6 The Stumbling-blocks of Economics: Complexity, Time and Change

This chapter will outline what Marshall saw as the deepest problems in the development of an economic science – problems which, I will claim, have continued to enjoy the same status: as unsolved, if not insoluble. In Chapter 6 these problems will be presented initially through Marshall's eyes: in the rest of Part II they will be dealt with as generalised problems for the field of economics and, in many cases, for all the social sciences. This chapter will be one that will make use of a relatively large number of quotations. These will be numbered throughout the chapter for easy reference.

There were issues of complexity, time and change which Marshall recognised as essential aspects of his subject but which were not readily dealt with by the 'scientific' techniques which he was helping to develop. As he feared, the forces which he helped to put in motion have in fact resulted in a situation wherein these bothersome but crucial issues have been pushed aside by techniques which are powerful in other achievements, but not sufficient for the degree of complexity that interested Marshall.

Much of Part II will be devoted to an exploration of approaches to economics which, it is hoped, will enable social economics to grapple with complexity, time and change in ways which, at best, may ultimately go beyond what neoclassical and Marxian economics have been able to accomplish; at the least it is hoped that the proposed ways of doing social economics will provide a good complement to the existing paradigms.

In anticipation of the discussion of later chapters of Part II we will begin, here, with a small conceptual tool that is so useful that it is worthwhile to make it available now, before going any further: namely, the distinction between two concepts, *accuracy* and *precision*.

ACCURACY AND PRECISION

This is not a distinction that is entirely novel: it is noted from time to time, but it has yet to be taken seriously enough in economics to make the difference in this field that it might. Among those who have pointed it out, one who has put the issue with special force and clarity is Andrew Kamark. His statement on the subject is worth repeating here:

> 'Accuracy' will be used to convey the meaning of 'correctness', or 'true value'. 'Precision' will be used to convey the meaning of 'degree of sharpness' by which a thing or concept is specified. For example: on Cape Cod, where the pace of life is unhurried and casual, you may ask a craftsman in June when he will come to repair your fence. If he answers, 'Sometime in the autumn', he is being accurate but not precise. If he answers, 'Ten a.m., October 2', he is being precise but not accurate – it is almost certain that on October 2, the fish will be running and he will be out in his boat. One of the recurring themes that we will find in our discussion is that too often in economics the choice is between being roughly accurate or precisely wrong.'[1]

Accuracy has to do with what I shall be discussing later under the name 'external consistency': that is, a well-specified and 'realistic' mapping from subjects in the real world to our symbolic representations of these subjects. *Precision*, by contrast, may be judged within the given model, without reference to the fit between the model and its real-world referents (if any). Precision does not necessarily involve *quantification*, but that is one of the most obvious and frequently employed avenues to its attainment. *Qualitative* analysis can be accurate; its precision cannot be measured, but its accuracy can be assessed.

Alfred Marshall sometimes expressed the conviction that, in order to be scientific, it was not enough to be accurate; it was also necessary to be precise: quantitative as well as qualitative description, discussion, and explanation was called for. His argument was a practical one, and perhaps the strongest that can be made on this subject:

> Mere qualitative analysis, then, will not show the resultant drift of economic forces. It may show gain here and loss there; but it will not show whether the gain is sufficient to overbalance the loss; whether the gain should be pursued in spite of the loss. And yet, for the purposes of practical action, this decision must be made. It is useless to say that various gains and losses are incommensurable, and cannot be weighed against one another. For they must be, and in fact they are, weighed against one another before any deliberate decision is or can be reached in any issue ('The Old Generation of Economists and the New' (1897) in *Memorials*, pp. 301–2).

Marshall's hope was to build a scientific discipline upon the special advantage which he saw economics as possessing, in dealing with subjects which can be at least approximately quantified. Thus,

> 3 The *raison d'être* of economics as a separate science is that it deals chiefly with that part of man's action which is most under the control of measurable motives; and which therefore lends itself better than any other to systematic reasoning and analysis. We cannot indeed measure motives of any kind, whether high or low, as they are in themselves: we can only measure their moving force. Money is never a perfect measure of that force; and it is not even a tolerably good measure unless careful account is taken of the general conditions under which it works, and especially of the riches or poverty of those whose action is under discussion. But with careful precautions money affords a fairly good measure of the moving force of a great part of the motives by which men's lives are fashioned.'[2]

This passage follows upon a paragraph which refers to 'trained common sense' as 'the ultimate arbiter in every practical problem', and then continues:

> **4** Economic science is but the working of common sense aided by appliances of organized analysis and general reasoning, which facilitate the task of collecting, arranging, and drawing inferences from particular facts. Though its scope is always limited, though its work without the aid of common sense is vain, yet it enables common sense to go further in difficult problems than would otherwise be possible (ibid.)

The 'appliances of organized analysis and general reasoning' to which Marshall refers are the tools of mathematics, statistics, graphical exposition, etc. One critical question, which he never addressed directly, is: How precise does quantification have to be in order to permit the use of these tools? Will estimates and approximations suffice for these purposes?

From his many comments tangential to this question, it would appear that Marshall often (though not always) assumed that, if mathematical analysis is built upon pretty good approximations, it can yield pretty good answers – though they will never be as precise or as satisfactory as the answers which can be got in the physical sciences.

We will see that he had a lively awareness that such a procedure was filled with dangers. His personal attitude towards mathematics (an attraction/repulsion wherein he felt that mathematics was a seductive game that might lure him away from the serious – i.e., moral – issues in life) sometimes prevented Marshall from carrying his techniques to their logical conclusion. It may be that this inhibition also prevented him from exploring more closely his rather casual assumptions (or hopes) regarding the usefulness of approximate measurements for quantitative techniques.

