13 Individualism vs rationality in economics

> Certainly, there is no general principle that prevents the creation of an economic theory based on other hypotheses than that of rationality. There are indeed some conditions that must be laid down for an acceptable theoretical analysis of the economy. Most centrally, it must include a theory of market interactions, corresponding to market clearing in the neoclassical general equilibrium theory. But as far as individual behavior is concerned, any coherent theory of reactions to the stimuli appropriate in an economic context (prices in the simplest case) could in principle lead to a theory of the economy. ... Not only is it possible to devise complete models of the economy on hypotheses other than rationality, but in fact virtually every practical theory of macroeconomics is partly so based.
>
> Kenneth Arrow [1986, p. s386]

One of the questions methodologists have been considering recently is whether methodology matters for economics [e.g. Caldwell 1990; Hoover 1995]. It is increasingly difficult to find evidence of or comprehend how ordinary methodology questions – for example, those concerning testability, instrumentalism, realism, tautology vs metaphysics, appraisal vs criticism, etc. – in any way constrain the decisions made by ordinary theorists. Few theorists would bother to do any flag-waving concerning their methodological decisions except when they incorrectly confuse methodology decisions with technical modeling decisions. Today, game theory is the current fad in modeling techniques that prompts a little bit of flag-waving. In the 1950s it was activity or vector analysis, followed by set theory which was heavily promoted in the 1960s; and in the 1980s there was much to do about chaos and fuzzy set theory. And so on. Even in the context of modeling techniques we see very little that resembles the issues that ordinary methodologists want to talk about.

Methodology can matter – but not in the way ordinary methodologists might think. Methodology does not matter to theorists who are explaining the behavior of individuals, but it does matter to the individuals whose behavior is being explained. And it has so mattered from the beginning of the economics discipline. In this chapter, I will discuss the fundamental question that is at the foundation of all neoclassical economics: what constitutes an acceptable explanation of autonomous individual behavior?

By design and practice, neoclassical economics is offered as proof that society can achieve a coordinated state which is the result of autonomous acts of individuals pursuing their own aims. It is also offered as an explanation of the society we see out our windows. It is not just any explanation but one which is intended to be consistent with methodological individualism, that is, with the commitment to the view that only individuals make decisions, things do not make decisions. But does it really accomplish this? Is the individualism that is at the foundation of neoclassical economics consistent with the logic of explanation that is taken for granted by economists? There are reasons to suspect that such a consistency is problematic. The reasons depend on what we mean by 'explanation' and what we intend to achieve by 'individualism'.

EXPLANATION AS APPLIED 'RATIONALITY'

Explanation in neoclassical economics is built upon one motivational assumption, the assumption that individuals seek to maximize. It is common to find economists using the term 'maximizing' interchangeably with 'rational'. As Samuelson noted many years ago, what most philosophers might call 'rationality' is a much stronger concept than what is required for the explanation of decision making. For Samuelson, 'consistency' was sufficient. While in many cases one could substitute 'consistent' for 'rational', it would be misleading when the stronger notion is intended. The stronger notion of rationality is often a confusion between the mechanics of giving an argument in favor of some proposition and the nature of the psychology of the person stating the argument. The psychology version is not what economists usually mean by 'rational' even though they sometimes refer to a failure of an argument as evidence of the 'irrationality' of the decision maker. The accusation of 'irrationality' is but a left-over artifact of the eighteenth-century rationalism which Voltaire parodies in *Candide*. The eighteenth-century rationalists would have us believe that if one were rational one would never make a mistake and thus whenever we make a mistake (e.g. state a false argument) then we must be irrational.

One does not have to take such a strong position to understand what economists mean by a rational argument. All that is intended is that whenever one states an argument – that is, specifies a set of explicit

assumptions – the argument will be rational if and only if it is logically valid. Logical validity does not require that the argument be true but only that the assumptions are logically sufficient, that is, that the conclusions reached are necessarily true whenever the assumptions are *all* true. But why the concern for 'rational' arguments? One reason for the concern is the *universality* and *uniqueness* provided by rational arguments. The promise of 'rationality' is that once the assumptions are explicitly stated, *anyone* can see that the conclusions reached are true whenever the assumptions are true. That is, if the argument is rational, everyone will reach the *same* conclusions if they start with the *same* assumptions. It is this universality of rational arguments that forms the basis of our explanation of behavior or phenomena. If the behavior or phenomena can be 'rationalized' in the form of a rational argument for which the behavior or phenomena are logical conclusions, then anyone can understand the behavior or phenomena if one accepts the truth of the assumptions.

In the nineteenth century this notion of universality was captured in the notion of maximization since both notions involve similar mechanics. If we can specify an appropriate objective function for a decision maker who is a maximizer then we can explain the choice made. This is because, if the objective function (e.g. a utility function) is properly shaped so that there is a unique optimum, then *everyone using this function while facing the same constraints will make the same choices*. Thus, again, it is the universality and uniqueness that form the basis of our mode of explanation. Every neoclassical theory is offered as an intentionally rational argument. The explicit assumptions include those which specify the shape of the objective function, the nature of the constraints and, of course, the motivational assumption of maximization. Every neoclassical theory asserts that each decision maker makes a rational choice which can be represented by a rational argument.

INDIVIDUALISM AS A RESEARCH PROGRAM

The view that neoclassical economics is firmly grounded on a research program of 'methodological individualism' is today rather commonplace. In my 1982 book I explained that methodological individualism is the view that allows *only* individuals to be the decision makers in any explanation of social phenomena. That is, methodological individualism does not allow explanations which involve non-individualist decision makers such as institutions, weather or even historical destiny. To put methodological individualism in model-building terms, all explanations require some givens – i.e. some exogenous variables. In a fundamental way, the specification of exogenous variables is probably the most informative theoretical assertion in any theoretical model. The various competing schools of economics might easily be characterized on the basis of which variables are considered exogenous. Marxian models take 'class interest' and 'rates of accumulation' as exogenous givens. Some institutional models take the evolution of social institutions as a given and use it to explain the history of economics. Many neoclassical models would instead attempt to explain 'rates of accumulation' and 'institutions', and it is conceivable that some might even try to explain 'class interest' as an outcome of rational decision making. Whatever the case, no one model can explain everything; there must be some givens. For neoclassical economics today the commitment to individualism conveniently restricts the list of acceptable givens. In a neoclassical model, only natural givens are permitted to play the role of exogenous variables if that model is to qualify as an explanation.[1]

INDIVIDUALISM AND EIGHTEENTH-CENTURY MECHANICAL RATIONALISM

So, individualism is a methodological view or doctrine about how social events and situations are to be explained. But, it is not enough to characterize neoclassical explanations as those conforming to methodological individualism. This is because not all methodological individualist explanations will be acceptable.

Since the eighteenth century, economists have participated in a social philosophy that advocates so-called rationalism. Not only must our explanation of any individual's behavior be 'rational' (of course, it is difficult to conceive of a non-rational *explanation*) but neoclassical economics is exclusively concerned with the metaphysical viewpoint that every individual decision maker is rational (at least to the extent that the individual's behavior can be explained with a rational argument). Unfortunately, rationality when coupled with individualism yields a view of decision making that is rather mechanical – that is, the individual is seen to be a machine. The problem here is that by compounding rationality with individualism we create an insurmountable dilemma between unity and diversity. On the one hand the universality of rationality undermines individualism by making all individuals mechanical and thus identical in a significant way. On the other hand, the nineteenth-century tendency, which views rationality as a psychological process, undermines any non-mechanical concept of individualism by making individuality exogenous and thus beyond explanation. These methodological problems can be illustrated with the following hypothetical situation which characterizes the problem facing any neoclassical explanation of individual behavior:

> Our closest friend has been caught robbing a bank. Demanding an explanation, we ask, 'Why did you rob the bank?' Before we allow our friend to answer, we must recall that, to be an acceptable explanation, any explanation given either by us or by our friend must be rational and conform to the requirements of methodological individualism. Individualism only precludes choices being made by things. Rationality is established by examining the logic of the situation facing our friend, the bank robber. By asking our friend for an explanation we are asking him to give a description of the logic of his situation. Specifically, we ask him to give reasons which represent (1) his motivating aims and (2) the constraints that restrict the achievement of his aims. If he can describe the logic of his situation such that we would agree that *anyone* who exactly faced that same situation (aims and constraints) would also rob the bank, then we would say that we *understand* why *he* robbed the bank. For example, he may tell us that his child needs a very expensive operation and he wants his child to have that operation but there is no legal way he could afford it before it would be too late. Robbing the bank was the only way to achieve his aim. If his description of the situation is true (i.e. there really is no other way possible), then given his aim (to save his child) it would be rational for him to rob the bank – in fact, it might be considered rational for *anyone* with that aim and those constraints.

Whether we are discussing our friend the bank robber or an individual consumer choosing to spend his or her money on tomatoes and cucumbers, the logical requirements of an explanation of individual behavior are the same. The aim of the individual consumer is supposedly the maximization of utility obtained from consuming goods purchased while facing the constraints of given prices, given purchasing power (the individual's budget or income) and a given utility function. Such utility-maximizing behavior is mechanically rational in the sense that any two individuals with the same utility function and same income facing the same prices will choose to consume the same quantities of goods. The only proviso is that each individual must aim to maximize his or her utility.

UNITY THROUGH MECHANICS AND UNIVERSALITY THROUGH UNIQUENESS

Universality and uniqueness are the hallmarks of machines. The paradigm machine is the clock. The key test is to start the clock at 12:00 and see if it always marks off the same number of minutes as a standard timepiece. If the design of our clock is correct, every clock produced will perform in exactly the same way. The last thing we want is an individualist clock! We thus understand clocks. In effect, the basis for understanding our friend the bank robber requires clock-like behavior. While it is easy to see that we would not be able to tell time with an individualist clock, it is not as obvious but nevertheless true that we would not be able to understand the behavior of an individual unless that behavior were mechanical. The methodological dilemma is thus the following: for behavior to be individualist it must be unique, but to understand that behavior it must be universal, that is, the same for all individuals.

While universality is assured by the identification of rationality with the design principles of machines, it is the identification of rationality with utility-maximizing behavior which is the late-nineteenth-century perspective that assures uniqueness in neoclassical economics. How is the unity-vs-diversity manifested in economics? The issue which determines the influence of mechanical rationality is embodied in our modeling dichotomy of endogenous and exogenous variables. In the simplest case we say the individual consumer is exogenously given the prices and income which form the constraints in the decision situation, and that the choice of how to allocate that income between goods is endogenous. Only the individual's utility function is unambiguously exogenous. While income and prices are treated as exogenously given constraints for the individual, for the economy as a whole they cannot be since ultimately we will explain prices and incomes. So whether they are endogenous or exogenous depends on the situation *we* choose to model. In neoclassical economics we set our task in accordance with methodological individualism, that is, we want to explain individual choices in order to explain how prices affect demand so that we can explain how individualist-based demand influences prices in the market. Prices must ultimately be explained because they are social phenomena, that is, phenomena not determined by any one individual.

In this sense, a single individual's choice is always easier to explain than a market's demand curve. In consumer theory we can always treat the prices and income facing the individual as exogenous variables, leaving only the consumer's choice as the endogenous variable to explain. But to explain a market's demand curve we are required to explain all consumers' choices as well as all the other market prices that these consumers face. Of course, we are required to explain the supply curve in every market in question since supply plays a role in the determination of prices, too.

METHODOLOGICAL INDIVIDUALISM AND UNITY-VS-DIVERSITY

By design, neoclassical economics still claims to explain all prices and the allocation of all fixed resources. How can one theory explain so much? The

basis for such an ambitious program of explanation is the method by which neoclassical economics accommodates both the unity and the diversity of unique individuals. The foundation stones of the neoclassical theory's accommodation are the nature of each individual's utility function and the nature of methodological individualism. Diversity is promoted by recognizing the diversity of how various individuals allocate their incomes. That is, some people will spend more of their income on, say, tomatoes than other people do. Unity is promoted by asserting that all individuals are maximizers. Since a necessary calculus condition for maximization is that the marginal utility curve be falling at the point of maximization, it is clear that all individuals must face falling marginal utility curves. By saying all people are identical are we denying individuality? No, we are not. If everyone faces a downward-sloping marginal utility curve, the absolute position of that curve (which depends on the individual's given utility function) need not be the same for all individuals. Consider equivalent amounts of tomatoes and cucumbers. Some may get more satisfaction from tomatoes; others get more from cucumbers. When comparing people, some people may have steeper marginal utility curves than others. There are two aspects of this to consider. On the one hand, individuality is preserved because, even facing the same prices and incomes, two maximizing individuals may choose different quantities if their exogenously given utility functions are different. On the other hand, universality is provided by the common marginal nature of utility functions, but only if it can be shown that all utility functions exhibit diminishing marginal utility as a matter of human nature.[2]

Surrendering to psychology to avoid the unity-vs-diversity dilemma merely raises two different dilemmas. One is a moral dilemma: if the robber's choice to rob the bank was a rational one, how can we object? This dilemma is not easy to overcome and in the end is more a question of philosophy than of psychology. The other is an intellectual dilemma: when our friend (as a bank robber or a consumer) provides an 'acceptable' explanation, one which says that anyone facing that position would choose to do the same thing, the *individuality* of the situation is revealed to be empty. If *any* individual would do the same, then there is nothing individualistic about the choice made. This intellectual dilemma is the foundation of attempts to promote psychology in the development of economic explanations of individual behavior. If from a viewpoint of psychology we allow ourselves to assume that all individuals are given different exogenous utility functions, then individuality would seem to be preserved in our explanations of rational choice. However, relying on psychology to promote individualism is a defeatist methodological stance.

It can be argued that individualism is in trouble here only because neoclassical economics misleadingly identifies the individual's aims with the individual's exogenously given utility function. When facing the same prices and the same income, any two individuals will usually choose different consumption bundles whenever they have different utility functions. As economists, our problem is to explain a wide diversity of choices made by people in the same income class. Although requiring psychological reasons for why people have different given utility functions would certainly seem to be a promising line of inquiry, it is not a *necessary* line of inquiry since one may just as easily presume that the individual's utility function is socially determined.

Any emphasis on individualism seems to force an excessive concern for diversity. Individualist economists (in contrast to sociologists) tend to overlook obvious social circumstances where diversity is more conspicuous by its absence. Specifically, individualist economists should be concerned to explain any obvious widespread conformity whenever considering consumption patterns. In most cultures, every social role is closely associated with a specific consumption pattern. For example, accountants or lawyers in similar income brackets will usually have similar consumption patterns. In any organized society, non-conforming individualism is more the exception than the rule. It is easy to see that, relative to the general population, corporate lawyers tend to dress alike, belong to the same social clubs, acquire the same ostentatious goods such as expensive automobiles, houses, etc. What is most important is the recognition that their conspicuous consumption is not an exogenous, psychologically determined phenomenon. Rather, conspicuous consumption shows how profoundly one's preference ordering is dependent on social structure. In short, one's consumption choices may be less influenced by one's personal tastes than by one's social position.

Now, while it is important to see that utility functions (or, more generally, personal aims) are matters of sociological inquiry, one must not see this as a rejection of individualism. Such is not the case. What I am arguing here is that one does not have to see deviations from narrow-minded neoclassical economics as expressions of irrationality. Nor should we see such deviations as demonstrations of a need to study the psychology of the individual decision maker. From a methodological perspective, irrationality is easily interpreted as merely an expression of the incompleteness in the description of the logic of the situation facing the individual. It can easily be argued that while a more complete description might involve psychology, invoking psychology here is not necessary. Whether an individual's utility function is completely determined by social conventions or psychologically given makes no difference with respect to whether that individual is capable of making a rational decision.

These dilemmas that follow from our historic efforts to live by methodological individualism and the hopes of eighteenth-century rationalism do not have an obvious means of resolution. In the remainder of this part of the book, I will discuss other fundamental methodological problems that I think must be recognized. Unlike the unity-vs-diversity dilemmas discussed here, the subsequent problems are widely recognized.

NOTES

1 The remainder of this chapter is a revised version of my 'Individualist economics without psychology' (Chapter 11 of *Psychological Economics*, edited by Peter Earl, 1988), and is used here with the permission of the publisher, Kluwer Academic.

2 In indifference curve analysis terms, unity is obtained by assuming all people face indifference curves that are convex to the origin and all maximizing consumers are making their choices such that at the tangency point of choice the slope of the indifference curve is the same for everyone (i.e. equal to the price ratio that is given to everyone). Diversity is obtained by saying the chosen points may be anywhere in the choice space depending on the individual's tastes – that is, the tangency point may be anywhere on the budget line and the location of the budget line differs depending on the individual's income.

14 Criticizing neoclassical equilibrium explanations

> it cannot be denied that there is something scandalous in the spectacle of so many people refining the analyses of economic states which they give no reason to suppose will ever, or have ever, come about. It probably is also dangerous.
>
> Frank Hahn [1970, pp. 1–2]

> The author specifically means to refute the idea that models in which equilibrium prices convey information are sufficiently descriptive of the world. Analyzing how economies handle information is certainly an important and uncompleted agenda, but the essay contains no model or evidence, limiting itself to rhetoric and anecdotes.
>
> I think the author needs to change his methodological stance in arguing this point. The *rules of the game* are to present a logically rigorous model or to provide empirical evidence about a model.
>
> *JPE* referee (March 1996)

In my 1981 *AER* article (Chapter 6 above) I examined various critiques of the realism of the neoclassical maximization assumption. I explained why all critiques of the realism of this assumption miss the point – among neoclassical economists, any failure of a neoclassical model will never be blamed on that assumption. But maximization by itself is not a sufficient foundation for neoclassical explanations of the economy we see outside our windows. So now the question is, what other assumptions are required in neoclassical models? There would appear to be one other fundamental assumption: specifically, the assumption of a market equilibrium. In this chapter I will critically examine two problems with this secondary assumption. First, under circumstances which depend on what we mean by the term maximization, the assumptions of an equilibrium and of universal maximization are equivalent. Second, and related, the extent to which the assumption of an equilibrium adds to the analysis depends on whether the model offers an *explanation* as to why the state of equilibrium exists.

As I explained in Chapter 13 (as well as my 1982 book), neoclassical economics is committed to methodological individualism. Methodological individualism at minimum says that only individuals make decisions. Neoclassical economists go beyond the minimum and further require that the only exogenous variables beyond acts of nature are psychologically given tastes as represented by utility functions. This narrow version of methodological individualism is called psychologistic individualism. The motivation for every decision is to maximize one's utility or profit. All prices are endogenous, unintended consequences of everyone's attempts to maximize. Specifically, a demand curve as defined is the implied relationship between price and the quantity demanded when all demanders are truly maximizing their individual utility and we define a supply curve as the similar implication of all suppliers truly maximizing their individual profits.

Given the definitions of demand and supply curves, if any market were not in equilibrium then at least one person (i.e. at least one demander or one supplier) would not be maximizing and, moreover, this would contradict universal maximization. It should thus be obvious that the assumption of universal maximization implies the existence of a state of equilibrium.

Now, if maximization implies equilibrium, how can the assumption of an equilibrium add anything to a model? To add something beyond the notion of universal maximization, reasons must be provided for why the state of equilibrium will necessarily be reached. How is this accomplished without resorting to the definitions involved in universal maximization? This problem of adjustment has been addressed by three Nobel prize winners, Ragnar Frisch, Paul Samuelson and Kenneth Arrow. Each provided conditions that must be met for a state of equilibrium to exist but no Nobel prize winner has successfully provided reasons for why the equilibrium state does exist, that is, for why the conditions are met.

Here I will contribute my argument for why the notion of equilibrium must be something other than universal maximization. Clearly, static notions of equilibrium must be avoided since they reduce equilibrium to universal maximization. For this reason, to go beyond maximization it is necessary to follow the lead of some Austrian theorists and recognize equilibrium as a *process* rather than a state of affairs. But recognizing equilibrium as a process raises essential questions of how participants in an economy become aware of an equilibrium and how they respond whenever an equilibrium is not achieved.

THE ANALYTICAL PROBLEM OF PRICE ADJUSTMENT

Let us begin with the theoretical problem that was clearly presented by Arrow almost forty years ago. Arrow said that our microeconomic theory explains an individual's behavior by presuming that the individual is a price taker while at the same time presuming that the individual faces equilibrium prices. At best, our microeconomic theory is incomplete; at worst, it is a contradiction. If we wish to provide a complete model of the behavior of all individuals who are presumed to be equilibrium-price takers, we need to explain the process by which prices are adjusted to their equilibrium values.

To appreciate the problem of adjustment discussed by Arrow and the other Nobel prize winners, consider the basic analytical model of a market equilibrium. Think of a single market of the usual variety where the demand curve is downward sloping and the supply curve is upward sloping and where all participants are price takers. If follow the lead of many current textbooks, this market will be represented by three equations, one for the demand, D, one for the supply, S, and one to assert that the market is in equilibrium. Specifically, we will have equations [1] to [3]:

$$D = f(P, R) \quad [1]$$

$$S = g(P, K) \quad [2]$$

$$D = S \quad [3]$$

Note that P is the going market price (which might not be the equilibrium price), R somehow represents the exogenous income (or wealth) distribution, and similarly K represents the exogenous allocation of capital to the producers. In each case, the equation represents, respectively, the demand and supply quantities that would maximize utility and profit for the given price, P, and the givens R and K.

Ordinarily, model builders who only want to know the equilibrium price will simply substitute equations [1] and [2] into equation [3] and solve for P given R and K. Beyond the peculiar pleasure some people get from such analytical exercises, not much is learned from the solution unless there are reasons given for why equation [3] should be true. So far, we do have reasons for why equations [1] and [2] are true – all individuals are optimizing and the two equations are merely logical consequences of such simultaneous optimization.

Traditionally, neoclassical theorists rely on some unspecified price adjustment process to correct for any discrepancy in equation [3]. By the term 'price adjustment' we usually mean how fast and in what direction the price changes. Following Frisch [1936], Samuelson [1947/65] and Arrow [1959], speed of adjustment is usually represented by a derivative, and its sign (positive or negative) represents the direction. So, as time, t, advances the price adjustment process is represented as equation [4]:

$$dP/dt = h(D - S) \quad [4]$$

where it is presumed that whenever equation [3] is true, dP/dt equals zero; and where it is also presumed that a greater difference between D and S means a faster change in P such that a positive difference means a rising price. These presumptions are represented as conditions [5] and [6]:

$$h(0) = 0 \text{ and} \quad [5]$$

$$d(h(D - S))/d(D - S) > 0 \quad [6]$$

Some neoclassical model builders might be satisfied to just assume *ad hoc* that equation [4] and conditions [5] and [6] are all true, and thereby presume to have 'closed the model', that is, to have completed the reasoning for why equation [3] is true. But it is not difficult to see that there is nothing here that tells us how long it would take for the going price, P, to equal the one price for which equation [3] is true (given equations [1] and [2]). If the condition [6] is specified such that the price never rises fast enough to cause the positive difference between D and S to become a negative difference before the equilibrium is reached, $(D - S)$ and dP/dt might both approach zero only as t approaches infinity. In other words, it may easily be that the equilibrium is never reached in *real* time (i.e. infinite time is not real time).

AD HOC CLOSURE OF THE ANALYTICAL EQUILIBRIUM MODEL

The task, as many neoclassical model builders see it, is to specify equation [4] and conditions [5] and [6] (or something that analytically serves the same purpose) such that equation [3] is true in real time. This is usually stated as a problem of explaining the 'speed of adjustment'. Note, however, that the question of the speed of price adjustment and the question of whether equation [3] is true are not the same question. Confusing them can be very misleading. But before we consider this troublesome issue, let us consider some of the ways in which the model of a market equilibrium is often thought to have been closed.

The classic means of closing the model is to assume that the market is run by an auctioneer. There are two different conceptions of the auctioneer: one is the 'scientist' and the other is the 'warden'. The scientific auctioneer does not trust the inherent stability of the market and so, before opening the market, surveys the demanders and suppliers and then calculates the price at which [3], the market clearing equation, will be true. When the market opens, the auctioneer just communicates the equilibrium price. The warden-type auctioneer communicates the current price and entertains the bids of demanders or suppliers who wish to alter the price. They wish to alter the price because they are not able to maximize their profit or utility at the current price. This type of auctioneer does not allow transactions to take place until everyone can accept the price. Here the auctioneer's job is to suspend trading until such an agreement is established. While both concepts of an auctioneer are sufficient to close the model, the warden-type auctioneer is usually assumed.

There are many criticisms of the auctioneer approach. An obvious one is that either of these conceptions is unrealistic even for markets which are truly auctions. Usually it is argued that the assumption of an auctioneer is merely *ad hoc*. That is, it is used solely to close the model (by establishing the truth of equation [3]). Contrarily, it could be claimed the assumption actually makes the model *in*complete. If the auctioneer is necessary to run the market, we might ask whether there is a market for auctioneers and who runs that market. Perhaps the auctioneer's services are provided costlessly; but that would seem to require an explanation of why the auctioneer works for nothing. We have either a missing price or a missing market; if not, then the explanation of why equation [3] is true is incomplete. If we proceed without the missing market (or price) then we are accepting a model which violates the requirements of methodological individualism. The determination of the market price depends on the exogenous functioning of the auctioneer but the auctioneer is not a natural phenomenon. The auctioneer is an unacceptable exogenous variable.

The most common alternative explanation of price adjustment is based on the theory of an imperfectly competitive firm; it is the alternative suggested by Arrow. An imperfectly competitive firm is thought to be facing a downward-sloping demand curve which refers to the demand at many prices rather than just one price. Explaining prices using such a firm begs the question of how a firm knows the entire demand curve it faces. A few economic theorists have interpreted this correctly to be a matter of learning methodology along the lines suggested by Hayek. Unfortunately, most economic theorists have viewed Arrow's problem as one of deciding what to assume when building a mathematical model of the market equilibrium.

Since Arrow's article was published, other *ad hoc* price-adjustment mechanisms have been proposed for why equation [3] can be true.[1] All sorts of additional mathematical conditions are imposed on the postulated settings and mechanisms to prove that, under those conditions, equation [3] will be true at some point in time. But, while some mathematical economists find such puzzle-solving games to be interesting, they never seem to get to the essential issue. The essential issue is that whatever setting or mechanism is proposed, it must be the result of a process of individual optimizations and not be exogenously imposed on the market.

So far, none of the other *ad hoc* adjustment mechanisms proposed are

capable of addressing the issue from a methodological individualist perspective. Why would individuals be constrained to behave as postulated? Do individuals choose to behave according to the postulated adjustment process? Why do all individuals choose to behave in the same way? How would individuals ever have enough information to make such choices?

