

8 The Peculiar Place of Meaning in the Social Sciences

Here we return to some issues that were raised in Chapter 6. In some important respects the difficulties experienced by the social sciences, including economics, in coming to grips with the problems of complexity, time and change, are similar to those experienced by natural (or physical) scientists as well. In other respects they pose different kinds of challenges to these two areas of human understanding.

One theme that will unfold as we continue in Part II will be the importance of being aware of both the differences and the similarities between the natural and the social sciences as they are pressed to the limits of what they can achieve.

Another continuing theme is the distance between the world that exists and the words, symbols, models, etc. that we use to represent it and communicate about it. We go outside of the areas which neatly submit themselves to rules and formulae whenever we think about how we are to use theories, definitions, assumptions and simplifications, as ways of bridging the distance between 'the real world' and 'communication'.

(Note: The term 'communication' will be used as a short-hand reference to all the activities we undertake in which we rely upon some translation from the reality presumed to exist outside of our heads and the symbolic version of that reality which we, in some sense possess: these activities include writing and talking; reading and listening; and just plain cogitating.)

This second theme will be taken up at some length in Chapter 10. For now, suffice it to say that the qualities (skills, characteristics, or whatever) that a human being, as scientist, must bring to bear in bridging the distance between world and idea are subjects which have received insufficient attention within many fields. Social scientists, often led by economists, have pursued techniques as though that pursuit alone would build the needed bridges.

Alfred Marshall stands out among the great economists who have repeatedly called for something else – something which Marshall called ‘common sense’ but which I will generally refer to as ‘judgment’ – to play a companion role to technique. Focusing upon the activities of the social scientist, we will see (this will be the focus of Chapter 9) that judgment has a role to play in selecting the important assumptions remaining to be spelled out; the meanings most relevant or important to dissect; the words and symbols that should be looked at most carefully; the methodological points to stress; the areas where one should begin the search for logical contradiction or incompleteness; etc. A third theme of Part II will be the conscious definition of some of these critical areas where judgment is called for.

SOME PRELIMINARY COMPARISONS BETWEEN THE PHYSICAL AND THE SOCIAL SCIENCES The social and the natural sciences had a common ancestor in philosophy – an area of speculation which (from what we know of the pre-Socratics in Greece), at least as soon as it grappled with issues of meaning and morals (‘what matters’), was also trying simply to identify and begin to comprehend what’s out there – ‘what is’. In reading modern physics it sometimes seems as though the wheel has come full circle: simply the attempt to identify the essential components (if they may so be called) of the universe – energy, matter, space, time, ‘space-time’, etc. – and to comprehend their nature, often seems to engage our philosophical as much as our scientific selves.

In this respect, when we consider the difficulties which economics has in dealing with time and change, we find in the physical sciences something similar, but more conscious, and likely to be carried to a deeper, more philosophical level. Some delightful examples of the perplexities arising in this area may be found in a collection of essays, called *The Nature of Time*,¹ which was written by members of the Oxford University faculty in the physical sciences and philosophy. In Chapter 11 I shall attempt to show, in a discussion of some of the kinds of paradoxes that have been with us since the time of the Greeks, that our concepts of measurement, when applied to both time and space, break down when carried too far in the direction of certain limits. The economic problems with time that were mentioned in Chapter 6 – e.g. the impossibility of capturing the instantaneous ‘now’ in a freeze-frame that does not erase its context, time; or the difficulty of writing algorithms that do not, at some *n*th derivative, finally deny further

changes in the process itself of change (a parallel statement in commonplace language is: 'the interesting events are the ones that could not be predicted') – these reappear, in more elemental and abstract form, in the musings on time of philosophically sophisticated physicists, or philosophers with knowledge of physics.

The problems posed by *complexity* in the physical sciences are somewhat different from the problems posed by *time and change*. At the outset they resemble the struggles we have seen in economics. It was from the natural sciences (including mathematics) that the statement came, which was quoted in Chapter 6: 'the ultimate model of a cat is of course another cat'. To dramatise the meaning of this, let me describe how it came home to me.

In the 1970s I was working on a modelling exercise with the architect, philosopher and mathematician, R. Buckminster Fuller (most often remembered as the inventor of the geodesic dome, and originator of the term, 'Spaceship Earth'). His conception of the accuracy and detail of the model to be created was extremely ambitious, and his colleagues experienced much frustration in their inability to produce anything even close to what he had in mind. My own frustration expressed itself in a dream in which, to show the resources, human trends and needs of the whole Earth, Fuller's team created a three dimensional globe that became larger and larger as the necessary detail was entered. Finally in the dream it became so large that it could no longer sit on the Earth; we had to push it off into space, where it hung side by side with the original; it was at last complete when it was the same size as the original, and an exact replica in every respect. The only true model of the Earth is, ultimately, another Earth.

The usefulness, of course, (as well as the practicability) of modelling exercises rests precisely in the fact that they never come anywhere near the 'ideal' or limit case of that dream. The *ultimate* (ideal) model of a cat or an economic system may be another cat or economic system, but we conceptualise it through words, mental images, etc., that depend upon much simplification.

Here, however, the physical and the social sciences begin to diverge, for in experimental science the 'ultimate' model *is* available. Mice are more often used than cats, but both are common enough in laboratories. The physicist who wants to communicate about electrons will employ many kinds of abstractions (diagrams, words, etc.) to do so; but ultimately s/he also has the option (at least the ideal, well-financed physicist has the option) of dealing (not 'directly', to be sure, but via complicated machinery, both for finding and for perceiving the particles) with the real things-in-themselves.

The relative infrequency with which the social scientist, *as scientist*, can deal directly with the *ding-an-sich* is one point of difference which we may note. At the same time, in his/her daily life the social scientist not only deals with, but *is* an 'ultimate model' for some part of his/her subject: because the subject of the social sciences is human beings. This brings into this area of human understanding an element of subjectivity, which, I would argue, cannot be wholly excluded, however much we may try. Indeed, since what the social scientist often wants to know about is a direct result of (if, indeed, it is not in fact) something subjective like motivation, belief, thought, or emotion, and since our only direct knowledge of these things arises through introspection, it is the case that *the more successfully subjectivity is excluded, the more the knowledge base for the social sciences is constricted.*

It is, most of all, the subject matter that makes for the most dramatic differences between the physical and the social sciences: human beings (in terms of their mental and behavioural, rather than their physical existence) are the subject of the social sciences. The aspect of this difference which may be most salient – and which will be the topic for much of this chapter – is the fact that, given that the subject is the mental and behavioural aspects of human beings, then a new dimension for study comes to the fore: the dimension of *meaning*. Not all of this large subject can be explored here. The aspect from which we will start will be that of the meaning – and meanings – which have to be dealt with in creating and communicating about the subject matter of the social sciences.

CREATORS AND RECIPIENTS OF SOCIAL SCIENCE TEXTS What you, or I, know of the work of other social scientists is not what is in those other minds, but is rather what we receive at the end of the form of communication which they extend. As we start to move into this topic it will be useful to examine the possibility that there is a significant area for what could be called creativity at the receiving as well as at the originating point of communication; and that, if this is so, then we have to regard the way that we read and listen as important parts of the reality of our field. This section and the next three will position this activity within certain social sciences traditions. Starting with the section entitled 'a conscious Approach to Conscious and Unconscious Levels of Meaning', a particular critical tool, which has the potential for broad application and usefulness, will be described.

