

### A Critique of Theories of Imperfect Knowledge

The epistemological basis of the Rational Expectations Hypothesis is the same theory of knowledge which underlies virtually all neoclassical models which deal with uncertainty and information. It is the view that presumes (1) that true knowledge requires inductive proofs, and (2) that the acquisition of knowledge is constrained by what we called the 'inductive learning possibilities function', which itself is constrained by the currently accepted information set. The standard view of information and uncertainty is that it would be unrealistic to assume 'perfect knowledge'. Of course, it is unrealistic. It would take an unrealistic amount of information or time to provide the presumed necessary (inductive) proof of 'perfect knowledge'. A realistic neoclassical model would have to presume some form of 'imperfect knowledge' or equivalently be based on 'imperfect information'.

We wish to close this chapter with a simple, but fundamental, criticism of all models which employ some form of 'imperfect' knowledge or 'imperfect' information. Our critique is rather straightforward. How would one ever *know* his or her knowledge is imperfect? If one knows what is imperfect, then one must know what is perfect. For example, how would one know that his or her expectations are not true (perfect)? The answer to this question is rather simple. Those economists who assert that anyone's knowledge is 'imperfect' are merely stating a form of Conventionalism which demands the attainment of an inductive proof before anyone's knowledge is to be considered perfect. Since induction is impossible, perfect knowledge is declared impossible.

It should be obvious by now that our argument is that all current conclusions about the quality of knowledge or information are based on an acceptance of Inductivism and that the acceptance of Inductivism is not warranted. The common view that knowledge is imperfect is based only on the presumption that an inductive proof is necessary for knowledge to be true. There is no inductive logic, and there never was. A theory or an expectation can be true even though we cannot prove that it is true [Popper, 1972, ch. 3]. Furthermore, even the quality of the information is irrelevant whenever Inductivism is rejected.

As Bertrand Russell argued many years ago, the Inductivist or empiricist view of knowledge is a view that does not even qualify on its own terms. There is no inductive proof that says that for knowledge to be true it must have an inductive proof! This is a general problem with all Conventionalist theories of knowledge – they are all self-contradictory. They deny truth status to theories but the denial is itself a theory which is asserted to have truth status!

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## On the 'Necessity' of Microfoundations for Macroeconomics

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The centerpiece of microeconomics is the purely competitive auction market in which buyers and sellers participate atomistically as price takers and where supply and demand are equated continuously by variations in price. These individual markets aggregate into a Walrasian general equilibrium model.... In that aggregation of perfect markets, shifts in nominal aggregate demand affect only prices and never quantities.

Macroeconomics contrasts sharply with these implications of aggregated microeconomics. It begins with the observation that output and employment display significant deviations around their supply-determined trends.... These fluctuations around the trend of real activity are the 'business cycle'.... Clearly, the business cycle could not happen in aggregated classical microeconomics. Thus any macroeconomics that is connected to microeconomics by a solid bridge must explain how it departs from the classical micro model in its conception of the operation of markets.

...Keynes... departed from classical microeconomics *only* by modifying the labor supply function to include a wage floor.... But his bridge was defective; none of the explanations flowed directly from the implications of optimization by economic agents or from a specific institutional constraint. Many of [his] followers ...operated with no bridge to microeconomics. Instead, they adopted the 'fixprice method'....

The fixprice finesse may have helped economists develop a sharper focus and a better understanding of real relationships.... But it had important costs. One of these was a professionally disturbing gap between macroeconomics and microeconomics....

Arthur Okun, [1980, pp. 817-19]

the same highly special theoretical presuppositions which led to Keynes' original attack on orthodox economics continue to pervade contemporary price theory and ... the Keynesian Counterrevolution would collapse without them.

... Like us, Keynes does not in any way deny the generality of orthodox equilibrium analysis; he only denies that orthodox economics provides an adequate account of disequilibrium phenomena.

Robert Clower [1965, pp. 104, 109]

There is virtually no discussion among economists of a need for macrofoundations for microeconomics, except, perhaps implicitly, in the writings of some institutionalists. In contrast, the demonstration of the existence of microfoundations for macrotheories is considered essential by many leading economists. The reason is the same for both and is easy to find. Demonstrating the dependence of all macroeconomics on microeconomic principles is essential for the fulfillment of the (methodological) individualist requirements of neoclassical economics. However – and this is not widely pointed out – this 'necessity' presumes that microeconomic theory, in the form of general equilibrium theory, is a successful individualist program. In some quarters, as we explained in the previous chapters, this is still an open question.

### General Equilibrium vs Macrotheory

Before going much further, we need to make sure that our use of the widely used terms 'general equilibrium', 'aggregation' and 'macroeconomics' are clearly defined. Historically, the concept of a general equilibrium is distinguished from that of a partial equilibrium. Specifically, much of microeconomics is concerned with the individual maximizer or an individual market. The use of the Marshallian strategy of *ceteris paribus* implies a temporary methodological disregard for other individuals or other markets. Thus, followers of Marshall's economics often speak of a partial equilibrium of one market so as to make clear that they have not yet assumed that all other markets are in equilibrium. However, as we noted before, any market is in equilibrium only if all participants are maximizing [see Hicks, 1939/46; Clower, 1965].

#### *General equilibrium*

It is unfortunate that in the effort to avoid discussing cardinal utility and to switch to the more general concept of ordinal utility or preferences, Hicks (and to a certain extent, Samuelson) caused an unnecessary confusion of the concepts of equilibrium and maximization. Specifically,

in the mathematical appendix to his book [1939/46, appendix to ch. 1], Hicks refers to a maximizing individual as being someone in a personal equilibrium without also requiring that the rest of the markets be in equilibrium. Of course, maximization is a form of stable equilibrium, but by confusing an individual's maximization with an individual's equilibrium we ultimately lost the distinction between individual decision-making behavior and market price-determining behavior. For example, to the extent that the individual depends on a market, one individual may be maximizing, but only temporarily, unless the market, and every other individual, is in equilibrium as well. Without the assurance that everything else in the world is in equilibrium, an individual's actions toward planned maximization may not be consistent with what is usually meant by an equilibrium (which implies a minimum degree of stability or feasibility) – thus an individual can be in equilibrium *only if* the market is also in equilibrium.

Perhaps Hicks can be excused because he was interested in promoting a combination of Walras' and Pareto's approach to economic explanations – namely, general equilibrium theory [1939/46, pp. 1-25]. In this sense, the assumption of general equilibrium provides the necessary or required feasibility and market stability in a straightforward manner. It provides an assurance that nothing outside the purview of the individual will upset the planned maximization.

Contrary to Roy Weintraub's recently promoted non-Walrasian version of general equilibrium, we do not think it is easier to comprehend current research if that comprehension is not based on a distinction between Keynes' macrotheory and the traditional concept of general equilibrium. Weintraub's concept of general equilibrium is really a form of 'generalized' equilibrium that covers any 'questions of multimarket interactions' which allows for 'any level of aggregation' [1977, pp. 1-2]. When we refer to general equilibrium we will always mean the traditional view which presumes explicitly either that all markets are in equilibrium or that all individuals are maximizing.

It does not matter which way the concept of general equilibrium is stated, since there is no way one market could be in disequilibrium (i.e., at least one demander or one supplier is not realizing his or her planned actions) while everyone is still maximizing [Hicks, 1939/46, p. 58, fn. 1; Clower, 1965, p. 106]. Similarly, there is no way one individual could have realizable gains (i.e., not maximizing) when all markets are in *equilibrium*, since that individual will upset the equilibrium in order to make the changes necessary for maximization. Note that these conclusions are based entirely on what is meant by the term 'equilibrium' – namely, the continued existence of a stable balance in the absence of any changes in the *exogenous* variables.

With this view of the minimum conditions for equilibrium, in order for an individual to be in an ‘equilibrium of the consumer’ (to use Hicks’ term), all givens – such as the prices of all goods – would have to be fixed or stable. Only when one or more of the markets are not in equilibrium would the given prices not be stable or fixed. Thus we see that Hicks, by identifying the individual’s maximization (or optimization) with the individual’s equilibrium, has in effect built in a presumption of general equilibrium in order to explain the behavior of any individual. We argue that this is a major source of the difficulties that have led to confusions concerning the differences between macroeconomics and general equilibrium analysis.