TOWARD CLOSURE THROUGH *AD HOC* IGNORANCE

Let us return to Arrow's suggestion that there may be a way to explain the price-adjustment by considering the price-setting mechanism embodied in the traditional theory of the imperfectly competitive firm. But to see his suggestion we have to think of the firm as setting its price to generate a demand that just equals the profit-maximizing quantity it will produce at that price. Consider Figure 14.1, where the profit-maximizing output for the demand curve shown is Q; the firm will, in this case, set the price at P. This is the textbook view of the price-setting monopolist. Unfortunately, it has one major flaw if it is to be used as an explanation of price *dynamics*, in the sense of adjusting prices toward the equilibrium price. For any given

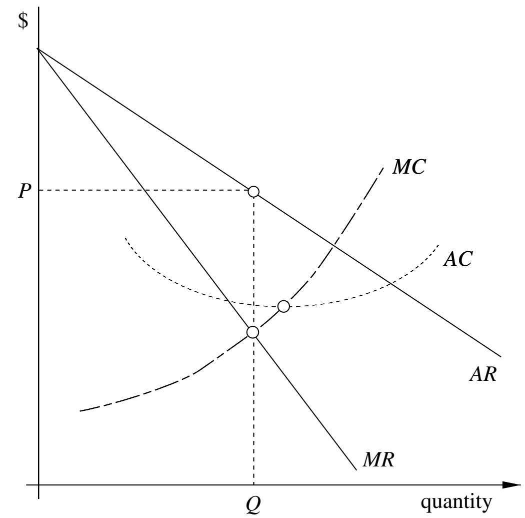

Figure 14.1 Profit-maximizing firm

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demand curve, if the firm already knows the curve, there are no dynamics. Knowing the curve, the firm will just jump to the one profit-maximizing point immediately. Here, any dynamics will be in the form of the comparative statics resulting from exogenous changes in the demand curve or cost curve, rather than in the form of the endogenous behavior of the price setter. If there are to be any endogenous adjustment dynamics, the firm must be ignorant of either the demand curve or the cost curve or both. Usually, it is the demand curve that is in doubt since the firm is unlikely to know what everyone in the market is going to demand.

How ignorant does the equilibrium firm have to be?

The question then is to specify how ignorant the firm has to be to explain the process of reaching the equilibrium as one of learning the details of the market's demand curve. There are many ways to deal with this Clower, 1959]. It could be assumed that the firm does not know its demand curve but only has a conjecture and a rule of thumb. Each time it goes to the market it tries a price and a quantity, then waits to see how much was bought. If not all the output is bought, little will be learned since the market has not cleared. If the whole output is sold at the trial price, the firm has learned one point on the demand curve although it may not be the optimum since with only one point it does not know the true elasticity of demand for its good. In effect, each trial price is a test of a conjectured elasticity of demand. Let us assume the price has been set according to the rule derived from the necessary condition for profit maximization, namely that marginal cost (*MC*<2pt space) equals marginal revenue (*MR*). By definition of *MR*, average revenue (*AR*) and demand elasticity (*e*), the equation [7] is always true:

$$MR \equiv AR[1 + (1/e)] \qquad [7]$$

When we recognize that by definition *AR* is also always the price (*P*), and we assume that profit will be maximized for a correctly estimated *e* (i.e. *MR* = *MC*), then the rule of thumb for setting the price for any given level of output will:

$$P = MC[e/(1 + e)] \qquad [8]$$

The firm is presumed to learn by trial and error to set the correct price for each level of output tried, by learning to correctly estimate the elasticity, *e*. But, unless there are very many trials, it still may be the case that not much will have been learned. Of course, if the price were instead determined in a market, whenever the expected quantity (or price) is incorrect, the price will adjust to clear the market for the quantity tried. Here each trial will yield additional information. Still, we need to be told how many trials it

will take to learn the true demand curve. Worse than this, a market-based means of providing sufficient information for the convergence of the learning process only brings us back to the question about *how* the market price is adjusted to clear the market whenever the firm's expectations are incorrect.

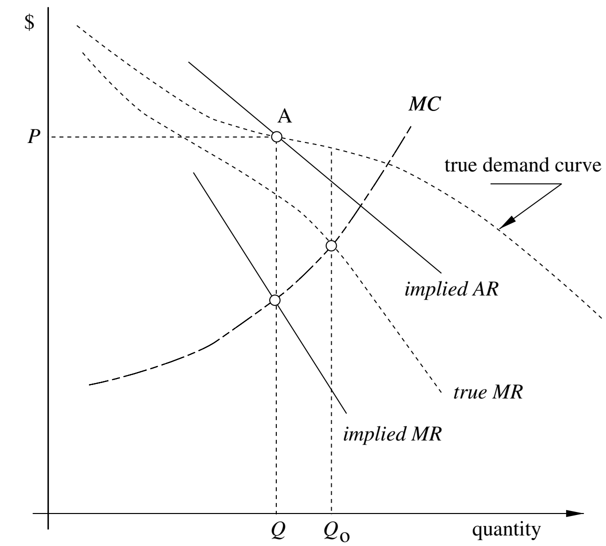

Figure 14.2 Ignorant monopolist

Clower's ignorant monopolist

Let us say the firm does learn by trial and error. Specifically, let us say that the firm forms an expectation of the elasticity of the demand curve and, on the basis of the expected elasticity and the average revenue, calculates the marginal revenue according to equation [7], and then the firm choses a supply output that will maximize profit on the assumption that the expectation is correct. But how does the producer interpret refuted expectations? Interestingly, Robert Clower [1959] presented a simple model that dealt with this question. Clower's ignorant monopolist in his simple model makes an *a priori* assumption that the demand curve faced is linear, which is contrary to the fact that the true demand curve is not linear. As a consequence of this false assumption, the monopolist mistakenly interprets each subsequent failed expectation as evidence of a shift in the linear demand curve. Assuming a stable configuration of cost and demand curves, the firm can easily reach an 'equilibrium' where the expected marginal revenue is not the true marginal revenue and hence the firm is not truly maximizing profit.

To show this, Clower uses his model to illustrate cobweb-type dynamics whereby the firm continues to assume that each failed expectation implies that a parallel shift in the demand curve has occurred since the last trip to the market. In the end, the firm's expected demand curve may converge to the state of equilibrium illustrated in Figure 14.2 as point A. In equilibrium the firm produces output Q which yields the market equilibrium price P. Since the market clears, there are no more 'shifts' in the expectations. But, since there is no reason for the firm to correctly estimate the true elasticity, the firm is likely to miscalculate the true marginal revenue. Had the firm correctly calculated the true marginal revenue for the true demand curve, it would have been producing at Q_o and be truly maximizing profit. Instead, it is in equilibrium at a non-maximizing output level. Thus, contrary to what our usual behavioral assumption would have us believe, there is no reason to think that the firm is truly maximizing when the market is in equilibrium. This puts into considerable doubt the viability of Arrow's hopeful strategy to deal with price adjustment.

EXOGENOUS CONVERGENCE WITH FORCED LEARNING

Usually, the process of learning is presumed to be inductive in situations such as this and thus take an infinity of trials to ensure convergence. That surely requires more time than is allowed before the demand curves would shift. As many see it, the real learning situation is one of estimating a demand curve that is stochastically shifting. Their reason is that we could never learn fast enough to avoid the effects of shifts. Again, this is just another expression of the implicit belief that the only learning process is an inductive one. Since this belief is not usually considered problematic in contemporary model-building exercises, let us now consider how it is employed to close the model of price adjustment.

The difficult question here is, how many observations would it take to ensure that the equilibrium price will be correctly set by the imperfectly competitive price setter? If we cannot answer this, we cannot be sure that equation [3] will ever be true. There are three ways in which this question is made to appear irrelevant. The first two are the Rational Expectations

Hypothesis and Hayek's implicit assumption that the market is stable with respect to both price-adjustment and quantity-adjustment behavior. The third way is a form of argument similar to Social Darwinism. In all three cases, the convergence process is exogenously given and it is merely left up to the individual to conform. Let us examine these tactics.

Using the Rational Expectations Hypothesis

Recall that the ubiquitous Rational Expectations Hypothesis merely assumes that the current economic theory being used to explain the economy's behavior is the one which has been inductively established as true. The presumed inductive basis for the current theory is thus exogenous to the individual's decision process. It is left to all individuals to use the information available to form expectations that are consistent with the current theory. When they are successful in forming consistent expectations, the economy will be in equilibrium. Assuming there is a reliable inductive learning method, we could see how individuals are forced to form such expectations when they use the same information that would be used to establish the current theory. Here, the force of inductive logic is being invoked, but no proponent of the Rational Expectations Hypothesis will ever be able to demonstrate that a reliable inductive logic exists.

Stacking the deck by assuming a stable market

Sixty years ago Hayek was in effect taking the same position when arguing for the superiority of the competitive market system over centralized planning. Unlike the Rational Expectations Hypothesis, his argument did not take *successful* inductive learning as an exogenous means of assuring the convergence to an equilibrium, or of assuring that equation [3] is true. Instead, he implicitly assumed that all demand curves are downward-sloping and all supply curves are upward-sloping so that the correct information is automatically provided and learned in the process of trial and error. But, as should be obvious by now, this argument merely assumes that equation [4] and conditions [5] and [6] are true as exogenous facts of nature. If individuals do learn when they are disappointed after going to the market, then they will learn the correct direction in which to respond. And, whenever an equilibrium *is* reached, it will be well defined by the presumed stable market configuration of demand and supply curves. *If* the individuals are ever going to learn the value of the equilibrium price they will be forced to learn the correct one. Unfortunately, this does not ensure convergence without perfect information and it does not explain how such knowledge would ever be acquired.

Social Darwinism applied

This brings us to the third way of forcing convergence exogenously. Almost fifty years ago Armen Alchian argued, in effect, that the process of reaching an equilibrium is a lot like Darwinian evolution – that is, 'natural selection' or the 'survival of the fittest'. In economics, the fittest are the ones who (consciously or not) have successfully solved all the problems of forming expectations and maximization in the face of uncertainties. According to this view, if the world is always limited in its resources and everything is potentially variable, we do not have to assume that each participant necessarily behaves according to the textbook with regard to profit or utility maximization, optimum learning processes, or perfect expectations. Such appropriate behavior is endogenous in the sense that it is implied by the achievement of any equilibrium of survivors. If any firm, for example, is incurring costs that exceed its revenues, it will not survive. And, since for the economy as a whole there must naturally be an equality between aggregate revenues and aggregate costs, should any one firm be making profits, some other must be making losses. If there are profits to be had, someone will find them. So if we are considering any economy consisting only of surviving firms (and households) we must be looking at an economy in long-run equilibrium, that is, one where all firms have learned enough to be making zero profits. And, as well, zero profits must be the best they can do.

The natural fact that any economy always has a finite amount of resources means that if no one is losing money then no one is gaining money. Thus, according to Alchian [1950], the need to survive forces the acquisition of adequate knowledge or learning methods. If we extend this to questions of stability, it says that Nature forces convergence regardless of how we explain the behavior of individuals. But, as clever as this tactic is, it still does not explain how long it would take. If there is a convergence here it is only because the convergence process is assumed to be exogenously given. This is the same as simply assuming that equation [3] is true, *a priori*, and thus rendering equation [4] and conditions [5] and [6] unnecessary.

ENDOGENOUS CONVERGENCE WITH AUTONOMOUS LEARNING

In each of these various forced-learning approaches to specifying the price adjustment process in mathematical models (or analytical theory), an equilibrium is always presumed to be possible. Sometimes it is even presumed to exist in advance. But the process is always either *ad hoc* or

exogenously imposed by circumstances. The point is that these usual ways of solving stability analysis problems may actually violate the requirements of methodological individualism. When building a complete model of the economy for which any equilibrium is stable but for which the stability is *endogenous*, the stability or convergence must not depend on exogenous considerations that are unacceptable for methodological individualism. In particular, whenever we successfully specify the necessary equations but the specification is *ad hoc* or exogenous, the completed model forms an explanation which is either incomplete or introduces exogenous variables that are not natural givens.

It is widely recognized that a minimum requirement for an equilibrium model is that any price adjustment process which fulfills the role of equation [4] and conditions [5] and [6] must be endogenous; that is, the process must be derivable from the maximizing behavior of individuals. This endogeneity requirement is the source of all the problems discussed in the literature concerning the disequilibrium foundations of equilibrium economics. Any shortcomings of current attempts to specify equilibrium models are almost always due to failures to recognize this requirement. To understand the endogeneity requirement we need to examine its implied procedural rules for the model builder.

The paradigm of maximizing behavior has always been the utility-maximizing individual. It is not clear whether such a paradigm can ever adequately represent all aspects of the problem of constructing an optimal price adjustment mechanism. The speed of adjustment (dP/dt), the left side of equation [4], is not a direct source of utility; that is, it is not desired for its own sake. The price-adjustment speed is merely a means to the acquisition of final goods from which the utility is derived. Few people drink wine (or beer) for its own sake but do so for its alcohol content, among other collateral attributes. The sources of the utility are the various attributes (or 'characteristics', to use Kelvin Lancaster's term [see Lancaster 1966]). Viewing the price-adjustment speed in this manner does not put it beyond the domain of choice theory. All that is required is a *representable* mechanism that shows how the price-adjustment speed affects the quantities of final goods. The specification of such a mechanism seems to be the ultimate purpose of the models built by theorists interested in stability analysis – and it is not totally unreasonable that one day such a mechanism might be constructed.

We must now ask, will any such mechanism do? Or are there some limits on what can be assumed in the process of constructing such a mechanism? Apart from satisfying the formal requirements of an optimizing model according to mathematical standards and techniques, there are really only the requirements of methodological individualism. If the mechanism is to be consistent with neoclassical theory, any alleged exogenous variable which is non-natural and non-individualist will need further explanation by acceptable means. A typical example of this requirement occurs in the explanation of the price-adjustment mechanism using monopoly theory. For a monopoly to exist – or, for that matter, anything less than perfect competition – there must be something restricting competition. Is that restriction exogenous or endogenous?

None of the well-known imperfect-competition stability models provide an explanation for *why* there is less-than-perfect competition. But, whenever any complete explanation is consistent with the psychologistic version of methodological individualism, a long-run equilibrium model of price-takers is assumed. Given that psychologism is almost always taken for granted in neoclassical economics (since the individual is always identified with his or her utility function), one wonders whether explanations of stability based on imperfect competition will ever satisfy all neoclassical model builders.

ARE THE FOUNDATIONS COMPLETE?

Assuming *ad hoc* the existence of a state of equilibrium may satisfy the tastes of economists interested only in the mathematical complexities of building models of neoclassical economics. Those of us who see neoclassical economics as an interesting collection of ideas will not be so easily satisfied. More is required. Recognition must be given to the dynamic aspect of the concept of equilibrium, and to the methodological need to assure that the attainment of a state of equilibrium is endogenous. But if the attainment of a state of equilibrium is endogenous then one cannot simply consider the existence of an equilibrium as one of the foundations of neoclassical economics.

NOTES

1 Other *ad hoc* price-adjustment mechanisms have been proposed. Two of the best known are called the 'Edgeworth Process' and the 'Hahn Process'. The Edgeworth Process simply says that a trade will take place if and only if both traders know it to be beneficial [Fisher 1983]. While this satisfies equation [5] it does not ensure that they will trade *whenever* it is beneficial. For obvious reasons, without an auctioneer there is no reason why every market participant has sufficient information to know all possible beneficial trades that might exist. The most that can be guaranteed is that *if* a trade takes place, it must be that the traders had good reason to complete the trade. Compared to the Edgeworth Process, the Hahn Process is claimed to be superior since the Hahn Process does not *require* beneficial trades to take place whenever they are possible. The participants are not required to know of all possible beneficial

trades. The Hahn Process only ensures that *after* a trade takes place all demanders or all suppliers (but not necessarily both groups) are satisfied.

The superiority of the Hahn Process is somewhat hollow in the sense that trades are assumed to take place yet how individuals decide to trade is not explained. Furthermore, the presumptions that everyone faces the same price and that the market is 'sufficiently well organized' beg more questions than are answered. To a certain extent, these presumptions are merely the auctioneer in a disguised form. Even worse, in the Hahn Process the adequacy of the speed of adjustment is just assumed, yet it is the speed of adjustment that we want explained. For a fuller discussion of these alternative mechanisms and modifications of them, see Boland 1986b, pp. 135–8.

15 On criticizing neoclassical dynamics

> What we must not abandon are Böhm-Bawerk's ... true insights – the things that are the strength of the 'Austrian' approach. Production is a process, a process in time. Though there are degenerate forms ... the characteristic form of production is a sequence, in which inputs are followed by outputs.
>
> John Hicks [1973, pp. 193–4]

> the Theory of Value in its strict form, the theory of rational conduct, must place itself in a timeless world, a world of a single moment which has neither past nor future.
>
> George Shackle [1973, p. 38]

> The lack of a comprehensible treatment of historical time, and failure to specify the rules of the game in the type of economy under discussion, make the theoretical apparatus offered in neo-neoclassical textbooks useless for the analysis of contemporary problems, both in the micro and macro spheres.
>
> Joan Robinson [1974, p. 11]

> The general equilibrium model ... abstracts from precisely those features that make the real world real – namely, the irreversibility of time and the uncertainty of the future.
>
> Paul Davidson [1981, p. 158]

In the 1970s, several notable writers charged that neoclassical economics is 'timeless' [e.g. Georgescu-Roegen 1971; Shackle 1972] or that it is not 'in time' [Hicks 1976]. This charge was considered a serious indictment of neoclassical economics by those who insisted that economic analysis of real-world problems must start from the proposition that real time matters [Dobb 1937; Robinson 1962, 1974]. However, this criticism has yet to be favorably received in the literature, not least because it is based on a narrow and somewhat misleading interpretation of neoclassical economics.[1]

Strictly speaking, neoclassical economics is not necessarily timeless. Indeed, several types of neoclassical models have treated time explicitly: as a subscript which locates goods and prices at a point in time [e.g. Arrow and Debreu 1954, Koopmans 1957, Debreu 1959], as a scarce resource [e.g. Becker 1971], and in the form of added time-differential functions or equations which define the rates of change of certain variables [e.g. Frisch 1936, Samuelson 1947/65]. The proper question to ask then is not whether neoclassical economics is timeless but whether its treatment of time is adequate. Whether it is adequate can only be determined with respect to a specific problem.

How time is treated is an important aspect of any explanation of historical change. Sixty years ago Hayek [1937] pointed out that an adequate explanation of a process of change in economics requires a recognition of the relationship between time and knowledge. He implicitly posed the following question, which for convenience I shall call the Problem of Rational Dynamics: 'How can we explain the process of change in economics and remain consistent with the principles of (individual) rational decision making?' Unfortunately, Hayek did not solve this problem although he suggested some requirements for an adequate solution.

One purpose of this chapter is to present my solution to the Problem of Rational Dynamics. My solution proposes a dynamic concept of knowledge in which learning is a real-time (irreversible) process. It is based on Hayek's recognition of the limitations of any individual decision maker's knowledge and Popper's theory of objective knowledge.

In the next section I argue that the existing neoclassical models which treat time explicitly are unsatisfactory solutions to the Problem of Rational Dynamics because time is considered to be an aspect of one or more static givens. In addition, I criticize the erroneous claim on the part of some critics of neoclassical economics that the inadequacy of this treatment of time is due to the timelessness of formal logic. In the subsequent section I humbly present my alternative solution. Finally, in the last section, I evaluate the solutions offered by Georgescu-Roegen and Shackle in which they proposed that formal logic or its use be modified.

THE PROBLEM WITH TRADITIONAL EXPLANATIONS OF DYNAMIC PROCESSES

The number of ways time can be incorporated into any model is limited by the types of statements included in the usual neoclassical model. Specifically, time can enter through the statements defining goods or prices and the behavioral functions relating them, through the statements which identify the constraints or givens, through the statements of conditions of 'equilibrium' or, as I shall eventually argue, through the statements concerning the process of knowing or learning the truth status of any of the above statements. I shall show that even though traditional models are not strictly timeless, they are still incapable of rendering explanations of dynamic processes.

Time and static models

For the purpose of illustrating how time is usually included, let us consider a simple model of Walrasian general equilibrium, specifically, the one proposed by Wald [1936/51]. In this model the explained (i.e. endogenous) variables are the output prices, resource-input prices, and quantities produced. In order to avoid vacuous circularities, every model must have at least one exogenous variable. There can always be more than one, but there must be at least one which we cannot explain within the model. Thus Wald specifies an exogenously given amount of available inputs and for them an exogenously fixed system of linear production coefficients and a set of exogenously given demand functions. For each output an equation is added which represents a necessary condition for a competitive equilibrium (price equals average cost). Wald's model is the following:

$$\begin{aligned} R &= A \cdot X \\ R &= R_o \\ X &= D(P,V) \\ P &= V \cdot A \end{aligned}$$

where X is the vector indicating the quantities of m outputs, P is the vector of their prices, R is a vector indicating the given quantities of n resource inputs, and V is the vector of the values of those inputs. Also, A is an $n \times m$ matrix of given input–output coefficients and $D(\ )$ is a vector formed of the appropriate m given demand functions for the outputs.

Note that there is no explicit time in Wald's model. Nonetheless, it is possible to give a temporal interpretation of every competitive equilibrium condition. Let us 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 Rs, Ds and As) and the truth of the statements about endogenous variables (the observed Ps, Vs and Xs) at the same point in time. But this is always a matter of interpretation.

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.[2] 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.

Models which include statements that are only assumed to be true at a specified point in time are static models by 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 seem rather limited.

Time-based variables

Koopmans [1957] and Debreu [1959] offer a means of overcoming the temporal limitation of static models by dating all variables with subscripts and building models which cover many points in time. In these models any good, say a hamburger H_t at time $t = t_0$, is not the same hamburger H_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 Xs) and equilibrium equations by the number of points in time being considered.[3] 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. 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.

Time preferences or the economics of time

Another method of including time is to make time a 'commodity', such as leisure time or waiting time. Examples are Becker's theory of time allocation [1971] and Böhm-Bawerk's period of production [1889]. In both models, time is spent on production, and increasing the time spent implies increasing the costs. In the Becker model the costs are the opportunities lost, and the amount of time is allocated to produce household benefits (e.g. meals, shopping, etc.) 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. Böhm-Bawerk's model can be illustrated in one simple diagram.

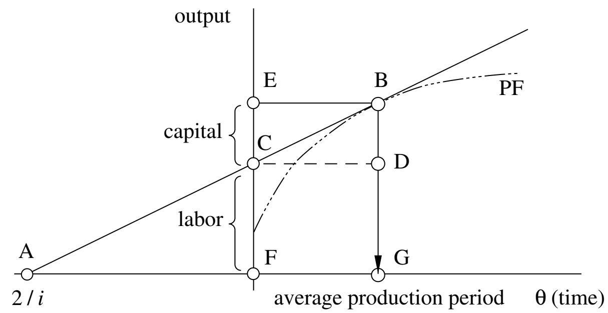

Figure 15.1 Böhm-Bawerk's capital theory

This diagram can be used in two different ways. On the one hand, in a Marshallian manner, we can take the interest rate i as the operative given (e.g. as an opportunity cost or Marshall's normal profit rate) and think of rotating the straight line about point A (representing $2/i$) until it is tangent to the exogenously given production function (PF) at point B. Doing so determines the total output, the average production period (θ), and the distribution of the output between labor and capital as shown by the location of point C. On the other hand, we can take the labor income (the classical wage-fund or working capital represented by the height of point C) as the operative given and rotate the line about this point until the same tangency is obtained on the exogenously given production function (PF). This way we are determining the output, the average production period (θ), and, at the point A where the line crosses the horizontal axis, the internal rate of return on capital (i).[4] What is important to notice here is that the only dynamics are those provided by the production function whose slope varies with the passage of time. To illustrate Böhm-Bawerk's model, the typical example would say that the product is a growing stand of trees or a maturing barrel of wine. In both cases, the dynamics are exogenously given by biological nature.

The fundamental difficulty with both Böhm-Bawerk's and Becker's approach is that time is 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 (the constraints, the tastes, the 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.

Variable 'givens' or lagged variables

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 famous 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.[5] However, usually in economics the logical relationships are assumed not to change over the relevant time period. The explanation of historical changes is entirely 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.

Of course, 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 [Nerlov 1972]. An example of this approach is the classic von Neumann 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 period of time 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 passed since the initial givens were established, and 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 results from an explicit choice not to explain the givens or their behavior, we have not explained the dynamic changes in 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.[6]

As noted by both Hicks and Georgescu-Roegen, in economics a point in time is treated logically the same way we treat a point in space. There is nothing (such as real time's irreversibility) which distinguishes time from space [Hicks 1976, p. 135; Georgescu-Roegen 1971, p. 130]. It is argued here that the dynamics in all the above approaches appear to be an illusion created by an arbitrary labelling of one or more exogenous variables. If we are going to explain any historical process with a fixed trajectory we must be able to explain that fixity as well.

Flow variables

The criticism 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 [Arrow 1959; Barro and Grossman 1971]. Typically, a theory of price adjustment is attached to our static equilibrium models. As I explained in Chapter 14, 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. In market demand and supply analysis this usually is an equation of the following form:

$$(dp_t/dt)_t = h(S_t - D_t)$$

where $dh/d(S_t - D_t)$ is negative and $h(0) = 0$. 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 nor why it is being changed.[7] 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.

As significant as some of us may consider such criticism to be [see 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 (including 'adaptive expectations' 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 we mean by 'in the long run' 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 assert the existence of a long-run equilibrium when its attainment requires an infinite length of time is simply to imply either that time does not matter or that we have no explanation.

TIME, LOGIC AND TRUE STATEMENTS

Going much further than I have here, some critics in the 1970s claimed that all neoclassical models are essentially timeless because, they said, all economic analysis has been merely logical derivations of solutions [Georgescu-Roegen 1971; Shackle 1972]. But I 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 something and arguments against. Arguments for something are formally in favor of the truth of a specific statement. Such arguments consist of one or more given statements which are alleged to be true and from which one can logically derive the specific statement in question.[8] Arguments thus have two contingent but essential parts: the purported validity of logical relationships between all the given statements and the statement in question, and the purported truth status of each of the given statements. As explained in Chapter 2, ordinary logic provides only the means of 'passing' on the truth of all the given statements to any statement which logically follows from them. However, the truth of each given statement must be established independently of the argument.

All the above models rely 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, I 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).[9] The internal consistency of some non-compound (single-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 $10, the quantity bought is 30, and the amount spent is $300') is true only if all 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, namely that it is not inconsistent when all its parts are true at the same point in time.

Any model can be seen to be a compound statement,[10] 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 that 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 with the exogenous variables is a necessary but not a sufficient condition for the truth of the model.

The static concept of a statement's truth status here 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 1977c]. Since static and dynamic statements can be false at some points in time, time will matter for their truth status. If any equation were meant to be a pure logical relation (e.g. a tautology), then it would be assumed to be always true; that is, it would be impossible to conceive 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 I think the critics are wrong, I am not suggesting that one must accept static descriptions in place of (possibly false) dynamic explanations.

What I suggest is that the charge of 'timeless' neoclassical models should be rejected because the critics' arguments are based on two fundamental mistakes. One mistake is their confusing 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. The other mistake is their failure 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 joint logical relationship between the values of all the endogenous variables with the time-based truth of the statements of the values of the exogenous variables. This latter mistake has probably been the major source of the 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, I am 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 statement, namely the statement representing the values of the exogenous variables.

TIME AND KNOWLEDGE: THE PROBLEM OF RATIONAL DYNAMICS

The previous discussion of the usual ways of including time seems to suggest that any reliance on only standard general equilibrium theory precludes an explanation of historical change. All the causes, motivations or reasons for change are beyond explanation because they are considered exogenous to the models. This problem was recognized by Hayek many years ago [1937] 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 our looking at the way individuals acquire and communicate their knowledge (of the givens). This, he argued, is because the acquisition of the (true) knowledge of the givens or facts is essential for any (stable) equilibrium.

Unfortunately Hayek did not provide an explicit solution to the problem, even though he implicitly outlined some acceptable requirements for a satisfactory solution. First, he wanted the individual's knowledge (of the relevant givens) to be explicitly recognized. Second, 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, p. 46]. Supposedly if one knows the individual's past experience one can logically infer the individual's current knowledge. However, Hayek confessed his inability to offer an explanation for even one individual's acquisition process; thus the problem of explaining change remained unsolved [1937, p. 47].