PROBLEMS WITH COMPLEXITY

Appendix C of Marshall's *Principles*, on 'The Scope and Method of Economics', is a particularly interesting essay which may present an appearance of ambivalence, as though the author was unable to make up his mind on which of two choices to take. In fact I believe the situation was that Marshall saw very well both the values and the pitfalls of a myriad methodological approaches. We have already divided these approaches into the qualitative (including accuracy) and the quantitative (stressing precision): for the purposes of the rest of this chapter we may roughly summarise them as the *intuitive* and the *mathematical* approaches. Marshall's conviction was that economics could only progress to the achievement of its best potential by the use of both; however, he also recognised the difficulty of proceeding thus upon two tracks, along with the attendant danger of trying to avoid that difficulty by concentrating upon one track alone.

Marshall's desire for quantitative analysis, and for sophisticated scientific techniques which could handle a world thus appropriately quantified, stemmed from his appreciation of the fact that the realities of interest to economics were so complex that they could not be handled on an intuitive level: they had to be reduced to 'bits' of information (as we would now say), so that they could be dealt with piecemeal in formal analysis. However, while Marshall was attracted to mathematical – especially statistical – means for handling complexity, he virtually never spoke in favour of these methods without attaching a warning. The following is typical:

> The longer I live the more convinced am I that – except in purely abstract problems – the statistical side must never be separated even for an instant from the non-statistical: on the ground that, if economics is to be a guide in life – individual and more especially

> social – people must be warned off by every possible means from considering the action of any one cause – beyond the most simple generalities – without taking account of the others whose effects are commingled with it. And since many of the chief of these causes have either no statistical side at all or no statistical side that is accessible practically for common use, therefore the statistical element must be kept subordinate to general considerations and included among them.'[3]

The 'general considerations' were, in Marshall's approach, to be the responsibility of 'common sense'. The use of that term has some problems (which will be discussed further in Chapter 9); for now, I would like to ignore the particular term, but think about how people go about dealing with complexity in every day life. My hypothesis will be that, in some respects, under many common kinds of circumstances, we do better at coping with complexity *without* many of the tools and techniques that have been developed for economics than we do with them.

There is a wide variation in the capacity that human beings have for handling complexity on an unconscious, or intuitive level. Even the low end of the normal range is, however, quite impressive. Consider the complex calculation of velocity, angle, acceleration, etc., that have to be made in a split second when a driver considers whether it will be possible to pass a car and to return safely to his/her own lane before an oncoming automobile reaches the place on the road where s/he will be before s/he can pull back into lane. On a conscious, formal level such calculations would either take too long to be practical, or (more often) would be beyond the capability of most of the people who actually drive cars. On that conscious level, comparable calculations can only be done by a translation of the elements involved into mathematical symbols.

Marshall's apparent assumption on this subject is an interesting one, and quite different from what seems to motivate the modern development of economic techniques. He suggested that mathematics and other kinds of analysis can deal with simple situations, but becomes less and less useful as the complexity of the subject increases. Such a conclusion is explicit in his statement that 'The most helpful applications of mathematics to economics are those which are short and simple, which employ few symbols; and which aim at throwing a bright light on some small part of the great economic movement rather than at representing its endless complexities.'[4] Why would an economist of renowned mathematical ability have felt such skepticism? One explanation is in the observation that:

> 6 numerical instances can as a rule be safely used only as illustrations and not as proofs: for it is generally more difficult to know whether the result has been implicitly assumed in the numbers shown for the special case than it is to determine independently whether the result is true or not (*Principles*, p. 688).

THE USE OF MATHEMATICS TO ADDRESS PROBLEMS WITH TIME AND CHANGE

Marshall's response to the problems of time and change were similar to his response to complexity. On the one hand, he would have liked to have found mathematical solutions to these problems; on the other, he doubted the power of mathematics to deal with these issues fully:

> while a mathematical illustration of the mode of action of a definite set of causes may be complete in itself, and strictly accurate within its clearly defined limits, it is otherwise with any attempt to grasp the whole of a complex problem of real life, or any considerable part of it, in a series of equations. For many important considerations, especially those connected with the manifold influences of the element of time, do not lend themselves easily to mathematical expression: they must either be omitted altogether, or clipped and pruned till they resemble the conventional birds and animals of decorative art. And *hence arises a tendency towards assigning wrong proportions to economic forces; those elements being most emphasised which lend themselves most easily to analytical methods*. No doubt this danger is inherent in every application not only of mathematical analysis, but of analysis of any kind, to the problems of real life. It is a danger which more than any other the economist must have in mind at every turn. But to avoid it altogether, would be to abandon the chief means of scientific progress.
>
> For reasons similar to those given in this typical case, our mathematical notes will cover less and less ground as the complexity of the subjects discussed in the text increases (*Principles* p. 700; italics added).

It may be that the truly great mathematicians are those whose intuitive ability to calculate is not inhibited by the conscious use of the formal apparatus of mathematics; the conscious and the intuitive operations continue in parallel, each serving as a check on the other, and neither acting to inhibit the other. For most people, however, a switch over into one mode tends to damp down the other. This is particularly evident in the struggles which economics has had in dealing with dynamic issues of time and change. On the intuitive level we live in time, and take change for granted as the fundamental fact of life; but time and change are both destroyed by the analytical processes which depend upon taking reality apart into timeless instants; and when we are engaged with those analytical processes we often fail to see that the dissection has altered that which we wished to study.