Economists communicate with each other and with the rest of the world in several ways; most notably through what they write, through formal verbal presentations (e.g., lectures), and through informal verbal presentations (e.g., conversations). Any of these communications may be regarded as texts, and subjected to textual analysis. Textual analysis is not only done in one known and stated way; at the extreme, one could claim that there are as many kinds of 'readings' as there are 'readers'. Retreating from such fine distinctions, I would nevertheless say that there is a technique, called 'discourse analysis', which has some important differences – some of them quantitative, and some qualitative – from the kind of reading that is most commonly afforded to economic texts. This technique, arising historically from the concept of 'close reading' of texts in literature studies, has increasingly been applied to 'texts' outside of the usual realm of literature-seen-as-art (e.g., to advertisements, speeches, newscasts, etc.), and then to other areas of human activity (television performances; structures – e.g., prison designs; laws; unwritten rules; institutional guidelines; etc.) which are 'read' as 'texts'.

To date, these methods have been employed largely from the critic's side of the fence. However, methods growing out of discourse analysis can also be taken to the other side, and employed by those who create social science texts; not only the writers of books and articles, but also, for example, economic practitioners.

Unfortunately, much of what has been written about, and many examples of, discourse analysis is arcane and jargon-ridden. However, the essential features of possible use to social scientists need not require a lifetime of language study to master. An additional goal of Part II will be to set out a simple and preliminary codification, or set of guidelines, starting on the critic's side of the fence, which can be employed on the other side as well, to assist social scientists in general, social economists in particular, to gain the self-consciousness which is the key to reaching some new relationships between goal and analysis; between theory and fact; between academic and empirical science.

Social science texts attempt to lead to, convey, or examine, purportedly true statements about human beings in society. In the spirit of Donald McCloskey² I claim that the quality most sought for in such texts is credibility; the effectiveness of an author is shown in the degree of credence, or belief, which s/he can claim from the recipients of the text. (Note: 'most sought for' here does not mean 'best' – it means no more than it says.)

The credibility/credence nexus in a social science text connects its creator with its recipients in a somewhat different manner than either the aesthetic element of a novel or a poem, or the invitation to repeated experimental proof in a natural science text. The types of communication involved in the three broad areas of scholastic discourse – arts and humanities, social sciences and natural sciences – is sufficiently different so that we should expect to find that the most useful types of discourse analysis will also fall into three general types, along the same lines of division. What will be offered here is a preliminary outline of some of the sorts of activities (including but not limited to activities which might go under the heading of ‘discourse analysis’) which will be used in this book (especially in Part III), as an approach which is particularly useful for the social sciences.

Specifically, I will name, distinguish between, and, to a limited extent, describe, a number of the different types of critical activities which I have employed in reading texts by Marshall and other economists, in my endeavour to learn some of the important, less obvious things about economics in particular, social science in general. This discussion will be written in a more personal tone than most of the rest of the book, as it will draw, for examples of procedure and methods, on my own experience with the material to be analysed later in the book.

THE FIRST TOOL OF ECONOMICS: THE MIND OF THE ECONOMIST A fundamental question will be concerned with the very basis upon which economics over the past half century has been structured. When one stops to think about it, there is something more than a little odd about the way this discipline has chosen to build itself, as though starting from a vacuum of knowledge about its subject. The very idea of axiomatisation in a social science is so counter-intuitive (for all that we have become accustomed to it) as to deserve a reappraisal.

The normal way for the human mind to work is to take each new piece of information or new speculation and evaluate it for truth, or usefulness, or other merit, *against the background of a life's experience*. The whole life's experience is not consciously present, but a tremendous amount (there are no really solid guesses as to how much) is ‘stored’ in some way, so that correspondences between the new input and the stored material call forth judgments on the new information.

By contrast, the economic view of the human being, since Marshall's time, has operated as though starting from nearly complete ignorance on the part of the economist him/herself. During the early period of modern formalisation a pitifully small amount of ideas about human motivation and behaviour were permitted to be 'known' in the consciousness of the field of economics. Then, as though these things were being discovered for the first time, an odd lot of singular observations about human nature was grafted on, in a rather haphazard way. The basic assumption of rational maximisation of self interest was padded out, here and there, with the 'rotten kid theorem'; the idea that people might choose to exercise 'exit, voice or loyalty'; 'bandwagon' and 'snob' effects; situations that could be named a 'prisoner's dilemma', or characterised as 'moral hazard'; etc.. What a curious, and incomplete collection! It does not seem likely that simply extending this collection is the way to achieve any kind of sufficient completeness.

The economic approach to human nature, in fact, has been to behave as though engaged in an effort to programme a computer to predict behaviour. A computer only knows what it is told. Nothing can go directly from the human unconscious into the computer's works: all that it receives must be delivered via programming (which is a conscious, intellectual activity) out of conscious human knowledge. In their areas of strength, computers can outperform any human mind. What they do not have access to is the stored experience of a human unconscious.

Even if a computer were programmed with far more than the above-cited collection of memorable names for bits of behaviour which have been economists' recent additions to the basic behavioural assumptions – even if, for example, it were fed all the factual descriptions of human motivation and behaviour contained in all the psychology, sociology, anthropology and history textbooks in existence (and were given some way to sort and prioritise the conflicting information contained therein) – it would not possess the ability to understand and respond which exists in human beings. The human ability to understand and respond which is different from, and more powerful than, the computer's ability, derives from that unconscious part of our mind which we cannot translate into a computer programme.

The point of this comparison is to suggest that the way we have gone about developing our 'human sciences' may not be the way that can best take advantage of our starting point – ourselves. It is not to say that we should not use computers – of course we should – but rather

that we should also use ourselves, and we should not use ourselves as though we were computers.³

In formal neoclassical economic work we are using only a scant fraction of what we, as individuals, understand about the common subjects of the social sciences: human motivation and behaviour. In recent years the analogy with computer sciences has lent weight to the idea that all assumptions and all knowledge about human motivations and behaviour can, and should, be made explicit in any scientific work: that which is not made explicit is presumed not to exist (i.e., our models are assumed not to tap into anything like 'unconscious belief'). The attempt to impose this particular kind of 'rigour' throws away so much information that it is worth questioning whether the gain has been worth the loss.

MATHEMATICAL MODELLING When confronted with an economic problem, the approach of neoclassical economics is to 'apply a model'. By contrast, the approach of social economics will be, first, to apply a trained intelligence.

What is the ideal of a model, as currently employed in economics? It is a set of assumptions which, in the most stringent modelling exercises, are supposed (a) to be exhaustively spelled out, and (b) to comprise the totality of knowledge/understanding about the world which is to be included in the particular modelling exercise. Normally, in fact, both (a) and (b) are impossible. (See n. 3.)

The models which are generally considered the most sophisticated examples of modern neoclassical economic reasoning may be accurately described, in Marshall's words, as 'long chains of deductive reasoning'.⁴ They normally start with a set of simplifications which would be considered merely absurd if it were not so apparent that, for this type of reasoning, such simplification is essential. The inputs from reality, thus stripped down, are then manipulated through long, often impressively difficult, mathematical exercises. What emerges at the end cannot be expected to bear *more* relation to reality than what went in at the beginning; frequently, some of what relevance there was to start with has been lost, as when, for example, highly stylised behavioural assumptions are employed recursively to demonstrate the effects of the passage of time. If the results of each iteration (to take a typical example) are taken as the starting point for the next period, while important influences have been left out through the necessary simplification process, and the behavioural assumptions were only a fair representation of reality under very limited and special circum-

stances to begin with, the multiplication of errors can take the conclusions a great distance away from reality.

