

# 10 The Micro Foundations for Textual Analysis

I have spoken of the sciences as attempts to map onto some 'communication' a scientist's perceptions, observations, or beliefs regarding the nature of the real world. (As mentioned previously, the 'communication' considered here may also include what goes on inside the mind of a scientist, even in solitude, as s/he tries to order observation into understanding.) In the case of the natural, or physical, sciences, it is the physical nature of the world that is of concern; in the case of the social sciences, the subject is the behaviour of human beings, usually in the context of human societies.

Behind all of the issues to be considered here is a concern with the way the sciences reflect reality. This concern emerged in Chapter 8 as we looked at the way that the meaning intended by the 'creator' of a text is transferred to its 'recipient'; and in the question of what kind of belief, or knowledge, the social scientist (or, indeed, anyone) accords to the claims of science. In this chapter the concern with the way the sciences reflect reality will be reflected in some discussions of the process of abstraction whereby human minds (and sensory functions feeding into the mind) process information about the world, and of the tension between empirical and theoretical attitudes. The chapter which follows will then continue with a discussion of some dangers that beset a theory when certain kinds of unreality are imported into it. The last-mentioned subject has special significance for the study of economics, because it goes against several decades of theoretical work that has been carried out on the claim that only the output of economics was important; the realisticness of the input did not have to be examined.

Returning to the carriers for the mapping from 'real world' to 'communication': words, models, and theories are what most obviously serve as such carriers. This chapter will begin with a consideration of these carriers as the essential building blocks for the communication which occurs between the creator and the recipient of a text. It is worth taking a little time to look at how words and symbols get attached to the things and ideas which they are intended to represent. They are not, in some simple way, directly engendered

out of these things and ideas, but are associated with them by a connecting chain composed of several links.

**THE CHAIN OF CONNECTIONS FROM THING OR IDEA TO WORD OR SYMBOL**

Figure 10.1 attempts to schematise, if crudely, some of the links in the chain that was just described. This figure depicts only a small piece of the full process on which we depend for virtually every interaction with the world. Note that steps (c) and (d) represent a part of the process of translating from world to thought (or 'fact') which is largely biological, but which is as necessary as any intellectual part of the process. Much is required on the biological level alone to interpret the distinct impulses of the relevant neural receptors so that they present to the mind a concrete, integrated image such as 'a tree'; this complex process is symbolised, in (d), by the action involved in turning the 'upside-down' picture created by the lens in the human eye, at one point in the process, into a 'right-side-up' image whose 'top' and 'bottom' will correspond with where we feel those parts to be when we put out our hands to confirm with our sense of touch the messages from our visual receptors.

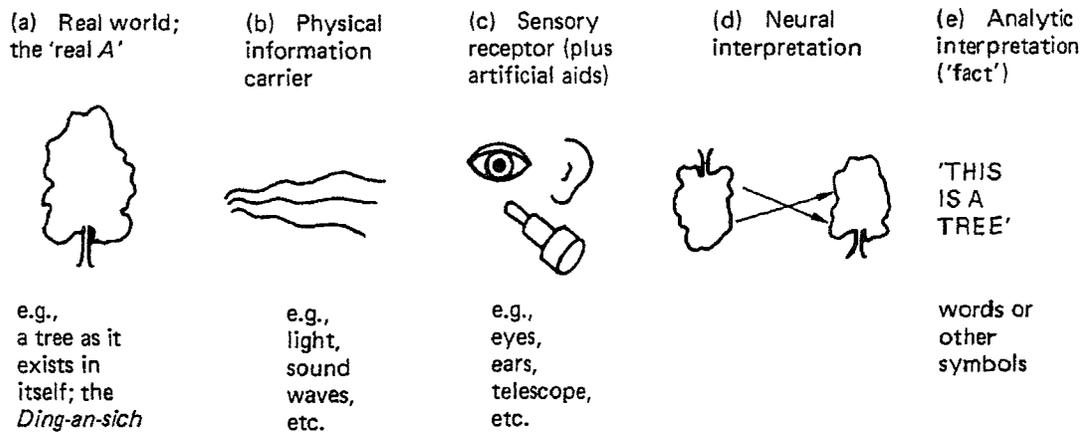


Figure 10.1 Part of the process of abstraction from world to word; the initial perceptual process

The raw data of the world is translated into 'facts' only with the assistance of experience, generalisations from experience, and something akin to theory. A certain level of sophistication is required (one which is, evidently, found in many other living species besides Man) to recognise that oaks and pines and dogwoods all belong to the same category, 'tree'. As our categories become more complex and meaning-oriented – 'hardwood tree', 'commercially useful hardwood',

‘unhealthy specimen of a commercially useful hardwood’ (‘meaning’ includes, but is not limited to, use) – it becomes clearer yet that, even just to put our observations of the world into words, theory is essential.

The purpose of indicating the extent of what is involved is to drive home the fact that words are often not just the *building-blocks* of theories, but are also the *results* of theories. The process shown in Figure 10.1 already takes a huge jump into abstraction – depending upon what could be thought of as ‘previously held theories’ – between steps (d) and (e). Yet the analytic interpretation – step (e) of Figure 10.1 – occurs, most of the time, on the unconscious or reflex level.

One definition of a theory is **a system of generalisations and abstractions employed to make sense of and connect together in the mind the raw details of the world as they are received by our senses and presented to the intellect by (largely unconscious) analytic interpretation.** I will return, later in this chapter, to consider a little more fully what ‘theory’ must mean as contrasted with ‘empiricism’. First, however, it will be useful to look more closely at some of the things we do as we create and employ the words and symbols which are both the building blocks of theory and, very often, the frame for our empirical observations.

#### LOOKING

#### CAREFULLY AT DEFINITIONS

In the parts of this book where there is textual analysis of the work of Alfred Marshall or of other economists, considerable attention is paid to definitions of certain words which have been used as though their meaning was straightforward when it turns out, upon close examination, that they are being used differently by different writers, or in different contexts. Such close attention makes any science a more effective tool; it is especially needed in the social sciences whose vocabulary is more dependent upon everyday speech, with all its richness and ambiguity. The upshot of such close attention need not be a departure from common speech (that way jargon lies), but it may lead to using ordinary language with heightened care and consciousness.

‘Value’ is an important example, in economics, of such a dangerously multi-meaning word. Another situation where words have a special economic meaning, different from the common one, is exemplified by ‘desire’ or ‘demand’. When employed by economists these words often (though not always) imply *effective* desire or demand, i.e., that which can be backed up by purchasing power. As with ‘value’, these words can be pivots upon which the level 3 operations can swing to employ *either*

the formal economic meaning (likely to be claimed on level 1), *or* the everyday meaning (which may help create the ‘general tone’ of level 2).