The method of marginal analysis has been welcomed for, among other things, its amenability to the calculus. The latter, it has been thought, was a way to deal mathematically with time. In fact, however, the way that the calculus does this comes out the same as though it performed its operations by *stopping* time – which, of course, effectively eliminates it. It appears to permit the calculation of change on the wing, as it were, by quantifying the direction, the rate, etc., of change at a timeless instant; and it can do this for any timeless instant over any period for which the change can be specified in a well-defined function. Not only does this push us back to the empirical problem of defining the function; it also leaves us, when we use these functions for marginal analysis, at a static conclusion. For example, the point of intersection of the rising marginal disutility of work with the declining marginal utility of pay is an infinitesimally thin slice of information. It reduces the dynamic picture of diversely motivated, changing human beings to a single point – in time, in motivation, and in state of being. It permits precision, but it is a precision of dubious accuracy.

The argument I have just made sounds a little like the argument against the idea of infinitesimals, which goes back at least to Zeno of Elea, in the fifth century BC. In 1734 Bishop Berkeley took up the cudgels in *The Analyst*, a book which he wrote in response to Isaac Newton's use of infinitesimals in the development of the calculus. Berkeley referred to Newton's 'fluxions' (infinitesimal 'instants' of time) as 'the velocities of evanescent increments . neither finite quantities, nor quantities infinitely small, nor yet nothing the ghosts of departed quantities.'[5]

The introduction, by Weierstrass in 1872, of the use of 'limits' to compute instantaneous velocity is a way around the necessity of using infinitesimals:

8 Instantaneous velocity is taken as the limit of ratios of time and space increments (average velocities) taken over decreasing time

> intervals; without velocities referred to as intervals, in contrast to single instants, instantaneous velocity would have no meaning. Although instantaneous velocity [in the limits approach] does characterise motion at an instant, it does so by implicit reference to what goes on at neighboring times.'[6]

The problem here, as we shall see in a moment, is that the concept of 'neighboring times' is translated into mathematics through the concept of the real number line *as a continuum*. The use of decreasing intervals towards a limit of zero is, ultimately, no more effective than the 'infinitesimals' approach to the calculus in creating a genuine identity between the real world and a mathematical concept of instantaneous time (as expressed, e.g., in the question, 'How fast was the car going at 11.15?') Any way that we can think up for tagging or identifying the real-world time, 11.15, must identify a span, or *period*, of time, not an instant.

MATHEMATICS AND THE REAL WORLD

I will press this point a little further: as we go on in Part II we will be increasingly concerned with the issue of *what is realistic* – i.e., what approaches to being an accurate representation of the real world. The role of mathematics in creating a language with which to represent reality – or not to represent it – is central to economics. Marshall may have been one of the first to perceive the meaning of the possibility that mathematics could help to grapple with complexity, time and change; he was among the last major economists to express doubt that this hope was, in fact, solidly based. It is of considerable importance, not only to economics, but to all of the social sciences which have followed its lead into mathematisation, that the validity of these doubts be re-examined.

I continue, then, with a few examples of discontinuities between mathematics and the real world. 'Motion at an instant' and 'motion over a period' are two fundamentally different concepts, connected by what may be considered a pun on the pivot-word, *motion*. For a use of this idea which comes to a different conclusion from my own, I refer the reader to 'Zeno and the Mathematicians' by G. E. L. Owen (in Salmon (ed.) 1970). Owen says that Aristotle 'failed to grasp that the two senses of 'moving' are not identical but yet systematically connected' (ibid., p. 161). Such a 'systematic connection' could constitute the basis for the 'mapping' from real world to symbolic representation which, I claim, is not sufficiently present in the mathematical and the common uses of the word, 'motion' to allow consistently accurate and precise use of that word as it shuttles back and forth between its (at least) two different realms of use.

Similar problems beset our notions of *space* and *matter* as those just suggested with respect to *time* and *motion*. All of these problems have in common the notion of 'continuity', or a 'continuum'. These words, too, however, turn out to have different meanings when applied, even on the grossest level, to space and matter on the one hand, and to time on the other. When we analyse these subjects more carefully (as we will do in Chapter 11) we will find that the words 'continuous' or 'continuum' also have different meanings when applied to matter at the sub-atomic level versus at what might be called the 'sensory' level. In part, the problem arises from an insufficiently examined use of analogy:

9 As usually understood, the real numbers (including integers, rationals and irrationals) in their natural order form a mathematical continuum. If we use the real number system to represent time we are assuming that there is an isomorphism between the real numbers and the temporal continuum One consequence is that instants of time do not have immediately preceding or succeeding instants; between any two instants there are infinitely many others. Another consequence is that there is a super-denumerable infinity of instants, and each instant has zero duration. The same type of situation occurs if the real number system is used to represent points on a line in physical space.' (Wesley Salmon, 'Introduction' to *Zeno's Paradoxes*, 1970, p. 35.)

This problem has to do with the 'denseness' property of the mathematical continuum of the real number line. In ordinary understandings of 'continuity' the term implies that there is no separation between parts: if any part is chosen, it will be found to be 'next' to the adjacent parts in such a way that there is no room for other parts in between. This is precisely *not* the case for the real number line, where, for any two points (numbers) that you can name, it will always be possible to find others between them. Thus,

10 Whitehead and Bergson, for example, have denied the possibility of providing the requisite correlation between the mathematical continuum and physical time. An answer to them must take the form of showing how it is possible to provide a correlation between

> the *later than* relation among instants of time having zero duration and the *greater than* relation among real numbers (ibid., p. 36).

Such a correlation may be constructed, and yet the 'systematic connection' we are seeking is still not complete enough for all uses. For the purpose of keeping time with clocks of all kinds, the *later than/ greater than* correlation has been entirely adequate, for most purposes, for millennia. As we shall see when we examine Zeno's paradoxes of motion (in Chapter 11), it breaks down entirely when we try to carry to its logical limit a conceptual understanding based upon such a correlation.