There are, of course, some good uses for modelling techniques.⁵ Short and simple models (eschewing *long* chains of deductive reasoning) can be a helpful expression of understanding, to clarify an individual's own thinking, or to assist in communicating an idea.

The other, more difficult road is to use mathematical models creatively, to uncover previously unknown facts. The acknowledged master of the technique in our time is Kenneth Arrow: his most striking results, while highly creative and illuminating, are essentially descriptions of the very narrow limits to what we can hope to prove when using these techniques with maximum rigour and honesty.

The question, as always, is: What are the alternatives? I will summarise here what will be spelled out further as we go on, in this section and throughout the book. Possible answers – or directions as to where to look for answers – include suggestions that the inputs to social economics should include:

- human values, those of both the subjects of the analysis and the analyst (these are now, as they must be, an input to existing systems of economic theory, but their role is not overt);
- material from the other social sciences; and particularly
- a recognition in social science analysis of what may be called unconscious processes, including intuition, judgment, and the full store of personal knowledge, only a small part of which is consciously present at any given time.

Can these matters be scientifically incorporated into a science of economics? Probably not, in the way that economics has defined science. Is there some other way? Probably so, and this will be a major task for social economics: it must find *a way to incorporate intuition, judgment and personal knowledge, along with human values, into a theoretic framework that is, to a sufficient extent, judgeable, teachable and applicable.*

THE STANDARDS REQUIRED FOR 'SCIENTIFIC BELIEF' There will unquestionably continue to be uses to justify putting some (not, perhaps, as great a proportion as at present) of the human resources available to the whole field of economics into the continued development of sophisticated techniques. However, we also need a system of economic theory that can develop

scientifically in a different direction. What this may require is an altered definition of what it means to be 'scientific'. In this respect, economics may be trailing behind the pack which it thought it was leading: the demise of positivism is older news in some other areas of the social sciences than it appears to be in our field.

It is increasingly clear, as we digest the Heisenberg uncertainty principle,⁶ along with Gödel's demonstration of the nonreflexiveness of mathematical proofs, that the positivist position, insofar as it depends upon claims to knowledge with certainty, is untenable. It is not the case, nevertheless, that the only alternative is then to turn to complete relativism. The 'facts' we hold in our minds may be best understood as *beliefs* rather than as *certain truths*; however, this is no reason to rush to the extreme of saying that all beliefs are equally good. We can work to define rules and procedures which both indicate (a) how to arrive at 'better' or 'worse' beliefs, and also (b) how to recognise which statements held out for our credence are 'better' or 'worse'. (Of course, our statements in these two areas, (a) and (b), will only be offered and held as 'beliefs', not as 'certain knowledge'.) Deeply, this is what methodology is about, and what makes it interesting.

Let us explore these issues a little further. It has become almost automatic to inquire, of any standards that are not taken as absolute, whether we should in that case interpret them as strictly relativistic. Relativism, in this context, can be of two kinds: moral relativism, which holds that any one person's or society's *values* are as good as any other's; and epistemological relativism, which says that, since knowledge with certainty is impossible, any belief is as good as any other. It must be stressed that neither of these, in the extreme form just described, is a logically necessary result of the debunking of the extreme positivism which flowered for a while earlier in this century. There is a good deal of reasonable ground between the two poles. An alternative to relativism, for example, is the philosophical position of realism. In the context of economics, it is described by the philosopher of economic methodology, Uskali Mäki, thus:

realism says that, independently of what economists think about the referential and representational capacities of economic theories, there is an objectively existing real world, and that the terms, statements and theories of economics can be used to refer to aspects of this reality (world realism) and can represent them truly or falsely (truth realism).⁷

Mäki goes on to consider 'the idea that truth and certainty somehow go together: to commit oneself to truth realism is to commit oneself to the possibility of certitude', and says,

This view is mistaken. Take the law of demand as an example. It is a statement about the relation between the price of a good and the demand for it. As such, it may be either true or false (perhaps with some qualifications of scope), in virtue of certain facts about social reality. Up to this point, I have made claims (on a semantico-ontological level) about truth but I have said nothing (on an epistemological level) about certainty. Certainty will enter the picture upon the formulation of an epistemological statement about the law, to the effect that, e.g., 'the law of demand is (or can be) known to be true (or false) on infallible evidential grounds.' I think this epistemological statement is false; but it does not follow from this that the law of demand is false. Truth and certainty belong to different realms, and should not be confused with each other. (Ibid, p. 97)

On the epistemological side it is important to look at what happens when the shift is made from 'certainty' to 'belief'. Both of those are mental states, which may be defined thus:

Certainty is the conviction that a direct, perfect, reproducible mapping can take place between the 'facts' of the real world and the mental constructs in an individual's mind. A person in a state of certainty believes that his/her mental constructs are, in some relevant sense, a perfect image of reality.

Belief is the state of mind which obtains when a person holds that, even though a perfect mapping between real facts and mental constructs is not possible, nevertheless his/her mental constructs are a pretty good approximation to a picture of reality.⁸

The state of mind described as 'certainty' holds that it (that state of mind) is directly created in response to facts of the real world; fact, and only fact, can produce certainty (according to those in that state of mind). 'Belief', however, recognises that it can, and usually does, come about in a number of ways: in part 'you believe what you see'; in part 'you believe what you are told'; in part 'you believe what you want to believe'. In other words, we may hope that, as in the construction of certainty, 'facts' have played the dominant role in constructing belief;

but we know that even perception ('what you see') is affected by prior beliefs and expectations; by our methods of sorting and categorising information as it reaches us; as well as by our wishes and values.⁹

We cannot, ultimately, prove that there *is* a reality 'out there'; nor can we define, without reference to our sensory experience, the correspondence between what our senses tell us and the presumed reality which we take to be the source of that sensory information. These are the discouraging conclusions which have pushed thinkers to such extreme positions as mysticism or complete relativism (moral or epistemological or both). Another, more moderate reaction is equally reasonable. It may be described as follows:¹⁰

- Although the scientific attitude prevents us from certainty about 'reality' or about 'truth', yet the alternative of rational, non-absolute belief need not be despised. *The recognition that one is operating in a science dependent upon belief rather than certainty is not inconsistent with standards that have long been associated with the ideal of scientific truth.*
- Absence of proof should not be regarded as absolute disproof. When we step away from the world of 'certain-yes or certain-no', a whole array of shades between belief and disbelief – with 'wait and see' attitudes in the middle – may come into play.
- Making inferences from the words and actions of others, and comparing these to our own sensory experiences, we may assemble images of 'the real world' which, while they remain subject to revision, are employed as beliefs about reality.
- An important goal is to establish rules for discovering and selecting (subject to further evidence) which particular beliefs are the ones we should choose to hold. (The art of rhetoric may be – though it is not always – understood as adhering to this goal.)
- While we accept (more or less reluctantly) that we can never be *certain* that our statements are perfectly true (or even that they are true at all), we strive nevertheless, with all the ingenuity and thoughtfulness we possess, to avoid practices which will obscure presumed-(though-not-known)-to-exist truth, and to adopt practices which will bring our beliefs into conformity with external reality. (This last statement summarises, I believe, the nature of all science.)

A great scientific apparatus was developed to ensure the recognition of truth as something certain; much of that apparatus can be used for

the more modest goal of making our beliefs as good as we possibly can, i.e., to give them what we (the people we respect, and we ourselves) deem to be the highest probability of having the closest correspondence with the guessed at (though never perfectly 'known') reality.