Similarly, in Part III, I will comment on the importance of the placing of the apostrophe in ‘consumer’s (versus consumers’) surplus’. Depending upon where it is placed, we have two terms which sound identical and look very similar, and yet pose very different problems. Failure to be clear about which term is the one under consideration glosses over the enormous difficulties of the aggregation issue, with all the problems of interpersonal/intertemporal comparisons that it raises.

A related, but not identical, issue is the way in which, within a scientific discipline, word usage may be distorted from common speech in order to maintain a particular status ranking in a field. A salient example of this is the way in which ‘theory’ in economics has been wrested away from its common usage. In general speech, as suggested earlier, ‘theory’ connotes a system of generalisations and abstractions employed to make sense of and connect together in our minds the raw details of the world as brought to us by our senses. As employed by most economists, however, the term now refers to a very specific subset of that general concept: a particular kind of mathematical modelling technique. Most people, if asked, would agree that ‘theory’ is a good and necessary thing; by a semantic imposition, the mathematical modelers in the field of economics have managed to gain a virtual monopoly over claims to ‘theoretical’ strength.

With respect to each of these issues, the first and best defence for both the critic and the creator of social science texts is a sensitivity to the meanings of individual words and phrases, and a habit of scepticism over whether it is really such a short step as it appears, to go from the meaning of one word or phrase to another which appears similar: e.g., from ‘demand’ to ‘effective demand’, or from ‘consumer’s surplus’ to ‘consumers’ surplus’. Similarly, it is helpful to note when a word, especially an abstract noun, may have importantly different uses, and to watch for unannounced ‘pivoting’ about such a word from one use to another.

**CONSIDERATION  
OF A SPECIAL  
CLASS OF  
ABSTRACT  
WORDS:  
‘UNICORN  
WORDS’**

This chapter began with a symbolic representation of how symbols relate to reality. The underlying assumption evidently is that there *is* something which is ‘real’: that we can talk about reality, and define ‘truth’ as a good match to reality, and that we can seek truth – though not assuming that we will know ‘for certain’ when we have found it. All of this

presupposes the philosophical position which, in Chapter 8, was described as ‘truth realism’ (see the section on ‘The Standards Required for Scientific Belief’).

The discussion of reality could be refined to a debate about different kinds of reality – about what it would mean to say that *goodness* is a word that refers to something real; or *happiness*, or *death*, or *life*. I myself would contend that there is some kind of meaning (here I am slipping in an implied relationship between ‘reality’ and ‘meaning’) to a statement such as: ‘I have a happy life’. I would have more trouble with a statement about the habits of unicorns; to be meaningful, that would have to be understood in a different context, not intended to represent reality.

This subject is taken up in *The Mathematical Experience*, where reality is discussed in terms of *what exists* (or, the old pre-Socratic concern, ‘what is’). ‘The unicorn’, it is said, ‘as a literary legend exists. As a zoological blueprint it exists. But as a live creature, which might potentially be caught and exhibited in a zoo, it does not exist.’<sup>1</sup> Some words or phrases are of a type which may be called ‘unicorn words’: we all know what they refer to, but that to which they refer is something invented by human beings – it is not to be found in the real world, outside of the human mind. As we shall see in the examples that follow, ‘utility’ is a good example of a unicorn word; so, for another example, is ‘general equilibrium’. Consider:

There exists the word, ‘utility’; and there exists an ordinary conceptual understanding of what the word is supposed to mean, so that two people can hold a conversation in which that word is used, with some hope that they will emerge from the conversation with a similar understanding of what had been communicated. Yet neither the existence of the word, nor the ordinary, shared understanding of what the word is supposed to mean, guarantees that anything in the real world has an existence corresponding to it.<sup>2</sup> (If there is any doubt about what this passage is intended to convey, try rereading it, substituting the word, ‘unicorn’ for the word ‘utility’.)

It is possible that the term, ‘total value’, which will be employed quite freely in this book, should nevertheless also be regarded as a unicorn word (or phrase). The word ‘total’ implies measurability; if value (in the sense which I have indicated by saying ‘human value’) is

immeasurable, then their joined use in a phrase is at least an oxymoron. This may be another way of understanding Marshall's difficulties with consumer's(') surplus, which caused him to conclude that it was 'a theoretical but not a practical tool in the economist's workbox'.<sup>3</sup> Consumer's(') surplus is, in effect, the difference resulting from subtracting the practical datum of exchange value from a theoretical construct ('total value') which is either an oxymoron or a unicorn phrase; subtracting the concrete from the undefinable leaves another undefinable.

All of this is not to say that unicorn words should never be employed. A good example is  $i$ , defined as  $\sqrt{-1}$ . The usefulness of this concept is undeniable, and, indeed, it turns up frequently in formulas which express truths about e.g., the geometrical relations of shapes which we have observed in the real world (cf. Euler's formula,  $e^{ix} = \cos x + i \sin x$ ). I am not about to suggest that the unicorn nature of  $i$  means that we should ban it, but it is important, under some circumstances, to remember that  $\sqrt{-1}$  does not exist in nature. For example, suppose an economist performed a calculation intended to predict inflation as a function of twenty variables, and  $\sqrt{-1}$  got into the computations somewhere along the way, and then stayed in. If the economist then went to a policy-maker and said, 'We've plugged your data and your objective functions into our model, and can state that, with the given parameters, in twelve months the rate of inflation will be  $15.6i$ ' – it would be obvious that something had gone very wrong.

Startling as it may seem, if you think carefully about this you will probably agree that 'infinity', like  $\sqrt{-1}$ , is also a unicorn word. Davis and Hersh take up this issue:

Mathematics, then, asks us to believe in an infinite set. What does it mean that an infinite set exists? Why should one believe it? In formal presentation this request is institutionalised by axiomatisation. Thus, in *Introduction to Set Theory*, by Hrbacek and Jech, we read on page 54: '*Axiom of Infinity. An inductive (i.e. infinite) set exists.*' Compare this against the axiom of God as presented by Maimonides (Mishneh Torah, Book 1, Chapter 1): '*The basic principle of all basic principles and the pillar of all the sciences is to realise that there is a First Being who brought every existing thing into being.*'

Mathematical axioms have the reputation of being self-evident, but it might seem that the axioms of infinity and that of God have the same character as far as self-evidence is concerned.<sup>4</sup>

Even more than ‘the square root of minus one’, infinity is a critically useful idea, but it is not something that is found in nature. There are a lot of blades of grass, and a lot of stars, but not an infinite number of them. The same may be said for molecules, atoms and subatomic particles – even if the latter are to be understood as probabilities, rather than as entities. A distance is, in theory, infinitely divisible, but theory is not identical with reality until proven to be so, and no distance ever has been divided into an infinite number of infinitesimal segments. The most infinite thing I know of is the gap between the real, finite world and the theoretical infinite. And that one is also a theoretical, not a ‘real-world’, infinity.