The point of this detour, using the example of the assumptions and implications of the calculus, has been to pose the question: Where shall we place the use of mathematics in economics, between (on the one hand) the everyday employment of clock time, where the time/number correlation is accurate enough for all practical purposes; and (on the other hand) the purely abstract theorising of Zeno, who 'proved' that all motion is impossible? My argument is that there are places where the subject of economics requires accuracy (in relation to the real world), but where some of the mathematics presently in use in the field can only provide precision, without external accuracy.

Recent developments in the field of pure mathematics may be healthy in this regard, in more consciously stating that some areas of mathematics can legitimately exist at some distance from the real world.

11 Until 100 years ago it was tacitly assumed by all philosophers and mathematicians that the subject matter of mathematics was objectively real in a sense close to the sense in which the subject matter of physics is real. [Query: is that the same sense in which the subject matter of economics is real? or of physical anatomy?] Whether infinitesimals did or did not exist was a question of fact, not too different from the question of whether material atoms do or do not exist. Today many, perhaps most, mathematicians have no such conviction of the objective existence of the objects they study. Model theory entails no commitment one way or the other on such ontological questions. What mathematicians want from infinitesimals is not material existence but rather the right to use them in proofs. For this all one needs is the assurance that a proof using infinitesimals is no worse than one free of infinitesimals. (Davis and Hersh, 1981, p. 252.)

The authors of the book just quoted cite, as an outstanding example of this new freedom, the mathematical conventions that have been adopted to accommodate 'pseudo-real' objects within the 'nonstandard universe' of nonstandard analysis as used, e.g., by A. Robinson. The achievement of this approach, they claim, is *precise definition* of such concepts as an infinitesimal neighborhood. Here (referring back to our earlier definition) we see an example of the word, *precise*, being used to refer to something like internal consistency, quite explicitly eschewing reference to any external reality. Cantorean set-theory is another branch of modern mathematics which has similarly taken advantage of the liberty achieved by not insisting upon a 'systematic connection' between mathematics and reality.

These developments may help to force into consciousness the difference between mathematics and reality; between, for example, a physical understanding or description of the notion of 'distance', and a mathematical one. There are, in mathematical theory, an infinite number of ways of subdividing a physical distance. This is very different from saying that, in physical fact, a distance can undergo an infinite number of subdivisions. Similarly, since Euclid, mathematicians have accepted the notion of a 'point' as something that has position (location), but no dimension. As an *idea*, this is not only imaginable, but exceedingly useful: as a piece of the real world, it does not exist.

In normal life, as well as in mathematics, we have a clear notion of 'boundaries', including the 'edges' of *things* as well as beginning and end points in *time*. These are practical notions for many everyday uses, and they are reflected with precision in many mathematical assumptions and operations; but there are levels of reality wherein it is important to understand that the imaginary line which we conceptualise as constituting an 'edge' or 'boundary' has no real existence. (See Chapter 11 n. 14 for elaboration on this point.)

To give one more example: integers map very well onto sheep in a herd, or passengers in an airplane, or fingers on a hand. They apply *in principle* to grains of sand on a beach, or stars in the sky: even though we doubt that anyone ever *will* count these things, the idea that it would 'in principle' be possible to do so seems acceptable. We begin to get into trouble when we try to apply numbers to 'utils' (that is why units of money are so often used as a proxy for utility; numbers, including decimals, map very nicely onto dollars and cents). We know (roughly, not precisely) what we mean by the word, 'happiness', but there is no reliably accurate way to map this reality onto the real number line.

Some abstract mathematical conceptualisations are well designed to map onto some of the objects of the real world,. at some levels of observation. Other such concepts only map onto other abstract conceptualisations. Much of the time, the safest way to treat mathematical applications in the social sciences is to think of them as extended metaphors. A mathematical model does not put something real – the real subject of interest to a social scientist – through a process where the outcome is determined by the inputs, assumptions and techniques: that process only operates upon the abstract descriptions of the subjects of interest. The outcome is, indeed, determined jointly by the inputs, the assumptions and the techniques (as well as the skill of the human being who uses these techniques, and, usually, some additional inputs of intuition, hunches and un-spelled-out assumptions, which are not readily visible in the model) -- but it is not necessarily an outcome that has any bearing on the real world.

I have suggested that there are places where the subject of economics requires accuracy (in relation to the real world), but where some of the mathematics presently in use in the field can only provide precision, without external accuracy. In relation to time and change, for example, modern economic techniques offer the use of lagged variables and moving equilibrium growth models, but these techniques requires consistency in change, if they are to be useful. The erratic, unpredictable changefulness of change goes beyond a second, or third, or *n*th derivative. Often the best we can do, on the conceptual level, is to abstract from the ragged time and change of reality to smooth, continuous functions. The blooming, buzzing confusion of brute facts is the dynamic reality. The formal body of analysis is many steps of abstraction removed from that reality, and its apotheosis is the concept of static equilibrium – a concept that exists in the world of abstractions, not in the world of real time. Here we see the danger that, in compressing our understanding of the world into mathematical forms, we retreat from that which we have wished to study.

A perfectly static set of conditions is relatively accessible to analysis by the tools that were available to Marshall, and which have been progressively refined since his time. To set up that set of conditions as a goal, then to perceive all relevant forces (both economic and noneconomic) as tendencies toward that goal, allows the use of more powerful, consistent techniques than any we know of for dealing with such processes in themselves. Yet the best that we can get out of these techniques is something like Walras' general (but still essentially static) equilibrium analysis, or its modern version in computable general equilibrium models.