The conclusions, here, are similar to McCloskey's:

- It is important to understand how our beliefs, as economists, are formed; in order to achieve that understanding, we have to look farther than the 'simple facts' to which these beliefs are supposed, by some, to relate in a simple and straightforward way.
- The person who labels his/her epistemological constructs as 'beliefs' recognises that these constructs are subject to persuasion. Moreover, s/he will be sceptical that beliefs can be safely viewed as 'purely positive' or 'value-neutral' in any areas in which the people involved have feelings or values.
- Some of the ways of looking for the antecedents of economic (or other social science) beliefs include the study of intellectual history; the methods of discourse analysis; and an examination of levels of meaning.

The last item on this list will be the central subject for the remainder of this chapter.

**A CONSCIOUS
APPROACH TO
CONSCIOUS AND
UNCONSCIOUS
LEVELS OF
MEANING**

It is only fairly recently that the social sciences have claimed a place in the Western scientific tradition. By so doing, they have put themselves in the shadow of the natural sciences – trying to live up to a concept of what it meant to be 'scientific' which came from that so much older, so much better developed tradition. Quite recently the juxtaposition

of psychoanalysis with other social sciences has begun to bring a new light onto the comparison, pointing up an important way in which separate terms are required for an understanding of the social versus the natural sciences.

A partial explanation for the impact which psychoanalytic psychology has had upon individuals in the Western world, upon Western societies, and upon the sciences which deal with humans as individuals and in society, arises from what it says about meaning. It insists that we must add to our appreciation of *conscious, intended* meaning an anticipation of at least one additional, deeper, *unconscious* level. What we say, what we do, what we perceive, even what we think

and believe, all may be examined for meanings on at least two levels, roughly called conscious and unconscious. There are many views on the validity, or lack thereof, of psychoanalysis; in spite of debate on that issue, the social sciences reflect the effects of a fairly wide absorption of this critical perception about individuals and groups.

Discourse analysis is the formalisation of one way to try to apprehend meaning at more than one level. Building upon what good literary critics have always done (at least unconsciously), discourse analysis may be consciously used to free ourselves from the constraint to look only at the meanings that are claimed on the level where an actor or spokesperson consciously, intentionally conveys a conscious, intended meaning to the conscious level of perception of the recipient(s).

As long as only one kind, or level, or dimension, of meaning is recognised, a dramatic difference between the social and the natural sciences remains unexplored. This critical difference stems from the fact that the social sciences, in dealing with human beings, must deal with meaning. This difference remained in the background so long as the social sciences were assumed to be concerned, like the natural sciences, with a single-level kind of meaning, identified with a singular 'reality'.

The natural sciences try to discover and elucidate facts and principles that belong to the single plane of the natural world – that is the reality with which they have to deal. It has often been assumed that the social sciences were doing pretty much the same thing, even though the components of their reality happened to be human beings. However, the reality with which the social sciences try to deal has multiple (e.g., conscious and unconscious) meaning-planes. Thus we cannot expect the operations of the social sciences to be easily mapped onto the single-meaning dimensions of the natural sciences.

A foreshadowing of this recognition might be seen in discussions of 'unintended' consequences, in Adam Smith, for example. But those unintended consequences were understood either in a purely naturalistic mode (a pendulum will describe its natural arc, whether or not the releaser knew, understood, or intended that trajectory), or else in a religious mode (Man proposes, God disposes). The workings of the invisible hand might be understood in both these ways; the unintended consequences of Men obeying their natural (hence God-ordained), self-serving impulses could also be seen as the intended consequences of a beneficent deity. But God's meanings were never self-contradictory or divided. The thickness of multiple meanings, hence multiple understandings, only comes to the forefront in the sciences of Man when the layered, inherently self-contradictory nature of Man, the

subject, is accepted through something like psychoanalytical awareness.

This is not to suggest that that layered, contradictory nature was utterly ignored before psychoanalysis. Great psychologists have recorded their observations for millennia; their understandings have been generally best preserved in literature, either oral or else written down in poetry, fiction or drama. The absence of the vital psychoanalytical concept for conscious understanding of this layered complexity gave rise to a specific type of adjustment; the psychologists of the past recognised the presence of contradictions, but, lacking the concept of distinct levels of meaning, often explained this recognition to themselves and to their audiences by the concept of hypocrisy. There are, of course, people who are consciously hypocritical – who purposely mislead others as to their real purposes. However, hypocrisy may be a less probable explanation for behaviour resembling that of the notable hypocrites of literature (Iago, Molière's Hypocrite, etc.) than the psychoanalytic explanation which suggests that people don't always know, on one level, what they mean on another; they may talk and act at some times on one level, at other times on another – or on several levels at once.¹¹ The multiplicity of not necessarily dishonest, even when contradictory, meanings which authors put into texts and which their readers take out of them is now – through discourse analysis following upon psychoanalysis – available for non-judgmental critical examination in the social sciences.¹²

IDENTIFYING AND SORTING OUT DIFFERENT LEVELS OF MEANING The dualism of 'conscious' versus 'unconscious' is only one way of slicing layers of meaning in the human sciences. I would like to propose, and will use through the remainder of this book, another approach which I have found particularly useful in social science textual analysis. My emphasis, here, will be upon the real-life form of economics – what is practised – more than what is preached. This emphasis will reveal differences among social science meanings on three levels:

- . What the social scientist formally and openly claims to be doing; the 'stated' or 'conscious' level.
2. The 'effective' or 'unconscious' level; the implied or taken-as-if-implied meanings that are adopted and built upon, both by the author of those meanings and by others.
3. The operations that are performed on the 'logical' or 'technical' level.

Before going on to develop these 'levels' in greater depth, I will give a few examples of the ways in which they are already familiar to us.

When someone is sceptical about the relationship between theory and practice, or when a reader or critic protests that the logic of a piece of social science does not support the claims made for it, s/he may be referring to a divergence between the first and third of these levels.

It is also possible to find situations where there is mutual congruence on the first and third levels, while these both diverge from the middle level. For example, an economist might make a (level 1) disclaimer as to why, in a particular situation, scientific rigour cannot be maintained; and yet at the same time the results may be presented and received (on level 2) as if they were on scientifically firm grounds. The deepest level of meaning (level 3) may include the reality that what is *in fact* being offered is a mix of common sense, opinion, analysis, and fact. Sometimes this deep level is accurately reflected on level 1, in an overt statement or disclaimer; however the world demands simple statements to lead to action, and then the result may be that neither the overt statement nor the deepest level of meaning are the ones which have the most impact.

An example of a level 1 statement which will be of interest to us in Part III of this book is the following, from Chapter II of Marshall's *Principles*:

An opening is made for the methods and the tests of science as soon as the force of a person's motives – *not* the motives themselves – can be approximately measured by a sum of money, which he will just give up in order to secure a desired satisfaction; or again by the sum which is just required to induce him to undergo a certain fatigue.

It is essential to note that the economist does not claim to measure any affection of the mind in itself, or directly; but only indirectly through its effect (*Principles*, pp. 12–13).

I will claim, in Part III, that while most of Marshall's actual practice (on level 3) accepted the limitations stated in the foregoing, the message that went out on level 2 through much of Marshall's writing was far too easily understood as a claim that economists, in fact, can and do measure such 'affections of the mind itself' as desires and satisfactions. We will see, in Chapter 15, that such commentators as A. C. Pigou and Marc Blaug responded, and objected, to this level 2 implication.