*Macroeconomics: Keynes’ ‘departure’*

In his 1937 *QJE* article, Keynes attempted to explain to his critics how his *General Theory* was a departure from ‘previous theories’. He discussed two major points. First was the matter of uncertain expectations:

recent writers like their predecessors were still dealing with a system in which the amount of the factors employed was given and the other relevant facts were known more or less for certain. This does not mean that they were dealing with a system in which change was ruled out, or even one in which the disappointment of expectations was ruled out. But at any given time facts and expectations were assumed to be given in a definite and calculable form; and risks, of which, tho admitted, not much notice was taken, were supposed to be capable of an exact actuarial computation. The calculus of probability, tho mention of it was kept in the background, was supposed to be capable of reducing uncertainty to the same calculable status as that of certainty itself....

Actually, however, we have, as a rule, only the vaguest idea of any but the most direct consequences of our acts....

By ‘uncertain’ knowledge ... I do not mean merely to distinguish what is known for certain from what is only probable.... Even the weather is only moderately uncertain. The sense in which I am using the term is that in which the prospect of a European war is uncertain, or the price of copper ... twenty years hence.... About these matters there is no *scientific basis* on which to form any calculable probability whatever. We simply do not know....

I accuse the classical economic theory of being itself one of these pretty, polite techniques which tries to deal with the present by abstracting from the fact that we know very little about the future. [1937, pp. 212-15, emphasis added]

His second major departure, according to Keynes, was concerned with the absence of an adequate macrotheory, specifically with

the traditional [theory’s] ... apparent conviction that there is no necessity to work out a theory of the demand and supply of output *as a whole*. Will a fluctuation in investment ... have any effect on the demand for output as a whole, and consequently on the scale of output and employment? What answer can the traditional theory [which he noted above ‘takes the amount of factors as given’] make to this question? I believe that it makes no answer at all, never having given the matter a single thought; the theory of effective demand, that is the demand for output as a whole, having been entirely neglected for more than a hundred years.

My own answer to this question involves fresh considerations. I say that effective demand is made up of two items – investment-expenditure ... and consumption-expenditure. Now what governs the amount of consumption-expenditure? It depends mainly on the level of income. People’s propensity to spend ... is influenced by many factors such as the distribution of income, their normal attitude to the future and ... by the interest rate. But in the main the prevailing psychological law seems to be that when aggregate income increases, consumption-expenditure will also increase but to a somewhat lesser extent.... This psychological law was of the utmost importance in the development of my own thought, and it is, I think, absolutely fundamental to the theory of effective demand as set forth in my book.... [1937, pp. 219-20]

It is easy to conclude from these fragments of Keynes’ own view of his departure that he was not arguing that macroeconomics lacked microfoundations. Rather, he was arguing that the traditional (micro) theory lacked necessary macrofoundations? We should also note, for future reference, that Keynes did not disagree with the hidden agenda of neoclassical microeconomics. First, when he referred to the lack of a ‘scientific basis’ for expectation formation he merely meant the lack of an *inductive proof* – that is, he still accepted the Problem of Induction. Second, to deal with the Problem of Induction in the 1937 article he specifically identified three different Conventionalist bases for the formation of expectations [1937, p. 214]. And third, his dependence on psychologism is openly admitted in the above quotation.

In effect, by denying the adequacy of the macrofoundations of traditional theory Keynes was simply arguing that microeconomic theory is *false*? Presumably, it is false because it is not logically consistent with all macrophenomena – such as persistent disequilibria – and thus, by *modus tollens*, at least one of the assumptions of microtheory is false and

hence microtheory as a whole is false. If this is granted, why is there a concern for the microfoundations of macrotheory? One might argue that the reason is that many believers in the truth of traditional microeconomic theory think that by showing Keynes's macrotheory to be logically consistent with microtheory (by providing the microfoundations), the strength of Keynes' critique of microtheory would be defused by the embarrassment of an inconsistency in Keynes' position. But we do not think such an obscure reason could support all of the recent concern for microfoundations. Rather, we argue that it is the implicit recognition that Keynes accepts the neoclassical hidden agenda that has thereby led many to think that he accepts neoclassical microtheory and, in particular, general equilibrium theory. If one accepts microtheory, then it would be easy to argue that Keynes's macrotheory – namely, his theory of aggregate demand and supply – must have microfoundations.

#### *Aggregative economics and microfoundations*

In one sense the market, by textbook definition of the market functions, is an aggregation of the planned demands and supplies. That is, a minimum condition for a market equilibrium is that the *sum* of all planned quantities demanded must equal the *sum* of all planned quantities to be supplied. What if we extended the aggregation to an entire economy? This is just what was accomplished with the Hicksian grand synthesis in 'Mr. Keynes and the Classics' [1937]. We are led to believe that all we need are some big demand and supply curves in the sky which can be seen to imitate microanalysis of demand and supply. That is, what we need are curves representing a macro view of the economy. There are two ways to go in the direction leading to macroeconomics, although to be logically consistent they cannot be different. One is the direct aggregated demand and supply analysis which Keynes introduced. The other is the Hicksian IS-LM analysis. Either vision is difficult to keep in focus, since nobody can ever directly see the aggregated quantities.

Nevertheless, the basis of macroeconomics is the view that it is possible to keep the aggregated quantities in focus. But most important is the view that all of macroeconomic analysis is methodologically and perfectly analogous to microeconomic analysis. In this sense, one must be able to transfer all the microeconomic principles of market equilibria into a macro or aggregate context. Thus whenever aggregate demand exceeds aggregate supply, the price index of all goods aggregated must rise in the same way that the individual market price rises whenever the market's demand exceeds the market's supply. Of course, this analogy presumes that the microeconomic theory of individual prices is true.

#### **The Problem of Microfoundations**

In principle, *if* neoclassical microeconomic theory were successful in terms of methodological individualism, then any neoclassical macroeconomic theory must eventually be explicable in terms of the microtheory. Methodologically speaking, this means that neoclassical macroeconomic theory must not have exogenous variables which do not exist in neoclassical microeconomic theory. If it does, then the completeness of microtheory would be in doubt. This is the problem of microfoundations. In these terms, the problem of providing microfoundations for macroeconomics becomes a purely technical matter. The problem of microfoundations is to show that necessarily the logical validity of any macroeconomic theory depends only on the logical validity of microeconomic principles. A corollary of this problem is that if there are problems with macroeconomic theory, as some have claimed [e.g., Weintraub, 1977], then there must be a problem with the (general) microeconomic theory underlying it.

We are saying that if microeconomic theory is true, then the nature of the macroview or the aggregated view of the economy cannot be inconsistent with the microview. Some critics of neoclassical theory thus have an alternative route to undermining neoclassical economics. They repeatedly demand a demonstration of the foundations – which, of course, must exist if the individualistic microtheory is true. But the failure to provide microfoundations today does not mean that they are impossible to provide. The critics would be better off taking the bull by the horns and trying to prove that it is impossible to provide them in the future. If the critics also fail, then the proponents of neoclassical economics are no worse off. If a successful microeconomic theory does exist, then the only uncertainty might be about how long it might take to solve the problem of microfoundations.

The key question underlying the dispute over microfoundations is: Are there any limitations to the success of the neoclassical microtheory in terms of methodological individualism? For example, does the individual decision-maker require perfect knowledge? Similarly, do the knowledge requirements (what ever they are) presume induction? As we saw in Chapter 4, whenever induction is presumed it is possible to postpone consideration of perfect knowledge. Nevertheless, if equilibrium requires the absence of possible gains from further recontracting, then equilibrium is reached only if there are really no possible gains *and* every individual decision-maker knows that there are no further gains to be had. How does he or she know this?

Consider the possibility of disequilibrium. If there are possible gains, then it is possible for at least one individual to perceive them. This is the basis of the Rational Expectations Hypothesis. But does the absence of

possible gains assure an equilibrium? If there really are no possible gains but someone *thinks* there are, the equilibrium will be upset. On what basis will individuals actually hold the correct view that there are no gains? What forces anyone to form the correct expectations? If induction works, then individuals may be forced to hold the correct expectations – although that may require a long time. But even if everyone currently thinks, erroneously, that there are no possible gains, we have no reason to think that even one person may change his or her mind. At the very minimum, the existence of an equilibrium in prices and quantities also presumes an equilibrium in knowledge acquisition along the lines of the Rational Expectations Hypothesis.