MARSHALL'S EQUILIBRIUM ANALYSIS

Marshall was significant in bringing into the centre of the discipline an issue whose complexity has since been hidden by the simplifications necessary in order to carry to its present height the theoretical precision of neoclassical economics. Modern economists who have any interest in the origin of their ideas, beyond the publications of the last few years, generally cite Alfred Marshall as a major contributor to the idea of comparative statics, in which is solidly embedded the notion of equilibrium as a description of *where things are*, or *whither things are tending*.[7] In fact, although he never approached Walras' attempt at detailing a general equilibrium analysis, Marshall's achievement in the area of equilibrium analysis is much diminished when it is seen only in the context of comparative statics, as just described. The following statement is characteristic of his position:

> **12** we look towards a position of balance or equilibrium between the forces of progress and decay, which would be attained if the conditions under view were supposed to act uniformly for a long time. But such notions must be taken broadly. The attempt to make them precise over-reaches our strength. If we include in our account nearly all the conditions of real life, the problem is too heavy to be handled; if we select a few, then long-drawn-out and subtle reasonings with regard to them become scientific toys rather than engines for practical work.[8]
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> . . . though the statical treatment alone can give us definiteness and precision of thought, and is therefore a necessary introduction to a more philosophical treatment of society as an organism, it is yet only an introduction (*Principles*, pp. 381–2).

Marshall most often used the term, *equilibrium*, in a manner which was slightly different from the ideas expressed as 'where things are or whither things are tending'. The short phrase which would generally have been acceptable to him in place of the word, 'equilibrium', (and which he not infrequently used in that way) was 'balancing of forces'.[9] This connotes a dynamic tension, rather than a tendency toward inertia. Marshall was almost always careful to state as exactly as possible what he meant, removing as far as he could any danger of misleading readers into unjustified simplifications. He was at particular pains to bring the reader with him away from a static notion of equilibrium as an end-point or goal, into the ultimately dynamic idea of 'balancing of forces'. He attempted to smooth the way by starting with 'a simpler balancing of forces which corresponds rather to the mechanical equilibrium of a stone hanging by an elastic string, or of a number of balls resting against one another in a basin' (*Principles*, p. 269) before bringing the reader to 'the higher stages' of his work, where he would introduce more complex biological metaphors such as a business firm, which 'grows and attains great strength, and afterwards perhaps stagnates and decays; and at the turning point there is a balancing or equilibrium of the forces of life and decay' (ibid.).

From here he goes on to expand his subject beyond the bounds of what his own methodology will ultimately be able to encompass; indeed, the remainder of this passage (particularly the section which I have italicised) is devastating to his own most ambitious hopes:

> **13** These considerations point to the great importance of the element of time in relation to demand and supply . . For, *in an age of rapid change such as this, the equilibrium of normal demand and supply does not thus correspond to any distinct relation of a certain aggregate of pleasures got from the consumption of the commodity and an aggregate of efforts and sacrifices involved in producing it*: the correspondence would not be exact, even if normal earnings and interest were exact measures of the efforts and sacrifices for which they are the money payments. This is the real drift of that much quoted and much-misunderstood doctrine of Adam Smith and other economists that the normal or 'natural' value of a commodity is that which economic forces tend to bring about *in the long run*. It is the average value which economic forces would bring about if the general conditions of life were stationary for a run of time long enough to enable them all to work out to their full effect.
>
> But we cannot foresee the future perfectly. The unexpected may happen; and the existing tendencies may be modified before they have had time to accomplish what appears now to be their full and complete work (*Principles*, pp. 288–9; italics added).[10]

Marshall's painstaking distinctions between the long and the short run were motivated by a number of considerations, of which only the most mundane (the different lengths of time required for adjustments of capital stock versus more fluid factors of production) continue to be well-known. To him the more interesting reason for this distinction was that it seemed to be a way of creeping up on a dynamic science. The need to do so has become especially clear in recent decades, as we have become more aware of the deficiencies in economics which have made it blind to many importance effects of human activity upon the natural environment. A weakness in the existing mainstream economic paradigms has been, precisely, their static character: the assumption of neoclassical economics, that markets adequately represent the future, has proven an insufficient representation of a reality in which present actions have not been guided by sufficient concern for future costs and benefits.[11]

GENERAL TENDENCIES AND PARTICULAR ACTORS; DEDUCTION AND INDUCTION; RIGOUR AND REALITY

We have seen Marshall write of a state of stable equilibrium to which things tend to return; but that, as he explained elsewhere, is only an illusion of changelessness; it is not a situation where there are no active forces, but rather one in which – as long as those forces remain unchanged – they happen to balance one another out. We have seen him complicate the notion of equilibrium to the idea of a centre *about which* variables tend to oscillate; and then complicate it yet further to an *average*, one which is rarely reached, but which we can imagine *would* be reached, *if* the highly improbable (i.e., long-lasting lack of change) occurred. Above all, the subject of equilibrium was to be thought of in terms of a *process, not of a goal.* Indeed, to slip into the idea that an equilibrium is a goal ('where things are, or whither things are tending') is to commit a logical fallacy, inserting a teleology where there is none.

As Marshall moves from Book V, Chapter i, which is an introductory chapter on 'balancing of forces' (or equilibrium) in markets, to Chapter ii, on 'Temporary Equilibrium of Demand and Supply', he is especially careful to bring in individual, personified actors, so that we can see that any teleology that actually exists is in the minds of human beings. Thus, with a boy picking blackberries, 'Equilibrium is reached when at last his eagerness to play and his disinclination for the work of picking counterbalance the desire for eating' (*Principles*, p. 276).