John Maloney, a contemporary economist who, like myself, has found Marshall a useful starting point from which to address many of

the same issues as those confronted in this book, made a generalisation which is similar to the one I have been making:

Indeed this writer doubts if economists' qualifications of their own theories does anything to dislodge them from their readers' minds. Faced with, say, a standard account of the neoclassical theory of the firm, with a few half-hearted 'real-life' exceptions tacked on to it, the reader may merely congratulate the writer on his realism, openmindedness and intellectual honesty, and take the paradigm as being all the more authoritative. Listing exceptions and qualifications may also make the reader feel that the more that is wrong with a paradigm, the more must be right with it, to have justified so much trouble in exposition (Maloney, 1985, p. 215).

The economists' qualifications and exceptions cited here are offered on level 1; the readers' response, as Maloney describes it, fits on level 2.

It is the effect of the meshing of the readers' needs and wishes (e.g., for simple truths) with something offered on an inexplicit or unconscious level of the text which carries through into the way the text is then used. It is level 2 which has the most impact upon any audience of readers or listeners, and which is most likely to lead to further action. The effective level of meaning is the one in which the *as if* behaviour has its effect. Disclaimers, such as those instanced by Maloney, may state that a given procedure cannot be truly scientific for lack of adequate data, or of methods for putting the available data into quantifiable, commensurable, aggregatable form, etc. But on the effective level of meaning, the social scientist may nevertheless proceed *as if* all the necessary ingredients for scientific analysis were at hand; 'scientific' analysis is performed upon not-quite scientific data. When this is convincing, as it often is (often simply for lack of anything else that is *more* convincing) it is *used* as though the effective level were the 'real' (or logical) one.

Although the terminology I have chosen may be unfamiliar, this way of breaking down levels of meaning is a common-sense approach which has been employed by others. Such a consciousness, for example, is implied in the question, 'What does so-and-so *really mean* here?' Although that is a common kind of question, it is open to a good deal of complexity in the answer. First of all, it suggests that there is an 'apparent' meaning, but that the questioner is looking for something distinct from that: the 'real' meaning. Then the question arises: is there *only one* 'real' (or, for that matter, only one 'apparent') meaning?

Common references to ‘real’ meanings usually attempt to get at what I have called the third, or logical, level of meaning. When people suggest that we should dig deeper, past the apparent level to the real one, they may, by the ‘apparent’ level mean *either* level 1 or 2: thus there is a basis for disagreement on what people more-or-less casually think someone has said, before they even get down to disagreeing about what was ‘really’ said.

None of this is news in the realm of public discourse; it is not even news for discourse in the physical sciences.¹³ If it has been late in coming to the social sciences, this is perhaps because they are the most threatened by it; the contextual and individual (subjective) character of meaning in the social sciences is particularly obvious and significant, therefore particularly threatening to their claim to being scientific – when the concept of science, taken from a now outmoded concept in the physical sciences, means ‘objective’ and ‘provable’.

Considerable attention will be paid in Part III to highlighting the differences in these kinds, or levels, of meaning as I have found them in Marshall’s texts. Additionally, almost all of the remaining types of analytical activity which I will describe in this section depend to some degree upon an everpresent consciousness *that meaning does occur on a variety of levels; that most authors do not clearly signpost what is going on at which level; and that a more complete understanding of a text is possible when these levels are sorted out.*

Let us return to a more detailed description of the three levels:

- *Level 1:* What the social scientist claims to be doing. The ‘conscious’ or ‘stated’ level of meaning is most typically to be found in self-consciously worked-out formal statements.
- *Level 2:* What the world acts as if it thinks the social scientist is doing.¹⁴ The ‘unconscious’ or ‘effective’ level of meaning may be thought of as something that does not exist in the text standing alone; it is created interactively, being revealed as the receivers of the text interpret (by the light of their own experience) something as vague as its ‘general tone’, to form a basis for both general and specific expectations, and for their own further thought and action.

Some people, of course, bring exceptionally idiosyncratic experience to all that they do, including their reception of texts. In speaking of a generalised ‘level 2 meaning’ of a text, I, as textual analyst, am referring to the cluster of interpretations that I assume are most commonly made, or that I guess to be most likely. (I will comment further, below, on the ‘assumptions’ or ‘guesses’ of this

nature that the critic must make, and the basis on which they may be made.)

The impact of level 2 meanings may be carried through to logical, intellectual activities, but the 'effective' level of communication passes, most commonly, from the pre-cognitive processes of one mind to the pre-cognitive processes of others.¹⁵ Interpretation and analysis of such communication necessarily contains a highly subjective element.

Note that level 2 is often used as the basis for work on level 3 – both by the person whose work generated the particular level 2 meaning, and by other people.

- *Level 3*: Those parts of what the social scientist is actually doing that can be defined or schematised in logical terms.

Unlike level 1, which takes the form of a *statement*; or level 2, which takes the form of a *belief*, the 'technical' or 'logical' level of meaning is a set of *operations*; it is the level on which inputs (statements, axioms, beliefs, etc.) are operated on by the methods of logic (or whatever other methods are used) to produce an output different from whatever the author took as the inputs to his/her work.¹⁶

DISCUSSION OF LEVEL 2, 'THE EFFECTIVE LEVEL OF DISCOURSE' If there is any subtlety in the foregoing model which might make it hard for the reader to translate the proposed levels of meaning into terms which s/he is accustomed to using, it is in level 2. Another way of describing this level of meaning is to point out that it is *what is taken as the basis for action or for further understanding* (hence the name 'effective level'). I will elaborate a little further.

It is in unfriendly criticism that one is most likely to encounter attention to the 'effective level' of discourse. In Chapter 13 I will use Marshall's contemporary, Joseph Nicholson, as an example of such an unfriendly critic. He brought attention to level 2 of Marshall's work for reasons which may be explained as follows.

A reading of almost any part of Marshall's work would lead most readers (Nicholson included) to believe that Marshall had a broad concern with all classes of people, and especially with the poorer members of society; that Marshall took needs (especially in the context of poverty) to be at least as important as wants; and that he believed that an understanding of motivations (including needs, wants, values and satisfactions) are at the heart of economic inquiry. Nicholson

stressed that, by contrast to this expectation, Marshall's most rigorous logic does *not* deal with needs, motivations, values, or even with satisfactions; it only deals with wants, and with them only as they are 'effectively' expressed in market power. Blaug, similarly, expressed his astonishment that Marshall's logic would retreat to the narrow range of events which can be assumed to 'affect in about equal proportions all the different classes of society'.

The reason to be surprised or disappointed in this aspect of Marshall's logic is that one had expected something different: the 'real' (level 3) meaning disappoints us as compared to what we had taken to be the 'apparent' meaning. But here, I claim, our disappointment derives from an expectation stemming from the apparent meaning *as it is found on level 2* – an expectation which we infer from the general tone of Marshall's writing, rather than from formal, explicit statements. By contrast, if we had derived our expectations exclusively from level 1 – Marshall's overt claims or formal statements – we would find that most of the time (especially if we ignore the early writings) his statements are quite consistent with the narrow working out of his logic. This was the consistency which Edgeworth emphasised (also discussed in Chapter 13, below), referring to statements of Marshall's which had, after all, promised no more than the use of prices as indicators of relative well-being within a very narrow range of circumstances.

My own interpretation of the general tone (level 2) of Marshall's writing is that it suggests that his hopes for the use of prices in economic studies were broader than this. But how have I identified the effective level of Marshall's discourse? It is worth taking a look here, using myself as an example, at an individual's interaction with the text, out of which level 2 is created.