It would appear to be a simple matter of adding knowledge, say, to Wald's model and thereby solving the Problem of Rational Dynamics. But, I shall argue, if knowledge or its acquisition process is treated as another exogenous or statically given variable the problem is not solved. Similarly, any model that requires an individual to have acquired the correct economic theory (e.g. Muth's [1961] 'rational expectations' model), thereby suppressing the individual decision maker's knowledge, does not solve 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, 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.

A POSSIBLE SOLUTION TO THE PROBLEM OF RATIONAL DYNAMICS

As Hicks [1976, p. 136] observes, the general problem of explaining change in the context of rational decision making is that the decision maker's knowledge (of the givens) is hopelessly static. Although Hicks appreciates the problem, he has missed the source of the difficulty. It is not that our knowledge itself is static, but rather that the traditional views of knowledge assert that knowledge is static. I argue here that there is not necessarily a problem with rational decision making except when its logical basis presumes that the individual's knowledge (of the givens), or his or her acquisition thereof, is exogenously given.

Traditionally we are required to choose between the two views of knowledge that I discussed in Chapter 8. On the one hand there is inductivism-based theory of knowledge, which asserts that knowledge is only the facts collected up to this point in time (Popper called this the

'bucket theory of knowledge'). On the other hand there is conventionalism-based theory of knowledge, which considers knowledge to be only the latest (accepted) theory (of the facts) at this point in time. Both views make knowledge static because it is exogenously given at any point in time.

What is salient in both of these views (or theories) of knowledge is that an empirical statement or a theory is considered knowledge only to the extent that it is supported by the facts. These views differ only in regard to what is meant by 'supported' by the facts or to what constitutes 'the facts'. With inductivism, factual support is alleged to be direct and logically complete.[11] However, with conventionalism, all knowledge can be considered an accepted system of catalogues used to file or 'capture' the available facts. As a filing system, knowledge is only 'better or worse' rather than 'true or false'.

As I explained in Chapter 8, both views are based on the common belief that a theory is not true knowledge unless it can be justified (i.e. proven true). This more fundamental view of knowledge, justificationism, is false (not only because it is unjustified itself). I shall argue below that by rejecting justificationism, that is, by separating the truth status of a statement from the provability of its truth status, the Problem of Rational Dynamics can be solved.

My solution to the Problem of Rational Dynamics is constructed from the following conjunction of ideas that are borrowed from Popper, Hayek, Hicks and Shackle:

- *Anti-justificationism*: first, all knowledge is essentially theoretical hence conjectural; second, it is possibly true, although we cannot prove it true (Popper).
- *Anti-psychologism*: every individual's knowledge is potentially objective (Popper).
- *Rational decision making*: what one does at any point in time depends on one's knowledge at that time and the logic of one's situation where that knowledge is used (Hayek, Hicks).
- *Situational dynamics*: one's behavioral changes can result from changes in one's knowledge and/or from intended or unintended changes in one's situation (Hayek, Shackle).

To solve the Problem of Rational Dynamics I begin by formulating a Popper–Hayek Program for explaining any rational dynamic process. It should be pointed out that the solution requires the rejection of Hayek's (inductive) epistemology and its replacement with Popper's concept of objective knowledge. The first step is to specify one or more actors (in the past or present) who have been causing or contributing to the change in question and what have been the theories they held at the time of their actions.[12] Next, I must specify the unintended consequences of their actions, entailing conjectures as to how their theories were false. Note that the falsity of the theories may be unknown to the actors at the time; in fact it is by means of these unintended consequences that an actor may learn that his or her knowledge is false. In short, this program asserts that economics *in time* is a sequence of unintended consequences of acting on the basis of (unknowingly) false theories [cf. Hicks 1965, p. 184].

Theoretical knowledge

Before considering other solutions, let us examine the elements of this solution. Discussing the nature of knowledge is quite difficult because knowledge itself is usually given a rather lofty status. Nevertheless, it cannot be avoided. I propose to recognize a simple separation between the truth status of someone's knowledge (i.e. whether it is true or false) and the role that knowledge plays in one's decision-making process (namely to provide a sufficient and logically consistent explanation of the world one faces). Of course knowledge must be logically consistent if it is to be able to provide a true explanation of something. This is so even though the logical consistency of any explanation does not imply its truth. Nevertheless, when explaining the behavior of a decision maker it is the consistency of his or her knowledge which plays the major role in our explanation. The truth of that knowledge is much more difficult to ascertain. But, more important, the truth of that knowledge is not always necessary for a successful action on the decision maker's part. It should be noted that by separating the truth status from the role of knowledge I am not suggesting that theories or knowledge cannot be true.[13] On the contrary, I am asserting that a theory can be true even though its truth status is usually unknown to us.

By saying that one's knowledge is essentially theoretical I am emphasizing that the truth status of anyone's knowledge is always conjectural (i.e. not completely justified) and that it is potentially objective. By 'potentially' objective I mean only that it can at least be stated in words or in other repeatable forms [Popper 1972, pp. 106ff.]. It could be argued that the potential objectivity of any decision maker's knowledge makes possible a so-called operationally meaningful explanation of his or her behavior.[14]

In my view, since all knowledge is theoretical it can be put on the table for everyone to see. The view that knowledge is potentially objective stands in opposition to the more common view, which I have been calling 'psychologism'. Psychologism presumes that knowing is a psychological

process and thus one's knowledge must be private or subjective [Popper 1972, pp. 1–7]; a corollary of psychologism is that one can never explain someone else's knowledge. The proposed solution requires at least a rejection of psychologism.

What the common psychologistic view of knowledge may be saying is that one cannot guarantee a *true* explanation of someone else's knowledge. I propose this reading of psychologism to explain why anyone might think it impossible to explain someone else's knowledge. If this reading is correct, then psychologism is merely another variant of the justificationism rejected earlier. In the remainder of this chapter, when I speak of one's knowledge I shall not be referring to anyone's private views but rather to their explanations or theories of the behavior and nature of the world around them.

The role of knowledge

Hayek and others have recognized that the individual decision maker must have knowledge of the 'givens' if the givens are to play an active role in the decision process. If this view is correct the individual's knowledge must also play an active role in any explanation of his or her behavior. This prescription is not novel. Since late in the nineteenth century most social scientists have adopted a methodology in which the actor is presumed to be 'rational' concerning his or her given situation. (Inductivists would even have us extend the 'rationality' to one's method of acquiring knowledge of the givens as well.) This is evident in much late-nineteenth-century sociology (e.g. Max Weber's), which often presumes a fixed frame of reference, an 'ideal type' whose behavior is based on perfect knowledge. The 'rational expectations' model is a modern legacy of this methodology. In the old methodology the behavior of an actual individual is explained by noting to what extent or why his or her behavior is not ideal or perfectly rational.

In ideal-type methodology, one source of an individual's deviance from the ideal stems from the so-called imperfections in his or her knowledge of the givens. The imperfections of one's knowledge might result from the fact that in real time an inductively rational acquisition of knowledge is always inadequate. With regard to explaining rational dynamic processes we may wish to give the imperfections a systematic and prominent role, but this is possible only to the extent that knowledge itself plays a role. Perhaps the only complaint one might have regarding the ideal-type methodology is that it actually neutralizes the role of the actor's acquisition process by presuming that there is some ('scientific') method of acquisition which will always give one the true knowledge of the givens. Such a method is essential for the definition of the ideal type. If such a method is presumed to apply, any deviance from the ideal can only result from the actor's 'irrationality'. Except for a few apriorists such as Ludwig von Mises, using the ideal-type methodology usually implies a reliance on inductive logic to provide the rational method of acquisition. With the prior rejection of inductivism we thus have rejected any reliance on ideal-type methodology with regard to the knowledge of the individual decision maker.

Here I argue that the question of the 'truth status' of an actor's knowledge (i.e. whether it is actually true or actually false) is a separate question from why the actor thinks or believes his or her knowledge is true. In particular, the truth status of any actor's knowledge is usually independent of the method of its acquisition. An actor's theory of something can be true regardless of how he or she came to hold that theory or why he or she thinks it is true. The actor could have invented the theory to explain numerous observations, or he or she could have dreamt it. Either method of acquisition may succeed or fail. In my view this separation of status and method is important because the truth status of the actor's knowledge and the method of acquisition play different roles in any ongoing decision process.

Hayek's use of the word 'acquisition' was consistent with an inductivism-based theory of learning, that is, where learning involves collecting facts (e.g. observing 'white swans') and then inductively leaping to the conclusion that some general proposition about them is true (e.g. 'that all swans are white'). Such general propositions or theories are said to have been 'acquired'. I do not wish to limit the concepts of learning or acquiring to exercises in inductive logic, since, as argued many times above, such learning requires an unreal (infinite) amount of time. The actual (real-time) discovery of refuting evidence that show's one's current theory to be false is also a form of learning. This form of learning (i.e. by having one's knowledge refuted) will be most important in our program for explaining dynamic processes. I shall argue that the status of an actor's knowledge may give a reason for change, but it does not tell us what the change will be. However, knowing the actor's learning 'methodology' may give a clue to what change he or she may attempt to effect.

To illustrate, let us consider an example from orthodox microeconomics. We traditionally say that consumers know their preferences and their givens (i.e. each individual knows what his or her budget will be as well as what the 'given' prices will be). We explain their behavior by, first, assuming that their preferences are convex, transitive, etc. and that prices are given, and, second, assuming that the consumers buy their 'best' bundle according to their preferences. Now, Hayek argued that consumers in a competitive market economy cannot always 'know' *a priori* what prices (or

availability) will be, or even what their individual incomes will be the next time they go to the market. In terms of the proposed epistemology each consumer has a theory of what his or her income and the prices will be, although that theory may not be provable with the facts known at any point in time prior to going to the market. Nevertheless, we (and the consumers) could, on the basis of their theories, logically predict what they will buy. Their individual theories might be inferred from past experience or deduced from knowledge of some prior institutional controls, or from the pronouncements of the local authorities, etc.

Even recognizing that our predictions might be wrong, this illustration has not gone far enough for my purposes. In addition to having a theory about what the individual consumer's price–income situation is, the consumer may also have only a theory of what his or her preferences are. Specifically, unless the consumers have tried all conceivable 'bundles' they cannot 'know' from experience what their individual preferences are or will be (even if their preferences do not change over time). The consumers may believe the orthodox demand theories, thus assume their preferences are 'convex', 'transitive', etc., and thereby rationally choose their optimum bundle for their expected price–income situation.[15] So long as consumers are able to buy what they think is their best bundle, there will be no reason for them to change to any other bundle. Consumers would have to be willing to test their theory of their individual preferences before we could expect them rationally to try another bundle (one which on the basis of their current knowledge they think would be non-optimal).

If our orthodox theory of consumer behavior is true, then the consumers will find that they are not made better off with their individual 'test bundle' and may return to the predicted optimum. If our theory is not true, the consumer may find that he or she is made better off by the test bundle (hence the consumer's prior knowledge about his or her preferences will have been revealed to be false). Or, the consumer still may not be better off by that particular test bundle.[16]

Consider an alternative situation. It is quite possible for the consumer's preferences to be concave somewhere, yet (for some unknown reason) he or she has picked the best bundle. Most important, if the consumer's theory of his or her preferences turns out upon testing to be wrong and if the consumer's preferences do play a significant role in his or her decision making, the consumer will at some point be led to change his or her behavior. Depending on the consumer's view concerning facts and knowledge, he or she may change immediately by buying the better test bundle if he or she has found one, or change at some future point when facing a new price–income situation. Unless we can say something about the consumer's methodology, logically anything can happen.

In general, if one's theory of the world plays a decisive role but is false, accepting it as true must eventually lead to errors in real time.[17] How one responds to such errors depends on one's view of knowledge and how it is acquired.

Responses to the need for change

The consideration of the role of knowledge suggests two possible reasons for change. First, an actor may change his or her behavior because exogenous changes in the givens can cause the actor's knowledge to be 'out of date', i.e. false. A typical example of this type of response is a movement along the demand curve. When the consumers learn that the price has gone up they adjust to the new price by buying less.

Second, an actor's mistakes which result from acting on the basis of false knowledge (even when the givens have not changed) will directly and endogenously cause changes in the future givens. For example, consider now an imperfectly competitive firm that must decide on the quantity to supply and its price given its current financial situation. Let us say that in making its decision it estimates the demand curve incorrectly. Having supplied the wrong quantity it soon discovers that it put the wrong price on its product – its actual sales do not correspond to the level it expected. This leads to unintended changes in its financial situation. The new givens will affect its future decisions even if it never learns anything about how to estimate the future demand curves.

This example is not designed to suggest that an actor's situation changes only as a result of unintended consequences. It is quite conceivable that an actor might change his or her situation deliberately. The firm (i.e. its owners or managers) may decide to invest in new machines in order to reduce production costs or change the nature of its product. Such changes in the givens would be *intended* consequences. As long as the givens have changed (intended or not) the future behavior will usually change. New givens require new knowledge of the givens. Since there is no foolproof method of acquiring new knowledge, one's new knowledge is very often false. False new knowledge yields new errors and new unintended consequences.

The evidence of errors or mistakes could be considered a criticism of the 'realism' of one's assumptions and would thereby seem to bring about a change in one's theory of the world. However, this depends crucially on one's methodology and view of knowledge. Using a conventionalism-based theory of knowledge one might find it possible to deflect such empirical criticism by some form of approximationism (see Chapter 3 above). For example, one might say that the evidence of a counterexample (an error) is

not really contrary to one's theory of the world, because that theory is probabilistic and thus allows a few counterexamples provided they are not 'too numerous'.[18] Or one might say that only when the errors continue to happen will one be pushed to consider changing one's view of the world (one must not 'jump to conclusions').[19] Thus a conventionalist may be slow to react to unintended consequences. On the other hand an instrumentalist who knowingly accepts false assumptions may never change.

Alternatively, someone with a 'scepticist' theory of knowledge may always be looking for indications that his or her knowledge is false and always be ready to modify it. His or her behavior, unlike that of a typical conventionalist, will appear very erratic and will certainly be more difficult to predict. More might be said about this; for our purposes it is enough merely to conjecture that the way one responds in real time to unintended consequences or counterexamples to one's assumptions reveals a great deal about one's theory of knowledge [see Boland 1992a, Chapter 6].

I do not wish to suggest that these epistemological considerations can only be applied to micro-theory. Macro-theory is an even more important area of concern. Government policies today are based on the assumption that specific macro-theories are true, and estimates are made of parameters of models of these theories, predictions are made, and so on. What would happen if their theories were false? How do governments respond to counterexamples? Although the fact is not always recognized, most macro-theories are based on the assumption that neoclassical economics is true. What if it is false?[20]

ALTERNATIVE SOLUTIONS TO THE PROBLEM OF RATIONAL DYNAMICS

What one considers a satisfactory solution to the Problem of Rational Dynamics depends to a large extent on how one views the problem. In the following subsection I present and criticize Georgescu-Roegen's view of what I have called the Problem of Rational Dynamics and his proposed solution. It is argued that his concept of the problem is wrong and his misconception leads to an inadequate solution. The second subsection is devoted to Shackle's attempt to deal explicitly with the Problem of Rational Dynamics. I shall explain why Shackle's solution fails even though his viewpoint is largely consistent with Hayek's.

Georgescu-Roegen's solution

In his 1971 book, *The Entropy Law and the Economic Process*, Georgescu-Roegen agreed implicitly with Hayek that orthodox economic analysis cannot by itself deal with the process of change. Instead of attributing this weakness to the lack of epistemological considerations, he blamed theorists' reliance on standard logic.[21] He argued that logic is timeless and thus that economic theory constructed on the principles of logic alone is incapable of explaining economic change. The problem, as he saw it, is that economists imitate physicists and thus cannot deal with qualitative change:

> The undeniably difficult problem of describing qualitative change stems from one root: qualitative change eludes arithmomorphic schematization. The leitmotiv of Hegel's philosophy ... is apt to be unpalatable to a mind seasoned by mechanistic philosophy. Yet the fact remains that Change is the fountainhead of all dialectical concepts. [1971, pp. 62–3]

His solution to the Problem of Rational Dynamics is to modify logic with what he called 'dialectical concepts'. These are concepts which may violate the so-called canon of non-contradiction (i.e. the axiom of standard logic which prohibits any statement used in a logical argument from being both true and false). Examples of such concepts are 'good', 'justice', 'likelihood', 'want', etc., which have no clear-cut boundaries of definition and 'are surrounded by a penumbra within which they overlap with their opposites' [1971, p. 45].

This approach was offered because 'change' can be interpreted in such a way that an object can both be in one place at a point in time and be moving at that time, which implies a contradiction of sorts.[22] In order to understand this approach, let us think of a photograph made with an open shutter in a darkroom using only a strobe light (a precise flashing light) illuminating a moving object (e.g. a dancer). Unless the observer recognized the photograph as a multiple exposure it might be interpreted as showing that a still object was at two different places at the same time. Such interpretations are to be allowed by Georgescu-Roegen's dialectical concepts even though they are apparent contradictions. These interpretations, if allowed, permit us to 'see' the state of economy as one image on an intuitive continuum of such images. In fact, he said, 'Change itself is inconceivable without this continuum' [1971, p. 67].

It seems to me that there may be something intellectually dishonest about allowing such 'dialectical concepts'. If my interpretation of them as strobe-light pictures is correct, his dialectical concepts are considered to be contradictions only because we do not have enough information (in the above example, that the camera lens was open for several seconds while pointed at a fixed background and that each position of the dancer represents a different point in time). This insufficiency of information is due not to an imperfection of 'our thoughts', as he suggests, but to his conception of our thought process or the admitted fact that he has not conceived of a

way to solve the so-called problem of induction – 'How do we know we learn from experience?'.

Georgescu-Roegen's allowance of dialectical concepts is nothing more than an admission of defeat. Since we cannot prove (using standard logic) that our knowledge of change is true – even when it is true – he abandoned any pursuit of truth by allowing truth and falsity to co-exist. This acceptance of contradictions can be attributed to his desire to maintain a belief in induction as the sole basis of 'knowing' whenever knowledge is about observed facts.[23]

Shackle's solution

Shackle's 1972 book, *Epistemics and Economics*, contains his attempt to solve the Problem of Rational Dynamics. His view of the problem is similar to Georgescu-Roegen's in that he too blamed standard logic for the alleged inability to explain change. His solution also is not unlike Georgescu-Roegen's, for he wished to modify our use of logic. Shackle advocated what he called 'Keynesian Kaleido-statics'. It is a methodology based on what might be called a 'reasonably incomplete justification of equilibrium'.[24] His view of dynamics is that today's situation can only be understood as one of many possible equilibria. One should not expect to be able to explain why it is the observed one rather than some other possible equilibrium position.

Following Hayek, Shackle argued that a true equilibrium requires that everyone's buying or selling plans are always fulfilled. But, according to Shackle, when one goes to the market one's preplanned rational decisions are impossible to justify or explain completely. Of course, this is because induction is insufficient, he said: 'technology and the practical wisdom of everyday living ... rests on inductive inference, no matter how lacking that may be in logical justification. We rely on it because there is no substitute' [1972, p. 407].

Being unable to completely justify an equilibrium means that an equilibrium need not be unique. His 'kaleidic' method of explanation was thus offered as an alternative to complete explanations of economic events. Specifically, his method 'presented us with descriptions of equilibrium positions for the economic society as a whole, which differ from those of the value-construct in not being optima, but merely positions which do not contain within their structure an immediate source of movement' [1972, p. 437]. This method is not a satisfactory solution to the Problem of Rational Dynamics. It is only an optimistic resignation to defeat. It would be better to give up the presumption that induction is necessary than merely accept the artificiality of Shackle's version of inductivism.

CONCLUDING LESSONS

The following is my program for explaining rational decision making in such a way that real (irreversible) time matters. I accept Hayek's view that all rational decision making must depend on knowledge of the givens and any explanation of rational decision making must include assumptions about how knowledge is acquired. I argued that this depends on the decision maker's theory of knowledge. Thus, in any explanation of an actor's behavior we must specify the actor's view of the nature of knowledge and how it is acquired. Traditional views of knowledge are unsatisfactory. Any static concept of the actor's knowledge or its acquisition – that is, a concept for which real time does not matter – renders Hayek's view incapable of explaining historical change. Furthermore, although it is well known that all models require at least one exogenous variable, any view which considers knowledge or its acquisition to be exogenous will not permit an explanation of the endogenous dynamics of a rational decision process.

My solution to the Problem of Rational Dynamics is based on a dynamic concept of knowledge where its acquisition is endogenous. In particular, the *process* of acquisition depends on the specific *view of knowledge* held by the actor. Primarily, all decisions are seen to be potentially part of the learning process. Learning, by definition, is irreversible; hence it is always a real-time process. The decision maker can learn with every decision made. What the decision maker may learn at least is that his or her theory of the givens is false. How the decision maker responds depends on his or her theory of knowledge. Thus an essential ingredient of the solution presented here is the requirement of an explicit conjecture concerning the actor's objective theory of knowledge. Moreover, this solution specifically recognizes that even when facing the same facts (i.e. the same experience) two rational decision makers who differ only with respect to their theories of knowledge will generally have different patterns of behavior over time, patterns that may not be equally predictable.

Rational decision making does not require proven true knowledge. It only requires the explicit assumption on the part of the decision maker that his or her knowledge is true. Actions based on knowledge that is actually (but unknowingly) false will eventually yield errors or other unintended consequences. These consequences are not evidence of the actor's 'irrationality'; rather, they are evidence that some of the actor's knowledge is false.

The view that one is irrational if one's knowledge is false presumes that there exists a rational mechanical process which yields guaranteed true knowledge. Unfortunately such a process does not exist, so that the charge of 'irrationality' is misleading. Yet the actor's knowledge does play an

essential role in his or her decision process. Not only is it not logically possible to assure that one's knowledge is true, it may actually be false. Thus I proposed an explicit separation of the truth status of knowledge from the role that knowledge plays in the decision process. Primarily, this permits us to separate the static nature of the truth of knowledge from the dynamic nature of the learning process. However, this separation alone is not enough to solve the Problem of Rational Dynamics. One must also assume that the learning process is not one of the exogenous givens of the explanatory model.

With traditional equilibrium dynamic models, explanations of changes rely on exogenous changes in the givens for the rational decision maker. Every decision maker is expected to respond to the new givens, and the new equilibrium is reached at the point where everyone's behavior is consistent with the new givens. Thus there are two types of observable change: long-run moving equilibria and short-run movements toward a new equilibrium. In models where real time does not matter these two types of change are indistinguishable by simple observation. By definition, an unambiguous short-run change is identifiable only where there have been no changes in the givens. Long-run equilibrium change occurs only because the givens have changed. Once an equilibrium has been reached, no changes should occur without exogenous changes in the givens.

Recognizing that knowledge can be false yields another source of change. Any current equilibrium may not be compatible with existing knowledge. Any definition of long-run equilibrium which requires that existing knowledge be compatible with the given equilibrium is in effect presuming that there exists a solution to the problem of induction. Since there is no solution to this problem, knowledge incompatibility is always possible. Depending on the actors' learning methodology, at least one of the givens (viz their theories of the givens) may change. Such a change, which can be explained in terms of the actors' theories of knowledge, leads to a new disequilibrium. If the actors learn with each decision their knowledge may always be changing. They will therefore always be in a state of disequilibrium. However, this state can be completely explained if we provide an explanation of how the actors respond to knowledge incompatibility.

The evidence that one's knowledge is incompatible with the equilibrium values of the givens and the variables is one's unfulfilled expectations. Unfulfilled expectations are interpreted as unintended consequences. This means that in equilibrium models unintended consequences are the motivating reasons for *endogenous* change. Thus, if we are going to explain change we must focus on the sources of unintended consequences, namely on the actor's false theories and his or her methodology, which together play a primary role in all learning and thus all dynamic processes.

NOTES

1 An earlier, shorter version of this chapter was published as Boland 1978.

2 Only if there is just one exogenous variable in a solution for an endogenous variable can one say this is a 'causal explanation'.

3 However, one must 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 considered by the preferences. But here the comparison cannot be made between two points at different times, since the time difference would explain the choice between them.

4 The unusal value, $2/i$, needs to be explained. It results from the actual interest cost for using the working capital (the cost of the labor) which has to be paid (over the production period θ) as represented by the area of the triangle BCD. The height of the triangle is obtained by considering the total output (segment BG) to be equal to the working capital multiplied by $(1 + i)$. That is, the total output in Figure 15.1 is said to be equal to $(1 + i)\cdot\text{DG}$, where BG = BD + DG. Thus for the span of time equal to θ, the height of the triangle (BD) equals $i\cdot\text{DG}$ and thus the area of the triangle is $(\theta\cdot\text{DG})\cdot(i/2)$. The $(i/2)$ represents the vertical increase in interest costs for each unit of working capital over one unit of time. And, as a matter of analytical geometry, the horizontal equivalent is simply the inverse, that is, $(2/i)$, which tells us the length of time needed to accumulate one interest point over the value of the working capital (DG).

5 I have offered my theory of how to deal with technical change in my 1992 book, Chapter 7.

6 We could, for example, assume that the given path was such that the exogenous variable grew at a constant rate. If we are 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 1989, Chapter 6].

7 Another version of this approach is to make the rate of change of price a function of the difference between intended and unintended inventory levels. But this approach leads to instability in some markets [cf. Hicks 1956, p. 148]. Moreover, it does not explain the price behavior. Although one can interpret price-makers' behavior as some sort of learning process where the rational maximizing decisions are statically behind the facts [cf. Gordon and Hynes 1970, p. 377], one still needs a long-run argument to guarantee stability.

8 The basic logical tool of all arguments in favor of something is the property *modus ponens* that I discussed in Chapter 2.

9 Of course, this does not refer to an indirect argument which begins by asserting that the desired proposition is false in order to show that such an assertion implies a contradiction in the argument. If one accepts the Axiom of the Excluded Middle and denies all contradictions, then to say the desired proposition is not false implies that it is true.

10 For a discussion of the methodological elements of model building see my 1989 book, Chapter 7.

11 Obviously, for induction to work, the 'facts', or singular objective observation statements, must be unambiguously true. As discussed in Chapter 9, it is for this reason that we traditionally distinguish between 'normative' and 'positive' statements. Since normative statements, by definition, are not objectively true, they cannot be used as givens in a 'logical' argument. According to positivism, we are supposed to believe that positive statements can be objective and provably true. If we give up inductivism we can also give up this classic distinction.

12 This program falls under the Lakatos-Popperian rubric of 'Rational reconstruction', the rules of which are discussed in Wong 1978, Chapter 2.

13 Certainly I am not saying 'all theories are false', since that is a self-contradiction (if it is true, then not all theories are false).

14 In effect, this was the argument for Samuelson's revealed preference analysis; see Wong 1978, Chapter 5.

15 Each consumer could instead follow Samuelson's axioms of consistent choice [1938] and merely try to act 'consistent' with his or her past choices. But the consumer would still need to know all past choices. If consumers instead followed the later Samuelson [1950] they could infer their preferences from actual choices, but that would presume the existence of a logic of induction! See Wong 1978, Chapter 6.

16 However, if a consumer were to try all possible bundles – an impossible task – and did not find a better bundle, then he or she would verify our theory. This, of course, is the same situation faced in the 'integrability problem' [Samuelson 1950], which presumes that the utility function both exists and can be inductively inferred. It is quite beside the point, because it is an impossible task [see Wong 1978, Chapter 4].