While it is an essential mathematical concept, infinity is also a dangerous one, because its ‘unicorn’ character is so seldom recognised. This is especially so in the field of economics, where mathematics is widely employed as though a mathematical proof were a proof about the real world, and where economists often fall into the danger of talking, and advising, as though the difference between ‘almost infinity’ and ‘infinity’ were not, itself, infinite. (For further discussion on this issue, see the next chapter.)

**PREEMPTIVE,** Amitai Etzioni gives some poignant examples of the  
**PERSUASIVE** way that a dominant paradigm, by definitions and by  
**AND** choice of words, can preempt the ground on which  
**PACKED** discussion is to occur (this is, indeed, an aspect of the  
**DEFINITIONS** art of rhetoric):

Intellectual circles in Europe were preoccupied for more than a century shadow-boxing with the ghost of Karl Marx, trying again and again to show that history is not dominated by economic or materialistic factors, that ideas matter. Similarly, social scientists and attending intellectuals, on both side of the Atlantic Ocean, have been preoccupied – and still are – with extolling, questioning, and attempting to shore up the notion of Rational Man (or *homoeconomicus*). Indeed, even those who challenge this notion often define their position in terms of various deviations from the rational model. This is evident in the frequent references to their concepts as dealing with a residual realm, the ‘non-rational’, rather than a category that can, by itself, be positively defined. Moreover, non-rationality is often confused with irrationality and tends to carry a negative connotation. *‘The trouble is that once one starts to talk*

*about rationality, it preempts the way we organise our views of human thought and behavior. We tend to think always in terms of default from a standard . . .*<sup>5</sup>

Neoclassical economics is full of examples of such preemptive definitions, from 'rigour' and 'theory', to 'free trade' and 'perfect competition'.

The latter two are also good examples of what the philosopher, Charles Stevenson, has called 'persuasive definitions'.<sup>6</sup> 'Perfect' and 'free' are not neutral words; they are morally loaded to be persuasive on the point that, e.g., this kind of competition (as opposed to 'impure' or 'imperfect' competition) is something desirable, something the achievement of which would make us feel, 'There! We've finally arrived at the place we were trying to get to!'

It is interesting that it is hard to find a description for the kind of competition economists want to talk about which is not morally loaded, if not in the 'good' direction, then in a way which makes it sound 'bad'. 'Untrammelled' and 'unfettered' competition make us think of 'Nature red in tooth and claw'; they imply that competition *should* be trammelled or fettered. If we use these terms we risk aligning ourselves with those who feel that competition isn't nice, and who have a nostalgia for some state of society (feudalism?) where everyone knew his/her place, and relations were so regulated as to leave little scope for competition. The most neutral term I have been able to find is 'unrestricted competition'.<sup>7</sup>

Closely linked to persuasive definitions, social science abounds with (indeed, one could say it is largely made up of) definitions which carry a large freight of assumptions. In accepting such definitions we are, unwittingly, accepting the assumptions packed into them; and as questions are formulated, using these definitions, they smuggle in as assumptions a part of what we might have expected would emerge, after analysis, in the answers. To take one example, a seemingly straightforward question about how a given policy would affect inflation may hide, in the terminology used, the myriad ultimately normative assumptions that were necessary in order to *define* inflation. The definition is normative because prices rarely move with perfect uniformity; to extract a general change in price level from various prices moving at different rates, sometimes in different directions, it is necessary to accept conventions regarding the assignment of weights.

'Conventions regarding the assignment of weights' sounds neutral enough, but underneath such conventions must be some way of

determining which things are *more or less important* (either for the society at large or for the particular use to which the convention is to be put); this is the subject of value-definition. (It is also the subject of accounting, which was discussed in Chapter 2.) The *use* of a rule always looks relatively impersonal, therefore impartial; but the *formulation* of any rule for defining price levels implies decisions about how to weight the marginal evaluations of different groups of people – where the marginal utility of money, let it be remembered, varies across individuals with different levels of income (it is likely that it varies within income levels, as well). For instance, a rule specifying that ‘the weight is to be the commodity’s share in GNP’ makes the implicit decision that the preferences of the rich are to be taken more into account (because their expenditures are larger, per person) than the preferences of the poor; and it determines how industrial and governmental expenditures are to be weighted in relation to personal expenditures. ‘One dollar, one vote’ is a rule which, once established, may be impartially applied; but its establishment is not impartial. (We will see more of this subject in Chapter 16, as ‘Fallout from the Aggregation Problem’.)

Aggregation is one of the most intransigent problems in welfare economics. Some aggregation-related problems can be understood as arising initially out of a failure to specify the concrete issues which have been dealt with too exclusively on an abstract level. Given an individual case, and enough facts, there are many situations in which economists can specify how a particular policy will impact upon a particular family or firm, or upon a carefully defined, relatively homogeneous group or class of individuals. What they cannot do – and perhaps should not be expected to do (but they are) – is to define the ‘net’ or ‘bottom line’ effect as a *summation of impacts that affect different individuals in different ways and to different extents*. It is, unfortunately, most frequently the case that some people are helped and some hurt by each proposed economic policy or action. Here the contemporary economist is tempted to throw up his/her hands and say to the politician, ‘*You* decide how to weight the different effects’. Unfortunately, the politician is often strongly motivated to shunt that responsibility right back to the ‘experts’ (what a science fiction writer has called ‘passing the buck on a Möbius strip’). It is questionable whether there is any training that would make a person an ‘expert’ in the subject of how to weight different people’s gains and losses; but, as this is a problem which so often confronts economists, it is one to which the profession should at least give some serious, formal attention.

**CHOICE OF LANGUAGE: TRANSLATING THROUGH MULTIPLE LAYERS OF ABSTRACTION** Marshall remarked that ‘an inference from one set of facts to another, whether it be performed by instinctive or by formal reasoning, involves not one process but two. It involves a passage upwards from particulars to general propositions and ideas; and a passage downwards from them to other particulars (‘The Old Generation of Economists and the New’ (1897) in *Memorials*, p. 298). Most of the methodology of which social scientists are aware concentrates upon the middle process – the ‘inference from one set of facts to another’. Economists today pay relatively little attention to the distance between the thing-in-itself (element (a) of Figure 10.1) and the abstractions out of which theory is created. While this whole process is so complex that no single piece of social science work, attempting to convey, examine or lead to, purportedly true statements about Mankind in society, could, at the same time, lay out the origins of each of the understandings on which that work is built – nevertheless it is worthwhile to develop sensitivity to this subject. In particular, it is helpful to note the words or symbols that serve to bridge the gap between the concrete and the abstract.