I have said that, in speaking of a generalised level 2 meaning of a text, I am referring to the cluster of interpretations which I assume are most commonly made. Sometimes I make such an assumption on the basis of a good deal of evidence; at other times it represents a guess about how people in general are likely to react. That guess is conditioned in part from my own lifetime experience which has generated a set of expectations about typical human reactions to each given situation; these expectations are, of course, modified by my own reaction to the particular experience, with an attempt to correct for whatever of my own responses experience has told me are not 'typical'.

In identifying the effective level of Marshall's discourse, then, first of all I took into account my own reactions. Specifically, by the end of my first reading of *Principles of Economics* I found myself in a state of

cognitive dissonance; on the one hand I had been convinced (mostly by the general tone – level 2 – but also by some overt statements on level 1) that prices could be used as the windows onto a tremendous amount of knowledge about human motivations and satisfactions. On the other hand, I seemed to be missing some critical pieces of the logical operation which could thus connect prices with the said human motivations and satisfactions. I reread the book, and found that I had missed that operation because it did not exist.

The general sense of the whole book (level 2) seemed to support the belief that the prime subject of Marshall's economics is *values*, in the sense of 'what matters to human beings', and that he had succeeded in dealing with this subject objectively, quantitatively and 'scientifically'. At the same time, there were a number of clear statements (on level 1) disclaiming such a wide ambition. And the ambition was not fulfilled on the operational level (level 3).

My own sense of having been led to expect something which, upon closer analysis, proved to be elusive on level 3 and was frequently disclaimed (though sometimes, contradictorily, claimed) on level 1, made me look for a similar disappointment in other readers. I have commented on my findings with respect to Nicholson, Pigou and Blaug. I would be willing to argue (but it would require a long discussion) that a similar sense of disappointment was among the motivations for Maloney's book, *Marshall, Orthodoxy and the Professionalisation of Economics*. More important, but virtually impossible to prove, is my hypothesis that the economics profession at large has adopted Marshall's level 2 hopes into its institutional belief structure.

**THE
IMPLICATIONS
OF LEVEL 2 IN
MARSHALL'S
WORK FOR THE
SUBSEQUENT
DEVELOPMENT
OF ECONOMICS**

The hypothesis just cited grows out of the following observations and reasoning (which will be expanded in Part III): Marshall's programme was to establish a consistent relation between the intangible subject of human welfare and the tangible measure of money, or price. He himself may have been seduced into this attempt by the fact that, in common parlance there is one word – *value* – which can be used to refer to both sides of the desired relation. Marshall was careful to restrict his own *formal* use of the word to its 'exchange value' side, saying more than once that he would not employ it in the sense of 'use value'. However, in talking of the 'real worth' or the 'real cost' of things he was, in effect, using 'value' as the pivot on which he could turn to either of the desired directions.

The result of using a word which had different meanings on levels 1 and 2 was confusion on the logical level, where Marshall, often without announcing (or, probably, recognising) that he was doing so, would operate on one meaning, and then switch midstream to use of the other. Thus, as I will attempt to show, Marshall built some of his economic operations upon a much broader level 2 meaning of 'value', *in spite of* its conflicts with his more restricted level 1 statements.

Moreover (this is the largest logical leap, to be critically examined by others), I contend that his level 2 meanings were sufficiently widely taken into the field generally (first by economists who read Marshall's work, then by others who were taught or influenced by those who had read his work), so that the beliefs and expectations generated therein continue to be perpetuated even today. Economic writings and discussions continue to be based upon scientific-sounding references to a variety of 'values' broader than those simply communicated through prices.

I will claim, further, that neoclassical economics is designed to deal with – and is received, on level 2, as though it can and does deal with – questions about how humans actually act on the basis of what they perceive as their wants. However, the theory is formally limited to consideration of effective demand – a much more limited concept than 'what humans perceive as their wants'.

As I go on, later, to discuss economic writings which seem to me to build upon level 2 in Marshall's writing, I will assume that there is, at any given time, a general sense of what economics is about, what it can and should attempt, and what it has already achieved; I do not pretend that all of that general sense of the field derives from readings of Marshall, but I will try to trace threads in that general sense for which Marshall's writing forms a credible source. Note that it would be possible to compile a set of statements from published economic texts which would support the above cited level 2 perception of what economics can and does provide; and another list of statements in the literature which would show full awareness of the logical limitations of the field. We would then be in the situation of having two sets of level 1 statements, the first set proposing to do something more ambitious than what turns out to be possible on level 3; the second set recognising the limitations of the logical level. What are we to make of that?

We might simply say that those economists who maintain level 1 statements more ambitious than what can be supported by the most sophisticated and subtle level 3 logic are bad economists, and should not be considered in our discussions. Such a conclusion would not

necessarily be in accordance with the status ordering of the field at any given time; it sounds like a good idea, but under the current sociological structure of the field it cannot be enforced so as to protect the unwary from such 'bad economics'.

More useful may be the adoption of practices which would legitimise the perceptions of even those who are not the most economically sophisticated, when they find that the pictures drawn by economics are not just simplifications from, but are distortions of, the real world. Recognition of the author's responsibility for the level 2 meanings contained in his/her text may prove to be an effective way to provide such legitimation. This may be an alarming or even a distasteful idea: who wants to be held responsible for flawed interpretations or misuse of his/her work? I quail at the thought, myself, as I imagine the fuzzy thinkers who might latch onto my abjuration of the kinds of 'rigour' called for in mainstream economics, using this as justification for a sentimentalist approach with which I would not be in sympathy.

All the same, given the quantity of material now being written in all the social sciences, it is not only possible but necessary to impose higher standards than ever before for what shall be culled out as the 'best' work. Those standards can and should include a requirement for writing to be as clear and direct as possible, so as to reduce the possibilities for misunderstanding and misuse. The *natural* ability to express oneself lucidly has always been valued. To this can be added a *learned* ability to recognise different levels of meaning in what one is writing, as well as in what one reads and hears, so as to avoid sending out unintended messages.¹⁷ Here again, what is being proposed has to do with how the discipline is taught as well as with its content.

DISCUSSION OF LEVEL 3 Contemporary analysis of economic texts is most likely to be carried out with regard to their level 3 meanings. This is often done as if it is, in fact, the only level of meaning.

To see the use made of logical implications in ordinary analysis of economic texts, we might look at any of the commentaries on Marshall that will be described in Chapter 16, below (e.g., by M. Friedman, L. E. Fouraker, D. A. Walker, E. B. Wilson, J. M. Bailey, etc.), where there are many attempts to disclose 'what Marshall is actually doing' (on level 3). The process of 'following through logical implications' has led me along a path closely parallel to that marked out by the commentators whom I will examine in Chapters 16 and 17. There I will

discuss this process in sufficient detail so that I will not describe it further here, but will merely summarise two of its principal elements.

One is the procedure of *pointing up areas of incompleteness, with their implications*. Again, Chapter 16 will exemplify this technique of textual analysis; here I will simply suggest as examples some criticisms of Marshall's statements about demand, which were incomplete in specificity of definition and in mathematical analysis. For instance, he failed to specify what, exactly, he meant by 'constant marginal utility of money'; what, exactly, he intended to hold constant at any given time; when he did and did not intend to restrict his analysis to commodities relatively unimportant within total expenditure; etc.