These considerations raise two problems. First, a logically consistent individualist theory of equilibrium must presume a general equilibrium, that is, an equilibrium for *all* individuals. Second, a 'stable' equilibrium presumes stable expectations or stable functional relationships [cf. Gordon, 1955]. We will examine the first problem in the present chapter and postpone consideration of the second until the next chapter.

### General Equilibrium and Aggregation

If one has conceived of a world which is in general equilibrium as a result of free and independent choices (that is, one where the choices are consistent with methodological individualism), then, as we have said, in that world there cannot be any potential gains (e.g., total excess profits are zero for every firm) and everyone is a maximizer. Now let us consider one of the necessary features of that world. In such a world certain local properties of all production functions are the same for all firms and certain marginal properties of all utility functions are the same for all individual consumers. For example, since everyone faces the same prices (and hence the same relative prices), every individual is experiencing the same marginal rates of substitution as everyone else. But what is most important mathematically is that every production function must be locally linear-homogeneous [Baumol, 1977]. In effect, the world at the point of equilibrium is a linear world.

#### *Linear worlds*

Linear worlds have very special properties which are useful for the conception of a world of decentralized decision-makers – that is, for a truly methodological individualist world. There probably is no better representation of a linear world than the first essay in Tjalling Koopmans' *Three Essays on the State of Economic Science* [1957]. Let us further examine the nature of this world.

Based on the mathematical properties of linear functions defined on linear spaces (such as the typical coordinate system represented by quantifiable variables along the lines of typical textbook diagrams), Koopmans shows (his Proposition 1) that if any given number of independent maximizers are price-takers, then *the maximization of aggregate profit implies maximization of individual profits and the converse*. This proposition provides *sufficient* conditions for the solution to the problem of microfoundations.

#### *There's no Santa Claus?*

There is a related result which Koopmans does not pursue. If an individual or aggregate production function is linear and homogeneous (the latter condition only requires the absence of a 'Santa Claus', to use Samuelson's term – that is, the absence of any exogenous source to cover losses or eat the profits), then maximization is sufficient to provide zero excess profits. A corollary is that any non-zero profit implies non-maximization on the part of at least one individual. In other words, if linearity were exogenously given, behavioral competition is redundant? There are even more impressive implications. If there are no excess profits, then profit maximization yields an income distribution with no room for social disharmony. Every factor is paid its marginal product (which is directly implied by maximization) *and* there is nothing left over to distribute using non-economic means.

Few theorists would consider linearity to be exogenously given, so the question is: how are linearity and homogeneity provided? Well, as it turns out, both are direct consequences of the requirement of methodological individualism. Specifically, it is the result of the requirement that there be no exogenous variables constraining the variability of the chosen variables (such as the levels of production inputs), so that all choices are free. Add to this the assumption of maximization (i.e., individual 'rationality') and we can see the role of competition among individuals. Competition drives excess profits to zero, leaving all production functions with the property of linear-homogeneity. If any production function were not linear-homogeneous, then increasing the level of each input to the point where it is being paid its respective marginal product (in order to maximize profit) would yield either profits or losses. Profits and losses mean the existence of potential gains to be made, hence the equilibrium has not yet been reached or some inputs have not been recognized and thus they are not necessarily optimally utilized. Or even worse, the residual must be absorbed by some unrecognized exogenous variable (e.g., a Santa Claus) – but that explanation for non-linearity or non-homogeneity is against the methodological rules.

The situation, then, is the following. In a world of individualists guided only by their own interests, whenever they freely choose and all variables are really variable – that is, subject to free choice – all of the non-natural and non-psychological variables can be explained away because they can be shown to be matters of choice. Furthermore, no social phenomena – such as the prevailing income distribution – will be left unexplained. The question of social harmony is not often recognized. A side benefit of the assumption of linear-homogeneity is that, when combined with universal maximization, it yields the absence of excess profits and thus there is no difference between a 'labor theory of value' and a 'capital theory of value', as all prices will be proportional to the equivalent labor value of any good or its equivalent capital value. This is a direct result of the 'duality' provided by the implied linear system.

Needless to say, there is an abundance of 'ifs' in this macro view of the world. Nevertheless, one can see the methodological virtues of a linear world with respect to the individualist agenda item. For many model builders it is too tempting simply to assume a linear-homogeneous world or, what amounts to the same thing, a competitive equilibrium (viz., no exogenously fixed inputs, zero excess profits, universal maximization and thus general equilibrium). In any assumed linear world, everything adds up: the aggregates can never differ from their atomistic parts; nothing is left over to be accounted for by any forbidden exogenous means; and, most important, there is nothing *endogenous* to upset the general equilibrium.

### Macroeconomics as a Conventionalist Construct

If, given a true neoclassical microtheory, all macroeconomics variables must be explainable as 'epiphenomena' – that is, by showing that they follow from the principles of microeconomics alone – why do we even have the sub-discipline we call macroeconomics? The answer is to be found in the combination of two factors. The first is that many, following Keynes, consider neoclassical *microeconomics* to be false. Their reason may only be the claim that there are exogenous variables other than those allowed by the neoclassical methodological individualism? Or their reason may simply be that a neoclassical equilibrium world, although easy to define, is impossible to realize, hence could never be the basis of a true explanation of the state of a real economy. The second factor is more philosophical, as it is a consequence of the attempts to deal with the Problem of Conventions. Specifically, Conventionalism, which is today's primary item on the hidden agenda of neoclassical economics, does not allow theories to be considered true or false. If

claims for truth or falsity were allowed, and Keynes was correct in claiming that neoclassical theory is false, then at the very minimum his version of economics would have to supplant neoclassical microeconomics completely. But since Conventionalism does not allow theories to be considered true or false and since there are no universally accepted or absolutely true criteria, there is always a danger that economics could be destroyed by a life-or-death struggle because it is still presumed that one theory must be chosen as 'best'.

One of the complaints against Inductivism was that it fostered such life-or-death struggles and outright dogmatism over whose theory was the one and only true theory [Agassi, 1963]. Conventionalism attempts to avoid such battles from breaking out in one, or a combination, of two ways. One way is to demonstrate that competing theories are merely two different ways of looking at the same thing – that is, the two competitors are logically equivalent. This way may take a long time. The other way is to compartmentalize the discipline, giving each competitor its own department? For example, in response to Keynes' 'departure' two new categories were created – micro to accommodate those who wish to retain individualist neoclassical 'value theory', and macro for those of all sorts who wish to consider aggregate variables. However, this second way is only a temporary measure whenever competitors deal with the same phenomena. Unless they are shown to be logically equivalent, there remains the possibility that the economics profession could be destroyed by a life-or-death struggle caused by those economists who think that neoclassical microtheory is applicable to all economic phenomena and thus think that there is no need for a separate macrotheory. For these economists macroeconomics can be accommodated only if it is shown that macrotheory is built upon a foundation of microeconomic principles.

### *Accommodating the macroeconomics of Keynes*

The point here is that Conventionalism cannot tolerate disagreements over the truth or falsity of theories. The basic premise is that whenever any two individuals accept the same assumptions (i.e., microeconomics) they must agree about the conclusions reached by any logically valid argument. The Conventionalist position is that if any two individuals disagree, there must be some prior assumption which they do not both accept. Otherwise, at least one of them is crazy or 'irrational' [Pirsig, 1974]. This then provides the avenue for avoiding disagreements – we should search for assumptions which form a foundation for agreement.

With this view of the fundamentals of Conventionalist agreement in mind, let us now examine the way in which Keynesian macroeconomics has been accommodated. The following is a 'rational reconstruction' of

the accommodation [cf. Lakatos, 1971; Wong, 1978]. The accommodation is founded on the following premises. It must be agreed, first, that (to be consistent with individualism) neoclassical macroeconomics must not be more than an aggregation of microeconomics. Second, equilibrium is the primary basis of macro behavior, that is, of observable non-individualist behavior. Third, general equilibrium assures the existence of a set of fixed prices which facilitate aggregation. Fourth, the nature of any general equilibrium prices can be explained by neoclassical microeconomics using only natural and (psychologicistic) individualist exogenous variables.

Let us see the ways in which these principles allow for an accommodation. Since so much of Keynesian economics is about aggregates, the primary obstacle in the way of an accommodation is what used to be called the 'Problem of Aggregation' [e.g., Klein, 1946; Leontief, 1947; Blaug, 1978, p. 492] – the problem of constructing Keynes' aggregate demand and supply quantities from the demand and supply curves of individuals or other sub-macro entities. We can always calculate unambiguous aggregates if we assume prices are fixed. The Problem of Aggregation is about whether the fixed-price aggregate quantities correspond to the quantities that would have to hold if one viewed the aggregate economy from a general equilibrium perspective.