As there is a need to be clear about the precise meaning (or, if it is imprecise, the imprecise meaning – this is an important point) of each word, there is no less need to be clear about the meanings which are to be attached to abstract symbols. When abstract symbols are employed it is often forgotten that they usually are replacements for words (which are, themselves, of course, abstract symbols.) No matter how theoretic, analytical, or symbolic a piece of work in economics may be, economists must deal with the fact that symbols used alone are normally incomprehensible; in the end the meaning of each symbol or collection of symbols must usually be reducible to words. This statement is, in fact, true for mathematics and for the natural as well as for the social sciences:

Interpreting a symbol is to associate it with some concept or mental image, and to assimilate it to human consciousness. The rules for calculating should be as precise as the operation of a computing machine; the rules for interpretation cannot be any more precise than the communication of ideas among humans.

The process of representing mathematical ideas in symbolic form always entails an alteration in the ideas; a gain in precision and a loss in fidelity or applicability to its problem of origin (Davis and Hersh, 1981, p. 125.)

In the necessary going-back-and-forth between symbols and words there is additional opportunity for lack of clarity to be ignored or disguised. When a word (e.g., 'utility') has been replaced by a symbol (e.g., 'U') the dangers of unaware unicorn thinking are hidden in yet another layer of abstraction.

It is generally simpler to check a verbal statement against reality than a mathematical one; the latter must first be translated into words. For example, if one is in the middle of a set of calculations which employ the imaginary number,  $i$ , it is impossible to check this reasoning against reality until the calculations are followed out to where the  $i$  has been got rid of. This does not mean, of course, that imaginary numbers should not be used; just that this should be a recognised disadvantage, to be weighed against their other conveniences. Deductive logic does not require that the symbols it uses, including words, be paired with anything in the real world; however it is more obvious in the case of a discussion of the mating habits of unicorns that this cannot be checked against reality than in the case of, e.g., a statement that  $\log L = \log w - U(l)$ .

The verbal language with which any human being is familiar – whether it be Mandarin, English, Spanish, or Swahili – is a complex system of symbols representing concrete and abstract things, ideas, actions, states of being, etc. A mathematical language is another system of symbols. Many of the common things that are daily talked about through the use of words are relatively easily translated from one verbal language to another. 'What a nice day this is!' or 'Is political freedom essential for the full development of human potential?' can be translated from Urdu to Rumanian, and back, in such a way as to communicate approximately the same thing – although cultural as well as personal differences place (probably undefinable) limits on the extent to which any phrase does in practice mean the same thing to two different people. By contrast, these two phrases would be difficult to translate fully into a set of mathematical symbols. Some other kinds of statements, dealing with other subjects, are easier.

The fact that some things are relatively easily – i.e., concisely and accurately, and requiring relatively simple definitions – represented in mathematical symbols is one of the reasons that economists like Marshall have been attracted to that method of writing down and/or communicating thoughts.

Much the same can be said for another kind of symbolic communication, graphical representation, which may be employed to regularise the thinking in back of a verbal or other symbolic

representation. At the same time, part of the power of a graph is its ability to express the summary of a large number of ideas, assumptions, simplifications and other abstractions, all in one image. Such a summary does not usually specify all the clues to its expansion into that-which-it-summarised; many conventions go into the reading of a graph, and the conventions may change without warning from one sub-area of economics to another, even from one article in a journal to another. With a little patience one can usually discover the conventions governing the reading of a particular graphical representation, but even then one is left with unnamed and undiscussed simplifications and assumptions which may require special cautions.

The standard economics of today operates on the basis of what (if it is thought through) must be enormous optimism about the ability of economists (and not only the few very best economists) to carry in their heads, and to maintain in proper relation to one another and to reality, thick layers of nested abstractions. The discussion of this chapter is motivated by some scepticism as to whether this optimism has been warranted with regard to much of what is written and taught in the field. The function of such scepticism in a critical analysis is to prompt the critic to translate into more concrete form the meanings embedded in all the layers of abstraction of the text under discussion.

A procedure which can be useful here is to rephrase, in quotation marks, what one thinks is being said by someone else. Although as a conversational gambit this sometimes requires more explanation than it is worth (most of my friends still have not got used to my doing it), as a tool for textual analysis it can be valuable in clarifying to oneself as well as to prospective readers just what is one's interpretation of some text.<sup>8</sup>

Where the text under consideration depends heavily upon symbols other than words, the device of restatement is first of all helpful simply in translating into words critical points in the logic and the conclusions. Economists ought routinely to do this for their own work; when they fail to do so, this becomes one of the first functions of textual analysis. Even when non-verbal symbols do not carry the main burden, if jargon or some kind of 'shorthand' has been employed, it may be useful to expand the text.

It often turns out that a confident statement (in which X puts 'in other words' what s/he thinks Y has said) is not seen, by Y, as the same thing at all: or X and Z might each put Y's statement in such different 'other words' that they will discover a strong disagreement whose existence they would not have guessed if they had not attempted the

rephrasing. Discussions of a text which do not begin by stating what each of the discussants think it says are often, in effect, parallel discussions of different texts.

**THEORY AND EMPIRICISM** Let us, at this point, refer back to the complex of biological and cognitive processes, a part of which was sketched out in Figure 10.1. We saw there a little of the process of generalisation and abstraction which could result in such a simple recognition as 'this is a tree'. I would like now to stretch our understanding to see the kinship between a concept such as 'tree', and another such as 'general equilibrium'.

Both of these concepts are generalisations which are built, fundamentally, upon the biological/cognitive processes of abstraction from messages conveyed to the brain by the senses. However, one represents more steps away from the starting point in the world, towards abstract understanding.

The generalisation, 'tree', though more immediately related to the input of our sense, is still much more than 'this particular leafy thing that shades my window', or than 'that particular structure that I climbed, and fell out of, as a child'. By creating, in our minds, the category, 'tree', we bring the individual case into association with a wide range of experiences and bits of knowledge. 'General equilibrium' is the result of yet more steps into abstraction, because it is built upon so many intermediate generalisations and abstractions. It includes, for example, the idea that some human beings, in some of their acts, may be recognised as 'suppliers'; also the idea of a 'supply curve', which summarises what many suppliers would do under a variety of circumstances; etc.