17 There is another important way in which this theory of consumer behavior might be in error besides incorrectly specifying such things as the shape of the consumer's map or how he or she predicts his or her price–income situation. The individual consumer might actually view the matter of choosing a commodity bundle from a totally different perspective. That is, the consumer might have a different theory of the information one needs in order to choose.

18 Of course it can be argued that this only begs the question of what is 'too' numerous [see Shackle 1972 and Hollis and Nell 1975].

19 Kuhn's paradigm-based theory of science is an example of this conventionalist strategy of dealing with counterexamples.

20 Econometrics itself has been constructed on the basis of assuming that classical statistical theory is true and then accommodating the fact that our economic data necessarily do not conform to the assumptions of classical statistics.

21 As noted in Chapters 2 and 3, non-formalist logic can be characterized as reliance on the following axioms or principles of logic: identity, excluded middle, and non-contradiction.

22 However, this interpretation is analogous to the relationship between a differentiable function and its first derivative.

23 He probably would have denied that his theory of knowledge is inductivist, but, as I argued in Chapter 8, any view which is merely a defeatist reaction to the (eighteenth-century) failure of inductivism must carry with it the inductivist viewpoint.

24 This may also be what Samuelson calls 'multiple equilibria' [1947/65, p. 49].

16 Criticizing the value-freeness of neoclassical economics

> All human conduct is psychological and, from that standpoint, not only the study of economics but the study of every other branch of human activity is a psychological study and the facts of all such branches are psychological facts.
>
> Vilfedo Pareto [1935/63, p. 1442]

When I began teaching in the 1960s, among my colleagues it was commonplace to claim that, unlike other social sciences, economics was value-free. To some extent this was merely the old advocacy of positive economics over normative economics. To some extent, however, it was also merely naive. Those advocating particular economic policies (e.g. privitization, deregulation, etc.) are advocating and promoting specific social values. In the subsequent years, the social values expressed by mainstream economists have changed many times. There was mainstream advocacy of pro-Keynesian government policies of the 1960s, the neoconservative monetarism of the 1970s, the anti-regulation policies of the 1980s, and the more extreme anti-governmental policies of downsizing being advocated in the 1990s. Could neoclassical economists ever explain such wild swings of expressed social values?

The purpose of this chapter is to present the view that neoclassical economics is methodologically incapable of explaining the existence or nature of values, or even facts involving values, because all neoclasssical theories are based on the methodological doctrines of psychologism.[1] I shall not argue here over the wisdom of psychologism; rather, I shall argue that one of the necessary consequences of adhering to its doctrines is that it precludes explaining values.[2]

While I have made reference to psychologism in the three previous chapters, I will try to be a little more specific in this chapter since its nature and how it constrains the mainstream views of neoclassical economics will play a major role here. In my 1982 [p. 32] book I explained the distinction

between two aspects of the psychologistic individualism that dominates neoclassical economics. Specifically, there are two different methodological principles that neoclassical economists wish to obey. One is the general notion of methodological individualism and the other is the psychologism that dominates almost all social sciences. In my 1982 book they were defined as follows:

> *Methodological individualism* is the view that allows *only* individuals to be the decision makers in any explanation of social phenomena. [p. 28]
>
> *Psychologism* is the methodological prescription that psychological states are the *only* exogenous variables permitted beyond natural givens (e.g. weather, contents of the Universe, etc.). [p. 30]

It is important to recognize that these are separate methodological principles. One can adhere to methodological individualism without limiting one's exogenous variables to natural givens including psychological states. Similarly, one could easily adhere to psychologism without limiting the explanation of social phenomena to the consequences of explicit decision making on the part of individuals. In effect, psychologism would have every theory of social phenomena to be reducible to the so-called laws of psychology and so individuals may think they are making autonomous decisions but it may be that they are driven unconsciously by some inner psychological forces. With this in mind, using the definition from my 1982 book, let me be more specific.

> *Psychologistic individualism* is the combination of methodological individualism and psychologism which simply identifies the individual with his or her psychological state. [p. 30]

As I shall explain, mainstream neoclassical economists try to adhere to this combined methodological principle. As we shall see, trying to adhere to psychologistic individualism limits neoclassical economics in a couple of important ways. If one fulfills the requirements of psychologism, one will have to consider all laws of society to be explicable in terms of human nature.[3] For example, a psychologistic view of institutions would be that they are merely epiphenomena of human nature. As many economists would probably view the primary task of psychology to be just that of explaining human nature, a crude version of psychologism is that the study of society must be reducible ultimately to nothing more than the study of psychology, as John Stuart Mill seemed to believe [Mill 1843; Popper 1945/63, vol. 2].

While to many of us it may seem to be stretching a point to see anything of relevance for today's neoclasssical theory in Mill's views of methodology, it would not be as difficult to show the importance of Pareto's views, particularly when it comes to the form and methodological presuppositions of modern mathematical economics. To give a brief idea of Pareto's psychologism, at the beginning of this chapter I have presented a quotation from his book *The Mind and Society*. Beyond this, he goes on to say: 'From an examination of the facts we were led, by induction, to formulate those notions'.[4] In other words, his view is based on inductivist methodology, which he suggests was also responsible for similar ideas of other great economists – Walras, Edgeworth, Marshall, etc. It is not clear whether Pareto's psychologism was being used to justify his inductivism or his inductivism was being used to justify his psychologism. The connection between inductivism and psychologism is most evident in those economists who, by analogy with physics, identify the individuals as the atoms of our analysis such that all social phenomena are merely epiphenomena that are reducible to the behavior of the atoms, the individuals.

PSYCHOLOGISM IN ECONOMICS: PARETO REVISITED

Let me begin by illustrating how the typical explanation of economic phenomena relies on psychologism. The standard explanation of the relative price of any two goods is based ultimately on the tastes of *each* and *every* individual who is buying those goods. For any given state of technology, any change in relative prices can only be explained, in the long run, by asserting the existence of a change in one or more individuals' tastes. Anyone who is familiar with ordinary neoclasssical theory will attest to the beautiful success of modern economic theory in working out all the logical consequences and all the logical requirements for this explanation of relative prices. Ultimately, in the long run and under certain specified conditions, the going relative prices are the only equilibrium prices which correspond to a Pareto optimum. As usually defined, such an optimum is an equilibrium situation where *no* individual can be made better off without making at least one other worse off. And, above all, one of the desirable attributes of such an *equilibrium* situation is that it is consistent with the requirements of methodological individualism because in the process of reaching the equilibrium only individuals decide such an issue (as being better or worse off). Moreover, in neoclassical models, individuals do so quite independently of each other.

PSYCHOLOGISM AND VALUES

The only values considered in a neoclassical equilibrium situation are those of individual consumers as expressed in their 'tastes', which are represented by utility functions. This restricted method of incorporating values into the explanation of relative prices (that is, by making values part of the study of an individual consumer's choice) raises yet another problem.

Today, if we were to attempt to *explain* how a society makes any social choice, concerning such things as welfare programs, space programs, school systems, etc., then an all-too-familiar question arises: the question of whether there exists a social institution which is analogous to an individual consumer's utility function and which can be used to explain social choices in the same way that individual consumers' choices can be explained. The only methodological problem is one of showing that this so-called Social Welfare Function follows logically from (or is reducible to) individual values.

Kenneth Arrow devoted much of his famous study of the relationship between any social choice and individual values to the specification of the explicit conditions for an acceptable Social Welfare Function. In his famous *Social Choice and Individual Values* [1951] he shows that this methodological problem is impossible to solve since the requirements of an 'acceptable' (i.e. psychologistic-individualist) Social Welfare Function are self-contradictory under most circumstances.

Of particular interest is his second condition. If a Social Welfare Function were to exist, to be acceptable it must respond positively to each and every individual's values. So, evidently, the psychologism sword cuts both ways. While on one hand it is used to *describe* the world as it is, on the other hand it is used to *prescribe* how the world should be if the first description turns out to be wrong.

When social choice was a favorite topic for mathematical economists, sociological explanations might have seemed equally relevant. Unfortunately, many economists have considered sociology to be less sophisticated than what economists think is appropriate for science. Interestingly, Talcott Parsons and George Homans, two mid-twentieth-century giants of sociology, were avowed followers of Pareto. Moreover, Parsons had a very long history of writing in economics journals. Perhaps Parsons' forays into sociology were encouraged by his perception of Pareto's successes in economics and by Pareto's claims that his own sociological system was analogous to his economic system.[5]

Psychologism in economic explanations is often quite explicit – tastes (or the absence of changes in taste in the case of Stigler and Becker [1977]) are straightforwardly assumed. I think that a careful perusal of any non-Marxist textbook in sociology will likely show its explicit psychologism, too. For example, what is the ordinary explanation of the nature of the institution of the family? When I was studying and teaching sociology in the 1960s and 1970s, the standard explanation would have been a delineation of the functions performed by the institution, and in the case of the family, its functions would be such things as survival, reproduction, care and feeding of children, regulation of sex, etc. These functions, of course, are either directly psychological needs, or reducible to psychological needs.[6] They are, so to speak, psychological 'givens', not to be tampered with or explained.

EXPLANATION AND PSYCHOLOGISM

I shall digress a moment to revisit the requirements of any empirical explanation since my case against adhering to psychologism will be based on the logical consequences of such requirements. The most fundamental distinction to be made in any model which purports to represent an explanation of social phenomena is, as I have noted several times already, the one which separates exogenous from endogenous variables. We know we must specify which phenomena are to be explained, the *endogenous variables*. And we must specify which of the remaining phenomena are relevant because they influence, but are not influenced by, the endogenous variables; these are the *exogenous variables* that are sometimes vaguely called the 'givens'. Parenthetically, note that the popular distinction of independent vs dependent variables refers only to endogenous variables, and this distinction arises only because of the difficulty of discussing a large number of simultaneous, related events. Although endogenous variables do not influence exogenous variables, they may influence each other, which gives rise to the independent–dependent distinction. That is, for the purpose of clarifying in one relationship how the independent variable affects the dependent variables, any other relationship as to how the dependent variables may affect the independent variable is temporarily ignored.[7]

We must always keep in mind that it is impossible to explain any set of (observable) variables without asserting the existence of another set of (observable) exogenous variables.[8] A corollary of this simple theorem is that one cannot explain everything. Obviously, many things can be considered exogenous in the explanation of an individual's choice. One role of psychologistic-individualism is to prescribe acceptable exogenous variables – as noted above, it specifies that ultimately the only acceptable exogenous variables in economics are those pertaining to Nature (in economics, for example, tastes, resource availability, the laws of physics, etc.); everything else in economics can be explained as the consequences of individual choices.[9]

In the above example of sociological explanation – that is, the list of functions performed by the institution of the family – it is implied that, for some sociologists, psychological functions are considered to be the needed

exogenous variables. The obvious difficulty with relying on functions to be the needed exogenous variables is evident to any Madison Avenue advertising agent; some functions can be social conventions (e.g. shibboleths and status symbols). Attempting to explain some functions as social conventions and leaving others as givens might be quite arbitrary, but any attempt to explain all institutions and all functions would lead to circular reasoning. One obvious alternative to assuming arbitrarily that certain functions are given (psychologically or otherwise) is to consider the problems facing a society, problems over which it has no control, as the givens. I will return to this suggestion later. For now, it can be said that psychologism as a method of explanation has its virtues – but only if one can accept either its arbitrariness or its philosophical justification as a 'reflection' of one's own values.

PSYCHOLOGISM AND GENERAL EQUILIBRIUM

The one aspect of neoclassical economics that is clearly incompatible with psychologism is the mainstay of textbook economics, the short-run or, equivalently, what might be called partial equilibrium analysis. The hallmark is, of course, the *ceteris paribus* specification in such explanations. The question is merely a matter of what variables are impounded in the *ceteris paribus* clause.

In the short-run theory of the firm, by definition of the short run, physical capital is fixed and given. But capital or its utilization is not a Nature-given variable. Instead, the supply of capital (e.g. machines) and how much each firm uses are explained in the long-run theory of the firm. Similarly, the textbook short-run theories of the consumer and the firm take prices as givens. But, of course, prices are the primary endogenous variables of neoclassical economics. Alfred Marshall was quite explicit in defining the long run to be such a large span of time that almost all variables can be considered open to choice and hence, we would say, almost all are potentially endogenous. But Marshall did specify what amounts to the key exogenous variables, those that still cannot be changed in the span of time defined by the long run. Today, we would say these are such things as technical knowledge, psychologically given tastes and productive skills, as well as population and the amounts and distribution of resources. Once we specify functions representing technology, tastes and amounts representing population and resources, we can deduce a set of market-clearing prices which would allow all individuals to be maximizers. If any exogenous variable changes, then the deduced set of prices will change. Stated this way, Marshall's long-run equilibrium is very much like Leon Walras' general equilibrium theory.

In Walrasian general equilibrium theory, one does not stop to discuss the short run, but instead jumps to the long run where the general equilibrium is identified by the same set of prices that one would deduce in a Marshallian long-run equilibrium. In turn, most of the analysis of individual behavior is a special form of partial equilibrium analysis where the short-run (non-exogenous) givens have long-run equilibrium values. Supposedly, with such a special form of partial equilibrium analysis we are to claim that we have explained the individual's choices – but this is only an acceptable explanation because the values of the short-run (non-exogenous) givens can always be explained by reference to the ultimate general equilibrium.

In either way, Marshall's or Walras', the list of exogenous variables is the same. Namely, the list includes only those Nature-given variables that would meet the requirements of psychologistic-individualism. Stated more strongly, *only* in the state of long-run or general equilibrium can a neoclassical economic theorist hope to satisfy the methodological requirements of psychologistic-individualism.

PSYCHOLOGISM AND VALUES AGAIN

So far I have probably led readers to think that all I am going to say is that the doctrines of psychologism are themselves values and therefore sociology and economics are not 'value-free'. Most would be quite satisfied with that, but I would not because such a statement cannot be considered a criticism for the simple reason that one cannot find a 'value-free' methodology. My argument is at a quite different level since it concerns how values are handled within psychologism. Values, within psychologism, cannot be social phenomena apart from human nature, hence in economics values are equated to 'tastes' and in traditional sociology they are equated to psychological needs. Since variables or functions representing tastes and psychological needs are required to provide at least one exogenous variable, they can never be explained. They are, so to speak, beyond explanation.

The best thing approaching an explanation is to work 'backwards' through a given model (i.e. a completed explanation) *by assuming it to be true* and deducing what its exogenous variables must have been; that is, deducing what were the necessary functions, tastes, preferences, etc. Such an approach might be called 'revealed values analysis'.[10] It must be remembered that a great deal must be assumed in such an approach. In any case, this approach precludes our ever explaining why values have changed. And when you think of it, if there is something like human nature, how could values ever change when they are psychologically given?

Well, obviously, my argument rests on a theory of explanation – namely, the theory that we cannot explain anything without *exogenous* factors. And it rests on the nature of psychologistic-individualism, which allows only natural givens to be the exogenous variables. Allowing only natural givens means that all aspects of human nature, including values, must be considered the methodologically required exogenous factors for the explanation of any social setting. Parenthetically, if one merely dropped psychological states from the list of allowable exogenous variables, then the neoclassical explanation would reduce to a mechanical exercise where physical Nature would determine everything. This would be a form of the dreaded 'holism' [Popper 1944/61], and to avoid it one would need to consider some other form of individualism such as 'institutional-individualism' [see Agassi 1960, 1975; Boland 1982]. If psychological states are not dropped from the list, but values and value judgments are to be explained – that is, be endogenous variables – what other variables are going to be considered the exogenous factors needed to keep the explanation from being circular?

VALUES AS SOCIAL CONVENTIONS

To a great extent, criticizing the explanatory value of psychologism may very well be considered a breach of trust, but it still might be worthwhile considering whether there are alternatives to psychologism. Holism, or collectivism, has long been recognized as an alternative; that is what Marx offered when he criticized psychologism, but I do not think that holism avoids the methodological difficulties, either. The alternative that I am about to suggest explains values as social conventions or, in other words, as social institutions.

I first presented my theory of social institutions in my 1979 *Journal of Economic Issues* article. In general, it says that each social institution is a specific attempt to manifest a particular solution to a particular problem (or a set of problems). Following Popper, my theory combines institutional-individualism and his 'situational logic' with my idea that sociological behavior occurs only when someone acts upon 'cumulative expectations' (that is, where person *A* expects person *B* to expect *A* to do *X*). This idea is a special case of the usual view of 'reciprocal expectations' (person *A* expects *B* to do *Y* and *B* expects *A* to do *X*). All this is incorporated in a dialectical learning process. Institutions are then the manifestations of social learning, where learning has a very Popperian meaning.[11] Often there may be many conceivable solutions to a given problem, which presents a second-order problem – to pick a solution. To pick a solution we need criteria or constraints. Of course, the paradigm of models of the rational use of criteria and constraints is the primary method of modern economic theory. It is just this methodological need for criteria that values exist to serve. Which values are chosen depends to a great extent on the given problem facing the society. It is in this way that an old institution can affect new institutions because it brings with it the values that were used to rationalize the choice of the particular solution that it represents.

I realize that on the surface my treating values as endogenous variables might lead some readers to think incorrectly that my theory is a mere variant of relativism. In contrast, in my theory, certain things are not decided on the basis of convention. For example, whether an attempted or institutionalized solution solves the given problems is not entirely decided by convention, nor is the matter of relevance of any particular set of values entirely a matter of convention. In other words, the logic of the situation is not a matter of convention, it is exogenous, as may well be the problems themselves.

To say that values are social conventions does not mean that individuals cannot change them. However, to the extent that they have been institutionalized, they have an existence beyond the individuals of the given society. The more concrete the institutionalization, the more the situation requires political action to change the values. The less concrete the institutionalization, the more the situation requires mass persuasion to change the values. How one goes about changing values depends, of course, on the institutional situation. Such considerations, clearly, are precluded by psychologism. So, if we wish to recognize that values change over time, as clearly they do, then these values need to be explained rather than merely assumed to be exogenously fixed givens. Simply stated, my argument here is that adherence to psychologistic-individualism unnecessarily limits the explanatory value of neoclassical theory.

NOTES

1 This chapter is an expanded version of a paper that was first presented to the Social Science Division of the Northwest Scientific Association meetings in March 1970 at Salem, Oregon.

2 For arguments concerning the validity of psychologism see for example Popper 1945/63, Chapter 14 and Jarvie 1961.

3 For a broad discussion of psychologism and its alternatives see Agassi 1960 and 1975.

4 This impressive philosophy of science appears in a footnote to section 2078 of Pareto 1935/36.

5 See Pareto 1935/63 and especially the aforementioned footnote to section 2078.

6 A classic reader on functionalism is the one from that era edited by Don Martindale [1965].

7 See further Boland 1992, Chapter 2.

8 If for no other reason, the existence of exogenous variables is one requirement of testability. See further, Boland 1989, Chapter 6.

9 See further Boland 1982, Chapter 2.

10 See Boland 1989, Chapter 5.

11 For a more detailed discussion of this theory of institutions, see my 1992 book, Chapter 8.

17 Criticizing the mathematics of neoclassical economics

> the consideration of [infinity] presents us with a genuine problem; for not only by asserting but also by denying its reality we seem to be landed in a number of untenable positions.
>
> Aristotle [*Physics*, III.4.203b]

> [Calculus is] the art of numbering and measuring exactly a thing whose existence cannot be conceived.
>
> Voltaire [1733]

In this chapter I want to talk about the foundations of modern microeconomics. At the outset I wish to make clear that I am not using the word 'foundations' in any profound philosophical way but only indicating that I am interested in examining the fundamental assumptions we all make in the development of our microeconomic theories and models. It is always risky talking about foundations since all benefits are obtained only at high costs. The foundations I have in mind are those directly implied by the neoclassical maximization hypothesis, that is, the one key behavioral assumption of neoclassical economics. 'Maximization' in the context of explanation directly involves the use of calculus, at least in all textbooks.[1] While textbooks will talk about 'marginal utility' or 'marginal revenue', actually they are discussing the

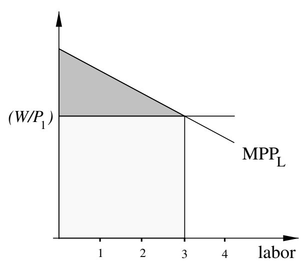

Figure 17.1 Continuous MPP$_L$

first derivatives of specific utility or revenue functions in the usual calculus sense. Whether calculus is always implied depends on what we mean by explanation and how our notion of explanation is incorporated in the neoclassical explanation of prices.

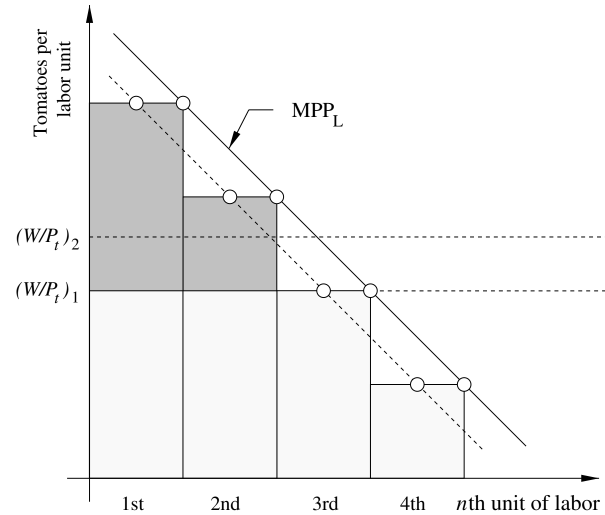

Figure 17.2 Discrete MPP$_L$

One of the ideas I uncovered while working on my 1982 book is that beliefs in induction and inductive learning are closely tied to the concepts of infinity and infinitesimals that are at the foundation of calculus. This alerted me to remember my undergraduate studies of calculus. Since so much of microeconomics is based on ordinary calculus concepts, I thought it appropriate to begin by examining the role of infinity and infinitesimals and then to examine their relationship to the recommended rejection of any dependence on induction.

INTEGRATING THE INFINITESIMAL

As we all know, the basic tools of calculus are the derivative, the partial derivative and the integral. I want to argue here is that sometimes these tools do not make real sense. The problem that is of the most concern is apparent in the idea of an integral. To illustrate the problem let us first present the textbook calculation of total output obtained by varying the level of one input as shown in Figures 17.1 to 17.3. Figure 17.1 represents the marginal (physical) product (MPP_L) for infinitesimal variations in labor input, and Figure 17.2 represents the marginal product for discrete units of labor. Supposedly, we can calculate the total output by integrating the function represented by the continuous marginal productivity curve as the input is increased from zero to the level in question – that is, by adding up the contributions of each infinitesimal unit of labor from zero to a specific level of input.

While integration always makes sense when calculating the total output for discrete units of input (Figure 17.2), there may be significant discrepancies when compared to the calculated output for infinitely divisible units of labor (Figure 17.1). The discrepancies are supposed to disappear when we define the differences in Figure 17.2 to be so small that for practical purposes the curve of Figure 17.1 would be indistinguishable from the line formed by connecting the upper right-hand corners of the boxes in Figure 17.2.

From a crude practical perspective any problem here is difficult to see, but the logical basis for the alleged equivalence of these two figures is not very satisfactory. In Figure 17.2 we see that by calculating output as the sum of the areas of all the boxes (each representing the marginal contribution of the *n*th unit of labor) we are thereby ignoring the little empty triangle at the top of each box and thus the calculated output is always less that of the area under the corresponding smooth curve representing the partial derivative.[2] So the question I ask is: why do we learn to ignore the obvious discrepancy revealed in the comparison of these two diagrams?

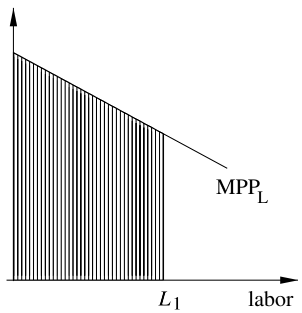

Figure 17.3 Infinitesimally discrete MPP_L

The usual strategies explain away the apparent discrepancy. One way is to appeal to a very special case where the marginal productivity curve is a straight line that connects the midpoints of the tops of all the boxes. In this special case the discrepancy disappears since the two triangles between the marginal productivity curve and the top of any box are congruent triangles and thus

the one which is an overestimate of the marginal productivity is cancelled out by the other one which is an underestimate – but this is a very special case and is accurate *only for straight-line marginal productivity curves*.

The more common strategy would have us consider each unit of labor to be extremely small, such that the width of each box in Figure 17.2 is less than what we could show by even a single vertical line, and thus would have us pretend not to see the discrepancy. For example, examine Figure 17.3, where I have drawn vertical lines with supposedly no space between them. In this sense the vertical lines fill the area under the curve. This is, unfortunately, more a commentary on printing technology than on the alleged equivalence of Figures 17.1 and 17.2. So long as labor is measured in discrete units there will always be an empty triangle uncounted at the top and the sum of the triangles will always be non-zero.

Leaving special cases and printing technology aside, the intellectual strategy to avoid the discrepancy would have us believe in the idea of an infinitesimal. That is, we are taught to believe that it is logically possible to have the unit of labor be so small that it is *as if* it has a zero width so that the triangle at the top has a zero area (since the length of its base would be zero), while simultaneously the area of the box is not zero even though it has the same zero base. We cannot honestly avoid the contradiction here.

Early critics of calculus were quite aware that we cannot have the sum of the areas of the boxes being positive while the sum of the areas of the corresponding triangles are being considered zero. Judging by today's calculus textbooks, it seems to be widely believed that there is no problem here. The accepted proof that there is no problem relies on an argument that the area under a curve can be considered to be the 'limit' of the sum of an *infinite series* of units of labor as the unit of measure 'approaches zero' – or, to use the older language, when the unit of labor is an infinitesimal. Now, this may solve the logical problem but *only* if we accept the idea of an infinite series or an infinitesimal which logically amounts to the same thing. If we do not accept either concept, then applications of calculus are left in a questionable state.

PROOFS VS INFINITY-BASED ASSUMPTIONS

For my purposes here, what is important is the following. All analytical theorems, which are 'proved' by the analytically sophisticated consumer theorists, involve some sort of infinity assumption. They do so either directly, by referring to an infinite set, or indirectly, by referring to infinitesimals in the neighborhood of the consumer's chosen point. The irony of this is that infinities must be invoked to explain the finiteness (or discreteness) of the consumer's unique choice or the market's unique demand curve.

The use of infinitesimals is obvious in any analytical proof involving derivatives or differentiable functions. Even the simplest definition of a derivative – namely the slope of a function – relies on the infinitesimal. Consider Figure 17.4, where I have drawn a non-linear function $f(X)$ and its slope at point X_o. The slope there is $(c+b)/a$, and if X changes by a finite amount a, the ratio of the change in $f(X)$ to the change in X is b/a. So long as a is not zero there is a difference between the slope and the ratio of the changes (or differences). The slope will equal the derivative if the derivative is defined as the ratio of the changes at the point where a paradoxically has the value of zero but not yielding the usual consequences of division by zero. Usually, dividing by zero is considered to yield an infinitely large ratio value. Printing technology notwithstanding, we are to think of the function as being complete in the neighborhood of X_o, in the sense that it is continuous, and no matter how small a gets there exists a value for $f(X_o+a)$. In effect, between X_o and X_o+a there must be an infinity of points on the function between $f(X_o)$ and $f(X_o+a)$ and the function as a mapping from X-space to a $f(X)$-space must be complete between X_o and X_o+a.