Later Marshall will comment on the meaning of a stable equilibrium, 'that is, the price, if displaced a little from it, will tend to return, as a pendulum oscillates about its lowest point' (ibid, p. 287); but we are intended to understand, here, that such a 'tendency' is not in any way teleological or normative, except possibly in the minds of individuals who have (more or less perfect) knowledge of the conditions of the market and understand how those conditions will be worked through to an anticipated result.

Mark Blaug has given a trenchant criticism of the situation in which neoclassical economics finds itself, in consequence of its choice of simplifications from the complexities of time and change. Blaug's criticism comes in the form of 'a central question about the entire history of orthodox economics in the last hundred years', namely:

> **14** that all the substantive finds of modern economics rest on the use of static equilibrium analysis and yet static equilibrium analysis seems to preclude fruitful discussion of such vital problems as the process of competition, the process of capital formation and the role of entrepreneurship'[12]

that is, problems which can only exist in the context of time and change.

It is important to keep in mind, as Marshall did, the reasons why he kept returning hopefully (only to remind himself sternly not to get carried away by his hopes) to quantitative approaches to economics. The subjects which Marshall most wished to be able to measure were in the area of welfare, which he hoped to be able to get at through consumers' surplus; that, in turn, required the ability to have exact measurements of demand – one route to which is a defined aggregate utility function. Marshall pinned many of his hopes upon the law of diminishing marginal utility (in such a murky area, the existence of any law at all seems like a great beam of light; cf. *Principles*, p. 79); but this, it turned out, was not to be sufficient (even if one were to make the enormous leap of taking for granted the measurement of utility) for deriving well-behaved demand curves.[13]

There is, however, an alternative route to the calculation of demand curves, wherein one can, by induction, piece together empirical observations, rather than building up deductively from utility functions. There is a long discussion in Book III, Chapter iii of *Principles*, relating to how, in principle, one might do this. Marshall concludes: 'And therefore if we had the requisite knowledge, we could make a list of prices at which each amount of [a commodity in general use] could find purchasers in a given place during, say, a year.' (*Principles*, p. 83).

The next chapter in *Principles* gives detailed examples of prices and their elasticities with regard to a large variety of things: the price and elasticity 'of wall-fruit, of the better kinds of fish, and other moderately expensive luxuries' (including plovers' eggs in London in April 1894); of wheat in London in 1335 compared to 1336; of bad concerts in small towns and good concerts in large towns; of water, as used by the poorer classes and by the middle classes; *etc. etc.* Marshall concludes these empirical observations with a new section (III, iv, 5) which starts out:

> **15** So far we have taken no account of the difficulties of getting exact lists of demand prices, and interpreting them correctly. The first which we have to consider arises from the element of *time*, the source of many of the greatest difficulties in economics.
>
> Thus while a list of demand prices represents the changes in the price at which a commodity can be sold consequent on changes in the amount offered for sale, *other things being equal*; yet other things seldom are equal in fact over periods of time sufficiently long for the collection of full and trustworthy statistics (*Principles*, p. 92).

Chapter iv of Book III ends with Marshall's 'Note on the statistics of consumption' in which, again, he stresses the empirical, inductive method, concluding that 'the general demand curve for a commodity cannot be drawn with confidence except in the immediate neighborhood of the current price, until we are able to piece it together out of the fragmentary demand curves of different classes of society' (*Principles*, p. 96).

There are a variety of ways of understanding the constraints which contemporary neoclassical economics has accepted for its investigation of the real world problems which are thought to be of an economic nature. It operates, to begin with, under standards of rigour which have an impact upon the question of what problems a professional economist will be willing to define as falling within his province; hence Blaug's frustration that his own favourite dynamic problems are scanted and even scorned as worthy objects of study.[14]

The meta-constraint which gives rise to the adoption of these perhaps needing-to-be-reexamined standards is the limitations of our abilities (separate limitations on our formal and on our informal, or intuitive, abilities) to deal with complexity. The neoclassical decision on how to deal with complexity has been, in effect, a preference for mathematical types of techniques which can handle simultaneously enormous numbers of variables. The price paid for that choice is that each variable or action entered into such a system must be defined unambiguously and must, usually, be defined so as to be conceptually quantifiable (even if, in many cases, no attempt will ever be made to quantify it).

It has been said that 'the ultimate model of a cat is of course another cat'.[15] The meta-constraint cannot be avoided: however we choose to try to deal with complexity, our systems of thought and analysis can never deal with the fullness of the world as we experience it. In all of our thinking and acting, we have to make do with something less than another complete world as model, as working theory, as construct for understanding. (See Chapter 8, below, for a further discussion of this subject.) The link between this meta-constraint and the kind of constraints under which different economists choose to operate is a process: starting with the limitations upon our abilities to deal with complexity, we may go, via a particular process of simplification, to a particular type of economic theory, the neoclassical. A different sort of process of simplification would lead to a different type of economic theory.

DEVELOPMENTS SINCE MARSHALL

The advent of modern computers has added significant pressure to the drive to find ways to quantify the things and events that are of economic interest in this world. What can now be achieved with quantitative statements has become enormously greater; so the motive to quantify has increased in proportion. But our ability to quantify has not kept pace.

Of all mathematical fields, Marshall had most optimism regarding the assistance that would be provided by statistics, and since his time this field has advanced in ways that seem to have justified his optimism. This has only thrown into sharper relief the distance between the refinement of the techniques and the reliability of the facts on which it has to work. Much of the subject matter of economics – a social science – remains intractable to 'exact measurements'. So we find ourselves in the position of having superb tools poised for use with inadequate material to use them on; we have the knowledge and the patterns to create a suit of armour, but, often, hardly enough material at hand to simulate a fig leaf.