This problem can be solved if all production functions are linear (constant returns) or all prices are fixed at their long-run equilibrium values (where all production functions are locally linear). This is where general equilibrium comes to the rescue. It can be shown that for any given set of resource endowments (which are fully employed) it is always possible to define a set of Walrasian prices which would clear all markets [Dorfman, Samuelson and Solow, 1958] in a general equilibrium sense. The beauty of the general equilibrium sense is that the only exogenous givens are the individual utility or production functions and the naturally given resource endowments. All other variables can be calculated [cf. Boland, 1975]. Using the general equilibrium prices it is always possible to perform an aggregation, if one assumes that the economy is in competitive equilibrium (zero excess profits). The economy is in equilibrium only if all individuals are maximizing, given these prices, and the absence of profits guarantees that the aggregate value of the resources must equal the aggregate value of the outputs.

The Walrasian prices correspond to the Marshallian long-run equilibrium prices where every producer is making zero excess profits. Thus, the actual short-run prices cannot always be used for aggregation. From the macro perspective of Walrasian general equilibrium the *total* profits cannot be other than zero (no Santa Claus) but this does not preclude the possibility that the profits and losses of individual firms

cancel each other out. In the short run the actual prices cannot be used for the aggregation except when one assumes that all production functions are linear-homogeneous. As we argued above, when all production functions are linear-homogeneous, if everyone is maximizing, then everyone is making zero excess profits. If one assumes that the *aggregate* production function is linear-homogeneous (e.g., a Cobb-Douglas production function), then it might appear that, since the aggregate profits cannot be non-zero, the aggregate supply function must reflect profit maximizing outputs, just as the individual supply curves of microtheory are determined by the profit maximization of the individual producers. But it must be realized that unless all individual production functions are linear-homogeneous, the so-called Problem of Aggregation has not been solved, since the actual prices do not necessarily correspond to the Walrasian prices used to perform the aggregation.

For many economists the air around the mathematics of general equilibrium theory is much too thin and the assumption that all production functions are linear-homogeneous begs too many questions. While a general equilibrium over the relevant period of time is a sufficient condition for the fixity of prices, it is not always necessary. It is much easier merely to assume that prices are fixed over the period of time needed to calculate any aggregate quantity such as the GNP. In this sense the aggregate quantities can be calculated and thus 'observed' even if there is no way to show that they correspond to the logically consistent but unobservable Walrasian general equilibrium prices. For many this is the only viable and realistic way to accommodate Keynes' aggregate economics.

In order to be consistent with neoclassical theory, the disagreement between micro and macro theorists can always be explained away as mere pedagogical differences over whether prices are actually fixed. If the economy were in general equilibrium then as long as exogenous givens did not change over the relevant period, prices would be fixed. So neoclassical economics can tolerate Keynesian aggregate economics if the only difference is that macroeconomics presumes fixed prices [Okun, 1980]. That one or more markets may have 'sticky' (and non-equilibrium) prices can only help in the aggregation. Even when there exist one or more markets that are not cleared, as long as their prices are sticky, the fixity of prices is assured without recourse to an assumption of general equilibrium. This still begs the question as to whether the inflexibility of the prices is due to an implicit introduction of a non-individualist and non-natural exogenous variable.

Some neoclassical economists interested in explaining non-fixed price situations, such as inflation, obviously cannot accept accommodation on these terms. Instead, to the extent that macroeconomics involves

changing macrovariables and to the extent that equilibrium theory is essentially an explanation of why prices could be fixed at particular levels, it is argued that, for macroeconomics to be consistent with microeconomics, prices must change only because a temporary disequilibrium exists. And as we saw above (e.g., Solow's comments), disequilibria are attributed to 'expectational errors'. The Rational Expectations Hypothesis can then be used to explain the 'expectational errors' away. In this way macroeconomics is accommodated as epiphenomena of the microeconomic decision problems which are caused by uncertainty. Either way, the accommodation, which Clower called the 'Keynesian Counterrevolution', tolerates the Keynesian 'revolution' only if Keynesian macroeconomics is concerned with temporary short-run phenomena.

#### *The Keynesian challenge to neoclassical theory*

Critics of this accommodation argue that it is completely against the thrust of Keynes' *General Theory* [e.g., Clower, 1965]. Keynes identifies 'classical theory' with the case of 'full employment'. What is wrong with the concept of full employment? First, full employment is a presumption of the orthodox Walrasian general equilibrium analysis which only attempts to identify the sufficient conditions for the existence of an equilibrium allocation of given supplies. Second, full employment is a necessary condition of any long-run equilibrium in a competitive world of price-takers. Again, if all production functions are linear-homogeneous, then profit maximization in the long run produces 'full employment' in the sense that further employment must not yield higher utilities for anyone without lowering the utility of others.

Now Keynes claimed to be opposed to all of these aspects of full employment. But if full employment is a logical consequence of any perfectly competitive, maximizing economy in the long run, how can Keynes' opposition to theories based on full employment be reconciled with classical theory? Is it only a matter of whether Keynes was speaking about a short-run world, or is it something more? Specifically, is it only a question of Keynes' macrotheory being a special case of classical theory? Is it that the short run has some temporary exogenous variables which in time can be made endogenous and that these temporary exogenous variables are the only cause of the deviations from full employment?

Can the so-called counterrevolutionaries safely explain away Keynes' opposition to classical theory in this matter? Keynes' specific indictment, according to Clower, is that Keynes only denies that orthodox economics provides an adequate account of disequilibrium phenomena [Clower, 1965, p. 109]. But can this interpretation of Keynes' indictment be correct? All explanations are based on

specifically recognized exogenous variables. If one can show that each of Keynes' disequilibrium conclusions follows only because of the intervening *temporary* exogenous variables, their existence *is* the basis of an explanation? It would appear that both Keynes and Clower were wrong.

This is the center of the whole matter. If the classical or the counterrevolutionary explanation is based on temporary exogenous variables which are neither natural nor individualist, then Keynes would be right all along. Keynes was right because the classical or counterrevolutionary position is nothing more than standard neoclassical theory and, as we have been arguing, neoclassical explanation allows only natural or (psychologistic) individualist exogenous variables. If the counterrevolutionaries must rely on the wrong type of exogenous variables to win their case against Keynes, they simultaneously violate their own requirements for a successful theory of economic phenomena. They can only win if the temporary exogenous variables are either naturally given or are aspects of individualism, such as psychological states.

#### *Keynes' psychologism and Inductivism*

Some of Keynes' defenders, notably Joan Robinson, argue that what Keynes was saying was that the results of past decisions are necessarily exogenous for current decisions and those results are not natural nor individualistic [Robinson, 1974]. That is, the individual decision-maker often makes mistakes which cannot be undone. Being mistakes, they cannot be explained as the outcomes of maximization, hence neoclassical explanations cannot be produced to explain away the temporal and temporary exogenous variables which supposedly yielded the short-run, disequilibrium situation.

On the surface Robinson's interpretation would appear to do the logical job that she intended. And it certainly appears to be consistent with the spirit of Keynes' argument in his *QJE* article of 1937. But if we allow this interpretation of Keynes' criticism of the classics, does his theory fare any better as an explanation of so-called disequilibria? We will argue that it does not.

In his 1937 *QJE* article Keynes took the opportunity to restate his objections to classical theory in more direct terms. But, unfortunately, he exposed his hand too much. As we noted above, when referring to his theory of the consumption function he said, 'This *psychological law* was of the utmost importance in the development of my own thought...' (emphasis added). This is not an idle reference to psychological laws. Keynes was famous for his theories of *subjective* probability. And as also noted above, one of his primary arguments against classical theory was that the individual decision-maker must form subjective



expectations concerning the future and those expectations cannot be inductively proven, hence decision-makers *must* make mistakes. This view has been admirably developed by George Shackle [e.g., 1972].

We see then that Keynes accepted both the psychologism and the Inductivism upon which neoclassical theory is founded. Some of his defenders may say that this is all the better because he was able to refute neoclassical theory on its own terms. But, to criticize neoclassical economics by basing the critique on the logical consequences of accepting psychologism and Inductivism *presumes* that psychologism and Inductivism are necessary for ‘rational’ decision-making. We will argue below that neither is necessary; hence the matter of the success of the Keynesian revolution is still an open question.