Historically, many students of calculus have been uneasy about relying on the mysterious and paradoxical concept of an infinitesimal which supposedly has a zero value but has properties of being non-zero. To avoid the use of such a concept, most textbooks today define the derivative in terms of what are called 'limits'. Rather than refer to infinitesimals, today the derivative of $f(X)$ shown in Figure 17.4 is defined as the limit of the ratio b/a as a approaches zero. The following is a simple calculus definition of a limit that can be found in any typical undergraduate textbook:

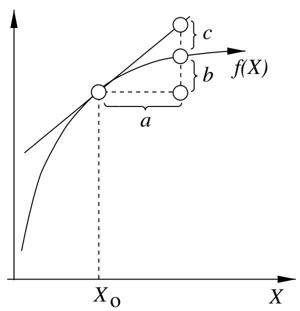

Figure 17.4 The derivative

> Let $f(y)$ be a function of y and let k be a constant. If there is a number L such that, in order to make the value of $f(y)$ as close to L as may be desired, it is sufficient to choose y close enough to k, but different from k, then we say that the limit of $f(y)$, as y approaches k, is L.

It is a mystery to me how defining a derivative in terms of the concept of a limit is in any significant way an improvement over an infiniesimal-based definition. Naive defenders of the limit-based definition will say that

it is because the derivative is defined by a real quantity, namely L, but this only begs the question of how we know we are at L. Sophisticated defenders will enhance the definition by referring to the limit L as the ultimate value of an infinite sequence of points where each additional point lies between the last point and the point representing L. Again, we are no better off and maybe worse off since we are again referring to an impossibility – namely, an infinite sequence.

THE AXIOM OF CHOICE

While the limit-based definition of a derivative is still widely accepted, some mathematicians have tried to express such definitions in terms of what they call the 'axiom of choice'. This axiom says: 'given any collection of sets, finite or infinite, one can [choose] one object from each set and form a new set' [Kline 1980]. While this axiom may be trivial for any finite collection of finite sets, there is no reason to accept it otherwise. Nevertheless, it can be used to define a limit along the lines of a paradox of Zeno [Boyer 1949/59]. Namely, take the distance between the limit L and any point different from L, form a set of the points representing one-third the distance and two-thirds the distance and choose the point which is closer to L. Now repeat this process *ad infinitum*. Supposedly, we can use the 'axiom of choice' to prove that the ultimate result is to choose L. Of course, this in no way escapes the criticism of relying on definitions and proofs which are impossibilities since they depend on infinite sets which are impossible.

It would probably be wiser to avoid trying to prove that the derivative of a function is the slope of a curve representing the function and accept the claim as a conjecture and move on from there.

FALSE HOPES OF SET THEORY

In the 1960s we learned to look away from these potential problems of calculus by restating the familiar economic propositions in terms of set theory. For example, consider Figure 17.5, where I have drawn an ordinary indifference curve for goods X and Y. Sixty years ago the indifference curve was viewed as a differentiable function and the slope of the curve was the partial derivative which was called the 'marginal rate of substitution' or MRS for short. Hicks and Allen [1934] argued that the usual propositions of demand theory could be shown to depend on the assumption that this partial derivative diminishes (i.e. approaches zero) as points to the right along the curve are considered. At any consumer's chosen point the partial derivative equals the ratio of the respective prices since that ratio is the slope of the usual price-taker's budget line. The requirement of a diminishing MRS was supposed to be methodologically superior to the older requirement of a diminishing marginal utility since the latter suggested a cardinal measure of utility and the former did not. Unfortunately, this was misleading as the function representing indifference was just a specific case of a multi-good utility function where the utility is held constant. How can utility be held constant without our being able to measure its cardinal value? Perhaps this is only a rhetorical question, but never mind because since the mid-1950s we have been taught to abandon calculus in favor of set theoretical interpretations of concepts such as indifference. Let me review the basics of the set theory approach.

Using set theory, the indifference curve becomes merely a set of points between which the individual consumer is said to be indifferent. Specifically, the indifference curve drawn through the chosen point is the boundary of two sets. On one side is the 'better than set' which contains all points considered superior to the chosen point. On the other side is the set containing all points which are considered worse than the chosen point. If we assume that the consumer has spent all of his or her budget, the only reason why the points in this 'better than set' are not chosen is simply that they are all outside the set of affordable points represented by the area of the right triangle formed by the budget line given the size of the budget (or income) and the prices of the two goods.

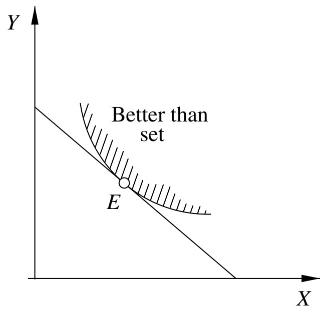

Figure 17.5 Convex indifference

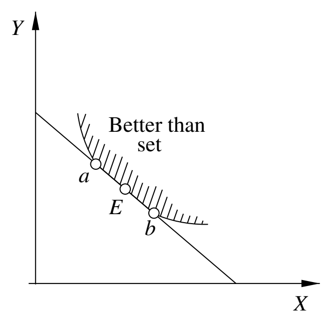

Figure 17.6 Non-convex indifference

Defining indifference in terms of set theory is still not enough if we want a complete description of the situation facing the individual who is

doing something like maximizing utility or, using set-theory terminology, choosing the 'best bundle or point among those that are affordable'. What we need for a complete description is an assumption that the 'no-worse set' (the union of the 'indifference set' and the 'better than set') forms a *convex* set. This is still not enough if the chosen point is supposed to be the only point that the individual would choose when facing the budget line in question. That is, for a unique point, the 'no-worse set' must be *strictly* convex. This rules out convex sets such as the one shown in Figure 17.6. Figure 17.5 shows a strictly convex 'no-worse set'. So far, this is all basic stuff.

Note that the indifference curve shown in Figure 17.6 would not satisfy the old Hicks–Allen assumption of diminishing MRS since between points *a* and *b*, MRS is not diminishing. Moreover, if the individual maximizer faced the indifference curve of Figure 17.6, we could not provide a complete explanation for why point *E* was chosen rather than *a* or *b*, or any other point on the line segment between *a* and *b*. When describing the uniquely chosen option, *E*, we assume either that each indifference curve always displays a diminishing MRS or that the 'no-worse set' is strictly convex. If we are going to keep our explanation of the individual's choice behavior consistent with the methodological requirements of neoclassical economics by maintaining that the individual must be sensitive to all price changes, the two supposedly different assumptions will have to be logically equivalent.

If the assumption of diminishing MRS and the assumption of strictly convex 'no-worse set' are equivalent, why would anyone bother reinterpreting all the propositions of economics into the language of set theory? Obviously, the use of set theory was thought to be an advance and thus the two assumptions must not be considered equivalent in any important way.

I do not think set theory represents an advance over the difficulties of calculus – only the names are changed to hide the problems of infinity and continuity. Let me now outline my indictments. Cardinality of utility was once considered too strong a requirement for any realistic analysis of consumer demand. Today, continuity of indifference curves is similarly considered to be more than what is necessary for a logically complete analysis of consumer demand.[3] When the consumer is said to choose the best point among those that he or she can afford, there is nothing obviously implied to indicate that the chosen point is on some continuum which allows for infinitesimal adjustments. Note, however, such a continuum is necessary for calculus-based partial equilibrium analysis.

By our abandoning calculus in favor of set theory, an individual's choice would is now a matter of choosing a particular integer from a set of integers. Such a set is not usually considered a continuum; rather it is 'connected'. A connected set is one which can always be separated into two subsets such that there is no point in the set that is not in one of the subsets [see Chipman 1960]. For example, the set of integers can be separated between those less than or equal to N and those greater than or equal to $N+1$. There is no integer in the set between N and $N+1$, by the usual definition of an integer. Now, if the use of set theory is to be considered an advance, the critical question is whether a set's being 'connected' is in any important way different from its being 'continuous'. Surely, the mere idea of recognizing the concept of an integer presupposes some number which is conceived not to be an integer. If not, then there cannot be any difference between the boundary of a connected set and a continuous function such as an indifference curve.

UNREALISTIC DISCONTINUITIES

For reasons unclear to me, it is still maintained that by discussing set theory, in the sense of a set of integers, we are in some way not discussing continuous functions and hence not discussing something for which calculus methods would be applicable. Even when discussing such things as a textbook 'kinked demand curve' or any continuous function which has a sharp bend in it, all that is begged is the question of why there are holes in the curve representing the derivative of that continuous function (or representing the partial derivative when there are many arguments in the function). Of course, what is really questioned here is the definition of a 'sharp bend'.

Consider Figure 17.7. If Figure 17.7(a) represents a continuous total revenue function, $f(X)$, that has a kink in it, then the usual idea is that the derivative appears as shown in Figure 17.7(b). The function representing the derivative, $f'(X)$, may be continuous with respect to X, in the sense that there are no values of X for which the value of the derivative is not defined. However, while mathematicians are only concerned with whether the derivative is always defined over the continuum of values of X, the derivative is not continuous with respect to the conceptual continuum of its own value as there are conceivable values (between r and t) which are not represented by the derivative-function. As economic theorists we want to give real-world meaning to the value of the derivative, such as when we set the value of marginal revenue equal to the value of the marginal cost for profit maximization. Of course, analytically we can have any kind of function we can conceive. But the question that might be asked is whether Figure 17.7(b) can actually represent a realistic process as in the case where the (partial) derivative represents marginal revenue. What Figure 17.7(b) implies is that as X increases value from that below X_o to that

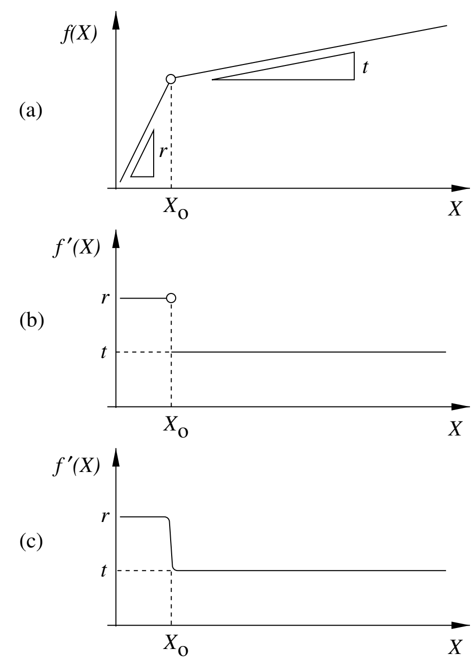

Figure 17.7 Discontinuous revenue

above, somehow the derivative instantaneously changes from r to t at X_o. The term 'instantaneous' really means infinitely fast; and since an infinite speed of change cannot be represented by a real-world process, the realism of Figure 17.7(b) is questionable.

My argument here is against the idea that the use of set theory constitutes an advance merely because it can deal with discontinuous functions. While set theory may be able to do that, the question concerns whether we want to deal with discontinuous functions this way. Such functions do not usually correspond to real-world processes. Of course, this only raises the question of what is meant by a 'real-world process'. While set theorists may be free to assume any analytical function they wish, I am just as free to say that anything requiring infinite speed or infinite time or space is something that is not of the real world. The case shown in Figure 17.7(b) is impossible but that in Figure 17.7(c) is possible. This is to say that the 'sharp bend' in the function of Figure 17.7(a) is one where the slope changes from *r* to *t* in a continuous way, such that there are no missing values between *r* and *t* as there were in Figure 17.7(b). I will have more to say about this view of 'realistic' functions in a later section. For now all that I wish to establish is that either we can always rule out any discontinuous functions as unrealistic functions or, what amounts to the same thing, we can say that any realistic boundary of a set of 'connected' points is also a continuous function. In this sense, it can be argued that there is nothing realistic to be learned from set theory that cannot be discussed using calculus concepts.

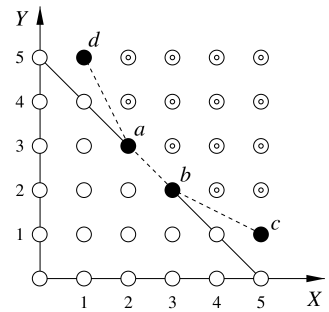

Figure 17.8 Incomplete explanation

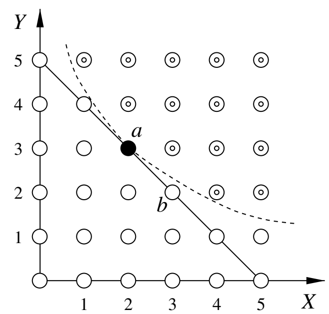

Figure 17.9 Complete explanation

My arguments not withstanding, set theory has served as the medium for many sophisticated presentations of the logical foundations of the neoclassical theory of the consumer [e.g. Chipman *et al.* 1971]. One obvious use of set theory allows for a realistic representation of the consumer's choice between bundles consisting of goods which are obtainable only in integer quantities. But using set theory to represent integer choices raises methodological difficulties concerning what constitutes a satisfactory explanation in neoclassical economics.

We say that the individual consumer chooses the 'best' point which he or she 'can afford' with the given budget (or income) and prices. The individual provides the subjective criterion used to define what is 'best' and the objective criterion determining what the individual 'can afford' is a matter

of arithmetic. What is to be explained is the specific choice or decision made by the individual in question. Put this way, the choice is necessarily unique and any explanation should entail such uniqueness. The theorist need only conjecture what the individual's preferences or decision criteria are to complete the explanation of the consumer's unique choice. To be complete the explanation must not only entail the chosen point but it must be the only point the individual would choose under the circumstances. That is, we must be able to explain why all other points are not chosen.

With this in mind, consider a consumer choosing one point in an integer space such as I have illustrated in Figures 17.8 to 17.10. Usually, we are to explain why the individual chose a point, say *a*, given a budget with which the individual could buy other integer combinations. These figures raise important difficulties for our theory of prices as well as possible problems for our explanation of even one consumer. If the conjectured indifference 'curve' is represented by the four solid points shown in Figure 17.8, then our explanation there will be incomplete since we are unable to explain why the individual chose point *a* rather than point *b*. That is, since both points lie on the budget line and both lie on the same indifference curve, they are equivalent according to both subjective and objective criteria. Thus, even though the 'no-worse set' is connected (i.e. there are no conceivable non-integer points) and it is convex, the explanation is incomplete.

The explanation can be made complete in two apparently different ways but neither is entirely satisfactory. If we allow the calculus-type analysis to define the conjectured indifference curve over the non-integers as well as the integers so that it appears as a smooth curve exhibiting the usual assumption of diminishing MRS (see Figure 17.9), the curve will be conjectured to be tangent to the budget line at only the chosen point. The other point, *b*, will be inferior. While this may seem convenient for some purposes, it raises the question of realism again but this time the problem is that the indifference curve refers to points which are unrealistic by not all being combinations of integer quantities. The other way of completing our explanation is the one illustrated in Figure 17.10. This way reveals a more serious methodological difficulty and one that Figure 17.9 suffers as well.

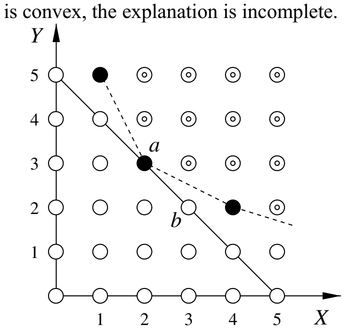

Figure 17.10 Complete explanation?

INTEGERS VS THE EXPLANATION OF PRICES

The issue that we have to face concerns the purpose of any explanation of any consumer's behavior. Again, every theorist is free to do whatever he or she wants. Nevertheless, the primary reason we discuss the consumer theory in the context of neoclassical economics has always been to see the consumer as a part of our larger theory of prices, where the individual is conjectured to play a significant role. It is important that if the consumers are to play a significant role in the ultimate determination of equilibrium prices then their choices must be sensitive to price changes (i.e. prices must matter). We see that in Figures 17.9 and 17.10 there is a range of possible prices for the unique chosen point where the individual does not alter the chosen point.[4] When we aggregate, the market demand curve becomes fuzzy in that there is a range of possible demand prices for any quantity hence our explanation of price will be incomplete even though the explanations for some of the individuals will be complete.

INFINITE SETS VS COMPLETE EXPLANATION

The question of the proper role of consumer theory in the neoclassical theory of prices leads to a broader question concerning the completeness of the conjectured preference ordering of the consumer. If we are to use the theory of the consumer as a foundation for price theory, then we must be able to explain the consumer's behavior no matter what price levels are present in the market. This is because to explain prices we must not only explain why the price is what it is, but we must also explain why it is not what it is not.[5] Thus, it is never enough to explain the individual's choice given just one budget line [cf. Batra and Pattanaik 1972]. No matter what the prices may be today or in the future, the individual must be able to make a distinct choice. This means that the conjectured preference ordering or indifference map must extend indefinitely in all directions. That is, the individual must be able to compare *any* two conceivable points, or be able to attach a specific level of utility to any conceivable point.

Certain methodological questions are raised by these considerations. In effect, the conjectured indifference map or preference ordering of neoclassical theory must extend over what may be an infinity of conceivable points. How does the individual learn what his or her preferences really are? Such knowledge might require an infinity of trials! But what is even worse, any sophisticated analysis of consumer preferences must also deal with preference orderings over an infinity of conceivable points regardless of how the individual learns.

Some very sophisticated consumer theorists have relied on a so-called

'axiom of choice' to extend knowledge about the preferences from being over realistic finite subsets to being over infinite sets as is required for completeness [see Chipman *et al.* 1971, p. 250]. This is the axiom noted above that is often used by mathematicians and it is to be distinguished from the axiom of choice discussed by economists [e.g. Frisch 1926/71; Samuelson 1938]. The important point is to recognize that the question of completeness of preference orderings too easily involves us in a discussion of infinite sets. This is an important problem because, in realistic terms, the meaning of 'infinity' always refers to an impossibility.

The common ideas of continuity, completeness, infinity and infinitesimals are all closely related, even though this is not always obvious. I just finished discussing the direct relationship between completeness and infinite sets. The relationship between infinity and infinitesimals is more obvious. For example, any ratio such as A/X is said to become an infinitesimal (i.e. approach zero) as X approaches infinity. I mentioned just above that some consumer theorists recognize the direct relationship between completeness and infinite sets. What may be still in doubt is the relationship between continuity and completeness. I will discuss this last relationship and then get to the real concern, which is the relationship between the complete preference orderings, infinite sets and inductive learning.

Continuity is very important for calculus considerations, as is well known. Nevertheless, establishing continuity always runs the risk of an infinite regression. We take for granted that ordinary Euclidian space can be represented by real numbers along each of the coordinates. For example, we can conceivably plot a consumer's choice point as being equal to one-half of a radio and two and one-third calculators, regardless of the question of whether such non-integer quantities make sense to us. Given the assumption that radios and calculators only come in whole units, the set of possible (as opposed to conceivable) choice points do not completely cover the Euclidian space representing quantities of radios and calculators. Now consider an indifference curve for either radios or calculators such as the one in Figures 17.8 or 17.10. If one insists on using the Euclidian coordinates to represent quantities of these indivisible goods, when only integer points are possible in the eyes of the consumer, then the indifference curve will only be a sequence of points that are unconnected in Euclidian space – that is, points with large (Euclidian) spaces between them. The preferences represented by this integer indifference map will be neither continuous nor complete *with respect to the Euclidian space* that we commonly use as our coordinates. But from the realistic viewpoint of the consumer, the non-integer points are irrelevant and thus the alleged discontinuities in the indifference map are misleading. This is why the question of considering the set of possible choice options as a connected set rather than a continuous space can be important in any analytical treatment of consumer theory.

Switching from incomplete continuous-space indifference maps to connected sets of possible choice points solves the problem of misleading non-continuity but it may not ensure that all preference orderings of such connected sets are complete. What if the individual is, perhaps for mysterious psychological reasons, unable to evaluate the single point representing three radios and three calculators? The indifference map, whether for Euclidian space or the connected set of possible choice points, will have a hole in it at that point. On the one hand, if the prices and income facing the individual consumer are such that he or she cannot afford to buy three radios and three calculators, then the hole in the map would seem to be irrelevant for our theory of the consumer's behavior. On the other hand, if income allows the consumer to afford this point, our explanation of why he or she bought any other point will be incomplete, since we cannot explain why the point representing three units of each good was not chosen. Inability to evaluate the point is not a sufficient reason, since the point is still possible and since a non-evaluation is not the same as an under-evaluation.

The idea here is simple. A continuous indifference map must also be a complete map – whether we mean continuous in the Euclidian space or in the restricted terms of the set of connected possible points. Any discontinuity (or hole) in the map is also an instance of incompleteness.

Much of what I have been discussing has been the concern of analytical consumer theorists who have tried to prove that demand curves with certain specified mathematical properties can always be shown to be 'generated by the maximization of a utility function' [Hurwicz and Uzawa 1971, p. 114]. More generally, they have been concerned with the problem of how much we must know about the demand curves to be able to deduce the utility function that is being maximized. Since a demand curve is the locus of utility maximization by all demanders, its calculus properties are those of the various relevant partial derivatives in the close neighborhood of the maximizing points. However, any demand curve (or demand function, if we wish to stress that more than one good is being simultaneously chosen) is just a line connecting a subset of singular points drawn from all the points on the indifference map. One demand curve cannot tell us much about the entire indifference map from which it was derived. To determine the underlying map or utility function we would need many observations of many demand curves. This problem of deducing the general nature of the utility function from the singular marginal properties of any particular set of demand curves (i.e. curves for many different choice situations) is the

one briefly mentioned in Chapters 4 and 15 and has been identified by many theorists as the 'problem of integrability'. But giving it a name does not make it solvable [see Wong 1978].

INDUCTIVE KNOWLEDGE AND INFINITY-BASED ASSUMPTIONS

While accepting complete preference orderings as conjectures about infinite sets would seem to satisfy the requirements of analytical proofs, there are still questions begged when we turn to consider the implications of such conjectures for the capabilities of the individual whose behavior is being explained. As noted earlier, if we say the individual chooses the one best point out of the infinity of possible points, how does the individual know it is the best point unless he or she has knowledge of the infinite set? Again, the question arises because the concept of infinity is by definition an impossibility. Does this mean that such knowledge is impossible?

It would be only if we were to continue the neoclassical tradition of believing that all learning must be inductive. Recall that inductive learning is based on the assumption that we learn with each new bit of information acquired. That is, with only singular observations of a particular instance of a general proposition, we are led to conclude that the general proposition is true. The typical illustration is that by repeatedly observing white swans flying south for the winter we are learning that all swans are white. Inductive learning is learning the truth of a general statement from observing numerous particular examples. It is in this sense that the individual might be conjectured to learn what his or her preferences are by merely tasting each conceivable point in the relevant goods-space. But unfortunately this theory of learning fails for simple reasons of logic. No amount of finite evidence about the singular elements of an infinite set could ever prove that such a set has specific general properties [see further Popper 1972, Chapter 1 and Appendix]. And, of course, the next swan to fly over may not be white.

These logical considerations raise doubts about all analytical models that presuppose that the individual consumer has sufficient knowledge. This not only criticizes the view that an individual could evaluate the point representing a million radios and a million calculators, it also criticizes the view that the consumer has the complete ordering needed to be able to evaluate a point representing one-millionth of a unit of tea and one-millionth of a unit of coffee. While it is easy to see that it would be difficult to learn about points approaching infinity, it should be equally apparent that it is just as difficult to learn about the infinity of points in the neighborhood of the maximum of any constrained and differentiable utility function. And so the use of (partial) derivatives to explain the shape of indifference curves or demand curves necessarily goes far beyond what is intellectually possible for the individual decision maker. While this might not matter for the analysis of a state of equilibrium, a disequilibrium analysis is predicated on at least one individual in some way being aware that he or she is not optimizing. If one insists on maintaining the common presumption of inductive learning, then disequilibrium analysis is impossible.

If one rejects the idea that people learn inductively, one will find it difficult to appreciate the many published articles and papers which provide proofs of propositions about the general properties of preference orderings or about demand curves based on those general properties. It does not matter whether the proofs are based on calculus concepts or set-theoretic concepts, since the proofs must always deal with some form of completeness of the individual's preference ordering and thus must refer to either infinite sets or infinitesimally close neighborhoods of specific points. A way out is to treat the individual's preference ordering or utility function as a conjecture *on the part of the individual consumer*. What is the cost of such an approach?

By viewing all individuals as inductive learners, theorists have been able to rely on the observability of the individual's objective situation to ensure unique and mechanically consistent choices. For any given type of preference ordering (determined by specific assumptions on the part of the theorist), proofs could thus be reliably constructed. But what if one does not really learn inductively? Even if an individual still has a specific type of psychologically given preference ordering, the individual consumer does not know its true nature and thus has to conjecture about his or her preference ordering. Using a conjectured preference ordering may not always produce choices consistent with the true ordering. This is because there is no reason why, without reliable inductive learning, the individual has been successful in learning his or her true preference ordering.

LESSONS UNLEARNED

In this chapter I have discussed fundamental methodological problems with calculus and its set-theoretic representations. I suspect that few neoclassical readers will be impressed. To raise such questions is to put oneself in the position of the child in Hans Christian Andersen's famous story 'The Emperor's New Clothes'.[6] After the child asks why the Emperor is not wearing clothes, his father apologizes for his 'ignorant' son. The question I ask is, why do so many people accept calculus and calculus 'proofs' when it is so easy to see that they are based on impossible entities such as infinity and infinitesimals?

Until a few years ago, almost all undergraduate programs in engineering, science or economics would require the completion of a calculus course at the beginning of the program. Even mathematics programs had such a requirement. I was told many times that, while engineering might find calculus useful, mathematics programs used the calculus course only as a means of prescreening potential mathematics majors. It was thought that if a student could not pass the calculus course, there was no chance that he or she would survive in a mathematics program. In retrospect, they were correct. Unless you can tolerate the unrealism of calculus, you will have difficulty tolerating all the other convenient assumptions typical of proofs involving infinity and infinitesimals.

NOTES

1 Textbooks that heavily promote game theory might be exceptions in the same way older textbooks based on linear programming avoided direct applications of calculus.

2 And amazingly, some neoclassical economists criticize Friedman's rather limited 'as if' methodology!

3 Game theory (and linear programming analysis) in effect merely reduces the explanation to a small subset of points on one indifference curve or one budget line.

4 The slope of the budget line represents the given ratio of prices and so many slopes can be consistent with the same chosen point.

5 See, for example, Nikaido 1960/70, p. 268.

6 For those reader, unfamiliar with Andersen's story, let me give a summary of the essential points. First, two 'confidence men', claiming to be sellers of dry goods, come into the Emperor's village and say they have a new special fabric which only competent people can see. They offer to make a suit of clothes for the Emperor (for a price, of course) which would be an excellent test for his subjects and his court. The Emperor accepts their offer and their price. Having the new, special suit of clothes, he (not willing to admit that he sees nothing) 'puts on his new clothes'. He then holds a parade to display his new clothes. As the Emperor marches by, a little boy in the audience asks his father, 'Why does the Emperor not have clothes?'. Most tellers of the story stop at this point. But actually it is the father's reaction that is the most important statement in Andersen's story. Specifically, the father says, 'Please excuse my *ignorant* child!'.