It should be evident that 'pure theory' and 'pure empiricism' are each equally impossible, for reasons which begin with the way in which we, as physical beings, relate to the world. Anything that could be conceptualised as 'a purely empirical fact' can exist only outside what we think of as mind; by the time it has entered our minds so that we can think about it, it has already been massaged by something like theory. The 'fact' – 'this is a tree' – is a construct, just as 'general equilibrium' is a construct; both constructs may be held more or less in common by many beholders, but neither exists outside of the minds of the beholders.<sup>9</sup>

What, in Chapter 6, we viewed as a tension between mathematical versus intuitive methods, may now be reconceived, as a tension between deductive and inductive approaches; and these, again, can be paired with theoretical versus empirical ways of understanding. Economics has a particular relation to the idea of human welfare. An essential question to raise here is: how tight are the restrictions which the scientific method must impose upon the humanitarian aims of economics? Much of modern neoclassical economics works within very tight (or 'rigorous') restrictions indeed. The areas where these restrictions seem most irksome (and where they continue to be most often broken through by the abandonment of one or another of the requirements that go with being 'scientific') are in the economics of labor and of development, and in welfare economics. Each of these fields contains, at one extreme, theorists who for the sake of scientific rigour are willing to jettison whatever elements of reality or of humanitarian aims seem to stand in the way of such rigour; and, at the other extreme, practitioners who will let rigour go by the boards when it cannot deal with the fulness of what they see as the relevant reality of the requirements of their aims. Most theorists and practitioners, of course, lie somewhere in between these extremes. In Marshall's words, 'all are bound more or less to collect and arrange facts and statistics . . . and all are bound to occupy themselves more or less with analysis and reasoning on the basis of those facts which are ready at hand'; however, there are some people who 'find the former task the more attractive and absorbing, and others the latter' (*Principles*, p. 11).

Referring back to the 'certainty versus belief' discussion of Chapter 8, I propose a tentative hypothesis: that those social scientists who are temperamentally most desirous of *certainty* are also the ones to whom theory is the ultimate attraction; while those who are content with *belief* are the ones who are more likely to claim an empirical approach. Such preferences can only lead people to a relative, not an absolute emphasis. However, the status structure of the field of economics today lends its support to a relative emphasis which is very far on the side of theory. Marshall anticipated this danger in a letter to Edgeworth, written in 1902:

In my view 'Theory' is essential. No one gets any real grip of economic problems unless he will work at it. But I conceive of no more calamitous notion than that abstract, or general, or 'theoretical' economics was economics 'proper'. It seems to me an

essential but a very small part of economics proper: and by itself sometimes even – well, not a very good occupation of time.

The key-note of my *Plea* is that *the* work of the economists is ‘to disentangle the interwoven effects of complex causes’; and that for this, general reasoning is essential, but a wise and thorough study of facts is equally essential, and that a combination of the two sides of the work is *alone* economics *proper*. Economic theory is, in my opinion, as mischievous an impostor when it claims to be economics proper as is mere crude unanalyzed history. Six of ye one, 1/2 dozen of ye other! (Quoted in *Memorials*, p. 437).

**MARSHALL AS A MODEL IN THE ISSUE OF THEORY AND EMPIRICISM** Given that there can be neither pure empiricism nor pure theory in the social sciences, is there some ‘best’ way of realising the interplay between these two unicorn constructs? Probably not; as Marshall said, there is room and need both for scholars with an emphasis upon collection of facts, and for those with an emphasis upon reasoning about the facts at hand. The question comes down to one of emphasis. That is worth thinking about, even though we may not expect to find the ‘ideal’ emphasis. It is especially important to think about it now, in the late twentieth century, when the emphasis especially in economics, but also in some other social sciences, has swung rather far in the theoretical direction.

Chapter 7 gave a sketch of where the field is now in this regard, and how it got here. At this point I would like to take a look at where Marshall stood on the issue. Throughout this book Marshall is used as an exemplar of many different things, not all of them things which will be set forth for emulation. In this particular connection however, he is a model in the normative sense: his interweaving of theory and empiricism has not been improved upon by any other economist.

One reason for Marshall’s excellence in this respect is his methodological self-consciousness. He considered with conscious care what is involved in turning sensory perceptions into intelligent observations; observations into useful theories; and theories back into the bases for practical action.

When it came to the third of these tasks (using theory as a basis for practical action), Marshall felt, in fact, a good deal of ambivalence, which was illustrated in the sections on ‘The Social Responsibility of the Social Scientist’ and ‘The Division of Labour Between Science and Common Sense’, in the previous chapter. His concern regarding the application of economics to practical matters stemmed from two

particular worries: one was his distrust of Man's ability to predict the future, based on any social or other science; the other was his conviction that the analytical tools of science must only be servants to other aspects of Man's mental capacity.

Marshall's best hopes for prediction<sup>10</sup> related to the statistical smoothing of heterogeneity:

For our present purpose the pliability of the race is more important than the pliability of the individual. It is true that individual character changes, partly in an apparently arbitrary way, and partly according to well-known rules. It is true for instance that the average age of the workmen engaged in a labour dispute is an important element in any forecast of the lines on which it will run. But as, generally speaking, young and old, people of a sanguine and a despondent temperament are found in about like proportions at one place as at another, and at one time as at another, individual peculiarities of character and changes of character are a less hindrance to the general application of the deductive method, than at first sight appears. Thus by patient interrogation of nature and the progress of analysis, the reign of law is being made to invade new fields in both therapeutics and economics: and some sort of prediction, independent of specific experience, is becoming possible as to the separate and combined action of an ever-increasing variety of agencies.

The function then of analysis and deduction in economics is not to forge a few long chains of reasoning, but to forge rightly many short chains and single connecting links (*Principles*, p. 638).

This is about as encouraging a statement as one can find anywhere in Marshall's writings on the possibility of useful theorising of the type which represents variables from experience as symbols or numbers, and then manipulates them through a process of mathematical (essentially deductive) reasoning, to produce, at the end, a statement or prediction regarding real human behaviour in the real world. His emphasis upon 'short chains' and 'single connecting links' has, however, a meaning which is worth spelling out in greater detail.

What Marshall himself did, and what he recommended, was to intersperse the thinking which he performed at a distance from the subjects of his thoughts with a 'return' to the world. He put little faith in any 'long chain of reasoning', but felt that the conclusion at the end of each link should be tested against observation of the world. (He

himself spent much time in first-hand observation, walking about the industrial cities of England, talking with manufacturers and union workers in England and America, etc.)