# 6

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## Time and Economic Theory

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economic problems arise always and only in consequence of change.... [T]he economic problem of society is mainly one of rapid adaptation to changes in the particular circumstances of time and place....

Friedrich Hayek [1945/48, pp. 82-3]

Economics ... is concerned with decisions; decisions come in as the intermediate stage in most of its causal processes. The immediate cause of an economic effect is, nearly always, a decision by someone; or it may be the combination of decisions that were made by different people. But it is not enough, in economic analysis, to refer to the decision; we are also concerned with the reasons for the decision, the causes of the decision.

...All causative analysis ... depends on theory. If we think the decisions to be obvious, that can only mean that we are taking the theory for granted.

John Hicks [1979, pp. 88, 67]

Often in the writings of economists the words ‘dynamic’ and ‘static’ are used as nothing more than synonyms for good and bad, realistic and unrealistic, simple and complex. We damn another man’s theory by terming it static, and advertise our own by calling it dynamic....

Paul Samuelson [1947/65, p. 311]

### Time in Economics vs Economics in Time<sup>1</sup>

For many it must seem obvious that any discussion of the need to explain any disequilibrium must also entail the need to explain the dynamics of such an economy, since, by definition, disequilibrium implies changes over time. But not much progress has been made towards a development of a neoclassical theory of a dynamic economy. The reason, according to some critics of neoclassical economics, partic

ularly those who reject the ‘Counterrevolution’ (such as Robinson [1974] and Shackle [1972]), is that, unlike Keynes’ macroeconomics, neoclassical economics is ‘timeless’ or that it is not ‘*in time*’ [Hicks, 1976]. They may be correct about Keynes’ macroeconomics, but, strictly speaking, neoclassical economics is not necessarily timeless.

There is growing concern among followers of Keynes’ macroeconomics about the adequacy of any microeconomic model that is based on the hidden agenda ever to deal with decision-making in real time. What we should be asking is not whether neoclassical economics is timeless but whether its treatment of time is adequate. The same question can also be asked concerning Keynes’ treatment of time. For any treatment of time to be adequate, it is necessary for the given model to be *in time* – that is, real time must matter in some fundamental way. The critics might thus argue that an adequate ‘dynamic’ model must include at least one dynamic process. But we will eventually have to ask: can such a model ever be consistent with the hidden agenda?

### The Elements of Dynamic Models

Not much progress has been made in neoclassical theory towards an adequate approach which deals with endogenous dynamics. This is partly due to a failure to distinguish between dynamics and dynamic processes. To a great extent, Samuelson is to blame for this. He foisted a simplistic version of the physicist’s distinction between ‘statics’ and ‘dynamics’ on us. This version of the distinction is not appropriate for economics problems. According to Samuelson, ‘a dynamical system might be regarded as any set of functional equations which together with initial conditions ... determine as solutions certain unknowns in function of time’, while ‘timeless, statical systems are simply degenerate special cases in which the functional equations take on simple forms and determine as solutions functions of time which are identically constants’ [1947/65, pp. 284-5].

The major difficulty with this simplistic distinction is that it confuses ‘timeless’ with ‘static’. Whether or not a system is static is more properly a question of dynamics. Specifically, a system is static only if the given ‘initial conditions’ are constant over time. In this sense, the distinction between static and dynamic is no more informative than the assumption that the givens are constant over the relevant time period. We will adopt a distinction between static and dynamic that more accurately reflects the sense in which the critics claim that a static model is limited by comparison with a dynamic model. Our distinction involves the disposition of any model’s exogenous variables.

### *Dynamic explanation vs. explanations of dynamics*

The basis of all explanations in economics is the behavior of exogenous givens. Once one has explained all the values of endogenous variables in any given model, their values cannot change without a change in at least one exogenous given. To the extent that neoclassical models involve at least one exogenous given which is also dynamic (i.e., its value changes with the passage of time), then it can be argued that such models are dynamic explanations. There are two aspects to this observation about neoclassical models. One involves the necessity of exogenous variables; the other involves what constitutes an explanation of changes over time.

All explanations are essentially causal explanations – there is no other type of explanation [Hicks, 1979]. No one model can explain everything; there must be some givens. Every model which is not circular has at least one variable which is exogenous. The values of endogenous variables are, in this sense, ‘caused’ by the values of the exogenous variables. When there is more than one, we cannot consider the exogenous variables to be causes in the ordinary sense. That is, we cannot say, for example, the price is determined by demand, since it also depends on the supply possibilities. This has long been a source of confusion in economics but it would be easily cleared up in the case of multiple ‘causes’ by referring to them as influences.

We point all this out only because the arguments raised below are not those raised by multiple ‘causes’ but rather those raised by the logic of explaining dynamic processes. Typically, changes in endogenous variables are explained by showing that they have been caused by changes in one or more exogenous variables – this is a simple matter in the case of one exogenous variable but a little confusing in the case of more than one. Since ‘change’ usually implies the passage of time, one could go further and explain the history of the endogenous variables as being caused by the history of the exogenous variables. In either case most economists would call these dynamic explanations. What we wish to consider here is whether one can have a dynamic explanation of the dynamics of any dynamical model. We shall argue that any model involving *exogenous* dynamic processes that does not explain those processes is, at best, incomplete.

### *Time in neoclassical models*

There are a limited number of ways in which time can be incorporated into any model. The number is limited by the logical types of statement usually included in the model [Boland, 1977a]. Specifically, time can be an element in the statements which define goods and prices and the behavioral functions relating them, in the statements which identify the constraints or givens, in the statements of the conditions of ‘equi

librium', or, as we shall argue, in the statements concerning the *process* of knowing or learning the truth status of any of the above statements. We shall argue that although neoclassical models are not strictly timeless, they are still incapable of rendering explanations of dynamic processes.

For the purposes of illustrating how time has typically been included in neoclassical models, some readers might find it helpful if we consider a simple model of Walrasian general equilibrium – such as the one first presented by Abraham Wald [1936/51; Boland, 1975]. In this model the endogenous variables are the output prices ( $P$ s), resource or input prices ( $V$ s), and the quantities produced ( $X$ s). Since every model must have at least one exogenous variable, Wald specifies exogenously given amounts of available inputs ( $R$ s) and for them an exogenously fixed system of linear production coefficients ( $A$ s) and a set of exogenously given demand functions ( $D$ s). For each output he adds an equation which represents a necessary condition for a competitive equilibrium (i.e., price equals average cost).

We note that there is no explicit time in Wald's model at this stage. It is the lack of explicitness that misleads the critics who claim that neoclassical models are timeless. It is quite possible to give a temporal interpretation of every competitive equilibrium condition. We shall consider each condition to be a statement which asserts an implicit consistency between the truth of the statements about the givens (the observed values of the  $R$ s,  $D$ s and  $A$ s) and the truth of the statements about endogenous variables (the observed  $P$ s,  $V$ s and  $X$ s) at the *same point in time*. But we must concede that this is our interpretation and may not have been Wald's intention.

A minimum requirement for any model to be considered an explanation of its endogenous variables is that one can always solve for those variables as positive stable functions of the exogenous variables and parametric coefficients of the other givens. Since this is not always possible for some values of the givens, Wald provides a set of additional conditions for the givens which will assure the solvability of his model for the values of  $P$ ,  $V$  and  $X$  at the same point in time as the givens are observed.

### Traditional Models of Dynamic Processes

Models which include statements that are only assumed to be true at a specified point in time are static models by our strict definition. Although a model's logical validity is timeless, its (empirical) truth status is always an open question. Therefore, with respect to any given model, today's values of the endogenous variables may be shown to be

consistent with today's values of the exogenous variables, but tomorrow their respective values may not be consistent. Since dynamic processes obviously refer to more than one point in time, the explanatory usefulness of a static model would indeed seem rather limited.

### *The 'time-based variables' approach to dynamics*

Koopmans [1957] and Gerard Debreu [1959] offer a means of overcoming the temporal limitations of static models. Their approach (which implements Hicks' suggestions [1939/46]) is to date all variables with subscripts and build models which cover many points in time. In these models any good, say, a bottle of beer ( $B_t$ ), at time  $t = t_0$  is not the same as a bottle of beer ( $B_t$ ) at time  $t = t_1$ . Of course, in such a model we have many more goods than one could observe at any one point in time. But formally, such a model is similar to Wald's except that we have multiplied the number of goods (the  $X$ s) and equilibrium equations by the number of points in time under consideration. One must, however, be very careful in applying one of Wald's conditions for his existence proof, namely, the weak axiom of revealed preference. It is usually defined in terms of a comparison between two points ranked according to the individual's preferences. But here the comparison cannot be made between two points at different times, since the time difference itself could explain the choice between them.