18 Criticizing stylized facts and stylized methodology

> Since facts, as recorded by statisticians, are always subject to numerous snags and qualifications, and for that reason are incapable of being accurately summarized, the theorist, in my view, should be free to start off with a 'stylized' view of the facts – i.e. concentrate on broad tendencies, ignoring individual detail, and proceed on the 'as if' method, i.e. construct a hypothesis that could account for these 'stylized' facts, without necessarily committing himself on the historical accuracy, or sufficiency, of the facts or tendencies thus summarized.
>
> Nicholas Kaldor [1963, p. 178]

In 1984 I was asked by the editors of the *New Palgrave Dictionary* to contribute two entries – one on 'methodology' and the other on 'stylized facts' – and I agreed to do so. While my entry on methodology was immediately accepted, the one on stylized facts had its own mysterious saga. When I agreed to write an entry on stylized facts, I asked the editors whether they wanted me to write on stylized facts as they are discussed by Nicholas Kaldor [1963] and Robert Solow [1970] or more generally as they are discussed today. I was told that they meant stylized facts in general and not the particular examples referred to by Kaldor and Solow. So, I wrote an entry on stylized facts as they are generally understood today. They did not like my entry at all. They said that I had 'emasculated' Kaldor's idea. So I wrote a second version of the entry. They said that this version extended Kaldor's idea so far as to empty it of whatever little meaning it originally had. Now, what did they say about whether I should write about how it is used today rather than how Kaldor discussed stylized facts? Well, I wrote a third version which was deemed to be 'admirable' and thus accepted and published [see Boland 1987b]. The editors did me a favor by forcing me to write this third version even if it was exclusively about Kaldor's idea.

Actually, Kaldor's idea is very interesting. Kaldor's objective was to find a set of agreed-upon facts (i.e. stylized facts) which both neoclassical economists and advocates of his Keynesian–classical model would attempt

to explain. Kaldor identified six so-called stylized facts. His methodological purpose was entirely polemical and, in the context of my critical approach to methodology displayed in this book, most admirable. But, except for the brief quotation above, little of his idea is understood today; and unfortunately, as a consequence of this misdirected saga, I never got to comment on the use of stylized facts by mainstream economists. In the next section I will present the essence of the first two versions of my entry, which addressed the use of stylized facts in the mainstream.

STYLIZED FACTS IN USE TODAY

Stylized facts are not what we ordinarily mean by facts. They are but convenient figments of our theoretical imagination. If they were ordinary facts, they would not be called stylized. Nicholas Kaldor identified six stylized facts that any respectable growth theory should explain. These included statements about the constant long-run growth rates of per-capita real output and the stock of real capital.

When we sit down at our desks and look out of our windows we see a rich and extremely diverse collection of ordinary facts. We might see the sun shining, a little rain, or the corner streetlight. If we look more closely we might see rising prices, rising interest rates, or even falling employment levels. But do we ever look out of our windows and see per-capita real output growing at a constant rate, the stock of real capital growing at a constant rate, or a constant ratio of capital to output? Departing from the things Kaldor wanted us to explain, do we ever actually *see* a downward-sloping demand curve or even a demand curve? Do we ever see a diminishing marginal product of labor or the implied rising supply curve for any produced good? Do we ever actually see a decision maker maximizing his or her utility?

It would be all too easy for a critic of one's theory or model to claim that the use of stylized rather than ordinary facts was invoked for the sole purpose of avoiding obvious empirical refutations. Avoiding this critical argument will always be difficult except when one also explains why the stylized facts in question are essential. Without an explanation of the essentialness of the stylized facts, the theorist's situation is completely arbitrary. Stylized facts are essential only by design. That is, stylized facts are a form of imaginary phenomena that many of today's models are designed to explain. We never directly observe stylized facts outside our windows. Except for a few extremist methodologists, most people think that the primary purpose for developing economic theories or models is to explain the observable facts of the real world, that is, of the world we see outside our windows. The term 'explain' usually means that we as economists should be able to give reasons for why we observe particular phenomena. The reasons fall into two categories. The first category consists of assumptions about the behavior of individual decision makers who affect the observable phenomena. The other category includes statements about the actual nature of the real world which also affect the observable phenomena. None of the assumptions of our theories or models can be considered stylized facts. Usually economists need to draw assumptions from both categories.

To illustrate the difference between assumptions and stylized facts, consider the design of consumer theory. The assumption that consumers choose their level of demand to maximize their utility is not a statement of fact but only a behavioral assumption. The statement that the marginal utility diminishes with the level of consumption is not considered a stylized fact for economists. Instead, the statement is merely a logical implication of the maximization assumption. By design, we use the statements involving utility maximization and diminishing marginal utility to explain why the demand curves are normally downward-sloping. For the purposes of consumer theory, downward-sloping demand curves are thus stylized facts. Since demand curves are not directly observed, we do not know for sure that they are always downward-sloping. Nevertheless, we continually make the capability of explaining the stylized fact of downward-sloping demand curves a requirement of any acceptable theory of consumer behavior. Similarly, we have never actually seen a supply curve but we still expect every short-run theory of the firm to be capable of supporting the establishment of the stylized fact that says all supply curves are upward-sloping, at least in the short run.

While many of us might not think of ordinary demand or supply curves as stylized facts, they seem to play a role analogous to Kaldor's stylized facts of growth theory. We can push the analogy even further. What does price theory explain? Prices? Which prices? Is it the daily prices that appear in the market in the center of a town such as Cambridge, England? Daily prices are neither the short-period nor the long-period prices that appear in the typical theory of the firm. Remember, Marshall identified three different prices. There was one for the very short period, one for the short period and one for the long period. He said that these periods corresponded to a week, a year and a generation [Marshall 1920/49, pp. 314–15]. While we might conceivably see a price for this week's market, we never see a year's short-period price or a generation's long-period price. Given that supply is difficult to change quickly, the very-short-period price is determined by demand alone. The short-period price is determined in equilibrium by the interaction of both short-period demand and short-period supply. The long-period price, according to Marshall, is determined by the supply conditions of the firm in the long period. So, if price is supposedly

determined by the interaction of demand and supply as most textbooks tell us, then the price explained in textbook theories is one of those that we never actually see. Yet we still judge the acceptability of a model of the firm or the consumer on the basis of whether it is capable of being used in the explanation of the stylized fact of a stable short-period equilibrium price. For such a capability, the demand curve must be downward-sloping and the supply curve must be upward-sloping at their intersection point. We can see that one stylized fact can easily lead to more stylized facts.

Now, obviously, we would never actually refer to downward-sloping demand curves or even long-run prices as stylized facts. The term 'stylized facts' seems to be reserved for less general facts but they are always the facts to be used to define the 'explicandum' – that which we wish to explain. What is the methodological motivation for stylized facts in mainstream neoclassical economics? In one sense no facts, stylized or ordinary, are directly observable. In the inductive sense – that is, without making assumptions in the process of making observations – all observations depend on the acceptance of auxiliary theories. Most economists today will recognize that facts are in this sense 'theory-laden'. This recognition is at the basis of the conventionalist criticism of naive inductivism. The auxiliary theories range from low-level conventionalist rules of acceptable econometric evidence and estimates, to high-level behavioral hypotheses such as the existence of general equilibrium values for given prices or the presumption of rational learning. Interestingly, models based on the so-called Rational Expectations Hypothesis involve both levels of auxiliary theories.[1] While the view that all facts are theory-laden is widely accepted today, we might still wish to ask an obvious question. If all facts are theory-laden, how do we choose what to explain? Is it just a matter of style?

The common recognition that all facts are theory-laden seems to imply that the situation is hopelessly arbitrary. However, the theorist who explains only theory-laden facts must still be putting forth a theory to explain those facts and not all theories will do. Moreover, when one puts forth a theory to explain, one is still claiming that at least one fundamental theory is true. The claimed truth of that fundamental theory should always be at stake in any explanation of an observable situation even when that situation is defined by auxiliary assumptions. The only methodological problem that might arise when purporting to explain stylized facts and the situation that they define is the potentiality of a circular argument. And thus, so long as the stylized facts to be explained by one's theory do not require the acceptance of the truth of the same theory that explains them, the recognition that some facts are a matter of style neither implies nor avoids important methodological problems.

CRITICIZING STYLIZED METHODOLOGY

Why do economics textbooks and some sophisticated mathematical models of the economy devote so much effort to explaining stylized facts rather than the facts we can see outside our window? It is because stylized facts are by design easier to explain, that is, more convenient. In this regard one is again reminded of the often-told story of the inebriated gentleman who late one evening lost his housekey far from the nearest streetlight and who spent all night looking under the streetlight because the light was better. Stable short-period equilibrium prices are stylized facts, and the time has come when we need to spend more time developing models and theories that can be used to search the dark disequilibrium world that exists between the equilibrium streetlights.[2]

Thinking back over the last twenty years of dealing with methodologists, one might argue that most of mainstream methodology is concerned with stylized methodology. Just what are the facts that methodologists are required to explain? Supposedly, we must explain why one theory is chosen over another. I ask, why? What problem is solved by choosing one theory over the other? While one can think of practical policy reasons to have to choose one theory – one can only apply one theory at a time – as I argued at the 1986 University of Manitoba workshop (see Chapter 3), one could easily carry a bag of theories. Some theories work in this case, other theories in other cases. We may not like this as a representation of the methodology-based stylized fact of theory choice, but there is no non-practical problem solved by theory choice. A possible exception might be the problem of writing economics textbooks. Surely, some will say, we have to choose one theory in order to write the textbook. I would disagree. Textbooks can just as easily be about the various available theories used to explain stylized or real facts of the economic world we wish to explain. I think Kaldor was right to focus on a set of facts that would put into focus differences between competing explanations. The same might be hoped for methodology. Are there questions that both conventionalist methodologists and Popperian critical-rationalist methodologists can agree must be explained? At this stage of my dispute with mainstream methodologists, it does not seem there are any questions of common interest.

NOTES

1 I have discussed this in Chapter 4 of my 1982 book.
2 This is a central theme of my 1986 book.

Part V

Popper and economic methodology

19 Understanding the Popperian legacy in economics

> I shall start with two general theses. My first thesis is this.
> (1) If anyone should think of scientific method as a way which leads to success in science, he will be disappointed. There is no royal road to success.
> My second thesis is this.
> (2) Should anybody think of scientific method ... as a way of justifying scientific results, he will also be disappointed. A scientific result cannot be justified. It can only be criticized, and tested.
>
> Karl Popper [1961/72, p. 265]

> Similarly, it is helpful to formulate the task of scientific method as the elimination of false theories (from the various theories tentatively proffered) rather than the attainment of established truths.
>
> Popper [1945/63, vol. 1, p. 285]

> From the point of scientific method, ... we can never rationally establish the truth of scientific laws; all we can do is to test them severely, and to eliminate the false ones (this is perhaps the crux of my *The Logic of Scientific Discovery*).
>
> Popper [1945/63, vol. 2, p. 363]

Warren Samuels asked me to review a book which was a collection of papers presented to a 1985 symposium of methodologists held in Amsterdam. The symposium was about what is claimed to be Popper's philosophy of science applied to economics and whether there is a possibility of a Popperian legacy in economics. The main results of this symposium are presented in *The Popperian Legacy in Economics*, edited by Neil de Marchi [1988b]. Despite my being the most published Popperian methodologist in economics, I was not invited to this conference. In retrospect, this is not surprising given that there was little in common between what I think are important methodological questions (such as those discussed in Chapters 13 to 18 above) and the stylized methodology that was so common even among those who thought they were talking about Popper. In this chapter I present an expanded version of my review of that book. Some of the papers in this book may be good illustrations of how methodology can be made uninteresting to mainstream economists.[1]

IS THERE A POPPERIAN LEGACY IN ECONOMICS?

In the minds of many, Karl Popper is the most important philosopher of science of the twentieth century. For some this is because they love his vision of science as a progressive and critical enterprise, while for others it is because they hate his rude dismissal of any traditional philosophy that would see science as a means of justifying beliefs. Those who love Popper's vision, and think economics should be considered scientific, often think there ought to be a Popperian legacy in economics. Unfortunately, until quite recently, in his many writings Popper was of little help to his economist fans.[2] In his infrequent references to economics, he treated economics so gingerly that he left considerable doubt about his views of economics or his view of the applicability of his philosophical concerns to the study of economics.

In his summary of the symposium, de Marchi concludes that there is no Popperian legacy [1988b, p. 12].[3] While it is easy to agree with his conclusion, I find the reasons provided in the symposium to be unsatisfactory. Is the absence of a Popperian legacy in economics due to (1) a fault of Popper, (2) the essential nature of economics or (3) a failure of proponents to understand or correctly apply Popper's views? Only one participant seemed to think the absence of a Popperian legacy is entirely due to Popper. The rest seemed to think it is due to one or another peculiarity of economics that distinguishes economics from scientific disciplines such as physics or chemistry. None of these participants were willing to admit that there may have been a failure on the part of economists to understand Popper's views or correctly apply them to economics.

Three of the prominent participants in this symposium have major Popperian credentials. First, there is Terence Hutchison, who is credited by almost everyone with being the first to introduce economists to Popper's views in 1938. Although he claims to have tempered his views since then, he still is the strongest advocate of an essential role in economics for Popper's falsifiability. In his paper, 'The case for falsification', Hutchison urges us not to abandon falsifiability as a primary operating rule in economics because economics is ultimately used to support politics and ethics. He argues that falsifiability is an essential tool against dishonesty. Second, there is the self-professed 'neo-Popperian' [1988b, p. 38] Mark Blaug, who is famous for promoting what the participants call 'falsi

ficationism' and for complaining that economists give only lip-service to falsifiability. Blaug's paper, 'John Hicks and the methodology of economics', critically examines the methodological views of Hicks and finds them incoherent. And finally there is Joop Klant, who is considered the leading proponent of Popper's views in economic methodology in the community of European economists.[4] Regrettably, most of the other participants fail to understand Popper's views and thus too often seem willing to throw the baby out with the philosophically dirty bath water.

CRITICISM OF POPPER'S VIEW OF SCIENCE

In one sense the critics of Popper's view of science are correct: Popper's view of science does not do a good job of solving the problems that these critics think must be solved. Most of his critics insist that any good philosopher of science must be able to provide criteria by which good theories can be distinguished from bad theories – that is, it must solve the problem of theory-choice. As I have noted several times in earlier chapters, very many methodologists in economics consider the primary concern of methodology to be that of determining conventional criteria to enable us to choose among competing theories much like consumers choose between apples and oranges. I have often criticized this view of methodology as well as the related view that is concerned with determining the attributes of scientific theories which allow them to be considered contributions to the growth of knowledge.[5] Of course, I think it is silly to criticize Popper for failing to solve problems that he obviously rejects. The root of the issue is the common view that anyone who discusses the philosophy of science must be promoting their form of a 'scientific method' and claiming that, if properly followed, their method will always produce scientifically acceptable results. For some people, the scientific method is needed in order to specify conventional criteria by which one would rationally choose the best theory from a list of competitors. For others it is a means of justifying or verifying the truth of one's prior beliefs.

As the above quotations from Popper's well-known work clearly show, he was not promoting or recommending a particular scientific method. Anyone reading Popper's work to find a recommended scientific method is doomed to disappointment. Typically, critics (and even some proponents) identify Popper with a normative view which says that true scientists should go out of their way to make their theoretical statements falsifiable. A superficial reading of Popper would seem to support this identification. Critics will then argue that many scientifically useful statements are not obviously falsifiable and very few scientific propositions are independently falsifiable (i.e. without depending on an assumption of other propositions being true and thereby begging more questions). They thus say it should be concluded that Popper's normative view of science is wrong.

Dan Hausman's contribution to the symposium, 'An appraisal of Popperian methodology', is just such a critique. From my perspective, such criticism seems to be an attempt to sculpture a representation of Popper from a piece of rotten wood. What Hausman's paper does, however, is to whittle the wood down to a square peg which he tries to cram into an analytical philosopher's round hole. Of course, square pegs do not fit into round holes – and Hausman wants us to think it is due to a flaw in the peg. I think it is the fault of the hole.

UNDERSTANDING POPPER'S VIEW OF SCIENCE

If readers of Popper's early work are more careful to observe the intended audience of his argument, they will find a much more cautious position. Specifically, the context must always be recognized in his arguments in favor of falsifiability. With regard to the importance of falsifiability, he saw himself arguing against the common view of the 1920s and 1930s that theories are scientific because they are verifiable. And Popper countered that falsifiability rather than verifiability would be a more appropriate means of demarcating science from non-science [e.g. 1957/65, p. 40]. But Popper is not claiming that falsifiability, as a static attribute of scientific theories, is a sufficient condition for anything. Obviously, many false propositions are falsifiable.

Since almost all of Popper's early discussion of science is concerned with disciplines such as chemistry and physics, there is no question of scientific status, but rather a question of just what makes chemistry or physics scientific. In his early work, he was merely claiming that verifiability, as a means of demarcation, is logically inadequate since every explanation requires universal propositions (such as 'all consumers are maximizers') which can never be verified (even when true). Such explanatorily essential propositions are, however, at least falsifiable – so, if one wants a means of demarcation, then logically one should require falsifiability rather than verifiability. In this context falsifiability is not obviously being promoted as a foundation for a normative scientific method. Besides, to the extent that every explanation involves universal propositions, falsifiability is assured. Unless one is defending verifiability as a means of demarcating scientific explanations, it is hard to imagine how one can fault Popper's view that falsifiability is an essential attribute of any scientific explanation.

FALSIFIABILITY IN ECONOMICS

To the extent that every economic theory, model or explanation involves assumptions in the form of universal propositions, Popper's views are obviously applicable in economics. So what are the alleged problems that arise when one claims that economic explanations should be falsifiable? During the Amsterdam symposium, the central problem often referred to was what Klant had elsewhere called the 'parametric paradox'. The alleged parametric paradox is not explicitly defined in de Marchi's book and Klant points out that it was introduced to criticize Samuelson's methodology. Together these considerations make it difficult to understand the reported discussion. In what follows I am conjecturing what the participants understood as Klant's concept of a parametric paradox (Blaug is explicit in his puzzlement and wonders why Klant would promote Popper given the paradox [1988b, p. 30]).

The parametric paradox seems to be an alleged conflict between the explanatory method of comparative statics and the common presumption that all testing requires constant parameters. While comparative static analysis requires that we change one of the exogenous variables and determine the effect on the endogenous variables (a common example is the calculation of multipliers in macroeconomics), Klant claims that 'If you assume parameters to be variables, you imply that your theory is not falsifiable' [p. 30]. If Klant's parametric paradox were a problem, then it would be a central obstacle to any fulfillment of the requirement of falsifiability in economics. The reason why variability of parameters is an issue is probably the recognition that on the one hand in the natural sciences, for all practical purposes, there are many constants (gravitational acceleration at a given height, absolute zero temperature, the speed of light) but on the other hand, as noted by Hicks, 'there are no such constants in economics' [1979, p. 39]. Judging by the reported discussion, it seems that many feel that Popper's view of falsifiability and testability is thus appropriate only when there are such natural constants.

Personally, I find the acceptance of Klant's claim – that the parametric paradox is a proof that modern economics is essentially unfalsifiable – to be astounding. The reason is that in my 1960s PhD thesis I *required* the assumption of the variability of both parameters and exogenous variables so that I could show what it takes to unambiguously refute some typical macroeconomic models [see Boland 1989, Chapters 2 and 3].

One of the reasons why many people think falsifiability is difficult to apply in economics is the claim (and possible observation) that few if any fundamental theories have ever been empirically refuted. And, presumably, these same people think refutation of fundamental theories in physics is an everyday occurrence. The main difficulty is that methodologists and historians of economics too often are concerned with grand theories rather than the everyday business of economics. The everyday business of economics is more involved with model building and, as is well known, a refutation of a model would seldom constitute a refutation of the theory represented (or presumed) by the refuted model.[6] In Mary Morgan's 'Finding a satisfactory empirical model' she reviews the history of econometrics with respect to whether econometricians have been concerned with refuting or even verifying fundamental economic theories. And she notes, 'econometricians have been primarily concerned with finding satisfactory empirical models, not with trying to prove fundamental theories true or untrue' [p. 199].

It is most important to recognize that, while the everyday business of economics is not concerned with refuting grand theories, particular modeling assumptions are refuted or rejected every day. Whenever a model builder finds that a linear model cannot fit the available data, that linear model is being rejected as refuted; that is, the linear *model* is considered in some sense false. Similarly, econometricians who reject ordinary least-squares in favor of generalized least-squares as a means of estimating a model's parameters do so because they have found models based on the former to be false in some important respect. Such considerations would lead me to conclude that a very modest form of falsification is quite commonplace in economics and certainly not inapplicable.

Even if it is accepted that there is a modest form of falsification employed on a regular basis in economics, this does not constitute evidence in favor of a Popperian legacy in economics. The practice of this modest form of falsification is more a consequence of economists accepting Paul Samuelson's methodological prescriptions. For example, in his PhD thesis, where early on he introduced his views of methodology, he says:

> An economist of very keen intuition would perhaps have suspected from the beginning that seemingly diverse fields – production economics, consumers' behavior, international trade, public finance, business cycles, income analysis – possess striking formal similarities, and that economy of effort would result from analyzing these common elements. ... [It] had not been pointed out to my knowledge that there exist formally identical *meaningful* theorems in these fields, each derived by an essentially analogous method. ... By a *meaningful theorem* I mean simply a hypothesis about empirical data which could conceivably be refuted, if only under ideal conditions. A meaningful theorem may be false. [1947/65, pp. 3–4]

I argued years ago that, more importantly, the primary reason for requiring falsifiability is to assure that any verified theories will not be confused with

tautologies.[7] The avoidance of tautologies was one of the main objectives of Hutchison's original promotion of falsifiability in his 1938 book. As I noted in Chapter 5, this idea of promoting falsifiability in opposition to tautologies was essentially the focus of the critical argument developed by Klappholz and Agassi in their well-known debate with Hutchison.

ATTEMPTS TO CREATE A POPPERIAN LEGACY

In a very interesting paper, 'Popper and the LSE economists', Neil de Marchi recounts the history of a group of well-known economists who in the late 1950s and early 1960s explicitly attempted to use Popper's view of science in economics. The group most notably contained Dick Lipsey and Chris Archibald. Their hidden agenda was to push economics beyond the dominating methodological views of the alleged arch-apriorist Lionel Robbins, who opposed quantification. As I noted in Chapter 9, it did not take them long to declare failure in their Popperian research program.

De Marchi's paper solved an old puzzle for me. Both Lipsey and Archibald have a reputation for being what some people might call methodologists. Yet I found it puzzling that:

(a) I met Lipsey many years ago and he told me that he learned everything he knew from my teacher Joseph Agassi. I ran to the library to look up Lipsey's famous book to see what he had to say about methodology. I was very disappointed.

(b) In 1967, or thereabout, I had a long conversation with Archibald. He tried in vain to convince me to switch my interests from methodology to something – anything – else.

Given their reputations, how was it possible for them to be so far divorced from my understanding? Nevertheless, I credit Archibald with providing me with a very important viewpoint. In our three-hour conversation he stressed that if I was going to study or promote the study of methodology, it was my obligation to always show that the methodology I wished to discuss matters to economists. I always try to apply this both as a consumer of methodology and as a methodologist. As a consumer I always ask: what have I learned that matters? As a methodologist I always assume my audience is poised to ask whether methodology can matter.

According to de Marchi, Archibald tried to apply Popper's views to some fundamental theoretical questions but eventually decided that the variability and/or ambiguity of 'parameters' in comparative static explanations implies that refutations are logically impossible. And Lipsey was more concerned with emphasizing the role of quantitative testing of economic theories but eventually decided that all testing must be based on statistical models and, according to de Marchi, this led to the conclusion that economic theories are irrefutable. In both cases, I think a more accurate conclusion might be that neither Archibald nor Lipsey understood Popper's views very well.

This reaction to the problem of testing grand theories with specific models is, of course, an instance of the well-known Duhem–Quine thesis.[8] Virtually everyone thinks it means that testing of grand theories is impossible [e.g. 1988b, p. 20]. I think this is a mistaken conclusion about testing in economics. Specific general statements can be tested in economics. As Klant points out, even without absolute refutations testing can necessitate adjustments in our general theories [1988b, p. 104]. If one carefully defines the test criteria, it is sometimes possible to test grand theories with specific models subject only to an agreement concerning ordinary test criteria.[9]

Almost everything presented at the Amsterdam symposium misses the point of Popper's approach to the philosophy of science. In de Marchi's introduction he acknowledges that many of us think that the importance of Popper's work is not that it sees science as an enterprise devoted to the growth of knowledge but that it sees science primarily as an instance of learning by criticism [1988b, p. 7]. As I will explain in the next chapter, I think Popper was interested in science as an ongoing human activity, a process, which is based primarily on a critical attitude. He was not interested in science as a static method of justification or as a formula for success. Unfortunately, hardly anyone pursued this critical-learning aspect of Popper's work.

In my opinion, the role of falsification in the growth of knowledge is promoted by Popper more to emphasize that *science is a process* than to argue that it embodies a method that assures progress. By his noting that anyone's claimed advance represents more a refutation than anything else, Popper's argument was always against those who think science progresses in a positive, verificationist manner. His idea of progress is more like Socratic learning – that is, one according to which we always learn by exposing our ignorance (i.e. false theories and beliefs). But, most important, he continually noted that the absence of a scientific method (one which would guarantee success) is not a problem since science is an ongoing process which is always going in the right direction.

The idea of emphasizing process and direction sounds to me like Austrian economics. It is easy to see a similar sentiment in Hayek's early emphasis on the market-based price system as an ongoing process where (in the absence of external influence) there is always movement in the right direction (namely, toward an equilibrium where resources are optimally

used and everyone is maximizing). Moreover, the competitive price system is best understood as a commendable process even though it may not always reach an equilibrium. Popper similarly wished us to recognize that it is not a guarantee of the successful attainment of true theories which motivates scientists but that refuting ignorance is always a movement in the right direction.

Market-oriented economists will often observe that by bidding up the price when there is excess demand, demanders always give the right signal and incentive to producers. As a process, the market forms a basis for social coordination that is always moving in the right direction (toward universal maximization). Popper similarly noted that by putting forth falsifiable explanations, scientists are in a position to improve our knowledge by refuting our ignorance. As a mere practical matter it is easy to see that the more falsifiable our explanations, the better will be our opportunity to learn. For Popper, science as an ongoing social enterprise must be based on falsifiable theories since it is devoted to eliminating ignorance even though complete elimination may never be achieved.

THE RHETORIC OF POPPER'S VIEW OF SCIENCE

As argued above, practicing economists and econometricians refute particular modeling assumptions every day even though they may wish to be modest and say only that the assumptions are rejected as not being 'satisfactory' [e.g. 1988b, pp. 204–8]. Of course, such modesty is merely rhetorical. Moreover, when practicing economists do talk of falsifiability, they are almost always following Paul Samuelson's lead. Rather than a symposium on a Popperian legacy in economics, I think there should be more discussion of the methodological legacy of Samuelson.

It is surprising that, with all their talk of the rhetoric of economics, Donald McCloskey and Arjo Klamer failed to examine the rhetoric involved in the typical discussion of Popper's view of science in economics or even of philosophy itself. Neither seemed to practice what they preached! Although McCloskey's paper, 'Thick and thin methodologies in the history of economic thought', did offer criticisms of Popper's view of science, nowhere does he seem to appreciate the rhetorical aspect of Popper's writings. Specifically, Popper was always willing to put his discussion in the terms of his intended audience (as noted in Chapter 11), and thus one must be very careful to separate Popper's views from those he was debating. McCloskey does engage in a little rhetorical inquiry by accusing the philosophy of science of being 'too thin'. One lesson that I think can be learned from McCloskey's general discussion on the rhetoric of the history of economics and of methodology is that we should not take philosophers of science as seriously as they take themselves. Unfortunately, in response to McCloskey's and Klamer's continued promotion of the rhetoric of inquiry, some of the participants eventually complained that the discussion of the rhetoric of economics was itself wearing thin.