This seems like a very simple piece of advice. It is, however, one whose importance for the usefulness of the social sciences can hardly be stressed enough; and it is advice which is far too seldom heeded today. Marshall himself is partly to be blamed for his. His stated goal was to 'deal with facts that can be observed and quantities which can be measured and recorded; so that when differences of opinion arise with regard to them, the differences can be brought to the test of public and well-established records; and thus science obtains a solid basis on which to work' (*Principles*, p. 22). The problem, here, was Marshall's reluctance to recognise that many of the facts of importance to a *social* science are *not* in the nature of 'quantities which can be measured and recorded' – in spite of his own recognition of the danger that economists would emphasise 'those elements . . . which lend themselves most easily to analytical methods.' (*Principles*, p. 700).

The base of a social science *cannot* be as 'solid', in the quantifiable, measurable ways Marshall wished for, as the base of a physical science. Yet this is no reason for social sciences to cut loose from experience and observation. Given the fuzziness of the initial observations on which subsequent deductions are to be built (e.g., the observations of human behaviour which have been stylised into the image of 'rational economic man', upon which so many deductions have then been erected), there is all the more reason to follow Marshall's prescription: keep going back to check whether the real world is proceeding according to the theory.

*How* frequent should this be? That is a matter of judgment. Marshall saw that an approach emphasising empiricism could also be carried too far, and he cautioned (especially when thinking of the German Historical School) that 'facts without theory are mute'. However he sensed, accurately, that the errors of the coming generation were less likely to be in the overly empirical direction than in that which was in fact taken by the neoclassical school – the masters of 'long trains of deductive reasoning'. To correct today's excesses he would doubtless counsel shortening the sections of models which are strung between observations. Social economics, following this guidance, would encourage only relatively short excursions into the mole-tunnels of perfectly abstract modelling, requiring its practitioners relatively frequently to come up for air and test their progress against what they know of the real world.

Related to the foregoing is the issue of the tension between *induction* from ‘observed’ or otherwise ‘known’ facts, versus *deduction* which uses theory (most generally understood) to go from the ‘facts of the real world’ to a set of proposed ‘facts’ which have not yet been observed in the real world. Here, again, Marshall took the correct, if uncomfortable stance, of championing both sides at once. He pointed out that each, in pure form, is impossible.<sup>11</sup> The discussion, at least for the social sciences, must be about where one chooses to be on the spectrum shown in Figure 10.2. Social economics, as it is proposed in this book, would distinguish itself from neoclassical economics by existing somewhat farther to the left on this spectrum.

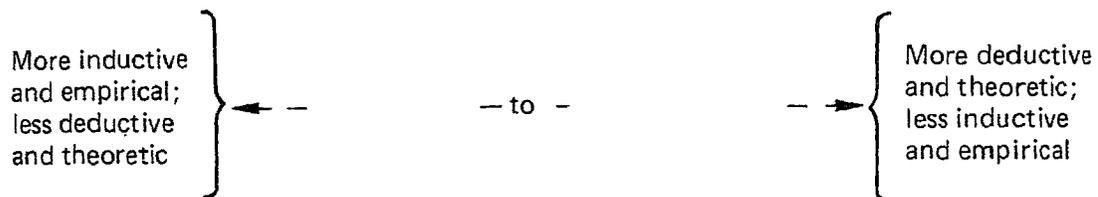


Figure 10.2 The spectrum from empirical to theoretic

What, in practice, does this mean? For one thing, a more empirical, less deductive science will have to foster different ideas and expectations about simplicity versus complexity. An area where this is particularly evident is in the matter of *brevity*, which, in turn depends upon *choice of language*.

It is often impossible to abbreviate the content of a statement of understanding in the social sciences in the same way as may be done in the natural sciences; there is no social science formula whose ‘power’ is comparable to  $E = MC^2$ . However, neoclassical economists, in their desire to create a discipline which would be ‘scientific’ like the natural sciences, respond to a very strong internal motivation (and external pressure, from their colleagues, journals, etc.) first to axiomatise, and then to summarise. Of even the most complex and subtle argument, regarding even the most difficult matters, it is widely believed that, if ‘what this economist is really saying’ can’t be compressed into a few pages, then it must not be very significant.

A prime counter example to such a belief is Alfred Marshall’s *Principles of Economics*; there is no way to boil down his 700 page book into something short which would convey ‘what Marshall is really saying’. Incidentally, the same is true of Samuelson’s fat volumes of introductory economics. The style of the latter is far less ambling, in an

idiom that is more familiar to modern audiences; but it remains true for Samuelson, as it was stated by Marshall, that ‘every plain and simple doctrine as to the relations between cost of production, demand and value is necessarily false: and the greater the appearance of lucidity which is given to it by skillful exposition, the more mischievous it is’ (*Principles*, p. 306).

‘SIMPLIFICATIONS’ As another way to describe the relationship  
AND OTHER TYPES between theory and reality, it is sometimes  
OF appropriate to say that the former is composed  
‘UNREALISTICNESS’ of elements which are simplifications of the latter.

The need for simplifying the experience of our senses in order to construct abstract models for the better understanding of the world is generally accepted – and it is accepted by economists with particular enthusiasm. One of the deficiencies in the field of economics is the lack of a well-thought-through and well-known methodology on the subject of simplification. There are at present virtually no generally accepted rules, or even guidelines or suggestions, regarding how far a social scientist should take the necessary simplifications of the experience of the real world. Economists simplify to whatever extent serves their purpose of the moment. Their work, once in print, is subject to critical scrutiny, but the criticism they receive is rarely on such methodological grounds.

In his 1953 ‘Essay on the Methodology of Positive Economics’, Milton Friedman contended that the realisticness of assumptions does not matter. In response to this, Uskali Mäki has written extensively on the general subject of ‘unrealisticness’. His categories make a useful starting point for our discussion:

Let us say that a statement is an *idealisation* if it is formulated or can be formulated in terms of a variable that is assigned the value 0 or  $\infty$ . An idealisation, in other words, involves so-called limit concepts. Notions like frictionless surface in physics and perfectly elastic demand in economics are dependent on idealisations. A statement is a *simplification* if it modifies some of the characteristics of its object so as to make it look simpler than it in fact is. Examples are spherical planetary motions in astronomy and linear production functions in economics. A statement is an *exaggeration* if the value of a quantity it attributes to its object is larger than its real value. Some idealisations are extreme exaggerations.

Idealizations, simplifications, and exaggerations are strictly false statements. An *isolation*, on the other hand, may be true of its object which it isolates from the influence of other objects for closer inspection. Physical experiments in laboratory conditions are based on material isolation, while isolations in economics are conceptual in character.<sup>12</sup>

It is commonly understood that a given theory does not aim to cover all of reality; one way of describing a theory would be to say that those parts of reality which it does intend to include are represented in the form of symbolic simplifications (e.g., words or mathematical symbols, sentences or mathematical phrases) of a small, carefully selected, *subset* of elements of reality. As Mäki points out, however, there are different ways of reducing reality to symbols, and some of them are strictly false, while others are not.