This form of equilibrium model implies that the explanation of  $P$ ,  $V$  and  $X$  is essentially static for the entire period of time over which the goods are defined. There are no dynamics to be explained here because nothing is changing [cf. Smale, 1976]. The values of the endogenous variables at any point can be shown to follow from the values of the exogenous variables statically given at the unique initial point in time. The individual makes his or her only decision at that one point in time.

### *The 'economics of time' approach*

Another method of including time in a neoclassical model is to make time a 'commodity', such as leisure time or waiting time. Examples are Gary Becker's theory of allocating time [1965] and Eugene Böhm-Bawerk's period of production [1889]. In both cases, time is spent on production, and increasing the time spent implies increasing the costs. In the Becker model the costs are the opportunities lost. The amount of time allocated to produce household benefits (e.g., meals, shopping, etc.) is such that utility is maximized over all possible uses of the time endowment. Similarly, in the Böhm-Bawerk model the costs are the needed working capital, which increases with waiting time. Time is allocated to waiting until the product is considered finished. The optimum waiting time will maximize the profit rate.

The difficulty with this approach is that time is just another exogenously scarce resource which can be uniquely and optimally allocated; thus the time allocation is viewed as another *static* variable that has been uniquely determined when it is logically consistent with other static and exogenous givens. Again, nothing is changing during the period of time considered. Neither Becker's nor Böhm-Bawerk's approach can avoid the static nature of the givens (constraints, tastes, production functions, time available, etc.). As with the Wald model, the endogenous variables are statically fixed by the exogenous givens. There is no reason for historical change; hence it cannot be explained.

*The 'variable givens' or 'lagged variables' approach*

As an alternative to the above approaches one might attempt to determine the *time-path trajectory* of the endogenous variables. Given that the solution of a model represents its explanation, the only way the endogenous variables can change over time is either by one or more of the exogenous variables changing, or by some of the parameters of the logical relationships autonomously changing, or both. The population's growth rate in Kaldor's growth model [1957] is an example of the former, and what Hicks [1976] called an 'autonomous invention' or a non-neutral change in technology might be an example of the latter [cf. Boland, 1971b]. However, in neoclassical economics the relationships are usually assumed not to change over the relevant time-period [cf. Wong, 1978]. The entire explanation of historical change is usually invested in the exogenous changes of the givens. The changes in the givens may be represented by movements along their fixed trajectories. Thus if some of the static givens of Wald's model are replaced by time-path trajectories for a specified time period, the result will be derivable trajectories for the endogenous variables over the same time period. With this method of including time we have only replaced a point in time with a static sequence of corresponding points in a fixed period of time. The solution will be a fixed sequence of changing values.

Obviously one does not necessarily have to assume that the time period of the exogenous variables is the same as that of the endogenous variables. One could assert that some of today's exogenous variables may be yesterday's endogenous variables [Nerlove, 1972]. An example of this approach is the von Neumann [1937/45] balanced growth model. With this 'lagged variable' approach we are able to derive a time-path trajectory for the endogenous variables. However, the position of the trajectory over a given time period will depend only on the initial set of values for the exogenous givens. The initial values of the givens are essentially the only exogenous variables of the model over the whole time period.

On the surface, the direct approach of including an exogenous time-path for the givens, or the indirect approach using lagged variables, looks like a solution to the problem of explaining historical change. But a closer examination will show this to be an illusion. In the exogenous trajectory approach, the endogenous variables are changing *only* because the exogenous variables are changing. In the case of lagged variables, the position of an endogenous variable on its trajectory is uniquely determined merely by the length of time which has transpired since the initial givens were established. The position of the trajectory itself is uniquely determined only by the initial values of the exogenous givens. In both cases the trajectories of the endogenous variables are exogenously fixed. The only 'dynamics' of the model are exogenous. Since exogeneity of any model results from an explicit choice to not explain the givens or their behavior [Boland, 1975], we have not explained the dynamic changes within the model. In other words we still are relying on a statically given time-path trajectory which is fixed over the relevant time period. We have not explained why it is that trajectory rather than some other.

We could, for example, assume the given path was such that the exogenous variable grew at a constant rate. If we should be asked why we did not assume an increasing rate, we cannot justify our assumption solely on the grounds that it yields the observed time-path of the endogenous variables. The truth of our assumptions regarding exogenous givens must be independent of our conclusions regarding endogenous variables [Boland, 1975].

*The 'flow variables' approach*

The criticisms raised against the approaches that add time by appropriately defining certain variables can be extended to those approaches that add a time-differential equation to an otherwise static model. One of the problems in using equilibrium models to explain prices is that observed prices may not yet have reached their equilibrium values. Thus it is often argued that we need an explanation of the disequilibrium behavior of the endogenous variables [Barro and Grossman, 1971]. Typically, a theory of price adjustment is attached to neoclassical equilibrium models. The basic approach is to add a differential (or difference) equation which gives the rate of change of the price as a function of the amount by which the two sides of one of the equilibrium equations deviate from equality prior to reaching equilibrium [Samuelson, 1947/65; Arrow, 1959]. In market demand and supply analysis this usually is an equation of the following form:

$$(dp_t / dt) = f(S_t - D_t), \quad [1]$$

where  $df/d(S_t - D_t)$  is negative and  $f(0) = 0$ . But unless this additional equation is explained, the dynamics are purely improvised and arbitrary. A make-shift differential equation for the ‘dynamics’ of the market does not even say who changes the price or why it is being changed. Until we can say why the price has changed (rather than describing how much it should change), we have explained neither the process of disequilibrium change nor the dynamics of the market.

#### *Real time vs. long run*

Significant as some may consider such criticism to be [Gordon and Hynes, 1970; Boland, 1977b], matters are even worse for the determination of the equilibrium level of prices. Most models which include time-differential equations only guarantee a solution in the long run. Such models are incapable of yielding a determinant and non-arbitrary solution for the prices at points of real (calendar or clock) time where equilibrium has been reached. If by ‘in the long run’ we mean that it takes anything approaching an infinite amount of time to yield a determinant solution, we are in effect conceding that we do not have a real-time explanation of the observed behavior of the endogenous variables. To be specific, we assert the following methodological proposition:

*To assert the existence of a long-run equilibrium when its attainment requires an infinite length of time is to imply either that time does not matter or that we have no explanation.*

Obviously, the usual Conventionalist argument that true knowledge is impossible, based on what we called the inductive learning possibilities function, is also based on this methodological principle.

#### **Time, Logic and True Statements**

Going much further than we have here, recent critics claim that all neoclassical models are essentially timeless because, they say, all economic analysis has comprised merely logical derivations of solutions to abstract mathematical problems [Georgescu-Roegen, 1971; Shackle, 1972]. We shall argue that this criticism stems from a misconception about the logical nature of a model.

The logical nature of any model is determined by the extent to which the model represents an argument, that is, an explanation of its endogenous variables. There are only two basic forms of valid logical arguments. Arguments for and arguments against: arguments for

something are formally in favor of the truth of a specific statement. Such an argument consists of one or more given statements which are alleged to be true and from which one can logically derive the specific statement in question. Arguments thus have two contingent but essential parts: (1) the purported validity of logical relationships *between* all the given statements and the statement in question, and (2) the purported truth status of *each* of the given statements. Standard logic provides only the means of ‘passing’ along the truth of all the given statements to any statement which logically follows from them [Boland, 1979a]. However, the truth of any given statement must be established independently of the argument.

With all the above models we have relied on a temporal interpretation of the truth status of individual statements. Each equation of a model is alleged to be a true statement of a given relationship between the observed (or observable) true values of the included exogenous and endogenous variables. The observation of the values of the variables is presumed to be made at the same time (or, in the case of lagged variables, at specifically defined but different points). Such a time-based or static concept of a ‘true’ statement is easily accepted. Moreover, we shall argue that it is the basis for the usual applications of logic in any explanation or argument.