Perhaps McCloskey and Klamer could have devoted some of their time to an inquiry into the rhetoric of the symposium. For example, it might have been possible for the participants to spend some of the symposium's time discussing Popper's 'critical rationalism' [1945/63, Chapter 24], or his logical 'negativism' [1957/65, p. 228], with respect to science as a critical process. Instead, the participating economists and methodologists seemed to be victims of the rhetoric of Latakos, who emphasized the *growth* of knowledge; thus, they spent too much time on questions of whether neoclassical economics is an 'empirically *progressive*' research program [1988b, p. 247]. Unfortunately this seemed to be a matter of design since the symposium was almost exclusively limited to the discussion of falsifiability and its relationship to the question of the growth of knowledge [1988b, pp. x, 2, 6–7]. Such a limitation allowed only a thin slice of Popper's view of science to be discussed. The thinness of the slice served up by this symposium is distressing to many of us interested in Popper's more general views of science and learning. And silly criticisms of the chosen thin slice of Popper's work seemed to be distressing for some proponents of falsifiability in economics such as Hutchison:

> What alarms me is that we are not building on the advances of the 1930s. In some respects, we are going back to the 1930s. The barbarians really *were* at the gates then, and in some ways they still are. [1988b, p. 25]

Judging by the thinness of the discussion of Popper's work in this symposium and the exclusive concern for thin questions such as whether falsifiability should be a guiding rule in economics, I think Hutchison should look around him. The barbarians are no longer at the gates – now they are within the gates.

NOTES

1 The remainder of this chapter is based on my review article 'Understanding the Popperian Legacy in Economics', *Research in the History of Economic Thought and Methodology*, *7*, 1990-92, and is used with the permission of the JAI Press.

2 A 1963 lecture by Popper given to economists has recently been published. For a review of this lecture, see Hands 1996.

3 All references to 1988b in this chapter are to de Marchi 1988b.

4 The symposium was held in honor of Klant's retirement from the Chair of History and Philosophy of Economics at the University of Amsterdam.

5 See Boland 1982, Chapter 10, and 1989, Chapters 4 and 5, as well as Chapters 3, 8 and 12 above.

6 See Boland 1989, Chapter 7; Cross 1982.

7 See Boland 1977b.

8 See Cross 1982 as well as Chapter 5 above.

9 See further Boland 1989, Chapter 8.

20 Scientific thinking without scientific method: two views of Popper

> Popper almost alone, and alone in our century, has claimed that criticism belongs not to the *hors d'oeuvre*, but to the main dish.
>
> Joseph Agassi [1968, p. 317]

> The importance lent to the falsifiability criterion and the demarcation problem by Popper and others distorts his thought.
>
> William Bartley [1968, p. 43]

> The idea that science can and should be run according to some fixed rules, and that its rationality consists in agreement with such rules, is both unrealistic and vicious. It is unrealistic, since it takes too simple a view of the talents of men and of the circumstances which encourage, or cause, their development. And it is vicious, since the attempt to enforce the rules will undoubtedly erect barriers to what men might have been, and will reduce our humanity by increasing our professional qualifications.
>
> Paul Feyerabend [1970, p. 91]

The 1980s saw a growing interest in Karl Popper's view of science among economists. This began with Mark Blaug's popular methodology book [1980] that espoused the 'falsificationism' that he attributed to Popper. As I explained in Chapter 11, Blaug's Popper was unrecognizable to me. As it turns out, there are two views of scientific thinking attributed to Karl Popper. The more popular among economic methodologists is not very challenging and to be useful requires only a minor adjustment to commonly held views. The less well-known view considers Popper's theory of science to be revolutionary and extremely challenging and requiring a major change in attitude toward science and scientific thinking. In this chapter I will explain the nature of these two views and their implications for the study of economic methodology. Also I will examine why there are two different views and why one is more popular than the other. Above all, I will try to explain why I think the less popular is the more important.

THE POPULAR POPPER

While Terence Hutchison may have introduced Popper's view of science to economists about sixty years ago by promoting the testability of scientific theory, in economics today the popular view of Popper's philosophy of science is more than likely due to the success of Blaug's 1980 book.[1] According to this view, scientific thinking is distinguishable from non-scientific thinking by merely noting that scientific theories are falsifiable and non-scientific theories are not. Popper's view is explicitly distinguished from a competing earlier view whereby scientific theories were distinguished from metaphysics using a criterion of empirical verifiability. Where scientific theories were claimed to be empirically verifiable and thus meaningful, metaphysics was alleged to be non-verifiable and thus not meaningful. In the 1930s Popper explained the earlier view by claiming that the old distinction was designed to solve a problem of demarcating science from metaphysics. Popper then argued that the earlier view's solution is inadequate. Since scientific theories are explanations, Popper argued that, for reasons of quantificational logic, if science is to be characterized as empirical knowledge, verifiability cannot be used to identify scientific theories. Specifically, as I have noted before, every explanation involves assumptions of a strictly universal nature (e.g. 'All swans are white'), and strictly universal statements can never be verified with empirical observations. However, such statements can be refuted by observation (e.g. 'Today, I saw a black swan in the zoo'). Popper offered his alternative solution, namely, that to the extent to which any science is empirical, its distinguishing characteristic must be its empirical falsifiability. Using Popper's view, the history of science can be seen not as an accumulation of verified theories (since they are impossible) but as the evolution and vicissitudes resulting from the empirical overthrow of false theories. Scientific knowledge is then considered merely a residue of failed attempts to refute, or, more specifically, a collection of falsifiable but as yet unrefuted conjectural theories.

The practice of falsificationism

On the strength of his many observations about empirical falsifiability, many writers, both critics and friends, have saddled Popper with a 'Popperian methodology' that he is presumed to be prescribing for practicing scientists. Usually, it says that scientists should (1) consider only falsifiable explanations, (2) limit scientific activity to trying to falsify existing explanations and (3) accept those explanations that have been tested but have so far not been falsified. Some argue that this so-called methodology is not necessarily a prescription, it is better considered a hopeful description of scientific practice.

Armed with the criterion of falsifiability, Popperian methodologists are thought to be engaged in an ongoing process of appraising past and present economic thinking using this Popperian methodology as the standard. For example, a minimum condition for any theory to be considered a possible contribution to scientific knowledge is that it be empirically falsifiable. Thus, this type of Popperian methodologist is always on guard to root out and prosecute anyone who does not display a concern for falsifiability. Of particular concern are both *ad hoc* adjustments that attempt to overcome refutations and 'immunizing stratagems' designed to protect favorite theories from premature refutation.

Surely, anyone who thinks methodology must be prescriptive will not be satisfied with the nihilism and negativity that is being attributed to Popper. From the perspective of economic methodology, falsifiability by itself is no more challenging than Paul Samuelson's version of operationalism (recall that for an economic proposition to be meaningful Samuelson requires only that it be 'refutable in principle'). Furthermore, according to some observers, if methodologists can tell the scientist what not to do, should they not also be able to give some positive advice? Surely, it is easy to think that those who actively engage in refuting one theory are doing so only because they have a better alternative theory in mind. All that we need are criteria to allow us to make a rational choice between competing theories.

According to the popular view of Popper, falsifiability is nothing more than one of the many criteria used to choose the best among competing explanations. Perhaps, as some say, it is the best criterion. In this sense, it would appear that Popper was offering advice to choose the most falsifiable: try to test it, reject it if it fails the test and then move on to test the next most testable theory. In this way one could see the history of science or economics as a sequence of conjectured theories offered as explanations of observed phenomena but which when empirically rejected are replaced by other conjectures. The only question here is whether the popular Popperian view of scientific method captures the widely believed view of the history of science, namely the stylized fact which says that since the time of Isaac Newton there has been a stable and continuous accumulation of scientific knowledge.

Falsificationism and the history of science

It was not obvious that Popper's so-called falsificationism could ever provide an adequate explanation for the stylized fact of the stability of science until Imre Lakatos came to the rescue. Lakatos presented a version

of falsificationism that substitutes what he called scientific research programs for singular theories. Lakatos explained why there can be continuity and stability while at the same time recognizing that the business of science involves conjectures, testing and refutations. History according to this view is a sequence of theories and models designed to carry out the research program. While the research program may change very slowly, there can be numerous conjectures and refutations of specific models and theories. The task for any historian of thought would seem to be the identification of those aspects of a program that do not change and those that do. Apart from identifying which programs rely on immunizing stratagems and which do not, falsification does not seem to play a big role in the explanation of any research program. While historians of science and economic thought have found the notion of a research program useful – since it may give them something to do while analyzing and modeling various research programs – there does not seem to be much for a so-called falsificationist methodologist to do.

THE SOCRATIC POPPER

There is a very different view of Popper's theory of science that is not well known in economics. In this alternative view, falsifiability plays a very minor role. Moreover, this view does not take for granted that the history of science is one of stability and progressive accumulation. Popper's theory of science emphasizes that science is embodied in a process which is not at all choice or endorsement but rather criticism or rejection. Theories are rejected because they do not meet available criticism – for example, the criticism may include empirical data that are thought to conflict with the theories. Where many traditional philosophers prior to Popper equated science with rationality and rational choice, Popper emphasized the critical role of rationality. Briefly stated, science for Popper is a special case of Socratic dialogue, namely one where we learn with the elimination of error in response to empirical criticism. Rationality is critical debate – with the emphasis on debate. Popper sometimes called this 'critical rationalism'. Given its emphasis on Socratic dialectics, I will call this the Socratic-Popper view.

Problem orientation and situational analysis

In his early work Popper openly employed a problem orientation, as is evident in his promotion of two problems which he called the Problem of Demarcation and the Problem of Induction. Popper both offered and recommended a problem orientation to facilitate the emphasis on criticism.

It is important to recognize that the problems he identified are tools which he manufactured to explain past events or theories. One must be careful when reading Popper not to confuse the message with the medium.

Followers of the Socratic-Popper view stress the centrality of problems. Specifically, to understand any economist we have to know his or her problems. Consider for example one of the favorite topics of historians of economic thought, namely the question of whether some particular idea is novel. It is not enough to indicate that the idea is or was new but, according to the Socratic-Popper view, one would want to show that it is a solution to some problem. But the new idea may not have been introduced to solve the conjectured problem literally. That is, the problem orientation is a heuristic. Every invention of an idea can be seen *post hoc* to solve a problem or answer a question. In other words, there may not be an answer for every question but there is a question for every answer and, similarly, there may not be a solution for every problem but there is a problem for every solution.

While problem orientation is central to Popper's view of science, it is also important to recognize how it is based on his view of rationality. When examining the contribution of an economic thinker, problem orientation always involves presuming that the thinker was implicitly or explicitly trying to solve a problem: achieving his or her aims by overcoming or dealing with all relevant obstacles. This orientation, sometimes called situational analysis, is second nature to every neoclassical economist. Consider the textbook consumer. A neoclassical economist sees the logic of the situation for a consumer to be one where the aim is utility maximization but the consumer faces the constraint of a limited budget as defined by available income and existing prices. The only difference for the followers of the Socratic-Popper view is this: they would say that the economist sees the consumer attempting to solve a choice problem. But it is important to keep in mind that problem orientation is always retrospective. The consumer has already made a choice and the economist *post hoc* tries to explain how the choice was made. For example, the consumer is thought to be facing a limited budget and psychologically given preferences. The budget defines what can be afforded and preferences enable the consumer to compare any two alternative decisions, and specifically to determine which is better. When the consumer is deciding how much of two goods, say *A* and *B*, to buy with that budget, he or she is thought to consider every possible bundle of quantities (where a bundle consists of a pair of quantities, one for each good). If the consumer chooses to spend the entire budget, certain tradeoffs must be made. It is thought that the consumer, having tentatively picked one affordable bundle, considers a second bundle which has one less unit of *A* and then compares how much more *B* could be purchased and whether the additional amount of *B* leaves the consumer better off or not. If the

second bundle is better, the consumer is presumed to switch to that second bundle and then to use it as a basis of the next comparison – one might see this as a trial-and-error elimination process. The consumer is thus thought to have solved the choice problem by determining which of many possible affordable bundles is better than any other affordable bundle. The economist's explanation thus explains why the consumer chose the bundle in question and why all other bundles were not chosen (i.e. all other bundles were either inferior according to the preferences or not affordable according to the limited budget, or both). Presented this way, it should be easy for everyone to understand Popper's problem orientation.

Practicing the Socratic-Popper view

As more and more writers in economics have begun to note, the essence of Popper's view of science is a matter of embracing a 'critical attitude'. While this is true, it somewhat misses the main point. The main point is that, as Socratic dialogue and critical debate, science is based on non-justificationist rationalism. Some writers think Popper was saying merely that it is impossible to justify one's beliefs. If their view were true, it would be saying that Popper was merely offering us his form of skepticism. The reason usually given for this interpretation of Popper is that he said that he had an unambiguously negative view of what can be called the problem of justification (i.e. the problem of providing a justification for any knowledge claim). What Popper was most negative about is the *necessity* to solve this problem.[2]

The practice of a Popperian methodologist who follows the notion that science is Socratic debate will differ considerably from the activities of those methodologists who see themselves as Popperian falsificationists. Methodologists who follow the Socratic-Popper view will devote most of their time to fostering and encouraging criticism. Problem orientation is the most popular approach. Using situational analysis, they will provide explanations of existing critiques, usually by identifying a problem for which existing solutions are inadequate or are in dispute. If there is any appraisal activity, it will be limited to the effectiveness of existing lines of criticism. The Socratic-Popper view is, of course, the inspiration for the essays in Parts III and IV above as well as my 1979 *JEL* article (see Chapter 2) and my 1981 *AER* article (see Chapter 6).

Learning and Socratic dialectics

The Socratic aspects of Popper's view are most evident in his claim that people learn from their errors. In Popper's terms, this is not only a process of trial-and-error, but a process motivated by rational criticism and not by the pursuit of a rational justification. Non-justificational rationalism says that the rationality of a debate or an argument does not guarantee its truth status. More important, the combination of trial-and-error with the absence of guarantees means that science is inherently unstable.

To say that science is Socratic dialectics begs an explanation of the nature of Socratic dialectics, at least with reference to learning. My view is that Plato's early dialogue *Euthyphro* is a perfect case study. Recall that in this dialogue the situation is that Socrates is on his way to his famous trial for impiety when he encounters Euthyphro who is on his way to a trial where he is prosecuting his own father for impiety. As I see this dialogue, Socrates is attempting to deal with a problem: he does not understand why he is being prosecuted for impiety since by his understanding of piety he has committed no crime – Socrates' understanding may be erroneous but Socrates cannot find the error. Now, Euthyphro is obviously an expert on matters involving piety and impiety – if for no other reason, only an expert would prosecute his own father. So, in this dialogue, Socrates is the student trying to learn from Euthyphro the expert. The dialogue proceeds by Socrates presenting his understanding of piety and impiety and inviting Euthyphro to point out where Socrates is in error – after all, if Socrates' understanding were correct he would not be seen to be guilty. Socrates wishes to learn where he is in error and thus lays out his understanding, step by step. Unfortunately, at each step Euthyphro agrees with Socrates – consequently, if there is an error in Socrates' understanding, Euthyphro failed to find it. At the end, Socrates invites Euthyphro to restart at the beginning but Euthyphro declines. Thus, while there was the perfect opportunity to learn – discovering one's error – Socrates failed to learn anything. For my purposes, Plato's *Euthyphro* illustrates all of the major ingredients of Popper's theory of learning: trying to learn by discovering error, inviting criticism in order to learn, putting one's own knowledge at the maximum risk in doing so, and demonstrating the absence of guarantees. Of course, it is important to emphasize that the person who wishes to learn asks the questions.

My interpretation of this dialogue is not universally accepted. I have been publicly criticized for allegedly not correctly realizing that Socrates is the teacher and Euthyphro is the student and thus this dialogue cannot illustrate what I claim is Popper's theory of learning – discovering the errors in one's knowledge. My critics say that it is obvious that Socrates is trying to show Euthyphro the shortcomings of Euthyphro's assumed knowledge of what is pious and impious. My critics say that Socrates leads Euthyphro into a circular argument to convince Euthyphro that his understanding of piety and impiety is inadequate. But Socrates fails and

thus Euthyphro does not learn. That there is a failure in learning here we all agree. But my critics claim that the evidence that learning did not take place is that Euthyphro did not see that his knowledge was in error. But, as can be plainly seen, my critics invoke Popper's theory of learning in order to claim that Euthyphro did not learn! So whichever way one interprets this dialogue, it would appear that it does illustrate that one learns by discovering one's errors and one fails to learn when errors are not uncovered. And either way, it illustrates the absence of a guaranteed outcome. What my interpretation captures but my critics' does not is why Socrates would go to the trouble of asking questions of Euthyphro in the first place.[3] The motivation is that Socrates recognizes that his problem is one that Euthyphro might be able to solve. In other words, Socrates wishes to learn and that is why he asks the questions. By either interpretation, Plato's *Euthyphro* provides a good metaphor to help understand Popper's view of the process of science; namely, science is critical theory without a method that can guarantee a desired outcome.

Science in flux

Apart from the recognition that even though Socrates follows his usual method of learning, success was not assured, the *Euthyphro* dialogue may not be the best way to bring out the revolutionary aspects of Popper's view of science. Another way to appreciate why Popper's view is revolutionary would be to consider the difference between how the relationship between rationality and science was viewed before and after Albert Einstein.

Looking as far back as the eighteenth-century one can find people who commonly believed that if science is rational then it is stable. Rationality provides universality and universality provides stability. The key point here is that a minimum requirement for an argument to be rational is that *everyone* who accepts the truth of its premises must by both the force and definition of logic accept the truth of all validly inferred conclusions from those premises. As I explained in Chapters 8 and 13, universality is provided by the fact that this is true for *everyone* who accepts the assumptions. When we also realize that people once thought that rational proof included infallible inductive proof, that is, proof based only on undisputed observations, there would be very little room for disagreement and hence for instability. Today, the task of the philosopher or historian of science is more often thought to involve explaining the success of science and thus there is even less room to see instability in science.

Throughout the nineteenth century, the most obvious evidence in favor of this equation between a rational science and a stable science was Newton's mechanics. But at the beginning of the twentieth century, Einstein's theories challenged the adequacy of both Newton's theories and inductivist scientific method and openly demonstrated that science is fallible. That is, the success of science is not necessarily the result of an infallible scientific method. Moreover, recognition of a fallible science meant that a rational science cannot assure a stable intellectual foundation on which so much of Western culture depends. In this regard, then, Popper's view is revolutionary since it is probably the first to deal with the post-Einsteinian reality of science. According to the Socratic-Popper view, science should be seen to be a process which is potentially in a state of constant flux rather than one which establishes incorrigible stable truths. There are no infallible methods, no authorities and no unquestionable facts. Science is scientific thinking without scientific method.

POPPER'S SEMINAR AND THE HIJACKER

During the 1950s Popper generated a group of self-declared disciples by means of his 'Tuesday Afternoon Seminar'. Popper-style seminars are notorious. There is much criticism, tension and above all constant interruptions. Nothing is to be protected from criticism. The rule seems to be, as noted by J.O. Wisdom, 'Thou shall not speak while I am interrupting!'. Students and participants who can handle all the tension, as well as the shameless disregard for the traditional rules, will usually find such seminars very stimulating and productive.

Since Popper-style seminars are almost exclusively concerned with learning and criticism, participants are warned at the outset to 'leave their toes outside the door'. That is, participants should not take criticism personally because if they do they limit their own opportunities to learn. Even when this warning is heeded, Popper-style seminars often run into difficulties. Students unfamiliar with the medium will often start looking for the rules and methods required to conduct a successful seminar and tension begins as soon as it is pointed out that there are no such rules or methods other than 'everything is open to question'. Interestingly, such difficulties are virtually the same ones which Popper faced in his struggles with the entire philosophy profession – which for most of the nineteenth century had been built on the presumption of a reliable method that would guarantee success.

Some of the early disciples of Popper and his seminar were Wisdom and John Watkins. Joseph Agassi joined the group at the beginning of 1953 when Paul Feyerabend was about to be Popper's assistant. When Feyerabend left for Vienna, Agassi became Popper's assistant. Assistants often were put in charge of constructing indexes for Popper's books. Ian Jarvie attended the seminar as an undergraduate. William Bartley joined

Agassi in the seminar and somewhat later Agassi brought Imre Lakatos. With the exception of Lakatos, all of them were Popper's devoted disciples, particularly with regard to Popper's constant complaints that he had not received the recognition he was due in the philosophy profession.

The disciples were united in their appreciation for what I am calling the Socratic version of Popper's philosophy of science. Criticism and problem orientation are essential to learning and understanding. Some of the disciples thought they understood this well enough to put Popper's views into practice – they even ventured criticism of Popper's views. Their efforts in this regard have led to much acrimony, sometimes at the level of soap-opera.

The all-consuming situation in the early 1960s was that, while there was a rapidly growing interest in the philosophy and history of science, the name most often mentioned was not Popper's but that of Thomas Kuhn. Everyone in almost every discipline seemed to be discussing Kuhn's 'paradigms'. Some of the disciples claim that Lakatos took advantage of the situation and, in effect, hijacked Popper's seminar. Supposedly, Lakatos convinced Popper that the desired recognition could be obtained by recasting Popper's views in a form closer to Kuhn's. Thus Lakatos and Popper made much more of the growth-of-knowledge implications of Popper's view and much, much less of the Socratic-dialectical aspects which the disciples advocated.

POPPER'S DISCIPLES VS POPPER AND THE HIJACKER

While some may wish to argue about which version of Popper's philosophy of science is the 'true Popper', I think it is more important to recognize that there is more than one view. But why are there two views? What are the sources of the arguments or disagreements? Is Popper at fault or his followers?

Admittedly, Popper's recommended method of criticism can itself be a source of disputes. When criticizing a writer's views, Popper insisted on a problem orientation whereby the critic must present the writer's problem and solution but only after making every effort to present the writer's views in the most sympathetic light. That is, the critic must make all unchallengeable improvements that can be made before launching the criticism. One would not wish to distract the debate into irrelevant side issues. In effect, the criticism must be conducted in terms that the writer can accept. This sympathetic problem orientation very often led Popper to lean backwards to grant as much as possible to the criticized writer and this in turn continues to lead readers to miss the rhetoric and thus to misunderstand Popper's own views.

Popper's Tuesday Afternoon Seminar itself is probably the major source of disagreements. In the early 1960s some of its participants, such as Agassi and Bartley, began publishing criticisms of Popper. The complaints from Agassi and Bartley seem to be based on apparent inconsistencies between what they thought Popper preached or practiced in this seminar and what he said in his writings. Those of us who never attended the famous seminar are left only with the views Popper expressed in his writings. And if one is not aware of his sympathetic problem orientation, it is all too easy to see inconsistencies where they do not exist.

Popper's writings do not seem to stress the importance of criticism nearly as much as his disciples claim his seminar did. The participants in the seminar equate Popper's view of science with what I have called the Socratic-Popper view. It is not surprising then that when Lakatos developed what he called the 'methodology of scientific research programs' as his version of Popper's view of science, the other members of the seminar were very critical. Bartley claimed that Lakatos added nothing of importance to the philosophy of science other than a few catchy phrases. Agassi claims that Lakatos did not know enough about the philosophy of science to make his pronouncements worthwhile. While almost everyone says that Lakatos made significant contributions to the philosophy of mathematics, the disciples routinely claim that Lakatos did not understand Popper and that the 'methodology of scientific research programs' of Lakatos does not represent the views of Popper. Moreover, they say, Lakatos misled Popper into a pursuit of fame at the expense of integrity, that is, at the expense of throwing the Socratic baby out with the inconveniently unmarketable dirty bath water.

THE POPULAR POPPER VS THE IMPORTANT POPPER

The major question to consider is why so much is known about the Lakatos version of Popper's philosophy of science and so little about the Socratic-Popper view promoted by Popper's disciples. An obvious reason is that the popularly accepted version of Popper's view allows one to see Popper as a philosopher making only minor improvements in the ordinary view of science. The ordinary view is that science is a stable enterprise and its stability is based on the avoidance of irresolvable questions such as those concerned with the absolute truth of scientific theories. After all, scientific theories cannot be proven true but only false. But Popper warned that the ordinary view allows any refutation to be avoided by refusing to accept the refuting evidence. Popper's disciples label the ordinary view 'conventionalism'. This is the same conventionalism that I have often referred to in earlier chapters. That is, according to conventionalism, theories are not to

be considered true in an absolute sense but only in a sense whereby a theory is 'true' as defined by the conventional notions of truth. Typically, a probability calculus is substituted for an absolute notion of truth status. According to the ordinary view, it is rational to accept a theory with a high probability of being true (given currently available empirical data) and to reject any theory with a lower probability. The issue thus is not one of truth status but one of *rational* acceptance by a community of scientists.

Rationality and conventionalism

There is much more to the ordinary view than its foundation of conventionalism. While the notion of rationality underlying conventionalism presumes science is rational, the presumption of rationality implies that any belief in a scientific theory can be proven (i.e. justified) – at least to the point of demonstrating its logical consistency with conventional acceptance criteria. This is definitely not the non-justificationalist notion of rationality presumed in the Socratic version of Popper's view. But there is even more to conventionalism. A notion that is alleged to be essential is that in science one strives to be able to choose the best theory from competing theories. Moreover, it is presumed that the criteria used in science are the best criteria.

While it may be difficult for followers of the popular Popper to see why anyone might strongly object to the commonplace notions of conventionalism, there are obvious reasons for why the followers of the Socratic-Popper view strongly reject conventionalism. It would be difficult to see how Socratic dialectics could be seriously pursued whenever it is allowed that one can always defend one's position by claiming that one's theories are not to be considered absolutely true but only the best available. The conventionalist defense that relies on the substitution of 'best' for 'absolutely true' seems to beg many questions. The most obvious question is whether the criteria that define 'best' are themselves really the best – such a question leads to an infinite regress, of course. Given the inherent possibility of avoiding contradictions with facts by denying the intended truth status and the impossibility of avoiding an infinite regress whenever rational acceptance is considered a substitute for truth status, how could one ever engage in a Socratic dialogue?

Conventionalism and the stability of science

According to the ordinary view of science, the everyday business of a methodologist seems to be either confined to a linguistic analysis of what economists say in their explanations or limited to a historical description of how particular economists reached their conclusions. Of course, there has always been room for a methodologist to make grand claims. Today, however, it would seem that moderation in methodology is much more common. Moderation may be the consequence of a certain complacency which also exists today. In terms of the alleged stability of science, there is an obvious consistency between stability and the presumptions of the ordinary view. Specifically, if everyone practiced conventionalism, the chances of a 'science in flux' would be very unlikely. It is very difficult to push on something so soft and forgiving. According to the ordinary view, Einstein's views can easily be seen as mere adjustments, such that Newton's views are viewed as a special case. In economics, Keynes' view need not be considered revolutionary but merely a special case of general equilibrium analysis. Of course, in economics there continues to be a problem of providing the micro-foundations of Keynesian macro-economics which would prove that Keynes was not a revolutionary.

Interestingly, it is Kuhn's conception of a paradigm that seems to capture the essence of the ordinary view of science. But Kuhn goes further to say that what makes science scientific is that the scientific community is made up of scientists imbued with a scientific mentality![4] I am not sure Kuhn's elaborated psychologistic view, if widely known, would be widely accepted. Nevertheless, the ordinary view does see science and scientific knowledge as an entity on a historical continuum. Revolutions are rare and ordinary science is more a question of day-to-day puzzle solving. It is difficult to see how we could have the current textbook-based education system without Kuhn's view being correct. It is exactly the textbook-based education system that presents an overwhelming obstacle to the appreciation of the Socratic version of Popper's view of science that the disciples promote.