One solution to this dilemma is simply to claim, anyway, that we can make, and have made, a suit fit for an emperor. We may find claims repeated throughout Marshall's writing which almost give the impression that he was prepared to content himself with this solution; but Marshall's personality was not well suited to such a course. Hence the ambivalence which is expressed, on the one hand, in his statements that economics can be made a (relatively) exact science; balanced, on the other hand, by his refusal to accord much respect to any of the techniques which could be used in such a programme. So we return to the practical, if modest, conclusions which we saw in the quotations from Marshall which were numbered 2, 3 and 4 in this chapter. A summary statement of these conclusions is the following:

> **16** The law of gravitation states how any two things attract one another; how they tend to move towards one another; and will move towards one another if nothing interferes to prevent them. The law of gravitation is therefore a statement of tendencies.
>
> It is a very exact [i.e., precise] statement . there are no economic tendencies which act as steadily and can be measured as exactly as gravitation can .
>
> The laws of economics are to be compared with the laws of the tides, rather than with the simple and exact laws of gravitation. For the actions of men are so various and uncertain, that the best statements of tendencies, which we can make in a science of human conduct, must needs be inexact and faulty. This might be urged as a reason against making any statements at all on the subject; but that would be almost to abandon life . since we *must* form to ourselves some notion of the tendencies of human action, our choice is between forming those notions carelessly and forming them carefully (*Principles*, pp. 25–7).

The tension expressed here is, again, between accuracy and precision: if we try too hard for precision in our description of 'the tendencies of human action' we will veer away from accuracy, for, in the real world, these tendencies are not very precise.

These conclusions, if true, are discouraging. We should not let our discouragement make us forget the other side of Marshall's insistence: that this does not mean we should give up the attempt to understand the world, even to understand it 'scientifically'. However, what is 'scientific' in the social sciences may have to be differently defined - may need to draw on a different mix of human mental capabilities - than we find in the physical sciences. The latter have often been used as a model for the social sciences. This approach can only be carried so far; then it becomes necessary to strike off in some new directions.

Since Marshall's time the search for the particular kind of rigour sought in economics has created a situation in which economists can use their tools and techniques most effectively in an analysis of comparative statics, which compares a series of equilibria defined as long run states. There the emphasis is upon equilibrium as a state – a goal – something hypothetically to-be-reached. It is the 'arrived at' version of equilibrium whose characteristics are quantified, defined, and compared with other 'arrived at' versions in other long runs.

An example of the realities which are likely to be hidden by such an approach is the possibility that Keynesian 'underconsumption' may again emerge, at the end of the twentieth century, as a global problem. It may be that the deficit-financed consumption levels of the United States conceal a trend for productive technology to outrun effective demand. One reason why Keynesian economics has never been really well integrated into mainstream neoclassical economic theory (the so-called 'Keynesian synthesis' notwithstanding) is that the hypothesis just suggested flies in the face of the established theory. Underconsumption *should* not be a possibility because productive technology should not be able to outrun consumer purchasing power. Price adjustments *should* always bring supply and demand into balance so that the market will clear.

Standard neoclassical theory shows that this string of statements *must* be true: in the long run the *shoulds* become *wills*. What the theory has never achieved is a precise specification for when the long run, with the anticipated equilibrium conditions, may be expected to arrive. It is possible, even likely, that, in some areas, it never will; for new disequilibrating circumstances will keep occurring before the old ones have been worked out. Underconsumption can be neglected as a 'transition phenomenon' by neoclassicists who (because this is what their techniques allow them to handle) are more interested in the previous, and the next, *state* of equilibrium than in the transition *processes* between them. (One thinks of the Red Queen offering Alice as wages 'jam yesterday and jam tomorrow – but never jam today.') Transition phenomena must, however, be of considerable interest in the real world, where a decade, a lifetime, or (in some cases) the entire imaginable future may be seen as a series of transitions; but they are not in transition to – that is, they will never reach – the theoretically predictable equilibrium.

It may be argued (this is, indeed, one justification for the comparative statics approach) that long-term trajectories have to be plotted at least enough to permit the use of the calculus to derive the direction of change in the instantaneous 'now'. However, the methodology which we have imposed upon ourselves, and which, with its requirement for conceptually quantifiable functions, serves as an additional constraint, may in any case be fancier than necessary; its precision outruns the accuracy of our knowledge. For example, as discussed above, change can only take place over some finite stretch of real time. Nothing can actually happen in instantaneous time: the addition together of any finite number of instantaneities will not take up even a second of real time.[16] Given this reality, it is often sufficient, in the relatively unusual cases where the change in question is amenable to representation in functional form, to use the less sophisticated method of arc estimation of the direction of change.

A companion to the complex of problems relating to the central concept of equilibrium as a goal-state is another complex involving over-emphasis on what happens at the margin; the definition of marginal activities as, generally, 'instantaneous', i.e., not happening through time; and the tendency to push marginal analysis in the direction of this kind of definition in order to make it amenable to the calculus.

Each of the pieces that has been listed here as trapped in the requirements for the mathematisation of economics needs to be shaken loose: more attention needs to be paid to infra- and extra-marginal economic activities and states of being; and some marginal analysis could usefully be reworked within a 'fuzzier' notion of the meaning of a margin, not specified as an infinitesimally thin slice -- of time, or money, or whatever. The calculus or more sophisticated techniques are not problems in themselves, but their use both diverts our attention from what we are not managing to achieve, and tempts us to go in the direction of the areas wherein they can be most helpful.