Applications of logical deductions in any direct argument in favor of some proposition always require that the given statements be known to be true (or at least not known to be false). The internal consistency of some non-compound (simple predicate) statements *may* assure their truth status (e.g., identities, definitions, etc.), but the consistency of a compound statement (e.g., a conjunction of two or more non-compound statements) does not generally assure its truth status [Quine, 1972, p. 10]. For example, a conjunction of three simple statements (say, ‘The price is \$100’, ‘The quantity sold is twenty’, and ‘The gross revenue is \$2000’) is true *only if all* of its parts are true. The truth of any of its parts may be time-based (thus possibly false), but the consistency of such a compound statement only requires consistency between its parts, that is, that it is not inconsistent when all of its parts are true at the same point in time.

Any model can be seen to be a compound statement [Boland, 1977a], and its general solution represents its explanation of the endogenous variables. Formally proving the solvability of an appropriate set of equations establishes the consistency of the explanation the model represents. But solvability does not establish the truth of its parts (such as the statements about the givens), because the logical consistency of the statically observed values of the endogenous and the exogenous variables is only a necessary condition for the truth of the model.

Our static concept of a statement's truth status presumes that equations (such as those representing competitive conditions) are capable of being false; hence they are not necessarily tautologies. But the static nature of the definition of a statement's truth status does not preclude the statement from being true at many points in time. Although by definition an allegedly true dynamic statement is supposed to be true at more than one point in time, it does not have to be logically true at all points in time, which means that conceivably it can be false [see Boland, 1977b]. Since static and dynamic statements can be false at some points in time, time will matter to their truth status. If any equation were meant to be a pure logical relation (e.g., a tautology), then it is assumed to be always true, that is, it is impossible to conceive of its being false. Its truth status is thus 'timeless'. Any statements that are logically true at all points in time are simply statements whose truth status is independent of time.

If one were only concerned with the known truth of a single (non-compound) statement, it would appear that a model-builder must choose between statically limited observations (i.e., descriptions) and timeless generalities (i.e., logically true statements for which time does not matter). Since neither alternative is very promising, this would seem to spell trouble for anyone trying to build dynamic neoclassical models which are true at all points in time yet in which real time matters. It is along these lines that the critics have charged that neoclassical economics is timeless. However, even though we think the critics are wrong, we are not suggesting that one must accept static descriptions in place of (possibly false) dynamic explanations.

What we suggest is that the charge of 'timeless' neoclassical models should be rejected because the critics' arguments are based on two fundamental mistakes. They confuse conceivably false (dynamic) statements which may happen to be true at all points in time with tautological statements which are true at all points in time only because they cannot conceivably be false. Also they fail to distinguish between a single statement (e.g., a model's solution) which may be a timeless logical relation, and the logical consistency of a specific joint logical relationship such as the one between the values of all the endogenous variables and the time-based truth of the statements of the values of the exogenous variables. This latter mistake has probably been the major source of misunderstanding about the alleged timelessness of neoclassical models. That a model or any explanation can be shown to be logically valid does not say that its truth status (as a compound statement) is timeless. This, we are arguing, is simply because *a model is not timeless if any of its parts is not a tautology*. All models must have at least one such non-tautological statement, namely, the statement representing the values of the exogenous variables.

### Time and Knowledge

Our previous discussion of the usual ways of including time seems to suggest that any reliance on neoclassical general equilibrium theory alone precludes an explanation of historical change. All the causes, motivations, or reasons for change are beyond explanation because they are being considered to be exogenous to the models. In other words, we always face the problem of having to choose between dynamic explanations and explanations of dynamics. Long before there was concern about the microfoundations or the Rational Expectations Hypothesis, this problem was recognized by Hayek [1937/48] and remains an essential consideration in most Austrian models [Hicks, 1973; Lachmann, 1976]. Hayek insisted that this methodological limitation of standard economic analysis only makes clear the importance of looking at the way in which individuals acquire and communicate their knowledge (of the givens). This, he argued (on the basis of an implicitly accepted Inductivism), is because the acquisition of the (true) knowledge of the givens or facts (constraints, etc.) is essential for any (stable) equilibrium.

Unfortunately, Hayek did not provide an explicit solution to the problem, although he implicitly outlined some acceptable requirements for a satisfactory solution. They were considered acceptable only because they were consistent with the hidden agenda of neoclassical economics. First, to be individualistic, he wanted the individual's knowledge (of the relevant givens) to be explicitly recognized. Secondly, to be consistent with inductivism, he claimed that the acquisition of one's knowledge must depend on objective facts, if the facts are to play an essential role in the *explanation* of the individual's behavior. For Hayek this was simply a matter of 'how experience creates knowledge' [1937/48, p. 46]. Supposedly, if one knew the individual's past experience, one could logically infer the individual's current knowledge. Given that there is no inductive logic, it is not surprising that Hayek was admittedly unable to offer an explanation for even one individual's acquisition process; thus the dilemma of having to choose between explaining dynamics and dynamic explanations remained unresolved [1937/48, p. 47].

Eliminating the dilemma would appear to be a simple matter of adding knowledge (or 'expectations'), say, to Wald's model. This approach seems to be what is now popular among avant-garde theorists, as we saw in the previous three chapters. But, we argue, if knowledge or its acquisition process is treated as another exogenous or statically given variable, then the problem of explaining dynamics remains. Similarly, no model that requires an individual to have the benefits of a correct economic theory (e.g., the Rational Expectations Hypothesis presumes

that the individual has correctly assessed the costs and benefits of collecting more information), thereby suppressing the role of the individual decision-maker's knowledge, solves the problem. Furthermore, if the individual's knowledge is suppressed only 'in the long run' we are brought back to the irrelevance of real time. To solve the problem of explaining dynamics, the individual's process of acquiring his or her knowledge must be endogenous; it must be something to be explained. In rational decision models in a dynamic context, the individual's process of learning and adapting must take place in real time.

#### *Towards an essential role for time*

In the previous chapters we observed that the reconciliation of Keynesian macrotheory has been founded on the view that since macroeconomists are most often concerned with immediate policy questions, it is reasonable to allow macrotheory to be centered on a theory of short-run disequilibria. To center macrotheory on the short run is to say that real time must matter. Furthermore, we noted, the primary means of explaining the existence of disequilibria is the recognition of 'expectational errors' which are in turn the result of dealing with real time. This is where the reconciliation rests – right where Hayek left it back in 1937.

Some progress towards incorporating real time in economics models would seem to have been made by some post-Keynesian theorists. For example, Shackle [1972] and Davidson [1972] have argued that the existence of money in an economy is a direct consequence of the importance of real time. Specifically, except in a barter economy where all transactions are direct and immediate, very many market transactions require placing an order at one point in time and acquiring the goods and sales revenue at another point in time. In many cases this involves a sales contract. A sales contract can specify the consequences of failure to deliver the goods. The penalty for failure is almost always expressed in monetary terms.

In this post-Keynesian view money makes real-time contracts possible. More important, contracts would be unnecessary without essential processes that involve the passage of time (e.g. growing corn, aging wine, etc.). But does recognizing money and contracts overcome the shortcomings of neoclassical models? If the only reason for the contracts is the exogenously given time-using processes, then we have not moved beyond the 'economics of time' approach of Becker and Böhm-Bawerk, which only makes the dynamics exogenous.

The only basis for the post-Keynesian view of the essential endogeneity of dynamics is the role of 'expectations'. Specifically, what is recognized in Shackle's view is 'uncertainty'. The fact that we cannot

know for certain that our expectations are true makes contracts (and money) an essential part of an explanation of 'rational' decision-making. It would be all too easy for a clever neoclassical theorist to argue that the recognition of uncertainty, expectations and contracts is to explain why certain contracts are better than others and thereby to bring the contracts and uncertainty into the neoclassical research program.

What is the basis for the post-Keynesian view that expectations necessitate contracts and the use of money? Unfortunately, it is our old friend the inductive learning possibilities function (from Chapter 4). On its basis one's views of the future could never be true, since proof of their truth would require an infinite amount of time. But, we argue, relying on an exogenous learning function is no different than relying on exogenous trajectories of the exogenous variables. There are no endogenous dynamics in these post-Keynesian models.