The other most striking element of level 3 analysis is the technique of *revealing contradictions: internal, external, and methodological*. Modern neoclassical economists are most apt to stress internal inconsistencies; this kind of analysis is what is most thoroughly exemplified in Chapter 16. There I will also look at the charge that some of Marshall's procedures and assumptions are inconsistent with the realities which he claimed to be studying. These might be described as *external inconsistencies*, since they refer to a contradiction between, on the one hand, some element within the Marshallian system and, on the other, that reality onto which the system was intended to be mapped.

We will also see (it will be Pigou who points it out) that there is a *methodological inconsistency* in Marshall's apparent fall-back upon assuming a direct measure of utility as a foundation of the system by which he hoped to be able to deduce measurements of utility.

INTERNAL AND EXTERNAL CONSISTENCY In Chapter 16 I will take up a question which has puzzled Marshall's commentators: namely, how could he have continued employing a particular group of assumptions which were mutually contradictory? I will suggest there that Marshall may have been less concerned with the model's internal consistency than he was that each of its elements be brought, as well as possible, into consistency with the known world. The following is my summary of the trade-off we confront between internal and external consistency:

Our knowledge is imperfect. Imagine that element A' of our model is a simplified reflection of our best understanding of some aspect, A, of the real world, and similarly for B', C', and so on in relation to elements B, C, etc., in the real world. Imagine, also, that we have particular difficulty in perceiving, say, the reality, C; then the result

may be inconsistency between A' , B' and C'' (so designated to indicate that its difference from C is greater than permissible).

What is the right solution to such a situation as this? Obviously, the *best* solution would be to identify the source of error, and correct it. But the problem is that our knowledge is imperfect, and we probably do not know which modelled element – A' , B' , or C'' – is the one containing the most serious divergence from the real A , B , and C . In such a situation of ignorance, if we begin to fiddle with these elements so as to bring them into consistency *with one another*, we are more likely to end up with some form of A'' , B'' , C'' than with our desired A' , B' , C' : there are more ways of guessing wrong than of guessing correctly.¹⁸

Marshall rejected what many commentators would have liked to impose as a requirement, namely that he carry his models through to their logical conclusions (which would have alerted him to their *internal* inconsistencies). His method was, instead, to pay more attention to the relationships between what I have schematised as A' and A , B' and B , C' and C , etc. than to what he saw as secondary relationships, between A' , B' , C' , etc. In other words, as compared to the more 'rigorous' economists of today, he often gave greater emphasis to 'external' versus 'internal' consistency.

To summarise what is implicit in these distinctions: if a model contains some elements which are, in some way, 'wrong' (e.g. they simplify from experience in ways that produce undesirable distortions in understanding and/or prediction), then internal consistency is not necessarily a virtue, and may be a disadvantage. Economics may have taken on the ambition of being internally consistent when it was yet too young to make a virtue of this quality; as the standard theory exists today, it contains a great number of effectively 'wrong' elements which are simply compounded in the rigorous consistency of its logic. This suggestion will not be employed as a blanket approval for all of Marshall's inconsistencies, but rather as a reminder of the too-rarely mentioned possibility that internal inconsistency may sometimes emerge as an alternative to something worse.

Anyone who employs this argument to justify an internal inconsistency in his/her own logic must, of course, be making an open admission along the lines of: 'Evidently some piece of what I am putting forth is a particularly poor description of the world; at this stage I don't know how to find or correct that external inconsistency, so the internal inconsistency remains as a reminder of the flaw in my

mapping from reality.' There are many places in the economic literature that already exists where such an admission would be appropriate, and healthy. Some changes in the social structure of the field will be necessary, however, before they could be made with confidence that they would be evaluated appropriately.

I am not proposing that Marshall himself consciously thought through a defence of internal inconsistency such as I have suggested here; I put it forward to explain the emphasis I will choose later, in Part III, when I examine Marshall's demand theory and what it has led to. Given Marshall's strong desire to be 'scientific', instances of sustained internal inconsistency in his work raise very interesting questions, e.g.: Why did he accept this – instead of what alternative? What may his successors have lost, in the process of resolving what appeared as internal contradictions in Marshall's work?

The attention which I will give to the analytical problems in this area of Marshall's work will not emphasise the internal inconsistencies in themselves, but will regard them, rather, as important among several kinds of indicators which point to the deeper problem which can be summarised by saying that Marshall was trying to do something which was impossible – and which, I will later suggest, he intuitively suspected to be impossible.

Marshall in fact attempted to continue to walk upon two paths, one increasingly dependent upon quantitative methods, the other including the richness of the political economy tradition of Adam Smith. However, it is not clear that this is still possible, at least not in the way that Marshall did it. The neoclassical path has proceeded a very long way from the fork where the two paths diverged. Were it to try to reintroduce the ambiguity of Marshallian language it might lose too much of the very real progress which it has made.

It is for this reason that I have thought in terms of developing an alternative path, of social economics. We have an option, in charting out a course which Marshall only began, but did not carry as far as I would like to go: now is the time to explore whether social economics must continue in a straight line away from the other path, or whether it may bend close enough to the neoclassical path to be able to borrow some of its tools.

For the neoclassical path to borrow much from social economics may be impossible; neoclassical economics starts from a context which is committed to the exclusion of ambiguity. But if the social economics path includes some recognition and acceptance of ambiguity in its very foundations, it will have the freedom to borrow tools developed in the

exclusive context of neoclassical economics. Some of these tools will be considered in the next two chapters, as we continue to consider the uses of 'judgment' as a companion/alternative to the neoclassical (and, increasingly, the Marxian) emphasis on technique.

Notes

1. (Basil Blackwell, Oxford, 1986) ed. by R. Flood and M. Lockwood. I am indebted to Dan Dennet for bringing this book to my attention.
2. Donald N. McCloskey, *The Rhetoric of Economics* (University of Wisconsin Press, Madison, 1985).
3. In all of this it may be that the field of economics resembles that of mathematics more closely than economists realise, in different ways than they would wish. A delightfully funny, and all-too-true, portrait of 'The Ideal Mathematician' is drawn in the book, *The Mathematical Experience*:

His writing follows an unbreakable convention: to conceal any sign that the author or the intended reader is a human being. It gives the impression that, from the stated definitions, the desired results follow infallibly by a purely mechanical procedure. In fact, no computing machine has ever been built that could accept his definitions as inputs. To read his proofs, one must be privy to a whole subculture of motivations, standard arguments and examples, habits of thought and agreed-upon modes of reasoning (Davis and Hersh, 1981, pp. 36–7).

The authors of the book then imagine a conversation between The Ideal Mathematician and a student, who asks, 'Sir, what is a mathematical proof?' The Mathematician answers:

what you do is, you write down the axioms of your theory in a formal language with a given list of symbols or alphabet. Then you write down the hypothesis of your theorem in the same symbolism. Then you show that you can transform the hypothesis step by step, using the rules of logic, till you get the conclusions. That's a proof.

Student: Really? That's amazing! I've taken elementary and advanced calculus, basic algebra, and topology, and I've never seen that done.

I.M.: Oh, of course no one ever really *does* it. It would take forever. You just show you could do it, that's sufficient .

Student: Then really what *is* a proof?

I.M.: Well, it's an argument that convinces someone who knows the subject. (ibid., pp. 39–40.)

