is the method of empirical ascertainment which bases scientific conclusions on probabilistic inference drawn on the basis of a random sample. An hypothesis may be confirmable even though the relevant experiment may not be technologically or economically feasible.

Diaspora: This refers to the scattering of some group of people. Originally it referred to the dispersion of Jews outside of Palestine.

Empiricism: This is the theory of knowledge which derives all knowledge from sense experience.

Epistemology: An epistemology is a theory of what types of statements constitute genuine knowledge claims. Historically rationalism and empiricism are the two most basic epistemologies.

Falsification: This is the method of scientific procedure first developed by Popper to alleviate the logical problem with verification as a form of induction. Since all possible outcomes of an experiment are never observable, contrary outcomes may not actually be observed. The absence of disproving evidence does not mean none will be encountered. However, disproving evidence may permit a valid conclusion to be made. Dogmatic falsification requires a theory be rejected if the evidence is negative. Methodological falsification requires only that a theory be refuted; it may be retained if no better theory is available. Sophisticated falsification requires a better theory for a falsified theory to be rejected.

Fideism: This is an approach to philosophical issues which relies on faith rather than reason.

Hedonism: This is the behavioral psychology of utilitarianism. It means that all behavior is a result of seeking pleasure or avoiding pain.

Hologram: This is an optical device which processes large amounts of information. Most holograms are constructed with defracted laser beams. The information is stored in the wave interference patterns of the defracted waves, which can be captured photographically.

Holography: This is the process of using or making a hologram.

Holonomic: Holonomic is a more general notion than holography. Holonomic includes other types of wave interaction like aural and electrical in addition to optical systems. Information enfolded in any pattern of wave interference which is linear in function is called holonomic. Linearity facilities computability.

Idealism: This is the theory that reality transcends the phenomena of sense experience.

Instrumentalism: This is the approach to science which maintains theories are only useful intellectual vehicles which make scientific research more successful in terms of predictive content. The truth status and realism of theories are of no concern to the instrumentalist because these issues raise unanswerable questions.

Justificationism: This is a metatheory of rationality that all genuine knowledge claims must be derivable from an authoritative source of knowledge. For the empiricist, the authoritative justification of knowledge is sense experience; for the rationalist, innate reason is the authoritative justification.

Martingale: A sequence of observations is a martingale (random walk) if the mathematical expectation of the value of the next observation is the same as the value of the current observation or:

$$E(x_{t=1}/\ x_t, x_{t-1}, x_{t-2}, \ldots.)=x_t$$

See Sargent (1972:75, note 5).

Methatheory: A metatheory is a general conceptual framework which encompasses all phenomena in the domain of inquiry.

Methodology: This is a set of criteria which constitute the accepted decision-prodecure in any science.

Nonjustificationism: This is a metatheory of rationality which implies that knowledge cannot consist of authoritatively justified knowledge claims. Such authoritative justification is a logical impossibility. The nonjustificationist locates human rationality in a multidimensional critical process. No particular mode of criticism is authoritative by itself.

Ontology: This is a basic conception of the nature of reality.

Paradigm: A paradigm can be defined in at least three distinct ways: (1) As a set of solved problems to be emulated in explaining new phenomena; (2) As a set of inter-related theories (research program); and (3) As a conceptual framework or world view.

Rationality: This is a basic description of the order and complexity of human activity. Various levels of complexity may be apparent in human affairs. Thus, varying degrees of rationality are possible. Traditionally, rationalists base the order or rationality of human affairs in intuition and empiricists in sense experience.

Rationalism: This is the theory of knowledge which derives all knowledge from innate, apriori intuition.

Real Balance Effect: It refers to the changing real value of money holdings as the price level changes.

Realism: This is the theory that the objects of sense experience and cognition exist independently of the senses and the mind.

Reduced-Form: These are equations in which the dependent variable is expressed in terms of predetermined independent variables and the disturbance of the system. The coefficients of the reduced-form equations are functions of the coefficients of the structural equations.

Refutation: A theory is refuted if falsifying evidence is relevant to the theory in question. A refuted theory may not be rejected until a better one is found. A better theory must have greater empirical content to replace the theory which precedes it.

Rejection: A theory is rejected if it is known to be contrary to fact and if a theory with greater empirical content is developed which will take the place of the original theory.

Research Program: A research program is a set of inter-related theories which form the theoretical content of any science.

Science: Traditionally defined, it means to have or possess knowledge. We would redefine science as the process of acquiring knowledge.

Structure: This is a notion of pattern, order, and hierarchy in our world and in our knowledge of our world. We prefer the active notion of "structuring" to more static notions. Thus, structuring would cover the evolutionary aspects of changing patterns, orderings, and hierarchies. See note 10 to chapter eight, p. 260.

Testing: Testing is the method of scientific procedure which is identical to confirmation except for one important difference. For a theory (or hypothesis) to be testable it must be feasible to run the appropriate experiments. A theory is not testable if technological, economic, or other constraints prohibit experimentation.

Verification: This is the method of scientific investigation which permits a theory to be accepted on the basis of positive evidence. This method is now viewed as contrary to logic. See falsification and confirmation.

Utilitarianism: This is the nineteenth-century social and ethical philosophy based on the "greatest good" principle. Society is to maximize the total happiness or total utility of the greatest number of individuals. Psychologically, utilitarianism has two components: (1) a behavioral psychology--hedonism, and (2) a theory of mind--associationism.

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