The type of 'realisticness' which I shall favour, in this section and in the next chapter, is a type which assumes that 'the whole truth' is not symbolically representable, but which makes clear distinctions between 'nothing but the truth' and something which is clearly *untruth*. As a start, such a 'safe realism' will:

- make it clear when its categories are, and when they are not, referring to real things that exist in the real world;
- though not describing all elements of the area of interest, strive for accuracy in describing the chosen elements;
- not so isolate the chosen elements from the context as to give a strictly false definition; and
- in general avoid making counter-factual statements; when these cannot be avoided, guard against them by making clear their nature, as statements contrary to fact.

Even in such 'safe realism', there will be distortions which will increase toward the margins of the areas, elements or characteristics receiving most theoretical attention. This is why there will always be competing versions, even within safe realism, none of which will be wholly true, but some of them—more useful for some purposes, some for others.

The term 'simplification' is, itself, normally understood to refer to a construct which represents some, but not all, of the aspects or characteristics of the reality which it partially reflects; it may not reflect even those aspects in their entirety, but what portions of them it does convey are conveyed faithfully. A Picasso sketch of bullfighters (using

lines as representational elements, instead of using words or mathematical notations as symbols) may convey faithfully the essential elements that strike the eye, in such a way that, although each line segment alone is virtually meaningless, the result appears as a faithful, although greatly simplified, representation of the portion of reality chosen by the artist.

By comparison, many aspects of economic theory are not in the same sense simplifications from reality; they are not reduced statements of fact (or statements of reduced fact), but are statements contrary to fact. For example, the world described in the first and second theorems of the formal theory of welfare economics is not recognisably the world we inhabit.<sup>13</sup> Here, a series of statements, mostly of the 'if . . . then' form (i.e., 'if the world did conform to these conditions, then the following further conditions would hold . . .'), have been combined to produce, at the end of a long chain of deductive reasoning, the conclusion that 'every Pareto optimum is potentially achievable through the operation of market mechanisms under conditions of perfect competition'. This is not a false statement, in the sense of containing contradictions to its own internal premises. Neither, however, is it a recognisable sketch (like the simplified Picasso drawing) of the real world.

The central problem is that the concept of a world of perfect competition is not a *simplification* from our real world; it is a *picture of a different world*. Many of the 'classical conditions' under which neoclassical theory operates are not simplifications in the sense of representing a stripped-down, but still faithful, representation of the world; often they are simplifications only in the sense that they simplify the work of the theorist.<sup>14</sup>

The same may be said of the 'single-cause/single-result' fallacy; i.e., the (perhaps in part aesthetically motivated) tendency of scientists to assign to each result a single cause, and to each cause a single result. The problem with such a procedure may be dramatised by a thought-experiment in which we imagine an important happening (a war, let us say, or a major inflation), and we imagine, also, that we are in the God-like position of knowing the 'true' causes of the happening. Let us imagine that what we know is that there were, in fact, 100 true causes, each bearing about equal responsibility. It is probable that an economist using modern techniques for testing correlations would not come out with anything approaching the true answer; and if we were to offer the true answer to the profession, it probably could not get published in an economics journal. The true answer would simply

appear too unsatisfactory; it would leave people saying, 'Yes, but what was the *real* reason?' We would be told, 'You will have to simplify your conclusions if you want to publish them.' To do so, however, would, again, be not a simplification but a distortion.

Mäki's term, *isolation*, is particularly applicable to the convenient notion of *ceteris paribus*. That Latin term asks us to consider 'all other things remaining the same' while we concentrate our attention upon a particular action or effect. Certainly there are useful understandings to be achieved by imagining a 'simplified' world wherein only one or a few changes are permitted at a time. The critical question to keep in mind is: what useful understandings are kept beyond our reach as long as we thus restrict our imaginations or our perceptions?

Additions could be made to Mäki's list of types of 'unrealisticness'. One that I would propose is *incorporated metaphors*. We can hardly speak without using metaphors; for example, when we speak of time 'flowing', we are unconsciously adopting a metaphor which regards time as a river; when we say a politician 'clawed his or her way to the top' we have metaphorically viewed the politician as a member of the cat family; etc. For rigorous philosophical speculation on the nature of time we will find we have prejudged the issue if we employ the river-metaphor, with its unidirectional flow; and for a rational debate on politics the cat-metaphor, too, states a pre-judgment. Nevertheless, ordinarily we do not feel that our perceptions have been distorted by commonplace use of such common metaphors. However, there are metaphors which sometimes get incorporated into a discipline and then treated as reality, and there the danger of distortion may be acute.

I will argue later (in Chapter 17) that an important example of this problem in neoclassical economics is the concept of the welfare function, which depends upon the implicit metaphor that the society is an individual being, and its welfare can be understood like the welfare of an individual. The graphic or other mathematical representation of, e.g., indifference maps can also be viewed as a metaphorical activity which sometimes gets out of control and is incorporated into our way of thinking as if it were a direct, not a metaphorical, representation; 'Here is the individual's (or the society's) indifference function' says the teacher, pointing to the blackboard.

The activity which I associate with all of the foregoing observations of types of unrealisticness in economics is an examination of theories to see what are the basic building blocks of which they are constructed; in this examination I recommend questioning so-called simplifications, with a distinction in mind between actual simplifications versus

distortions. A theory built on the latter, I hypothesise, is particularly prone to run into trouble of the ‘external inconsistency’ type that was described in the previous chapter, and that will be the subject of the next one. There we will be especially concerned with the type of ‘unrealisticness’ which is commonly known as an *idealisation*.

## Notes

1. Davis and Hersh, 1981, p. 146.
2. Machlup has pointed to the same issue, using the phrase ‘the fallacy of misplaced concreteness’ for the situation where a ‘thought-object’ is mistaken for ‘an object of sense-perception, that is, for anything in the real, empirical world’ (Fritz Machlup, ‘Theories of the Firm: Marginalist, Behavioral, Managerial’ *American Economic Review*, 57, p. 26).

Plato suffered from, and purveyed, a ‘unicorn word’ confusion when he first encountered a class of words which had only just been introduced to the Greek then spoken: namely, abstract nouns. His, and his audience’s unfamiliarity with such words is evident in the way that (particularly in the early Dialogues) Plato kept inserting explanations, e.g., ‘Beauty – by which I mean “the beautiful”’; ‘Justice – I mean, “the just”’; etc. Out of this unfamiliarity came the idea of what have been called ‘Platonic Ideals’ which expressed the notion that: if there exists a word such as *beauty* or *truth*, and we can comprehend and communicate what that word is intended to mean (the commonality of such comprehension is, of course, another whole subject for debate), this must be so because there exists, somewhere in the universe, an actual form or pattern to which the word refers.