#### *Time and liquidity preference*

The most recent attempt to deal with the problem of time in economics is Hicks' book *Causality in Economics* [1979]. There are some very promising aspects of dynamic processes in his approach that warrant close examination. It is interesting that although Hicks has criticized neoclassical economics for not being *in time* [1976], in this book he does not reject the formal (timeless) 'Keynesian' models which he helped to create; he wishes only that they be put into perspective by considering three types of causal explanations which he calls 'static', 'contemporaneous', and 'sequential'. Static causality corresponds to timeless physical theories. Contemporaneous causality corresponds to Book V of Marshall's *Principles* (e.g., relative to a given time period such as the short run) and to Keynesian models of period equilibria. Sequential causality corresponds to the theory of decision-making and liquidity which was Keynes' major departure from orthodox (textbook *laissez-faire*) economics. Hicks argues that (1) formal 'Keynesian' models are appropriate only for situations of contemporaneous causality, and (2) any improvement over orthodox explanations must be seen in terms of the sequential causality of realistic decision-making.

The primary methodological thrust of his book is that the methodology and causal precepts of physics are inappropriate for economics. The methodology of physics presumes the existence of natural constants which are to be discovered or proven. There are no *natural* constants in economics. Experimental sciences presume timeless (i.e., universal) facts from which one can argue by 'induction' [Hicks, 1979, pp. 28ff.]. There are no timeless facts in economics. All data collected in economics are historical – that is, *in time*. The use of the methodology of physics in economics must presume the existence of stable constants;

hence the applicability of such methodology is limited to very short periods of time over which the ‘constants’ can be considered constant. Actually, Hicks argues, the constants of physics are limited to a finite amount of time (e.g., the life of the sun); however, the amount always can be considered to exceed the range of practical problems. But the problems of economics are in real time – the short run – and thus constancy is an open question.

Hicks’ rejection of physics methodology presents a problem for his argument that the most important improvement over Keynes would be an emphasis on sequential causality. Since the time of Hume, sequential causality has usually been associated with physics – that is, with mechanics. If an object is in a state of (stable) equilibrium, it will remain in equilibrium unless caused by an outside force to change its position to another equilibrium. In physics there is no effect (change in position) without a prior cause (an outside force). The problem is that Hume’s sequential causality must be instantaneous or constrained to a mechanical trajectory which is fixed by stable constants or coefficients. On the other hand, in economics there may be a considerable time lag between cause and effect. In economics, Hicks argues, whenever one explains the effect as a result of a *prior* cause, one must also explain why it takes so long – that is, what *causes* the delay – without the benefit of a fixed trajectory.

In the case of contemporaneous causality, where the cause and effect occur, or are *perceived* to occur, in the same period of time (e.g., a year of a production period), the lag is either irrelevant or not perceived. This is clearest in the case of the relation between stocks and flows [Hicks, 1979, ch. 5]. Stocks are perceived at the beginnings and ends of ‘accounting periods’. Flows are the accumulated effect over the period (e.g., sales). If the flows are caused by changes in the stocks, both will be perceived to have occurred contemporaneously.

Contemporaneous causality (the ‘equilibrium method’) presents no problem for two of the major elements of formal Keynesian models – namely, the consumption function (or the multiplier) and the marginal efficiency of capital. But, as Hicks argues, when it comes to the element of liquidity, contemporaneous causality fails to deal with what Keynes intended; we need to use sequential causality.

According to Hicks, the necessary existence of a lag between cause and effect explains the need and purpose of liquidity. The key to the explanation, he says, is the recognition that ‘Economics is specifically concerned with the making of decisions, and with the consequences that follow from the decisions’ [p. 5]. In this he seems to be giving the same view as Shackle. But, as Hicks says, ‘it is not enough ... to refer the effect to the decision; we are also concerned with the reason for the

decision, the causes of the decision’ [p. 88]. Hicks thus begins his theory of the relation between liquidity and sequential causality by noting that

sequential causation in economics has two steps in it: a prior step, from the objective cause to the decisions that are based on it, or influenced by it, and a posterior step, from the decisions to their (objective) effects. With respect to the decision, the prior step is one of formation, the posterior of execution. Each of these steps may take time, so the total *lag* between cause and effect consists of two parts.... In order to explain the lag ... we have to explain the prior lag *and* the posterior lag. [p. 88]

Most analyses of economic history, dynamic models, or lagged cause and effect are concerned only with the posterior lag. The reason is simple: the posterior lag is rather mechanical. The analysis of Keynes was concerned with the importance of the problems of the prior lag [e.g., Keynes, 1937]. By considering those problems Hicks attempts to explain the decision-maker’s need for liquidity.

For financial institutions, questions of liquidity may be treated as a matter of marginal adjustment and hence of contemporaneous causality. But outside the financial sphere, problems of liquidity cannot be so easily explained. For Hicks, ‘Liquidity is freedom’ [p. 94]. Marginal adjustments are made on the boundary of possibilities because there is no freedom except for the allowance of only marginal adjustments (this is what Latsis calls a ‘single exit, or straight-jacket’ view of rational decision-making [1972, p. 211]). Such adjustments are adequate whenever there are no surprises and are thus only mechanical changes. But the prior lag part of the decision’s cause and effect always involves ‘information and negotiation’ [Hicks, 1979, p. 93], neither of which can be ‘scientifically precise’ or mechanical. There are no automatic responses (decisions) whenever new information appears. Liquidity *facilitates* a fast response but it does not require it. It also facilitates a slow response, as a little liquidity in the form of excess capacity permits some delaying of crucial decisions. Thus, it would seem, Hicks’ emphasis on liquidity as a key endogenous variable opens the door to explaining the speed of adjustment that has been so elusive in the models discussed above.

We will argue in the Chapter 11 that the speed of the decision-maker’s response is a matter of explaining the *methodology* of the decision-maker. But, more important, whenever there is liquidity, the usual (causal) explanations must break down in real time, because the economy is not operating on the boundary of its production possibilities. Hence not all of the usual necessary conditions of optimization (of what Hicks calls the Economic Principle and what Marshall called the



Principle of Substitution) will be operative. Thus, explanations which assume optimization (e.g., ‘Keynesian’ models) are, at best, inadequate for reality.

Has Hicks succeeded in overcoming the shortcomings of the usual neoclassical macroeconomic models of dynamics? Not completely. His Inductivist concept of a true science surely needs to be questioned. The same is true of his misleading concept of ‘static’ explanation, which suggests a timeless world; but as we explained above, a ‘static’ model is not timeless whenever it is considered to be an explanation. Nevertheless, we should applaud his attempt to develop his ‘theory of liquidity’ [pp. 94ff.] and raise the question of the adequacy of the microfoundations to deal with the deliberate efforts of some decision-makers to avoid being put into a position of making decisions only on the margins of production possibilities. We argue that not much progress will be made in this direction as long as the decision-maker is assumed to be forced to make only marginal moves along the inductive learning possibilities function. If the reconciliation of Keynes’ macroeconomics with neoclassical microeconomics is founded on a common acceptance of the inductive learning possibilities function, then Keynes has won after all!

Our argument is straightforward. In real (calendar or clock) time, inductive learning cannot be a theory of successful decision-making but only a means of *explaining away* failures. Moreover, if neoclassical economists accept ‘expectational errors’ as the means of accommodating Keynes, the cost is an admission of the impossibility of the neoclassical research program of psychologistic individualism. Neoclassical economics can honestly survive the indictments of Keynes only by rejecting induction and psychologism.

With his little book about the methodology of macroeconomic theory Hicks is attempting to salvage something from his contribution to the foundations of ‘Keynesian’ economics (as distinguished from the economics of Keynes). He says that his interest in the methodological questions he examines grew out of his dissatisfaction with the profession’s excessive concern for *microfoundations* of macroeconomics. He specifically argues that we should first be concerned with the foundations of macroeconomics ‘without attention to “micro”’ [1979, p. viii]. His first question then is, ‘What is macro-economics for?’ Although he recognizes many different answers, the one that interests him is that macroeconomics is used for the analysis of facts. For Hicks, this puts the methodological questions of the *adequacy* of macroeconomic theory (as a basis of explanation of facts) at center stage, in the spot-light. He does not go far enough.

We argued in Chapter 5, following Keynes [1937], that not only must we examine such methodological questions, but we must also question

our views of methodology. Not only do economists hold views about their methodology, but they also attribute such views to the individual decision-maker who also must be assumed to have some methodology to deal with the available facts. Explaining how individuals deal with factual evidence should be the purview of methodology, so let us now turn to a consideration of the economists’ views of methodology.

### Footnotes to Chapter 6

1. Parts of this chapter have been drawn from [Boland 1978] and our review of Hicks [1979] in the November issue of the *Canadian Journal of Economics*.

## PART III

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*CONVENTIONALIST METHODOLOGY IN  
ECONOMICS*

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