Understanding the Socratic-Popper view

Followers of the ordinary view include some of those methodologists who have been promoting pluralism and whom I discussed in Chapter 12. It probably also includes some of the newly converted Popperian methodologists. All of these methodologists have considerable difficulty in understanding the disciples' alternative view of Popper. This difficulty needs to be explained and understood. The situation is very complex. As can be seen above, there are differences concerning theories of rationality, the history of science, the necessity of a scientific method, the nature of dialectics and, above all, the presumption that all true knowledge can be justified.

The presumption taken for granted by all followers of the ordinary view says that we must justify our knowledge before we can claim to know

anything. There is widespread fear that without a method which will assure that only true knowledge claims will be justified, we would have to give knowledge claims of mysticism, fundamentalist religions, and similar 'unscientific' disciplines an equal status with science. There is nothing in the Socratic version of Popper that would overcome this fear. But more important, the disciples claim that this fear could never be overcome. Proponents of the popular falsificationist Popper, however, think the requirement of falsifiability is a sufficient prophylactic. It may be sufficient; but the disciples claim that it too often rules out potentially scientific notions that happen not to be, at the time, in a form that is easily falsifiable. And besides, some aspects of science such as metaphysics may not be falsifiable but they are essential. In one sense, every theory that is designed to explain observable events is an application of a particular metaphysics. After all, one cannot explain everything at once. Something must be assumed. The obvious example is the one I discussed in Chapter 6, namely, in neoclassical economics, every theory or model will assume that the decision maker is an optimizer even though it is virtually impossible to refute this assumption. As I noted, this is simply because the neoclassical decision maker is presumed to maximize *something*. Since the 'something' does not always need to be specified, it is difficult to define what would constitute a refutation of the assumption of maximization.

For many centuries, rationality was viewed as a stable and reliable means to convince everyone that one's view was true, that is, to justify one's knowledge by means of irrefutable logical proof. Since the time of David Hume, the ability of rationality to deliver on this promise has been in doubt. Moreover, it is against this promise that some of Popper's disciples argue that rationality is better understood as a means of criticizing. Criticism is built upon discovering logical contradictions. After all, an empirical refutation is merely a contradiction between the theory and the available empirical data (i.e. both cannot be true). Except for tautologies, rationality does not guarantee that one's knowledge is true but rationality can be a means of proving that one's knowledge is false. This asymmetry parallels Popper's distinction between verifiability and refutability. Every argument consists of (two or more) assumptions and at least one conclusion which is claimed to be true whenever all of the assumptions are true. In terms of rhetoric, it would be better to say the conclusion is true whenever one accepts the assumptions as true. In one sense it could be claimed that the conjunction of the assumptions forms a justification of the truth of the conclusion statement. But the justification is conditional on the actual truth of the assumptions. Thus, such a justification is always open to question. From a non-justification standpoint, the argument is a means of criticism. For example, if one accepts all the assumptions as true then one cannot at the same time accept statements which contradict any valid conclusion based on those assumptions. Specifically, if one had a consumer theory that said that the demand curve for a good is downward-sloping when certain conditions are met, then if those conditions are met and the assumptions are all accepted as true, one could not at the same time claim to accept the existence of so-called Giffen goods. So rationality may still retain its universality and ability to convince but the disciples argue that the ability may be limited to criticism and refutations.

The widespread presumption that rationality-based science is a successful stable enterprise is denied by Popper's disciples. Nevertheless, since the presumption is so widespread, they cannot completely ignore it. Some of the disciples claim that the history of science appears to be stable only to those who wish to ignore the impact of Einstein's overthrow of Newton's mechanics. In the 1950s, when I was a high-school student, some science textbooks led one to think that there existed an infallible scientific method which if followed step by step would lead to the establishment of a scientific law. The first step was the collection of data. The second was the formation of a hypothesis to explain the collected data. The third step was the formation of an experiment to test the hypothesis. If the hypothesis passed the test, the hypothesis was declared a theory. If the theory passed the tests of all other scientists, then one's theory would become a law! While today's atmosphere of moderation would not be so optimistic, the old textbook writers were quite confident. The basis for their confidence was their belief that the success of Newton's physics was sufficient proof that such a method existed and it worked. What is most disturbing for Popper's disciples is the presumption that any success in science must be due to a practiced scientific methodology. Again, the disciples take the view that methodology has no more guarantees than a Socratic dialogue. Unfortunately, proponents of the ordinary view of science seem to want more.

The foundation of the belief in the stability and reliability of science has always been a belief in the universality and certainty of a scientific method. When it turned out that Newton's mechanics failed under certain conditions, believers in scientific method chose to switch rather than fight. Specifically, they held to a view that still claimed there was an infallible method but switched to say that it never was a method for proving the truth of scientific theories but only a method for choosing the best from existing competitors. So when Einstein or Popper claim that theories are either true or false, believers in the existence of an infallible scientific method are at a loss about what to do. They still wish to believe that scientific knowledge has been accumulating in a positive, progressive and stable way. Thus, it is easier to soften the goal of science so as to maintain a belief in Newton's

positive contribution than to admit that Newton's theory is somehow false. It could be argued that the softened version of scientific method not only lacks guarantees but also lacks a purpose other than possibly to apologize for Newton's failure.

While the ordinary view sees a scientific method providing a stable and certain science, Socratic dialectics lacks guarantees, as I illustrated with the *Euthyphro* dialogue. And while the softened version of scientific method also lacks guarantees, at least Socratic dialectics promotes a potential for learning. The potentiality is mostly due to Socratic dialectics maintaining that theories are true or false (rather than better or worse). But by promising only potentiality while requiring that theories be absolutely true, we face a dilemma. On the one hand, since the softened version of scientific method promises very little, success is easily achieved. On the other hand, while profound learning is possible with Socratic dialectics, it may take a long time. It is always possible that by engaging in a Socratic dialogue one might discover monumental truths, but more often the dialogue is one like *Euthyphro*. Perhaps only one of a hundred dialogues is productive. For methodologists in a hurry, dialectics does not seem to be a promising endeavor.

THE FUTURE OF POPPERIAN ECONOMIC METHODOLOGY

Kuhn's view of science presents a very comfortable (albeit dull) picture of a science of hard-working and level-headed scientists who rarely if ever stage a revolution. The Lakatos view appears less dull but that may be due merely to its spicy language of 'hard cores' and 'protective belts'. Both views seem to provide a clear picture of a stable science. If instead of following Kuhn or Lakatos we were to follow the disciples' version of Popper, then the picture would be much less clear. What is clear is the disciples' rejection of the substitution of a probability calculus for truth status. According to the disciples' Socratic version of Popper, theories are either true or false. With such a severe stance regarding the truth status of theories it would seem that science would always be in a state of rapid flux, possibly even chaos. So how do the disciples deal with the commonly accepted view that science seems to be rather stable?

Explaining stability away

In economics, the obvious example of a well-developed and stable research program is neoclassical theory, which in terms of its basic ideas (i.e. the principles of economics) has not changed much in the last hundred years. With this program in mind, the Socratic-Popper view of science would seem to be of limited use. Either the Lakatos-Popper view, with its emphasis on a well-protected core, or the Kuhnian textbook-based view would seem to be more appropriate. But their comparative advantage may be illusionary.

Why might an ordinary methodologist think the Socratic-Popper view of science implies a science in a constant state of rapid flux and chaos? The source of this supposition would have to be the ordinary view's notion that scientists are actively *choosing* among competing theories. According to the ordinary view, should any theory be refuted (i.e. proven false) there would then be an immediate switch to the next-best theory. Such alternating refutation and theory switching would almost definitely see science in a state of flux. But the disciples say that, while all theories are open to testing, a state of rapid flux or chaos is not a necessary outcome. There is nothing that forces one to choose any theory. One *may* choose to accept a theory that has not been refuted by the latest test, but there is no reason for why we *must* make a choice. The fact that there is no reason to make a choice leads to a certain type of stability, but this type of stability cannot be seen to be caused by the existence of a reliable method. It is certainly not due to the acceptance of a rationality designed to justify the currently chosen theory.

While the Socratic version of Popper's view would seem to imply a science that is in constant flux and turmoil, expecting such a state of affairs presumes too much. Most obvious is the presumption that since science is fallible it is easy to overturn. For any discipline to be rapidly changing it would seem to require all science teachers to be on the frontiers of knowledge developments. Since significant changes would involve challenging strongly held views (i.e. the accepted paradigm), peer review processes are unlikely to grant funds to someone whose views seem far out. While we give lip-service to the notion that a PhD thesis is to be not only significant but also original, any thesis that was completely original would be difficult to assess on the basis of the currently accepted paradigm. Advances in any discipline are usually marginal because marginal changes are easy to understand. This notion of marginalism parallels Popper's views of social change and social policy, which he calls 'piecemeal engineering'.

There are many reasons for the apparent stability of science in general and of neoclassical economics in particular. Foremost is the recognition that science is a social institution involving such things as educational institutions, research funding institutions based on peer reviews, textbook publishers and overall the constraining influence of the sociology of any scientific community. And we must not overlook the necessity for any theory or research program to be based on some metaphysical notions that are purposefully put beyond question or are at best very difficult to test.

What some disciples argue is that the apparent stability of any science is an intended consequence of decisions made within the scientific community. The stability, apparent or otherwise, is a social artifact and not in any way an inherent logical property of scientific knowledge.

The practicing Popperian methodologist

In this chapter I have tried to present the disciples' view of Popper's theory of science that, judging by some of the views expressed by some of my critics, does not seem to be widely understood. Briefly stated, according to the Socratic-Popper view of science, criticism is the main course and falsifiability, situational analysis, critical rationalism all belong to the *hors d'oeuvre*. This chapter also represents my perspective on my own efforts at practicing Popperian methodology. It explains why I have had considerable difficulty communicating with those falsificationist methodologists who see their role as that of appraising various aspects of economics. My communication failures initially were due to my failure to realize that they believed that Lakatos correctly portrayed Popper's philosophy of science as falsificationism. Things are actually much worse. It would appear that the followers of Lakatos are totally unaware of the disciples' view of Popper.

Socratic dialectics is central to Popper's view of science. Accordingly, science is critical debate. As with any debate, there is no foolproof method, no guarantees. Problem orientation is Popper's medium for conducting debates but it is not the central message. Situational analysis is only a convenient vehicle for interpreting the rationality of the problem situation but nothing more. Critical rationalism is a means of differentiating and precluding a justificational interpretation of the rationality of the problem situation but nothing more. In all of this, falsifiability is merely a logical condition required by critical rationalism. And rationality is essential but still it is only one aspect of criticism.

When I started working in the field of economic methodology, at a time before Lakatos began promoting his version of Popper, I knew only the disciples' version of Popper. In my work falsifiability plays at most a minor role. Until my 1992 book, which is explicitly about methods of criticizing neoclassical economics, I took the criticism-based Socratic-dialectical view of Popper for granted. It was not until the 1980s that I began encountering methodologists who equated Popper with a 'falsificationist methodology'. When I challenged them by explicitly rejecting such an equation, rather than their taking the opportunity to re-evaluate their own view, they dismissed all of my methodology writings as irrelevant. Things would seem to be looking up for the disciples' view of Popper. But those who might now want to consider the Socratic-Popper

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view instead of the falsificationist Popper will still not know what to do. My advice is that they should stop talking about methodology and start doing it. I invite them to consider all of the examples of how to practice the Socratic-Popper-based methodology that I have presented in this book.

NOTES

1 The remainder of this chapter is a slightly revised version of my 'Scientific Thinking without Scientific Method: Two Views of Popper' which appeared as Chapter 8 in Roger Backhouse (ed) *New Directions in Economic Methodology*, (London: Routledge, 1994). Its use here is with the permission of the publisher.

2 Unfortunately, Popper insisted on declaring some of his rejections of the necessity of solving specific problems to be 'solutions' of those problems. For example, see Chapter 1 of his *Objective Knowledge* [1972], where he claimed to have solved the unsolvable problem of induction.

3 It is amazing to me that someone would think that here we have Socrates going to court to be prosecuted for the serious crime of impiety and yet drops everything in order to enter into some sort of Sophist dialogue so as to expose the alleged stupidity of a poor, insignificant fellow like Euthyphro.

4 This at least was his oral response to Lakatos in their debate during the 1970 American Association for the Advancement of Science meetings in Boston.

Epilogue

Critical comments on the sociology of economic methodology

> The world in which I live is not one in which one feels oppressed by existence theorems or proofs of them or provers of them. ... If I feel oppressed by anything it is by the NBER and that flood of yellow-covered working papers. None of them contains an existence theorem. Most of them are empirical. They do indeed test hypotheses. The trouble is that so many of them are utterly unconvincing, utterly forgettable, utterly mechanical, and there is no way of knowing in advance which are and which are not. ...
>
> The problem is that economics is not as cumulative as we would like in its quantitative understanding of the way the world works. Those yellow NBER papers are a symptom of that; they never settle anything. I think it is for a cluster of reasons that have to do with the way economics is done and with the very nature of its problems.
>
> Robert Solow [1990, p. 30]

As should be evident in Chapters 19 and 20, despite what I said earlier, I do have a methodological 'position' of sorts. I am clearly advocating a 'critical attitude'. Not just any 'critical attitude', of course, but the one which is at the foundation of what I have called the Socratic-Popper view of learning through criticism. But the reader must be careful not to read too much into this admission. I am not promoting a simple-minded prescription or proscription such as 'Choose only falsifiable models or theories'. I deny the existence of any reliable simple-minded criterion such as this. Nor is there even a simple measure of success. I have been arguing repeatedly against just such a mechanical, formula-based view.

In this epilogue I want to assess some of the general matters that I have addressed in the various parts of this book in terms of the 'critical attitude'. Specifically, I want to talk about the sociology of journal referees, the intolerance of liberal-minded pluralism, the hypocrisy of specialized journals, the hypocrisy in matters deemed to be ideological, the bleak future of methodology and the imperviousness of neoclassical economics.

THE SOCIOLOGY OF JOURNAL REFEREES

Ideology obviously plays a major role in the editorship of major mainstream journals. How many editors of major mainstream journals would take the risk of publishing an article by a competent Marxian scholar? Few if any. I think it is just as bleak for scholarly methodologists even though I agree there is a lot of silly methodology published by non-major journals.

Being both a frequent referee and a frequent recipient of referee reports, I think I am familiar with this business of the journal publication aspect of our discipline. I have on many occasions received helpful referee reports. On other occasions I have received reports from people who are more concerned with their careers than with mine. In particular, I thought the referee's report that I received from the *Journal of Political Economic* for my subsequent 1979 *Journal of Economic Literature* article[1] was mean-spirited and uncaring (i.e. uncaring for me vs the referee's relationship with the editor). Careerism has become the bane of scholarly economics.

Careerism has been made worse by the common practice of double-blind refereeing. Even those journals that do not use double-blind refereeing will still withhold the referee's identity from the author. Interestingly, some of the reports on my 1979 paper by the referees of the *JEL* were signed. I do not know what the practice is today in philosophy of science journals, but twenty-five years ago blind refereeing was the exception. Even today, referees for the *Philosophy of Social Science* journal know the name of the author and the referees are asked whether they are willing to allow the editor to give their names to the author.

In 1990 I submitted a paper that is critical of both critics and advocates of certain applications of Chicago-school economic methodology to accounting theory [Boland and Gordon 1992]. I told the editor that it would be pointless to send it to a referee that was an advocate of that methodology or to one that was an enemy of Chicago-school methodology. Despite my warning to the editor, instead of locating a neutral referee, he sent it to one of each extreme. Of course, both referee reports were useless. Both referees grabbed at anything they could find to dismiss our article.[2]

The main problem with blind refereeing is that there is no accountability. It is all too easy for an editor who is unfamiliar with a sub-discipline such as methodology to fail to see the hidden agenda of the referee. For example, referees who are beginning their careers too often try to impress the editor by being not only negative but unfair. This is made worse when the referee knows the author's name and the author is an obvious competitor. The same can be true if positions are reversed and the older referee tries to protect his or her high position in the profession. In all

cases, the referee's report can be useless when you do not know where it is coming from. This, I think, is the major shortcoming of the double-blind refereeing system. If I am criticizing some philosophical position, it is useful to know whether the referee is predisposed in favor of or against that position. If the critical comments come from a referee who is an opponent of the position that I am criticizing, I would put more weight on them than if they are from a proponent. In all cases, ignorance of the hidden agenda of the referee severely limits the information content of the referee's report. Maybe some journal, somewhere, should try a reverse single-blind system where the referee's name is known to the author but not the reverse. It is only a suggestion. But, whatever problem people think double-blind refereeing is solving, I think it is time to recognize that it may be causing equally troublesome problems of injustice that happen when referees put their careers ahead of integrity and fair play.

THE INTOLERANCE OF LIBERAL-MINDED PLURALISM

While at first blush it would be easy to jump on Bruce Caldwell's pluralism bandwagon, the question is, what problem is solved by pluralism? Surely, pluralism sounds very liberal-minded but, in practice, it is a means of suppressing criticism. True proponents of Popper's critical rationalism and the Socratic-Popper view of learning through criticism are rarely invited to conferences organized by proponents of pluralism.

Tolerance is, of course, a good thing. However, is tolerance of intolerance good or bad? In other words, there are always limits to tolerance. One limit is that it is not clear how proponents of pluralism tolerate the critical attitude. The question I have is whether, by modifying his pluralism to 'critical pluralism', Bruce is trying to have it both ways: pluralism with criticism. But will any criticism be acceptable? While Bruce is very willing to be tolerant, my experience with other proponents of pluralism is that they tend to take a relativist position with regard to the truth status of theories, assumptions and models. That is, theories, assumptions and models are never to be considered 'absolutely true'. And if one takes a critical stand that asserts that someone's theory is false, that is, absolutely not true, one is deemed to be intolerant. Pluralism as a process might be acceptable to followers of Popper, but as an end product it does not seem to be compatible with Popper's critical realism (the view which Popper sometimes called 'scientific realism' [e.g. 1972]). Since Popper's Socratic view of learning is that one learns through criticism, how can one's learning be relative?

THE HYPOCRISY OF SPECIALIZED JOURNALS

It is interesting that over the last decade or so we have seen the creation of many specialized journals. Liberal-minded economists have hailed this development as a clear opportunity for all of the non-mainstream scholars to have an opportunity to publish. Unfortunately, this liberal hope has not been realized. What we have today is a proliferation of specialized journals, each catering to just one narrow-minded special-interest group. Moreover, the traditionalist journals have used the excuse of the existence of these specialized journals to give up their social responsibility to service the wider interests of their memberships. In the case of methodology this has been very telling since many members of the organizations which sponsor generalist journals have a long-standing interest in methodology.

For all practical purposes, methodology has been marginalized by the founding of journals that specialize in methodology rather than pushing for more space in mainstream journals. As I argued in Chapters 19 and 20, Popperian methodology literature been hijacked by followers of Lakatos. Similar hijackings have occurred with specific methodological viewpoints such as institutionalism. Journals (or organizations) that are hijacked by special-interest groups prohibit anyone except those who toe the line of the current editors. What is most distressing about these hijackings is that a certain element of self-righteousness leads editors to think that it is *acceptable* to treat the 'outsiders' with less than a minimal civility. I think the treatment I received from the editor of the *Journal of Economic Issues* in 1986 is a perfect example (see Chapter 4 above).

There is a sense in which one has to have a little sympathy for the journal editors. Tenure committees in almost all North American economics departments have turned over their duty to assess the quality of their colleagues' research to the journal editors. As a consequence, editors are under pressure not to take chances. My 1979 *Journal of Economic Literature* and 1981 *American Economic Review* articles were the result of the editors taking chances. Interestingly, since the departure of those editors over ten years ago, only two methodology papers have been published in those journals – both in the *JEL* (viz Caldwell 1991b, Mäki 1995). I think that the desperateness of this reality leads to the selfish careerism that dominates the sub-discipline of methodology as it dominates most sub-disciplines in economics.

The problem facing proponents of the critical attitude or the Socratic-Popper view of science is that mainstream editors avoid controversy and want methodology to be well behaved and serve only a positive role of apologetics or of justification of the status quo. This attitude encourages complacency in the mainstream of economics. My methodology articles

have challenged this desired complacency simply because I have offered internal criticism. Interestingly, I do not think Blaug's challenge is sufficiently internal to cause a concern. Stan Wong's 1973 *AER* article challenged the typical understanding of the Friedman–Samuelson schism in only a limited way. My advice to Stan was to show that Samuelson's critique of Friedman required Samuelson's acceptance of Friedman's methodology and Friedman's defense required an acceptance of Samuelson's methodology. But, unfortunately, Stan backed off from such a challenging attack on Samuelson. Given Stan's success at getting his very first article published in the top journal of the profession, it is difficult to argue with his judgement.

THE HYPOCRISY IN MATTERS DEEMED TO BE IDEOLOGICAL

As I have discussed in Part I, much of the methodological criticism surrounding Friedman's 1953 methodology essay is ideologically motivated. The ideological basis of the criticism seems too often a sufficient justification for unfair criticism. It also seems to justify a certain degree of hypocrisy. Let me illustrate.

In 1983 a 'rump' Cambridge conference to celebrate the 100th birthday of John Maynard Keynes was organized by the *Cambridge Journal of Economics* to discuss Keynes and methodology. I say 'rump' because this was not the celebrated big-name mainstream conference held at King's College but the one held at Trinity College by non-mainstream post-Keynesians. The call for papers invited contributions concerning 'Keynes and method'. Seeing myself as an obvious methodologist, I submitted a proposal and it was accepted. I expected this would be a conference of methodologists. But, as it turned out, over half of the participants were econometricians! The reason is that the econometricians thought that since Keynes had published a critique of econometric methodology [Keynes 1939], the conference was obviously about econometric methods.

This conference was the best conference I have yet attended. Thanks, I think, to Geoffrey Harcourt's hidden role, it was very fair-minded. The conference was organized so that there was only one session at any point in time. At the end of the conference we had a group discussion about the entire conference. I took the opportunity to do a survey of the econometricians' view of methodology. I outlined the fundamental notions of Friedman's instrumentalist methodology but without ever mentioning his name. Then I asked who in the group agreed with these fundamental methodological notions. As I recall, all of the econometricians held up their hands to show agreement. Amazing, I thought. As I recall, I then asked who among them agreed with Friedman's methodology. Not surprisingly, since virtually every one of them attended the conference because they identified with left-of-center post-Keynesian economics, they all denied any agreement with Friedman's instrumentalist methodology. I am not sure whether this inconsistency was evidence of hypocrisy or mere ignorance of methodology.

THE FUTURE OF SUBSTANTIVE METHODOLOGY

It is ironic that, despite the enormous growth of methodology literature in the 1980s, today's mainstream economic theorists are even more ignorant of methodology than were those in the 1960s and 1970s. This may be partially explained as the result of the loss of sex-appeal of closet discussions of methodology that were typical in those earlier decades. It may also be explained by recognizing that the closet discussions were made possible by disputes within the philosophy of science community – between followers and critics of Karl Popper (conjectures and refutations), Thomas Kuhn (paradigms and revolutions), Imre Lakatos (hard cores and protective belts), etc. – which were used to spice up the methodology discussion.

Today, mainstream economic theorists take methodology for granted. Appreciating the necessity of testability is an obvious example. Avoiding any open or rash claims for the truth of one's assumptions is pervasive. Almost all of today's economic theorists' efforts are devoted to modeling techniques. Very little consideration is given to issues, methodological or otherwise, that might challenge their belief in the veracity of neoclassical economics and in particular the maximization hypothesis.

As noted in the Prologue, many methodologists worry about the question 'Does methodology matter?'. Too often, they give little consideration to *how* it can matter. Instead, they are concerned only with why it *should* matter. Obviously, the study of methodology matters to other methodologists; but why should boring discussions of the need for realism, of whether ad hocness is a virtue or a vice, of whether economics should be considered a science, of whether methodology should be a form of literary criticism, and so on, ever be of interest to mainstream economic theorists? Even some methodologists find these discussions very uninteresting. If methodology is to have a future beyond being an obscure, marginalized sub-discipline of the already marginalized history of economic thought, then methodologists will have to spend more time reading economic theory and less time on their favorite philosophers of science.

Despite what most neoclassical theorists may think, their closets are full of skeletons most of which are concerned with what theorists consider an adequate explanation. As I asked in Chapter 13, is methodological individualism compatible with the mechanical eighteenth-century rationalism that

all neoclassical economists take for granted? As discussed in Chapter 14, are the explanations of equilibrium prices consistent with the behavior assumed in disequilibrium situations? As discussed in Chapter 15, are neoclassical economic models ever capable of explaining rational dynamics such that real (irreversible) time matters? There are other problems with neoclassical economics which have not been discussed here but ones which methodologists could help clarify. One methodological problem raised by Arrow [1986, 1994] concerns the existence of social knowledge. While individual decision makers must have knowledge about society, the existence and possession of such knowledge seems contrary to methodological individualism. Can social knowledge exist autonomously or must it be possessed by individuals and thus cease to be social knowledge? There are more profound problems lurking in the common presumption of learning by induction that are built into every neoclassical model that tries to deal with how the individual decision maker acquires needed knowledge [Boland 1996]. All of these issues suggest avenues for critical analysis of neoclassical economics, and in particular, criticism that neoclassical economists should be able to understand.

THE IMPERVIOUSNESS OF NEOCLASSICAL ECONOMICS

Neoclassical economics has been the object of methodological criticism for several decades. Yet there is no sign of any adjustments in response to these criticisms. In addition to those criticisms discussed in Part IV, I have humbly offered other criticisms of the methodology practiced by neoclassical model builders but, with one exception, nobody seems willing to respond. The one exception is David Hendry [1995], who responded to the criticism of econometric-based hypothesis testing in my 1989 book.

Obviously, one effective defense against criticisms from outside is to ignore them. But, beyond deliberate ignorance, why is neoclassical economics so impervious to criticism? Is neoclassical economics a matter of belief and faith and thus deliberately put beyond criticism? I think those positivists whom I called LSE positivists certainly began trying to promote empirical criticism. But, by focusing only on econometric model building as the LSE positivists did, fundamental neoclassical theory is isolated from direct criticism.

I think there is a more insidious reason for the imperviousness of neoclassical economics. What kind of student is attracted to neoclassical economics? Clearly, anyone who decides that it is in their best interest to be selfish would find that neoclassical economics provides a powerful justification of their selfishness. This is not to say that everyone in the mainstream of economics is selfish, but only that it is all too easy to identify colleagues who are very skilled at using their neoclassical explanations to deflect challenges to their selfish pursuits.

Hypothetically speaking, if neoclassical economics attracts predominantly selfish-oriented people, it would be in their self-interest to ignore telling criticism of neoclassical economics. If neoclassical economics were refuted, if that is even logically possible, I think there would be a serious crisis of integrity for many neoclassical economists.

All needling aside, I find it an interesting dilemma for Popperian methodologists. Since Popper says that 'science' is characterized primarily by its critical attitude, neoclassical economists seem unwilling to entertain methodology and its inherently methodological criticism of neoclassical theory. It is all too easy to argue that neoclassical economists are cowards. But, more important from my Socratic-Popper perspective, unwillingness to tolerate methodological criticism may simply demonstrate that neoclassical economists are 'unscientific'.

NOTES

1 See the quotation at the top of Chapter 1.

2 The journal in question uses double-blind refereeing and thus one of the referees referred to the 1986 criticism of me by Ken Dennis but of course made no mention of my 1987 refutation of that criticism.

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