4. See, e.g., *Principles*, pp. 637–8 and 644: quoted in Chapter 10, below, and 7, above.

5. There are, obviously, many discussions on this subject. One I particularly like is in Chapter 2, 'Theory, Formal Model and Reality' of János Kornai, *Anti-Equilibrium* (North-Holland Publishing Company, Amsterdam, 1971) see esp. p. 16.
6. This asserts that there is an incompatibility between measurement of position (e.g., of electrons) and measurement of momentum: you can know either separately, but both together are immeasurable. Indeed, it seems that an electron is not simultaneously characterised by both momentum and position; when one characteristic is observed, not only is the other then unobservable – the other characteristic then does not exist. Hence the ordinary understanding of the Heisenberg uncertainty principle, that it refers to the way that observation affects the fundamental characteristics of the thing observed.
7. Uskali Mäki, 'How to Combine Rhetoric and Realism'; *Economics and Philosophy*, 4, 1988, p. 96.

While Mäki has used McCloskey's work (and, to a lesser extent, that of Arjo Klamer) as a most useful foundation for further development of these subjects, he also makes an important criticism:

There seems to be an interesting incongruence or tension between how Klamer and McCloskey see the nature and tasks of economics on the one hand and the metatheory of economics on the other. They seem to be (implicitly) committed to the following normative statement: Whereas it is not and should not be the goal of economists to strive for truth about the economy, it should be the goal of metatheorists to pursue truth about economics (*ibid.*, p. 97).

What he points out is that, in emphasising the role of rhetoric (the art of persuasion) in economics, McCloskey and Klamer have adopted an anti-realism, excessively relativistic stance – even while, as metatheorists (individuals who theorise about theory) they implicitly hold themselves to a standard of 'truth realism'. Like Mäki, I believe that it is the 'metatheoretical' stance of the proponents of recognising rhetoric in economics which, in accepting 'truth realism' even without certainty, represents the best side of the 'rhetorical' strand in modern economic thought.

8. A third possibility is *faith*, which holds that there is some underlying truth 'more true than reality' such that real facts are not required to prove that which is held by faith; the most absolute faith may not even be shaken by contradictory 'evidence' from the real world.

A fourth possibility, *delusion*, is defined similarly to faith, except that a delusion is a belief-held-in-the-face-of-evidence, which belief (unlike the belief of 'faith') is *not* supported by the surrounding society.

9. See Chapter 10, below.

A famous (though quite inconclusive) discussion of these issues is to be found in the notes by Ludwig Wittgenstein which were printed as a book called *On Certainty*, ed. by G. E. M. Anscombe and G. H. von Wright (Harper & Row, New York, 1972). Especially interesting for our consideration is Wittgenstein's connection of a state of mind (whether it

be called *belief* or *knowledge*), which results from confrontation with 'facts' or 'evidence', with something like trust, regarding *which* evidence, *which* facts to accept: knowledge, Wittgenstein says, 'is related to a decision (p. 47e).

A way of preserving the term, *knowledge*, but toning it down to accord with what I have called *belief*, is presented by J. R. Lucas (in Flood and Lockwood (eds) 1986, p. 126), who says: 'Knowledge is . . . subject to a retrospective withdrawal proviso. So long as the prediction works out, you really did know all along: but if the prediction proves false in the event, then the knowledge claim has to be withdrawn, and you never really knew what you thought you did.' Cf. again Wittgenstein (in Anscombe and Wright (eds) 1972, p. 3e): "'I know" seems to describe a state of affairs which guarantees what is known, guarantees it as a fact. One always forgets the expression "I thought I knew".'

10. What is described below is logically just as relevant – and appears to be at least as acceptable – to the physical as it is to the social sciences.
11. For those who still seek to see all behavior explained by a single, rational motive, a modern approach is described by Etzioni:

A thesis shared by several members of the Public Choice school and several other neoclassical economists is that individuals will lie, cheat, and violate other moral precepts and laws whenever they expect they can get away with it or when the penalty will be smaller than the gain. Williamson argues that rational actors who pursue self-interest are expected to act opportunistically, which often entails acting immorally. For example, those who are skilled at dissembling realize transactional advantages. Economic man, assessed with respect to his transactional characteristics, is thus a more subtle and devious creature than the usual self-interest seeking assumption reveals (1975, p. 255). (Etzioni, 1988, p. 58.)

12. For a similar approach, see the Introduction to the paperback edition of *James and John Stuart Mill; Father and Son in the Nineteenth Century*, by Bruce Mazlish (Transaction Press, New Jersey, 1988). This work is an excellent example of an historian's broad application of the insights of psychoanalysis to a range of other social sciences.
13. Cf. the 'externalist' tradition, associated with the names of J. D. Bernal (e.g., *The Social Function of Science* (Routledge, London, 1939) and of Boris Hessen, whose 1931 paper, 'The Social and Economic Roots of Newton's Principia' (printed in *Science at the Crossroads* (Frank Cass, London, 2nd edn, 1971)) presented at the Second International Congress of the History of Science in 1931, attempted to 'recontextualise' science: to recognise, that is, that even in the 'hardest' sciences there is room for interpretation, which is affected by the social and economic context of the human scientist. (Hessen may be read as distinguishing between a core of 'hard fact' which is not affected by the social context; and all the deductions, applications, etc., therefrom, which are.) A recent paper by Loren R. Graham, 'The Socio-political Roots of Boris Hessen' (in *Social Studies of Science* (Sage, London, Beverly Hills and New Delhi) vol. 15,

1985), has done for Hessen – treating the latter in his role as social scientist – what Hessen did for Newton, as a physical scientist.

14. By 'the world', as referred to in the description of level 2, I mean either the world of policy-makers and citizens who regard economists as experts; or the rest of the economics profession, which is often only too eager to proceed as if some knotty problem had been resolved. A good example of the latter, to which my attention was drawn by Pankaj Tanden, was the response to the publication of Robert Willig's 'Consumer's Surplus Without Apology' (*AER*, September 1976). Errors in emphasis, definition and analysis have been pointed out which throw significant doubt upon Willig's claim that 'observed consumer's surplus can be rigorously utilized to estimate the . . . correct theoretical measures of the welfare impact of changes in prices and income on an individual' (p. 589). However, in numerous subsequent references to this article, which implied or stated that the measurement of consumer's surplus was no longer a problem, the profession exhibited a credulity which is best explained as prompted by the desire to believe that there are rigorously adequate quantifiable proxies for the important unquantifiable variables in welfare economics. (Willig's article will be discussed in some detail in Chapter 17 of this book.)
15. I do not attempt to deal, here, with the area of manipulative or propagandistic communication wherein the communicator *knowingly* puts out covert messages that may be at variance with his/her level 1 statements or level 3 logic, but which are intended to sway the audience through emotional or other non-cognitive means.
16. A fourth level could be added to the above: (level 4) the effect the social scientist ultimately wishes to have, i.e. what s/he would most like to achieve through the work being examined, as the ramifications and consequences of that work unroll in the world. What Alfred Marshall, for example, wanted as the ultimate effect of his work was a world of greater economic prosperity of a type which would contribute to the 'progressive' development of the human spirit. However, this goal may perhaps be better kept separate from the categorisation of *meanings*; it does spill over into meaning, but is most directly understood as *goals*. It has already been discussed under that heading, in Chapter 2; for a further development, it will have to await *Social Economics*, volume 2.
17. An alternative possibility is that a social scientist may choose to bring the intended operations on level 3, and the formal statements of level 1, into accord with the messages sent out on level 2, once s/he has recognised what those messages are.
18. This point is not unique to the social sciences; an example of recognition of the same problem in the natural sciences is ascribed to Francis Crick, who said that 'a good model is one that does *not* account for all the data, . . . for some of the data are bound to be wrong.' (George Johnson, 'Two Sides to Every Science Story', a review of *What Mad Pursuit* by Francis Crick, in *The New York Times Book Review*, 9 April 1989, p. 41.)