In modern usage I would only think of an abstract noun as a unicorn word if it is employed in a manner similar to Plato’s: to imply that there is some *thing* to which the abstraction refers.

3. C. W. Guillebaud, ‘Some Personal Reminiscences of Alfred Marshall’, in John C. Wood (ed.) *Alfred Marshall, Critical Assessments* (Croom Helm, London, 1982).
4. Davis and Hersh, 1981, pp. 154–5. If you are not yet persuaded that the concept of *infinity* requires a special place in our mental organisation of concepts – closer to *unicorns* than to *chickadees* or even, I would claim, than to *happiness* or *hunger* – consider the following conversation between a sceptic and a person who had never questioned the realism of the concept, ‘infinity’:

Sceptic: Nothing exists in reality which has the characteristic, ‘infinity’ – there is no actual thing to which one can apply the adjective ‘infinite’.

Believer: That’s nonsense! What about the number of times you can halve the distance, say, between, zero and one?

- S.: Zero and one what?  
 B.: Well, inches.  
 S.: Here's a line an inch long on this piece of paper; let's start halving the distance from the beginning to the end point . . . after a while the area we're trying to mark off is all black, we can no longer distinguish further 'halvings'.  
 B.: The problem is that that pencil isn't infinitely sharp.  
 S.: What pencil is? *Conceptually* we could go on halving the distance forever, but in reality we cannot.  
 B.: Alright then, what about the series of real numbers; surely you'll concede that that is infinite?  
 S.: In theory, yes; in reality, there does not exist anywhere a thing to which can be put the name 'the infinite series of real numbers'. That is, while we cannot name the number that is the last one in the series, we also cannot point to anything in reality that corresponds to the infinite length of such a theoretically infinite series.  
 B.: But you know perfectly well the meaning of an infinite series: it would be expressed by our starting to count, and never stopping –  
 S.: But we *would* stop.  
 B.: Why?  
 S.: We would die.  
 B.: Then suppose we set up an institute for counting, and every generation would assign someone fulltime –  
 S.: (*helpfully*) The counter could go by millions, or billions.  
 B.: That wouldn't actually make any difference –  
 S.: You mean no one would ever actually reach infinity?  
 B.: No, but the *concept* is there –  
 S.: I'll believe it when you can show me a real example of it.

5. Etzioni, 1988, p. 93; italics added. The passage which Etzioni quotes is from Robert P. Abelson, 'Social Psychology's Rational Man', in S.I. Benn and G.W. Mortimore (eds) *Rationality and the Social Sciences* (Routledge and Kegan Paul, London, 1976) p. 61.
6. See Charles L. Stevenson, *Ethics and Language* (Yale University Press, paperback edn, 1960).
7. Although I am inclined to favour this term as one which does not prejudge the issue, it may be that the social sciences are *not* best served by simply recommending the avoidance of 'persuasive definitions'. While the intent behind their use may be to persuade, to the sophisticated reader/listener their function is also to *signal the position* (or point of view) of the author/speaker. We might ideally require of social scientists that they first know, and then state, their prejudgments or ideologies; but until that ideal is put into practice there is at least some gain in the use of words which warn us of those positions.

Marshall struggled with the semantic problem just discussed, but from a different point of view worth noting; to him the word 'competition' itself carried with it such *negative* connotations that he protested 'we ought . . . not to brand the forces, which have made modern civil-

isation, by a name which suggests evil' (*Principles*, p. 6); and then went on to say,

We need a term that does not imply any moral qualities, whether good or evil, but which indicates the undisputed fact that modern business and industry are characterised by more self-reliant habits, more forethought, more deliberate and free choice. There is not any one term adequate for this purpose: but *Freedom of Industry and Enterprise*, or more shortly, *Economic Freedom*, point in the right direction; and it may be used in the absence of a better (*Principles*, p. 8).

One could hardly find a clearer example of a persuasive definition! Marshall evidently recognised their existence, but (like most people who are aware of persuasive definitions) objected to their use only if they were persuasive in the wrong direction.

8. A good example of this procedure is found in the discussion of a paper by Muth, in pp. 92–96 of Donald McCloskey, 1985. Another illustration may also be found at the end of the discussion of Harberger, in Chapter 17, below.
9. It is, of course, the *construct* (the set of associations and generalisations which come together in a category to which human beings can give a common name – and to which other animals also have a demonstrated capacity to attach a common concept abstraction) that exists only in the minds that hold it. While we cannot prove that the *ding-an-sich* does exist outside of our minds, it seems reasonable to believe that it does. (This, again, is the philosophical position of world realism.)
10. A wish poignantly revived, in the 1960s, by the excitement surrounding the Club of Rome models, and their implicit promise that computers would be able to do what mankind had so long sought from magic. With the brief flowering of 'projection as prediction', it seemed that such magical powers really were attainable by human beings, upon attainment of a PhD in economics or in systems analysis. Some disillusionment has set in since then. On the idea that the science of economics might someday be able to fulfill the ages-old human wish to be able to predict the future, Marshall commented, in one of the last pieces of economic writing of his life, thus:

Prediction in economics must be hypothetical. Show an interrupted game at chess to an expert and he will be bold indeed if he prophesies its future stages. If either side makes one move ever so little different from what he has expected, all the following moves will be altered; and after two or three moves more the whole fact of the game will have become different (*Memorials*, p. 360; 'Fragments by Marshall'; dated 1922.)

11. Unless, as may be the case, some or all of pure mathematics may be described as purely deductive, without any necessary base in the real world. If this is an exception to the statement just made in the text, it is not one which weakens my claim, as far as the social sciences are concerned.

12. Uskali Mäki, 'Types of Unrealisticness in Economics: The Case of J. H. von Thünen's Isolated State', paper prepared for presentation at the Annual Meeting of the History of Economics Society in Richmond, Va., 10–13 June 1989; preliminary draft, p. 10.
13. These theorems are discussed in Chapter 13, below.
14. Note that this statement implies a belief about the nature of the relevant part of reality – namely, that virtually no economy that actually exists is perfectly competitive. We will press further, in the next chapter, on the question of the usefulness of describing reality as though it is approaching such an idealisation as perfect competition; there, too, we will return to the 'external consistency' issue raised here and in the preceding chapter, regarding the mapping of communication carriers (words, models, theories) upon 'the real world'.