One implication, then, of the problems of complexity, time and change in economics, is that we will come closer to an ability to analyse their effects by the use of *less* rather than *more* sophisticated and difficult mathematical techniques. Emphasis upon:

> finite, definable time periods, rather than the humanly inexperienceable instantaneous point of time;
> the knowable present rather than the never-reached long run; and
> the process of change rather than the putative (but virtually never-reached) equilibrium goal of that change;

all of these bring us away from the abstractions of theory in the economic sense (as mathematical modelling), and towards observable aspects of the real world.

Marshall evidently suspected or even assumed this to be so. Here was one of the places where he made it clearest that, at this fork, he would not continue down the neoclassical path. Given a choice between an elegant and precise model which could not deal accurately with the realities of complexity, time, and change, versus a rougher system, less fully worked out in analytical terms, which would at least recognise the existence of the realities it could not systematise, Marshall's most frequent preference was for accuracy over precision.

This has not been the choice of the neoclassical economists who, coming after Marshall, have preferred precision at almost any cost to accuracy. The reason why the mainstream in the field defines itself as it does today requires some further exploration. That will be the subject of the next chapter.

Notes

1. Andrew M. Kamark, *Economics In the Real World* (University of Philadelphia Press, 1983) p. 2.
   Aspects of this topic have been recognised at least since the time of Aristotle, who said, 'It is the mark of an educated man to look for just as much precision in each enquiry as the nature of the subject allows' *(Nichomachean Ethics*, 1.3).
2. *Principles*, p. 32. This passage was written for the first edition of *Principles*, and retained throughout the seven succeeding editions, essentially unchanged except for the deletion, in the 3rd edition, of a footnote regarding the roles of science versus common sense.
3. Quoted in A. C. Pigou, *Alfred Marshall and Current Thought* (Macmillan, London, 1953) pp. 16–17.
4. A. Marshall, 'Mechanical and Biological Analogies in Economics' (1898) quoted in *Memorials*, p. 313.
5. Quoted in Philip J.Davis and Reuben Hersh, *The Mathematical Experience* (Birkhäuser, Boston, 1981) p. 244.
6. Wesley Salmon, 'Introduction' to *Zeno's Paradoxes*, (ed.) Wesley C. Salmon (Bobbs-Merrill, Indianapolis, 1970) p. 24. I am grateful to Martha Nussbaum for bringing this book to my attention.
7. A fair example of this meaning of equilibrium, taken from a standard microeconomics course at MIT, is the following:

   > Realistically, of course, the establishment of the new equilibrium may take an appreciable time; and, in the meantime, other changes of data are all too likely to occur. This means that full equilibrium is never actually reached. But it is still true that our understanding of the laws of

motion of a dynamic economy must be based, directly or indirectly, on concepts of static equilibrium. Thus we may visualise the constantly changing economy as always moving in the direction of the static equilibrium, even though that equilibrium may itself be constantly moving. In this manner we can understand why actual prices and quantities are rising or falling, if they are constantly in the process of adjusting toward the moving target of their respective equilibrium magnitudes (Robert Bishop, *Lectures on Microeconomics*; in mimeo).

'. This idea will return frequently, most often tagged by Marshall's phrase, 'long trains of deductive reasoning'.

). Cf:

> Consider, for instance, the balancing of demand and supply. The words 'balance' and 'equilibrium' belong originally to the older science, physics; whence they have been taken over by biology. In the earlier stages of economics, we think of demand and supply as crude forces pressing against one another, and tending towards a mechanical equilibrium; but in the later stages, the balance or equilibrium is conceived not as between crude mechanical forces, but as between the organic forces of life and decay . . .
>
> The balance, or equilibrium, of demand and supply obtains ever more of this biological tone in the more advanced stages of economics. The Mecca of the economist is economic biology rather than economic dynamics (A. Marshall, 'Mechanical and Biological Analogies in Economics' (1898) in *Memorials*, p. 318).

0. It is interesting to compare this passage with the one from Bishop, in n. 7, above. In many respects, of course, they are pointing out the same things, giving the same warnings. The difference is that Marshall is more reluctant to see 'equilibrium' as anything *but* a process.

11. See the passage from Batie quoted on pp. 266–7 below.

12. Mark Blaug, *Economic History and the History of Economics* (New York University Press, 1985) p. xviii.

'3. We will return to this subject in Part III. The literature in this area is immense. For a good summary I would refer the reader to Blaug's chapter on 'Marshallian Economics: Utility and Demand' in *Economic Theory in Retrospect*, 4th edn (Cambridge University Press, 1985; first printed in 1962). See especially pp. 350–1:

> The effort to link utility to demand in the Marshallian manner via the 'law of satiable wants' is beset by two difficulties. When we replace additive by generalised utility functions, the law of diminishing marginal utility does not furnish 'one general Law of Demand'. Moreover, a generalised utility function robs us of any operational procedure for the cardinal measurement of utility. With the elimination of cardinal measurement, the very notion of deriving *diminishing* increments of utility from additional units of a good loses all meaning and it is no longer possible to make statements about the welfare effects of a change in price.

> No wonder then that Marshall tried to simplify his argument by the device of holding MUe [the marginal utility of money] approximately constant.

14. Cf. the italicised passage in quotation no. 7 above and the quotation from Blaug on p. 394 below.
15. A. Rosenblueth, N. Wiener and J. Bigelow, 'Purpose and Teleology', *Philosophy of Science*, 10, p. 23.
16. See Chapter 11, below, for the concept of a 'pass-through limit': this will be a useful way of understanding both the approach to time described here as adapted to the methods of the calculus, and also the neoclassical use of the ideal concept of equilibrium.